School Re-enrollment and Crime Prevention: The Role of Centralized Dropout Monitoring Systems

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ABSTRACT. While many dropout prevention policies have proven cost-effective in keeping at-risk students enrolled, evidence on how to re-enroll those who have already left the school system —who are arguably at greater risk of engaging in crime— remains surprisingly scarce. Chile's Ministry of Education developed a low-cost, centralized dropout monitoring system to identify dropouts in a timely manner and provided all public schools with reports encouraging them to leverage their local networks to facilitate students' re-enrollment, whether in the same school or another. The list of dropout students for each school included only those who had dropped out before May 30, 2022. We identify the causal effect of being listed in the report through an RD design that exploits the predetermined cutoff date as an exogenous determinant of eligibility for inclusion, and compare high-school students who dropped out from the same school just before and just after the cutoff date. Being listed in the report increased re-enrollment by 17% in the following two months after report distribution, which in turn led to a similar increase in grade completion by the end of the year. Local connections played a crucial role, with most re-enrollments occurring in schools other than the school of origin but within the same educational network. As a result of the report-driven increase in school re-enrollment, two years later the violent crime rate of listed dropouts was 79% lower compared to that of non-listed dropouts. The cost of implementing dropout reports at scale averages US$93 per dropout student, yielding a benefit-cost ratio of 103:1 for preventing violent crime. Even without considering labor market returns from education or savings from reduced crime in adulthood, the reports generate annual savings equivalent to US$30 million, or about 0.01% of Chile's GDP. Centralized dropout monitoring systems offer a highly cost-effective solution to tackle the dropout-crime cycle.

KEYWORDS. Dropout Monitoring Systems, School Re-enrollment, Juvenile Crime.

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1. Introduction

School dropout is hitting a wall. After a decade of steady decline, the rate of adolescents not attending upper secondary school has stalled in the post-pandemic era, lingering at around 5% in higher-income countries and 12% in upper-middle-income countries (UNESCO, 2023)[1]. Beyond education, a critical concern is the well-documented relationship between school dropout and crime (Cook and Songman, 2016; Sviatschi, 2022). The quantity and quality of schooling also influence criminal behavior in adulthood, underscoring the long-term effects of dropping out on criminal activity (Freeman, 1996; Lochner, 2004, 2011; Lochner and Moretti, 2004; Deming, 2011; Machin et al., 2011; Aizer and Doyle, 2015; Hjalmarsson et al., 2015; Bell et al., 2016, 2022; Rose et al., 2022; Bacher-Hicks et al., 2024). A wide array of dropout-preventive measures have proven to be cost-effective in curbing school dropout and its potential link to juvenile crime, including minimum dropout age (MDA) laws (Angrist and Krueger, 1991; Lleras-Muney, 2002; Oreopoulos, 2006), comprehensive education support initiatives (Oreopoulos et al., 2017; Lavecchia et al., 2020), summer jobs (Heller, 2014), cognitive and non-cognitive behavioral therapies (Heller et al., 2017; Algan et al., 2022), test-based promotion policies (Jacob and Lefgren, 2009; Manacorda, 2012; Eren et al., 2017; Diaz et al., 2021; Larsen, 2021), extended school day programs (Berthelon and Kruger, 2011; Rodríguez-Planas, 2012), high-school career counseling (Dinkelman and Martinez, 2014; Barnes et al., 2021), belief-updating interventions (Jensen, 2010; Dizon-Ross, 2018; Rogers and Feller, 2018), performance report cards (Andrabi et al., 2017; Berlinski et al., 2022), early warning systems (Bowers, 2021; Adelman et al., 2021), and conditional cash transfers (Rumberger, 2020, García and Saavedra, 2022), among others[2]. All these initiatives share a common goal: preventing school dropout before it happens. However, none explicitly focus on re-enrolling students who have already abandoned the school system. Once dropout-preventive measures fail and at-risk students leave school, institutions lose oversight, weakening their incapacitation role and increasing the likelihood of dropouts engaging in criminal activity (Cook and Songman, 2016; Sviatschi, 2022).

In this paper, we examine the causal role of centralized dropout monitoring systems in facilitating the re-enrollment of students who have left the school system and their subsequent effects on crime prevention. Our study focuses on Chile's voucher system, where students can choose between public and subsidized private schools regardless of residential location. This decentralized and non-captive educational market makes it difficult for schools to track disenrolled students. As a result, schools often do not know whether a former student has re-enrolled elsewhere or dropped out entirely[3].

Post-pandemic, Chile's Ministry of Education launched a re-enrollment initiative targeting all dropout students from public schools. Known as the Dropout Reports, MINEDUC first generated automated, school-specific reports listing former students who had left school in the past 12 months

[1]These figures scale up to 36% for lower-middle income countries and to 53% for low-income counterparts (UNESCO, 2023), and can be even larger according to Sandefur (2022).

[2]Other dropout-preventive initiatives like General Education Development (GED) programs may generate the opposite effect (Heckman et al., 2012).

[3]This contrasts with other systems, such as the U.S. school system, where students attend schools in their local district and the public education market is captive, meaning district authorities are the sole providers. Additionally, state-level reporting ensures greater standardization. This combination of neighborhood zoning and local oversight reduces information frictions about disenrolled students in the U.S. compared to Chile.

and had not re-enrolled elsewhere. These reports were then sent to each school. A key innovation is that the reports are generated using MINEDUC's Education Management Information System (SIGE), a centralized platform that enables large-scale production and distribution at very low cost. Specifically, the Dropout Reports leverage the requirement that school principals must notify SIGE whenever a student enrolls or leaves, allowing MINEDUC to promptly identify dropouts —students who leave without transferring elsewhere. The entire process is fully automated: SIGE compiles the necessary information, generates the reports as simple PDFs, and distributes them directly through the same platform, which ensures that schools receive up-to-date dropout lists through an efficient and low-burden process. Also, the Chilean SIGE system is not unique: Education Management Information Systems (EMIS) are widely used worldwide to collect, process, store, and analyze education-related information (UNESCO, 2020; Ortiz et al., 2021), meaning the Dropout Reports are potentially scalable to other contexts different from Chile's education market.

Importantly, even when schools are aware of a student's dropout status, they may have little incentive to bring them back as dropouts tend to underperform academically, and schools face no consequences for failing to track them. Likewise, students who have already chosen to leave may be reluctant to return to the same school, further reducing their likelihood of re-enrollment. Therefore, the report does not require schools to re-enroll former students in their own classrooms but instead encourages them to activate their institutional and spatial networks to facilitate re-enrollment, whether in the same school or another[4].

The reports were delivered on June 28^{th}, 2022, across all public schools in the Chilean education system. However, the list of dropout students for each school only included those who had disenrolled before May 30^{st}, 2022, and had not re-enrolled in any other school by that date. We identify the causal effect of being listed in the report by exploiting this predetermined cutoff date as an exogenous determinant of eligibility for inclusion. Since treatment compliance was not full, we use a Fuzzy RD setup to compare the re-enrollment and criminal behavior of students who disenrolled just before the cutoff date with that of those who disenrolled right after it. First, balance and manipulation tests support the internal validity of our design. Second, we demonstrate that students who disenrolled around the cutoff date are similar to those who disenrolled at other times throughout the year. Therefore, assuming stable re-enrollment incentives for both students and schools, our results are expected to exhibit greater external validity than is typically found in studies using RD designs.

After two months of program exposure, we find the re-enrollment rate among disenrolled students listed in the report increased by 17% compared to ineligible compliers, which in turn led to a similar increase in grade completion by the end of the year. The results are robust to placebo cutoffs and placebo samples, as well as to multiple variations of the optimal bandwidths around the cutoff date. The effects are primarily driven by high-school students. Back-of-the-envelope calculations suggest that of the 46,897 high school dropouts in 2022, half would have re-enrolled without the dropout reports. However, had all been included in the reports, dropouts would have decreased from 23,448 to 16,977, meaning 6,472 students, or 28% of not re-enrolled dropouts, would have re-enrolled.

[4]Gallego et al. (2024b) emphasize the value of personal assistance in interventions targeting long-term educational improvements. In dropout reports, schools leverage networks to re-enroll students, reinforcing the authors' view that personal support can foster lasting behavioral change.

Second, the re-enrollment effects were largely driven by dropouts enrolling in schools other than their schools of origin, with school networks playing a crucial role in facilitating this transition. Rather than directly re-enrolling dropouts, origin schools leveraged their institutional and spatial networks to support students' placement in alternative institutions. This aligns with the reports' policy design, which positioned schools as intermediaries rather than direct re-enrollment hubs. Moreover, we show that the effectiveness of these networks depended on school capabilities—schools with stronger management structures were more successful in facilitating student transitions, underscoring the importance of organizational capacity in dropout recovery efforts.

Having shown a positive effect on school re-enrollment, we next examine whether the report-driven re-enrollment effects prevented dropouts to engage in crime, for which we leverage data from the Youth National Protection Program (SENAME) holding the universe of young aged 14-17 who have committed violent crimes, including homicide, rape, and aggravated assault, as well as property crimes and drug trafficking. First, we observe that about 10% of high school dropouts in Chile engage in violent crimes, which quintuplets the same figure among average students. Second, the SENAME data allows us to follow the criminal records of students for the period 2016-2024, such that we can study the dropout-crime link over time. By means of an event-study design, we document that the violent crime rate increases sharply in the semester immediately after school dropout, an effect that persists in the following semesters.

Third, by exploiting the very same RD framework we employed to study re-enrollment effects, we show that two years after the report distribution, the juvenile crime rate among listed students diminished by 79% relative to ineligible compliers. This is a large effect. Had all 46,897 high school students who dropped out in 2022 been listed in the report, we estimate that the number of dropouts engaging in violent crimes for the first time would have been reduced from 4,526 to 952, meaning 3,574 students at risk of criminal behavior would have followed a non-criminal path due to the report.

A conservative estimate of the cost per crime in Chile is US$ 9,600—factoring in anticipation, consequence, and response costs— while the at-scale cost of implementing the reports is just US$ 93 per dropout student, which covers the design of the SIGE centralized dropout monitoring system, the production and delivery of dropout reports, and schools' efforts to reintegrate dropouts. The result is a benefit-cost ratio of 103-to-1. Scaling-up the reports to all dropout students in 2022 would yield to annual savings equivalent to US$ 30 million, or about 0.01% of Chile's GDP. This is likely to be a lower-bound estimate of the cost-effectiveness of dropout reports, since we do not include additional savings from reduced crime in adulthood nor the labor market returns to education of re-enrolled dropouts[5,6]. Furthermore, we do not account for changes in schools' disenrollment policies derived from the reports. For example, schools might tighten criteria for removing students from their rolls

[5]In light of the abundant evidence on the long-run crime-reduction effects of schooling (Lochner and Moretti, 2004; Machin et al., 2011; Aizer and Doyle, 2015; Hjalmarsson et al., 2015; Bell et al., 2016, 2022), we expect our causal estimate of the re-enrollment-crime elasticity become even larger when including adult crime, an exercise that it is out of the scope of this paper since the subjects of study are still too young to evaluate adult crime.

[6]Dropout students have not only shown to earn lower wages (Angrist and Krueger, 1991; Card, 1999; Heckman et al., 2006; Oreopoulos, 2006), but also take worse decisions in terms of health (Lange and Topel, 2006; Grossman, 2006; Cutler and Lleras-Muney, 2010), teenage pregnancy (Black et al., 2008), and marriage and parenting (Oreopoulos and Salvanes, 2011), all of which could translate into higher mortality rates (Lleras-Muney, 2005).

to minimize the appearance of high dropout rates in the reports, reflecting accountability-driven (extrinsic) motivations. Alternatively, they may adopt more supportive measures to prevent at-risk students from disengaging entirely, guided by a genuine concern for student welfare (intrinsic motivation). Indeed, we document that schools substantially reduced the share of disenrolled students after the disbursement of the reports, reinforcing the idea that our cost-effectiveness estimates likely represent a lower bound on the true benefits of centralized dropout monitoring.

Our study contributes to several literatures at the intersection of the economics of education and crime, and the role that anti-dropout policies may play in between. A substantial body of evidence suggests that making school compulsory may not be enough to prevent dropout, but additional information-based incentives can play a critical role in keeping students in school (Oreopoulos, 2007). For example, correcting misbeliefs about the returns to education has been shown to improve enrollment rates (Jensen, 2010; Dizon-Ross, 2018; Rogers and Feller, 2018; Gallego et al., 2024a). Providing report cards with information on student performance (Andrabi et al., 2017; Berlinski et al., 2022), dropout risks (Lichand and Christen, 2021; Bettinger et al., 2021), or details about financial aid programs for higher education (Dinkelman and Martinez, 2014) has also proven effective in boosting enrollment. More recently, Early Warning Systems that identify at-risk students and offer targeted support have gained traction, particularly in developing countries (Adelman et al., 2021)[7]. Interventions such as tutoring, fellowships, social and emotional learning (SEL) programs (Chappell et al., 2015; Huan et al., 2016; Rumberger, 2020), school-based management initiatives (Gertler et al., 2012), and conditional cash transfers (Dearden et al., 2009; García and Saavedra, 2022) have also shown success in addressing dropout. Finally, out-of-school programs like Pathways to Education have been particularly effective at reducing dropout among disadvantaged high school students (Oreopoulos et al., 2017; Lavecchia et al., 2022).

All these initiatives share the common objective of preventing dropout before it occurs; however, none are designed to facilitate the re-enrollment of students who have already left the school system, as is the case with the Dropout Reports. Our main contribution is to show, for the first time, that low-cost, information-based policies like centralized dropout monitoring systems can be a highly cost-effective way to simultaneously achieve two key development objectives: (i) re-enrolling dropouts and (ii) preventing juvenile crime. Moreover, the widespread use of EMIS systems to manage education data suggests that Dropout Reports could be a scalable solution globally.

Our results are consistent with Sviatschi (2022) who demonstrates that early exposure to illegal labor markets in Peru fosters the development of industry-specific skills, steering children toward a criminal career in the cocaine trade. In particular, the study finds that individuals who grew up in coca-producing regions and experienced high coca prices during childhood are approximately 30% more likely to face incarceration than their peers. However, the same study shows that providing

[7]See also Agasisti and Bowers (2017), Adelman et al. (2018), Sansone (2018), and Ham et al. (2024). EWSs are not out of critique. Early warning ML algorithms typically go beyond school and environment-level attributes and use individual-level information such as test scores, attendance, disciplinary history, free or reduced-price lunch status, gender, and race to predict dropout, which may engage in discrimination against minorities without necessary improve graduation rates. Perdomo et al. (2023) shows that is the case for the Dropout Early Warning System (DEWS) used in Wisconsin public schools. Indeed, in light of evidence by Woelfel et al. (2023) showing that Wisconsin's DEWS predictions were wrong at significantly greater rates for Black and Hispanic students than were for White students, Wisconsin education authorities have recently decided to shut down the DEWS system.

conditional cash transfers that encourage education reduces child labor in these illicit sectors, meaning CCTs can curb the accumulation of criminal capital while fostering formal human capital development.

The substantial crime-reduction savings make Dropout Reports a more cost-effective policy than other successful developmental crime prevention (DCP) programs, such as preschool, cognitive-behavioral interventions, and summer jobs. For instance, Heller et al. (2017) present results from three large RCTs in Chicago, assessing interventions to lower crime and dropout rates among disadvantaged youth. One of the programs they study called "Becoming a Man", improved school participation and led to a 28-35% drop in total arrests, and 45-50% decline in violent-crime arrests. Another program at the Cook County Juvenile Detention Center decreased readmission rates by 21%. These interventions yielded benefit-cost ratios that go from 5-to-1 to 30-to-1. Likewise, Algan et al. (2022) show that the Montreal Longitudinal Experimental Study, which provides social skills coaching and self-control training to 7-9 youth from low-income neighborhoods, reduced aggression and arrests by age 24, for a cost effectiveness of 1:11 (including labor market returns by age 39). Heller (2014) and Davis and Heller (2020) show that participating in summer jobs combined with a curriculum built on cognitive behavioral therapy principles reduces violent-crime arrests among disadvantaged high-school students in Chicago, for a benefit-cost ratio of 1:6[8]. Koegl et al. (2023) conduct a meta-analysis of cost-benefit analysis studies of DCP interventions, and find the cost-effectiveness of the initiatives range between 1:1 to 1:32, far below the 1:103 cost-benefit ratio we document for the case of dropout reports.

Our crime-reduction estimates are also large relative to similar estimates for the average youth, which is unsurprising. First, our target population are dropout students, a vulnerable population that is more likely to engage in criminal activity compared to the average youth, hence every re-enrollment has a greater potential impact on crime reduction. Second, our crime measure is based on violent crimes, whose incidence is quite low relative to non-violent crimes among juvenile population, thus our estimated effect relative to the mean is naturally larger. For instance, Fu et al. (2022) estimate a dynamic model of teenagers' choices of schooling and crime in Chile, and find that providing to average, not dropout students a free-tuition treatment combined with a decent schooling opportunity would increase the fraction of those consistently enrolled throughout primary and secondary education from 69% to 79%, and reduce the fraction of those ever arrested for violent or not-violent crimes by age 18 from 5.4% to 4.8%, for a school-enrollment-crime elasticity of -0.83, which is smaller than our estimated effect.

In the U.S. context, Cook and Songman (2016) documents that youths aged 13-15 who have a shorter time to contemplate dropout prior to graduation are in fact less likely to drop out, which results in a lower involvement in juvenile crime, for an enrollment-crime elasticity of -0.32. Using a triple DiD model, Anderson (2014) finds that 16-to-18-year-olds residing in US States adopting a minimum dropout age of 18 years-old are 17% less likely to be arrested for property/violent/drug-

[8]Similarly, Blattman et al. (2017) show that combining cash grants with an eight-weeks cognitive behavioral therapy designed to foster self-regulation, patience, and a noncriminal identity and lifestyle can also reduce crime and violence among high-risk adults in Liberia. Also with adults, Bhatt et al. (2024) tested the impact of READI, an 18-month program offering jobs, cognitive behavioral therapy, and social support to men at high risk for gun violence in Chicago, and find arrests for shootings and homicides dropped by 65%, for a benefit-cost ratio between 4:1 and 18:1.

related crimes compared to their counterparts residing in States with MDAs below 18 years-old, on average. Jacob and Lefgren (2003) find that the level of property crime committed by juveniles in the U.S. decreases by roughly 14% on days when school is in session due to teacher in-service days, and Luallen (2006) exploit teacher strikes as a source of variation in school attendance to show that property and violent crimes could fall by as much as 29% and 36%, respectively[9].

Finally, one may be tempted to exploit our RD design to obtain a structural estimate of the re-enrollment-crime elasticity. That is, use whether the student disenrolled before or after the report-eligibility cutoff date as an instrument for being re-enrolled in school to estimate the causal effect of school re-enrollment on the probability of committing a crime afterward. Note, however, that the excludability assumption requires our instrument to reduce crime only by increasing the chances that the student is re-enrolled in school, which may not be met in practice. A simple example is the hypothetical case of a dropout student who was listed in the report and, even though the school attempted to bring him back, remained a dropout. Still, the dropout student could have felt that someone (the school) was trying to take care of him, a "psycho-gain" that eventually reduced his chances of committing a crime, thereby violating the excludability assumption. In Section 6, we elaborate on the limitation of recovering causal estimates of elasticity parameters involving two endogenous variables through a single RD design.

The remainder of this paper is organized as follows. In Section 2 we describe the institutional setting governing schools in Chile, and characterize the population of dropouts. Section 3 details the dropout reports program, and provides a description of the data sources and timeline of the study. Section 4 introduces the identification strategy, the sample of study, and a battery of internal and external validity tests. Then, in Section 5 we present the main empirical findings on school re-enrollment, while Section 6 do the same for crime outcomes. Finally, Section 7 provides a cost-benefit analysis of the report's policy, and Section 8 concludes.

2. School Dropout in Chile

Institutional Setting. The Chilean Educational System has a decentralized management with central oversight. Since 1981, the administration of public schools, including kindergartens, primary, and secondary schools, has been delegated to local municipalities and private providers that are responsible of the day-to-day management of municipal and private voucher schools, respectively[10]. School funding is through a per-student voucher system that applies to both municipal and private voucher schools[11]. Every time a student leave, the school loses the voucher resources associated to

[9] Landersø et al. (2017) find a crime-enrollment elasticity of similar magnitude exploiting a discontinuity in school starting age in Denmark. By exploit admission cutoffs in over-subscribed secondary programs in Finland, Huttunen et al. (2023) find that men who are admitted to secondary schools are 52% less likely to be convicted in a district court within 10 years after admission than men who are not admitted. Other papers documenting incapacitation effects include Gottfredson (1985), Farrington et al. (1986), and Witte and Tauchen (1994).

[10] Since 2018, Chile has embarked on significant reforms aimed at strengthening public education, notably through the creation of Servicios Locales de Educación (Local Education Services). This initiative aims to transfer the management of public schools from municipalities to new specialized bodies.

[11] There are two main types of vouchers: the general per-student voucher and the Subvención Escolar Preferencial (SEP), a targeted voucher program designed to support at-risk, disadvantaged students by providing additional resources to schools for each eligible student.

that student, which promotes competition among schools to attract students.

MINEDUC and other institutions including the National Agency for the Quality of Education and the Superintendency of Education are in charge of overseeing macro-level educational policies, standards, and guidelines, while also ensuring quality by supervising and evaluating schools. A key operating tool is the General Student Information System (SIGE), which streamlines student registration processes, ensuring the integrity and accuracy of students data[12]. Specifically, schools are required to notify SIGE whenever a student leaves or enrolls in the school. Since the funding model relies on a per-student voucher system, the enrollment and disenrollment records must be precise — inaccuracies can result in severe penalties, which discourages misreporting. Leveraging this centralized enrollment management system, MINEDUC can accurately identify dropout students, i.e., students who disenroll from a school without transferring to another.

A key friction to re-enrolling dropouts in Chile is costly information acquisition, as Chile's voucher system allows students to freely choose between municipal and private voucher schools regardless of residential location, creating a decentralized and non-captive educational market that makes it difficult for schools to track disenrolled students. As a result, schools often do not know whether a student has re-enrolled elsewhere or has dropped out entirely[13]. Additionally, the lack of accountability discourages schools from taking action. Even when aware of a student's dropout status, schools have little incentive to re-enroll them, as dropouts tend to underperform, and there are no penalties for failing to track them. Similarly, dropouts, having already shown a preference for leaving, may be reluctant to return, further reducing their chances of re-enrollment.

Dropout Trends. School year in Chile goes from March to December. Chile's dropout rate is calculated annually by dividing the number of students who do not re-enroll in the year $t+1$ by the total number of students enrolled at the end of year t[14]. As illustrated in Appendix Figure A.I, the dropout rate followed a persistent decline throughout the past decade, from 2.2% to less than 1.5%. However, the post-pandemic trend shows an uptick. In absolute terms, the number of disengaged students in Chile rose from around 40,000 in 2019 (1.38%) to over 50,000 (1.56%) in 2023, a 25% increase in dropout students that backs dropout rate to 2015 levels. This rise is primarily driven by high-school students, which constitute about two-thirds of all dropout students. This is in line with the post-pandemic trend in dropout rates around the world. A meta-analysis conducted by Moscoviz and Evans (2022) shows that 6 out of 9 country-level studies show positive breaks in dropout trends

[12]The SIGE system emulates an Education Management Information System (EMIS), which are widely used in education systems globally. EMIS function as extensive data management systems, allowing educational institutions and government agencies to gather, process, store, and analyze education-related data. Their main objective is to aid decision-making, policy formulation, planning, and the monitoring of educational activities. For a more in-depth discussion of EMIS, refer to UNESCO (2020) and Ortiz et al. (2021)

[13]This is unlike the U.S., where students attend schools in their local district, and where the public education market is captive in the sense that district authorities are the sole providers. This results in lower information frictions about the status of disenrolled students compared to Chile.

[14]Dropout rates are measured in various ways worldwide, leading to complexities in cross-country comparisons (Lichand and Christen, 2021). Common methods include cohort-based rates, tracking a specific group of students from entry to graduation; event-based rates, focusing on specific exit points; and status-based rates, capturing the current number of non-enrolled students. Following MINEDUC standards, we use April 30^{th} as the specific date to count dropout students each year. Using a different date does not alter the magnitude and dynamics of dropout rate over time.

after the pandemic.

Target Population. Our target population consists of students who dropped out during the 2022 school year —those who were enrolled by March 1^{st}, 2022, but later disenrolled without transferring to another school[15]. Withdrawing a student from a school is not a unilateral decision but it requires the agreement of the student's tutor, who must sign a disenrollment contract with the school principal. This contract ensures that both the school and the tutor agree with the decision to disenroll the student and that the school is not violating the student's right to education. Once the disenrollment agreement is signed, the school of origin is no longer responsible of whether the withdrawn student became dropout or re-enrolled in another school.

In 2022, administrative data from the SIGE system recorded 90,013 dropout events in Chile's public schools, with around 5% of students dropping out more than once[16]. Panel (a) of Appendix Figure A.II shows the monthly distribution of dropouts. Two-thirds occur in the first semester (March–July), with a declining trend that resumes after a brief uptick in August, marking the start of the second semester. Re-enrollment within a month of disenrollment remains stable at around 60% during the first semester but drops sharply to 10% by year-end. Since the decline in the second semester could be influenced by the introduction of the dropout reports (more in Section 3), we replicate the exercise for 2016, which works as a pre-treatment comparison year that shares the same calendar structure than 2022. The patterns of dropouts and re-enrollments are nearly identical, with a similar second-semester drop in re-enrollment rates.

Both demand (students) and supply (schools) factors may explain this trend. For instance, students who disenroll in the second semester might strategically delay re-enrollment, opting to restart the following year or take free exams. Likewise, schools might prefer to incentivize re-enrollment in the new academic year to reduce re-enrollment costs. Beyond the underlying causes of this trend, it seems important to distinguish between within-year and between-year dropouts, as the causes may differ. Although the dropout reports target both groups, the timeline of our evaluation allows us to focus only on within-year dropouts: they are the target population of our study.

Appendix Table A.I provide descriptive statistics for within-year dropouts throughout the pre-treatment period March 1^{st} through May 30^{th}, 2022, i.e., previous to the implementation of the dropout reports. These dropouts sum 42,644 in total, corresponding to 1.71% of students in the Chilean public system. Among them, 68% managed to re-enroll before May 30^{th}, while the resting 32% remained as dropouts. Note that a small fraction (5%) of dropout students who re-enrolled did so at their origin school. This is likely because schools have little incentive to re-enroll dropouts, who often underperform. Similarly, dropout students may be hesitant to return if their decision to leave reflects dissatisfaction with the origin school.

Dropout students are disproportionately male and more likely to be migrants. Compared to their classroom peers at their previous school, dropout students exhibit significant academic lag, which is evident in lower school attendance, GPA, standardized test scores, and graduation rates[17].

[15]Throughout the text, we use the terms "disenrolled student" and "withdrawn student" interchangeably.
[16]Throughout the analysis, we treat each dropout event as a separate dropout throughout the analysis.
[17]For standardized academic achievement tests, we utilize the 2018 National SIMCE Assessment in Language and Math,

When compared to their new classmates in the re-enrolled schools, re-enrolled dropouts continue to exhibit poorer performance. However, the performance gap narrows significantly, indicating an academic stratification of disenrollment dynamics. Finally, we analyze how dropout students who did not manage to re-enroll compare with their re-enrolled counterparts. As expected, not-re-enrolled dropout students show poorer performance across all dimensions. They are, on average, one-third of a year more lagged in their educational trajectory; their school attendance is 4 *pp.* lower; they have significantly lower GPAs and perform worse in standardized tests, all of which lead to substantially lower graduation rates, on the order of 8 *pp*.

3. The Dropout Reports

As school principals in Chile are required to report to the centralized SIGE platform whenever a student enrolls or disenrolls from the school, MINEDUC is able to identify dropout students (i.e., those who withdrew from a school without transferring to another), something of which the school of origin may not be aware of as they are not responsible for tracking disenrolled, former students of the school. Specifically, MINEDUC created the "Reports for Monitoring Students with Interrupted or Irregular Educational Trajectories" (henceforth "Dropout Reports" or just "Reports"), which are automated, school-specific reports listing former students who had left school in the past 12 months and had not re-enrolled elsewhere. The report includes aggregate statistics on dropout behavior at the school level, as well as specific information on each dropout student, encouraging the school to facilitate their re-enrollment. These reports were then sent to each school.

Appendix Figure A.III illustrates how the Centralized Dropout Monitoring System works in practice. Note that all the information included in the report is embedded in the centralized SIGE system, thus automating the production and delivery of the dropout reports incurs virtually zero cost, which enhance its potential in terms of cost-effectiveness and scalability.

Appendix Figure A.IV presents a visual reference. The report commenced with a comprehensive overview of the school's dropout rate, calculated as the share of students who, having enrolled in 2021, did not enrolled by March 1^{st}, 2022 (initial date of 2022 school year) or enrolled by that date but later withdrew and did not re-enrolled in any state-recognized schooling institution by May 30^{th}, 2022. The report then provides a roster of disenrolled students under dropout status by May 30^{th}[18]. The dropouts list comes in a detailed table capturing dropout-level information, including full name, social security number, gender, age, and the municipality of residence from the preceding year, as well as the grade level they were enrolled in and grade completion status.

Importantly, even when schools are aware of a student's dropout status, they may have little incentive to bring them back as dropouts tend to underperform academically, and schools face no consequences for failing to track them. Likewise, students who have already chosen to leave may be reluctant to return to the same school, further reducing their likelihood of re-enrollment. Therefore, the report does not require schools to re-enroll former students in their own classrooms but

as it is the most recent year with census-level data. Data for subsequent years is unavailable due to the social unrest in 2019 and the disruptions caused by the COVID-19 pandemic in 2020-2021.

[18]We exploit that cutoff date as an exogenous shifter of the probability of being included in the report to identify the causal effect of the report (more in Section 4).

instead encourages them to activate their institutional and spatial networks to facilitate re-enrollment, whether in the same school or another.

The report was disseminated in June 28^{th}, 2022, across the universe of publicly funded schools in the country, and principals were able to download their school-specific report from the SIGE-linked School Community Platform[19]. Our target population are 5,868 schools with disenrolled students under dropout status by May 30^{th}, 2022, such that their reports effectively included a list of dropout students[20]. On average, the share of dropouts listed in the report relative to total school enrollments is 1.04% for primary school, 1.06% for middle school, and 1.4% for high-school students. As is shown in Appendix Figure A.V, the distribution of the dropouts share per school has a larger mass in the right tail for the case of high school students versus primary and middle counterparts, meaning dropout students are disproportionately represented by students in older cohorts. Note, however, that the share of students listed in the reports is still very small, meaning the potential spillover effects from listed to not-listed classmates are likely to be very limited[21].

Data and Timeline. Our study focuses on the 2022-2024 period and examines students who disenrolled in the proximity of the cutoff date for report generation, May 30^{th}, 2022. The primary outcomes of interest include indicators of school re-enrollment and graduation among dropout students, as well as their involvement in criminal activity. As is detailed in Appendix Section B, we use several official administrative data from MINEDUC on student-level enrollment, graduation records, grades, and demographics and school-level observables, as well as SENAME administrative data on criminal records of juvenile population.

Figure 1: Timeline

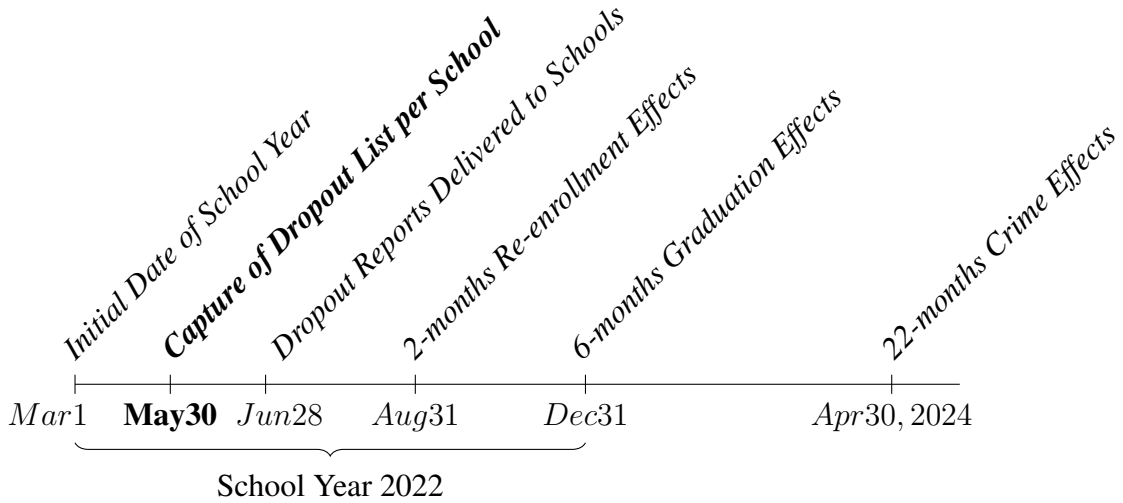


Figure 1 provides a timeline of our research design. The school year's initial date is March 1^{st}, 2022. On May 30^{th}, 2022, MINEDUC compiled the list of students per school who disenrolled up to

[19]The School Community Platform is the official communication channel between MINEDUC and schools. School principals typically check it on a weekly basis in order to gather new information provided by MINEDUC, for which login credentials are provided privately by MINEDUC.

[20]In case the school had not disenrolled students under dropout status by May 30^{th}, 2022, the list of dropout students was empty. Later in Section 7 we exploit this variation to test for accountability effects.

[21]Estimating spillover effects on non-listed students is out of the scope of this paper.

that date, and had not by the time re-enrolled in any school (i.e., dropouts), and generated the school-specific dropout reports. Then, one month later, on June 28^{th}, MINEDUC makes available the report in the School Community Platform so that schools can download it for their revision. MINEDUC data on student-level enrollment is available for August 31^{st}, which serves as benchmarks for measuring 2-months impact on re-enrollment rates. Then, MINEDUC administrative records on 2022 grade completion are used to measure 6-months graduation effects. Finally, 2022-2024 crime records from the SENAME administrative data are used to estimate the report's impact on criminal behavior up to 22 months after delivering the reports.

4. Identification

Our target population are dropout students, i.e., those who disenrolled from a school without transferring to another for at least 24 hours[22]. The MINEDUC disenrollment data provide within-school, student-level cross-section variation in the withdrawal date around May 30^{th}, 2022, the cutoff date when the disenrollment data was captured for building the report[23]. The probability of being listed in the report depends on "when" (in which specific date) the student withdrew from the school, if before or after May 30^{th}. While all schools have an associated dropout report, not all students who disenrolled were listed in the report, but only those who disenrolled before or on May 30^{th} and did not re-enroll in any state-recognized schooling institution up to that cutoff date, i.e., they remained disenrolled by May 30^{th}. More concretely, by the end of May 30^{th}, MINEDUC captured the cumulative list of students per school who were disenrolled by that day, and produced the school-specific reports.

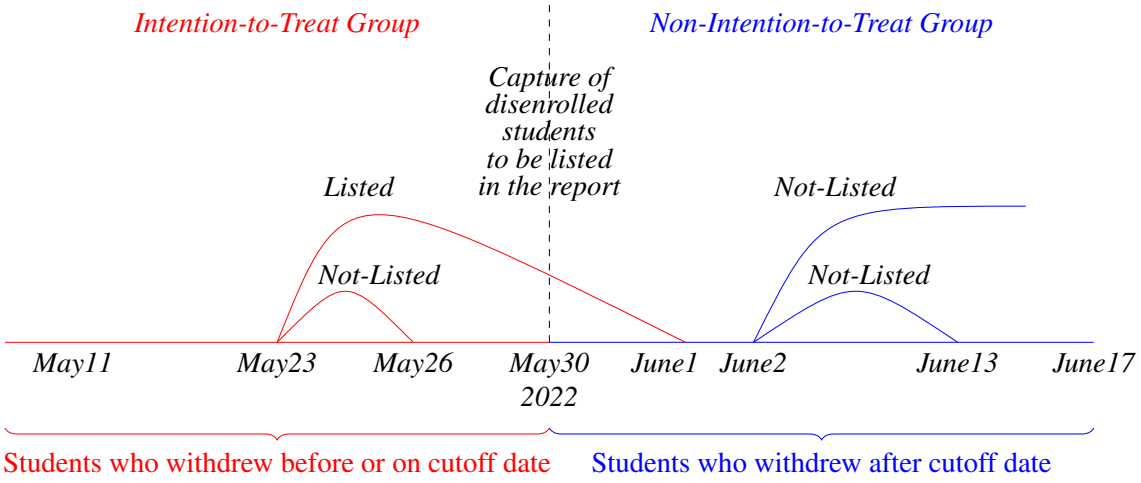
Figure 2 exemplifies the timing of report construction. For instance, if a student disenrolled from school on May 23^{rd} and re-enrolled in another school on May 26^{th}, i.e., before the cutoff date, then she is not listed in the dropout report as she does not appear as a disenrolled student by May 30^{th}. Instead, if she re-enrolled on June 1^{st}, i.e., after the cutoff date, then she is listed in the report since by the cutoff date she appeared as disenrolled from the school system. Likewise, students who disenrolled after the cutoff date were not eligible to be included in the report, and this is regardless of whether they re-enrolled or continued disenrolled afterwards.

Since the cutoff date is as-good-as-random, students who withdrew right before the cutoff date (i.e., those with high probability of being listed in the report) are expected to be similar than students who withdrew right after that date (i.e, students that would have been listed in the report with high probability if the cutoff date had been extended by a few days beyond May 30^{th}, 2022), such that comparing groups of disenrolled students at each side of the cutoff date works as an intention-to-treat estimate of the effects of the dropout report. The withdrawal date is reported by the school principal, hence we exploit within-school, student-level variation in the number of days between reported withdrawal date and cutoff date as an exogenous predictor of the probability of being listed in the report, and use the discontinuous variation in that probability to identify the causal effects of being

[22] Withdrawn students who re-enrolled in another school within 24 hours are not considered by the Chilean school system as dropout students but as "transfer students", in the sense that their educational trajectory in the system was not interrupted.

[23] Throughout the text, we use the terms "withdrawal date" and "disenrollment date" interchangeably.

Figure 2: Example of Report's Construction



listed in the report.

More formally, let w_{isd} be the date when student i in day-of-week d withdrew from school s. Our running variable $r_{isd} = w_{isd} - c$ captures the difference in school days from withdrawal to cutoff date c, fixed on Tuesday May 30th, 2022. The cutoff date c is assumed to be as-good-as-random such that, on average, students who disenrolled right before and right after the cutoff only differ in their probability of being listed in the report, with r_{isd} determining a fuzzy discontinuity in that probability around c.

Importantly, students who disenrolled before the cutoff date could still re-enroll in other schools before the arrival of the cutoff date, in which case they are not listed in the report. Moreover, the larger the time window between the disenrollment and the cutoff date, the higher is the chance of re-enrolling, and thus the lower the chance of being listed in the report. Hence, for a sufficiently small time window r_{isd} (i.e., considering disenrolled student with limited time to re-enroll), we have that $lim_{r_{isd}\uparrow 0}\mathbb{E}[T_{isd}|c - r_{isd}] > lim_{r_{isd}\downarrow 0}\mathbb{E}[T_{isd}|c + r_{isd}]$, with $T_{isd} = 1$ if the student is listed in the report and zero otherwise. However, in the absence of the report ($T_{isd} = 0$), the probability of re-enrollment (Y_{isd}) is, in expectation, equally distributed across the two sides of the cutoff, i.e., $lim_{r_{isd}\uparrow 0}\mathbb{E}[Y_{isd}|c - r_{isd}] = lim_{r_{isd}\downarrow 0}\mathbb{E}[Y_{isd}|c + r_{isd}]$. More generally, we assume the unobserved characteristics of students determining school re-enrollment are continuous across the cutoff date, i.e., students disenrolling right before or right after the cutoff date only differ in terms of their chances to be listed in the report. Therefore, any post-treatment difference in re-enrollment rates across groups can otherwise be attributed to differences in the probability of being listed in the report, and not to other potential causes[24].

[24]Note that ours is not a pure RD-in-time design where variation comes from the same unit of analysis observed before and after the cutoff date (Hausman and Rapson, 2018). In contrast, our design leverages across-student variation over a time variable (disenrollment date) that determines the probability of being treated based on a cutoff date that is common to all students in the sample.

4.1. Evaluation Sample

Identifying Disenrolled Students. For each school receiving the report, we first identify the list of students who disenrolled in 2022, and then use the withdrawal date and optimal bandwidth procedures to restrict the sample to disenrollments in dates sufficiently close to May 30^{th}, 2022, our cutoff date.

The enrollment records from the SIGE platform indicate whether a student is enrolled or has disenrolled from a school. Based on the criteria used by MINEDUC to define the target population of the reports, we take all students in publicly funded schools from grades 1 to 12 who were enrolled in 2021 and should enroll in 2022, for a total of 2.5 million students. The latter excludes students who graduated from 12^{th} grade in 2021, as they are not expected to enroll in 2022. Additionally, we exclude students from the private paid sector (7% of students population) and those in special or adult education (<1%), as they were not part of the initiative.

We define disenrolled students as those who, having enrolled in 2021, did not enrolled by March 1^{st}, 2022 (initial date of 2022 school year) or enrolled by that date but disenrolled later in the year, totalling 240,000 students. Approximately 150,000 disenrollments and re-enrollments in our data occur on the same day or even before a disenrollment is declared. These are identified as “transfer" students, and MINEDUC treat these cases as uninterrupted enrollments, thus we exclude them from the analysis[25,26]. In all, our sample of disenrollments end up containing 90,013 cases, all of which experienced at least 24 hours of disenrollment in state-recognized schooling institutions, and thus are considered dropouts.

Optimal Bandwidth. The MINEDUC disenrollment data provide within-school, student-level cross-section variation in the withdrawal date around May 30^{th}, 2022, which we use to build our running variable, defined as the number of days between withdrawal date and cutoff date. To estimate the optimal bandwidth, we use pretreatment data from 2016-2021 (excluding 2020 due to school closure), with student graduation as the outcome. This approach ensures the selected bandwidth remains unaffected by the treatment. In particular, we use the common mean squared error (MSE) optimal bandwidth selector (Calonico et al., 2014) across five pre-treatment years, and estimate a first degree polynomial to obtain an optimal bandwidth for each year. We then replicate the exercise but controlling for pre-treatment graduation and day-of-week fixed effects to capture time-varying potential confounders associated to our running variable. As is shown in Appendix Table A.IV, the estimated optimal bandwidths are relatively consistent across pre-treatment years and models, with these spanning between 9 and 14 days from cutoff date for first degree polynomial models. Drawing from the year closest to the treatment, i.e., year 2021, we use a window of $\pm$ 14 days as the optimal bandwidth.

Note that identification comes from within-school, student-level cross-section variation, hence we keep only schools where there is at least one dropout student at each side of the cutoff, for a total of

[25] For robustness, in Section 5.1 we perform a falsification check using a sample restricted to transfer students.

[26] A small group of disenrollments made on holidays and weekends was also removed (approximately 0.5% of the sample universe), thereby ensuring each week accounted for 5 days. Finally, we excluded 75 cases of students over 20 years old.

12,068 dropouts distributed across 2,330 schools. This is our evaluation sample for the first-degree polynomial model, which is our main specification[27,28]. Then, using the same criteria (year 2021), for models involving a second degree polynomial we determined a ± 18-days as optimal bandwidth (N=15,098). As we later detail in Section 5.1, our results are consistent across different bandwidth selections, including all combinations of optimal bandwidths presented in Appendix Table A.IV, regardless of the pre-treatment year, and this is the case for both linear and quadratic models.

4.2. Descriptive Statistics

Appendix Table A.II displays pre-treatment summary statistics of dropout students, and how they compare to the full sample of students enrolled in the educational system by 2022. First, dropout students in 2022 (column 2) are different relative to the universe of enrolled students in the same year (column 1). For instance, they are more likely to be male, and 12% of dropout students are migrants compared to only 6% among their enrolled counterparts. Over half of dropouts are in high school, confirming that dropout rates are higher among secondary education students. Second, dropout students are more vulnerable: they are more likely to be classified as prioritary students, i.e., students who are from quintile 1 and 2 in the vulnerability distribution. The latter is also reflected in poorer educational outcomes, e.g., lower school attendance (83% vs. 87%), lower GPA (5.5 vs. 5.9), and lower graduation rates (91% vs. 97%).

In terms of school level characteristics, dropout students come from schools of similar size relative to enrolled students. However, dropout students come disproportionately from municipal schools (instead of publicly funded voucher schools administered by a private owner), which generally capture students from lower socioeconomic status, and around two third of dropout students come from low SES schools compared to only 52% among enrolled students. As a result, they study in schools with significantly lower SIMCE test scores, on average, and this is the case using both math and language tests. Overall, the very nature of dropout students contrast with that of their enrolled counterparts, as the first typically exhibit greater vulnerability in educational and social indicators. The latter suggests that any generalization of our conclusions to a broader population beyond dropout students should be approached with caution.

Finally, note that dropout students in the evaluation sample, i.e., those who dropped out in dates within the optimal bandwith (column 3) are very similar to the full sample of dropout students, with the magnitude of the differences across samples being generally quite small and close to zero. Some variables show statistically significant differences across groups, yet this is mostly due to large sample size. This implies the withdrawal date does not play a major role in terms of sample selection, suggesting the external validity of our results is not confined to the specific time window we analyze. Therefore, our results are expected to exhibit greater external validity than

[27] Few cases dropped out more than once within the optimal window of ±14 school days, which represent 0.54% of the evaluation sample. We treat them as separate dropouts throughout the analysis.

[28] As we see later in Section 4.4, the probability of being included in the report is not uniform across the running variable but it increases as the withdrawal date get closer to the cutoff date. Hence, observations in our main specification are weighted by a triangular kernel approximation. The latter reduces the sample size from 12,098 to 11,268, and this is because the triangular kernel assigns a zero weight to observations in the extreme (left and right) margins of the optimal window.

is typically found in studies using RD designs. However, the latter requires to assume time-stable re-enrollment incentives for both students and schools, which might or might not be the case. For instance, students who drop out close to the end of the school year (e.g., November) may have higher incentives to re-enroll relative to those who dropped out at the beginning of it (e.g., March) since the graduation prize requires a smaller effort in terms of time in school. Likewise, schools may have larger incentives to re-enroll students who are close to graduation as it implies increasing graduation rates at a relatively low cost.

4.3. Internal Validity

Manipulation of Disenrollment Date. An internal validity threat is that school principals manipulate the withdrawal date reported to the SIGE system in order to avoid their students appear in the dropout report[29]. For instance, if a student disenrolled on May 25^{th}, the school principal may want to report that she disenrolled on June 1^{st}, i.e., after the cutoff date, such that she is not listed in the report, thereby violating the identification rule. This is implausible for various reasons. First, the reports were delivered to school principals about one month after the cutoff date, on June 28^{th}, i.e., the principals got informed about the list of dropout students contained in the report long after the cutoff date used to build the report, preventing them to manipulate the list of students that were included in the report. Unless the school principals were unofficially informed by MINEDUC about the timeline of the report's design before its implementation, there are no obvious reasons for school principals to manipulate the reported withdrawal dates. The dropout reports initiative is not something they could expect either as no interventions of this sort had been previously implemented. Finally, even if school principals were informed about the cutoff date, there are no clear incentives for misreporting as having high or low dropout rates (as it would be reflected in the dropout report) is not associated with penalization on the side of MINEDUC[30].

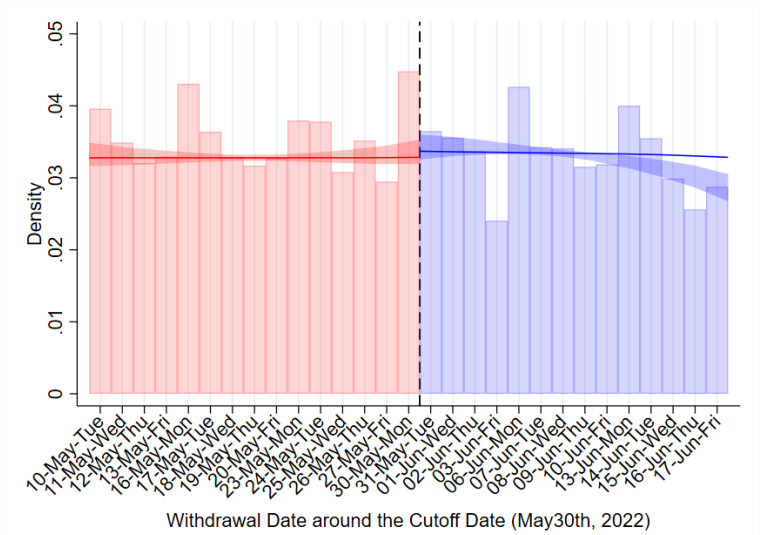
We examine the possibility of manipulation of reported withdrawals by using McCrary (2008)'s density test which verifies whether the density of reported withdrawals varies around the cutoff date. First, as is shown in Figure 3, the distribution of reported disenrolled students around May 30^{th} is not uniform but exhibit some seasonality, with reports being more likely to occur at the beginning of the school week. Indeed, schools are disproportionately more likely to report disenrollments on Mondays than on any other day of the week. Still, we find no graphical evidence of date manipulation in withdrawals reporting around the cutoff date. For our optimal bandwidth of $\pm$ 14 days, the difference in the probability of withdrawal reports before and after the cutoff is statistically indistinguishable from zero, and this is the case for both conventional and bias-corrected point estimates, suggesting no strategic misreporting in disenrollment date by schools[31].

[29] Chilean schools have shown to act strategically when reporting school quality signals. For instance, Cuesta et al. 2020 document that low-performing students are underrepresented in test days, generating distortions in information related with school quality.

[30] Certainly, schools that do not report a disenrollment are subject to high penalizations from MINEDUC as they would receive a voucher for a student not enrolled in the school, yet such type of misreporting is out of the scope of our identification strategy.

[31] We replicate the test for the entire range of optimal bandwidths specified in Appendix Table A.IV ($\pm$ 9 days through $\pm$ 18 days), and only in 1 out of 10 tests we reject the null hypothesis of no manipulation at the standard levels of

Figure 3: Distribution of Reported Disenrollements across Withdrawal Dates around the Cutoff Date



Notes: Distribution of reported disenrollments across the number of school days from withdrawal date to cutoff date of Monday May 30^{th}, 2022, using $\pm$ 14 days as optimal bandwidth. Each value of the running variable (X axis) is associated to a given day of the week. A density estimation plot and its associated 95% CI is overlaid, which provides a smooth representation of the density of disenrollments across the timeline, allowing for a shift in density at the cutoff date. The local polynomial order used to construct the density estimator is is 1 (p(1)). Then, for the bias-corrected (robust) point estimate (and its associated confidence interval) to be (I)MSE optimal, a second order local polynomial is used (q(2)), and that explains why the confidence intervals are not centered at the point estimate in the graph. The bias-corrected (robust) *p*-value associated to the null hypothesis of no difference in the probability of withdrawal before and after the cutoff date is 0.5533.

Baseline Balance. Appendix Table A.III assesses the statistical comparability of before- vs. after-cutoff dropout students in our evaluation sample across numerous predictors of dropout, including demographics and background characteristics such as gender, age, socioeconomic status (SES), migrant status, parental education, and income, as well as pre-treatment performance indicators like GPA, school attendance, and graduation rates, among others. We test for mean differences by regressing the pre-treatment outcome against the running variable, the before-after dummy, and their interaction, and controlling for day-of-the-week and school fixed effects. As is observed in Panels A and B, the mean differences across groups around the cutoff are small in magnitude and precisely estimated, suggesting statistical balance between dropout students on either side of the cutoff. There seems to be some imbalances in the share of students in middle and high school, suggesting a breakdown analysis by grade could be appropriate.

In terms of outcomes, grade completion is well balanced across groups in pre-treatment years (Panel C). Naturally, lagged graduation is not observed for all students in all years, because some had not yet reached schooling age to get enrolled in school and that explain the lower number of observations. Hence, sample selection issues may arise. We thus further test whether treatment and control group students are balanced in terms of missing information in lagged graduation, for which we create a dummy that equals 1 if the observation is missing in any of the lagged graduation years and zero otherwise. We do not reject the null hypothesis of no differences across groups, suggesting

statistical significance, which is what we would expect to occur only by chance.

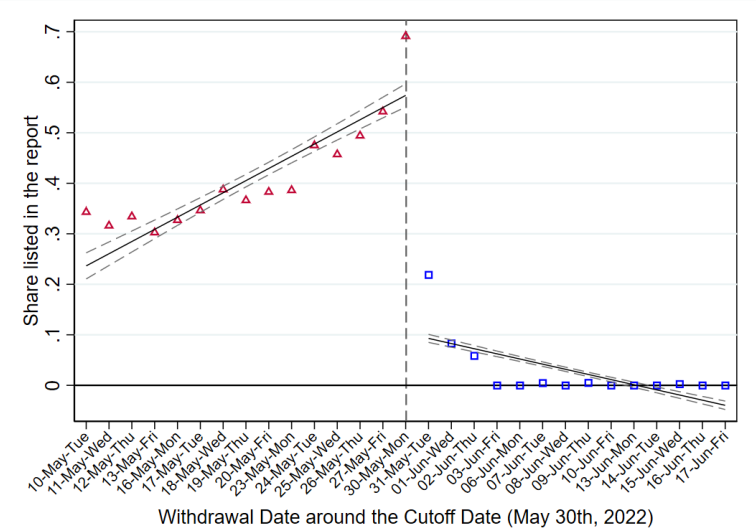
selected attrition does not affect treatment balance in pre-treatment graduation.

Finally, we test for statistical balance on the probability of re-enrollment (Panel D). For dropout students in the pre-cutoff group, we create a dummy that equals 1 if the dropout student re-enrolled before the cutoff date (May 30^{th}), and zero otherwise. We do the same for the post-cutoff group, but use a placebo cutoff date that counts 14 school days after the cutoff (June 17^{th}), such that the maximum time window for re-enrollement is equal for both before- and after-cutoff dropout students. The balance test shows no evidence of differential selection across pre- and post-cutoff groups in re-enrollment behavior.

4.4. Treatment Compliance

Report's Listing: Student-level Supply. A relevant condition for causal identification implies the running variable predicts a discontinuous variation in the student's probability of being listed in the report, i.e., that students who disenrolled right before the cutoff date are significantly more likely to be listed in the report than their counterparts who disenrolled right after it.

Figure 4: Probability of being listed in the report as a function of timing of disenrollment



Note: Probability of being listed in the report as a function of the timing of disenrollment date relative to the cutoff date, Monday May 30^{th}, 2022, using $\pm$ 14 days as optimal bandwidth. Each value of the running variable (X axis) is associated to a given day of the week. The running variable counts the number of school days between the disenrollment date and the cutoff date, which indicate the point in time at which disenrollment data was captured for the report's construction (vertical dashed line). Each square represents the likelihood of being listed in the report conditional on have been disenrolled in that specific date. Red triangles indicate days before the cutoff date and blue squares represent days after the cutoff date. The solid lines, separated for the periods before and after the cutoff date, show the fitted linear prediction of the marginal effect of one-day approximation to the cutoff date on the probability of being listed in the report (95% C.I. in dotted lines). For before-cutoff disenrollments (in red), approximating to the cutoff date in one day increases the chances of being included by 2.1 *pp.*, on average (Coef.=0.021; SE=0.001). For after-cutoff disenrollments (in blue), approximating to the cutoff date in one day increases the chances of being included by 1 *pp.*, on average (Coef.=0.010; SE=0.001).

Figure 4 plots the probability of being listed in the report by disenrollment date at each side of the cutoff date, using the optimal bandwidth of $\pm$ 14 days. 41% of students who withdrew from

school before the cutoff date ended up being listed in the report. This is because dropout students still had the chance of re-enroll before the cutoff date comes in, the majority of which did so and thus were not listed[32]. Second, the probability of being listed in the report increases with the proximity to the cutoff date. For instance, about 55% of students who disenrolled on Friday May 27^{th} ended up being listed in the report, but only 35% of those who disenrolled 10 days before did so. Thus, having a couple of more days to re-enroll decreased the listing rate. Overall, for every additional day of approximation to the cutoff date, the probability of being listed in the report increases by 2.4 *pp.*, on average[33]. Third, the chance of being listed in the report shows a small spike for after-cutoff students who disenrolled very close to the cutoff date, which represent 3% of the control group. This is mostly due to endogenous non-compliance induced by MINEDUC officials. In particular, MINEDUC identified some right-hand side withdrawn students as late-registration students who, based on their crossing administrative procedures, should have been reported before May 30^{th}, and thus decided to list them in the dropout report.

Report's Take-Up: School-level Demand. Producing and delivering the dropout reports is not a sufficient condition for changing the re-enrollment behavior of dropout students. Schools still need to open and read the report, and eventually take the necessary actions to re-enroll dropout students. According to the National Survey for Monitoring Education Schools (Canales et al., 2022), a representative survey including a battery of questions designed to analyze school's reception of the report, 84% of school principals reported to have downloaded the report, suggesting a high take-up rate. The report was well-received: half declared that would like to receive the report on a monthly basis, and 90% at least once every 6 months.

In a subsequent representative survey (MINEDUC, 2024), school principals were asked about the various ways they had used the reports, with the option to select more than one alternative. First, 90% of school principals declared the information about dropout students was correct and sufficient for re-enrollment purposes. Second, they reported to have primarily used the report to inform the management team (79%), to implement a monitoring system for tracking student attendance featured in the report (75%), and to contact the students listed in the report and/or their families (71%). Less than 1% of those who downloaded the report indicated that they did not use the report for any purpose. Overall, school principals grade the report with an average of 6.4 in a 1-7 scale.

5. Estimating Treatment Effects on Re-enrollment and Graduation

We exploit the exogenous variation in the distance from withdrawal to cutoff date to estimate the causal effect of being listed in the report on re-enrolling and graduation rates. We regress a re-enrollment dummy that equals 1 if the dropout student i in school s who withdrew on day-of-week

[32] A special case are students who disenrolled on May 30^{th} and did not re-enroll the same day. MINEDUC took the decision to give them 48 hours to re-enroll, otherwise they were listed in the report. Indeed, 65% of them were not able to re-enroll and thus were listed in the report.

[33] This is obtained by regressing a dummy of being listed in the report against a cutoff dummy, the number of school days between disenrollment and cutoff dates (i.e., our running variable), and their interaction, controlling for day-of-week and school fixed effects.

d re-enrolled by August 31^{st}, 2022 (two months after the report's release date of June 28^{th}, 2022), on whether the dropout student disenrolled before or after the cutoff date used to design the report (May 30^{st}, 2022), controlling for a flexible relationship between withdrawal and cutoff date as follows:

$$Y_{isd} = \alpha + \beta B_{isd} + \gamma_0 f(r_{isd}) + \gamma_1 f(r_{isd}) \times B_{isd} + \eta_s + \phi_d + X'_{isd}\gamma + \epsilon_{isd} \quad (1)$$

where B_{isd} is a "before" dummy that equals 1 if $c - r_{isd} \leq 0$ (i.e., the student withdrew before or on the report's cutoff date of May $30th$, 2022) and zero otherwise. $f(r_{isd})$ is a flexible function of the absolute time distance (in school days) of student i's disenrollment date to the cutoff date. The interaction term allows for the relationship between disenrollment date and the re-enrollment dummy to vary depending upon the time distance of disenrollment to the cutoff date. Our parameter of interest, β, pins down the causal effect of disenrolling immediately before the cutoff, which makes the dropout student discontinuously more likely to be listed in the report. We then replicate the analysis for the outcome being whether the student was promoted to the next grade in December 2022— six months after releasing the report.

Our identification comes from variation at the student level within each school, hence we control for school fixed-effects η_s, which capture school-level factors that are constant across students within the same school. Also, as emphasized by Hausman and Rapson (2018), employing a time-based RD design raises the concern of seasonality, where day-of-the-week effects might account for the observed discontinuities in outcomes instead of the treatment effect. This is particularly pertinent given the uneven distribution of withdrawals across school days around the cutoff. As was shown in Figure 4, withdrawals are more likely to occur at the beginning of the week, although this may vary across students within the same school. We thus also include day-of-the-week fixed-effects ϕ_d (e.g., Monday fixed effects, Tuesday fixed effects, and so on) to capture any within-week time trends that could simultaneously affect both the disenrollment date as well as the probability of re-enrolling afterwards. Finally, we also control for a set of pre-treatment covariates in vector X', including a dummy for whether the student graduated from the school in the previous year, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, a dummy if the student is classified as preferent, and a dummy if the student is in primary school.

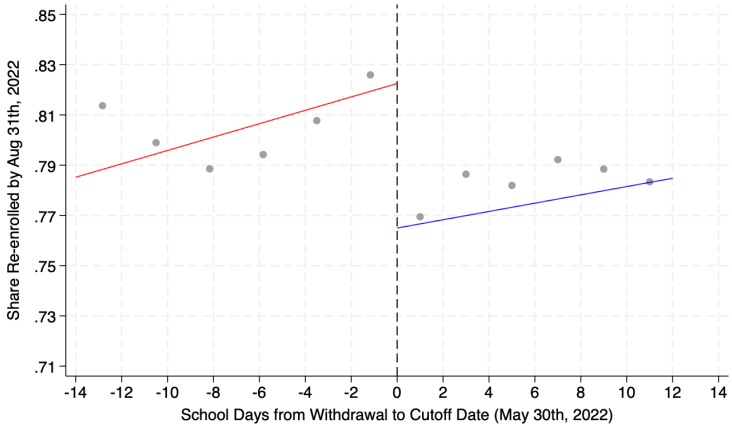
The identifying assumption behind our RD model is that the unobserved characteristics of students determining school re-enrollment and/or graduation are continuous across the cutoff date, and that "before" and "after" disenrolled students only differ in terms of their chances to be included in the report. Hence, following Gelman and Imbens (2019), we choose a linear functional form for $f(r_{isd})$ in our main specification. For robustness, the results are also reported under two alternative modeling specifications: a local linear polynomial regression (Calonico et al., 2014) and an OLS model with $f(r_{isd})$ being a quadratic function.

Note that our evaluation sample was selected based on how dropout students were assigned to the treatment, and this assignment was clustered at both schools and time, not generated by independent sampling. Namely, within each school, dropout students were assigned to the treatment or control group depending on whether the dropout student disenrolled before or after the cutoff date.

Hence, we follow Abadie et al. (2017) and cluster the standard errors at the school-group level[34,35,36]. Finally, our analysis from Section 4.4 shows that the probability of being listed in the report is not uniform across the running variable but increases with the proximity of disenrollment to the cutoff date. Consequently, observations are weighted by using a triangular kernel weight, such that observations more proximate to the cutoff are weighted more than their counterparts located farther away[37].

Results. Figure 5 shows the probability of re-enrollment by August 31^{st} (two months after the report's release) as a function of the distance —in school days— between disenrollment date and cutoff date. On average, 78% of students who disenrolled after the cutoff date were able to re-enroll in the school system. In contrast, students who disenrolled before the cutoff date show, in the limit, a re-enrollment rate that jumps up drastically. Importantly, the across-groups difference increases with proximity to the cutoff date, suggesting this is driven by the probability of being listed in the report.

Figure 5: Effect of disenrolling before Cutoff Date on School Re-enrollment



Note: Binned scatterplot of the share of re-enrolled students by August 31, 2022, as a linear function of the distance (in school days) between disenrollement date and cutoff date, controlling for school fixed effects. Optimal bandwidth is fixed at $\pm$ 14 school days. Red (Blue) bins aggregate the re-enrollment rate of students who disenrolled before (after) the cutoff date. The solid lines trace a linear fit at each side of the cutoff. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight.

[34] As stated by Abadie et al. (2017), "clustering is in essence a design problem, either a sampling design or an experimental design issue" (p.2). Since we are working with the universe of withdrawn students in 2022, there are no sampling design issues. Instead, units are assigned to groups based on whether the dropout student disenrolled before or after the cutoff date, with this process occurring separately within each school, justifying the clustering of standard errors at the school-group level.

[35] Our results are robust to either clustering the standard errors at the school level or by using robust standard errors.

[36] Since our running variable only takes a moderate number of distinct values on each side of the cutoff, one might consider following Lee and Card (2008) and Lee and Lemieux (2010) and cluster the standard errors by the running variable (CRV). However, as noted by Kolesar and Rothe (2018) (p.4, fn 3), when data are not generated by independent sampling, as is the case here, the choice between clustering by the running variable (CRV) or using the RDHonest procedure becomes less pertinent, and standard errors should instead be clustered at the sample design-based level.

[37] Our results are robust using alternative weighting schemes, including uniform weights.

Table 1: Treatment Effects on Re-enrollment and Graduation

	If Re-enrolled by Aug. 31^{st}, 2022				If Graduated by Dec. 31th, 2022			
	Loc.Lin. Poly. (1)	OLS Linear (2)	OLS Linear (3)	2SLS (4)	Loc.Lin. Poly. (5)	OLS Linear (6)	OLS Linear (7)	2SLS (8)
W.D. $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	0.061***	0.051***	0.048***		0.067***	0.056***	0.052***	
	(0.017)	(0.018)	(0.017)		(0.019)	(0.021)	(0.020)	
If listed in the Report ($T_{isd} = 1$)				0.105***				0.112***
				(0.037)				(0.043)
Ineligible Mean	0.780	0.780	0.780		0.642	0.642	0.642	
Eligible Complier Mean				0.7136				0.5831
Ineligible Complier Mean				0.6089				0.4707
Observations	11,268	11,268	11,268	11,268	11,268	11,268	11,268	11,268
Schools	2,330	2,330	2,330	2,330	2,330	2,330	2,330	2,330
Optimal Bandwidth	14	14	14	14	14	14	14	14
Polynomial Degree	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Graduated in 2021 (= 1)	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes
1^{st} Stage Estimate				0.460				0.460
Cragg-Donald F-Statistic				957.13				957.13
Kleibergen-Paap F-Statistic				665.28				665.28

Note: Sample of analysis are evaluation sample students. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022) and/or graduated from school by the end of the year (Dec 31^{st}, 2022). Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. The probability of being listed in the report is not uniform but increases with the proximity of disenrollment to the cutoff date, hence observations are weighted by using a triangular kernel weight. All models control for school fixed effects. Columns (1) and (5) estimate a local linear polynomial model, using the command `rdrobust` in `Stata`. Regression models in columns (2) and (6) show estimates from a linear OLS model, controlling for day-of-week fixed effects and a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year) or not. Regression models in columns (3) and (7) additionally control for a set of pre-treatment covariates, including gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, a dummy if the student is classified as preferent, and a dummy if the student is in primary school. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Columns (4) and (8) show 2SLS estimates of the effect of being listed in the report (T_{isd}) by exploiting whether the student disenrolled before or after the cutoff date (B_{isd}) as an instrument for being listed in the report. First stage estimate is 0.460 (significant at the 1% level of statistical significance) and is obtained from regressing T_{isd} against B_{isd}, a linear function of our running variable r_{isd}, and their interaction, controlling for the full set of covariates, as in main equation 1. 2SLS estimates come from estimating equation model 2, with the 2SLS coefficient being equivalent to the reduced form estimate of β from equation 1 divided by the first stage estimate. It also reflects the difference between the Eligible and Ineligible Complier Means computed following Abadie (2002). Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table 1 show the RD estimates. We present local linear polynomial estimates as well as estimates derived from a linear OLS, with and without controlling for the full set of covariates detailed in main equation 1. The re-enrollment effect ranges between 4.8 and 6.1 *pp.*, with the effect being statistically robust across models. Considering the full specification in column (3), the effect represents a 6.2% increase in re-enrollment relative to the counterfactual mean.

Then, in column (4), we analyze the causal effect of being listed in the report by using a fuzzy RD model that exploits whether the student withdrew before or after the cutoff date (B_{isd}) as an

instrument of being listed or not in the report (T_{isd}). First stage estimate is obtained from regressing T_{isd} against B_{isd}, a linear function of our running variable r_{isd}, and their interaction, controlling for the very same list of covariates as in main equation 1. Then, our second stage is:

$$Y_{isd} = \alpha + \beta^{2SLS}\widehat{T_{isd}} + \gamma_0 f(r_{isd}) + \gamma_1 f(r_{isd}) \times B_{isd} + \eta_s + \phi_d + X'_{isd}\gamma + \epsilon_{isd} \quad (2)$$

Our first stage estimate is large: students who disenrolled before the cutoff date are 46 *pp.* more likely to be listed in the report than their after-cutoff counterparts. As a result, our 2SLS estimates somewhat double in size relative to intention-to-treat figures, for an effect size of 10.5 *pp.* In order to interpret the order of magnitude of this result, we follow Abadie (2002) to estimate the probability of re-enrollment among ineligible compliers (i.e., disenrolled students who were ineligible for being listed in the report and were indeed not included in it) through the following specification:

$$Y_{isd} \times (1 - T_{isd}) = \delta^{2SLS}(1 - T_{isd}) + \gamma_0 f(r_{isd}) + \gamma_1 f(r_{isd}) \times B_{isd} + \eta_s + \phi_d + X'_{isd}\gamma + \epsilon_{isd} \quad (3)$$

, where $(1 - T_{isd})$ is a dummy that equals 1 if the disenrolled students was not listed in the report. When instrumenting $(1 - T_{isd})$ by the eligibility indicator B_{isd}, the 2SLS coefficient δ^{2SLS} reflects the average outcome for ineligible compliers[38].

We estimate that 61% of listed students in the ineligible group re-enrolled in the school system. The latter suggests the local average effect of being listed in the report is large in magnitude, representing a 17% (=0.105/0.609) increase in re-enrollment relative to ineligible compliers.

The gains in re-enrollment then translate into gains in graduation rate. While 64% of students who disenrolled after the cutoff date managed to graduate from school by the end of the year, this raises to 69%-71% among intention-to-treat students, with the effects being robust across models. According to the 2SLS specification in column (8), being listed in the report increased the graduation rate by 24% relative to the graduation rate among ineligible compliers. To have an order of magnitude of the effect, consider the total number of disenrolled students who would have not graduated by the end of the year in the absence of the report, which amounts to 47,644 students[39]. If we extrapolate the treatment effects to all disenrolled students in 2022 beyond those included in the time window used for the RD design, i.e., if all disenrolled students were included in the report, then the number of non-graduated students would be reduced to 37,563 students, meaning 10,082 students or about 21% of disenrolled students would graduate from school due to the report.

5.1. Robustness Checks

Functional Form. Appendix Table A.V replicates the results adding the running variable squared plus an interaction of it with the "before" cutoff dummy to model in equation 1. All the results hold under this quadratic specification, suggesting the effects are not sensitive to the assumed relationship between the running variable and the outcomes of study.

[38] Likewise, by replacing $(1 - T_{isd})$ by T_{isd} at each side of equation 3 we recover the mean outcome for compliers in the eligible group. Also, the mean difference across eligible and ineligible compliers is equivalent to β^{2SLS} in eq. 2.

[39] This is calculated as the total number of disenrolled students during 2022 (=90,014) times the share of ineligible compliers who did not graduated by the end of the year (1-0.4707).

Pre-treatment Effects. We conduct a falsification test that compare how before and after-cutoff students in our evaluation sample differ in their graduation rates throughout the pre-treatment period 2016-2021. Appendix Figure A.VI summarizes the yearly dynamics of graduation effects using our optimal bandwidth of + 14 days, where for each pre- and post-treatment year, we plot the estimate of parameter β in equation 1 (i.e., model (2) in Table 1) and its associated 95% confidence interval. The effects are shown to be statistically not different from zero in pre-treatment years, yet large and significant after the implementation of the reports.

Robustness to alternative bandwidths. We test the robustness of the results to alternative time windows. Appendix Figure A.VII presents point estimates and their associated 95% confidence intervals for the β parameter from our main equation 1 using a wide range of bandwidths around the cutoff date of May 30^{th}, 2022. Our analysis cover 7 to 20 school days, i.e., one month in total[40]. We observe that the effect on graduation is robust across the full range of bandwidths. The effect grows closer to the cutoff, which is expected since the treatment compliance rise as the disenrollment approaches to the cutoff date, yet the precision decreases due to smaller sample size.

Second, in Appendix Figure A.VIII we replicate the analysis for the pre-treatment period 2016-2021, and find that regardless the pre-treatment year and bandwidth we pick, we never reject the null hypothesis of no effect. The latter reinforce our assumption that the cutoff date works as an identification rule and thus the effects observed in 2022 are indeed attributable to the report.

Finally, Appendix Figure A.IX extends the robustness analysis by using year-specific cohorts in pre-treatment years, i.e., for each year-cohort of disenrolled students, we test whether disenrolling before or after May 30^{th} each pre-treatment year makes any difference in graduation rates[41]. The differences are shown to be mostly null across pre-treatment cohorts, when the reports did not exist. If anything, only in some bandwidths of one out of five pre-treatment years (2019) we find the difference is statistically different from zero at the 90% confidence level, which is what we would expect to happen by chance.

Falsification test using alternative cutoff dates. We replicate our analysis but using April 29^{th} as a pre-treatment placebo cutoff date, i.e., we re-estimate the effects on re-enrollment and graduation but using disenrolled students who withdrew from school within $\pm$ 14 school days from this placebo cutoff date as the sample of analysis[42]. As Appendix Table A.VI shows, we do not reject the null hypothesis of no effects, and this is the case for the two outcomes under study.

Falsification test using transfer students. As detailed in Section 4.1, students who withdrew and

[40]Naturally, the sample size shrinks as we approach to the cutoff date. Unfortunately, the number of observations becomes too small to make accurate predictions of the treatment effects in the range of 1 to 6 school days from the cutoff date, and thus we exclude them from this analysis. We do not believe this should be a major concern since, as we showed in Section 4.1, the common mean squared error (MSE) optimal bandwidth selector (Calonico et al., 2014) never suggests optimal bandwidths in this range, but these span between 9 and 14 days from cutoff date for first degree polynomial models, and between 12 and 18 days for second degree polynomial models.

[41]We exclude 2020 from the analysis, as no official enrollment data was released that year due to school closures.

[42]We choose April 29^{th} because it is the penultimate date of April, as it is the case of May 30^{th} for May.

re-enrolled in another school within the next 24 hours after withdrawal are identified as “transfer" students. We exclude transfers from the analysis as MINEDUC treat them as withdrawn students with uninterrupted enrollment who are not required to be included in the dropout report. Hence, we should expect no effects of the report on re-enrollment and graduation behavior for transfer students. Indeed, as is shown in columns (3) and (6) in Appendix Table A.VI, when the effects on re-enrollment and graduation are estimated using transfer students, we find null effects, and again, this is the case for the two education outcomes under study.

Robustness to Inclusion in Subsequent Dropout Report. By the end of the year, specifically on December 1^{st}, 2022, MINEDUC sent a subsequent report to schools which updates the list of dropout students included in the first report. Since we found positive re-enrollment effects from the first report, then we should expect the probability of being listed in the December report decreases for those who were eligible to be listed in the first report. We test for this in Appendix Table A.VII, where we find that students who disenrolled before the cutoff date are significantly less likely to be listed in the December report relative to their ineligible counterparts. The latter reinforces our evidence on the positive re-enrollment effects from first report, and second, it provides credibility to that dropout reports are accurate and they effectively include dropout students.

Multiple Hypothesis Testing. In studies with multiple outcomes, statistically significant effects may arise purely by chance — the more tests conducted, the higher the likelihood of incurring in a Type I error. We thus apply Bonferroni family-wise error rate (FWER) corrections, adjusting the p-values of individual tests based on the number of outcome variables (Bonferroni, 1936)[43]. Specifically, we calculate Bonferroni FWER corrections at the 10% significance level by dividing the desired test size (10%) by the total number of outcome variables, which in our case is two: school re-enrollment and graduation, meaning we reject the null of no effect only if $p_i \leq \frac{0.1}{2} = 0.05$. While this seems the standard cutoff for statistical validity in social sciences, the multiple test will become even more stringent as we test for heterogeneous effects in later sections, as subgroup analysis increases the number of outcomes under evaluation.

We find the statistical validity of our results remain unchanged under this stringent test, and this is the case for all the specifications in main regression results from Table 1 as well as for all the robustness tests presented in the corresponding appendix tables.

5.2. Heterogeneous Effects by School Grade

As emphasized by Rumberger (2020), worldwide the dropout phenomenon is predominantly a concern among high-school students. Chile is not an exception as the bulk of dropouts are 9-12^{th} grade disenrolled students, who collectively contribute to two-thirds of the total dropout rate, and

[43]The Bonferroni correction for multiple hypothesis testing is often unpopular as it is overly conservative. Let $H_1, \ldots, H_q$ be a family of null hypotheses and let $p_1, \ldots, p_q$ be their associated p-values. Let q be the total number of null hypotheses, and let q_0 be the number of true null hypotheses (unknown). The family-wise error rate (FWER) is the probability of rejecting at least one true H_i, or, equivalently, making at least one type I error. Then, the Bonferroni correction implies rejecting the null hypothesis for each $p_i \leq \frac{\alpha}{q}$, with α the test size, typically set at the 10% significance level.

most of the upward trend in dropout rates in recent years are explained by high-school students (see Section 2). This is in part due to that students in primary and secondary school enjoy less autonomy on dropout decisions than high school counterparts. Also, dropout students in high school may be costlier to bring back to school since their greater autonomy makes them less enforceable.

Appendix Table A.VIII presents treatment effects across separate sub-samples of students in primary (1-4 grades), middle (5-8), and high-school (9-12). The report's impact on re-enrollment is mostly driven by high-school dropout students, the only subsample for which we reject the null hypothesis of no report effects[44]. While the estimated coefficients between subsamples are not statistically different under conventional level of statistical significance, the difference in order of magnitude of the effects are sizable. According to the reduced-form model in columns (5) for high-school dropouts, the intention-to-treat effect on re-enrollment represents an 11% increase relative to the ineligible mean, which almost doubles the relative change observed in the 5-8 grades (column (3)) and is about 5 times larger compared to the relative change among dropouts in 1-4 grade (column (1)). This is also the case of graduation effects. The differential magnitude of the effects among high-school disenrollments is more than 100% relative to the effects among primary school counterparts, and almost 200% relative to middle school dropouts[45]. Overall, this is not an unexpected, as the re-enrollment efforts are largely concentrated on high school students, who constitute about 70% of listed cases in the reports. Second, baseline re-enrollment rates among high school dropouts are much lower, meaning its potential growth is naturally higher[46].

Next, according to the 2SLS specification in column (6), being listed in the report increased the re-enrollment rate by 27% relative to the re-enrollment rate among ineligible compliers. The 2SLS effect on graduation rate among high-school students is even higher, on the order of 55% (column (12)). A back of the envelope calculations suggest that in the absence of the reports program, 23,683 high-school students who disenrolled during 2022 would have not graduated from school by the end of the year[47]. For extrapolation, if all high-school students who disenrolled from school during 2022 were included in the report, then the number of non-graduated students would have reduced to 15,382 students, meaning 8,301 students or 35% of non-listed disenrollments would have graduated from school due to the report.

[44]By design, we keep only observations from school-grades with at least one disenrollment at each side of the cutoff.

[45]There seems to be some effects on dropout students from primary school as well, yet the treatment coefficients are too imprecise to be statistically significant, possibly due to small sample size. Anecdotal evidence suggests 1-4th grades students listed in the report were perceived as prioritary cases and thus more urgent to help re-enroll due to their greater vulnerability compared to older dropout students. Likewise, they are typically less costly to re-enroll than older cohorts of disenrolled students. The latter could have influenced the considerable effect size we find for this subgroup.

[46]The statistical validity of the subgroup analysis holds when making inference under Bonferroni corrected p-values. Specifically, we have 3 subgroups (1-4; 5-8; 9-12) and 2 outcomes (re-enrollment; graduation), meaning there are 6 potential outcomes under analysis. This implies the Bonferroni-corrected p-value is calculated as 0.1/6 = 0.016. Consequently, we would reject the null hypothesis of no treatment effect for a specific subgroup regression if the estimated coefficient is significant at the 1.6% level. P-value in column (5) is 0.012, while p-value in column 11 is 0.003.

[47]This is calculated as the total number of disenrolled students in high school during 2022 (=46,897) times the share of ineligible compliers who did not graduated by the end of the year (1-0.495).

5.3. Re-enrolling in Origin vs. Not-Origin Schools

As emphasized in Section 2, the overwhelming majority of dropout students who manage to re-enroll do so in schools different from the origin schools from which they initially dropped out. A minor portion of dropouts do not re-enroll in schools but take free exams to validate the curriculum, which is an alternative form of re-enrollment. This pattern may be influenced by both demand and supply factors. On the demand side, disenrolled students may reveal a preference for not to remain in the school of origin, thus re-enrollment is unlikely to occur in the same school. On the supply side, the school of origin may lack the incentive to re-engage dropouts, as they have already revealed poor academic outcomes. MINEDUC recognizes this when designing the dropout reports and does not require origin schools to re-enroll listed students. Instead, the reports encourage these schools to facilitate re-enrollment, whether in the same school or another institution.

Appendix Table A.IX examines the re-enrollment effects on high school dropouts, by type of re-enrollment[48]. The re-enrollment effect is largely driven by dropout students re-enrolling in schools other than their schools of origin. While the estimated coefficient is only statistically different from zero at the 10% level, and it is not robust to the Bonferroni correction for multiple hypothesis testing, this result aligns with the objective of the report policy and suggests that the origin schools receiving the reports leveraged their networks to support students' re-enrollment in other institutions[49].

5.4. The Role of School Networks

Local network effects typically function through spatial proximity, allowing nearby schools to reduce transaction costs and support each other. Receiving institutions may include regular schools serving the general student population or lower-quality non-regular schools offering remedial programs, such as adult education or special education schools, some of which accommodate dropouts.

Spatial Networks. Appendix Table A.X explores the role of spatial networks as a mediator of the treatment effects. To do so, we examine how the re-enrollment effects are distributed across schools located within the same municipality of the school of origin and schools located elsewhere[50]. The results show that most re-enrollment effects are driven by dropouts re-enrolling in schools located within the same municipality of the school of origin, suggesting that geographic proximity does play a role. Second, the higher re-enrollment rate is driven by a simultaneous increase in re-enrollment in regular schools and a decrease in re-enrollment in non-regular schools, which implies the report-driven re-enrollments do not come at the expense of reducing school quality[51].

[48] We focus on the re-enrollment behavior of high school dropouts as the report effects are mostly driven by this subgroup, yet the effects are similar for the full sample.

[49] Around 3.8% of the evaluation sample of dropouts are reported to have re-enrolled in an institution other than the school of origin, but no administrative data on the receiving institution is available for them, and thus we code them as N.A. re-enrollments (last column in Appendix Table A.IX). The report did not change the probability of N.A. re-enrollments at the conventional levels of statistical significance.

[50] Students re-enrolling by taking free exams do so in official institutions that are not attached to any specific location, and thus we consider them in a separate category

[51] This result holds when making inference under Bonferroni corrected p-values. Specifically, we have 4 subgroups (same municipality; different municipality; free exams; N.A.), and within same and different municipality subgroups there are two additional sub-branches (regular schools; non-regular schools), meaning there are 6 potential outcomes under

Institutional Networks. In some cases, school networks are institutionalized, i.e., not only are schools geographically close, but they are also governed by a shared organization that coordinates curriculum development and resource allocation. This structure may further facilitate re-enrollment. We explore this possibility in Appendix Table A.XI, examining how the re-enrollment effects within the same municipality are distributed across schools within and outside the dropout's former school's network. We find most of the report effect comes from re-enrollment in regular schools within the same institutional network, accompanied by a decline in non-regular school enrollment outside the network. Overall, these results support the reports' aim of leveraging institutional networks to facilitate school dropouts' re-enrollment[52].

5.5. The Role of School Capabilities

Finally, school re-enrollment involves efforts associated with contacting disenrolled students and their families, as well as coordination efforts with potential receiving schools, and both requires active engagement from school management staff. For instance, the report effects are expected to be greater among schools where the density of professionals dedicated to the strategic and operational management of the school is higher, including roles such as principals, vice principals, department coordinators, and counselors. In order to examine this hypothesis, we first compute, within each school of origin, the number of school workers in management staff over the total number of classrooms at baseline, which we call the managerial staff to classroom ratio.

Appendix Figure A.X plots how the report effect on re-enrollment rate varies across the quintile distribution of managerial staff to classroom ratio. We observe the re-enrollment effects tend to be higher in origin schools whose managerial staff per classroom is larger. Appendix Table A.XII digs in further and show estimates across sub-samples of schools whose managerial staff to classroom ratio was above versus below the median. The report effect is fully explained by origin schools whose managerial capacities are above the median, with the effects being robust to Bonferroni correction for multiple subgroups analysis. Overall, our results suggest school capabilities can be an important mediator for the effectiveness of the reports. This is in line with Muñoz and Prem (2024), which shows that Chilean schools whose management staff were strengthened by the ADP reform increased their students' yearly attendance, test scores, and college admission scores.

analysis. This implies the Bonferroni-corrected p-value is calculated as 0.1/6 = 0.016. Consequently, we would reject the null hypothesis of no treatment effect for a specific subgroup regression if the estimated coefficient is significant at the 1.6% level. P-value in column (3) is 0.005, while p-value in column (4) is 0.0144.

[52]Considering the 2 subgroups (from origin school's network; out of origin school's network) and the 2 additional sub-branches (regular schools; non-regular schools), there are 4 potential outcomes under analysis, thus the Bonferroni-corrected p-value is $0.1/4 = 0.025$. P-value in column (3) is 0.026, while p-value in column (4) is 0.053.

6. School Re-enrollment and Crime Prevention

6.1. School Dropout and Juvenile Crime: Stylized Facts

The SENAME administrative data track all adolescents aged 14-17 years old who have committed crime throughout this age period and are processed by the Chile's Adolescent Criminal Responsibility Law #20084 (LRPA)[53]. The LRPA is designed to address and process crimes committed by adolescents, but focuses only on violent crimes including homicide, rape, and aggravated assault, as well as property crimes and drug trafficking. Given the gravosity of these type of crimes, most crime events processed through LRPA are subject to pretrial injunctions and precautionary measures that speed up the pace of judicial proceedings, meaning their sanctions are dictated soon after the crime date. We accessed SENAME data covering the universe of crimes processed through LRPA and their corresponding sanction date, and used the MINEDUC enrollment and dis-enrollment data to merge crime observations to individuals aged 14-17 years old who disenrolled from the school system for the first time over the 2016-2024 period.

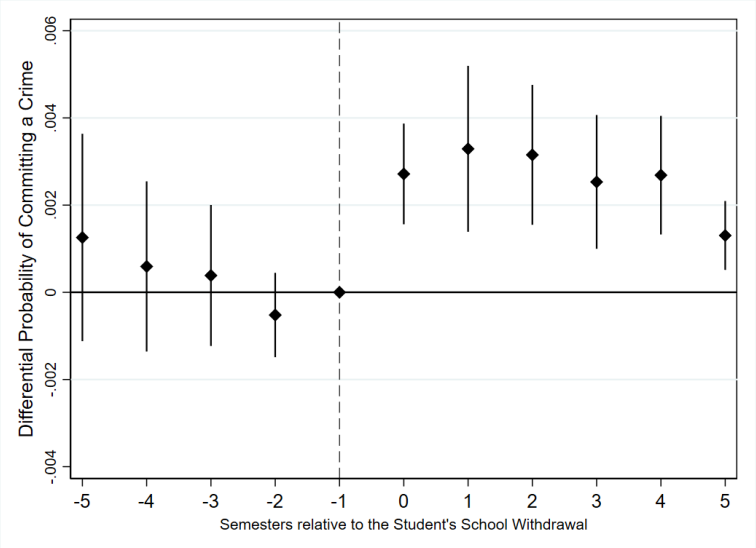
Since both disenrollment and crime data are indexed by date, we can examine how the propensity to commit a crime evolves before and after school disenrollment. Our focus is on the role of school dropout on initiation of criminal activity, hence the relevant points in time are first disenrollment and first crime. Specifically, we take 2016-2021 as a pre-treatment report intervention period, and build an event-study panel data set at the individual and semester-year levels covering a maximum of 5 lead and 5 lag semesters from the first dropout semester-date, for a total of 2.5 years of pre-disenrollment and 2.5 years of post-disenrollment. Considering that SENAME data is restricted to adolescents between 14 and 17 years old at the moment of the crime, we include students with at least one semester of SENAME eligibility before and after school dropout, i.e., students aged between 14.5 and 17.5 years old at the time of their first disenrollment. The latter guarantees a minimum time window for crime to be observed in pre- and post-event periods.

Overall, our event-study panel includes roughly 110,000 SENAME eligible students who dropped out from school for the first time at some semester-year within pre-intervention period 2016-2021. Among them, 5% committed at least one crime throughout the period, and more than 95% of crimes are committed by male youth. This figure almost doubles the average juvenile crime rate in Chile in 2016-2021. This is not unexpected as our sample of study are students who disenrolled at least once during their education, and thus are more vulnerable to engage in criminal activity compared to the average youth[54]. Also, this is in line with global evidence showing that crime rates are larger among youth populations (Levitt and Lochner, 2001). Second, 70% of the crimes occur after school (first) disenrollment, indicating that dropping out of school may be a significant determinant of initiation

[53] The LRPA law establishes a legal framework for addressing criminal behavior among minors aged 14 to 17. The law aims to balance accountability and rehabilitation by ensuring age-appropriate sanctions, emphasizing educational and social reintegration, and providing measures for prevention, intervention, and reintegration to reduce recidivism among young offenders. SENAME is the public institution in charge of implementing social reintegration programs for adolescent processed by LRPA law, all of whom are mandatory admitted into SENAME programs.

[54] According to CEAD (2024), the population aged 14-17 committed an average of 34,900 crimes per year in the period 2016-2021. For an average population of 996,000 youth aged 14-17 years old in the same period (INE, 2024), the average crime rate is 3.5%. Considering only violent and drug crimes (i.e., crimes that are eligible for SENAME programs), the crime rate reaches 2.1%.

Figure 6: Probability of committing Crime before and after School Dropout



Note: Leads and lags point estimates (and their corresponding 95% confidence intervals) of an event-study regression of the form $Crime_{it} = \alpha + \sum_{j=1}^{J} \beta_j (Lead_j)_{it} + \sum_{k=0}^{K} \gamma_k (Lag_k)_{it} + \eta_t + \phi_i + X'_{it}\Omega + \epsilon_{it}$, where $Crime_{it}$ is a dummy that equals 1 if the student i committed a crime in semester-year t and zero otherwise, and leads and lags are binary variables indicating that a given student was a given number of semester-years away from disenrolling from school for the first time in the respecting semester-year period t. The semester-year in which the student disenrolled from school for first time is fixed as period "0", and the period prior to first disenrollment ("-1") is included as the omitted base period in the regression, hence β_j's and γ_k's represent the differential probability of committing a crime relative to the semester immediately prior to first dropout. η_t are calendar year-semester fixed effects capturing all common factors associated with particular semesters of 2016-2021 years. ϕ_i are student fixed effects, and X'_{it} is a vector of time-varying factors that includes student's age and a first vs. second semester dummy. Standard errors are estimated such that the unobserved stochastic error term ϵ_{it} can be arbitrarily correlated within each student over time, i.e., they are two-way clustered at the student and calendar-year semester levels.

of criminal activity.

We begin by analyzing crime dynamics before and after school dropout using an event study design, which examines crime rates leading up to and following the first instance of school disenrollment (see Figure 6). We observe flat pre-dropout leads, meaning there is no differential likelihood of committing a crime across periods before school dropout, which in turn suggest no presence of anticipation effects. However, once the student abandons the school system for the first time, this chance climbs up sharply in about 0.003 percentage points relative to the pre-dropout period, on average. Considering that the crime rate is 0.6% in the semester immediately prior to school dropout ("-1"), dropping out from school is associated to about 50% higher chances of committing a crime, and such differential probability remains positive for at least five semesters before bouncing back to pre-dropout crime levels, possibly because the cost of committing crime rises as youth turn 18 and become legally liable for prosecution as adults. Overall, the large effect size as well as the persistence of the effect over time suggest that, at least for the case of Chile, school dropout indeed correlates positively with juvenile crime[55].

[55]This is consistent with several previous works documenting a positive effect of school dropout on crime (Jacob and Lefgren, 2003; Luallen, 2006; Anderson, 2014; Cook and Songman, 2016; Landersø et al., 2017; Fu et al., 2022; Huttunen et al., 2023). Still, our estimated correlation may not reflect a causal dropout-crime elasticity due to the

6.2. The Causal Effect of Dropout Reports on Juvenile Crime

This section examines the causal effect of being listed in the dropout report on the propensity to commit a crime. The exercise requires to consider only crimes that were effectively committed after the disbursement of dropout reports, June 28^{th}, 2022, and up to April 30^{th}, 2024, the last date considered in SENAME data. This only provides information on the judicial sanction date, not the crime date. However, the data distinguish whether the offense was subject to pretrial injunction (PI) or not (Not-PI). For PI-crimes, a sanction is usually imposed within 24-48 hours of the crime, and thus using sanction date as a proxy of crime date is reasonably accurate in these cases. We thus count PI-crimes whose sanction date is July 1^{st}, 2022 or later, and that is our main outcome of study[56].

For estimation, we replicate the regression discontinuity analysis from Section 5 but using PI-crimes as the outcome of interest as follows:

$$Crime_{isd} = \alpha + \beta B_{isd} + \gamma_0 f(r_{isd}) + \gamma_1 f(r_{isd}) \times B_{isd} + \eta_s + \phi_d + X'_{isd}\gamma + \epsilon_{isd} \tag{4}$$

The crime outcome is a dummy that equals one if the disenrolled student committed a PI-crime between July 1^{st}, 2022, and April 30^{th}, 2024, and zero otherwise. Drawing from the year closest to the treatment, i.e., year 2021, we use a window of $\pm$ 12 days as the optimal bandwidth and keep only schools where there is at least one disenrolled student aged 14-17 years old at each side of the cutoff (May 30^{th}, 2022), for a total of 3,142 disenrolled students distributed across 840 schools[57,58].

Disenrolled students at each side of the cutoff date are well balanced at pre-treatment level. About 2% committed crime before May 30^{th}, 2022, and students who disenrolled before and after that date are not statistically different in their chances to commit a crime. The evidence is robust considering a wide range of bandwidths, as is shown in Appendix Figure A.XI. The criminal and arrest history can correlate with future criminal behavior of individuals, hence our regression analysis will control for baseline crime. Pre- and post-cutoff disenrolled students are also similar in a large set of dimensions, including age, gender, socioeconomic status, GPA, school attendance, and graduation rates in previous years, which warrants internal validity to the RD exercise (see Appendix Table A.XIII).

Columns (1)-(3) in Table 2 presents the ITT effects on PI crimes. Among dropout students who disenrolled after the cutoff date, 3.2% committed PI crimes in the aftermath of report disbursement.

presence of time-varying unobservables affecting both dropout and criminal behavior, none of which are controlled by in our event-study design. For instance, residential movements or within-neighborhood changes (e.g., neighborhood composition, poverty rate, local investment, etc.) could lead youth to drop out and/or commit crimes more/less frequently (Kling et al., 2005; Goux and Maurin, 2007; Chetty et al., 2016). Likewise, within-school compositional changes can alter the dynamics of peer effects across students, affecting both dropout (Carrell and Hoekstra, 2010; Black and Salvanes, 2013) and criminal behavior (Deming, 2011; Billings et al., 2019; Eren et al., 2022).

[56] PI-crimes correspond to 75% of cases processed by LRPA law. The primary purposes of pretrial injunctions are to ensure that the accused appears at trial and to protect the community from potential harm that the accused might pose if released.

[57] Few cases dropped out more than once within the optimal window of $\pm$12 school days, which represent 1.65% of the evaluation sample. We treat them as separate dropouts throughout the analysis.

[58] Since the probability of being included in the report is not uniform across the running variable but it increases as the disenrollment date get closer to the cutoff date, observations are weighted by a triangular kernel approximation. assigns a zero weight to observations in the extreme (left and right) margins of the optimal window, which reduces the sample size from 3,142 to 2,928.

Disenrolling from school before the cutoff date reduces the probability of committing a PI crime by a range of 2.1 to 3.8 *pp.*. The effects are generally robust across models, regardless of whether we control or not for day-of-week fixed effects, pre-treatment crime, and the large set of additional pre-treatment covariates, which reinforces the internal validity of the results[59].

Table 2: Treatment Effects on Crime

	If committed Crime PI cases July 1st, 2022 - April 30, 2024				If committed Crime PI + Not-PI cases July 1st, 2022 - April 30, 2024			
	Loc.Lin. Poly. (1)	OLS Linear (2)	OLS Linear (3)	2SLS (4)	Loc.Lin. Poly. (5)	OLS Linear (6)	OLS Linear (7)	2SLS (8)
W.D. $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	-0.0213*	-0.0331**	-0.0382**		-0.0092	-0.0297*	-0.0354**	
	(0.0126)	(0.0160)	(0.0161)		(0.0144)	(0.0178)	(0.0179)	
If listed in the Report ($T_{isd} = 1$)				-0.0762**				-0.0706**
				(0.0322)				(0.0357)
Ineligible Mean	0.0321	0.0321	0.0321		0.0383	0.0383	0.0383	
Eligible Complier Mean				0.0203				0.0361
Ineligible Complier Mean				0.0965				0.1068
Observations	2,928	2,928	2,928	2,928	2,928	2,928	2,928	2,928
Schools	840	840	840	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
If Crime at baseline (= 1)	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Day-of-week FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes
1st Stage Estimate				0.5019				0.5019
Cragg-Donald F-Statistic				240.94				240.94
Kleibergen-Paap F-Statistic				175.63				175.63

Note: Sample of analysis are students aged 14-17 years old. Regression discontinuity estimates of the effect of dropping out before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on a dummy of whether the disenrolled student committed a crime or not. Only crime events produced between July 1st, 2022 and April 30th, 2024, are counted, i.e., we start counting since the month immediately after the release of dropout reports (June 28th, 2022). Regression models (1)-(4) only count crime events with pretrial injunction (PI), whose sanction is usually imposed within 24-48 hours of the crime. In models (5)-(8) we also count Not-PI crime events whose court order is one year ahead or more of the dropout report date (i.e., Not-PI crime events from July 1st, 2023 onwards). All models control for school FEs. Columns (1) and (5) estimate a local linear polynomial model. Regression models in columns (2) and (6) show estimates from a linear OLS model, controlling for day-of-week fixed effects, and a dummy on whether the dropout student committed crime before the reports were disbursed, as well as dummies for the severity (1-5) of the committed crime. Regression models in columns (3) and (7) additionally control for pre-treatment covariates, including a dummy on whether the student graduated in 2021, gender, age dummies, lag-age dummies, parents' schooling dummies, school attendance's tercile dummies, grade dummies, GPA tercile dummies, if preferent student, if migrant student, and if in primary school. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Columns (4) and (8) show Fuzzy IV estimates exploiting the cutoff date (B_{isd}) as an instrument for being listed in the report (T_{isd}), which reflects the ratio of the reduced form estimates over the first stage estimates. It also reflects the difference between the Eligible and Ineligible Complier Means computed following Abadie (2002). Opt. bandwidth is fixed at $\pm$ 12 school days, which is selected following MSE-optimal bandwidth choices. The probability of being listed in the report increases with the proximity of disenrollment to the cutoff date, hence observations are weighted by using a triangular kernel weight. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Next, in column (4) we estimate the causal effect on crime of have been listed in the report. Our

[59]The results are robust to multiple hypothesis testing (q=2) as well as to the use of a quadratic OLS model, yet the effects are less precise. See Appendix Table A.XV.

first stage estimate indicates that students who disenrolled before the cutoff date are 50% more likely to be listed in the report than their after-cutoff counterparts. As a result, our 2SLS estimates double in size relative to intention-to-treat figures, for a treatment effect of 7.62 *pp.*. In order to interpret the order of magnitude of this result, we follow Abadie (2002) to estimate the probability of committing crime among disenrolled students who were ineligible for being listed in the report and indeed were not included in it (i.e., ineligible compliers). The effects are large in magnitude, representing a 79% reduction relative to the probability of committing crime among ineligible compliers.

6.3. Robustness Checks

Appendix Figure A.XII replicates the analysis for several different bandwidth selections, including all combinations of optimal bandwidths presented in Appendix Table A.IV. The results are shown to be robust in that we generally reject the null hypothesis of no effect at the conventional levels of statistical significance.

We then replicate the analysis but also considering Not-PI crimes, i.e., we add Not-PI crimes to the outcome dummy. Importantly, the court orders of Not-PI-crimes can arrive several months after the crime. While LRPA law mandates a maximum of 6 months to dictate sanctions, some cases may take up to one year due to judicial backlog. Therefore, to be on the safe side we count Not-PI-crimes whose sanction date is one year after the dropout reports' disbursement (i.e., July 1^{st}, 2023 onwards), thereby ensuring that Not-PI counted crimes are indeed produced after the reports. Columns (5)-(8) show these results. Overall, we find the effects are similar in magnitude, adding robustness to the results.

Finally, while PI-crimes usually get sanctions within 24-48 hours of the crime event, there could still be some outlier cases that take longer than usual. The latter implies our estimates could suffer from one-sided measurement error, i.e., we could be counting crimes whose sanction was dictated after the report's delivery date of June 28th, 2022, yet were committed before that date. To ensure accuracy, we replicated the analysis by including only PI-crimes with a sanction date at least six months after the report's delivery date, thereby minimizing the likelihood of overcounting crimes. As is shown in Appendix Table A.XIV, the results are robust to the use of this stringent measure, and this is the case when counting only PI-crimes as well as when we combine PI and Not-PI events.

6.4. On Estimating the School Re-enrollment-Crime Elasticity: A Cautionary Note

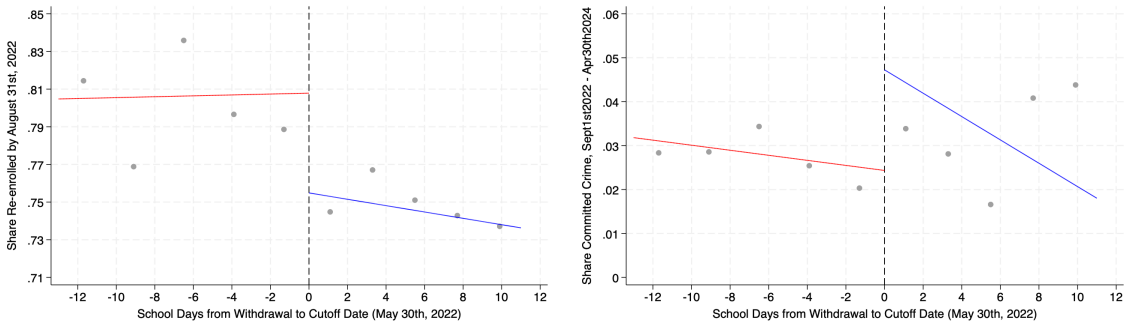
Having shown positive effects of the report on re-enrollment but negative effects on crime, school re-enrollment emerges as a natural mechanism underlying the relationship between dropout reports and crime. Indeed, about 4.2% of dropout students who do not re-enroll in school commit crime within the 18 months following dropout, which is 58% larger relative to the crime rate of their dropout counterparts who re-enrolled. This is in line with our event-study estimates from Section 6.1 showing that dropping out from school is associated to about 50% higher chances of committing a crime compared to the semester immediately prior to school dropout.

Figure 7 plots how the propensity to re-enroll in school by August 31^{st}, 2022 (left), and the propensity to commit crime after August 31^{st}, 2022 (right) vary across our running variable deter-

mining the likelihood of the student to be listed in the report. We observe a discontinuous increase of approximately 5 *pp.* in the re-enrollment rate around the report-eligibility cutoff date, and a discontinuous decrease of about 2 *pp.* in the PI-crime rate. The latter suggests that we could exploit whether the student disenrolled before or after the report-eligibility cutoff date (B_{isd}) as an instrument for being re-enrolled into school by August 31^{st}, 2022. A priori, that would allow us to estimate the causal effect of school re-enrollment on the probability of committing crime after August 31^{st}, 2022, i.e., a causal estimate of the re-enrollment-crime elasticity. However, the excludability assumption may not be met in practice, impeding a causal interpretation.

The excludability assumption requires our instrument reduces the probability of committing crime (Sept. 1st, 2022 onwards) only by increasing the chances the student is re-enrolled in school by August 31^{st}, 2022, and not through other channels. A simple example is the hypothetical case of a student who was listed in the report, so the school attempted to bring him back, yet he remained disenrolled by August 31^{st}, 2022. He did not commit crime afterwards, but this was because he felt someone (the school) was trying to take care of him, a "psycho-gain" that reduced his chances of committing a crime. So, even though being listed in the report did not change re-enrollment, it reduced the chances of committing crime due to alternative channels, violating the excludability assumption.

Figure 7: Disenrollment Date, School Re-enrollment, and Juvenile Crime



Note: Left panel is a binned scatterplot of the share of re-enrolled students by August 31th, 2022, as a function of the distance (in school days) between disenrollement date and cutoff date (May 30^{th}, 2022), controlling for school fixed effects and day-of-week fixed effects. Optimal bandwidth is fixed at $\pm$ 12 school days. Red (Blue) bins aggregate the re-enrollment rate of students who disenrolled before (after) the cutoff date. The solid lines trace a linear fit highlighting the relationship between distance to cutoff and the probability change in the outcome at each side of the cutoff. Right panel replicates the same exercise but for the share of dropout students who committed PI or Not-PI crimes between Sept 1st, 2022 (i.e., after reenrollment capture of Aug 31st) and April 30th, 2024. The probability of being listed in the report is not uniform but increases with the proximity of disenrollment to the cutoff date, hence observations are weighted by using a triangular kernel weight.

7. Cost-Benefit Analysis

A crucial policy question resides on where is the marginal dollar best spent, if in police, in prisons, or in schools — while all might reduce crime, education offers benefits that prisons and police do not. Table A.XVI present estimates of the crime-reduction benefits that would be obtained from the implementation of the reports had this covered the entire population of high-school dropouts in 2022. Importantly, the program savings are restricted to the reduced costs in crime; they do not

include the labor market returns to education for re-enrolled dropouts, and thus should be considered as a lower bound of its overall potential benefits.

Alfaro et al. (2024) estimate that the total cost of crimes committed in Chile by 2022 is US$ 6,280 MM. This includes mostly crimes of “high social connotations" (HSC). According to CEAD (2024), Chile experienced 654,278 HSC crimes in 2022, for a per-crime cost of US$ 9,599, on average[60]. While some of the crime categories classified as HSC crimes are not as violent (costly) as the type of juvenile crimes to be processed by LRPA, the per-crime cost derived from Alfaro et al. (2024) can still be used as a lower bound of LRPA crimes and thus it is useful for a conservative cost-benefit analysis. In terms of program implementation costs, these include the design of the SIGE centralized dropout monitoring system, which covers 23% of implementation costs; production and delivery of dropout reports (2%); and school agency costs, i.e., expenditures schools incur in efforts to reintegrate dropout students into the education system (75%). Considering the full population of high-school dropouts in 2022, the per-student cost of implementing the reports is US$ 93.37, on average. That is, the per-unit savings of preventing a crime is more than 100 times larger than the per-student cost of implementing the report, for a benefit-cost ratio of 103-to-1.

The latter suggests dropout reports are a very cost-effective crime-prevention policy. Indeed, in 2022 there were 46,897 high-school students who dropped out at some point of the year. Had all of them been listed in the dropout reports, the implementation cost would be US$ 4.3 millions. Given the crime prevention effects of the dropout reports, we estimate 3,574 of them would have committed crime had not been listed in the report, for crime savings that amount to US$ 34.3 millions given the per-unit crime costs. Hence, we estimate the total savings are roughly US$ 30 millions, which is about 0.01% of Chilean GDP in 2022. Finally, our research design measures the impact of sending dropout reports to schools 4 to 6 weeks after a student exits the system, thus expanding the intervention to reduce the delay between the dropout event and report delivery could trigger quicker re-enrollment responses, potentially resulting in greater cost efficiency.

Our estimate of the cost-effectiveness of the reports is conservative for various reasons. First, it excludes additional savings from reduced adult crime. Second, it does not take into account the labor market returns of re-enrolled students. Third, our research design captures the effect of delivering dropout reports 4 to 6 weeks after a student leaves the system, meaning a scaled-up version of the program that shortens this gap could then prompt more timely re-enrollment efforts, potentially leading to even higher cost-effectiveness.

Finally, the dropout reports could have also triggered changes in schools’ disenrollment policies, reinforcing the prevention of school dropout. For example, schools might tighten criteria for removing students from their rolls to minimize the appearance of high dropout rates in the reports, reflecting accountability-driven (extrinsic) motivations. Alternatively, they may adopt more supportive measures to prevent at-risk students from disengaging entirely, guided by a genuine concern

[60]HSC crimes include homicides, rape and sexual offenses, robbery with violence or intimidation, sudden or surprise theft, grievous, moderate, and minor bodily harm, domestic violence, burglary, vehicle theft, other thefts involving force against property, and larceny. 43% of the per-crime cost corresponds to anticipation costs, i.e., crime preventive expenditures, and one-third are response costs associated to expenditures linked to the administrative costs of the judicial system and prisons. The resting 25% are consequence costs or expenditures incurred as a direct effect of criminal acts, from compensation to victims to the value of destroyed goods, among others.

for student welfare (intrinsic motivation). Indeed, schools seems to have substantially reduced the share of disenrolled students after the disbursement of the reports. Specifically, we analyze whether the cumulative share of disenrollments reported by schools changed before and after the report's disbursement, with the cumulative share defined as the number of high-school disenrollments per school occurred from March 2022 (the beginning of the school year) up to each subsequent month of 2022, divided by the number of enrollments per school in March 2022. Note that the cumulative share must increase over months since the accumulated number of disenrollments can only augment over time but the initial number of enrollments is fixed. Also, school-specific secular trends might play a role. Hence, in order to isolate seasonality and school-specific components, we take the 2022-2016 month-year difference in the cumulative share of high-school disenrolled students per school, with 2016 having the very same year calendar than 2022, thereby allowing us to absorb time fixed effects accurately.

Appendix Figure A.XIII plots an event-study graph showing the 2022-2016 month-year mean difference in the cummulative share relative to June, which is used as the reference month before the reports were delivered (June 28^{th}), all controlling for school fixed effects. The event study takes the three months previous to the report's disbursement (April, May, June) and the three months after it (July, August, September). We proceed separately for schools whose reports included a dropout list with at least one dropout student and schools whose dropout list was empty, i.e., their disenrolled students managed to re-enrolled and thus no dropout student was listed.

Up to June 2022, we find no meaningful month-year differences in the average cumulative share of disenrolled students reported by schools, and this is the case for both schools with non-empty and empty dropout lists. However, after the dropout reports disbursement in June 28^{th}, 2022, schools whose dropout list was not empty abruptly reduced the disenrollment rate relative to 2016 levels, by about 3 to 4 *pp*[61]. In contrast, counterpart schools with empty-lists show no changes: facing no dropout students listed may have led them to perceive their disenrollment policy is effective, which generated no variation in their disenrollment rates after the report. Overall, this suggests the release of the dropout report may have influenced the disenrollment policy of treated schools[62].

All in all, our cost-effectiveness estimates likely provide a lower bound on the true benefits of centralized dropout monitoring. If the reports prompted schools to adjust their disenrollment policies —whether by tightening criteria for removal or implementing more proactive support measures— then the intervention not only facilitated re-enrollment but also indirectly prevented additional dropouts from occurring in the first place. This dynamic effect implies that the reports may have amplified their long-term impact beyond what our primary estimates capture, further strengthening

[61] Note that the downward trend in disenrollment rates does not necessarily reveal anything about intrinsic or extrinsic motivation factors. For example, after receiving a reports with a full list of dropout students, schools might develop a greater intrinsic concern for at-risk high-school students, thereby reducing the disenrollment rate. In contrast, the reduction could indicate a shift in the composition of student profiles enrolled in the school. For instance, schools that previously employed a pushout strategy by disproportionately disenrolling at-risk students may have moderated it, which in turn reduced the disenrollment rate.

[62] We do not argue a causal relationship. Even controlling for school fixed effects and isolate across-months seasonality, this is a pre-post analysis that may still be subject to selection problems, as unobserved time-variant factors within schools could influence the pre- and post-treatment trends (e.g., new school principal, changes in the composition of students, etc).

the case for their scalability as a cost-effective tool for dropout prevention and crime reduction.

8. Conclusion

Despite numerous policy efforts aimed at reducing school dropout rates and their subsequent effects on juvenile crime, addressing students who have already left the school system has remained elusive. In this paper, we provide causal evidence on the role of centralized dropout monitoring systems as a low-cost and scalable solution for re-enrolling dropouts. By equipping Chilean schools with timely and targeted information about dropout students, the system increased their re-enrollment and grade completion, with school networks playing a crucial role for school re-enrollment. The re-enrollment effects in turn led to a significant reduction in violent crime among re-enrolled students.

A conservative estimate places the at-scale cost of implementing the reports at just US$93 per dropout student, resulting in a cost-benefit ratio of 1:103 in terms of crime-prevention gains —far surpassing the cost-effectiveness of many existing juvenile crime prevention programs. Scaling the reports to cover all dropout students in 2022 would generate crime-reduction savings equivalent to US$30 million, or about 0.01% of Chile's GDP. This estimate is likely cautious, as it excludes additional savings from reduced adult crime and the labor market returns of re-enrolled students, as well as dropout reductions prompted by adjustments in schools' disenrollment policies. Notably, the Chilean SIGE system is not unique: Education Management Information Systems (EMIS) are widely used worldwide to collect, process, store, and analyze education-related information (UNESCO, 2020; Ortiz et al., 2021), meaning the Dropout Reports are potentially scalable to other contexts different from Chile's education market.

A limitation of this paper is that we are unable to account for potential spillover effects from listed dropouts to their non-listed peers. If such effects exist and are positive, our estimates of the impact on re-enrollment and crime prevention may be underestimated. While we acknowledge this possibility, we believe spillovers are likely small, as listed students make up only about 1% of the population per school—likely too few to exert a significant influence on the broader student body. However, this remains an open question for future research. Finally, our results focus on violent crime. Future research should explore re-enrollment effects on non-violent crime and other social outcomes, as well as whether re-enrollment has long-term effects on adult crime rates and labor market outcomes. This would enrich our understanding of the broader impacts of re-enrollment policies on both dropout students and society at large.

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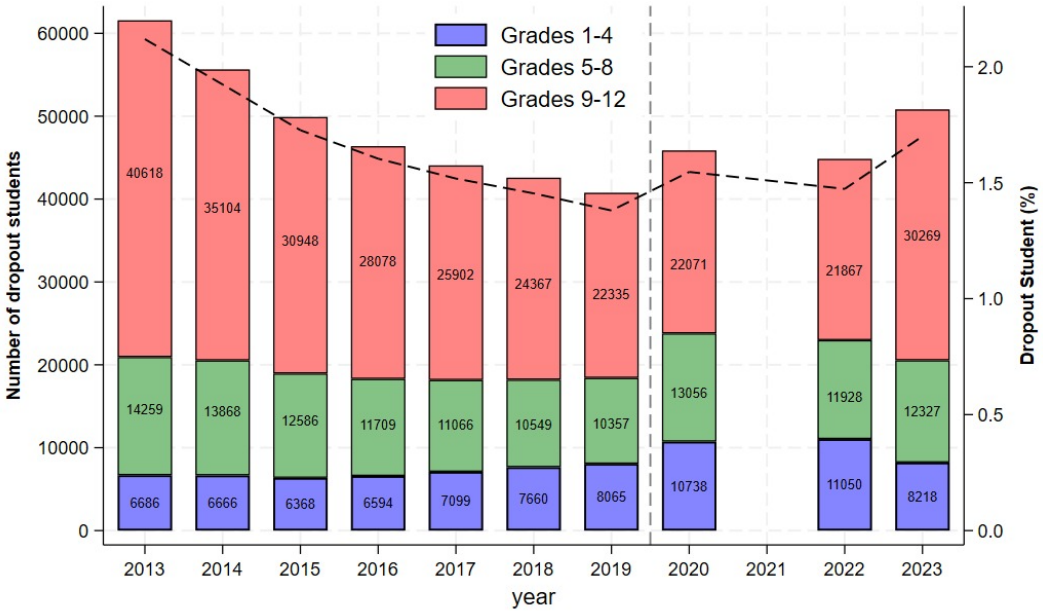
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A. Appendix Tables and Figures

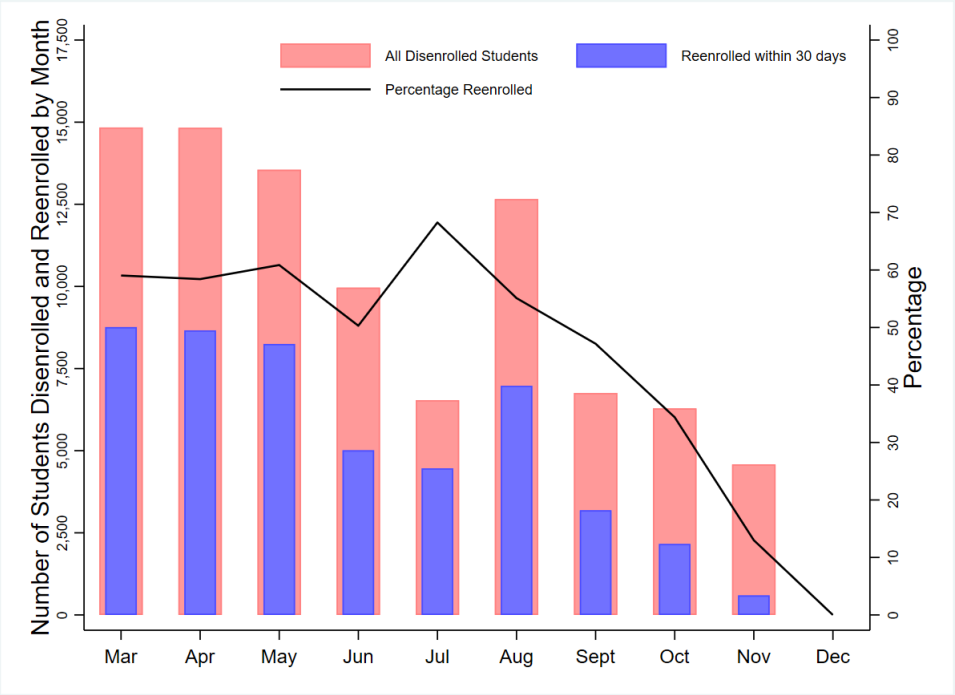
Figure A.I: School Dropout Trend in Chile



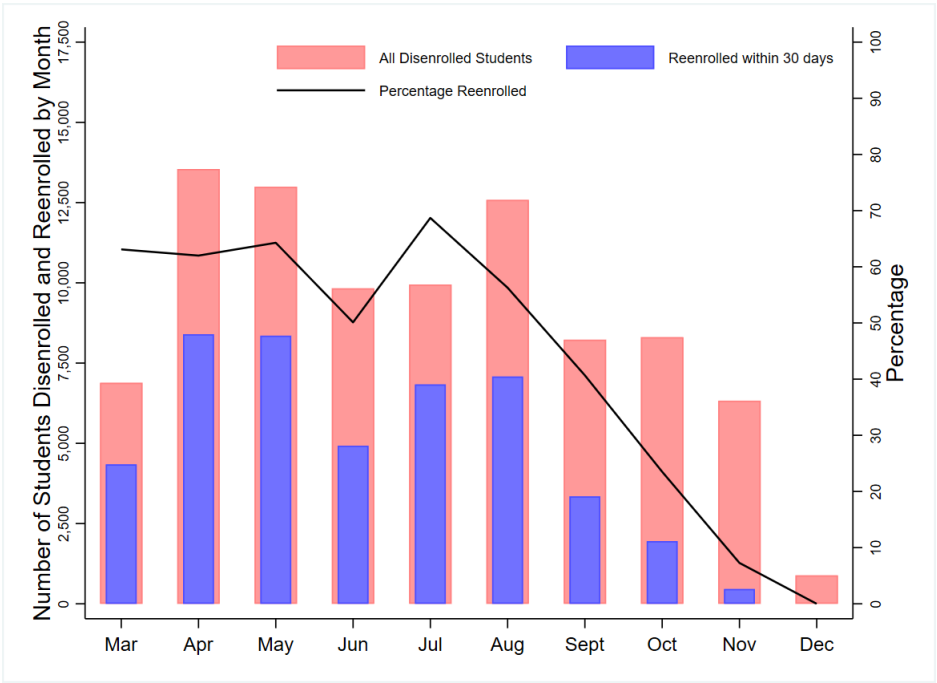
Notes: National trends in school dropout for the period 2013-2023. Dropout rates in year t is estimated as the number of students who do not re-enroll in year $t+1$, divided by the total number of students enrolled at the end of year t. Dropout rate in 2021 is omitted since the pandemic generated automatic promotion of students that year, making the dropout rate not comparable with that of other years.

Figure A.II: Across-Months Distribution of Dropouts and Re-enrollments

(a) All Dropouts in 2022



(b) All Dropouts in 2016



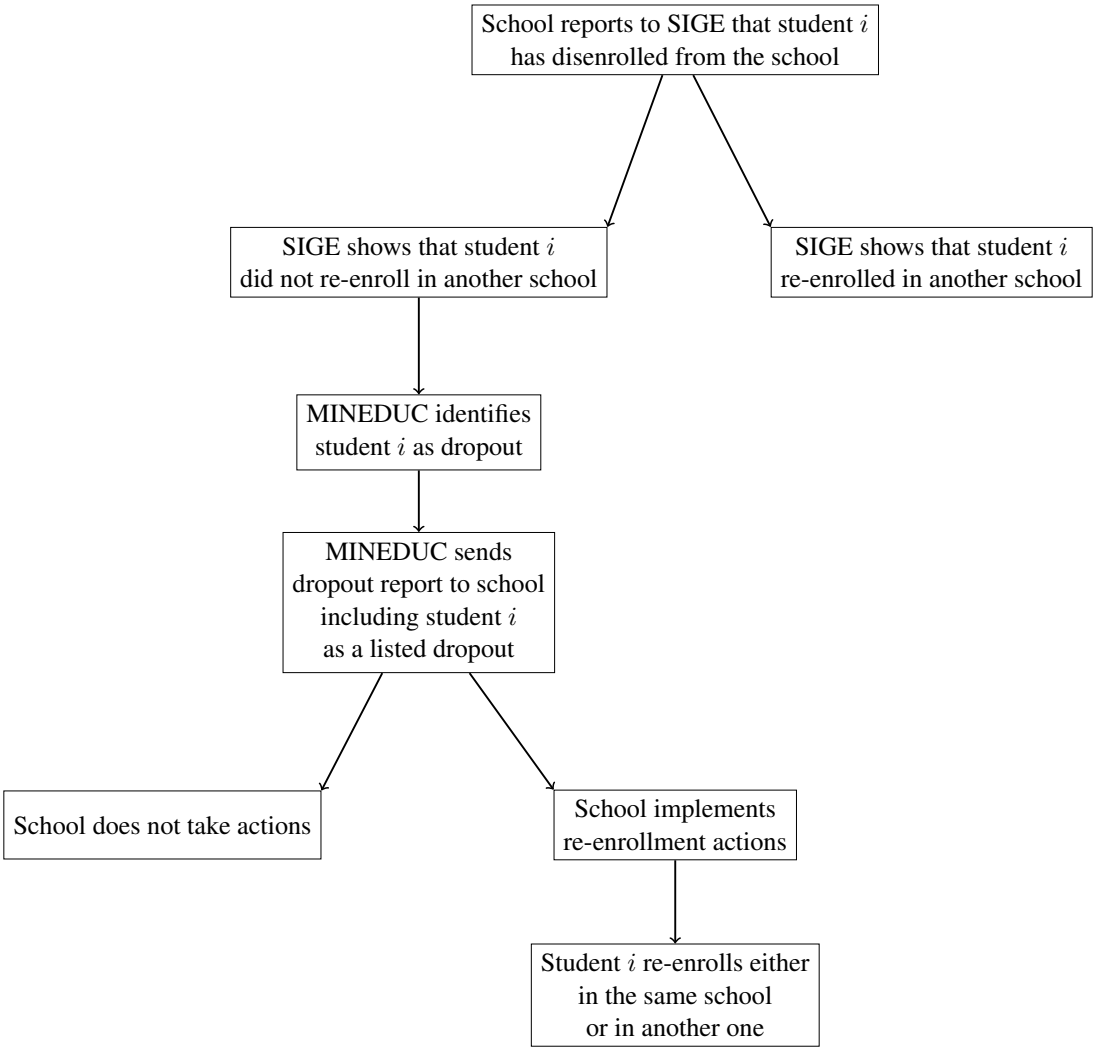
Notes: Sample of analysis are students who were enrolled by March 1^{st}, each year, but later disenrolled without transferring to another school, i.e., became dropouts. Some of them then re-enrolled, some continued being dropouts. Each bar indicate the number of disenrolled students who became dropouts in each month (red) and the number of them who re-enrolled within a month from disenrollment (blue), separated by 2022 dropouts (Panel (a)) and 2016 dropouts (Panel (b)). Across-bar line reflects the share of dropout students in each month who re-enrolled within a month from disenrollment (referred as “Percentage" in Y-axis on the right). We use 2016 as the pre-treatment comparison year because it shares the same calendar structure as 2022, resulting in an identical distribution of school days and dates.

Table A.I: School Dropout and School Re-enrollment between March 1^{st} and May 30^{th}, 2022

	All Dropouts		Re-enrolled Dropouts		Not Re-enrolled Dropouts	Re-enrolled Dropouts	
Observations	42,644		29,094		13,550	29,094	
Share of Total (%)	1.71%		1.16%		0.54%	1.16%	
Share of Disenrolled (%)					31.77%	68.23%	
Re-enrolled in Origin School (%)						5.38%	
Re-enrolled in Other School (%)						94.62%	
	Mean	Mean Diff. wrt. Old Classmates	Mean	Mean Diff. wrt. New Classmates	Mean	Mean	Mean Diff.
Male	0.537	0.013***	0.512	-0.011***	0.581	0.512	0.069***
Migrant	0.119	0.036***	0.062	0.000	0.216	0.062	0.154***
Age-grade lag 2021	0.332	0.236***	0.205	0.045***	0.549	0.205	0.344***
School attendance 2021	0.833	-0.036***	0.848	-0.007***	0.808	0.848	-0.040***
School GPA 2021 (Z)	-0.531	-0.389***	-0.413	-0.204***	-0.736	-0.413	-0.323***
SIMCE Math. 2018 (Z)	-0.365	-0.117***	-0.330	-0.061***	-0.427	-0.330	-0.097***
SIMCE Lang. 2018 (Z)	-0.330	-0.103***	-0.295	-0.052***	-0.390	-0.295	-0.095***
Graduation 2021	0.905	-0.059***	0.935	-0.001	0.854	0.935	-0.081***

Note: Sample of analysis are students who were enrolled at school by March 1^{st}, 2022, but at some point before May 30^{th} disenrolled without transferring to another school, i.e., dropouts (42,644 in total, column All Dropouts). Students who dropped out more than once are treated as separate cases (604 cases, corresponding to 1.41% of the All Dropouts sample). Some dropouts managed to re-enroll either in the origin or other school before May 30^{th}, and are labeled as Re-enrolled Dropouts. In contrast, Not Re-enrolled Dropouts are students who dropped out and did not re-enroll in any school by May 30^{th}, 2022. Share of dropout students is 1.71%, of which 68.23% managed to re-enroll in the school system. Among Re-enrolled dropouts, 94.62% re-enrolled in other (not the origin) school. Columns under the heading All Dropouts compare baseline characteristics of dropout students with their "old" classmates in origin school who did not dropped out, controlling for classroom fixed effects. Columns under the heading Re-enrolled Dropouts compare baseline characteristics of re-enrolled dropouts (either in the origin school or in a another school) with new classmates, controlling for classroom fixed effects. Last column compare baseline characteristics between Not-Re-enrolled vs. Re-enrolled Dropouts, regardless of whether they were classmates or not. For hypothesis testing of mean differences, standard errors are clustered at the school level. *p<0.10; **p<0.05; ***p<0.01.

Figure A.III: MINEDUC's Centralized Dropout Monitoring System

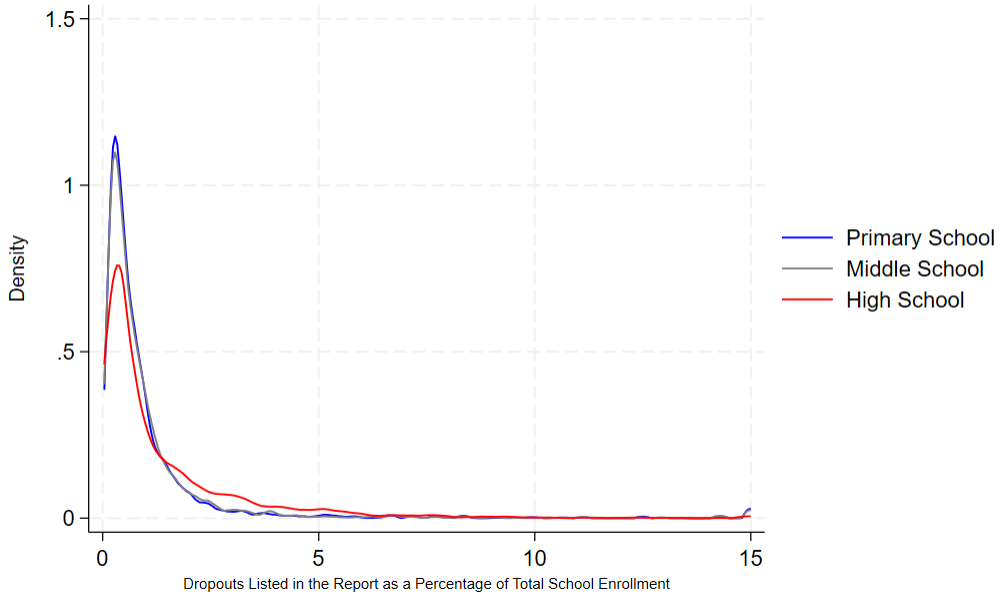


Notes: This figure illustrates how the Centralized Dropout Monitoring System works in practice. Schools in Chile are required to report to the centralized SIGE platform whenever a student enrolls or disenrolls from the school. MINEDUC identify dropout students as those who disenroll from a school without transferring to another. MINEDUC create school-specific reports called "Report for Monitoring Students with Interrupted or Irregular Educational Trajectories" (Dropout Report), which provide schools with a list of all disenrolled students that became dropouts after leaving the school, encouraging their re-enrollment in the educational system. Schools download their school-specific report from the SIGE-linked School Community Platform, which is the official communication channel between MINEDUC and schools. School principals typically check it on a weekly basis in order to gather new information provided by MINEDUC, for which login credentials are provided privately by MINEDUC.

Figure A.IV: Example of Dropout Report



Figure A.V: Share of the school's enrollment that was listed in the Dropout Report



Notes: Unit of analysis are public schools. Each public school receives a dropout report listing former students who, having enrolled in 2021, did not enrolled by March 1^{st}, 2022 (initial date of 2022 school year) or enrolled by that date but withdrew and did not re-enrolled in any state-recognized schooling institution by May 30^{th}, 2022. X-axis is the share of listed students per school calculated as the number of listed students in the report divided by the total school enrollment at the beginning of 2022 school year. The density of dropout shares are shown separately for primary (1-4), middle (5-8), and high school students (9-12). Average dropout share is 1.04% for primary school, 1.06% for middle school, and 1.4% for high-school students.

Table A.II: Descriptive Statistics

	Enrollments Full Sample N=2,513,974	Dropout Students Full Sample N=90,013	Dropout Students Evaluation Sample N=12,098				
	Mean (1)	Mean (2)	Mean (3)	Mean Diff. (2)-(1)	*p*-val.	Mean Diff. (2)-(3)	*p*-val.
Panel A. Student level							
Male (Female)	0.515	0.534	0.526	0.019	0.000	0.008	0.136
Age 2021	11.291	11.878	12.211	0.587	0.000	-0.333	0.000
Migrant	0.059	0.120	0.116	0.061	0.000	0.004	0.495
Primary School in 2022	0.271	0.164	0.156	-0.107	0.000	0.008	0.009
Middle School in 2022	0.378	0.315	0.300	-0.063	0.000	0.015	0.000
High School in 2022	0.352	0.521	0.543	0.169	0.000	-0.022	0.000
Prioritary 2022 (Q1+Q2)	0.573	0.640	0.654	0.067	0.000	-0.014	0.009
Preferent 2022 (Q3+Q4)	0.270	0.197	0.197	-0.073	0.000	0.000	0.967
Integrated student 2021	0.129	0.142	0.131	0.013	0.000	0.011	0.000
School attendance 2021	87.417	83.269	82.540	-4.148	0.000	0.729	0.000
School GPA 2021	5.976	5.535	5.487	-0.441	0.000	0.048	0.000
Graduation 2021	0.968	0.910	0.911	-0.058	0.000	-0.001	0.880
Panel B. School level							
Log total enrollment 2021	6.335	6.197	6.396	-0.138	0.000	-0.199	0.000
Public (Private voucher)	0.424	0.540	0.544	0.116	0.000	-0.004	0.290
Low SES	0.525	0.658	0.681	0.133	0.000	-0.023	0.000
Med SES	0.336	0.270	0.262	-0.066	0.000	0.008	0.071
High SES	0.138	0.071	0.057	-0.067	0.000	0.014	0.000
Rural (Urban)	0.091	0.088	0.048	-0.003	0.013	0.040	0.000
SIMCE Math 2018	255.954	246.552	246.028	-9.402	0.000	0.524	0.024
SIMCE Language 2018	261.545	253.172	251.335	-8.373	0.000	1.837	0.000

Notes: Enrollments in column (1) correspond to all students in publicly funded schools from grades 1 to 12 who enrolled in 2022 plus students who, having graduated in 2021, should have enroll in 2022 and did not do it. The latter excludes students from private institutions, as well as those from special and adult education. Sample in column (2) are students who disenrolled from the school system at least more than 24 hours during 2022, and thus are considered dropouts. Students who dropped out more than once throughout 2022 are counted as separate cases, and corresponds to less than 8% of the full sample of dropouts. Evaluation Sample in column (3) corresponds to students who disenrolled from the school system at least more than 24 hours (i.e., dropouts) in dates within a window of $\pm$ 14 school days from the cutoff date (May 30^{th}, 2022). Students who dropped out more than once within the window of $\pm$ 14 school days are counted as separate cases, and corresponds to 0.54% of the evaluation sample.

Table A.III: Balance Checks – Evaluation Sample

	Control Mean	Mean Diff.	SE	Obs. Left	Obs. Right
Panel A. Demographics					
Male (=1)	0.527	-0.024	(0.021)	5,983	5,285
Migrant (=1)	0.117	-0.023	(0.014)	5,983	5,285
Age in 2022 (=1)	13.285	-0.186	(0.180)	5,983	5,285
Age-grade lag in 2022 (=1)	0.687	-0.039	(0.035)	5,983	5,285
Primary School in 2022 (=1)	0.223	-0.011	(0.017)	5,983	5,285
Middle School in 2022 (=1)	0.304	0.033*	(0.019)	5,983	5,285
High School in 2022 (=1)	0.473	-0.022	(0.018)	5,983	5,285
Prioritary Student in 2022 (=1)	0.649	0.018	(0.021)	5,983	5,285
Preferent Student in 2022 (=1)	0.193	-0.004	(0.018)	5,983	5,285
Integrated Student 2021 (=1)	0.126	0.004	(0.014)	5,983	5,285
Log(Income)	12.656	0.003	(0.076)	1,745	1,527
Panel B. Baseline Performance					
School Attendance 2021	82.319	-0.075	(0.606)	5,983	5,285
School GPA 2021	5.481	-0.005	(0.032)	5,908	5,232
4th Grade Achievement Z Score 2014-18	-0.355	-0.081	(0.073)	2,444	2,144
Panel C. Lagged Outcomes					
If Committed Crime Before May 30^{th}, 2022 (=1)	0.009	0.004	(0.004)	5,983	5,285
Lagged Graduation 2021 (=1)	0.908	-0.011	(0.013)	5,983	5,285
Lagged Graduation 2020 (=1)	0.928	0.002	(0.012)	5,552	4,868
Lagged Graduation 2019 (=1)	0.854	0.022	(0.018)	5,128	4,485
Lagged Graduation 2018 (=1)	0.844	0.010	(0.018)	4,751	4,168
Lagged Graduation 2017 (=1)	0.849	-0.002	(0.018)	4,336	3,804
Lagged Graduation 2016 (=1)	0.844	0.010	(0.020)	3,932	3,443
Missing in Lagged Graduation (=1)	0.409	-0.005	(0.020)	5,983	5,285
Panel D. Re-enrollment					
If Re-enrolled Before Cutoff (=1)	0.350	0.015	(0.026)	5,983	5,285

Notes: Baseline characteristics of students in evaluation sample (14-days bandwidth). Control Mean is the average value of each covariate for students who withdrew after May 30^{th}, 2022, the cutoff date. Diff. denotes the mean difference between students on either side of the cutoff, and are obtained by estimating the very same equation model 1 but using baseline data, i.e., we regress each baseline variable against the running variable, the treatment dummy, and their interaction, and controlling for day-of-the-week and school fixed effects. Observations are weighted by a triangular kernel centered at May 30^{th}. As in the post-treatment analysis, since the triangular kernel weight observations at each extreme of the running variable by zero, the total number of observations is reduced from 6,464 to 5,983 in the left side, and from 5,634 to 5,285 in the right side, for a total sample size reduction from 12,098 to 11,268. Standard errors clustered at the treat-school level are shown in parentheses in column S.E. Math and Language Z-Score Achievement data is compiled for a subsample of high school students, specifically those who had achievement records available from when they were in 4th grade during the years 2014 to 2018 (no achievement data is available for the years 2019-2021). Lagged graduation indicates the share of students from the 2022 cohort that graduated in each year of the period 2016-2021. Missing in Lagged Graduation is a dummy that equals 1 if the observation was missing in any of the lagged graduation years, and zero otherwise. If Re-enrolled Before Cutoff is a dummy that equals 1 if the left-hand-side disenrolled student re-enrolled before May 30^{th} and zero otherwise. For right-hand-side disenrolled students, the dummy takes a value of 1 if the student re-enrolled before a placebo date set as of June 17^{th}, i.e., it counts 14 school days after the cutoff (May 30^{th}), such that the maximum time window for re-enrollement is equal for both before- and after-cutoff disenrolled students. *p<0.10; **p<0.05; ***p<0.01.

Table A.IV: Optimal Bandwidth Selection

Outcome	Year Data	OLS Linear (1)	OLS Linear (2)	OLS Quadratic (3)	OLS Quadratic (4)
Panel A. Graduation					
If Graduated (=1)	2016	10.00	12.00	14.00	13.00
If Graduated (=1)	2017	12.00	11.00	12.00	12.00
If Graduated (=1)	2018	9.00	12.00	15.00	14.00
If Graduated (=1)	2019	13.00	14.00	16.00	16.00
If Graduated (=1)	2020	No Data on withdrawals due to pandemic			
If Graduated (=1)	2021	14.00	14.00	18.00	18.00
Panel B. Crime					
If committed crime (=1)	2016	16.00	16.00	19.00	19.00
If committed crime (=1)	2017	16.00	17.00	17.00	18.00
If committed crime (=1)	2018	20.00	21.00	18.00	18.00
If committed crime (=1)	2019	16.00	16.00	22.00	22.00
If committed crime (=1)	2020	No Data on withdrawals due to pandemic			
If committed crime (=1)	2021	12.00	12.00	16.00	16.00
Polynomial Degree		1	1	2	2
Day-of-the-week FE		No	Yes	No	Yes
Lagged Graduation		No	Yes	No	Yes

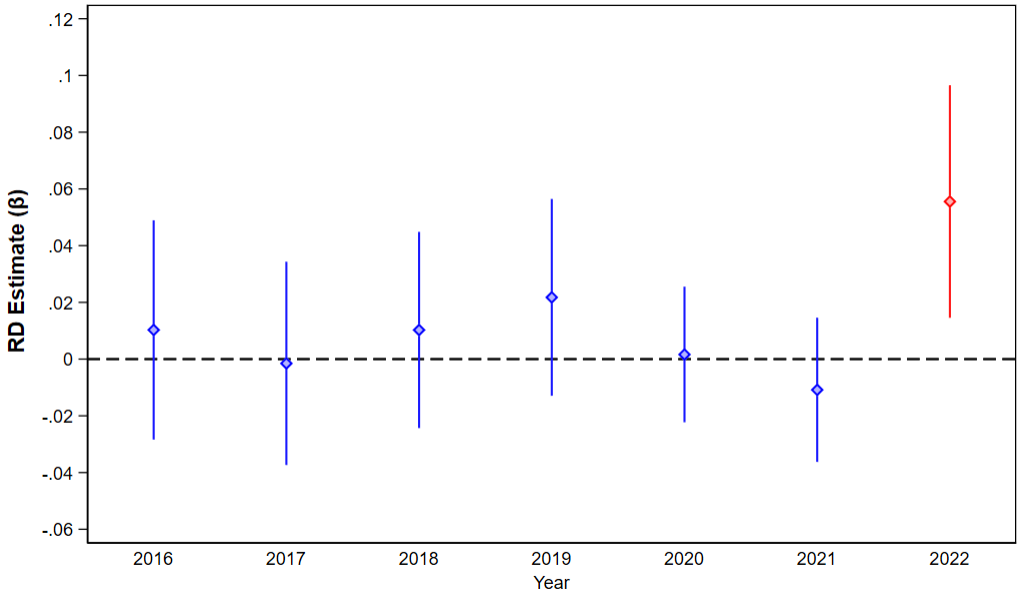
Notes: Optimal bandwidth in number of school days around cutoff date of May 30^{th} using the common mean squared error (MSE)-optimal bandwidth selector from `rdrobust` (Calonico et al., 2014). Outcome in Panel A is a dummy that equals one if the disenrolled student graduated that year, and zero otherwise. Outcome in Panel B is a dummy that equals one if the disenrolled student had committed crime before or within that year, and zero otherwise. Errors are clustered at the school-group level. Model (1) is a first degree polynomial regression. Model (2) adds day-of-the-week fixed effects and controls for a dummy on whether the student graduated in previous year or not. Models (3) and (4) replicate the analysis but using a second-degree polynomial. Year 2020 is excluded from the analysis as schools were closed around the cutoff date that year, and no data on disenrollments were produced.

Table A.V: Impact of withdrawing before Report's cutoff date - RD Estimates using Quadratic Model

	If Re-enrolled by August 31^{st}, 2022			If Graduated by December 31th, 2022		
	Local Quadratic Polynomial (1)	OLS Quadratic (2)	OLS Quadratic (3)	Local Quadratic Polynomial (4)	OLS Quadratic (5)	OLS Quadratic (6)
Withdrawal Date $\leq$ May 30^{th}, 2022	0.057***	0.065***	0.060**	0.069***	0.062**	0.055**
	(0.022)	(0.023)	(0.023)	(0.024)	(0.028)	(0.027)
Ineligible Mean	0.781	0.781	0.781	0.642	0.642	0.642
Observations	15,098	15,098	15,098	15,098	15,098	15,098
Schools	2,330	2,330	2,330	2,725	2,725	2,725
Optimal Bandwidth	18	18	18	18	18	18
Degree	2	2	2	2	2	2
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FE	No	Yes	Yes	No	Yes	Yes
Graduated in 2021 (= 1)	No	Yes	Yes	No	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes

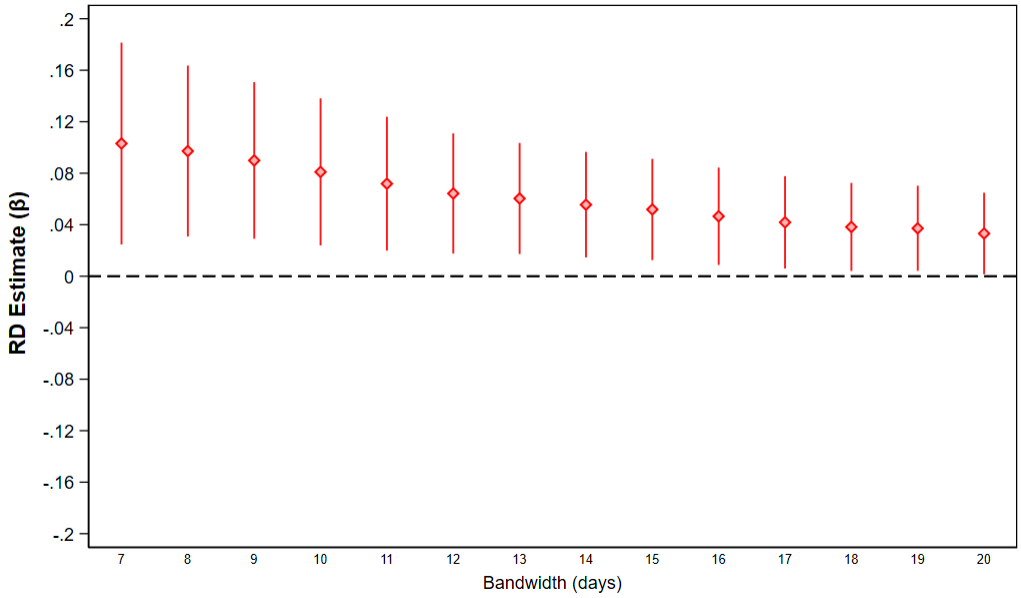
Note: Sample of analysis are evaluation sample students. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022) and/or graduated from school by the end of the year (Dec 31^{st}, 2022), using a second degree polynomial. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Optimal bandwidth is fixed at $\pm$ 18 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight. All models control for school fixed effects. Columns (1) and (4) estimate a local linear polynomial model, using the command `rdrobust` in `Stata`. Regression models in columns (2) and (5) show estimates from a quadratic OLS model, controlling for day-of-week fixed effects and a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year) or not. Regression models in columns (3) and (6) additionally control for a set of pre-treatment covariates, including gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, a dummy if the student is classified as preferent, and a dummy if the student is in primary school. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Figure A.VI: Report Effects on Graduation: Yearly Dynamics



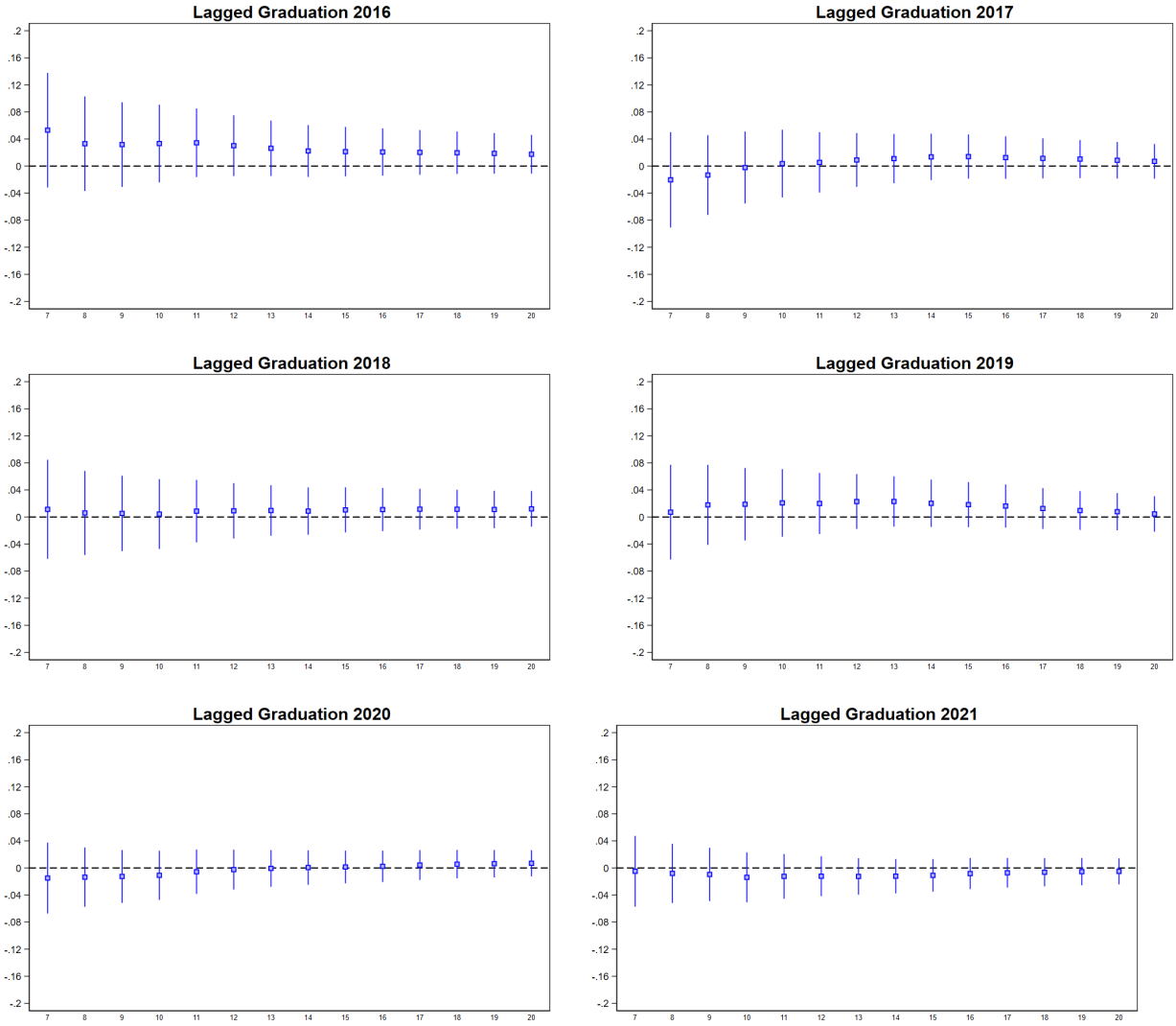
Note: Sample of analysis are 2022 students who disenrolled within the optimal bandwidth of $\pm$ 14 school days from the cutoff date, May 30^{th}, 2022. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student graduated by Dec 31^{st} in each pre-treatment year (2016-2021, in blue) as well as in the treatment year (2022, in red). For each year, each dot represents the estimate of the parameter β (and its associated 95% confidence interval) from equation 1, i.e., we control for school fixed effects, day-of-week fixed effects and a dummy on whether the disenrolled student graduated in previous year or not. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight. Withdrawn students in 2022 who have not yet entered into school at pre-treatment year are considered as missing observations in those pre-treatment years.

Figure A.VII: Report Effects on Graduation: Robustness to Alternative Bandwidths



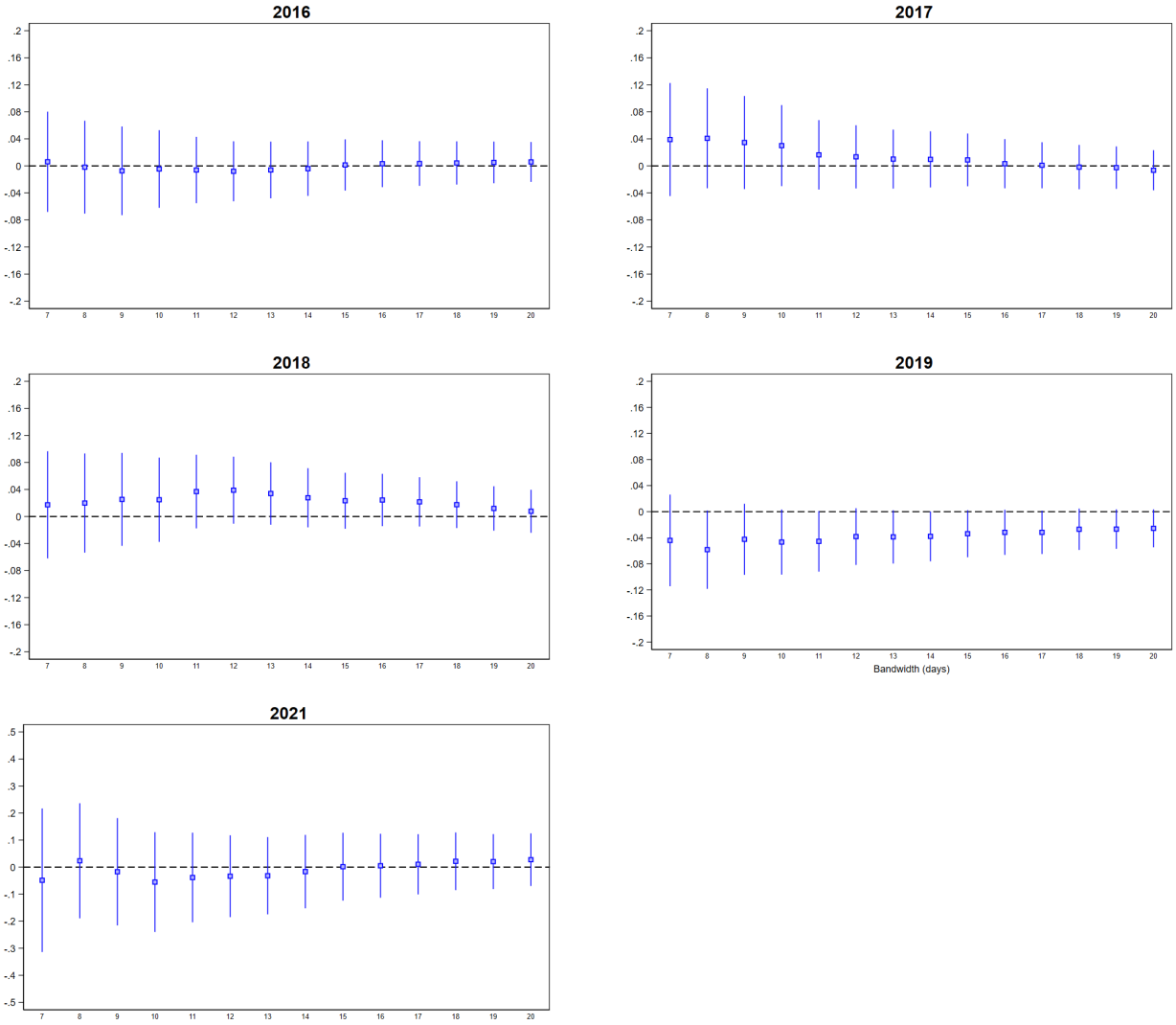
Note: Point estimates and their associated 95% confidence intervals for the β parameter from estimating our main equation 1 on graduation in 2022 across a range of optimal bandwidths around the cutoff date (May 30^{th}, 2022), from 7 to 20 school days, i.e., one month in total. All models control for school fixed effects, day-of-the-week fixed-effects, and lagged graduation (t-1). Standard errors are clustered at the group-school level. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. For instance, the point estimate associated to the bandwith 15 indicates the difference in graduation rates in 2022 between students who disenrolled from school in a date 15 or less school days before the cutoff date of May 30^{th}, 2022, with that of their counterparts who disenrolled from school 15 or less school days after the cutoff date.

Figure A.VIII: Robustness to Alternative Bandwidths in Pre-Treatment Years



Notes: Point estimates and their associated 95% confidence intervals for the β parameter from estimating our main equation 1 on lagged graduation, covering the pre-treatment years 2016-2021. A range of optimal bandwidths around the cutoff date (May 30^{th}, 2022) is used, from 7 to 20 school days, i.e., one month in total. All models control for school fixed effects, day-of-the-week fixed-effects, and graduation in the year previous to lagged graduation. Standard errors are clustered at the group-school level. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight. Each figure show the effects on school graduation in a given year for students who withdrew from school within a given bandwidth around May 30^{th}, 2022, where each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. For instance, in Figure Lagged Graduation 2021, the point estimate associated to the bandwith 15 indicates the difference in graduation rates in 2021 between students who disenrolled from school in a date 15 or less school days before the cutoff date of May 30^{th}, 2022, with that of their counterparts who disenrolled from school 15 or less school days after the cutoff date.

Figure A.IX: Robustness to Alternative Bandwidths: Pre- and Post-treatment Cohorts



Notes: Point estimates and their associated 95% confidence intervals for the β parameter from estimating our main equation 1 on lagged graduation, covering the pre-treatment years 2016-2021. A range of optimal bandwidths around the cutoff date (May 30^{th}, 2022) is used, from 7 to 20 school days, i.e., one month in total. All models control for school fixed effects, day-of-the-week fixed-effects, and graduation in the year previous to lagged graduation. Standard errors are clustered at the group-school level. Each year represents a different cohort of withdrawal students, i.e., 2016 cohort includes all students who withdrew from school in a window of 7 to 20 school days around May 30^{th}, 2016, while 2021 cohort includes all students who withdrew from school in a time window of 7 to 20 school days around May 30^{th}, 2021. For instance, in sub Figure 2021, the point estimate associated to the bandwith 15 indicates the difference in graduation rates in 2021 between students who disenrolled from school in a date 15 or less school days before May 30^{th}, 2021, with that of their counterparts who disenrolled from school 15 or less school days after that cutoff date. Observations are weighted using a triangular kernel centered on May 30^{th} each year. Year 2020 is excluded from the analysis since schools were closed around the cutoff date that year, and no data on withdrawals were produced.

Table A.VI: Falsification Tests

	If Re-Enrolled by Aug. 31^{st}, 2022			If Graduated by Dec. 31^{st}, 2022		
	Benchmark	Fals. Test 1	Fals. Test 2	Benchmark	Fals. Test 1	Fals. Test 2
Cutoff	Official Cutoff May30, 2022	Fals. Cutoff Apr29, 2022	Official Cutoff May30, 2022	Official Cutoff May30, 2022	Fals. Cutoff Apr29, 2022	Official Cutoff May30, 2022
Sample	May30 Evaluation Sample	Apr29 Falsif. Sample	May30 Transfer Stud. Sample	May30 Evaluation Sample	Apr29 Falsif. Sample	May30 Transfer Stud. Sample
Model	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
W. Date $\leq$ Cutoff Date	0.051***	-0.024	0.003	0.063***	-0.001	-0.005
	(0.018)	(0.021)	(0.004)	(0.020)	(0.023)	(0.013)
Counterfactual Mean	0.781	0.801	0.994	0.688	0.697	0.932
Observations	11,268	13,786	10,311	11,268	13,786	10,311
Schools	2,330	2,638	2,251	2,330	2,638	2,251
Optimal Bandwidth	14	14	14	14	14	14
Polynomial Degree	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FE	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (=1)	Yes	Yes	Yes	Yes	Yes	Yes

Note: Regression discontinuity estimates of the effect of withdrawing before or on cutoff date on re-enrollment and graduation rates. Estimates represent the β parameter from our main equation 1. All models control for school fixed effects, day-of-the-week fixed-effects, and lagged graduation ($t-1$). Optimal bandwidth is fixed at $\pm$ 14 school days. Regression models in columns (1) and (4) use the evaluation sample and the experimental cutoff date corresponding to May 30^{th}, 2022. Regression columns (2) and (5) use April 29^{th}, 2022, as a placebo cutoff date, which correspond to the penultimate day of April, with the sample of analysis including disenrolled students who withdrew from school within $\pm$ 14 school days from the placebo cutoff date. Regression columns (3) and (6) replicates exercise from columns (1) and (4) but sample of analysis includes transfer students only, defined as students who withdrew and re-enrolled in another school within 24 hours. The probability of being listed in the report is not uniform across the running variable but increases with the proximity of withdrawal to the cutoff date, hence in all models observations are weighted by using a triangular kernel weight. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.VII: Robustness Test: If Listed in Subsequent Dropout Report

	If Listed in December 2022 Report		
	Loc.Lin. Poly. (1)	OLS Linear (2)	OLS Linear (3)
W.D. $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	-0.0374**	-0.0430**	-0.0399**
	(0.0188)	(0.0205)	(0.0198)
Ineligible Mean	0.3328	0.3328	0.3328
Observations	11,268	11,268	11,268
Schools	2,330	2,330	2,330
Optimal Bandwidth	14	14	14
Polynomial Degree	1	1	1
School FE	Yes	Yes	Yes
Day-of-the-week FE	No	Yes	Yes
Graduated in 2021 (= 1)	No	Yes	Yes
Additional Controls	No	No	Yes

Note: Sample of analysis are evaluation sample students. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student was listed in a subsequent dropout report delivered on December 1^{st}, 2022 (5 months after the first report of June 28^{th}, 2022). Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. Observations are weighted by using a triangular kernel weight. All models control for school fixed effects. Columns (1) estimates a local linear polynomial model, using the command `rdrobust` in `Stata`. Regression model in column (2) show the RD estimate from a linear OLS model, controlling for day-of-week fixed effects and a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year) or not. Regression model in column (3) additionally control for a set of pre-treatment covariates, including gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, a dummy if the student is classified as preferent, and a dummy if the student is in primary school. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.VIII: Treatment Effects on Re-enrollment and Graduation, by Grade

	If Re-enrolled by August 31^{st}, 2022						If Graduated by December 31^{st}, 2022					
	Grades 1-4		Grades 5-8		Grades 9-12		Grades 1-4		Grades 5-8		Grades 9-12	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Withdrawal Date $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	0.059		0.014		0.075**		0.053		0.040		0.096***	
	(0.056)		(0.036)		(0.030)		(0.069)		(0.045)		(0.032)	
If Listed in the Report ($T_{isd} = 1$)		0.151		0.036		0.138**		0.136		0.102		0.177***
		(0.142)		(0.090)		(0.055)		(0.175)		(0.114)		(0.059)
Ineligible Mean	0.843		0.861		0.682		0.699		0.732		0.528	
Eligible Complier Mean		0.761		0.772		0.638		0.659		0.610		0.495
Ineligible Complier Mean		0.610		0.736		0.500		0.523		0.508		0.319
Observations	1,396	1,396	2,285	2,285	4,444	4,444	1,396	1,396	2,285	2,285	4,444	4,444
Schools	486	486	714	714	1,005	1,005	486	486	714	714	1,005	1,005
Optimal Bandwidth	14	14	14	14	14	14	14	14	14	14	14	14
Polynomial Degree	1	1	1	1	1	1	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grad. in 2021	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1^{st} Stage Est.		0.393		0.397		0.541		0.393		0.397		0.541
Cragg-Donald F-Statistic		71.69		134.33		509.72		71.69		134.33		509.72
Kleibergen-Paap F-Statistic		39.28		80.30		372.72		39.28		80.30		372.72

Note: Regression discontinuity estimates of the effect of withdrawing before the cutoff date (May 30^{th}, 2022) on re-enrollment and graduation rates, separated by school grades (1-4; 5-8; 9-12). In all cases, optimal bandwidth is fixed at $\pm$ 14 school days. Odds columns show estimates from an OLS regression model as in main equation 1, i.e., controlling for school fixed effects, day-of-week fixed effects, and a set of pre-treatment covariates including a dummy for whether the withdrawn student graduated in 2021 or not, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, a dummy if the student is classified as preferent. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Pair columns show 2SLS estimates of the effect of being listed in the report (T_{isd}) by exploiting whether the student disenrolled before or after the cutoff date (B_{isd}) as an instrument for being listed in the report. First stage estimates are obtained from regressing T_{isd} against B_{isd}, a linear function of our running variable r_{isd}, and their interaction, controlling for the full set of covariates, as in main equation 1. 2SLS estimates come from estimating equation model 2, with the 2SLS coefficient being equivalent to the reduced form estimate of β from equation 1 divided by the first stage estimate. It also reflects the difference between the Eligible and Ineligible Complier Means computed following Abadie (2002). The probability of being listed in the report is not uniform across the running variable but increases with the proximity of withdrawal to the cutoff date, and thus observations are weighted by using a triangular kernel weight. Std. err. clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.IX: Treatment Effects on School Re-enrollment, by Type of Re-enrollment — High School Dropouts

	If Re-enrolled by Aug. 31^{st}, 2022					
	All	Re-enrolled in School of Origin	Re-enrolled NOT in School of Origin			
	Total	Total	Total	Re-enrolled in Different School	Free Exams	N.A.
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
W.D. $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	0.0749**	0.0151	0.0598*	0.0417	0.0133	0.0048
	(0.0297)	(0.0197)	(0.0330)	(0.0339)	(0.0179)	(0.0121)
Observations	4,444	4,444	4,444	4,444	4,444	4,444
Ineligible Mean	0.683	0.070	0.612	0.498	0.076	0.038
Schools	1,005	1,005	1,005	1,005	1,005	1,005
Optimal Bandwidth	14	14	14	14	14	14
Polynomial Degree	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Grad. in 2021	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are high school students from the evaluation sample. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022), by school type. School of Origin is a dummy that equals 1 if the student re-enrolled in the same school from which she dropped out. Different School is a dummy that equals 1 if the student re-enrolled in a school other than the school of origin. Free Exams is a dummy that equals 1 if the student re-enrolled in the school system by taking Free Exams, not through re-enrollment in a school. N.A. is a dummy that equals 1 if the student is reported to be re-enrolled in the school system, but there is no information on the type of re-enrollment other than the school of origin where she re-enrolled. Estimate in column (1) is the overall effect on re-enrollment, and corresponds to the sum of the effects in columns (2) and (3). "Total" in column (3) is the sum of the effects in columns (4)-(6). Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. The probability of being listed in the report is not uniform but increases with the proximity of disenrollment to the cutoff date, hence observations are weighted by using a triangular kernel weight. All models control for school fixed effects, day-of-week fixed effects, and a set of additional pre-treatment covariates including a dummy for whether the withdrawn student graduated in 2021 or not, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, and a dummy if the student is classified as preferent. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.X: Role of Spatial Networks on Treatment Effects on Re-enrollment — High School Dropouts

	If Re-enrolled by Aug. 31^{st}, 2022								
	All	Same Municipality than School of Origin			Different Municipality than School of Origin			Free Exams	N.A.
	Total	Total	Regular School	Non-Regular School	Total	Regular School	Non-Regular School	Total	Total
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)
W.D $\leq$ May 30^{th}	0.0749**	0.0324	0.0796***	-0.0472**	0.0244	0.0124	0.0120	0.0133	0.0048
	(0.0297)	(0.0320)	(0.0283)	(0.0193)	(0.0302)	(0.0155)	(0.0277)	(0.0179)	(0.0121)
Ineligible Mean	0.683	0.312	0.235	0.077	0.257	0.039	0.218	0.076	0.038
Observations	4,444	4,444	4,444	4,444	4,444	4,444	4,444	4,444	4,444
Schools	1,005	1,005	1,005	1,005	1,005	1,005	1,005	1,005	1,005
Optimal Bandwidth	14	14	14	14	14	14	14	14	14
Polynomial Degree	1	1	1	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grad. in 2021	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

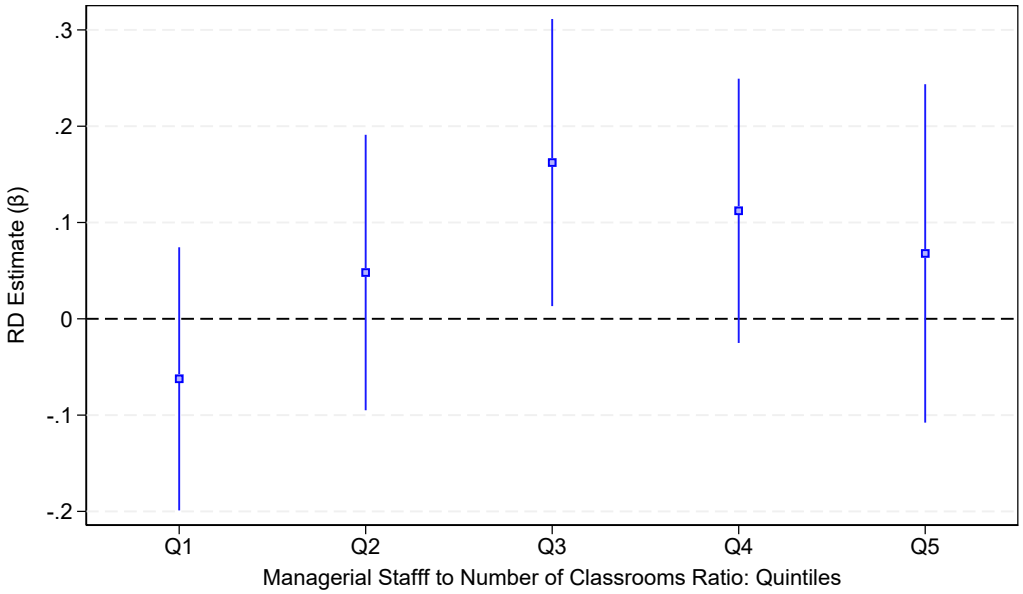
Note: Sample of analysis are high school students from the evaluation sample. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022). Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. Estimates are OLS coefficients from main equation 1, i.e., controlling for school fixed effects, day-of-week fixed effects, and a set of additional pre-treatment covariates including a dummy for whether the withdrawn student graduated in 2021 or not, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, and a dummy if the student is classified as preferent. Outcome dummies are defined based on whether the dropout student re-enrolled in a school located within the same municipality of the origin school (columns (2)-(4)), in a school located in other municipalities (columns (5)-(7)), if took free exams (columns (8)), or if no information on the receiving institution is available (columns (9)). Estimate in column (1) is the overall effect on re-enrollment, and corresponds to the sum of the effects in columns (2), (5), (8), and (9). Regular schools provide structured learning programs based on official curricula, serving the general student population. Non-regular schools encompass flexible or remedial programs for students with special needs (e.g., adult education programs, special education schools, among others). The columns "Total" in columns (2) and (5) sums regular and non-regular schools under each heading. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Std. err. clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.XI: Role of Institutional Networks on Treatment Effects on Re-enrollment — High School Dropouts

	If Re-enrolled by Aug. 31^{st}, 2022						
	Same Municipality than School of Origin						
	All	If Re-enrolled in a School From Origin School's Network			If Re-enrolled in a School Out of Origin School's Network		
	Total	Total	Regular School	Non-Regular School	Total	Regular School	Non-Regular School
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)
W.D $\leq$ May 30^{th}	0.0324	0.0342	0.0509**	-0.0167	-0.0018	0.0287	-0.0305*
	(0.0320)	(0.0251)	(0.0229)	(0.0119)	(0.0255)	(0.0216)	(0.0158)
Ineligible Mean	0.312	0.133	0.109	0.024	0.179	0.126	0.053
Observations	4,444	4,444	4,444	4,444	4,444	4,444	4,444
Schools	1,005	1,005	1,005	1,005	1,005	1,005	1,005
Optimal Bandwidth	14	14	14	14	14	14	14
Polynomial Degree	1	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grad. in 2021	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are high school students from the evaluation sample. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022). Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. Estimates are OLS coefficients from main equation 1, i.e., controlling for school fixed effects, day-of-week fixed effects, and a set of additional pre-treatment covariates including a dummy for whether the withdrawn student graduated in 2021 or not, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, and a dummy if the student is classified as preferent. Outcome dummies are defined based on whether the dropout student re-enrolled in a school located within the same municipality of the origin school (column (1)), which can be either in a school from origin schools's network (columns (2)-(4)), or in a school out of the origin school's network (columns (5)-(7)). Students who re-enrolled in the origin school are coded as if re-enrolled in the origin school's institutional network. Column (1) estimate sums the total estimate in columns (2) and (5). Regular schools provide structured learning programs based on official curricula, serving the general student population. Non-regular schools encompass flexible or remedial programs for students with special needs (e.g., adult education programs, special education schools, among others). The columns "Total" in columns (2) and (5) sum regular and non-regular schools under each heading. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Std. err. clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Figure A.X: Report effects across Quintiles of Managerial Staff to Classrooms Ratio in Origin School



Notes: Sample of analysis are 2022 high school students who disenrolled within the optimal bandwidth of $\pm$ 14 school days from the cutoff date, May 30^{th}, 2022. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st}, by whether the school of origin from which the student dropped out had a Managerial Staff to Classrooms Ratio located in Quintile 1,2,3,4, or 5 by the time the reports were delivered (June 28^{th}, 2022). For each quintile, each dot represents the estimate of the parameter β (and its associated 95% confidence interval) from equation 1, i.e., we control for school fixed effects, day-of-week fixed effects, and a set of additional pre-treatment covariates including a dummy for whether the withdrawn student graduated in 2021 or not, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, and a dummy if the student is classified as preferent. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight.

Table A.XII: Heterogeneous effects by Managerial Staff to Classrooms Ratio in Origin School

	If Re-enrolled by Aug. 31^{st}, 2022		
		Ratio of Managerial Staff to Number of Classrooms in Origin School	
	All	Above Median	Below Median
	OLS (1)	OLS (2)	OLS (3)
W.D $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	0.075**	0.126***	0.015
	(0.030)	(0.045)	(0.044)
Ineligible Mean	0.682	0.701	0.665
Observations	4,444	2,126	2,318
Schools	1,005	493	512
Optimal Bandwidth	14	14	14
Polynomial Degree	1	1	1
School FEs	Yes	Yes	Yes
Day-of-week FEs	Yes	Yes	Yes
Grad. in 2021	Yes	Yes	Yes
Additional Covariates	Yes	Yes	Yes

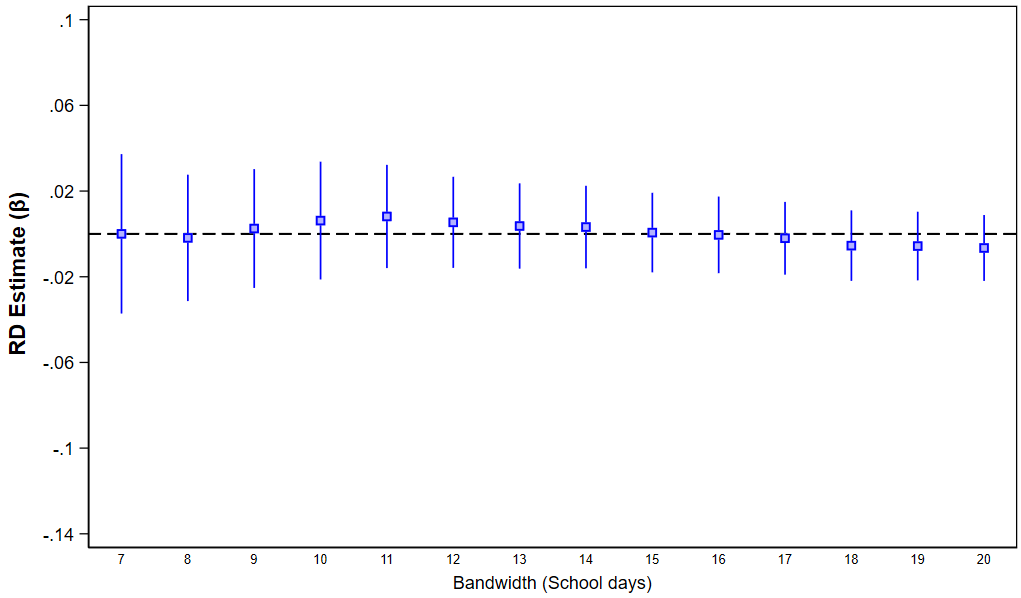
Note: Sample of analysis are high school students from the evaluation sample. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022). Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. Estimates are OLS coefficients from main equation 1, i.e., controlling for school fixed effects, day-of-week fixed effects, and a set of additional pre-treatment covariates including a dummy for whether the withdrawn student graduated in 2021 or not, as well as gender, age dummies, lag-age dummies, parents' schooling dummies, a dummy for whether the student committed crime, crime severity dummies, gpa dummies, and a dummy if the student is classified as preferent. Sample in column (1) includes all dropouts students from high school. Sample in column (2) includes only high school students who dropped out from schools of origin whose managerial staff to classroom ratio was above the median by the time the reports were distributed, with the median being 0.357 managerial personnel per classroom. Sample in column (3) includes only high school students who dropped out from schools of origin whose managerial staff to classroom ratio was below the median. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Std. err. clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.XIII: Balance Checks – Evaluation Crime Sub-Sample

	Control Mean	Mean Diff.	SE	Obs. Left	Obs. Right
Panel A. Demographics					
Male (=1)	0.516	-0.043	(0.045)	1,534	1,394
Migrant (=1)	0.075	0.019	(0.025)	1,534	1,394
High School in 2022 (=1)	0.896	0.003	(0.027)	1,534	1,394
Age in 2022 (=1)	15.660	-0.076	(0.088)	1,534	1,394
14 years old (=1)	0.172	0.017	(0.032)	1,534	1,394
15 years old (=1)	0.259	-0.008	(0.041)	1,534	1,394
16 years old (=1)	0.307	0.041	(0.046)	1,534	1,394
17 years old (=1)	0.262	-0.050	(0.042)	1,534	1,394
Integrated Student 2021 (=1)	0.119	0.045	(0.029)	1,534	1,394
Log(Income)	12.691	0.052	(0.112)	831	769
Panel B. Baseline Performance					
School Attendance 2021	81.380	1.816	(1.237)	1,534	1,394
School GPA 2021	5.166	-0.093	(0.060)	1,514	1,378
4th Grade Achievement Z Score 2014-18	-0.338	-0.017	(0.106)	1,082	998
Panel C. Lagged Outcomes					
If Committed Crime Before May 30^{th}, 2022 (=1)	0.017	0.005	(0.011)	1,534	1,394
Lagged Graduation 2021 (=1)	0.882	-0.014	(0.029)	1,534	1,394
Lagged Graduation 2020 (=1)	0.948	-0.011	(0.022)	1,534	1,394
Lagged Graduation 2019 (=1)	0.876	0.031	(0.029)	1,534	1,394
Lagged Graduation 2018 (=1)	0.874	-0.025	(0.026)	1,534	1,394
Lagged Graduation 2017 (=1)	0.881	0.016	(0.026)	1,534	1,394
Lagged Graduation 2016 (=1)	0.875	0.029	(0.025)	1,534	1,393
Missing in Lagged Graduation (=1)	0.001	0.004	(0.002)	1,534	1,394
Panel D. Re-enrollment					
If Re-enrolled Before Cutoff (=1)	0.362	0.021	(0.052)	1,534	1,394

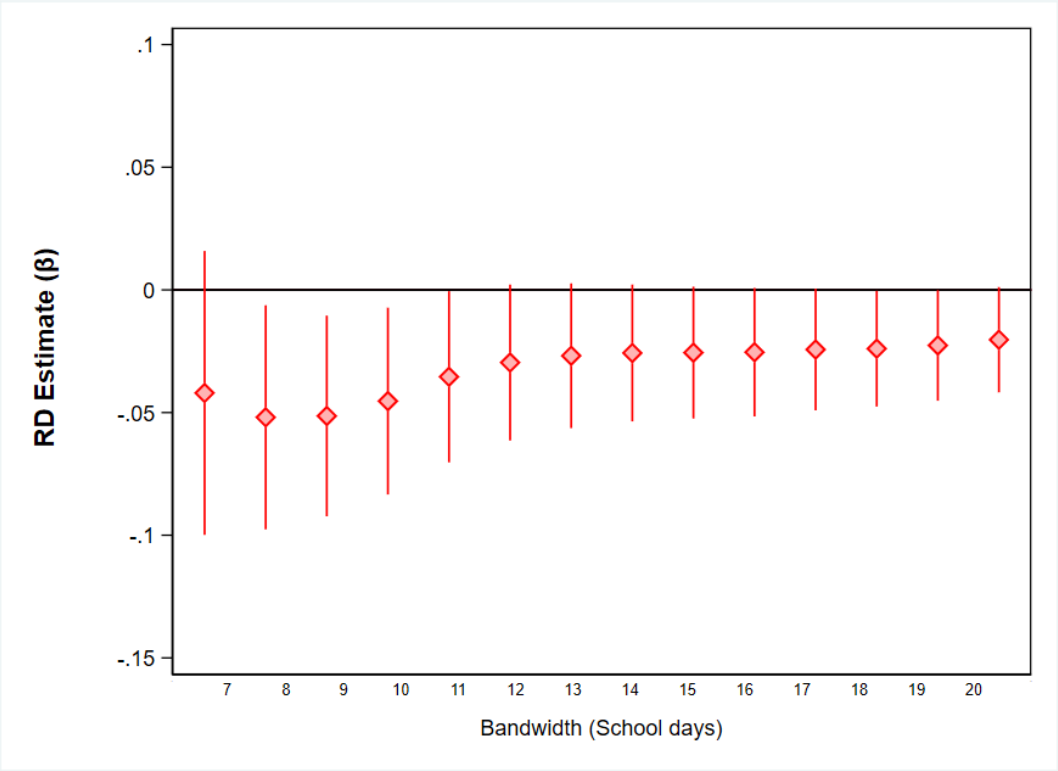
Notes: Baseline characteristics of students in evaluation sample to assess effects on crime (12-days bandwidth). Control Mean is the average value of each covariate for students who disenrolled after May 30^{th}, 2022, the cutoff date. Diff. denotes the mean difference between students on either side of the cutoff, and are obtained by estimating the very same equation model 1 but using baseline data, i.e., we regress each baseline variable against the running variable, the treatment dummy, and their interaction, and controlling for day-of-the-week and school fixed effects. Observations are weighted by a triangular kernel centered at May 30^{th}. As in the post-treatment analysis, since the triangular kernel weight observations at each extreme of the running variable by zero, the total number of observations is reduced from 3,152 to 2,928. Standard errors clustered at the treat-school level are shown in parentheses in column S.E. Math and Language Z-Score Achievement data is compiled for a subsample of high school students, specifically those who had achievement records available from when they were in 4th grade during the years 2014 to 2018 (no achievement data is available for the years 2019-2021). Lagged graduation indicates the share of students from the 2022 cohort that graduated in each year of the period 2016-2021. Missing in Lagged Graduation is a dummy that equals 1 if the observation was missing in any of the lagged graduation years, and zero otherwise. If Re-enrolled Before Cutoff is a dummy that equals 1 if the left-hand-side withdrawn student re-enrolled before May 30^{th} and zero otherwise. For right-hand-side withdrawn students, the dummy takes a value of 1 if the student re-enrolled before a placebo date set as of June 15^{th}, i.e., it counts 12 school days after the cutoff (May 30^{th}), such that the maximum time window for re-enrollement is equal for both before- and after-cutoff disenrolled students. *p<0.10; **p<0.05; ***p<0.01.

Figure A.XI: Rob. Check to Alternative Bandwidths: If enrolled in SENAME programs before May30th, 2022



Notes: Point estimates and their associated 95% and 90% confidence intervals for the β parameter from our main equation 1 using a wide range of optimal bandwidths around the cutoff date, May 30th, 2022. Optimal bandwidths range from 7 to 20 school days, i.e., one month in total. Outcome of interest is whether the withdrawn student participated in SENAME programs before May 30th, 2022, meaning they committed a SENAME-eligible crime after 14 years old but before the cutoff date. All models control for school fixed effects, a dummy for whether the withdrawn student graduated in 2021 (pre-treatment year) or not, and day-of-week fixed effects, as in main equation 1. Standard errors clustered at the group-school level. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight centered on cutoff date.

Figure A.XII: Robustness to Alternative Bandwidths: If committed a PI-Crime



Notes: Point estimates and their associated 95% confidence interval for the β parameter from our main equation 1 using a wide range of optimal bandwidths around the cutoff date, May 30^{th}, 2022. Optimal bandwidths range from 7 to 20 school days, i.e., one month in total. Outcome of interest is whether the disenrolled student committed a PI-crime between July 1^{st}, 2022, and April 30^{th}, 2024. All models control for school fixed effects, day-of-week fixed effects, and a dummy for whether the disenrolled student committed crime in pre-treatment period. Standard errors clustered at the group-school level. The probability of being listed in the report is not uniform but increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight centered on cutoff date.

Table A.XIV: Treatment Effects on Crime: Robustness

	If committed Crime PI cases Dec 1st, 2022 - April 30, 2024				If committed Crime PI + Not-PI cases Dec 1st, 2022 - April 30, 2024			
	Loc.Lin. Poly. (1)	OLS Linear (2)	OLS Linear (3)	2SLS (4)	Loc.Lin. Poly. (5)	OLS Linear (6)	OLS Linear (7)	2SLS (8)
W.D. $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	-0.0192*	-0.0236*	-0.0279**		-0.0198*	-0.0244*	-0.0288**	
	(0.0107)	(0.0134)	(0.0136)		(0.0109)	(0.0136)	(0.0139)	
If listed in the Report ($T_{isd} = 1$)				-0.0557**				-0.0574**
				(0.0272)				(0.0277)
Ineligible Mean	0.0215	0.0215	0.0215	0.0215	0.0229	0.0229	0.0229	0.0229
Eligible Complier Mean				0.0130				0.0122
Ineligible Complier Mean				0.0687				0.0696
Observations	2,928	2,928	2,928	2,928	2,928	2,928	2,928	2,928
Schools	840	840	840	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
If Crime at baseline (= 1)	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Day-of-week FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes
1st Stage Estimate				0.5019				0.5019
Cragg-Donald F-Statistic				240.94				240.94
Kleibergen-Paap F-Statistic				175.63				175.63

Note: Sample of analysis are students aged 14-17 years old. Regression discontinuity estimates of the effect of withdrawing before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on a dummy of whether the withdrawn student committed a crime or not. Only crime events produced since Dec 1st, 2022 up to April 30th, 2024 are counted, i.e., since 5 months from the release of dropout reports (June 28th, 2022). Regression models (1)-(4) only count crime events with pretrial injunction (PI), whose sanction is usually imposed within 24-48 hours of the crime. In models (5)-(8) we also count Not-PI crime events whose court order is one year ahead or more of the initial counting date (i.e., Not-PI crime events from Dec 1st, 2023 onwards). All models control for school FEs. Columns (1) and (5) estimate a local linear polynomial model. Regression models in columns (2) and (6) show estimates from a linear OLS model, controlling for day-of-week fixed effects, and a dummy on whether the withdrawn student committed crime before the reports were disbursed, as well as dummies for the severity (1-5) of the committed crime. Regression models in columns (3) and (7) additionally control for pre-treatment covariates, including a dummy on whether the student graduated in 2021, gender, age dummies, lag-age dummies, parents' schooling dummies, school attendance's tercile dummies, grade dummies, GPA tercile dummies, if preferent student, if migrant student, and if in primary school. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Columns (4) and (8) show Fuzzy IV estimates exploiting the cutoff date (B_{isd}) as an instrument for being listed in the report (T_{isd}), which reflects the ratio of the reduced form estimates over the first stage estimates. It also reflects the difference between the Eligible and Ineligible Complier Means computed following Abadie, 2002. Opt. bandwidth is fixed at $\pm$ 12 school days, which is selected following MSE-optimal bandwidth choices. The probability of being listed in the report increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.XV: Impact of withdrawing before cutoff date on Crime - RD Estimates using Quadratic Model

	If committed Crime July 1st, 2022 - April 30, 2024				If committed Crime Dec 1st, 2022 - April 30, 2024			
	PI cases		PI+Not-PI cases		PI cases		PI+Not-PI cases	
	OLS Quad. (1)	OLS Quad. (2)	OLS Quad. (3)	OLS Quad. (4)	OLS Quad. (5)	OLS Quad. (6)	OLS Quad. (7)	OLS Quad. (8)
W.D. $\leq$ May 30^{th}, 2022	-0.0447*	-0.0481*	-0.0358	-0.0399	-0.0437*	-0.0475**	-0.0464**	-0.0504**
	(0.0264)	(0.0263)	(0.0281)	(0.0281)	(0.0235)	(0.0237)	(0.0235)	(0.0238)
Ineligible Mean	0.0321	0.0321	0.0383	0.0383	0.0215	0.0215	0.0229	0.0229
Observations	2,928	2,928	2,928	2,928	2,928	2,928	2,928	2,928
Schools	840	840	840	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12	12	12
Polynomial Degree	2	2	2	2	2	2	2	2
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
If Crime at baseline (= 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes	No	Yes	No	Yes

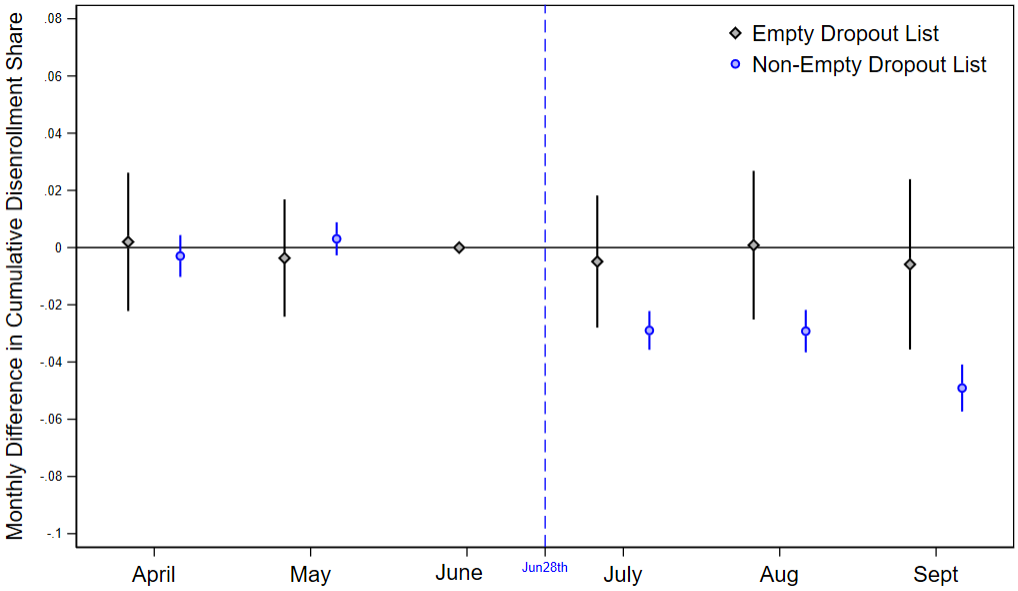
Note: Sample of analysis are students aged 14-17 years old. Regression discontinuity estimates of the effect of withdrawing before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on a dummy of whether the withdrawn student committed a crime or not, using quadratic OLS models. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Regression models (1)-(4) only count crime events from July 1st, 2022 to April 30th, 2024, while models (5)-(8) only count crime events from Dec. 1st, 2022 to April 30th, 2024. Regression models (1),(2),(5), and (6) only count crime events with pretrial injunction (PI), while models (3),(4),(7), and (8) also count Not-PI crime events whose court order is one year ahead or more of the initial counting date (i.e., Not-PI crime events from Dec 1st, 2023 onwards). All models control for school FEs, day-of-week fixed effects, and a dummy on whether the withdrawn student committed crime before the reports were disbursed, as well as dummies for the severity (1-5) of the committed crime. Regression models in columns (2), (4), (6), and (8) additionally control for pre-treatment covariates, including a dummy on whether the student graduated in 2021, gender, age dummies, lag-age dummies, parents' schooling dummies, school attendance's tercile dummies, grade dummies, GPA tercile dummies, if preferent student, if migrant student, and if in primary school. Opt. bandwidth is fixed at $\pm$ 12 school days, which is selected following MSE-optimal bandwidth choices. The probability of being listed in the report increases with the proximity of withdrawal to the cutoff date, hence observations are weighted by using a triangular kernel weight. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.XVI: Cost-Effectiveness: Crime-reduction Savings derived from Reports Implementation

Item	Description	Per-Unit Cost
Crime Costs (Savings)		Per-crime Cost
Anticipation Cost	Expenditures aimed at directly preventing and/or deterring the occurrence of crimes and their immediate impacts, e.g., alarms, security devices, guards, and others.	$ 4,131
Consequence Cost	Expenditures incurred as a direct effect of criminal acts, e.g., those related to the treatment and recovery of crime victims (medical and psychological care), and the destruction of goods and property (value of destroyed goods).	$ 2,272
Response Cost	Expenditures that occur after the commission of a crime and are aimed at the investigation, prosecution, and resolution of such crimes, e.g., police and prison management, admin. costs of judicial system, SENAME and inmates rehabilitation programs, etc.	$ 3,197
Unit Cost	Anticipation + Consequence + Response Costs, per crime	$ 9,599
Report Implementation Costs		Per-Student Cost
Centralized Dropout Monitoring System (SIGE)	SIGE System design, software development, server infrastructure, cybersecurity measures, and ongoing maintenance by MINEDUC.	$ 21.32
Dropout Reports	Identification of dropout students, and production and delivery of dropout reports.	$ 1.76
School Agency Costs	Expenditures schools incur in efforts to reintegrate dropout students into the education system, usually in the form of staff time to contact families, gathering information about potential schools that could accept the students, and coordinating the logistics of re-enrollment.	$ 70.28
Unit Cost	SIGE + Dropout Reports + School Monitoring, per High-School dropout student	$ 93.37
Benefit-Cost Ratio	Saved cost per crime divided by implementation cost per High-School dropout student	103:1
Cost Effectiveness		
# HS Dropouts in 2022	Number of High School dropout students in 2022	46,897
Total Implementation Costs	# HS Dropouts in 2022 $\times$ per-student cost	$4,378,547
# HS Dropouts in 2022 (would-be criminals)	Estimated number of high-school dropout students in 2022 that would have committed crime had not been listed in the report.	3,574
Total (would-be) Crime Costs (Savings)	# HS Dropouts in 2022 (would-be criminals) $\times$ per-crime cost	$34,306,826
Total Savings	Total (would-be) Crime Costs (Savings) - Total Implementation Costs	$ 29,928,279
Share of GDP in 2022 (%)		0.010%

Note: Yearly costs in US dollars of 2022 (873.18 Chilean pesos per 1 USD). The cost of crime are taken from Alfaro et al. (2024). Only “Crimes of High Social Connotation" are considered, including homicides, rape and sexual offenses, robbery with violence or intimidation, sudden or surprise theft, grievous, moderate, and minor bodily harm, domestic violence, burglary, vehicle theft, other thefts involving force against property, and larceny. Design Costs of Centralized Dropout Monitoring Systems (SIGE) and costs associated with the production of dropout reports is based on budget information provided by MINEDUC. School Agency Costs are calculated as the 10% yearly average wage of a school administrator per school in the Chilean educational system. According to https://data.worldbank.org/, GDP in Chile by 2022 is $US 301 billion.

Figure A.XIII: 2022-2016 Monthly Differences in Cumulative Disenrollment Share



Notes: Unit of analysis are schools. Mean differences in the monthly cumulative disenrollment share of high-school students per school between 2016 and 2022, with 2016 as the baseline year, which aligns with 2022 in terms of calendar days. June 28^{th} is highlighted as the report release date, and June is used as the omitted month, hence estimates reflect mean differences relative to mean differences between June 2022 and June 2016. Sample of analysis includes 2,608 schools whose dropout report included a list of dropout students and 390 schools whose dropout report included an empty list, i.e., no dropout student was listed in their report. The two samples correspond to the universe of schools with at least one disenrollment in the analyzed period.

B. Data Description

Enrollment Data. These are enrollment records from the SIGE platform, spanning from 2016 to 2023, excluding 2020 due to school closures that resulted in the system's suspension. Schools in Chile are required to notify MINEDUC whenever a student enrolls or withdraw the school. We utilize a raw version of the enrollment data containing all school declarations submitted to the system, which includes the precise date and time each declaration was made. Since the Chilean educational funding model relies on a per-student voucher system, the enrollment and withdrawal records must be accurate, as inaccuracies can result in penalties from the Superintendency of Education. The latter allows us to track student movements between schools with high precision.

Disenrollment Data. MINEDUC provide anonymized data on disenrolled students at three temporal intervals during 2022: May 30^{th} (data used to build the dropout reports), August 31^{th}, and December 1^{st}. The dropout data is restricted to students who, having enrolled in the previous year, did not enrolled in the actual year or enrolled but withdrew without subsequent re-enrollment in any state-recognized schooling institution, and have not completed 12^{th} grade[63].

Performance Data. This is student-level microdata on graduation from 2016 to 2023, sourced from the Annual Performance Data released by MINEDUC. It includes a dummy indicating whether a student was promoted to the next grade or, for 12^{th} graders, graduated from high school. The data also comprise average yearly attendance, grade point average (GPA), sex, grade, age, and migrant status, among others.

Other data sources. We use historical academic achievement data, specifically math and language test scores from the Chilean System for Measuring Educational Quality (SIMCE) for the years 2014 to 2018. This allows us to merge 4^{th}-grade academic achievements for cohorts present in grades 8 through 12 in 2022. We also draw on SIMCE parent surveys for insights into students' socioeconomic backgrounds and use public records on teachers elaborated annually by MINEDUC to construct teaching staff and educators school-level capabilities, the latter being used as proxies to explore school-level educational drivers of our main effects. Lastly, we integrate the 2022 SEP socioeconomic data, categorizing socioeconomic status into three groups: Priority Students (from the lowest income quintiles 1 and 2), Preferential Students (from quintiles 3 and 4), and Non-SEP Students (those not considered economically disadvantaged or without available information).

Crime Data. We measure juvenile crime through the SENAME administrative data, which track 14-17-year-olds processed under Chile's Adolescent Criminal Responsibility Law (LRPA) for serious crimes like homicide, rape, and aggravated assault, as well as property crimes and drug trafficking. SENAME is the public institution in charge of implementing social reintegration programs for adolescent processed by LRPA law, all of whom are mandatory admitted into SENAME programs. Our dataset focuses on violent crimes, excluding adolescents in programs for rights violations without court orders.

[63]To ensure accuracy, MINEDUC adjust for various factors that might lead to disenrollment, including student death or changes in social security numbers, the latter being common among migrant students.