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ABSTRACT

The Impact of Raising the Minimum Legal Drinking Age on Academic Achievement and Risky Behaviour: A Difference-in-Discontinuities Approach*

This study examines the impact of increasing the Minimum Legal Drinking Age (from 16 to 18 years old) on the academic performance, substance use, and peer behaviours of teenagers. Using a difference-in-discontinuities design, we exploit regional MLDA reforms in Spain and PISA data to identify significant improvements in mathematics and science performance, particularly among male teenagers and those from lower socioeconomic backgrounds. A complementary analysis using data from the Survey on Drug Use in Secondary Education in Spain indicates that these academic gains coincide with reductions in alcohol consumption, intoxication, smoking, and marijuana use, suggesting a link between substance use and educational outcomes. Moreover, the reform led to less drinking and less use of illicit drugs within peer networks, highlighting the amplifying role of peer effects in policy impact.

JEL Classification: I18, I12, I21

Keywords: minimum legal drinking age, education, risky behaviour, alcohol, difference-in-discontinuities, PISA, ESTUDES, Spain, teenagers

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1 Introduction

The Minimum Legal Drinking Age (MLDA) is a widely used policy tool designed to regulate alcohol consumption among young individuals with the primary objective of protecting their health and well-being. A substantial body of research has documented the negative consequences of early alcohol consumption, including adverse health effects, increased likelihood of alcohol dependence, and higher rates of accidents and injuries (Dee, 2001; Carpenter and Dobkin, 2009; Carpenter and Dobkin, 2011).

Beyond these direct health consequences, alcohol consumption is an important determinant of how well a teenager learn and perform academically.[1]. Imposing some limits on how young people can access alcohol are shown to improve schooling behaviour and academic performance (Carpenter and Dobkin, 2009; Lindo et al., 2016).

Moreover, delaying the age of first alcohol consumption can have broader implications and unintended consequences for youth behaviour. Although the evidence is less clear-cut, adolescents who cannot legally obtain alcohol may compensate by substituting for alternative substances, such as tobacco or illicit drugs, which could pose even greater risks (Dee and Evans, 2003; Deza, 2015; Crost and Guerrero, 2012; Crost and Rees, 2013). Additionally, the inability to legally purchase alcohol may lead young people to change their social circles, seeking out peers who can provide access to alcohol or other substances. This reconfiguration of peer group formation can further influence their engagement in risky behaviours and alter their social interactions, potentially affecting school performance and future opportunities.[2]

Assessing the impact of the MLDA presents a significant challenge – primarily due to other confounding factors, such as individual differences in risk factor and other preferences. Thus, a standard causal approach in the literature exploits a Regression Discontinuity design (RD) (e.g., Carpenter and Dobkin, 2009; Crost and Guerrero, 2012; Crost and Rees, 2013), which compares outcomes of individuals around the threshold age.

Building on this literature, we examine the impact of a reform that increased the MLDA during adolescence. Specifically, our analysis exploits the case of three regions in Spain—Castilla y León (2007), Galicia (2011), and Asturias (2015)— that raised the

[1]Past work that examines the relationship between health and academic achievement has provided substantial evidence suggesting that improved health is closely associated with enhanced human capital, particularly in terms of academic performance (e.g., Grossman, 2000; Case et al., 2005; Currie, 2009)

[2]Beyond direct policies targeting alcohol consumption, prior research has demonstrated that external factors unrelated to classroom instruction or inputs can significantly influence academic achievement, for instance, shifts in time allocation and leisure choices (Lindo et al., 2012; Metcalfe et al., 2019; Stinebrickner and Stinebrickner, 2008; Lee, 2013).

MLDA from 16 to 18 years. Together with the data availability whereby we can observe a cross-sectional sample of teenagers over multiple years before and after the reform in each region, we can leverage an empirical strategy of Difference-in-Discontinuities (diff-in-disc). The approach goes beyond a standard RD design – lifting the key requirement that drinking restrictions are the only changes at a given age. Consequently, it enables us to isolate the causal effect of the reform, which restricts alcohol access for teenagers aged 16 years, on their academic performance and engagement in risky behaviours.

Our study contributes to the literature in several ways. Our first contribution is on new evidence on the impact of alcohol consumption restrictions on academic achievement by leveraging a policy change that affected an entire cohort of students. In this regard, the MLDA reform in Spain offers new insights in two key aspects. First, it applies to a younger population than in the US, where the legal drinking age is 21. Second, it allows us to assess the potential educational gains among young people who could no longer legally obtain alcohol after the MLDA was raised from 16 to 18. Moreover, to our knowledge, there is limited evidence on the effects of the MLDA on academic achievements from a European perspective.[3]

We find that raising the MLDA from 16 to 18 years old leads to an improvement in PISA test scores, with gains ranging from 0.07 to 0.17 standard deviations in our most conservative specification. The effect is particularly strong for male students, who experience a 0.2 standard deviation increase in maths and science scores, and for students with less educated parents, where gains reach 0.25 standard deviations across subjects.

Our second contribution is to provide novel evidence on heterogeneous responses in drinking behaviour to the MLDA increase. We apply our diff-in-disc estimation to the repeated cross-sectional sample of teenagers in the Survey on Drug Use in Secondary Education in Spain (ESTUDES). The reform reduces the probability of ever drinking, with stronger effects for teenage boys and those from less educated backgrounds (6.6 and 10.3 percentage points, respectively). In contrast, teenage girls do not significantly alter their likelihood of ever drinking. In terms of intoxication, teens from lower socioeconomic backgrounds reduce the likelihood of being drunk in the last month by 8.4 percentage points (pp.). The heterogeneity of the responses in our Spanish teenagers corresponds to what Ahammer et al. (2022) finds for Austria.

Third, we document the spillover effects of the MLDA reform on the consumption of

[3] Recent works that provide European-based evidence of the effect of the MLDA (at 16 years old) on drinking behaviours are Ahammer et al. (2022) for Austria; Dehos (2022) and Kamalow and Siedler (2019) for Germany; Gupta and Nilsson (2020) for Denmark.

other substances. The probability of ever smoking tobacco declines by 8.5 pp. for males and 10 pp. for teens with less educated parents. Similarly, the probability of ever using marijuana drops by 4.3 pp. among boys. These results suggest that delaying access to alcohol not only affects drinking behaviour but also reduces engagement in other risky behaviours commonly associated with early alcohol consumption.

Finally, we provide evidence on the impact of the MLDA reform on students' perceptions of peer substance use, shedding light on the potential mechanisms driving the observed effects. The reform reduces the perceived frequency of drinking among friends, with stronger effects among male students and those from lower socioeconomic backgrounds. These findings support the causal effect of the reform by indicating that changes in peer behaviour are not driven merely by self-reporting biases. Furthermore, the heterogeneity in peer effects aligns with the patterns observed in PISA scores, reinforcing the link between alcohol consumption restrictions and academic achievement.

Our work is closely related to the contemporaneous work of Bagues and Villa (2025) that also exploits the reform of MLDA in Spain to study its effect on educational outcomes and own consumption of alcohol and other substances. Their study also utilises a sample of teenagers from the PISA and ESTUDES datasets. However, our approach diverges from theirs in the estimation strategy. While we exploit the age threshold of the law within a regression discontinuity framework—restricting our sample to individuals closer in age by month and from only the three regions that reformed their MLDA, their causal estimation relies on a two-way fixed-effects model.[4] Consequently, their sample covers a broader age range, including 17- and 18-year-olds, and covers all 17 Spanish regions. Overall, their main findings on PISA test scores and alcohol consumption align with ours. However, our results differ in two key dimensions. First, we find that teenagers from lower

[4] Several factors motivated our decision to favour a Difference-in-Discontinuities (Diff-in-Disc) approach over a Two-Way Fixed Effects (TWFE) model. First, since the source of variation comes from individual differences around a threshold across periods, this method allows us to control for time-varying factors that differ across regions, reducing concerns about unobserved heterogeneity. Secondly, in a context where all control regions had already reformed their MLDA during the period under analysis, the estimated parameter in a TWFE model has an interpretation consistent with a Difference-in-Differences in Reverse (DDR) approach Kim and Lee (2019). The DDR framework estimates a pre-treatment effect on the group that eventually adopts the policy change by comparing their outcomes before implementation to those in regions where the MLDA had already been enforced. This shifts the interpretation of the policy effect: instead of identifying the impact of MLDA implementation per se, the estimate captures how not having the policy for longer affected the switching regions relative to always-treated regions. While DDR is useful in staggered policy settings, its interpretation may require more caution, as it primarily reflects pre-treatment behaviour in switching regions rather than the direct causal effect of the MLDA reform. In contrast, a Diff-in-Disc approach enhances identification by focussing on individual-level variation around the MLDA threshold, which allows for sharper comparisons within the same regional and temporal context. This strategy avoids potential biases from regional trends and pre-existing differences across treated and control regions that may be present in a TWFE model.

socioeconomic backgrounds benefit disproportionately from the MLDA reform. Second, while their study finds limited evidence of changes in the consumption of related substances, we observe significant reductions, particularly among teenage boys and those with less educated parents.

In summary, our findings highlight the broader implications of raising the MLDA. Restricting alcohol access for 16-year-old adolescents not only leads to a reduction in harmful behaviours associated with risk taking, but also has the potential to improve educational outcomes, particularly among disadvantaged groups. By mitigating the negative effects of early alcohol consumption, the policy may contribute to narrowing achievement gaps and fostering greater equity in academic performance.

The paper is structured as follows. Section 2 discusses related literature and institutional background of the minimum legal drinking age laws in Spain. Section 3 outlines our estimation design and describes the main datasets (PISA and ESTUDES). Section 4 presents the main results and Section 5 discusses and concludes.

2 Background

2.1 Related Literature

Previous work on minimum drinking age laws has explored their impact on educational outcomes, particularly in relation to school performance, dropout rates, and long-term attainment. Stricter drinking age laws have been found to reduce alcohol consumption among young people, leading to improvements in cognitive function, classroom behaviour, and academic performance. Beyond immediate academic outcomes, early alcohol exposure can negatively affects college attendance and labour market prospect. Cook and Moore (1993) linked adolescent drinking to lower university attendance and reduced labour market prospects, highlighting the lasting effects of early substance use.

Employing a regression discontinuity design to the context of the United States, where the minimum drinking age is 21, delaying access to alcohol decreases high school dropout rates, increases college enrolment and (Carpenter and Dobkin, 2009) and academic performance (Carrell et al., 2011). Similarly, Lindo et al. (2016) found that changes in drinking age laws in Australia led to short-term reductions in alcohol-related school absences and improved exam performance.

Some studies also emphasise the role of peer effects, showing that when legal drinking ages are lowered, alcohol consumption increases not only among those directly affected but also among younger students within peer networks, amplifying the negative impact on education (Dee and Evans, 2003). Overall, these findings provide strong evidence that raising the minimum drinking age can serve as an effective policy tool to promote better educational and social outcomes.

There are also gender differences in how minimum drinking age laws affect educational outcomes. Generally, the negative impact of alcohol consumption on education appears to be stronger for men than for women, particularly in terms of exam performance, school completion, and dropout rates. For example, Lindo et al. (2016) found that the adverse effects of alcohol consumption on academic achievement in Australia were more pronounced among young men, who experienced larger declines in exam performance when given legal access to alcohol. Similarly, Carpenter and Dobkin (2009) reported that in the United States, the increase in mortality and risky behaviour associated with turning 21 was much higher for men, suggesting they are more susceptible to the negative consequences of early alcohol consumption. In addition, negative effect of drinking on educational attainment is found to be stronger for men, with a significant decline in the likelihood of completing high school and enrolling in college (Dee and Evans, 2003).

The effects of drinking age law on women tend to be smaller or less consistent. One reason is that women are less likely to engage in heavy binge drinking compared to men, which may partly explain the smaller impact on their education. However, Cook and Moore (1993) show that women who drink heavily during adolescence are more likely to experience interruptions in their education, though the effect was still weaker than for men.

Aside the direct effect on drinking age laws on alcohol consumption, early access to alcohol can also pose additional effects on other risky behaviours - acting as a gateway to other drug use and smoking. Lowering the minimum drinking age increases both smoking, drug use, and binge drinking among teenagers (Dee, 2001; Deza, 2015; Ahammer et al., 2022). Evidence on how alcohol consumption and cannabis use is related is less clear-cut. Whilst some studies (Carpenter and Dobkin, 2009; Wen et al., 2015) show that both activities are complementary, others present evidence that they are substitutes (Crost and Guerrero, 2012; Deza, 2015) or are even statistically unrelated (Crost and Rees, 2013).

Broadly, the gateway effect is more profound among men than women in. Dee and Evans (2003) show that men are more likely to engage in binge drinking and polydrug use when exposed to early alcohol access. On the other hand, women still exhibited a rise in risky behaviours, albeit to a lesser extent. Nevertheless, past work suggests that women tended to exhibit higher levels of alcohol dependence when they started drinking at younger ages, which correlated with increased smoking and drug use (Cook and Moore, 1993). Moreover, Crost and Guerrero (2012) find that the substitution effect between marijuana and alcohol is stronger among women.

2.2 Minimum Legal Drinking Law in Spain

Prior to the reform that increased the minimum legal drinking age (MLDA) to 18 years across Spain, the legal framework regarding alcohol consumption varied significantly across autonomous communities. Historically, the national legal drinking age was set at 16 years (in 1982), allowing minors to consume alcoholic beverages with lower alcohol content, such as beer and wine, while stronger alcoholic drinks remained restricted until adulthood. This regulatory framework reflected a cultural acceptance of moderate alcohol consumption among young individuals, particularly in social and familial contexts. However, concerns over rising alcohol consumption among adolescents and its associated social and health consequences led to legislative efforts aimed at standardizing and tightening regulations across the country.

The push for reform to lift the MLDA started in Catalonia in 1987 but only gained momentum in the late 1990s and early 2000s, driven by public health campaigns and growing evidence linking early alcohol exposure to negative long-term outcomes. As a result, several regions (or referred to as *autonomous communities*) undertook legislative changes to align their drinking age policies with the European standard of 18 years.[5] Three regions that implemented these reforms much later were Castilla-León, Galicia, and Asturias. Each adopted the new MLDA at different points in time and with varying enforcement mechanisms.

In Castilla-León, the reform was introduced on March 15, 2007, through a modification of Law 3/1994 via Law 3/2007. Under the previous regulatory framework, individuals aged 16 to 18 were legally allowed to consume alcoholic beverages with an alcohol content of less than 18 degrees. The revised legislation prohibited all alcoholic beverages for those under 18 years and introduced stringent enforcement measures, with penalties ranging from 601 to 10,000 euros for non-compliance. This policy change was accompanied by increased public awareness campaigns aimed at reducing adolescent alcohol consumption and ensuring compliance with the new restrictions.

Galicia followed suit on February 28, 2011, with the enactment of Law 11/2010, which

[5]Appendix Tables A.13 and A.14 summarise the timing of the MLDA reform of each region, along with its corresponding availability of the waves of both PISA and ESTUDES data.

implicitly modified the provisions of Law 2/1996. Similar to Castilla-León, the previous Galician legislation permitted 16 to 18-year-olds to consume alcoholic beverages with lower alcohol content. The new legal framework introduced a blanket prohibition on alcohol consumption for individuals under 18 years, imposing fines ranging from 3,005 to 15,025 euros for violations. The implementation of this reform coincided with broader initiatives aimed at reducing youth drinking, including educational programs and community-based interventions.

Asturias, in contrast, introduced the reform at a later stage, with Law 4/2015 coming into effect on May 20, 2015. Unlike Castilla-León and Galicia, Asturias' previous legislation had allowed individuals aged 16 to 18 to consume any type of alcoholic beverage. The reform imposed a complete ban on alcohol consumption for individuals under 18 years, with penalties structured based on the type of beverage consumed. Occasional consumption of low-alcohol beverages incurred fines of up to 3,000 euros, while stronger drinks with an alcohol content exceeding 23 degrees could result in penalties of up to 15,025 euros. This phased approach allowed for a gradual transition and adaptation of enforcement strategies to the new regulatory framework.

While the overarching goal of these regional reforms was to raise the MLDA to 18 years and curb underage drinking, their implementation reflected important differences. Castilla-León and Galicia both had similar previous legal frameworks that allowed moderate consumption for minors, whereas Asturias had no such restrictions, making the transition more pronounced. Additionally, the timeline of implementation varied significantly, with Castilla-León leading the effort in 2007, followed by Galicia in 2011, and Asturias adopting the reform in 2015. Differences in enforcement approaches were also evident, with varying penalty structures and levels of stringency across regions.

3 Empirical Approach

3.1 Empirical Specification: Difference-in-Discontinuities Design

Estimating the impact of the MLDA is complex. The decision of individuals to consume alcohol at a specific age is influenced by various personal, peer, and environmental factors, many of which are unobserved by the researchers.[6] In line with prior work in the literature

[6]This includes not only the non-random nature of an individual's drinking habits, but also other characteristics such as their social environment, peer influence, cultural attitudes towards drinking, or socioeconomic background, which can affect both their likelihood of consuming alcohol and the potential consequences of such consumption. Similarly, contextual factors such as local enforcement policies, avail-

(e.g., Carpenter and Dobkin, 2009; Crost and Rees, 2013; Deza, 2015; Lindo et al., 2016; Ahammer et al., 2022), our initial empirical approach begins by leveraging a RD design that exploits the discontinuity in legal access to alcohol for individuals whose age (in months) is just above or below the MLDA threshold, in conjunction with a legal reform that postponed the MLDA progressively in these three Spanish regions. The RD design would allow us to estimate the causal effect of legal alcohol access by comparing individuals who have just reached the legal drinking age to those who are just below it, under the identifying assumption that near the age threshold, individuals are comparable in all relevant aspects except for their legal access to alcohol. That is, no other factors, aside from the MLDA restriction, experience a discontinuous change around the 16th birthday.[7]

Therefore, to account for potential violations of this assumption, our main estimation strategy exploits the timing of the MLDA reform and extend our RD design to estimate differences around the minimum drinking age threshold, before and after the introduction of stricter MLDA regulations. With this refinement, our difference-in-discontinuities design (diff-in-disc) allows us to account for unobserved confounding factors and enhance the credibility of our estimates.[8]

$$
\begin{aligned}
Y_{it} = &\alpha_1 \cdot 1\{b_i \geq 0\} + \alpha_2 \cdot 1\{b_i \geq 0\} \cdot post_t + g(b_i) \\
&+ \gamma_s + \eta_{st} + \beta X_i + v_{it}
\end{aligned}
\tag{1}
$$

where Y_{it} represents the outcomes of individual i in survey year t. In our main estimation, this is the standardised PISA test scores. Recall that the MLDA is 16 years old in the old regime. Therefore, the indicator function $1\{.\}$ equals 1 if the individual was exactly 16 years old or older, $b_i \geq 0$, at the time of the survey, and 0 otherwise, capturing the effect of legal access to alcohol. For our estimation, this is the age by months at the time

ability of alcohol, or public awareness campaigns may also influence individuals' decisions regarding alcohol consumption and its related outcomes.

[7]The assumption of continuity in unobserved factors is challenged in our context. Turning 16 coincides with several legal milestones, such as eligibility for employment (BOE-A-2015-11430 Articles 6 and 7), obtaining a moped license, and partial emancipation, all of which may independently influence drinking behaviour beyond the MLDA. Educational transitions to higher education or vocational training introduce new social environments that can also impact alcohol consumption. Additionally, cultural norms and social expectations promote greater autonomy and nightlife participation, increasing exposure to alcohol. Reduced parental supervision at this age further complicates the analysis. Businesses may target individuals nearing the legal threshold with marketing efforts, while regional variations in law enforcement could create inconsistencies. Peer influence from older individuals and anticipatory behaviours may also lead to changes before reaching the legal drinking age. These factors highlight the complexity of isolating the causal effect of the MLDA and the need for careful interpretation of RD estimates.

[8]Some examples of past works that exploit a difference-in-discontinuities design strategy are Grembi et al. (2016) and Chicoine (2017).

of the PISA assessment.[9] The variable $post_t$ is a dummy that equals 1 for periods after the implementation of stricter MLDA regulations and 0 otherwise, allowing us to assess changes over time. Finally, v_{it} represents the idiosyncratic error term.

The coefficient α_1 measures the difference in the outcome before the MLDA reform (i.e., PISA scores) between teenagers who were 16 years or older and those who had not yet reached the MLDA age.[10] The coefficient of the interaction term, α_2, captures the change in this difference following the introduction of stricter legal drinking age regulations. This is our parameter of interest - identifying the causal effect of an increase in the MLDA on the population who used to have legal access to alcohol. The function $g(b_i)$ is a polynomial specification of the running variable designed to account for potential non-linear age effects on academic outcomes where we allow the slope terms to be different on each side of the threshold (Buckles and Hungerman, 2013).

The running variable b_i represents the number of months before or after the student's sixteenth birthday at the time of the test (for PISA) or of the survey (for ESTUDES). The vector X_i includes individual-level control variables. The terms γ_s and η_{st} correspond to region (i.e., autonomous community) fixed effects and survey-year fixed effects, respectively, with the latter allowing for variation across regions to account for regional differences in educational policies and other unobserved factors.

Following Calonico et al. (2014) and Calonico et al. (2017), we consider two data-driven bandwidths for each of the outcomes. The first method balances the bias-variance trade-off by minimising the Mean Squared Error (MSE) of the local polynomial RD point estimator, referred to as the MSE-optimal bandwidth. The second method, the MSE-bias-corrected bandwidth, explicitly accounts for bias that can arise from the local polynomial approximation, particularly when the bandwidth is large.[11] Based on these findings, we report results using alternative bandwidths of 2 and 5 months. For the 2-month bandwidth, we employ a linear specification for $g(.)$, while for the broader 5-month bandwidth, we adopt a quadratic specification as a conservative approach. To conserve space, the main results

[9] In our estimations with ESTUDES, this is the age by months at the time of the survey.

[10] Following our earlier discussion, this is the causal parameter that a conventional RD design would estimate. However, it may yield biased estimates if other regulations besides the MLDA, which could influence educational performance and risky behaviours, also take effect at age 16. In Spain, several key policy changes indeed occur at this age, including the end of compulsory schooling, the minimum driving age, and the entry age to the labour market. For a similar discussion on identification threats in Austria, see Ahammer et al., 2022.

[11] For the PISA sample, The results for these optimal bandwidths are presented in Appendix Table A.1. On average, the bandwidths range from approximately 3 to 5 months, with the MSE-bias-corrected bandwidth often being larger. This occurs when the bias is already small – such as in cases where a local linear approximation performs well compared to higher-order polynomials – or when variance dominates due to a smaller sample size.

are presented using a 2-month bandwidth with a linear local regression, while the results for the 5-month bandwidth with a quadratic specification are provided in the Appendix as a robustness check.

3.2 Main Datasets

PISA Dataset and Educational Outcomes: The Programme for International Student Assessment (PISA), conducted by the OECD every three years, evaluates the reading, mathematics, and science proficiency of students aged 15 and 16 years old worldwide. The dataset comprises student-level responses alongside school- and country-level information, enabling cross-national comparisons of educational outcomes.[12] A range of variables includes test performance, socioeconomic background, gender, parental education, and school resources.

Spain has participated in all PISA cycles since 2000 but only from the 2003 survey that it is possible to identify the specific region of residence (autonomous communities) of the student participants. Our analysis focuses on the three regions that reformed their MLDA in later dates, and therefore there are available waves of PISA that correspond to the period before as well as after its reform. These are Castilla-León (2007), Galicia (2011), and Asturias (2015). Appendix Table A.13 lists the availability of PISA data in each Spanish region, and group the survey waves into the pre- and post-reform waves.[13]

A crucial variable to our estimation design is the availability of age in months when students participated in the survey, including their month of birth and year of birth. With this, we have a rather precise cut-off month before and after a student turns 16 (the minimum drinking age of the MLDA before the reform) when they take the test.

The main educational outcomes in PISA are students' test scores in maths, science, and reading (Spanish language). We convert the so-called plausible values of each test into a region-specific standardised score. This is to account for the changes in the regional composition across PISA waves that may, by construction, contaminate the comparison of students from the same region over the years. Aside the test scores, PISA also elicit selected behavioural measurements that may be influenced indirectly by the raising of the MLDA. Specifically, across the waves, we construct consistent measures (self-evaluated) of

[12] PISA microdata can be obtained for free from the OECD website after completing a registration process and agreeing to the terms of use for research purposes. For more detail, see https://www.oecd.org/en/about/programmes/pisa.

[13] Given that the 2000 wave does not have a region identifier, our analysis therefore uses the PISA waves of 2003, 2006, 2009, 2012, 2015, 2018, and 2022.

'sociability' and 'school truant'.[14]

Data on alcohol consumption and drug use from ESTUDES: The ESTUDES survey (Encuesta sobre Uso de Drogas en Enseñanzas Secundarias en España) is a national study conducted by the Spanish Government's Delegation for the National Plan on Drugs (PNSD) and Ministry of Health to assess substance use among secondary school students. It is carried out biennially since 2004 to 2021, with the main targets students aged 14 to 18.[15] Our empirical analysis will focus on students aged 15 and 16, of whom the information on their age by months as well as their month of birth and year of birth is available. As before, we exploit only the sample of students from the three autonomous communities (Castilla-León, Galicia, Asturias) that reformed their MLDA after 2000.[16]

Specifically, we construct the following groups of variables, which are available consistently in all waves to gauge individual's own evaluation of their alcohol consumption and other drug use, namely: (i) alcohol consumption (ever drink, drink in the past month, drink regularly), (ii) intoxication (ever intoxicated, intoxicated in the past month, frequency of intoxication in the past month), (iii) tobacco (ever smoke, smoke in the past month, quantity of tobacco smoked daily), (iv) marijuana consumption (ever take marijuana, take in the past year, take in the past month, taken marijuana by the age 16 years old).[17]

Risky behaviours of peer group in ESTUDES: Furthermore, we can construct additional consistent measures (self-evaluated) from ESTUDES that gauge the degree of risky behaviours of the peer groups (with regard to their consumption of alcohol, tobacco and some other illicit drugs). Each respondent is asked to provide an estimation of the share of their peer group (namely, none, some, almost all) engaging in each of these activities.

[14] See Appendix Table A.15 for the detail of each variable in PISA survey waves.

[15] For more information, see https://pnsd.sanidad.gob.es. Secondary school students include those in ESO (compulsory secondary education), Bachillerato (final 2 years of high school), and vocational training. The survey is administered anonymously in schools and provides valuable data on the consumption of alcohol, tobacco, cannabis, and other substances, as well as students' perceptions of drug use and the availability of these substances. Beyond substance use, ESTUDES also explores other risk behaviours, peer groups, family relationship, and attitudes. Access to micro-level data of ESTUDES is possible through a formal request to La Delegación del Gobierno para el Plan Nacional sobre Drogas at the Spanish Ministry of Health.

[16] See Appendix Table A.16 for the information on the ESTUDES waves and Appendix Table A.14 for the corresponding grouping of each survey wave with respect to the timing of the change of MLDA in each region.

[17] See Appendix Table A.16 for more details. Our variables follow standard measures of risky behaviours in the literature (e.g., Carpenter and Dobkin, 2011.)

4 Empirical Results

4.1 Validating Identifying Assumptions

Our analysis relies on the assumption that the observed change around age 16, between the periods before and after the reform delaying the legal drinking age, is solely attributable to the policy change and not to other unobserved factors. While this assumption is not directly testable, we assess its plausibility by examining predetermined individual covariates to ensure they behave smoothly around the cut-off.

Appendix Tables A.3 and A.5 examine the balance of these characteristics around the age of 16 for PISA (Tables A.2 and A.3) and ESTUDES (Tables A.4 and A.5). For each data source, we test two hypotheses and report the corresponding p-values. First, we test the null hypothesis of no differences in the characteristics of individuals around the age threshold - either before or after the reform. See Appendix Tables A.2 and A.4 for the results of PISA and ESTUDES, respectively. Next, we allow that individuals around the threshold age can be different in the period before whilst we test a null hypothesis of no change in these group differences between the pre- and post- periods. We report the corresponding p-values of the second test in Appendix Tables A.3 and A.5. Note that each table includes different polynomial specifications of the running variable, and for each specification, we also explore alternative bandwidths. The results confirm that our sample presents a variation that is consistent with a quasi-experimental design. In detail, for the closest bandwidth allowed by each polynomial specification, we fail to reject the null hypothesis for almost all variables, except for the foreign-born dummy in the PISA dataset. In the case of ESTUDES, while some differences in this proportion are detected over the entire period (Appendix Table A.4), we cannot reject the second null hypothesis and can conclude that this difference remains stable over time.

The second identifying assumption of our design concerns the perfect manipulation of the running variable McCrary (2008), as in a RD design. Evidence on bunching of observations around the cut-off would suggest some degree of manipulation. Panel A of Appendix Figure A.1 shows the raw histogram for the running variable (months around the sixteenth birthday) with no apparent bunching. However, we formally test for manipulation based on the running variable around the cut-off. We follow the approach of Cattaneo et al. (2018) to formally test for manipulation.[18] Using a data-driven bandwidth selection and a c.d.f. restricted to be equal on both sides of the threshold, we fail to reject the null

[18]The test is implemented using the `rddensity` command in `STATA`.

hypothesis of continuity, with p-values of 0.88, 0.81, and 0.71 for polynomial specifications of degree one, two (default), and three, respectively (Panels B-D in the figure).

4.2 MLDA and Test Scores: Evidence from PISA

Figure 1 and Appendix Figure A.2 present the graphical illustrations of our main educational outcomes for linear and quadratic specification of $g(.)$, respectively. Each row reports the score of each PISA test subject (maths, reading, science) around the cut-off (16 years old); and the columns represent the findings of each period (i.e. before and after the MLDA reform). Three key observations can be made. First, regardless of the polynomial specification, we observe an increase in the discontinuity after the reform, which is consistent with a positive effect of the policy change associated with raising the legal drinking age. This suggests that limiting access to alcohol of 16-year-old teens had a favourable impact on their academic performances. Second, in the period before the reform, the effect of reaching the legal drinking age (i.e. being above the cut-off age of 16 years old) on PISA scores varies depending on the degree of $g(.)$. Specifically, under the linear specification shown in Figure 1, reaching the drinking age appears to have a positive effect on performance in the pre-reform period (left column). However, when using a quadratic specification (Appendix Figure A.2), this effect becomes statistically non-existent for mathematics and turns negative for reading and science subjects. Finally, after the reform (right column), we consistently observe a positive difference in PISA scores for students who had reached their sixteenth birthday, regardless of whether a linear or quadratic specification is used for $g(.)$.

Next, we summarise the key estimates of Equation 1 in Table 1 and Appendix Table A.6, using two selected bandwidths (2 and 5 months). Each panel reports the estimates for maths, reading, and science, respectively (measured in standardised value). Column (1) reports the estimates for the full sample. Columns (2) and (3) report the heterogeneity by gender while columns (4) and (5) show the analysis by parent's educational level (with and without university degree). For brevity, we report only the estimate of the effect of reaching the legal drinking age before the reform (α_1), labelled as 'Above the threshold', and the change in this effect after the reform (α_2), labelled as 'Above X post MLDA'.

The first observation from the full sample is that, before the increase in the legal drinking age, reaching this age did not have a significant effect on any of the three subjects. Second, the raising of the minimum drinking age from 16 to 18 years old in the new regime is associated with an improvement in PISA scores ranging from 0.07 to 0.90 standard deviations (s.d.) in our most conservative specification (Table 1) – although this effect is only statistically significant for science in the estimation with the 2-month bandwidth (0.90 s.d.). When the bandwidth is expanded to 5 months around the threshold, we observe a significant increase in test scores across all subjects, ranging from 0.11 s.d. in reading to 0.17 s.d. in science.

4.3 Heterogenous Effects of MLDA on Test Scores

Gender: Columns (2) and (3) in Tables 1 and A.6 present the analysis by gender. Regardless of the specification, the results clearly indicate that the positive impact of the increase in the MLDA is primarily driven by male students. In our most conservative specification (Table 1), male students experience an increase of approximately 0.2 s.d. in maths and science scores, and an increase of 0.15 s.d. in reading. In contrast, for female students, the MLDA reform is associated with a decrease of approximately 0.05 s.d. in reading (but non significant for a bandwidth of 5 months, Table A.6).

Socioeconomic status: Finally, columns (4) and (5) present the heterogeneity analysis in different socioeconomic backgrounds (defined by parental education). The positive impact of the reform is concentrated among students whose parents have lower educational attainment. In our most conservative specification, we observe an increase of 0.16 s.d. in maths. Moreover, when using a bandwidth of 5 months, the results show a significant increase of approximately 0.25 s.d. across all subjects. In contrast, for students with highly educated parents, the effect is not only insignificant but also remains negligible across different specifications – reinforcing the robustness of the findings.

4.4 Student's Schooling Behaviours: Evidence from PISA

Given the larger impact observed among male students and those with less educated parents, it is crucial to assess whether the reform influenced school-related behaviours that could mediate its effects on academic performance. Therefore, to better understand the mechanisms driving the observed effects of delaying the legal drinking age, we explore whether the policy led to changes in students' school engagement and own sociability in school.

Following a similar structure, Table 2 (and Appendix Table A.7 for a 5-month bandwidth) presents the impact on a sociability index and three indicators related to truant, namely, skipping school, skipping classes, and arriving late to school. For the full sample, the reform led to a reduction of approximately 20 pp. in the probability of skipping school or classes. While a decline of 14 pp. is observed among female students, the reduction is nearly twice as large for male students, who experience a 27 pp. decrease in the likelihood of skipping classes. However, the impact on the probability of skipping school is twice as large for female students, with a reduction of 24 pp. The analysis by parental education level reveals that the reform in MLDA is associated with improvements in school attendance across all measures, particularly benefiting students with more educated parents.

Overall, these measures in PISA data on school-related behaviours provide suggestive evidence that the restriction of alcohol access to 16-year-old teenagers under the new MLDA regime are related to an improvement in school engagement - especially among the demographic groups who are found to also experienced better test scores. In the next step, we will explore further if the policy directly led to a change in alcohol consumption as well as potential spillovers into other related risky behaviours - of the individuals themselves and their peer groups.

4.5 MLDA and Own Risky Behaviours

So far, the findings in the previous section provide partial insight into the larger positive effect of the reform among male students, primarily through behavioural changes in school attendance. However, they do not fully account for the more pronounced impact observed in PISA scores among students with less educated parents, suggesting that additional mechanisms may be at play.

In this section, we aim to explore two potential channels through which delaying the legal drinking age may have influenced academic outcomes. First, we assess whether the reform had an impact on own alcohol consumption and other risk-related behaviours commonly associated with alcohol use, such as tobacco and substance consumption. Second, we examine whether these behavioural changes exhibit heterogeneous effects aligned with the patterns observed in PISA scores. By identifying these underlying mechanisms, we seek to provide a more comprehensive understanding of how restricting early alcohol access translates into educational gains, particularly for students from different socioeconomic backgrounds.

Tables 3 and 4 (Tables A.8 and A.9 for the 5-month bandwidth in the Appendix) report the impact of the reform on various outcomes related to individual drinking behaviour. The first three outcomes in Table 3 aim to measure the extensive margin of drinking through the indicators: 'Ever drank alcohol', 'Drank in the last month', and 'Drinks weekly'. The results for the full sample reveal a significant reduction in the probability of drinking, estimated at approximately 4 pp.

The analysis by gender indicates that this effect is primarily driven by young men, who experience a 6.6 pp. reduction in the probability of ever having consumed alcohol. In contrast, for young women, the effect is not statistically significant, and the magnitude of the estimate is less than one-third of that observed for males. The analysis by parental education level shows a significant effect for both groups, but the probability of ever drinking declines by almost twice as much among individuals with less educated parents (10 pp. versus 5 pp. for those with more educated parents).

Table 4 presents the impact of the reform on intoxication-related outcomes. The first notable observation is that for the full sample, there is no significant impact on the probability of ever being intoxicated. However, the effect emerges when considering the frequency of intoxication events. Specifically, we observe a reduction of approximately 7.5 pp. in the probability of intoxication in the last month, as well as a decline in the number of intoxication episodes (Panel C).

The analysis by gender suggests that the reduction in intoxication events is driven by young women, who experience an approximately 10 pp. reduction in the probability of being intoxicated in the last month. These findings suggest that the reform affects two distinct aspects of alcohol consumption: while young men are more likely to delay their first drink, young women do not significantly reduce their likelihood of drinking but instead exhibit a decline in the intensity of alcohol use.

The results for tobacco (Table 5) and marijuana (Table 6) closely mirror the findings for the extensive margins of alcohol consumption. The increase in the MLDA reduced the probability of ever smoking for males, lowering their likelihood of ever smoking tobacco and marijuana by approximately 8.5 and 4.3 pp., respectively. In contrast, no significant effects were found for young females. Additionally, the probability of ever smoking declined by 10 pp. among individuals with less-educated parents.

Several hypotheses could explain these patterns. First, the reform likely influenced only those who had not yet started consuming alcohol. If girls are more likely to have older peers who had already begun drinking, the reform may have been less effective for them, as their drinking opportunities remained largely unchanged. Second, the effectiveness of the reform may depend on the extent to which peers are also affected. If a greater proportion of an individual's peer group faces increased restrictions, the reform is more likely to alter consumption behaviour. In fact, various studies on Spain highlight that the ease of access to alcohol is a key factor driving excessive drinking (e.g., Villalbí et al., 2014; Sureda et al., 2018; Villalbí et al., 2019). Combined with the well-documented positive gradient in alcohol consumption by socioeconomic gradient (e.g., Donat et al., 2022; Karlamangla et al., 2006; Grittner et al., 2013; Elgar et al., 2005), the reform would be more effective among individuals with lower parental education, as they may be more exposed to restricted peer networks.

To further investigate these potential mechanisms, in the next section, we examine the extent to which the reform influenced consumption of alcohol and related substance within peer groups and shed light on the role of peer networks in shaping drinking behaviours.

4.6 MLDA and Peer Group

Table 7 (and Appendix Table A.12, using a 5-month bandwidth) presents the impact of the reform on the risky behaviours of peers, as reported by respondents. The first key finding is that the reform led to a reduction in the reported frequency of drinking among friends. This result is consistent with the idea that peers play a crucial role in shaping individual consumption patterns. Notably, the impact is stronger among peers of young males and individuals with less-educated parents, reinforcing the hypothesis that the effectiveness of the reform depends on the extent to which one's social network is affected. The only exception is for intoxication, where the observed reduction in frequency is similar across genders.

The second key finding is that the MLDA reform is also associated with a significant decline in the perceived frequency of tobacco and marijuana consumption among friends—by approximately 12 pp. As with alcohol consumption, this effect is more pronounced among young men and individuals from lower-educated families. This pattern suggests that the reform not only influenced alcohol use but also had spillover effects on other risky behaviors within peer groups, particularly among those who were more exposed to the constraints imposed by the policy.

Finally, the reform is linked to a decrease in the reported frequency of cocaine consumption among peers. While this effect is primarily driven by individuals with less-educated parents, it is particularly striking that young women report a decline in the frequency of cocaine use within their social circles. This finding aligns with the broader pattern observed in the subsample of females, where the reform appears to have had a stronger impact on extreme drinking and the use of more serious substances. These results suggest that while peers mediate the impact of the MLDA reform across all groups, the nature of peer networks may differ between genders, potentially explaining the observed heterogeneity in responses to the policy.

5 Discussion and Concluding Remark

This study provides new insights into the fact that increasing the Minimum Legal Drinking Age (MLDA) improved academic performance of teenagers, particularly among young men and those of lower socioeconomic backgrounds. The reform, which lifted the MLDA from age 16 to 18 years old, led to significant gains in PISA test scores among teens who no longer have a legal access to alcohol, especially in maths and science, with improvements ranging from 0.07 to 0.17 standard deviations. These effects align closely with the estimated reductions in alcohol consumption, which were concentrated among the same groups. Specifically, young men experienced a 6.6 pp. reduction in the probability of ever having consumed alcohol, while students with less-educated parents saw an even larger 10 pp. decline. The similarity in the patterns across educational outcomes and drinking behaviour suggests that restricting alcohol access had a causal impact on academic performance.

The heterogeneity in academic gains also mirrors reductions in other substance use, further reinforcing the link between substance consumption and school performance. The reform significantly reduced the probability of ever smoking tobacco (by 8.5 pp. for males) and marijuana (by 4.3 pp. for males), with no significant impact observed among females. This pattern suggests that the reduction in alcohol access may have had broader spillover effects, reducing engagement in other risky behaviours that negatively affect school achievement.

A key factor that may explain these effects is the role of peer networks. The reform not only reduced individual consumption but also led to a decline in peer drinking and substance use, particularly among young men and students from lower socioeconomic backgrounds. Given the well-documented influence of peer norms on adolescent decision-making, this suggests that social interactions may have reinforced the policy's effectiveness. The fact that young women did not significantly reduce their drinking but experienced a decline in extreme intoxication and cocaine use among their peers further highlights the potential mediating role of social networks in shaping substance use behaviours.

These findings underscore the importance of considering peer influence when designing policies to reduce adolescent drinking. While increasing the MLDA was effective in improving academic outcomes, its impact was likely amplified by changes in peer behaviour.

Future research should explore whether these effects extend to long-term educational and labour market outcomes, as well as the potential for unintended consequences, such as shifts toward alternative substances or social behaviours.

Overall, this study highlights the complex interplay between policy, substance use, and peer networks. Effective interventions should not only focus on restricting alcohol access but also consider peer dynamics and broader behavioural spillovers to maximize educational and societal benefits.

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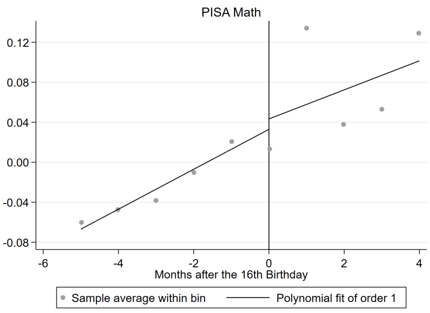
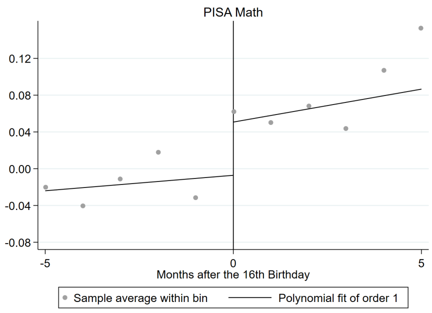
6 Tables and Figures



(a) Maths before MLDA



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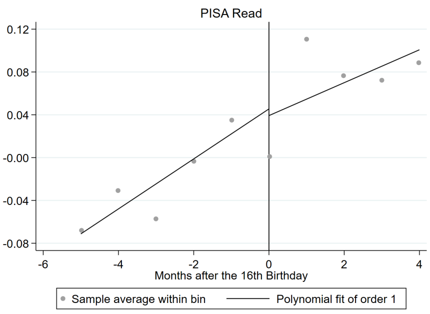
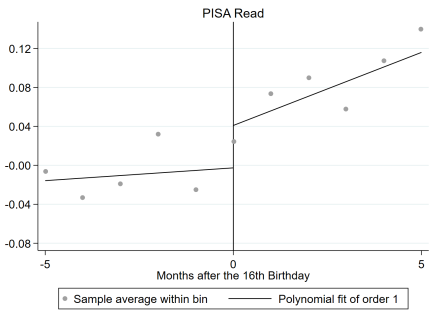


(c) Reading before MLDA



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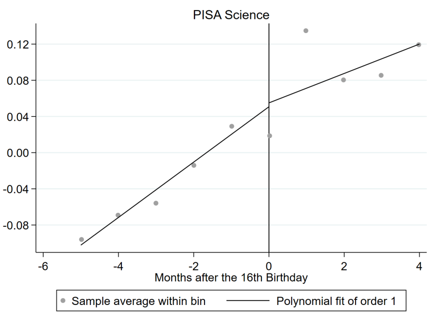
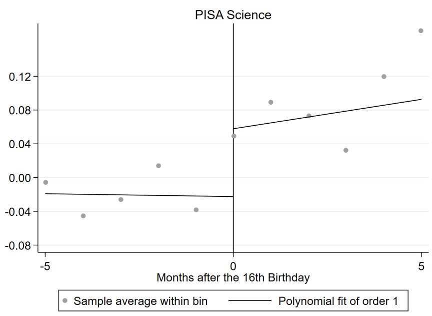


(e) Science before MLDA



(f) Science after MLDA

Figure 1: PISA score discontinuity around the age of 16: Left panels show scores before the introduction of MLDA, while right panels display scores after the reform. Local linear specification

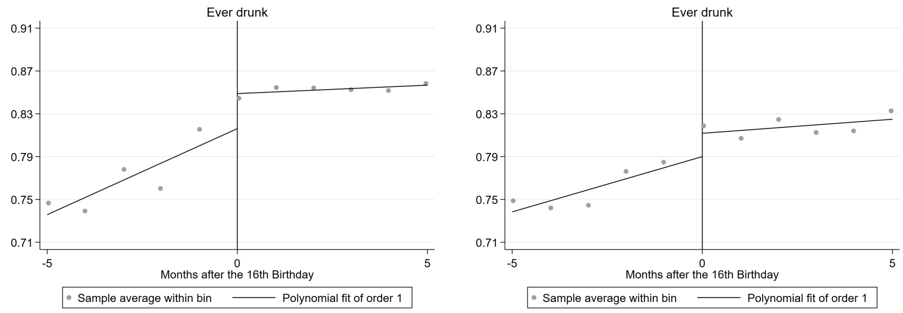


Figure 2: ESTUDES discontinuity in the fraction of individuals who ever drunk around the age of 16: Left panel fraction before the introduction of MLDA, while right panel after the reform. Local linear specification

Table 1: Impact of MLDA on selected PISA scores. (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Maths (std)					
Above threshold	0.026	0.113*	-0.058	-0.004	0.098**
	(0.040)	(0.047)	(0.032)	(0.048)	(0.025)
Above X post MLDA	0.071	-0.058	0.193**	0.155**	-0.029
	(0.044)	(0.046)	(0.053)	(0.052)	(0.041)
Obs.	14974	7489	7485	7316	7658
Panel B: Reading (std)					
Above threshold	-0.017	0.050**	-0.070***	-0.046	0.042***
	(0.019)	(0.017)	(0.017)	(0.030)	(0.009)
Above X post MLDA	0.058	-0.049**	0.145**	0.114	-0.010
	(0.033)	(0.015)	(0.054)	(0.058)	(0.023)
Obs	14974	7489	7485	7316	7658
Panel C: Science (std)					
Above threshold	0.001	0.066	-0.055**	-0.011	0.040
	(0.030)	(0.039)	(0.021)	(0.032)	(0.031)
Above X post MLDA	0.093**	-0.037	0.210***	0.110	0.062
	(0.024)	(0.028)	(0.048)	(0.056)	(0.034)
Obs	14974	7489	7485	7316	7658

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the PISA wave years, fixed effects for regions, and parental education. Student characteristics are allowed to have effects that vary across regions. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table 2: Impact of MLDA on selected behavioural outcomes. (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Sociability index					
Above threshold	0.000	-0.017	0.019	-0.002	0.003
	(0.008)	(0.014)	(0.016)	(0.011)	(0.021)
Above X post MLDA	-0.086**	0.067**	-0.233***	-0.079	-0.098***
	(0.023)	(0.018)	(0.042)	(0.052)	(0.019)
Obs	9431	4764	4667	3577	5854
Panel B: Truant - Skip school					
Above threshold	0.203**	0.114**	0.268**	0.128	0.256**
	(0.064)	(0.037)	(0.079)	(0.070)	(0.069)
Above X post MLDA	-0.219**	-0.142**	-0.271**	-0.228**	-0.221**
	(0.065)	(0.044)	(0.075)	(0.063)	(0.068)
Obs	9251	4664	4587	3484	5767
Panel C: Truant - Skip classes					
Above threshold	0.160***	0.220***	0.097*	0.040	0.304***
	(0.010)	(0.043)	(0.040)	(0.049)	(0.041)
Above X post MLDA	-0.179***	-0.230***	-0.122*	-0.108	-0.291***
	(0.018)	(0.041)	(0.054)	(0.058)	(0.041)
Obs	9254	4671	4583	3485	5769
Panel D: Truant - Late for school					
Above threshold	-0.149**	-0.171	-0.105	-0.264**	0.193*
	(0.053)	(0.104)	(0.096)	(0.072)	(0.093)
Above X post MLDA	0.064	0.078	0.023	0.146	-0.264**
	(0.069)	(0.103)	(0.123)	(0.088)	(0.095)
Obs	7604	3859	3745	2554	5050

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the PISA wave years, fixed effects for regions, and parental education. Student characteristics are allowed to have effects that vary across regions. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table 3: Impact of MLDA on selected ESTUDES outcomes (own drinking). (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever drink alcohol					
Above threshold	0.031	-0.012	0.068	0.118***	-0.084
	(0.020)	(0.021)	(0.034)	(0.017)	(0.049)
Above X post MLDA	-0.038**	-0.017	-0.066***	-0.103***	0.051**
	(0.012)	(0.039)	(0.013)	(0.023)	(0.020)
Obs	3604	1809	1795	2047	1557
Panel B: Drank in the past month					
Above threshold	-0.071**	-0.049*	-0.086**	-0.125***	0.009
	(0.021)	(0.021)	(0.030)	(0.024)	(0.019)
Above X post MLDA	0.011	0.006	0.016	0.035	-0.038
	(0.026)	(0.024)	(0.038)	(0.032)	(0.043)
Obs	3222	1613	1609	1818	1404
Panel C: Drink weekly					
Above threshold	0.060	0.010	0.112**	0.026	0.106**
	(0.034)	(0.035)	(0.041)	(0.036)	(0.032)
Above X post MLDA	-0.036	0.005	-0.069	-0.008	-0.098**
	(0.025)	(0.035)	(0.035)	(0.028)	(0.037)
Obs	3013	1530	1483	1691	1322

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table 4: Impact of MLDA on selected ESTUDES outcomes (intoxication). (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever been intoxicated					
Above threshold	-0.059***	-0.120***	-0.024	-0.088**	-0.021
	(0.013)	(0.012)	(0.022)	(0.022)	(0.018)
Above X post MLDA	-0.021	0.018	-0.036	0.045	-0.113**
	(0.042)	(0.021)	(0.061)	(0.052)	(0.042)
Obs	3522	1771	1751	1999	1523
Panel B: Intoxication in the last month					
Above threshold	0.053**	0.019	0.079***	0.095***	-0.009
	(0.018)	(0.027)	(0.017)	(0.018)	(0.066)
Above X post MLDA	-0.076***	-0.104**	-0.028	-0.084**	-0.055
	(0.017)	(0.038)	(0.020)	(0.029)	(0.043)
Obs	3256	1629	1627	1838	1418
Panel C: Frequency of intoxication in the last month					
Above threshold	0.175*	-0.001	0.307***	0.225*	0.111
	(0.080)	(0.104)	(0.072)	(0.091)	(0.072)
Above X post MLDA	-0.156**	-0.141*	-0.115	-0.184***	-0.095
	(0.046)	(0.065)	(0.083)	(0.042)	(0.063)
Obs	3160	1581	1579	1778	1382

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table 5: Impact of MLDA on selected ESTUDES outcomes (own tobacco). (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever smoked					
Above threshold	0.012	-0.063**	0.081**	0.011	0.009
	(0.017)	(0.017)	(0.030)	(0.012)	(0.030)
Above X post MLDA	-0.080**	-0.066	-0.085**	-0.107**	-0.047
	(0.028)	(0.053)	(0.026)	(0.034)	(0.048)
Obs	3598	1809	1789	2047	1551
Panel B: Smoked in the last month					
Above threshold	-1.762***	-0.500	-2.973***	-1.241**	-2.364***
	(0.355)	(0.453)	(0.291)	(0.407)	(0.289)
Above X post MLDA	0.041	-0.209	0.321	-0.247	0.277
	(0.615)	(0.373)	(0.883)	(0.494)	(0.651)
Obs	2997	1542	1455	1685	1312
Panel C: No. cigarettes smoked daily					
Above threshold	-0.046	-0.129	-0.008	0.014	-0.122
	(0.145)	(0.186)	(0.178)	(0.073)	(0.244)
Above X post MLDA	0.026	-0.087	0.190	-0.086	0.046
	(0.177)	(0.235)	(0.212)	(0.193)	(0.236)
Obs	2744	1395	1349	1544	1200

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table 6: Impact of MLDA on selected ESTUDES outcomes (own marijuana). (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever took marijuana					
Above threshold	0.053**	0.027	0.071**	0.076**	0.009
	(0.016)	(0.015)	(0.019)	(0.024)	(0.041)
Above X post MLDA	-0.034	-0.011	-0.043**	-0.016	-0.049
	(0.020)	(0.027)	(0.014)	(0.029)	(0.043)
Obs	3561	1793	1768	2024	1537
Panel B: Took marijuana in the last year					
Above threshold	0.046**	0.011	0.077**	0.055***	0.028
	(0.013)	(0.011)	(0.028)	(0.008)	(0.035)
Above X post MLDA	-0.045**	-0.015	-0.062***	-0.033**	-0.066
	(0.017)	(0.029)	(0.015)	(0.010)	(0.043)
Obs	3544	1787	1757	2010	1534
Panel C: Took marijuana in the last month					
Above threshold	0.026	0.047	0.009	0.011	0.039
	(0.014)	(0.029)	(0.028)	(0.034)	(0.022)
Above X post MLDA	-0.048	-0.032	-0.058	-0.025	-0.090*
	(0.045)	(0.043)	(0.052)	(0.056)	(0.038)
Obs	3142	1575	1567	1765	1377
Panel D: Took marijuana first time by age 16					
Above threshold	0.049*	0.029	0.064*	0.067**	0.011
	(0.023)	(0.022)	(0.027)	(0.024)	(0.055)
Above X post MLDA	-0.032	-0.022	-0.029*	-0.022	-0.037
	(0.016)	(0.025)	(0.014)	(0.022)	(0.043)
Obs	3534	1780	1754	2003	1531

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

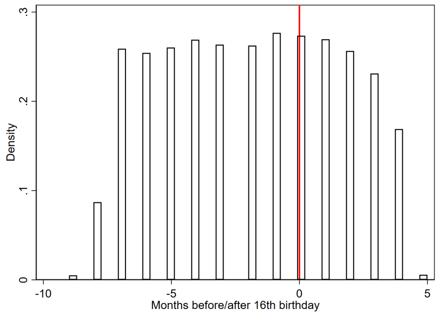
Table 7: Impact of MLDA on selected ESTUDES outcomes (peer risky behaviours). (Two months bandwidth and linear specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Drink alcohol					
Above threshold	0.271***	0.189***	0.353***	0.208***	0.384***
	(0.024)	(0.026)	(0.032)	(0.048)	(0.031)
Above X post MLDA	-0.187***	-0.141**	-0.258***	-0.211***	-0.189**
	(0.025)	(0.047)	(0.029)	(0.033)	(0.061)
Panel B: Smoking					
Above threshold	0.190***	0.091*	0.272***	0.228**	0.154***
	(0.032)	(0.040)	(0.056)	(0.063)	(0.031)
Above X post MLDA	-0.129**	-0.098*	-0.155*	-0.213**	-0.001
	(0.034)	(0.041)	(0.065)	(0.066)	(0.076)
Panel C: Intoxication					
Above threshold	0.246***	0.125***	0.349***	0.210**	0.297**
	(0.021)	(0.028)	(0.043)	(0.063)	(0.092)
Above X post MLDA	-0.184***	-0.175*	-0.173**	-0.234**	-0.109
	(0.021)	(0.073)	(0.066)	(0.065)	(0.102)
Panel D: Take marijuana					
Above threshold	0.217***	0.015	0.392***	0.265***	0.166***
	(0.020)	(0.027)	(0.033)	(0.037)	(0.015)
Above X post MLDA	-0.123***	0.024	-0.237**	-0.153**	-0.079
	(0.027)	(0.041)	(0.064)	(0.044)	(0.042)
Panel E: Take cocaine					
Above threshold	0.084**	0.085	0.074***	0.123***	0.017
	(0.023)	(0.047)	(0.017)	(0.027)	(0.033)
Above X post MLDA	-0.074**	-0.113**	-0.024	-0.096**	-0.036
	(0.023)	(0.030)	(0.034)	(0.025)	(0.053)
Obs	3561	1793	1766	2024	1536

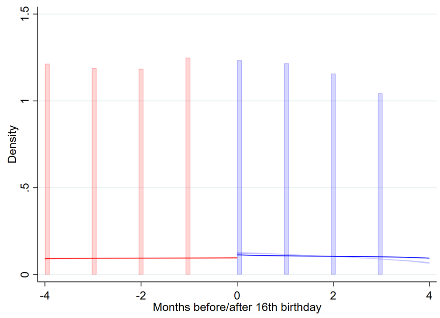
Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

A Appendix

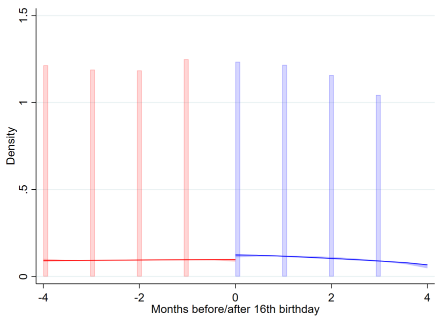
A.1 Validation Tests with PISA and ESTUDES data



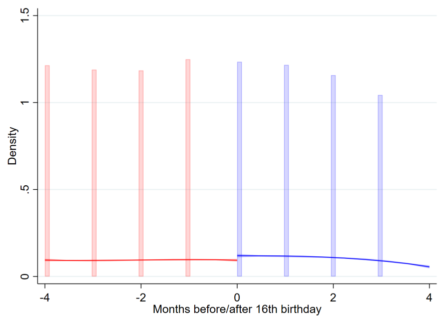
(a) Raw histogram relative age (months) around 16.



(b) Local-polynomial degree 1

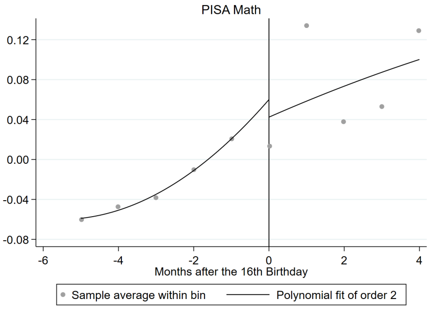


(c) Local-polynomial degree 2

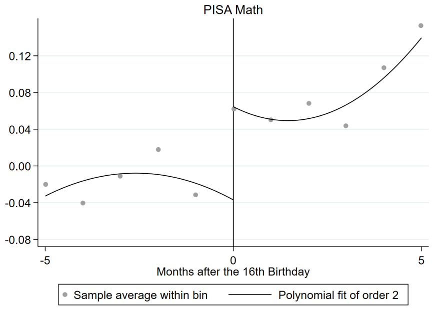


(d) Local-polynomial degree 1

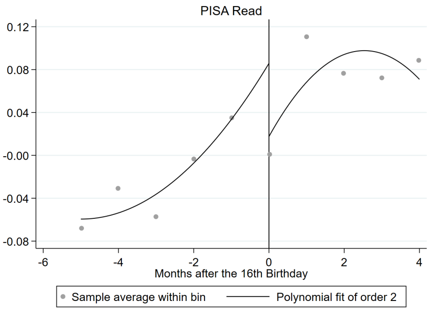
Figure A.1: Panel (a) shows the raw histogram for months distribution regarding the 16th birthday at time of PISA. Panels (a), (b) and (c) display polynomial estimations using a first-degree, second-degree (default), and a third-degree polynomial, respectively.



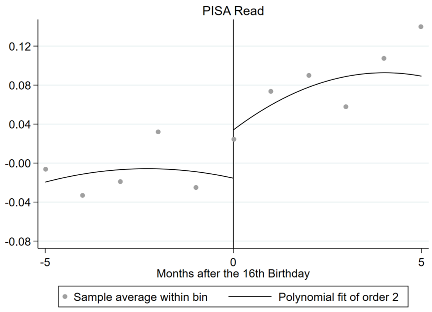
(a) Maths before MLDA



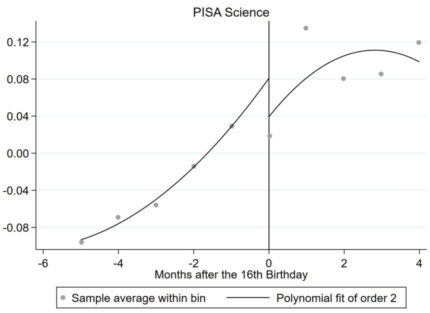
(b) Maths after MLDA



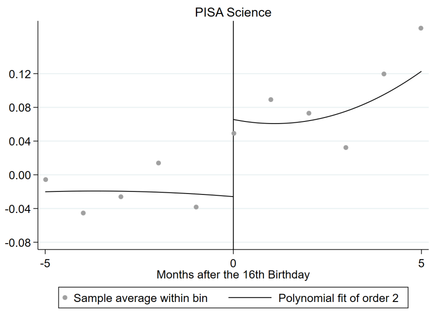
(c) Reading before MLDA



(d) Reading after MLDA



(e) Science before MLDA



(f) Science after MLDA

Figure A.2: PISA score discontinuity around the age of 16: Left panels show scores before the introduction of MLDA, while right panels display scores after the reform. Polynomial specification of second degree.

Table A.1: Optimal bandwidth (months) by selected outcomes

PISA Math	MSE	3.4
	MSE-bias	5.3
PISA Read	MSE	3.5
	MSE-bias	5
PISA Science	MSE	3.7
	MSE-bias	5.1

Notes: MSE is Mean Squared Error (MSE) optimal bandwidth. MSE-bias is MSE-optimal bandwidth selector for the bias of the RD treatment effect estimator. Both bandwidths were chosen following Calonico et al. (2017),and implemented with the command `rdbwselect` in STATA,using a local linear polynomial and triangular kernel.

Table A.2: Differences in predetermined students' characteristics around the 16th birthday, PISA (p-values reported).

Bandwidth	Female	Native		Parents		Books	Social-Cultural
		speaker	born	Education	Occupation		Status
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A. $g(b_i)$ of degree 3				
5 months	0.061	0.013	0.000	0.226	0.142	0.000	0.320
4 months	0.068	0.011	0.000	0.862	0.064	0.000	0.170
			Panel B. $g(b_i)$ of degree 2				
5 months	0.068	0.101	0.033	0.051	0.247	0.002	0.156
4 months	0.132	0.129	0.007	0.035	0.255	0.002	0.154
3 months	0.092	0.219	0.002	0.210	0.427	0.000	0.517
			Panel C. $g(b_i)$ of degree 1				
5 months	0.216	0.240	0.074	0.672	0.347	1.000	0.195
4 months	0.159	0.181	0.037	0.695	0.407	0.794	0.315
3 months	0.075	0.289	0.020	0.423	0.338	0.259	0.373
2 months	0.118	0.304	0.014	0.046	0.611	0.193	0.404

Notes: For each of the variables (w_i), reported on the top of the columns, we run the regression, $w_i = \alpha_s + \eta_t + \gamma_1 * 1\{b_i > 0\} + \gamma_2 * 1\{b_i > 0\} * post + g(b_i) + v_i$ where $1\{b_i > 0\}$ is an indicator variable taking a value of one for students with birthday margin above 16 (192 months), and zero otherwise. *post* is a dummy variable taking a value one for the periods after the MLDA implementation, and zero otherwise. α_s and η_t are region, and PISA-year fixed effects, respectively. $g(b_i)$ a polynomial specification in the running variable that we allow to differ between the periods before and after the reform. The null hypothesis for which the p-values are reported is $H0 : \gamma_1 = \gamma_2 = 0$, that is, there is no difference in the predetermined variable over and below the cutoff.

Table A.3: Change in differences in predetermined students' characteristics around the 16th Birthday between periods before/after MLDA. (PISA) (p-values reported)

Bandwidth	Female	Native speaker	Native born	Parents Education	Parents Occupation	Books	Social-Cultural Status
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. $g(b_i)$ of degree 3						
5 months	0.027	0.012	0.443	0.618	0.075	0.282	0.165
4 months	0.033	0.015	0.267	0.628	0.129	0.868	0.192
	Panel B. $g(b_i)$ of degree 2						
5 months	0.047	0.043	0.041	0.465	0.102	0.004	0.176
4 months	0.148	0.049	0.027	0.449	0.110	0.020	0.167
3 months	0.037	0.096	0.248	0.727	0.265	0.153	0.321
	Panel C. $g(b_i)$ of degree 1						
5 months	0.095	0.126	0.046	0.447	0.211	0.984	0.134
4 months	0.077	0.162	0.017	0.431	0.236	0.551	0.173
3 months	0.156	0.188	0.007	0.449	0.166	0.115	0.218
2 months	0.052	0.534	0.011	0.854	0.348	0.118	0.534

Notes: For each of the variables (w_i), reported on the top of the columns, we run the regression, $w_i = \alpha_s + \eta_t + \gamma_1 * 1\{b_i > 0\} + \gamma_2 * 1\{b_i > 0\} * post + g(b_i) + v_i$ where $1\{b_i > 0\}$ is an indicator variable taking a value of one for students with birthday margin above 16 (192 months), and zero otherwise. *post* is a dummy variable taking a value one for the periods after the MLDA implementation, and zero otherwise. α_s and η_t are region, and PISA-year fixed effects, respectively. $g(b_i)$ a polynomial specification in the running variable that we allow to differ between the periods before and after the reform. The null hypothesis for which the p-values are reported is $H0 : \gamma_2 = 0$, that is, there is no additional difference in the predetermined variable over and below the cutoff, after the reform.

Table A.4: Differences in predetermined students' characteristics around the 16th birthday, ESTUDES (p-values reported).

Bandwidth	Female	Native born	Mother educ.	Father educ.	Mother employment	Father employment
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. $g(b_i)$ of degree 3					
5 months	0.088	0.940	0.330	0.033	0.064	0.512
4 months	0.057	0.360	0.397	0.020	0.114	0.622
	Panel B. $g(b_i)$ of degree 2					
5 months	0.010	0.012	0.982	0.049	0.143	0.522
4 months	0.039	0.060	0.496	0.108	0.068	0.454
3 months	0.008	0.073	0.269	0.439	0.028	0.372
	Panel C. $g(b_i)$ of degree 1					
5 months	0.290	0.060	0.734	0.376	0.109	0.279
4 months	0.191	0.007	0.756	0.165	0.111	0.294
3 months	0.203	0.005	0.831	0.529	0.231	0.431
2 months	0.047	0.031	0.774	0.172	0.057	0.954

Notes: For each of the variables (w_i), reported on the top of the columns, we run the regression, $w_i = \alpha_s + \eta_t + \gamma_1 * 1\{b_i > 0\} + \gamma_2 * 1\{b_i > 0\} * post + g(b_i) + v_i$ where $1\{b_i > 0\}$ is an indicator variable taking a value of one for students with birthday margin above 16 (192 months), and zero otherwise. *post* is a dummy variable taking a value one for the periods after the MLDA implementation, and zero otherwise. α_s and η_t are region, and ESTUDES wave-year fixed effects, respectively. $g(b_i)$ a polynomial specification in the running variable that we allow to differ between the periods before and after the reform. The null hypothesis for which the p-values are reported is $H0 : \gamma_1 = \gamma_2 = 0$, that is, there is no difference in the predetermined variable over and below the cutoff.

Table A.5: Change in differences in predetermined students' characteristics around the 16th Birthday between periods before/after MLDA. (ESTUDES) (p-values reported)

Bandwidth	Female	Native born	Mother educ.	Father educ.	Mother employment	Father employment
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. $g(b_i)$ of degree 3					
5 months	0.381	0.732	0.202	0.018	0.070	0.356
4 months	0.410	0.954	0.238	0.055	0.045	0.463
	Panel B. $g(b_i)$ of degree 2					
5 months	0.434	0.445	0.874	0.028	0.281	0.277
4 months	0.447	0.570	0.254	0.042	0.133	0.280
3 months	0.480	0.816	0.197	0.239	0.248	0.586
	Panel C. $g(b_i)$ of degree 1					
5 months	0.247	0.585	0.449	0.184	0.209	0.293
4 months	0.386	0.310	0.487	0.067	0.231	0.228
3 months	0.692	0.494	0.943	0.278	0.745	0.229
2 months	0.447	0.504	0.513	0.311	0.652	0.777

Notes: For each of the variables (w_i), reported on the top of the columns, we run the regression, $w_i = \alpha_s + \eta_t + \gamma_1 * 1\{b_i > 0\} + \gamma_2 * 1\{b_i > 0\} * post + g(b_i) + v_i$ where $1\{b_i > 0\}$ is an indicator variable taking a value of one for students with birthday margin above 16 (192 months), and zero otherwise. *post* is a dummy variable taking a value one for the periods after the MLDA implementation, and zero otherwise. α_s and η_t are region, and ESTUDES wave-year fixed effects, respectively. $g(b_i)$ a polynomial specification in the running variable that we allow to differ between the periods before and after the reform. The null hypothesis for which the p-values are reported is $H0 : \gamma_2 = 0$, that is, there is no additional difference in the predetermined variable over and below the cutoff, after the reform.

A.2 Additional estimation results from PISA data

Table A.6: Impact of MLDA on selected PISA scores. (5 months bandwidth and quadratic specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Maths (std)					
Above threshold	0.038	0.072	-0.002	0.000	0.124**
	(0.038)	(0.047)	(0.054)	(0.043)	(0.047)
Above X post MLDA	0.092	-0.044	0.227***	0.218***	-0.037
	(0.055)	(0.064)	(0.060)	(0.045)	(0.076)
Obs	26080	12987	13093	12841	13239
Panel B: Reading (std)					
Above threshold	-0.008	0.039	-0.050	-0.047	0.078***
	(0.031)	(0.022)	(0.048)	(0.040)	(0.022)
Above X post MLDA	0.083	-0.049	0.206**	0.176***	-0.025
	(0.059)	(0.054)	(0.068)	(0.053)	(0.064)
Obs	26080	12987	13093	12841	13239
Panel C: Science (std)					
Above threshold	0.005	0.058	-0.046	-0.017	0.056
	(0.033)	(0.039)	(0.034)	(0.033)	(0.042)
Above X post MLDA	0.137**	-0.036	0.306***	0.202***	0.074
	(0.054)	(0.069)	(0.048)	(0.050)	(0.073)
Obs	26080	12987	13093	12841	13239

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the PISA wave years, fixed effects for regions, and parental education. Student characteristics are allowed to have effects that vary across regions. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table A.7: Impact of MLDA on selected behavioural outcomes. (5 months bandwidth and quadratic specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Sociability index					
Above threshold	-0.003	-0.024	0.030	-0.004	0.022
	(0.008)	(0.015)	(0.019)	(0.008)	(0.018)
Above X post MLDA	-0.024	0.144***	-0.187**	0.017	-0.075
	(0.057)	(0.031)	(0.076)	(0.095)	(0.044)
Obs	16417	8270	8147	6324	10093
Panel B: Truant - Skip school					
Above threshold	0.205**	0.135***	0.257**	0.167*	0.213**
	(0.074)	(0.043)	(0.097)	(0.085)	(0.081)
Above X post MLDA	-0.240***	-0.159***	-0.302***	-0.329***	-0.176**
	(0.063)	(0.046)	(0.073)	(0.064)	(0.073)
Obs	16058	8065	7993	6141	9917
Panel C: Truant - skip classes					
Above threshold	0.143***	0.224***	0.077**	0.040	0.270***
	(0.023)	(0.060)	(0.031)	(0.055)	(0.060)
Above X post MLDA	-0.206***	-0.264***	-0.159**	-0.153*	-0.302***
	(0.037)	(0.056)	(0.055)	(0.075)	(0.037)
Obs	16031	8055	7976	6135	9896
Panel D: Truant - Late for school					
Above threshold	-0.255***	-0.347**	-0.136	-0.425***	0.201
	(0.069)	(0.113)	(0.162)	(0.076)	(0.147)
Above X post MLDA	0.114	0.179*	0.017	0.181**	-0.297*
	(0.074)	(0.081)	(0.169)	(0.080)	(0.140)
Obs	13131	6618	6513	4454	8677

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the PISA wave years, fixed effects for regions, and parental education. Student characteristics are allowed to have effects that vary across regions. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

A.3 Additional estimation results from ESTUDES data

Table A.8: Impact of MLDA on selected ESTUDES outcomes (own drinking). (5 months bandwidth and polynomial specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever drink alcohol					
Above threshold	0.011	-0.047**	0.064**	0.112***	-0.114**
	(0.017)	(0.017)	(0.027)	(0.028)	(0.044)
Above X post MLDA	-0.016	0.029	-0.059***	-0.094***	0.088**
	(0.017)	(0.034)	(0.017)	(0.024)	(0.032)
Obs	7751	3950	3801	4345	3406
Panel B: Drank in the past month					
Above threshold	-0.060**	-0.030	-0.087***	-0.133***	0.026
	(0.026)	(0.044)	(0.025)	(0.042)	(0.019)
Above X post MLDA	0.025	0.033	0.019	0.063*	-0.025
	(0.017)	(0.023)	(0.026)	(0.032)	(0.037)
Obs	6906	3561	3345	3847	3059
Panel C: Drink weekly					
Above threshold	0.035	-0.015	0.083**	0.007	0.064
	(0.030)	(0.039)	(0.031)	(0.034)	(0.045)
Above X post MLDA	-0.013	0.007	-0.031	-0.026	-0.015
	(0.016)	(0.029)	(0.025)	(0.020)	(0.045)
Obs	6508	3338	3170	3622	2886

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table A.9: Impact of MLDA on selected ESTUDES outcomes (intoxication). (5 months bandwidth and polynomial specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever been intoxicated					
Above threshold	-0.088***	-0.129***	-0.062***	-0.105***	-0.068**
	(0.014)	(0.023)	(0.016)	(0.013)	(0.030)
Above X post MLDA	0.004	0.014	0.010	0.040	-0.040
	(0.038)	(0.011)	(0.069)	(0.033)	(0.058)
Obs	7588	3879	3709	4251	3337
Panel B: Intoxication in the last month					
Above threshold	0.032**	-0.029	0.085***	0.092**	-0.050
	(0.014)	(0.037)	(0.016)	(0.032)	(0.062)
Above X post MLDA	-0.071***	-0.093**	-0.039	-0.107**	-0.008
	(0.013)	(0.030)	(0.036)	(0.046)	(0.062)
Obs	7012	3604	3408	3894	3118
Panel C: Frequency of intoxication in the last month					
Above threshold	0.122	-0.078	0.299***	0.233*	-0.023
	(0.072)	(0.081)	(0.082)	(0.110)	(0.072)
Above X post MLDA	-0.154***	-0.141***	-0.141*	-0.256**	0.026
	(0.037)	(0.026)	(0.071)	(0.097)	(0.110)
Obs	6835	3510	3325	3788	3047

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table A.10: Impact of MLDA on selected ESTUDES outcomes (own tobacco). (5 months bandwidth and polynomial specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever smoked					
Above threshold	0.020	-0.071***	0.102**	0.047	-0.001
	(0.021)	(0.017)	(0.035)	(0.027)	(0.046)
Above X post MLDA	-0.090***	-0.052*	-0.117***	-0.167***	-0.001
	(0.014)	(0.024)	(0.020)	(0.035)	(0.046)
Obs	7733	3945	3788	4342	3391
Panel B: Smoked in the last month					
Above threshold	-1.552***	-0.023	-3.062***	-1.038***	-2.435**
	(0.456)	(0.647)	(0.435)	(0.298)	(1.020)
Above X post MLDA	0.004	-0.263	0.236	-0.272	0.730
	(0.689)	(0.575)	(0.808)	(0.402)	(1.436)
Obs	6493	3391	3102	3571	2922
Panel C: No. cigarettes smoked daily					
Above threshold	-0.237*	-0.299	-0.235	0.012	-0.524*
	(0.121)	(0.195)	(0.162)	(0.068)	(0.270)
Above X post MLDA	-0.034	-0.225	0.200	-0.277*	0.214
	(0.091)	(0.169)	(0.186)	(0.137)	(0.148)
Obs	5895	3044	2851	3258	2637

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table A.11: Impact of MLDA on selected ESTUDES outcomes (own marijuana). (5 months bandwidth and polynomial specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever took marijuana					
Above threshold	0.029*	-0.013	0.062***	0.079**	-0.024
	(0.015)	(0.017)	(0.020)	(0.026)	(0.046)
Above X post MLDA	-0.048**	-0.005	-0.080***	-0.072	-0.024
	(0.017)	(0.019)	(0.024)	(0.044)	(0.034)
Obs	7661	3913	3748	4297	3364
Panel B: Took marijuana in the last year					
Above threshold	0.040***	-0.005	0.079***	0.083***	-0.010
	(0.013)	(0.015)	(0.022)	(0.024)	(0.040)
Above X post MLDA	-0.064**	-0.010	-0.109**	-0.105**	-0.020
	(0.024)	(0.024)	(0.035)	(0.045)	(0.027)
Obs	7628	3906	3722	4270	3358
Panel C: Took marijuana in the last month					
Above threshold	0.012	0.026	-0.002	0.023	-0.007
	(0.015)	(0.028)	(0.028)	(0.034)	(0.040)
Above X post MLDA	-0.032	-0.017	-0.043	-0.040	-0.033
	(0.038)	(0.037)	(0.043)	(0.054)	(0.030)
Obs	6820	3474	3346	3769	3051
Panel D: Took marijuana first time by age 16					
Above threshold	0.031	-0.008	0.064**	0.077**	-0.020
	(0.019)	(0.018)	(0.026)	(0.027)	(0.056)
Above X post MLDA	-0.047**	-0.014	-0.068**	-0.078*	-0.013
	(0.016)	(0.017)	(0.026)	(0.043)	(0.035)
Obs	7590	3887	3703	4238	3352

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table A.12: Impact of MLDA on selected ESTUDES outcomes (peer risky behaviours). (5 months bandwidth and polynomial specification)

	Full	Female	Male	Low-Edu	High-Edu
	(1)	(2)	(3)	(4)	(5)
Panel A: Drink alcohol					
Above threshold	0.215***	0.084	0.349***	0.197***	0.271***
	(0.045)	(0.056)	(0.048)	(0.051)	(0.065)
Above X post MLDA	-0.177***	-0.122**	-0.251***	-0.237***	-0.122*
	(0.024)	(0.050)	(0.027)	(0.035)	(0.060)
Panel B: Smoking					
Above threshold	0.146***	0.018	0.268***	0.218***	0.073
	(0.030)	(0.041)	(0.065)	(0.058)	(0.045)
Above X post MLDA	-0.081**	-0.025	-0.137**	-0.187**	0.058
	(0.032)	(0.034)	(0.057)	(0.062)	(0.046)
Panel C: Intoxication					
Above threshold	0.177***	0.038	0.304***	0.134*	0.248**
	(0.055)	(0.061)	(0.067)	(0.071)	(0.081)
Above X post MLDA	-0.134***	-0.135*	-0.132***	-0.204***	-0.034
	(0.026)	(0.065)	(0.038)	(0.048)	(0.098)
Panel D: Take marijuana					
Above threshold	0.168***	-0.037	0.356***	0.228***	0.118**
	(0.025)	(0.026)	(0.051)	(0.040)	(0.042)
Above X post MLDA	-0.098***	0.035	-0.214***	-0.161***	-0.027
	(0.019)	(0.049)	(0.053)	(0.032)	(0.043)
Panel E: Take cocaine					
Above threshold	0.055**	0.053	0.052**	0.096***	-0.012
	(0.025)	(0.046)	(0.019)	(0.023)	(0.037)
Above X post MLDA	-0.062**	-0.104**	-0.015	-0.091***	-0.011
	(0.023)	(0.036)	(0.023)	(0.024)	(0.041)
Obs	7657	3919	3738	4295	3362

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of 5 months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions, and parental education. A second-degree polynomial specification for $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

A.4 Data Appendix

Table A.13: Year of the MLDA increase and PISA waves of each region

		PISA Waves						
MLDA Date	Autonomous Community	2003	2006	2009	2012	2015	2018	2022
20-May-15	Asturias	n/a	pre	pre	pre	pre/post	post	post
28-Feb-11	Galicia	n/a	pre	pre	post	post	post	post
15-Mar-07	Castilla-León	pre	pre	post	post	post	post	post

Notes: Each column indicates whether a specific PISA wave is available for each autonomous community in our main analysis, and whether the data corresponds to the period prior to the MLDA reform (*pre*) or after the reform (*post*).

Table A.14: Year of the MLDA increase and ESTUDES waves of each region

		ESTUDES								
MLDA Date	Autonomous Community	2004	2006	2008	2010	2012	2014	2016	2019	2021
20-May-15	Asturias	pre	pre	pre	pre	pre	pre	post	post	post
28-Feb-11	Galicia	pre	pre	pre	pre	post	post	post	post	post
15-Mar-07	Castilla-León	pre	pre	post	post	post	post	post	post	post

Notes: Each column indicates whether a specific ESTUDES wave is available for each autonomous community in our main analysis, and whether the data corresponds to the period prior to the MLDA reform (*pre*) or after the reform (*post*).

Table A.15: Variable descriptions: PISA data

Variable	Description	Availability in PISA
(A) Main identification variables		
Age by months at survey	Age by months at the survey	All years
Region of residence	Region of residence	All years
(B) Individual characteristics		
Gender	1 if female, 0 if male	All years
Native born	1 if born in Spain, 0 otherwise	All years
Native language	1 if Spanish is native language, 0 otherwise	All years
Whether mother has a university degree	1 if university, 0 otherwise	All years
Whether father has a university degree	1 if university, 0 otherwise	All years
Number of books	Number of books at home	All years
Social-cultural status	Index of economic, social and cultural status	All years
(C) Academic performance		
Maths	PISA maths test, standardised at the survey x region level	All years
Reading	PISA reading test, standardised at the survey x region level	All years
Science	PISA science test, standardised at the survey x region level	All years
(D) Schooling		
Index of child's sociability	Self-assessed index on how much child feels sociable and belong at school (normalised, 0-1)	N/A in 2006, 2009
Truancy: skip school	whether skipped a whole school day in the last two full weeks of school	N/A in 2003, 2006, 2009
Truancy: skip some classes	whether skipped some classes in the last two full weeks of school	N/A in 2003, 2006, 2009
Truancy: late for school	whether late for school in the last two full weeks of school	N/A in 2006, 2009, 2012

Notes: The PISA waves used in the analysis are 2003, 2006, 2009, 2012, 2015, 2018, 2022. N/A is when the data is *not available* in a specific wave.

Table A.16: Variable descriptions: ESTUDES data

Variables	**Description**	**Avaliablity in ESTUDES**
(A) Main identification variables		
Age by months at survey	Age by months at the survey	All years
Region of residence	Region of residence	All years
(B) Individual characteristics		
Gender	1 if female, 0 if male	All years
Native born	1 if born in Spain, 0 otherwise	All years
Whether mother has a university degree	1 if university, 0 otherwise	All years
Whether father has a university degree	1 if university, 0 otherwise	All years
Whether mother works	1 if works, 0 otherwise	All years
Whether father works	1 if works, 0 otherwise	All years
(C) Alcohol consumption		
Ever had alcohol	1 if ever consumed alcohol in their life	All years
Whether drink in the last month	1 if had alcoholic drink in the past 30 days	All years
Whether drink on a weekly basis	1 if consume alcohol regularly each week	N/A in 2014
(D) Being drunk		
Ever intoxicated	1 if ever been intoxicated	All years
Intoxicated in the last month	1 if had been drunk in the past 30 days	All years
Frequency of intoxication last month	No. days being intoxicated in the last month	All years
(E) Tobacco consumption		
Ever smoked tobacco	1 if ever smoked cigarettes once in their life	All years
Whether smoked in the last month	1 if smoked cigarettes in the past 30 days	All years
No. cigarette smoked per day	Range of number of cigarettes smoked daily	All years
(F) Marijuana consumption		
Ever took cannabis in their life	1 if ever took cannabis in their life	All years
Took cannabis last year	1 if took cannabis in the past 12 months	All years
Took cannabis in the last month	1 if took cannabis in the past 30 days	All years
First took cannabis by age 16	1 if ever took cannabis by age 16 or younger	All years
(G) Peer risky behaviours		
Share of peers who smoke	0 = none, 1 = some, 2 = most peers smoke	All years
Share of peers who drink alcohol	1 = none, 1 = some, 2 = most peers drink	All years
Share of peers who get drunk	2 = none, 1 = some, 2 = most peers get drunk	All years
Share of peers who take cannabis	3 = none, 1 = some, 2 = most peers take cannabis	All years
Share of peers who take cocaine	3 = none, 1 = some, 2 = most peers take cocaine	All years

Notes: The ESTUDES waves used in analysis are 2004, 2006, 2008, 2010, 2012, 2014, 2016, 2019, and 2021. N/A is when the data is *not available* in a specific wave.

Table A.17: Summary statistics, by timing (pre-post) and cut-off age: PISA Data

	(1) Below 16 Before	(2) Above 16 Before	(3) Below 16 After	(4) Above 16 After
Panel A: Bandwidth of 2 months				
PISA Math	-0.009	0.061	-0.008	0.060
PISA Reading	-0.012	0.056	-0.009	0.058
PISA Science	-0.012	0.077	-0.016	0.072
Female student	0.494	0.486	0.499	0.502
Foreign born	0.953	0.945	0.927	0.930
Educ. Parent	4.106	4.128	5.803	5.881
Residence Galicia	0.270	0.284	0.359	0.362
Residence Asturias	0.441	0.442	0.187	0.184
N	2705	2855	5020	4839
Panel B: Bandwidth of 5 months				
PISA Math	-0.032	0.070	-0.032	0.064
PISA Reading	-0.034	0.063	-0.030	0.064
PISA Science	-0.045	0.085	-0.038	0.071
Female student	0.498	0.481	0.497	0.501
Foreign born	0.948	0.947	0.932	0.928
Educ. Parent	4.102	4.129	5.814	5.800
Residence Galicia	0.292	0.274	0.371	0.360
Residence Asturias	0.431	0.449	0.188	0.182
N	5421	4323	9845	7277

Table A.18: Summary statistics, by timing (pre-post) and cut-off age: ESTUDES.

	(1) Below 16 Before	(2) Above 16 Before	(3) Below 16 After	(4) Above 16 After
Panel A: Bandwidth of 2 months				
Ever drank	0.788	0.855	0.770	0.821
Ever smoke	0.414	0.458	0.358	0.391
Ever marihuana	0.684	0.320	0.222	0.255
Female student	0.484	0.495	0.505	0.518
Mother educ.	3.703	3.530	3.904	3.648
Father educ.	3.648	3.462	3.803	3.534
Mother work status	0.618	0.599	0.633	0.588
Father work status	0.850	0.780	0.822	0.796
Residence Galicia	0.382	0.354	0.419	0.413
Residence Asturias	0.527	0.577	0.183	0.050
N	1289	1048	1603	958
Panel B: Bandwidth of 5 months				
Ever drank	0.770	0.856	0.753	0.823
Ever smoke	0.402	0.453	0.360	0.407
Ever marihuana	0.630	0.326	0.222	0.275
Female student	0.503	0.503	0.507	0.518
Mother educ.	3.679	3.499	3.916	3.769
Father educ.	3.656	3.463	3.790	3.634
Mother work status	0.614	0.590	0.653	0.622
Father work status	0.841	0.795	0.824	0.800
Residence Galicia	0.383	0.363	0.407	0.392
Residence Asturias	0.524	0.575	0.169	0.127
N	2576	2080	3311	2572

Table A.19: Impact of MLDA on selected ESTUDES outcomes (own drinking). (Two months bandwidth and linear specification)

	(1) Ever Drink	(2) Drink last month	(3) Drink weekly	(4) Ever drunk	(5) Drunk last month	(6) Number drunk last month
	Panel A: Complete sample					
Above threshold	0.028	0.002	0.070*	-0.077*	0.009	0.114*
	(0.044)	(0.019)	(0.029)	(0.036)	(0.014)	(0.051)
Above X post MLDA	-0.035	-0.122***	-0.075**	0.023	-0.001	-0.083
	(0.051)	(0.017)	(0.022)	(0.067)	(0.029)	(0.073)
N	3604	3222	3013	3522	3256	3160
	Panel B: Gender Heterogeneity					
	Panel B.1: Girls					
Above threshold	-0.028	0.050	0.011	-0.102	-0.018	-0.084
	(0.019)	(0.026)	(0.023)	(0.054)	(0.028)	(0.081)
Above X post MLDA	0.006	-0.158**	-0.032	0.004	-0.053	-0.072
	(0.045)	(0.057)	(0.047)	(0.095)	(0.091)	(0.138)
N	1809	1613	1530	1771	1629	1581
	Panel B.2: Boys					
Above threshold	0.085	-0.049**	0.132***	-0.064	0.040**	0.284***
	(0.065)	(0.014)	(0.032)	(0.035)	(0.012)	(0.046)
Above X post MLDA	-0.092	-0.075*	-0.099***	0.057	0.067	-0.008
	(0.068)	(0.036)	(0.019)	(0.052)	(0.048)	(0.044)
N	1795	1609	1483	1751	1627	1579
	Panel C: Educational Heterogeneity					
	Panel C.1: Low educ level					
Above threshold	0.067**	-0.024	0.022	-0.097**	0.045*	0.096**
	(0.024)	(0.028)	(0.028)	(0.026)	(0.021)	(0.035)
Above X post MLDA	-0.026	-0.149***	-0.033	0.060	0.003	-0.023
	(0.030)	(0.036)	(0.027)	(0.056)	(0.046)	(0.183)
N	2047	1818	1691	1999	1838	1778
	Panel C.2: High educ level					
Above threshold	-0.023	0.042	0.133***	-0.052	-0.055	0.103
	(0.088)	(0.022)	(0.032)	(0.065)	(0.061)	(0.131)
Above X post MLDA	-0.030	-0.106**	-0.163**	-0.037	0.020	-0.102
	(0.087)	(0.032)	(0.054)	(0.091)	(0.043)	(0.152)
N	1553	1400	1318	1518	1413	1377

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions times survey-year, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.

Table A.20: Impact of MLDA on selected ESTUDES outcomes (own drinking). (Two months bandwidth and linear specification)

	(1) Ever Drink	(2) Drink last month	(3) Drink weekly	(4) Ever drunk	(5) Drunk last month	(6) Number drunk last month
Panel A: Complete sample						
Above threshold	0.028	0.002	0.070*	-0.077*	0.009	0.114*
	(0.044)	(0.019)	(0.029)	(0.036)	(0.014)	(0.051)
Above X post MLDA	-0.035	-0.122***	-0.075**	0.023	-0.001	-0.083
	(0.051)	(0.017)	(0.022)	(0.067)	(0.029)	(0.073)
N	3604	3222	3013	3522	3256	3160
Panel B: Gender Heterogeneity						
Panel B.1: Girls						
Above threshold	-0.028	0.050	0.011	-0.102	-0.018	-0.084
	(0.019)	(0.026)	(0.023)	(0.054)	(0.028)	(0.081)
Above X post MLDA	0.006	-0.158**	-0.032	0.004	-0.053	-0.072
	(0.045)	(0.057)	(0.047)	(0.095)	(0.091)	(0.138)
N	1809	1613	1530	1771	1629	1581
Panel B.2: Boys						
Above threshold	0.085	-0.049**	0.132***	-0.064	0.040**	0.284***
	(0.065)	(0.014)	(0.032)	(0.035)	(0.012)	(0.046)
Above X post MLDA	-0.092	-0.075*	-0.099***	0.057	0.067	-0.008
	(0.068)	(0.036)	(0.019)	(0.052)	(0.048)	(0.044)
N	1795	1609	1483	1751	1627	1579
Panel C: Educational Heterogeneity						
Panel C.1: Low educ level						
Above threshold	0.067**	-0.024	0.022	-0.097**	0.045*	0.096**
	(0.024)	(0.028)	(0.028)	(0.026)	(0.021)	(0.035)
Above X post MLDA	-0.026	-0.149***	-0.033	0.060	0.003	-0.023
	(0.030)	(0.036)	(0.027)	(0.056)	(0.046)	(0.183)
N	2047	1818	1691	1999	1838	1778
Panel C.2: High educ level						
Above threshold	-0.023	0.042	0.133***	-0.052	-0.055	0.103
	(0.088)	(0.022)	(0.032)	(0.065)	(0.061)	(0.131)
Above X post MLDA	-0.030	-0.106**	-0.163**	-0.037	0.020	-0.102
	(0.087)	(0.032)	(0.054)	(0.091)	(0.043)	(0.152)
N	1553	1400	1318	1518	1413	1377

Notes: * p<0.05, ** p<0.01, *** p<0.001. A bandwidth of two months around the potential discontinuity point is used. Each specification includes a gender dummy variable, no-native dummy, fixed effects for the ESTUDES wave years, fixed effects for regions times survey-year, and parental education. A linear specification for the function $g(.)$ is employed, with the slope allowed to vary on either side of the potential discontinuity and across periods of the reform. Standard errors are clustered at the level of the running variable.