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 centralized dropout monitoring system that is both low-cost and readily scalable. It identifies dropouts in a timely manner and provides 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 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 32% 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 role, with most re-enrollments occurring in schools other than the school of origin but within the same educational network. Two years later, we find the crime rate of listed dropouts was 79% lower compared to that of non-listed dropouts. Both extensive margin effects associated with incapacitation as well as intensive margin effects related to lower crime rates in schools of destination contribute to the overall crime prevention result. At-scale implementation costs averages US$98 per dropout student, yielding a benefit-cost ratio of 6:1 for preventing crime. Even without considering labor market returns from education or savings from reduced crime in adulthood, the reports generate annual savings equivalent to US$9 million, or about 0.003% of Chile's GDP. Centralized dropout monitoring systems offer a 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 (Jacob and Lefgren, 2003; Cook and Songman, 2016; Heller et al., 2017). The quantity and quality of schooling also influence criminal behavior in adulthood, underscoring the long-term effects of dropping out on criminal activity[2]. A wide array of dropout-preventive measures have proven to be cost-effective in reducing school dropout and its link to juvenile crime[3]. In line with school retention as the traditional focus of education policy, the vast majority of these initiatives share a common goal: preventing dropout before it occurs. Yet far less is known about how to bring back students who have already left the system. This gap carries significant policy relevance. Once preventive measures fail and at-risk students drop out, schools lose their incapacitation role, thereby increasing the risk of criminal involvement.

In this paper, we examine the causal role of centralized dropout monitoring systems in facilitating the re-enrollment of dropout students 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 student who disenrolled from their dependencies has re-enrolled elsewhere or dropped out entirely[4].

Post-pandemic, Chile's Ministry of Education launched a re-enrollment initiative targeting all dropout students from public schools. MINEDUC built school-specific reports listing former students who had left school in the past 12 months and had not re-enrolled elsewhere. Known as Dropout Reports, a key innovation is that these are generated using MINEDUC's Education

[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]A non-exhaustive list of papers documenting the role of education on crime includes 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; Boshoff et al., 2025

[3]This includes, for instance, minimum dropout age (MDA) laws (Angrist and Krueger, 1991; Lleras-Muney, 2002; Oreopoulos, 2006; Anderson, 2014), 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. Other dropout-preventive initiatives like General Education Development (GED) programs may generate the opposite effect (Heckman et al., 2012).

[4]This contrasts with other systems, such as the U.S. school system, where the large majority of students (84%, U.S. Census Bureau, 2023) attend public schools assigned by their local district, and where district authorities are the main providers. Additionally, state-level reporting ensures greater standardization. This combination of residential zoning and centralized oversight reduces information frictions about disenrolled students in the U.S. compared to Chile. While students who move to private schools or homeschooling are harder to track, they still represent a relatively small share compared to Chile, where the education market is fully decentralized and cross-district enrollment is common, making tracking far more difficult.

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[5]. 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 an up-to-date list of dropouts through an efficient process.

The Dropout Reports include aggregate statistics on school-level dropout behavior, along with detailed information on each individual dropout, with the goal of encouraging schools to facilitate their re-enrollment. Importantly, even if aware that a student has dropped out, schools may have little incentive to bring dropouts back: they often underperform academically and there are no penalties for failing to follow up. Also, some dropouts are reluctant to return to the same school as they have already shown a preference for leaving, further reducing their chances of re-enrollment. Aware of these barriers, MINEDUC does not mandate that schools re-enroll former students in their own classrooms. Instead, the reports are designed to prompt schools to activate their institutional and geographic networks —for instance, by coordinating with nearby schools or municipal authorities— to help students re-enter the education system through alternative pathways[6].

The Dropout Reports were delivered on June 28^{th}, 2022, across all public schools in the Chilean education system. Crucially, the list of dropouts only included those who had disenrolled before May 30^{st}, 2022, and had not re-enrolled either in the same or other school by that date. Hence, 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, and use a regression discontinuity design 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. Since the disenrollment date recorded in the SIGE system is endogenous to each school's reporting practices, identification relies on within-school comparisons of dropouts. We therefore restrict the sample to "two-sided" schools, i.e., schools with at least one dropout on each side of the cutoff. These schools tend to have higher dropout rates and serve more vulnerable students than the average. While this limits the external validity of our findings, it focuses the analysis on precisely those schools where the risk of criminal involvement among dropouts is highest, and where targeted policy tools like the Dropout Reports are most urgently needed.

Our population of interest are dropouts aged 14-17, as this is the cohort for which juvenile crime data is available, which we further merge with MINEDUC enrollment and disenrollment records. After two months of program exposure, we find the re-enrollment rate among dropouts listed in the report increased by 32% compared to ineligible compliers, which in turn led to a similar increase in

[5]MINEDUC distinguishes between at-risk students and dropouts. Students with low levels of attendance are considered at risk of dropping out, but as long as they remain formally enrolled in a school, they are not classified as dropouts, and thus are not listed in the report.

[6]This strategy aligns with the findings of Gallego et al. (2024b), who emphasize the importance of personal assistance in interventions aimed at long-term educational improvement. By mobilizing networks to identify and support dropouts, schools can offer a more individualized and context-sensitive response, reinforcing the view that sustained personal engagement can foster lasting behavioral change.

grade completion by the end of the year. The results are robust to placebo cutoffs, placebo samples, and different functional forms, as well as to multiple variations of the optimal bandwidths around the cutoff date.

The re-enrollment effects were largely driven by dropouts enrolling in schools other than their schools of origin. 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. The effectiveness of these networks depended on school capabilities —schools with stronger management structures were more successful in facilitating the reallocation of dropouts, underscoring the importance of organizational capacity in dropout recovery efforts.

Having shown a positive effect on school re-enrollment, we next examine whether report-driven re-enrollment helped prevent dropouts from engaging in crime. To do so, we leverage data from the Youth National Protection Program (SENAME), which includes the universe of youth aged 14–17 who have committed crimes and is available for up to 24 months following the report's distribution. Our results show a sizable reduction in the propensity to commit crime among listed students relative to ineligible compliers, with this mostly driven by a reduction in non-severe crimes, including assaults, property damage, thefts, and drug-related offenses.

We hypothesize that crime prevention is partly driven by extensive margin effects associated with incapacitation factors, i.e., a mechanical decline in criminal activity resulting from the institutional control that schools exert over re-enrolled dropouts, which restricts their opportunity to offend (Jacob and Lefgren, 2003; Luallen, 2006; Lochner, 2011; Landersø et al., 2017; Huttunen et al., 2023). Note, however, that our results do not imply that every student who re-enrolled due to the report went from committing a crime to not committing a crime. Our 2SLS estimate on re-enrollment is 18.7 *pp.*, which almost triples in size the effect on crime prevention (7.6 *pp.*). That is, had all the crime prevention were explained by re-enrollment, then the re-enrollment-crime elasticity would be -0.4 ($= -7.6/18.7$)[7]. While this elasticity seems large, it is far from being implausible in our context. First, our population of study is extremely vulnerable from engaging in crime. One in ten dropouts aged 14-17 in Chile commit a crime, which is about twenty times higher than that for non-dropouts. This is in line with the substantial qualitative evidence linking school dropout to criminal behavior in Chile (Mendoza-Lira et al., 2023, Alvarez, 2014, and Reyes-Quilodrán et al., 2025). Second, intensive margin effects related to the characteristics of the school of destination (SOD) of re-enrolled dropouts may also contribute to the observed impacts. That would be the case if, for instance, peers in the schools of destination of treated re-enrolled dropouts show lower crime levels than peers in schools where untreated dropouts re-enrolled.

We examine the presence of intensive margin effects by comparing the crime levels among

[7]Of course, the elasticity estimate does not warrant causal interpretation. Alternatively, one may be tempted to exploit the exogenous cutoff date as an instrument for being re-enrolled into school to estimate the causal effect of school re-enrollment on crime. However, the excludability assumption may not be met in practice, impeding a causal interpretation. For instance, a student who was listed in the report may remain disenrolled and not commit crime afterwards because he felt the school was trying to take care of him, a "psycho-gain" that reduced his chances of committing a crime. That is, being listed in the report did not change re-enrollment, but it still reduced the chances of committing crime, violating the excludability assumption.

SOD-peers of re-enrolled dropouts who disenrolled before (treatment) versus after (control) the cutoff date used for report construction. Since such a comparison is subject to selective attrition —the report generated a larger re-enrollment rate among treatment dropouts— we follow Lee (2009) and build upper and lower bound estimates of the intensive margin effect. We find that the crime rate of SOD-peers faced by re-enrolled dropouts eligible for the report is, on average, significantly different from that faced by their non-eligible counterparts, specially in the case of non-severe crimes committed by male peers, which is precisely the types of crimes driving the main prevention effects. Also, we find eligible re-enrolled dropouts are more likely to enroll in schools with a full-day schedule rather than half-day or evening programs, and they are less likely to attend schools offering adult education programs —typically held during high-crime hours.

Overall, considering only students aged 14–17 who dropped out from two-sided schools, we estimate that 1,338 of them would have engaged in crime in 2022. Had all been included in the reports, dropouts committing crime would have decreased to 281, meaning 1,056 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– meaning crime savings amount to US$ 10.14 millions. On the other side, the at-scale cost of implementing the reports is US$ 98 per dropout, for US$ 1.35 millions in total. Discounting the implementation costs, this means total savings of US$ 8.78 millions, which represents about 0.003% of Chilean GDP in 2022. The latter leads to a Return of Investment (ROI) of more than 600%, and a benefit-cost ratio of about 6-to-1, i.e., for every US$1 spent in the reports, the reports generated US$6 in crime savings.

Our estimate of the cost-effectiveness of the reports is conservative and likely to be a lower-bound for various reasons. First, it excludes additional savings from reduced adult crime[8]. Second, it does not take into account the labor market returns of re-enrolled students[9]. 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. Fourth, 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 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.

[8]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.

[9]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).

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)[10]. 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; Sviatschi, 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. 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, meaning the Dropout Reports are potentially scalable to other contexts (UNESCO, 2020).

Even though Dropout Reports target students after they have dropped out, its effectiveness is comparable to that of leading developmental crime prevention (DCP) programs that intervene before dropout occurs, such as free tuition, preschool, cognitive-behavioral interventions, and summer jobs. 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 by age 18 from 5.4% to 4.8%, for a school-enrollment-crime elasticity of -0.83.

Heller et al. (2017) present results from three large RCTs in Chicago, assessing interventions

[10]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.

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 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[11].

Dropout students who engage in criminal activity are not entirely abandoned by the State; rather, they often enter the child protection system, which seeks to support and reintegrate them into society. Our results suggest that the government can play a preventive role before that stage, by reducing the likelihood that dropouts engage in crime in the first place. A closely related study by Cooper et al. (2023) evaluates Mi Abogado (My Lawyer), a legal aid and social services program for foster children—many of whom are school dropouts or youth offenders. The program reduces the length of institutionalization in foster care, leading to higher school attendance and lower crime rates. While Dropout Reports operate at the extensive margin by helping schools re-enroll students before they enter the child protection system, Mi Abogado works at the intensive margin, improving outcomes for those already in it. Despite differences in target populations—foster children are at substantially higher risk of offending than average school dropouts—both interventions appear highly cost-effective. Accounting for implementation costs, the crime-reduction benefits are estimated at US$ 4,135 per foster child for Mi Abogado and US$ 633 per student listed in the Dropout Reports. Together, these findings suggest that public investments at both margins can generate meaningful reductions in youth crime.

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[12]. School funding is through a per-student voucher system that applies to both municipal and private

[11] 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. More generally, 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.

[12] 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.

voucher schools[13]. Every time a student leave, the school loses the voucher resources associated to 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[14]. Since the funding model is based on a per-student voucher system tied to school attendance, accurate records of enrollment, disenrollment, and individual attendance are essential. Schools are required to report the number of days each student attends per month and must promptly notify SIGE whenever a student enrolls or leaves.

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. As soon as the disenrollment agreement is signed, the school is mandated to immediately inform to the SIGE system and upload a copy of the agreement, after which the school of origin is no longer responsible of whether the disenrolled student re-enrolled in another school or became dropout. To prevent misuse, the system imposes strict penalties for omissions or inaccuracies on enrollment or disenrollment reporting, including wrong disenrollment/enrollment dates, student name, address, etc. Otherwise schools could claim vouchers for non-existent students or corrupt the system. These penalties, which range from financial sanctions to school closure, play a key role in deterring misreporting.

Leveraging this centralized enrollment management system, MINEDUC can accurately track all student entries and exits across schools and thus identify dropouts, i.e., students who disenrolled from a school without transferring to another for at least 24 hours[15]. Note that MINEDUC distinguishes between at-risk students and dropouts. Students with low levels of attendance are considered at risk of dropping out, but as long as they remain formally enrolled in a school, they are not classified as dropouts.

Importantly, schools often do not know whether a student who disenrolled from their dependencies has re-enrolled elsewhere or has dropped out entirely. This is in part due to costly information

[13]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.

[14]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)

[15]In the Chilean school system, students who re-enroll in another school within 24 hours of leaving their previous one are not classified as dropouts but rather as "transfer students," as their educational trajectory is not considered to have been interrupted. Typically, these students formalize their disenrollment from the origin school only after securing admission to a new school, ensuring continuity in their enrollment. As a result, the SIGE system often records zero hours between disenrollment and re-enrollment. However, in some cases, a delay of a few hours may occur as the system processes and matches the enrollment agreement from the new school with the disenrollment from the previous one —hence the 24-hour window for classification as a transfer.

acquisition as Chile's voucher system allows students to freely choose schools regardless of residential location, creating a decentralized and non-captive educational market that makes it difficult for schools to track students once they disenroll[16]. Additionally, the lack of accountability discourages schools from taking action. Even when schools are aware that a student has dropped out, they have little incentive to re-enroll them: dropouts often underperform academically, and there are no penalties for failing to follow up. Likewise, dropouts may be reluctant to return to the same school as they have already shown a preference for leaving, 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[17]. 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 dropouts in Chile rose from around 40,000 in 2019 (1.38%) to over 50,000 (1.56%) in 2023, a 25% increase that backs dropout rate to 2015 levels. This rise is primarily driven by high-school students, for about two-thirds of all dropout students. Chile's post-pandemic trend in dropout rates are more or less in line with worldwide records. A meta-analysis conducted by Moscoviz and Evans (2022) shows that 6 out of 9 country-level studies show positive breaks in dropout trends after the pandemic.

14-17 aged Dropouts Population. Central to our analysis are students who dropped out from school at ages between 14 and 17 years old, for whom we can access their criminal records, and thus characterize the relationship between school dropout, school re-enrollment, and crime (more in later sections). Appendix Table A.I provides an overview of 14-17 aged dropouts in Chile's public schools. In 2022, the SIGE system recorded 43,126 events, who represent 4.7% of the 14-17 aged students population[18]. Roughly half of them managed to re-enroll in the school system, the majority of whom did so in schools other than the origin school (95.6%). This is likely because dropout students may be hesitant to return if their decision to leave reflects dissatisfaction with the origin school. Likewise, schools have little incentive to re-enroll dropouts, who often underperform.

Dropouts can be classified as inter-annual dropouts, i.e., students who were enrolled in 2021 and did not enrolled by March 1^{st}, 2022 (the beginning of school year); or intra-annual dropouts, i.e., students who enrolled by March 1^{st}, 2022, but later disenrolled without transferring to another school. Although the dropout reports target both groups, the timeline of our evaluation allows us to focus only on within-year dropouts, who sum up 34,816 in total (Panel B).

[16]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.

[17]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.

[18]Around 5% of dropout students dropped out more than once. We treat each dropout event as a separate dropout throughout the analysis.

A particular subgroup of interest for our study are intra-annual dropouts from two-sided schools, which allows us to implement our identification strategy. These are schools with at least one dropout student who disenrolled from the school at each side of the cutoff date May 30th, 2022 (i.e., at least one dropout before and one dropout after that date), considering an optimal bandwidth of ± 12 school days (more in Section 5.1). We count a total of 13,862 dropouts who disenrolled from two-sided schools in 2022. This is our target population.

Panel (a) of Appendix Figure A.II shows the monthly distribution of disenrollments and re-enrollments for this subgroup. Two-thirds of disenrollments 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 70% during the first semester but drops progressively to reach less than 10% by year-end[19]. 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. Still, first- and second-semester dropouts seem to be similar across various demographic and educational attributes (see Appendix Table A.II).

We further explore how dropout students behave relative to their classroom peers in the origin school (see Appendix Table A.III). Dropout students are more likely to be migrants and exhibit significant academic lag, which is evident in lower school attendance, GPA, standardized test scores, and graduation rates (see columns (1) and (2))[20].

Second, we analyze how dropout students who did not manage to re-enroll compare with their re-enrolled counterparts (columns (3) to (5)). 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 4.4 *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 9 *pp*.

Finally, when compared to their classroom peers in the school of destination (columns (6) and (7)), re-enrolled dropouts continue to exhibit poorer performance, yet the performance gap narrows significantly, indicating a stratification of disenrollment dynamics. For instance, while school attendance of dropouts was 4.1 percentage points lower relative to their old classmates, this is just 0.4 percentage points lower relative to their classroom peers in the re-enrolling school, and the gap in grades decreases to about one-third. In fact, re-enrolled dropouts show higher graduation rates than their new classmates. This is reflected at the school level as well (columns (8) to (10)), as the schools of destination have, on average, lower attendance and graduation rates, and are less likely to be classified as Regular Education Schools, with almost 10% of them imparting education for adults.

[19] 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 (this year works as a pre-treatment comparison year that shares exactly the same calendar structure than 2022, hence the same number of school days and weeks). The patterns of dropouts and re-enrollments are nearly identical, with a similar second-semester drop in re-enrollment rates.

[20] For standardized academic achievement tests, we utilize the 2018 National SIMCE Assessment in Language and Math, 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.

3. The Dropout Reports

During 2022, MINEDUC implemented a dropout monitoring system oriented to re-enroll dropout students (see Appendix Figure A.III). Based on information from the centralized SIGE platform, MINEDUC sent to each school a "Report for Monitoring Students with Interrupted or Irregular Educational Trajectories" (henceforth "Dropout Reports" or just "Reports"). These 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. 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 of the report. This starts with a comprehensive overview of the school's dropout rate, and then provides a roster of disenrolled students under dropout status by May 30^{th}, 2022. 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.

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[21]. In case the school had not disenrolled students under dropout status by May 30^{th}, 2022, the list of dropout students was empty[22]. The share of students listed in the reports is very small relative to total school enrollments. On average, this is 1.04% for primary school, 1.06% for middle school, and 1.4% for high-school students (see Appendix Figure A.V). This suggests the potential spillover effects from listed to not-listed classmates are likely to be very limited[23].

Importantly, while the report informs origin schools about the dropout status of former students, these schools may have little incentive to bring them back. Dropouts often underperform academically, and schools face no consequences for failing to follow up. Likewise, students who have already chosen to leave may be reluctant to return to the same school, further reducing their likelihood of re-enrollment. Aware of these barriers, MINEDUC does not require origin schools to re-enroll former students in their own classrooms. Instead, the report encourages schools to activate their institutional and spatial networks to facilitate re-enrollment—whether in the same school or a different one.

[21] 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.
[22] Later in Section 10 we exploit this variation to test for accountability effects.
[23] Estimating spillover effects on non-listed students is out of the scope of this paper.

4. Data and Timeline

Our study focuses on the 2022-2024 period and examines 14-17 aged 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. We use several official administrative data from MINEDUC on student-level enrollment, graduation records, grades, and demographics and school-level observables, as well as administrative data from SENAME on criminal records of the 14-17 aged juvenile population (see Appendix Section B for details).

Figure 1: Timeline

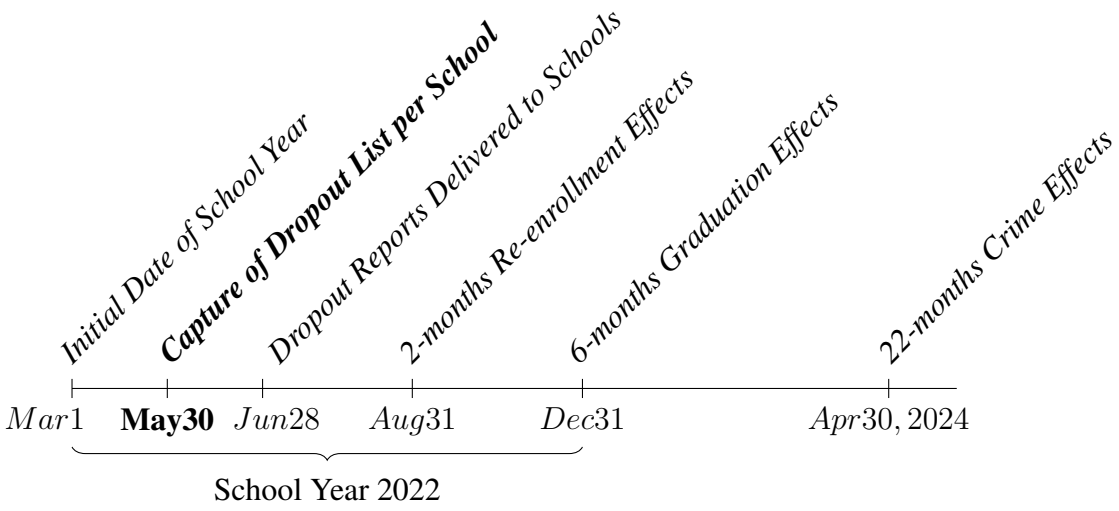


Figure 1 provides a timeline of our research design. The school year's initial date is March 1^{st}, 2022. On May 30^{th}, 2022, MINEDUC compiled the list of dropouts per school, and generated the school-specific dropout reports. Then, one month later, on June 28^{th}, MINEDUC makes available the report 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.

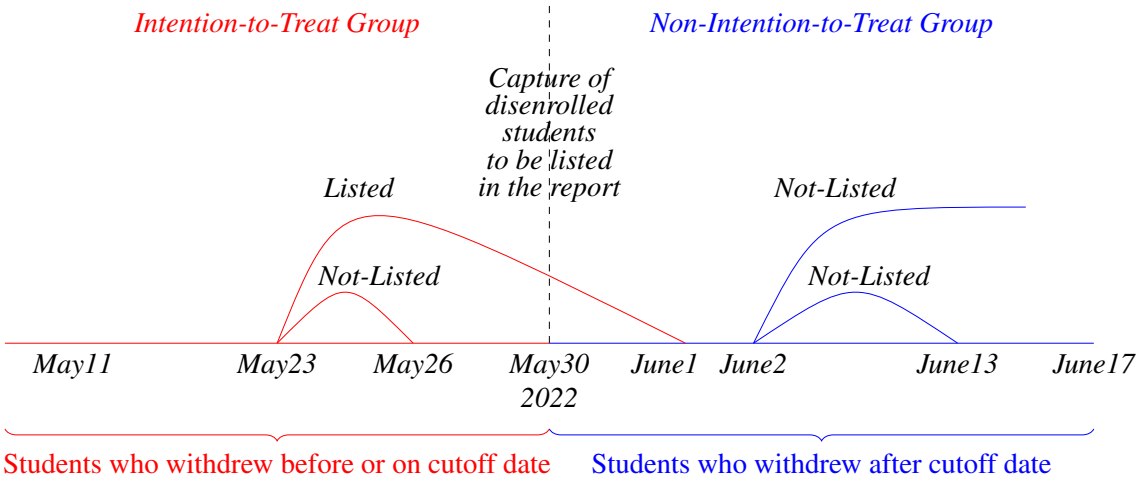
Crucially, the SENAME data only include individuals who were between 14 and 17 years old at the time of perpetrating the crime, hence our sample of analysis is restricted to dropout students aged 14-17 the month before the report disbursement (June 28^{th}, 2022). The latter allows us to observe pre- and post-treatment criminal behavior for each observation, which is a necessary condition to test the statistical validity of our identification strategy[24].

[24] An alternative is to select all dropouts that were 14 years old at any point in the time window that goes from July 1^{st}, 2022 and April 30^{th}, 2024, and thus eligible to be observed their criminal behavior. However, a problem is the missing data on crime for those who were not 14 years old before the report disbursement, which would impede us testing for statistical balance, and thus validate the identification strategy. Second, the criminal activity among the youth is concentrated in 14-17 aged cohorts, and so it is natural to consider that age group as the evaluation sample across the full period of analysis. Third, we are interested in characterizing the causal link of incapacitation effects, which

5. Identification

MINEDUC use May 30^{th}, 2022 as the cutoff date for building the reports, i.e., being listed in the report depends on "when" (in which specific date) the dropout withdrew from the school, if before or after May 30^{th}. Yet, not all dropouts who disenrolled before the cutoff date were listed in the report, but only those who remained disenrolled by May 30^{th}. Figure 2 exemplifies the timing of report construction. For example, if the dropout 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 he is not listed in the dropout report as he does not appear as a disenrolled student by May 30^{th}. If, instead, he re-enrolled on June 1^{st}, i.e., after the cutoff date, then he is listed in the report since by the cutoff date he appeared as disenrolled from the school system. Likewise, dropout 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.

Figure 2: Example of Report's Construction



Provided the cutoff date is as-good-as-random, dropout 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 those who withdrew right after that date. Then, comparing groups of disenrolled students at each side of the cutoff date is a causal estimate of the intention-to-treat effects associated to the dropout report. Moreover, the number of days between reported withdrawal date and cutoff date is an exogenous predictor of the probability of being listed in the report, such that we can use the discontinuous variation in that probability to identify the causal effects of being listed in the report. Importantly, the withdrawal date recorded in the SIGE platform is reported by each school's principal, making it potentially subject to school-specific, endogenous reporting practices. Then, to mitigate this concern we focus on within-school, student-level variation in reported withdrawal date.

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

implies observing the full process of disenrollment, reenrollment, and crime, which can only be executed for those who are 14-17 years old at the time of disenrollment.

date c, fixed on Tuesday May 30^{th}, 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. 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., $lim_{r_{isd}\uparrow 0}\mathbb{E}[Y_{isd}|c - r_{isd}] = lim_{r_{isd}\downarrow 0}\mathbb{E}[Y_{isd}|c + r_{isd}]$.

Overall, 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[25].

5.1. Evaluation Sample

The cutoff date for building the reports was May 30th, 2022, but the reports were sent in June 28th, 2022. Hence, for building our sample of analysis we start taking all 14-17 aged dropouts in publicly funded schools, and then identify those who became dropout students in dates sufficiently close to May 30^{th}, 2022[26].

Optimal Bandwidth. Our running variable is the number of days between withdrawal date and cutoff date, and the outcome of interest is a dummy that equals 1 if the dropout student committed any crime, and zero otherwise. To estimate the optimal bandwidth, we use pretreatment data from 2016-2021 (excluding 2020 due to school closure), which 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 the 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 day-of-week fixed effects to capture time-varying potential confounders associated to our running variable. As shown in Appendix Table A.IV, the estimated optimal bandwidths are relatively consistent across pre-treatment years and models, with these spanning between 12 and 20 days from cutoff date. Drawing from the year closest to the treatment, i.e., year 2021, we use a window of $\pm$ 12 days as the optimal bandwidth[27].

[25] 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.

[26] 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. 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.

[27] As we later detail in Section 6.1, our results are consistent across different bandwidth selections, including all

Importantly, we restrict the sample to dropouts from schools that only have dropouts at both sides of the optimal bandwidth around the cutoff date (the so-called “two-sided" schools). This restriction is not accommodating, but fundamental to warrant causal identification. First, the treatment variation is at the student level within schools (some are listed in the report, some are not), thus considering schools with no within-school-variation in treatment status implies assuming across-school identification assumptions, which are difficult to justify. For instance, the frequency of observation of dropouts within the SIGE platform depends on “how frequently" the school principal declares the dropouts in the SIGE system, if every week, every month, etc. Second, school principal declaration practices may reflect school-specific attributes that influence the behavior of students, then comparing dropout students from schools with different declaration practices may confound the causal effect of the report with school-specific attributes affecting the students' behavior. Thus, an apple-to-apple comparison requires within-school comparisons, i.e., comparing students who are governed by the very same school principal's motivation. Keeping only dropouts from two-sided schools bring us a total of 3,142 dropouts distributed across 840 schools. This is our evaluation sample[28,29].

5.2. External Validity

The two-sided schools restriction certainly reduces the sample size as well as the representativeness of our results. Dropout students in one-sided schools represent 44%, yet one-sided schools represent 69% of all schools, suggesting two-sided schools have a higher incidence of dropouts. Indeed, two-sided schools have, on average, roughly three times more dropouts than two-sided schools (3.7 vs. 1.3), reflecting a higher vulnerability. As we show in Appendix Table A.V, dropouts from two-sided schools have lower levels of school attendance, lower grades, are less likely to graduate, and are more likely to exhibit age-grade lag. Also, they go to schools with a higher share of low-SES students. The latter implies our results are only generalizable to “high-frequency" dropouts schools whose students are more vulnerable than the average.

Second, our results are specific to our RD sample of analysis, i.e., students who dropped out in a window of $\pm$ 12 school days from May 30th, 2022. Whether these effects are externally valid or not to students who dropped out in a different time period is, in principle, a testable assumption. We examine this in Appendix Table A.VI, which displays pre-treatment summary statistics for dropouts in and out of the optimal window. The magnitude of the differences across samples is generally quite small and close to zero, and we never reject the null hypothesis of no differences across groups. At first glance, this implies the withdrawal date does not play a major role in terms of sample selection, meaning the external validity of our results is not confined to the specific time window we analyze.

combinations of optimal bandwidths presented in Appendix Table A.IV, regardless of the pre-treatment year.

[28] Few cases dropped out more than once within the optimal window of $\pm$12 school days, which represent less than 0.5% of the evaluation sample. We treat them as separate dropouts throughout the analysis.

[29] As we see later in Section 5.4, 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 3,142 to 2,928, and this is because the triangular kernel assigns a zero weight to observations in the extreme (left and right) margins of the optimal window.

Note, however, that despite the fact that in- and out-of-optimal-window dropouts are statistically similar, our results are not necessary generalizable to different timings of report implementation since re-enrollment incentives may vary over time for both students and schools. For instance, had the reports been implemented in March (three months before actual implementation), we would expect the schools to have more time to plan ahead re-enrollment strategies, which may change outcomes. Likewise, students who drop out near the end of the school year (e.g., November) likely face stronger incentives to re-enroll, as the effort required to obtain a graduation certificate is relatively small compared to those who dropped out earlier in the year (e.g., May-June), as is the case of our sample of analysis. Overall, generalizing the results to other implementation windows requires to assume time-stable re-enrollment incentives for both students and schools, which limits the external validity of our results.

5.3. Internal Validity

Manipulation of Disenrollment Date. An internal validity threat is that school principals manipulate the withdrawal date reported to the SIGE system. This may avoid their students appear in the dropout report, thereby violating the identification rule. For example, if a student disenrolled on May 25^{th}, the school principal may want to report that he disenrolled on June 1^{st}, i.e., after the cutoff date, such that he is not listed in the report[30].

This is implausible for various reasons. First, the dropout reports initiative is not something they could expect since no interventions of this sort had been previously implemented. Second, the reports were delivered to school principals about one month after the cutoff date, on June 28^{th}. In consequence, 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. But 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[31].

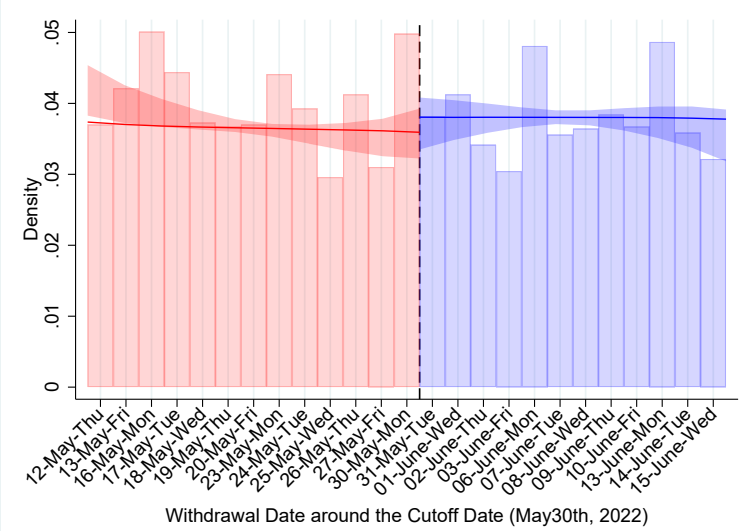
We examine the possibility of manipulation of reported withdrawals by using McCrary (2008)'s density test which verifies whether the density of reported withdrawals varies around the cutoff date. As shown in Figure 3, the distribution of reported disenrolled students around May 30^{th} is not uniform 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, suggesting day-of-week fixed effects are important to control for in regression analysis (more in Section 6). Still, we find no graphical evidence of

[30] 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.

[31] 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.

date manipulation in withdrawals reporting around the cutoff date. For our optimal bandwidth of $\pm$ 12 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.

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



Notes: Distribution of reported 14-17 aged disenrollments from two-sided schools across the number of school days from withdrawal date to cutoff date of Monday May 30^{th}, 2022, using $\pm$ 12 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 1 (p(1)). 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 0.9643. 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 (red and blue shaded areas) are not centered at the point estimates (red and blue lines) in the graph.

Baseline Balance. Appendix Table A.VII assesses the statistical comparability of before- vs. after-cutoff dropout students in our evaluation sample across numerous pre-treatment predictors of dropout, including demographics and background characteristics such as gender, migrant status, age, socio-economic status, and income, as well as performance indicators like GPA, school attendance, and graduation rates. We test for mean differences by regressing the pre-treatment outcome against the running variable, the before-after dummy, and their interaction, all controlling for day-of-the-week and school fixed effects. The pre-intervention differences across treatment and control groups around the cutoff are small in magnitude and precisely estimated, confirming statistical balance between dropout students on either side of the cutoff. In terms of outcomes (Panel C), grade completion is well balanced across groups for multiple pre-treatment years, and this is also the case for whether the dropout student committed crime before the cutoff date.

For robustness, we also test whether there is statistical balance in pre-treatment outcomes for bandwidths different than our optimal bandwidth of $\pm$12 school days. As shown by Appendix Figures A.VI and A.VIII, both graduation and crime outcomes are fully balanced for a wide range

of bandwidths covering 7 to 20 school days around the cutoff date (May 30th, 2022), i.e., one month in total. Moreover, this is even the case if instead of using the 2022 cohorts of dropouts we use year-specific pre-treatment cohorts, i.e., for each year-cohort of dropout students, we test whether disenrolling before or after May 30^{th} in each pre-treatment year makes any difference in graduation and crime rates (see Appendix Figures A.VII and A.IX for graphical evidence[32]). The differences are shown to be mostly null across pre-treatment cohorts, when the reports did not exist, reinforcing the exogeneity of May 30th as our cutoff date. If anything, only in some bandwidths of one out of five pre-treatment years (2019) we find the difference on graduation rates is statistically different from zero (at the 90% confidence level), which is what we would expect to happen by chance. Overall, this evidence reinforces the internal validity of our design.

Finally, we test for statistical balance on the probability of re-enrollment before the cutoff date (Panel D in Appendix Table A.VII). For dropout students in the pre-cutoff group (treatment), 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 (control), but use a placebo cutoff date that counts 12 school days after the cutoff (June 15^{th}), such that the maximum time window for re-enrollement is equal for both treatment and control groups. The balance test shows no evidence of differential selection across pre- and post-cutoff groups in re-enrollment behavior, suggesting the non-compliance rate among control group dropouts would be the same as that of they treatment counterparts had they been dropped out before the official cutoff date of the experiment.

5.4. 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 dropouts 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 plots the probability of being listed in the report by disenrollment date at each side of the cutoff date, using the optimal bandwidth of $\pm$ 12 days.

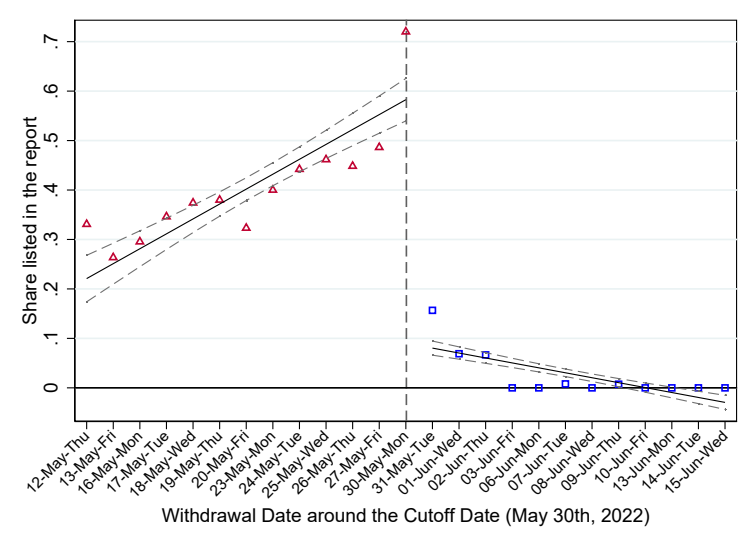
The likelihood of being listed in the report is not 100% for treatment group dropouts, but around 42%. This is because they still had the opportunity to re-enroll before the cutoff date, and some did, which excluded them from being listed[33]. Second, the probability of being listed in the report increases with proximity to the cutoff date. For instance, about 50% of dropouts who disenrolled on Friday May 27^{th} ended up being listed in the report, but only 30% 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 2.1 *pp.*, on average. 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 2.7% of the control group. This is mostly due to endogenous non-compliance induced by MINEDUC officials.

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

[33] 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. 75% of them were not able to re-enroll and thus were listed in the report.

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.

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



Note: Sample of analysis are 14-17 aged dropouts who dropped out from two-sided schools within $\pm$ 12 days from Monday May 30^{th}, 2022, our cutoff date. The Y axis shows the probability of being listed in the report. The X axis, our 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 value of the running variable is associated to a given day of the week. Red triangles indicate the likelihood of being listed in the report conditional on disenrolling before the cutoff date, while blue squares indicate the likelihood of being listed in the report conditional on disenrolling 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.=0.027; SE=0.000). This is obtained by regressing a dummy of being listed in the report against the number of school days between disenrollment and cutoff dates (i.e., our running variable), controlling for day-of-week and school fixed effects. Likewise, 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=0.000).

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 designed to analyze school's reception of the report, 84% of school principals reported to have downloaded the report. 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. Then, 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. 90%

of school principals declared the information about dropout students was correct and sufficient for re-enrollment purposes. They used the report primarily to inform the management team (79%), to implement a monitoring system to track 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-to-7 scale.

6. Estimating Treatment Effects on School Re-enrollment

We exploit the exogenous variation in the time distance from dropout's withdrawal date to cutoff date to estimate the causal effect of being listed in the report on the probability of school re-enrollment. We regress a re-enrollment dummy Y_{isd} that equals 1 if the dropout student i in school s who withdrew on day-of-week d re-enrolled by August 31^{st}, 2022 (two months after the report's release date of June 28^{th}, 2022), on whether he dropped out 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 dropout 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 dropout 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 date, which makes the dropout student discontinuously more likely to be listed in the report. We then replicate the analysis with 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 dropout student level within each school, hence we control for school fixed-effects η_s, which capture school-level factors that are constant across dropouts 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. As was shown in Figure 3, withdrawals are more likely to occur at the beginning of the week, and 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 within-week time trends that could simultaneously affect both the disenrollment date and the probability of re-enrolling afterwards. We further include a dummy for whether the dropout student was promoted to the next grade in the previous year, and evaluate the sensitivity of our results to adding a set of pre-treatment covariates in vector X', including gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is

classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies.

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 5.4 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[37,38].

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 time distance —in school days— between disenrollment date and cutoff date. On average, 75% 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 to about 80%.

[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 dropout students aged 14-17 who withdrew around the cutoff date, there are no sampling design issues. Instead, units are assigned to treatment and control 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.

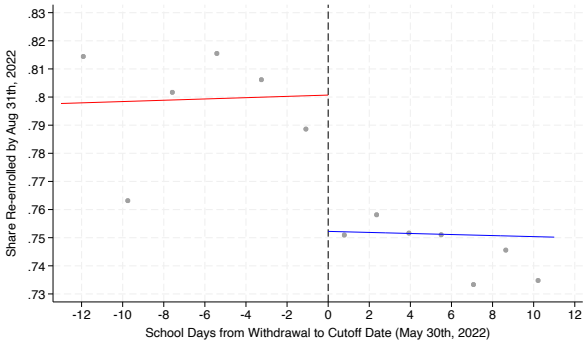
[35] Our results are robust to either clustering the standard errors at the school level or by using robust standard errors. See robustness section for details.

[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 it is our case, 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, which is what we do.

[37] Our results are robust using alternative weighting schemes, including uniform weights. See robustness section for details.

[38] Note that the triangular kernel approximation assigns a zero weight to observations in the extreme (left and right) margins of the optimal window, thereby reducing the estimation sample size from 3,142 to 2,928.

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



Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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. 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 at each side of the cutoff.

Table 1 shows the RD estimates. We present robust estimates using a local linear polynomial model (Calonico et al., 2014) 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 7 and 9 *pp.*, and robust across models. Considering the full specification in column (3), the effect represents a 12.5% 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 dropout 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 50 *pp.* more likely to be listed in the report than their after-cutoff counterparts. As a result, our 2SLS estimate doubles in size relative to intention-to-treat figures, for an effect size of 18.7 *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. The specification works as follows:

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

, 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[39].

[39]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.

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 ($B_{isd} = 1$)	0.069**	0.078**	0.094***		0.065*	0.088**	0.105***	
	(0.032)	(0.036)	(0.036)		(0.035)	(0.041)	(0.041)	
If listed in the Report ($T_{isd} = 1$)				0.187***				0.209***
				(0.072)				(0.081)
Ineligible Mean	0.751	0.751	0.751		0.678	0.678	0.678	
Eligible Complier Mean				0.775				0.704
Ineligible Complier Mean				0.589				0.496
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
Day-of-the-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (= 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes
1^{st} Stage Estimate				0.501				0.501
Cragg-Donald F-Statistic				240.564				240.564
Kleibergen-Paap F-Statistic				174.854				174.854

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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 $\pm$ 12 school days, which is consistent with the optimal bandwidth used for estimating crime effects. 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 FEs, day-of-weeks FEs, and a dummy on whether the dropout student graduated in the previous year. Columns (1) and (5) compute the robust estimate using a local linear polynomial model (Calonico et al., 2014, command `rdrobust` in `Stata`). Regression models in columns (2) and (6) show estimates from a linear OLS model. Regression models in columns (3) and (7) additionally control for pre-treatment covariates, including gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. 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.

We estimate that 59% 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 32% (=0.187/0.589) increase in re-enrollment relative to ineligible compliers. Consider the total number of dropouts who would have not re-enrolled in the absence of the report, which

amounts to 5,697 students[40]. If we extrapolate the treatment effects to all 14-17 aged dropouts from two-sided schools in 2022 beyond those included in the time window used for the RD design, then the number of dropouts would be reduced to 3,105 students, meaning 2,592 students or about 45% of not re-enrolled dropouts would back to school due to the report.

The gains in re-enrollment then translate into a higher graduation rate. While 68% of dropouts who disenrolled after the cutoff date managed to graduate from school by the end of the year, this raises to 73%-78% among intention-to-treat dropouts, 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 42% relative to the graduation rate among ineligible compliers[41].

6.1. Robustness Checks

Robustness to Quadratic Functional Form. Appendix Table A.VIII replicates the results using a quadratic version of equation 1. As shown by columns (1) and (2), 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.

Falsification test using Pre-treatment Years. Appendix Figure A.X compares how before and after-cutoff students in our evaluation sample differ in their graduation rates throughout the pre-treatment period 2016-2021, as well as for 2022, the treatment year. For each year, we plot the estimate of parameter β in equation 1 (i.e., model (7) in Table 1) and its associated 95% confidence interval. Differences are statistically null in pre-treatment years, yet large and statistically significant after the implementation of the reports.

Robustness to Alternative Bandwidths. Appendix Figure A.XI 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, covering 7 to 20 school days, i.e., one month in total[42]. We observe that the effect on both re-enrollment and graduation is robust across the full range of bandwidths. The effect grows closer to the cutoff, which is expected since the treatment compliance rises as the disenrollment approaches to the cutoff date, yet the precision decreases due to smaller sample size.

Falsification Test using Alternative Cutoff Dates. We replicate our analysis but using March 30^{th} and April 29^{th} as pre-treatment placebo cutoffs, i.e., we estimate the effects on re-enrollment and graduation but using dropout students who withdrew from school within $\pm$ 12 school days from these placebo cutoff dates, separately[43]. Appendix Table A.IX shows the differences in re-enrollment

[40] This is calculated as the total number of 14-17 aged dropouts from two-sided schools in 2022 (13,862) times the share of ineligible compliers who did not re-enrolled (1-0.589).

[41] For an extended analysis considering other age cohorts and school grades, please see Online Appendix Section C.1 where we separately show how the report effects behave across dropout students in 1-4th, 5-8th, and 9-12th grades.

[42] 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 5.1, the common mean squared error (MSE) optimal bandwidth selector (Calonico et al., 2014) never suggests optimal bandwidths in the 1-6 range.

[43] We choose March 30^{th} and April 29^{th} because these are the penultimate date of March and April, respectively, as it is the case of May 30^{th} for May.

rate across dropouts who disenrolled before and after these placebo cutoff dates are small and not statistically different from zero (columns (1) and (2)), and this is also the case when testing placebo effects on graduation (columns (4) and (5)).

Falsification Test using Transfer Students. As detailed in Section 2, students who withdrew and 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 treats them as withdrawn students with uninterrupted enrollment who are not eligible to be included in the dropout report. As a result, we should expect no effects of the report for transfer students. We test for this in columns (3) and (6) in Appendix Table A.IX. We find transfer students do not re-enroll nor graduate more as a result of withdrawing before versus after the cutoff date.

Robustness to Inclusion in Subsequent Dropout Report. By the end of the year, specifically on December 1^{st}, 2022, MINEDUC sent a second report to schools which updates the list of dropout students included in the first report of June 28^{th}. 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. Appendix Table A.X tests for it. 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. This result reinforces our evidence on the positive re-enrollment effects from first report. It also provides credibility to that dropout reports are accurate and they effectively include dropout students.

Robustness to Clustering the Standard Errors at the School level. Our main regression specification follows Abadie et al., 2017 and cluster the standard errors at the school-group level. For robustness, we replicate the exercise but clustering the standard errors at the school-level only. The statistical validity of the results do not change (see Appendix Table A.XI).

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

As emphasized in Section 2, a large majority of dropout students who manage to re-enroll do so in schools different from the origin schools from which they initially dropped out[44]. 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, preventing the re-enrollment 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, but encourage these schools to facilitate re-enrollment, whether in the same school or another institution. This seems to be well understood by schools. Indeed, as shown by Appendix Table A.XII, more than two-thirds of the re-enrollment effect is driven by dropout students re-enrolling in schools other than their schools of origin. The latter suggests that the origin schools receiving the reports leveraged their networks to support students' re-enrollment in other institutions[45].

[44] 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.

[45] Around 2.7% 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

6.3. The Role of School Networks

School re-enrollment involves efforts associated with contacting dropout students and their families, as well as coordination with potential receiving schools. Closely located schools are expected to support each other due to low transaction costs. Since origin schools are not required to re-enroll listed students in their own classrooms, we hypothesize that those unwilling to take back former students may seek support from nearby schools to facilitate their re-enrollment elsewhere. Appendix Table A.XIII explores the role of spatial proximity 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[46]. The results show that most of the effect is 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, receiving institutions may include either regular schools serving the general student population or, in contrast, lower-quality, non-regular schools offering remedial programs, such as adult education or special education schools. We find 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. This will have consequences on the intensive margin effects of school re-enrollment on crime prevention (more in Section 9).

Finally, 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.XIV, 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, and this is at the expense of a decline in re-enrollments in schools outside of the origin school's network.

Collectively, these results are consistent with the reports' aim of leveraging spatial and institutional networks to facilitate school dropouts' re-enrollment.

6.4. The Role of School Capabilities

School re-enrollment may require active engagement from school management staff. Having a higher density of professionals dedicated to the strategic and operational management of the school could be beneficial, including roles such as principals, vice principals, department coordinators, and counselors. For instance, Muñoz and Prem (2024) show that Chilean schools whose management staff were strengthened by the ADP reform increased their students' yearly attendance, test scores, and college admission scores.

(last column in Appendix Table A.XII). The report did not change the probability of N.A. re-enrollments at the conventional levels of statistical significance.

[46] 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

We start computing the number of school workers in management staff over the total number of classrooms within each school of origin, which we call the managerial staff to classroom ratio. Appendix Figure A.XII plots how the report effect on re-enrollment rate varies across the quintile distribution of the managerial staff to classroom ratio in the year prior to treatment. We observe the re-enrollment effects tend to be higher in origin schools whose managerial staff per classroom is larger. Appendix Table A.XV digs in further and shows estimates across sub-samples of schools whose managerial staff to classroom ratio was above versus below the median at baseline. The report effects on re-enrollment is fully explained by origin schools whose managerial capacities are above the median.

7. School Dropout and Juvenile Crime in Chile

7.1. The SENAME Crime Data

Chile's Adolescent Criminal Responsibility Law #20084 (LRPA) establishes a legal framework for addressing criminal behavior among minors aged 14 to 17. 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. 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 LRPA law[47]. LRPA processes severe crimes, including homicides, rapes, aggravated assaults, and recidivism, as well as not-severe crimes including assaults, property damage, thefts, and drug-related offenses. 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.

7.2. A Glance at the Relationship Between School Dropout and SENAME Crimes in Chile

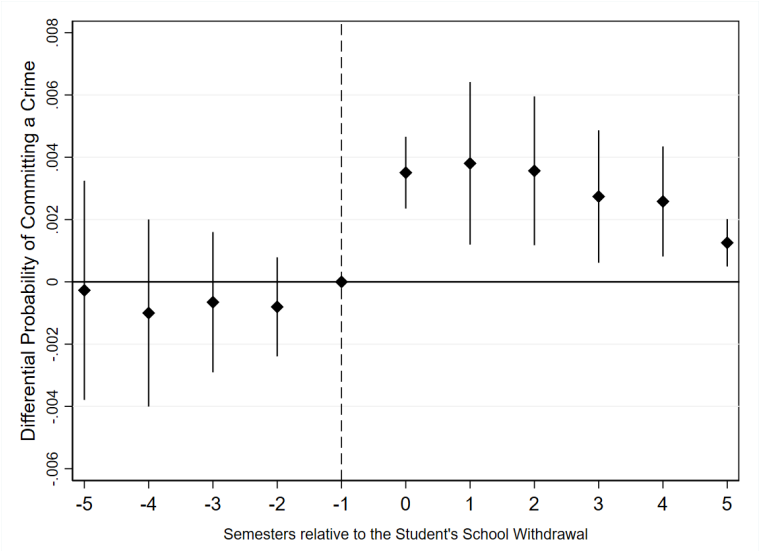
We accessed SENAME data covering the universe of crimes processed through LRPA and their corresponding sanction date, and used the MINEDUC enrollment and dis-enrollment data to merge crime observations to individuals aged 14-17 years old who disenrolled from the school system for the first time over the 2016-2024 period. For comparability purposes, we impose the very same school-type restriction as in our RD sample and thus keep only 14-17 aged dropout students who disenrolled from two-sided schools.

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 intervention period (i.e., before the 2022 reports), 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

[47]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.

total of 2.5 years of pre-disenrollment and 2.5 years of post-disenrollment. Our panel allows us to observe the 2016-2024 criminal behavior of every 14-17 aged dropout student who disenrolled from the school in the period 2016-2021. 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.

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



Note: Sample of analysis are dropout students aged 14-17 who disenrolled from two-sided schools over the 2016-2021 period. Outcome of interest is whether the dropout student committed crime in semester-year t, which covers the 2016-2024 period. The figure shows 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.

Overall, our event-study panel includes roughly 60,000 SENAME eligible students who dropped out from a two-sided school for the first time at some semester-year within pre-intervention period 2016-2021. Among them, 7% committed at least one crime throughout the period, and 73% of the crimes occur after school disenrollment, indicating that dropping out of school may be a significant determinant of initiation of criminal activity. Second, 84% of crimes are committed by male youth, with the crime rate being 11% among this group. This figure is more than 20 times larger than the juvenile crime rate among non-dropout students in Chile in 2016-2021 (CEAD, 2024), which is

in line with global evidence showing that crime rates are larger among youth populations (Levitt and Lochner, 2001). The high propensity to commit crime among dropout students should not be surprising in light of the abundant qualitative evidence in Chile highlighting that dropouts are considerably more vulnerable to engage in criminal activity compared to the average youth (see, for instance, Mendoza-Lira et al., 2023, Alvarez, 2014, and Reyes-Quilodrán et al., 2025).

As shown by 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 0.004 percentage points relative to the pre-dropout period, on average. Considering that the crime rate is 0.7% in the semester immediately prior to school dropout ("-1"), dropping out from school is associated to about 50% higher chances of committing a crime in the dropout semester. This is an instantaneous effect. However, such differential probability remains positive for at least five semesters before bouncing back to pre-dropout crime levels, suggesting a large cumulative effect. The effect bounce back possibly because the cost of committing crime rises as youth turn 18 and become legally liable for prosecution as adults. Overall, this evidence suggests that, at least for the case of Chile, school dropout correlates positively with juvenile crime[48].

8. Estimating Treatment Effects on Crime Prevention

We examine whether being listed in the dropout report reduced the propensity to commit a SENAME crime. The exercise requires to consider only crimes that were effectively committed after the disbursement of dropout reports, June 28^{th}, 2022. The last date considered in SENAME data is April 30^{th}, 2024, meaning we should interpret the results as a cummulative crime effect within a 2 years window after reports. Second, the crime data 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[49].

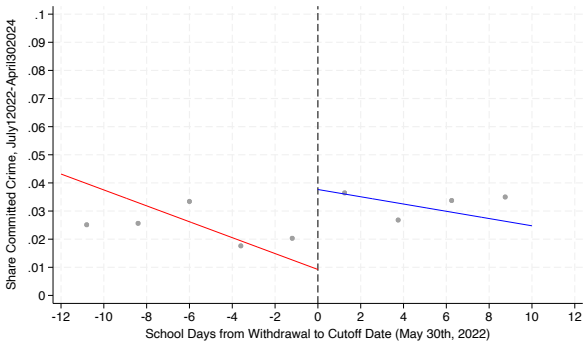
For estimation, we replicate the regression discontinuity analysis of main equation 1 over the very same evaluation sample of dropouts, but using PI-crimes instead of re-enrollment/graduation

[48]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 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).

[49]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.

as the outcome of interest. This 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, and we distinguish between severe crimes (homicides, rapes, aggravated assaults, and recidivism) and not-severe crimes (assaults, property damage, thefts, and drug-related offenses)[50].

Figure 7: Effect of disenrolling before Cutoff Date on Crime



Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. Binned scatterplot of the share of dropouts who committed any PI crime between July 1st 2022 and April 30th 2024, as a linear function of the distance (in school days) between disenrollement date and cutoff date. Optimal bandwidth is fixed at $\pm$ 12 school days. Red (Blue) bins aggregate the crime rate of students who disenrolled before (after) the cutoff date. The solid lines trace a linear fit at each side of the cutoff.

Figure 7 shows the probability of committing any PI crime as a function of the time distance —in school days— between disenrollment date and cutoff date. On average, 3.2% of dropouts who disenrolled after the cutoff date committed crime. In contrast, dropouts who disenrolled before the cutoff date show, in the limit, a sharp fall in their crime rate. The across-groups difference increases with proximity to the cutoff date, suggesting this is driven by the probability of being listed in the report.

Columns (1)-(3) in Table 2 presents the ITT effects on PI crimes. Disenrolling from school before the cutoff date reduces the probability of committing crime by a range of 3.3 to 4.1 percentage points. The effects are generally robust across models, regardless of whether we control or not for the large set of additional pre-treatment covariates, which reinforces the internal validity of the results[51]. Next, in column (4) we estimate the causal effect of have been listed in the report. Our 2SLS estimates double in size relative to ITT 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 a crime among disenrolled students who were ineligible for being listed in the report and indeed were not included in it (i.e., ineligible compliers), which is estimated in 9.65%. Then, the 2SLS effect represents a 79% reduction (=-0.0762/0.0965) relative to the probability of committing crime among ineligible compliers.

[50]Dropouts at each side of the cutoff date are well balanced at pre-treatment level for a large set of demographic, educational, and socio-economic characteristics, as well as for crime outcomes (Appendix Table A.VII). This is even the case when considering a wide range of bandwidths (Appendix Figure A.VIII) or different cohorts of dropouts (Appendix Figure A.IX), which supports the internal validity of our RD strategy on crime outcomes.

[51]The results are robust to the use of a quadratic OLS model. See Appendix Table A.VIII.

Table 2: Treatment Effects on Crime

	If committed Any Crime PI cases July 1st, 2022 - April 30, 2024				If committed Severe Crime PI cases July 1st, 2022 - April 30, 2024				If committed NOT Severe Crime 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)	Loc.Lin. Poly. (9)	OLS Linear (10)	OLS Linear (11)	2SLS (12)
W.D. $\leq$ May 30^{th} ($B_{isd} = 1$)	-0.0413***	-0.0331**	-0.0383**		-0.0008	-0.0074	-0.0114		-0.0405***	-0.0257**	-0.0269**	
	(0.0125)	(0.0160)	(0.0162)		(0.0083)	(0.0105)	(0.0108)		(0.0108)	(0.0131)	(0.0132)	
If listed in Rep. ($T_{isd} = 1$)				-0.0762**				-0.0227				-0.0535**
				(0.0322)				(0.0215)				(0.0262)
Ineligible Mean	0.0321	0.0321	0.0321		0.0132	0.0132	0.0132		0.0189	0.0189	0.0189	
Eligible Complier Mean				0.0203				0.0179				0.0023
Ineligible Complier Mean				0.0965				0.0406				0.0559
Observations	2,928	2,928	2,928	2,928	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	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
If Crime at baseline (= 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
1st Stage Estimate				0.502				0.502				0.502
Cragg-Donald F-Statistic				240.940				240.940				240.940
Kleibergen-Paap F-Statistic				175.635				175.635				175.635

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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. We count only crime events with pretrial injunctions (typically sanctioned within 24–48 hours) committed between July 1, 2022, and April 30, 2024, that is, starting immediately after the dropout reports' release (June 28th, 2022). Severe crimes include homicides, rapes, aggravated assaults, and recidivism. Not-severe crimes include assaults, property damage, thefts, and drug-related offenses. All models control for school FEs, day-of-week fixed effects, and a dummy on whether the dropout student committed crime before the reports were disbursed and a dummy of the associated gravosity of the committed crime (1-5). Columns (1), (5), and (9) compute the robust estimate using a local linear polynomial model (Calonico et al., 2014, command `rdrobust` in `Stata`). Regression models in columns (2), (6), and (10) show estimates from a linear OLS model. Regression models in columns (3), (7), and (11) 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), (8), and (12) 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.

Second, when disaggregating the crime measure, we find no effect on severe crimes; rather, the crime prevention effect is driven by reductions in non-severe offenses. This pattern is consistent with an incapacitation mechanism rather than a deeper behavioral transformation. That is, re-enrollment may reduce opportunities to engage in opportunistic crimes by limiting students' unsupervised time, without necessarily altering the underlying propensity to commit more serious or violent offenses (more on this in Section 9).

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 apply Bonferroni family-wise error rate (FWER) corrections, adjusting the p-values of individual tests based on the number of outcome variables (Bonferroni, 1936)[52]. Specifically, we calculate Bonferroni FWER corrections at the 10% significance level by dividing the desired test size by the total number of outcome variables within this family, which in our case is three. We thus reject the null of no effect only if $p_i \leq \frac{0.1}{3} = 0.0333$. Our results are marginally robust to this stringent criteria for the case of not-severe crimes, and fully robust for the outcome "Any crime".

Also, the effects are robust to clustering the standard errors at the school level (see Appendix Table A.XI). Third, our results are robust to several different bandwidth selections, including all combinations of optimal bandwidths (see Appendix Figure A.XIII). Fourth, we replicate our analysis but using March 30^{th} and April 29^{th} as pre-treatment placebo cutoffs and, as Appendix Table A.IX shows, the differences in crime rate across dropouts who disenrolled before and after these placebo cutoff dates are small and not statistically different from zero. Fifth, the same table shows no statistically significant effects on crime rates among transfer students, which is what we would expect to occur as transfers are not eligible to be included in the dropout report.

Finally, we replicate the analysis but also considering Not-PI crimes, whose court orders 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 ensure accuracy we count Not-PI-crimes whose sanction date is one year after the dropout reports' disbursement (i.e., July 1^{st}, 2023 onward), which warrants that Not-PI counted crimes are indeed produced after the reports. As shown in columns (5)-(8) of Appendix Table A.XVI, we find the crime prevention effects are similar in magnitude, adding robustness to the results.

9. School Re-enrollment and Crime Prevention

Having shown that the report increases re-enrollment and reduces crime, school re-enrollment emerges as a natural mechanism linking the dropout reports to crime reduction. This suggests that crime reductions may be primarily driven by extensive margin effects associated with incapacitation factors, i.e., a mechanical decline in criminal activity resulting from the institutional control that

[52]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. 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.

schools exert over re-enrolled dropouts, which restricts their opportunity to offend[53]. Note, however, that our results do not imply that every student who re-enrolled due to the report went from committing a crime to not committing a crime. Our 2SLS estimate on re-enrollment is 18.7 *pp.*, which almost triples in size the effect on crime prevention (7.6 *pp.*). That is, had all the crime prevention were explained by re-enrollment, then the re-enrollment-crime elasticity would be -0.4 ($= -7.6/18.7$)[54]. It is still large, yet not implausible.

First, our population of study is extremely vulnerable from engaging in crime. As detailed in Section 7.2, one in ten dropouts aged 14–17 in Chile commit a crime, and this is considering only SENAME crimes, and only those who are caught. Accounting for undetected or non-SENAME crimes would likely raise this rate even further. Even so, the observed figure is roughly 20 times higher than that for non-dropouts. This is not surprising given the substantial qualitative evidence linking school dropout to criminal behavior in Chile (Mendoza-Lira et al., 2023, Alvarez, 2014, and Reyes-Quilodrán et al., 2025).

Second, the report effects on crime reduction among dropouts are, in relative terms, comparable to the dropout-crime inverse estimate obtained from the event study in Section 7.2, which is what we would expect under the hypothesis of incapacitation effects. Concretely, our 2SLS effect on crime prevention represents a 79% reduction in the probability of committing a crime relative to ineligible compliers (=-0.076/0.096), all within a two-year window after the report disbursement. In contrast, the event-study provides a measure of the increase in crime rate when passing from non-dropout to dropout status using a flow, time-varying semester-student level data. Still, the change in the probability of committing crime once the student becomes dropout relative to the period immediately before dropping out is 60% (=0.004/0.007). While the methodological differences may distort the comparability of the two estimates, the fact that these are not too far from one another reinforces the credibility of our estimate.

Finally, intensive margin effects related to the characteristics of the school of destination (SOD) of re-enrolled dropouts may also contribute to the observed impacts. That would be the case if, for instance, peers in the schools of destination of treated dropouts show lower crime levels than peers in schools where untreated dropouts re-enrolled. We examine the presence of intensive margin effects by comparing the crime levels among SOD-peers of re-enrolled dropouts who disenrolled before (treatment) versus after (control) the cutoff date used for report construction.

Note, however, that such a comparison would be subject to selective attrition since the report generated a larger re-enrollment rate among treatment dropouts (extensive margin), thereby inducing a selection problem. A solution is to use the bounding approach of Lee (2009) to construct upper and lower bounds for the causal effect of the report on the SOD-peers crime rate. Implementing

[53]The substantial literature on incapacitation effects is consistent with this result (Jacob and Lefgren, 2003; Luallen, 2006; Lochner, 2011; Landersø et al., 2017; Huttunen et al., 2023)

[54]Of course, this does not warrant causal interpretation. One may be tempted to exploit the before/after cutoff disenrollment dummy (B_{isd}) as an instrument for being re-enrolled into school to estimate the causal effect of school re-enrollment on the probability of committing crime. However, the excludability assumption may not be met in practice, impeding a causal interpretation. For instance, a student who was listed in the report may remain disenrolled and not commit crime afterwards because he felt the school was trying to take care of him, a “psycho-gain" that reduced his chances of committing a crime. That is, being listed in the report did not change re-enrollment, but it still reduced the chances of committing crime, violating the excludability assumption.

Lee bounds requires taking only re-enrolles in each group and assume two extreme cases: one where all the differential re-enrollment rate in favor of the treatment group is driven by treated dropouts who re-enrolled in schools with the lowest crime rate; the other assuming that they went to the schools with the highest crime rate. A key identifying assumption is monotonicity, meaning disenrolling before the cutoff date influences sample selection in only one direction, i.e., we assume that there are some dropouts who would have not re-enrolled if they had not been disenrolled after the cutoff date of May 30^{th}, but that no dropouts avoid re-enrollment as a result of having disenrolled before the cutoff date. In each extreme case, we trim the distribution of treatment enrolles until the re-enrollment rate of the treatment group equals that of the control group[55].

Appendix Table A.XVII presents the results. We find that the crime rate of SOD-peers faced by re-enrolled dropouts eligible for the report is, on average, not significantly different from that faced by their non-eligible counterparts—except in the case of non-severe crimes. SOD-peers of eligible re-enrolled dropouts are between 0.003 and 0.006 percentage points less likely to commit non-severe crimes. This difference is larger and statistically significant when focusing on male peers, with Lee bounds ranging from 0.015 to 0.020 percentage points. These results suggest that the crime prevention effects associated with being listed in the dropout report may, in part, be driven by lower crime rates among peers at the destination schools of re-enrolled dropouts.

We further examine attributes of the destination schools related to academic performance and instructional time (Appendix Table A.XVIII). SOD-peers of eligible re-enrolled dropouts appear to have higher attendance rates, higher graduation rates, lower rates of grade repetition, and more educated parents. However, the evidence is statistically weak, as we never simultaneously reject the null of no effect based on both the lower and upper bounds. Regarding instructional time, eligible re-enrolled dropouts are more likely to enroll in schools with a full-day schedule rather than half-day or evening programs, and they are less likely to attend schools offering adult education programs —typically held in the evenings and targeted at vulnerable populations, often during high-crime hours. Overall, this evidence supports the hypothesis that attributes of the destination schools contribute to the crime-preventing effects of school re-enrollment.

10. Cost-Benefit Analysis

Where is the marginal dollar best spent to reduce juvenile crime: in police, in prisons, or in schools? Although all may reduce crime, education offers benefits that prisons and police do not, which *a priori* makes it a more appealing policy. In this section, we provide a simple estimate of the Return of Investment (ROI) of the reports, which consists in calculating the number of crimes that are prevented based on the RD estimates, scale that number by the cost per crime, and then compare that to the total costs of the dropout reports. Specifically, we estimate the crime-reduction savings that would be obtained from the implementation of the reports had this covered the entire population of 14-17 aged dropouts in high-dropout schools in 2022 (for which we do find evidence

[55] For instance, for estimating upper bound effects, we trim the treatment group observations that are below the percentile 6*th* of the distribution of SOD-peers crime rate such that re-enrollment rate is the same across treatment and control groups. Likewise, for estimating lower bound effects, we trim observations in the treatment group that are above the percentile 94*th* of the distribution of SOD-peers crime rate.

of crime effects), and then compare them to the total cost of implementing the reports across all 14-17 aged dropouts in high-dropout schools in the same year. Importantly, our cost-effectiveness analysis is restricted to the reduced costs in crime due to the report-driven re-enrollment of dropouts; 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 at-scale potential benefits. Table A.XIX presents estimates of the crime costs and reports implementation costs.

Crime Costs. 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$ 9,600, on average[56] 43% of the per-crime cost corresponds to anticipation costs (US$4,131 per crime), i.e., crime preventive expenditures; one-third are response costs associated to expenditures linked to the administrative costs of the judicial system and prisons (US$3,197 per crime); and 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 (US$2,272 per crime).

Report Implementation Costs. These include (i) the SIGE centralized dropout monitoring system, which involved the design of the system as well as the permanent hiring costs of engineers in charge of the system maintenance, for a per dropout cost of US$72.14 according to MINEDUC internal data; (ii) the costs associated to the production and delivery of the dropout reports, which involves hiring a permanent team of 3 professionals in charge of downloading the information from the monitoring system, design the reports, compile the information, upload the reports to the platform, and technical support like respond inquiries and provide clarification to school staff regarding the content, interpretation, or use of the reports (US$0.62 per dropout); and (iii) school agency costs, i.e., expenditures schools incur in efforts to reintegrate dropout students into the education system, from taking contact with dropout students and their families to coordinate their re-enrollment in the same or another school, which is approximately 10% of the yearly average wage of a school administrator (US$25 per dropout). Overall, and only considering dropouts aged 14-17 in two-sided schools, we calculate the report implementation cost is US$ 97.76 per dropout.

Cost-Effectiveness. In 2022 there were 13,862 students aged 14-17 in two-sided schools 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$ 1.35 millions (=97.76×13,862). Given the crime prevention effects of the dropout reports, we estimate 1,056 dropouts would have committed crime had not been listed in the report, for crime savings that amount to US$ 10.14 millions (=9,600×1,056). Discounting the implementation costs, this means total savings of US$ 8.78 millions or about US$ 633 per listed dropout. This represents 0.003% of Chilean GDP in 2022. The latter leads to a Return

[56]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. All these crime types are also processed by the LRPA law that originate the data we use to measure juvenile crime, and thus are a good benchmark to approximate the per-crime costs in our context.

of Investment (ROI) of 650% (=8.78/1.35), i.e., for every US$1 spent in the reports, the reports generated US$6.5 in crime savings, on average, for a benefit-cost ratio of about 6-to-1.

This exercise relies on two assumptions. First, our estimates of crime savings depend on the number of dropouts that would have committed crime had not been listed in the report, a number that is derived from crime effects that are specific to our RD sample of analysis, i.e., students aged 14-17 who dropped out from two-sided schools in a window of $\pm$ 12 school days from May 30^{th}, 2022. As discussed in Section 5.2, these effects may not be externally valid to "less vulnerable" dropouts from one-sided schools, and thus the cost-benefit analysis is circumscribed to dropouts from two-sided schools only.

Second, the reports include only students who dropped out before May 30^{th}, 2022, raising the question of whether the per-student cost should be treated as fixed or vary depending on whether the student disenrolled early or later in the year. As shown in Appendix Table A.II, students who dropped out in the first versus the second semester show similar characteristics, on average. This suggests that the marginal cost of listing dropouts does not vary over time, and we assume this to be the case.

Overall, while the reliance on assumptions warrants caution in interpretation, the estimates nonetheless highlight that the Dropout Reports benefits likely go beyond its implementation costs.

Discussion. 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 for student welfare (intrinsic motivation).

To examine this possibility, 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.XIV 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*[57]. 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[58].

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

11. Conclusion

In line with school retention as the traditional focus of education policy, the vast majority of initiatives aim to prevent dropout before it occurs. Yet far less is known about how to bring back students who have already left the system. This gap carries significant policy relevance: once preventive measures fail and at-risk students drop out, schools lose their incapacitation role, thereby increasing the risk of criminal involvement.

This paper provides causal evidence on the effectiveness of centralized dropout monitoring systems —built on Education Management Information Systems (EMIS)— as a low-cost and scalable solution for re-enrolling dropouts. By equipping Chilean schools with timely, targeted information on former students (the so-called "Dropout Reports"), the system increased both re-enrollment and grade completion, with school networks playing a key role in facilitating students'

[57] 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.

[58] 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).

return. In turn, re-enrollment led to a significant reduction in crime.

EMIS platforms are widely used around the world to collect, process, and analyze education-related data, suggesting that Chile's Dropout Reports are potentially scalable to other contexts with different education markets (UNESCO, 2020). A conservative estimate places the cost of implementing the reports at just US$98 per dropout, yielding a cost-benefit ratio of 1:6 in terms of crime-prevention gains. This likely underestimates the true value, as it excludes additional benefits such as reduced adult crime, improved labor market outcomes for re-enrolled students, and potential reductions in future dropout through changes in schools' disenrollment practices. Future research should examine the long-term effects of re-enrollment on adult crime and labor outcomes to better assess the broader societal returns of such policies.

The external validity of our findings is limited to students who dropped out from high-dropout schools, which tend to serve more vulnerable populations than the average. While this limits generalizability, it also focuses the analysis on precisely those settings where the risk of criminal involvement is highest —and where targeted interventions like the Dropout Reports are most urgently needed.

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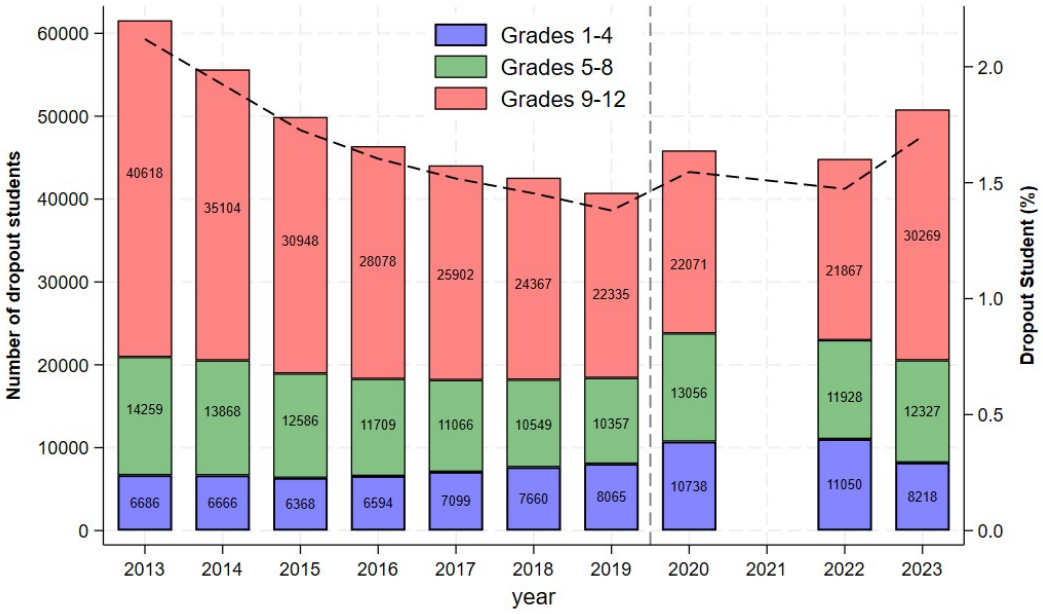
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A. Appendix Tables and Figures

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.

Table A.I: Dropouts Population aged 14-17 in 2022

Panel A. Full Population of 14-17 aged Dropouts (inter-annual + intra-annual dropouts)

	Total Dropouts	Not Re-enrolled Dropouts	Re-enrolled Dropouts
Observations	43,126	22,669	20,457
Share of Total 14-17 aged Students (%)	4.72%		
Share of Total Dropouts (%)		52.56%	47.44%
Share Re-enrolled in Origin School (%)			4.40%
Share Re-enrolled in Other School (%)			95.60%

Panel B. Within-Year 14-17 aged Dropouts (intra-annual dropouts)

	Total Dropouts	Not Re-enrolled Dropouts	Re-enrolled Dropouts
Observations	34,816	14,359	20,457
Share of Total 14-17 aged Students (%)	3.81%		
Share of Total Dropouts (%)		41.24%	58.76%
Share Re-enrolled in Origin School (%)			4.40%
Share Re-enrolled in Other School (%)			95.60%

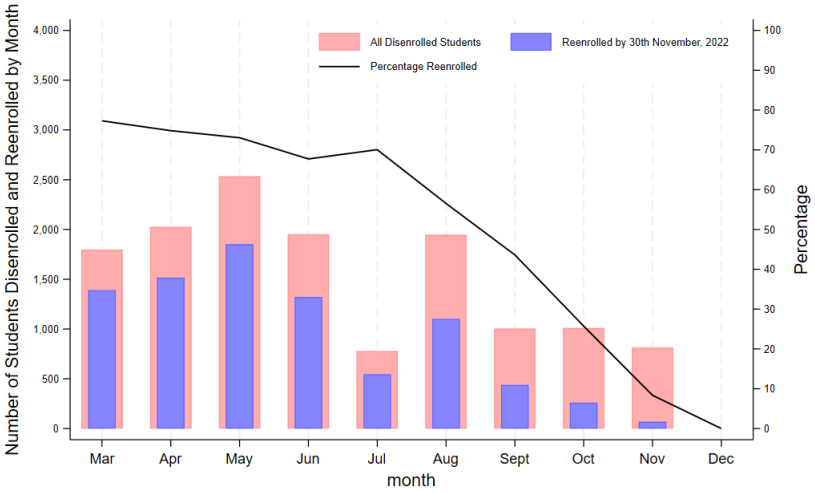
Panel C. Within-Year 14-17 aged Dropouts in Two-Sided Schools (intra-annual dropouts)

	Total Dropouts	Not Re-enrolled Dropouts	Re-enrolled Dropouts
Observations	13,862	5,844	8,018
Share of Total 14-17 aged Students (%)	1.52%		
Share of Total Dropouts (%)		42.16%	57.84%
Share Re-enrolled in Origin School (%)			4.45%
Share Re-enrolled in Other School (%)			95.55%

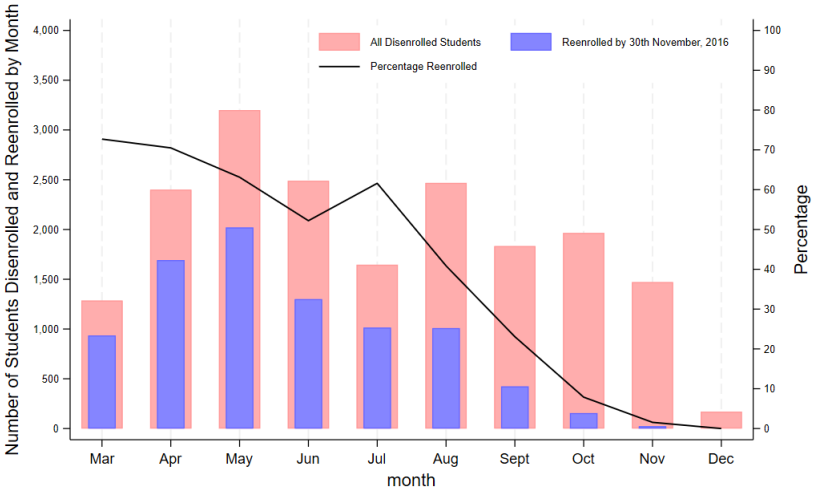
Notes: Full Population (Panel A) includes all dropout students aged 14-17 years old who were enrolled in 2021 and did not enrolled in 2022 (inter-annual dropouts) plus those who enrolled in March 2022 but disenrolled at some point throughout 2022 (intra-annual dropouts). Within-Year 14-17 aged Dropouts (Panel B) includes intra-annual dropouts only. Within-Year 14-17 aged Dropouts in two-sided schools (Panel C) includes intra-annual dropouts in two sided-schools only. Two sided schools are schools with at least one dropout student who disenrolled from the school at each side of the cutoff date May 30th, 2022 (i.e., at least one before and one after that date), considering an optimal bandwidth of ± 12 school days. Dropout students who re-enrolled in Origin School came back to the same school from which they had dropped out.

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 intra-annual dropout students aged 14-17 who disenrolled from two-sided schools. Some dropouts later re-enrolled, while others remained out of school. Each bar represents the number of students who dropped out in a given month (red), and, among them, those who re-enrolled (blue). Panel (a) corresponds to 2022 dropouts, while Panel (b) corresponds to 2016 dropouts. The year 2016 is used as a pre-treatment comparison because it shares the same school calendar structure as 2022, with an identical distribution of school days and dates. The line across bars indicates the proportion of monthly dropouts who re-enrolled within one month (labeled "Percentage" on the right Y-axis).

Table A.II: Mean Differences across dropout students who disenrolled in 1^{st} semester vs. Any semester

	Students Dropped Out within 1^{st} semester N=8,399	Students Dropped Out within 1^{st} or 2^{nd} semester N=13,862		
	Mean (1)	Mean (2)	Adj. Mean Diff. (2)-(1)	*p*-val.
Male (Female)	0.527	0.536	0.008	0.025
Age 2021	15.718	15.734	-0.007	0.298
Migrant	0.088	0.090	0.001	0.648
Age-grade lag 2021	0.640	0.632	-0.008	0.149
Prioritary 2022 (Q1+Q2)	0.677	0.683	0.003	0.313
Preferent 2022 (Q3+Q4)	0.202	0.202	0.001	0.785
Integrated student 2021	0.115	0.116	0.001	0.500
School attendance 2021	0.821	0.814	-0.003	0.029
School GPA 2021	5.207	5.160	-0.039	0.000
Graduation 2021	0.881	0.875	-0.003	0.230
If committed crime in 2021	0.018	0.017	-0.001	0.150

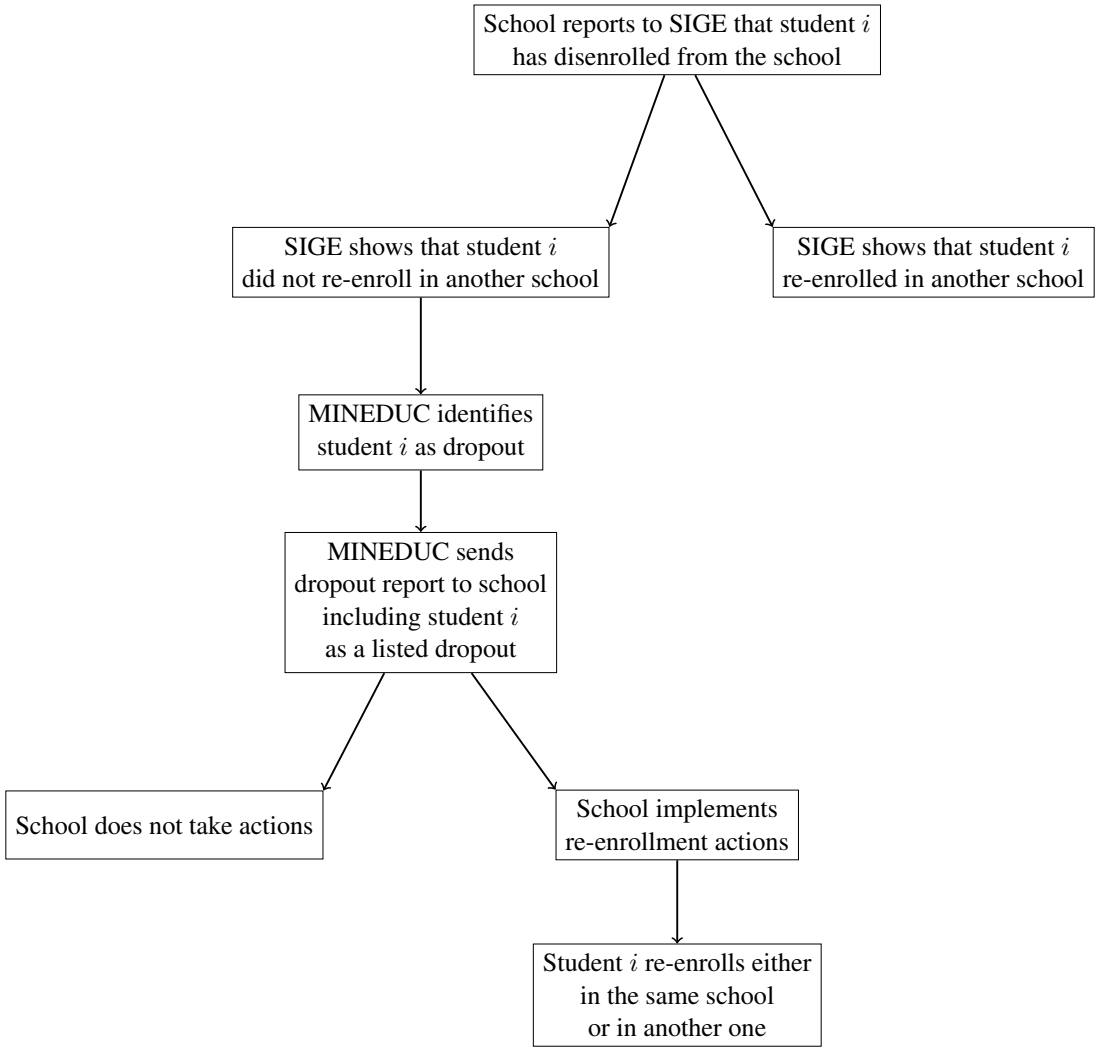
Notes: Sample of analysis are intra-annual dropout students aged 14-17 who disenrolled from two-sided schools. Sample in column (1) are dropouts who disenrolled within the first semester (before July 25th, 2022), while those in columns (2) are dropouts who disenrolled at any time throughout the year. Students who dropped out more than once throughout 2022 are counted as separate cases, and corresponds to less than 5% of dropouts. Adjusted mean differences are estimated by regressing the variable of interest against a group dummy that equals 1 if the student dropped out within the first semester, and zero if at any time throughout the year, controlling for school fixed effects. *p*-values derived from standard errors clustered at the school level.

Table A.III: Dropouts vs. Old and New classmates

	Dropouts vs. Old Classmates		Not Re-enrolled vs. Re-enrolled			Re-enrolled vs. New Classmates		Re-enrolled Old vs. New Schools		
	Mean Dropouts	Mean Diff. wrt. Old Classmates	Mean Not Re-enrolled Dropouts	Mean Re-enrolled Dropouts	Mean Diff.	Mean Re-enrolled Dropouts	Mean Diff. wrt. New Classmates	Mean Origin School	Mean Destination School	Mean Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Student Level										
Male (Female)	0.529	0.003	0.581	0.493	0.088***	0.493	-0.024***			
Migrant	0.088	0.019***	0.150	0.045	0.106***	0.045	-0.011***			
Age-grade lag 2021	0.408	0.263***	0.597	0.279	0.318***	0.279	0.032***			
Priority 2022 (Q1+Q2)	0.676	0.034***	0.649	0.694	-0.044***	0.694	0.037***			
Preferent 2022 (Q3+Q4)	0.200	-0.049***	0.186	0.209	-0.023***	0.209	-0.031***			
Integrated student 2021	0.139	0.002	0.148	0.133	0.015***	0.133	-0.014***			
School attendance 2021	0.824	-0.041***	0.789	0.848	-0.059***	0.848	-0.004*			
GPA (Z) 2021	-0.586	-0.474***	-0.895	-0.378	-0.518***	-0.378	-0.185***			
Simce Math (Z) 2014-2018	-0.345	-0.111***	-0.437	-0.292	-0.145***	-0.292	-0.031**			
Simce Language (Z) 2014-2018	-0.311	-0.100***	-0.387	-0.267	-0.120***	-0.267	-0.040***			
Graduation 2021	0.895	-0.065***	0.839	0.933	-0.093***	0.933	0.006**			
School Level										
Log total enrollment 2021								6.474	6.863	0.390***
Regular Education School								0.996	0.901	-0.095***
Adult Education School								0.004	0.093	0.090***
School Graduation 2021								0.914	0.888	0.026***
School Attendance 2021								0.822	0.810	-0.012***

Notes: Sample includes only 14–17-year-old students who dropped out from two-sided schools during 2022. Old classmates are students from the same class as dropouts in their school of origin. Column (2) reports the estimate from regressing the variable of interest on a dummy equal to 1 if the student is a dropout and 0 if the class average, controlling for student fixed effects (standard errors are clustered at the school level). Column (3) includes dropouts who did not re-enroll, while Column (4) includes those who re-enrolled. Column (5) shows the estimate from regressing the variable of interest on a dummy equal to 1 if the dropout did not re-enroll and 0 if re-enrolled, controlling for school fixed effects (standard errors are clustered at the school level). New classmates are students in the destination school of re-enrolled dropouts. Column (7) reports the estimate from regressing the variable of interest on a dummy equal to 1 if the student is a re-enrolled dropout and 0 if the class average in the destination school, controlling for student fixed effects (standard errors are clustered at the school level). Column (8) refers to schools of origin of re-enrolled dropouts, while Column (9) refers to the schools of destination of re-enrolled dropouts. Column (10) shows the estimate from regressing the variable of interest on a dummy equal to 1 if school of destination and zero if school of origin (robust standard errors). *p<0.10; **p<0.05; ***p<0.01.

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



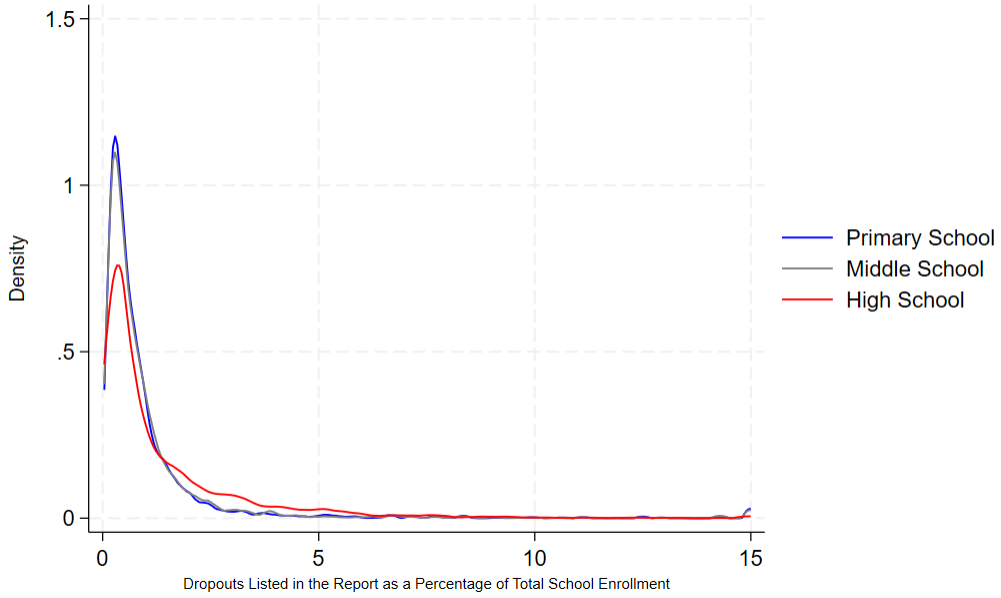
Notes: The figure illustrates how the Centralized Dropout Monitoring System works in practice. Schools in Chile are required to report to the centralized SIGE platform whenever a student enrolls or disenrolls from the school. MINEDUC identify dropout students as those who disenroll from a school without 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



Notes: Example of the dropout report. The second page details statistics about 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 third page provides a roster of disenrolled students under dropout status by May 30^{th}. The dropouts list comes in a detailed table capturing dropout-level information, including social security number, full name, gender, age, the municipality of residence from the preceding year, the grade level they were enrolled in, and grade completion status by the end of 2021. 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.In case the school had not disenrolled students under dropout status by May 30^{th}, 2022, the list of dropout students was empty.

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.IV: Optimal Bandwidth Selection

Year Data	Optimal Bandwidth (1)	Optimal Bandwidth (2)
2016	16.00	15.00
2017	16.00	16.00
2018	20.00	19.00
2019	16.00	15.00
2020	No Data on Withdrawals	
2021	12.00	12.00
Day-of-the-week FE	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 is a dummy that equals one if the disenrolled student committed crime within that year or before, and zero otherwise. Errors are clustered at the school-group level. Model (1) is an OLS linear regression model, with no controls, while Model (2) adds day-of-the-week fixed effects. 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: External Validity I: Mean Differences across dropouts from Two-sided vs One-sided schools

	Dropouts from Two-sided Schools Within Opt. BW Included in Evaluation Sample N=3,142 S=840	Dropouts from One-sided Schools Within Opt. BW Excluded from Evaluation Sample N=2,437 S=1,849		
	Mean (1)	Mean (2)	Mean Diff. (1)-(2)	*p*-val.
Panel A. Student Level				
Male (Female)	0.525	0.517	0.009	0.590
Age 2021	15.642	15.189	0.454	0.000
Migrant	0.079	0.074	0.005	0.544
Age-grade lag 2021	0.610	0.564	0.046	0.091
Prioritary 2022 (Q1 + Q2)	0.690	0.677	0.013	0.366
Preferent 2022 (Q3 + Q4)	0.198	0.204	-0.005	0.644
Integrated Student	0.123	0.160	-0.036	0.000
School Attendance 2021	0.817	0.832	-0.016	0.075
School GPA 2021	5.187	5.344	-0.157	0.000
Graduation 2021	0.879	0.918	-0.039	0.000
If committed crime in 2021	0.002	0.001	0.000	0.719
Panel B. School Level				
% Prioritary Student	0.313	0.292	0.021	0.000
Mean SIMCE 2018 (Z)	-0.167	0.037	-0.204	0.000
If Public (Private Voucher)	0.526	0.538	-0.012	0.581
Low SES	0.751	0.681	0.070	0.000
Med SES	0.204	0.247	-0.044	0.013
High SES	0.044	0.070	-0.026	0.006

Notes: Sample of analysis are dropout students in 2022 who were 14-17 years old the month before the report disbursement, and who disenrolled from school within the optimal bandwidth of $\pm$ 12 school days from May 30th, 2022, our cutoff date. Two-sided schools have dropouts at both sides of the optimal bandwidth around the cutoff date. One-sided schools only have dropouts in one of the two sides of the optimal bandwidth around the cutoff date. N indicates number of dropout students. S indicates number of schools. Mean differences are estimated by regressing the variable of interest against a group dummy that equals 1 if the student dropped out from a two-sided school, and zero if dropped from a one-sided school, with standard errors clustered at the school level.

Table A.VI: External Validity II: Mean Differences across dropouts in and out of Optimal Bandwidth

	Dropouts from Two-sided Schools Within Optimal BW Included in Evaluation Sample N=3,142 S=840	Dropouts from Two-sided Schools Full Sample N=13,862 S=840		
	Mean (1)	Mean (2)	Adjusted Mean Diff. (2)-(1)	*p*-val.
Male (Female)	0.525	0.536	-0.011	0.137
Age 2021	15.642	15.734	-0.006	0.680
Migrant	0.079	0.090	-0.007	0.119
Age-grade lag 2021	0.610	0.632	-0.006	0.627
Prioritary 2022 (Q1+Q2)	0.690	0.683	0.009	0.182
Preferent 2022 (Q3+Q4)	0.198	0.202	-0.004	0.490
Integrated student 2021	0.123	0.116	0.003	0.577
School attendance 2021	0.817	0.814	-0.004	0.108
School GPA 2021	5.187	5.160	0.003	0.820
Graduation 2021	0.879	0.875	-0.004	0.429
If committed crime in 2021	0.016	0.017	0.000	0.815

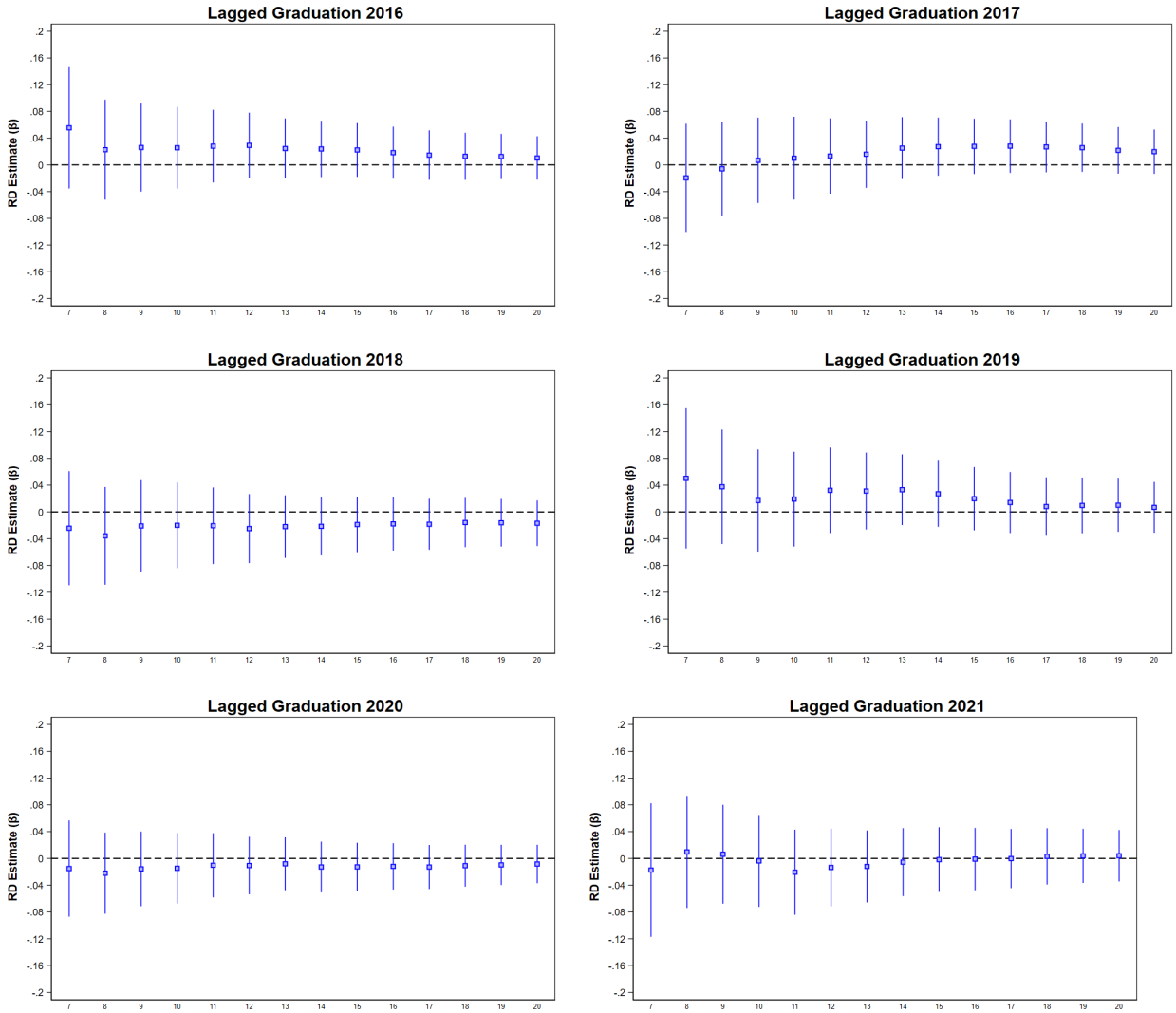
Notes: Sample in column (1) corresponds to our Evaluation Sample, i.e., dropout students who disenrolled from two-sided schools in dates within a window of $\pm$ 12 school days from the cutoff date (May 30^{th}, 2022), and were 14-17 years old the month before the report disbursement. Sample in column (2) are all dropout students who disenrolled from two-sided schools in 2022, and were 14-17 years old the month before the report disbursement. Adjusted mean differences are estimated by regressing the variable of interest against the group dummy, controlling for school fixed effects. *p*-values derived from standard errors clustered at the school level.

Table A.VII: Internal Validity: Balance Checks – Evaluation Sample

	Control Mean	Adjusted Mean Diff.	Std. Err.	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					
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
If Committed Crime Before May 30^{th}, 2022 (=1)	0.017	0.005	(0.011)	1,534	1,394
Panel D. Re-enrollment before Cutoff					
If Re-enrolled Before Cutoff (=1)	0.362	0.021	(0.052)	1,534	1,394

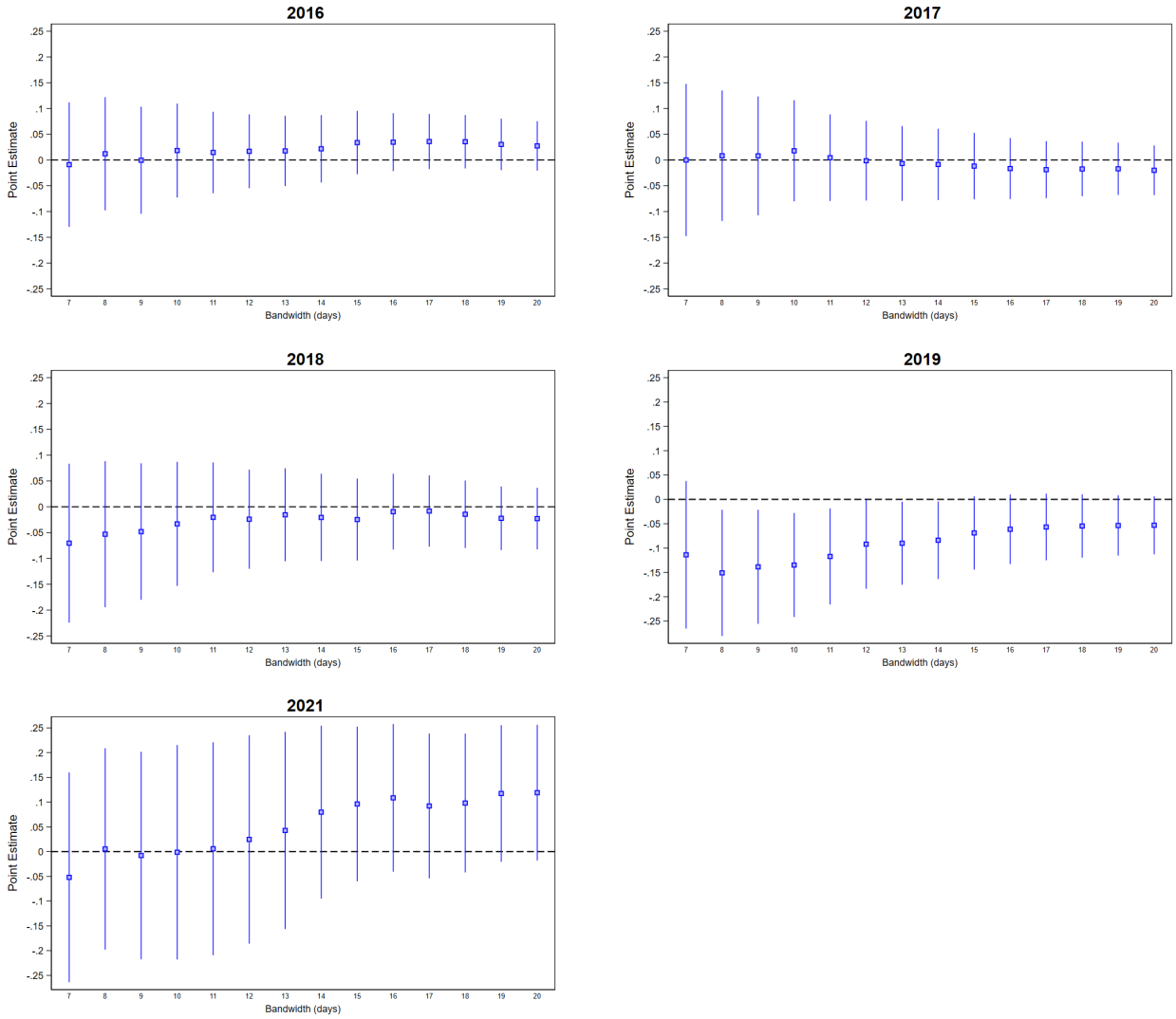
Notes: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. Opt. bandwidth is fixed at $\pm$12 school days, which is selected following MSE-optimal bandwidth choices. Control Mean is the average value of each covariate for students who disenrolled after May 30^{th}, 2022, the cutoff date. Diff. denotes the mean difference between students on either side of the cutoff, and are obtained by regressing the pre-treatment outcome against the running variable, the before-after dummy, and their interaction, all controlling for day-of-the-week and school fixed effects. 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. As in the post-treatment analysis, the triangular kernel weight observations at each extreme of the running variable by zero, hence the total number of observations is reduced from 3,142 to 2,928. Standard errors clustered at the school-group level are shown in parentheses in column Std.Err. 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 withdrawn students, that 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.VI: Baseline Balance I: Lagged Graduation for the 2022 Cohort across multiple alternative bandwidths



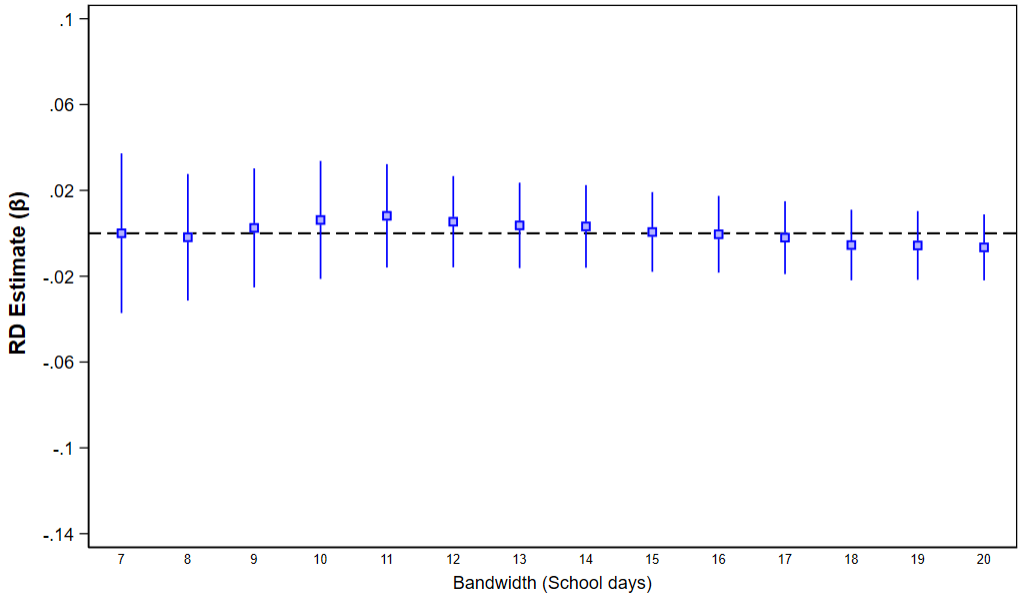
Notes: Sample of analysis are 14-17 aged dropouts from 2022 cohort who dropped out from two-sided schools. Each figure shows the mean difference (and its associated 95% confidence interval) on lagged school graduation in a given pre-treatment year for students who withdrew from school before the cutoff date of May 30th, 2022, versus after it. A range of optimal bandwidths around the cutoff date is used, from 7 to 20 school days, i.e., one month in total. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. Specifically, we regress the lagged graduation dummy against the before-after dummy, the running variable, and their interaction, all controlling for day-of-the-week fixed effects, school fixed effects, and if graduated in the year previous to lagged graduation. The point estimate reflects the parameter estimate accompanying the before-after dummy. For instance, in Figure "Lagged Graduation 2021", the point estimate associated with a bandwidth of $\pm$ 15 reflects the difference in graduation rates in 2021 between students who disenrolled from school in 2022 within 15 school days before the cutoff date of May 30th, 2022, and those who disenrolled within 15 school days after the cutoff date. 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.

Figure A.VII: Baseline Balance II: Graduation for 2016–2021 Pre-Treatment Cohorts across multiple alternative bandwidths



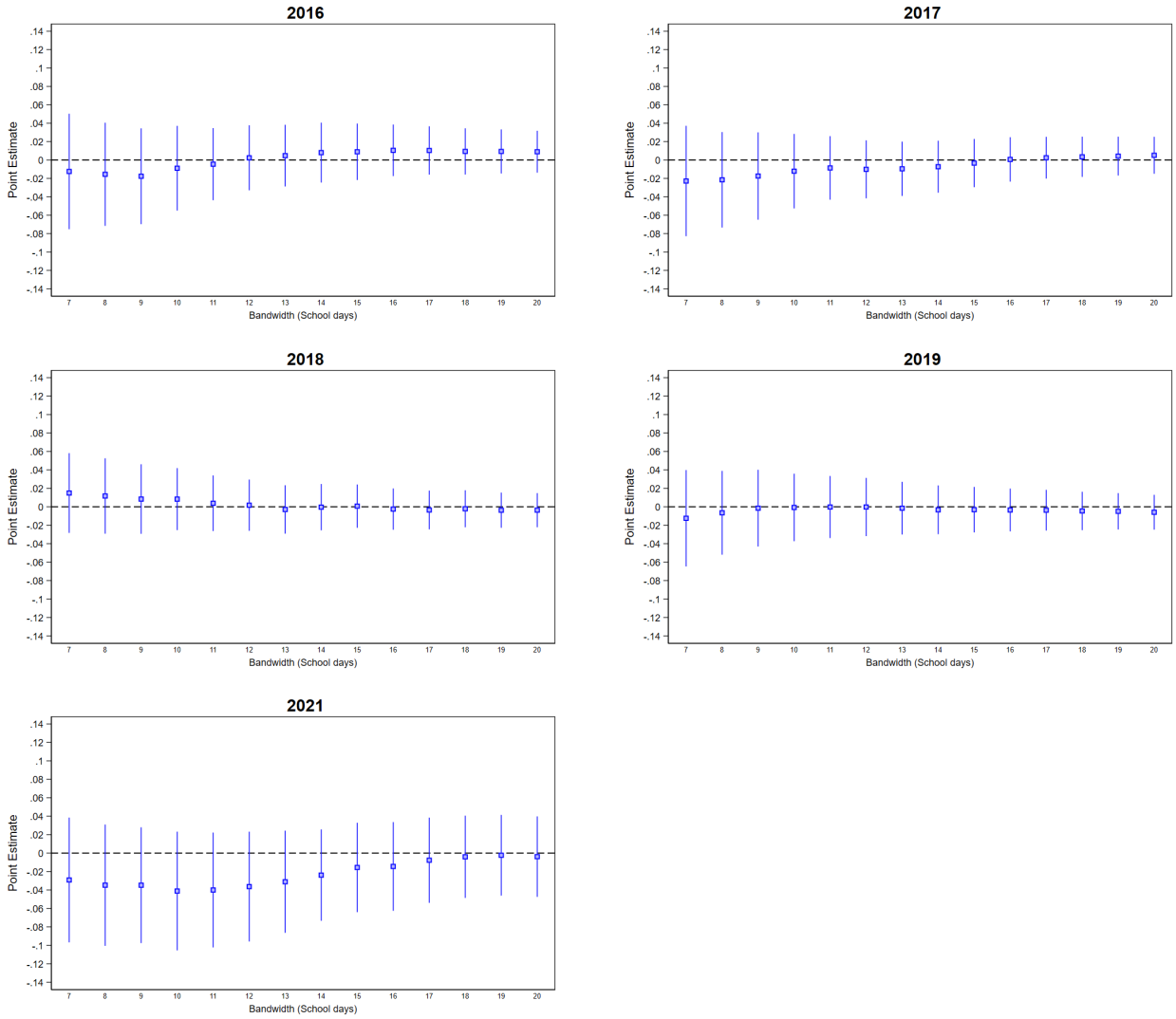
Notes: Sample of analysis are 14-17 aged dropouts from pre-treatment cohorts 2016-2021 who dropped out from two-sided schools. Each figure shows the mean difference (and its associated 95% confidence interval) on contemporanous graduation in a given pre-treatment year for students who withdrew from school before May 30th of each year versus after it. A range of optimal bandwidths around the cutoff date is used, from 7 to 20 school days, i.e., one month in total. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. Specifically, we regress the contemporaneous graduation dummy against the before-after dummy, the running variable, and their interaction, all controlling for day-of-the-week fixed effects, school fixed effects, and if graduated in the previous year. The point estimate reflects the parameter estimate accompanying the before-after dummy. Standard errors are clustered at the group-school level. Each year represents a different cohort of dropout students. For instance, in Figure "2016", the point estimate associated with a bandwidth of $\pm$ 15 reflects the difference in graduation rates in 2016 between students who disenrolled from school in 2016 within 15 school days before the cutoff date of May 30th, 2016, and those who disenrolled within 15 school days after the cutoff date. The year 2020 is excluded from the analysis, as schools were closed around the cutoff date and no withdrawal data were available.

Figure A.VIII: Baseline Balance III: Lagged Crime for the 2022 Cohort across multiple alternative bandwidths



Notes: Sample of analysis are 14-17 aged dropouts from 2022 cohort who dropped out from two-sided schools. The figure shows the mean difference (and its associated 95% confidence interval) on whether the dropout student committed crime before the cutoff date of May 30^{th}, 2022 for students who withdrew from school before the cutoff date versus after it. A range of optimal bandwidths around the cutoff date is used, from 7 to 20 school days, i.e., one month in total. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. Specifically, we regressing the crime dummy against the before-after dummy, the running variable, and their interaction, all controlling for day-of-the-week fixed effects, school fixed effects, and if graduated in the year previous to lagged graduation. The point estimate reflects the parameter estimate accompanying the before-after dummy. For instance, the point estimate associated with a bandwidth of $\pm$ 15 reflects the difference in pre-treatment crime rate between students who disenrolled from school in 2022 within 15 school days before the cutoff date of May 30th, 2022, and those who disenrolled within 15 school days after the cutoff date. 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.

Figure A.IX: Baseline Balance IV: Crime for 2016–2021 Pre-Treatment Cohorts across multiple alternative bandwidths



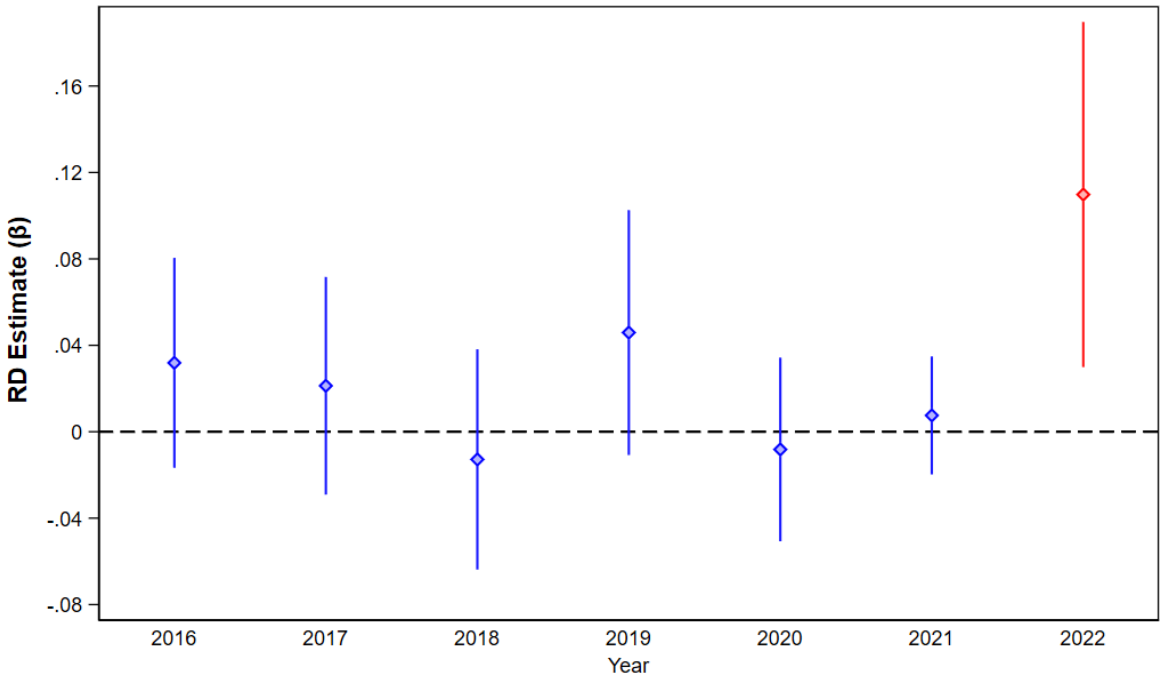
Notes: Sample of analysis are 14-17 aged dropouts from pre-treatment cohorts 2016-2021 who dropped out from two-sided schools. Each figure shows the mean difference (and its associated 95% confidence interval) on contemporanous crime in a given pre-treatment year for students who withdrew from school before May 30th of each year versus after it. A range of optimal bandwidths around the cutoff date is used, from 7 to 20 school days, i.e., one month in total. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. Specifically, we regress the contemporaneous crime dummy against the before-after dummy, the running variable, and their interaction, all controlling for day-of-the-week fixed effects, school fixed effects, and a dummy on whether the dropout student committed a crime in the previous year. The point estimate reflects the parameter estimate accompanying the before-after dummy. Standard errors are clustered at the group-school level. Each year represents a different cohort of dropout students. For instance, in Figure "2016", the point estimate associated with a bandwidth of $\pm$ 15 reflects the difference in crime rates in 2016 between students who disenrolled from school in 2016 within 15 school days before the cutoff date of May 30th, 2016, and those who disenrolled within 15 school days after the cutoff date. The year 2020 is excluded from the analysis, as schools were closed around the cutoff date and no withdrawal data were available.

Table A.VIII: Robustness I: RD Estimates using a Quadratic Model

	If Re-enrolled by Aug. 31^{st}, 2022	If Graduated by Dec. 31^{st}, 2022	If committed Crime PI cases July 1st, 2022 - April 30, 2024		
			Any Crime	Severe Crime	Not Severe Crime
	OLS Linear (1)	OLS Linear (2)	OLS Linear (3)	OLS Linear (4)	OLS Linear (5)
W.D. $\leq$ May 30^{th}, 2022	0.1185**	0.1593**	-0.0472*	0.0005	-0.0476**
	(0.0599)	(0.0692)	(0.0264)	(0.0186)	(0.0217)
Ineligible Mean	0.7512	0.6783	0.0321	0.0132	0.0189
Observations	2,928	2,928	2,928	2,928	2,928
Schools	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12
Polynomial Degree	2	2	2	2	2
School FE	Yes	Yes	Yes	Yes	Yes
If Graduated in 2021 (= 1)	Yes	Yes	Yes	Yes	Yes
If Crime at baseline (= 1)	Yes	Yes	Yes	Yes	Yes
Day-of-week FE	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. All models are OLS quadratic models. Regression discontinuity estimates of the effect of dropping out before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31st (two months after the report – delivered on June 28th, 2022), on whether graduated from school by the end of the year (Dec 31st, 2022), and on crime outcomes. We count only crime events with pretrial injunctions (typically sanctioned within 24–48 hours) committed between July 1, 2022, and April 30, 2024, that is, starting immediately after the dropout reports' release (June 28th, 2022). Severe crimes include homicides, rapes, aggravated assaults, and recidivism. Not-severe crimes include assaults, property damage, thefts, and drug-related offenses. All models control for school FEs, day-of-week fixed effects, a dummy on whether the dropout student graduated in the previous year, a dummy on whether the dropout student committed crime before the reports were disbursed, dummies of the associated gravosity of the committed crime (1-5), and additional controls for pre-treatment covariates, including gender, age dummies, lag-age dummies, parents' schooling dummies, school attendance's tercile dummies, grade dummies, GPA tercile dummies, if preferent student, and if migrant student. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. 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.

Figure A.X: Robustness II: Report Effects on Graduation across Non-Treatment and Treatment years

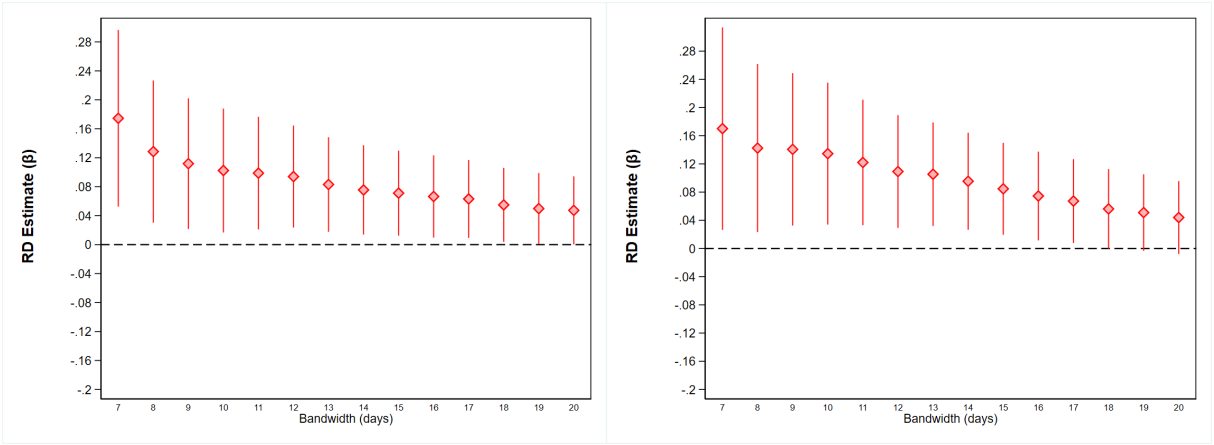


Note: Sample of analysis are 14-17 aged dropouts from 2022 cohort who dropped out from two-sided schools within the optimal bandwidth of $\pm$ 12 school days from the cutoff date, May 30^{th}, 2022. Outcome of interest is a dummy that equals 1 if the dropout student graduated that year or not. For each 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. For instance, the point estimate associated with pre-treatment year 2016 (in blue) reflects the difference in graduation rates in 2016 between students who disenrolled before May 30^{th}, 2022, and those who disenrolled after that date. Likewise, the point estimate associated with treatment year 2022 (in red) reflects the difference in graduation rates in 2022 across the very same two groups. 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.XI: Robustness III: Re-enrollment and Graduation Effects across multiple alternative bandwidths

(a) Re-enrollment Effect

(b) Graduation Effect



Note: Sample of analysis are 14-17 aged dropouts who dropped out from two-sided schools. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. Point estimates and their associated 95% confidence intervals for the β parameter from estimating our main equation 1 on re-enrollment (left panel) and graduation (right panel) in 2022 across a range of bandwidths around the cutoff date (May 30^{th}, 2022), from 7 to 20 school days, i.e., one month in total. Each point estimate comes from a separate regression involving students who disenrolled in dates up to the specified bandwidth. For instance, in the right graph, the point estimate associated to the bandwidth 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. All models control for school fixed effects, day-of-the-week fixed-effects, and a set of additional pre-treatment covariates 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, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. 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.

Table A.IX: Robustness IV: Falsification Tests

	If Re-Enrolled by Aug. 31^{st}, 2022			If Graduated by Dec. 31^{st}, 2022			If committed Crime - PI Cases July 1^{st}, 2022 - April 30^{th}, 2024		
	Fals. Test 1	Fals. Test 2	Fals. Test 3	Fals. Test 1	Fals. Test 2	Fals. Test 3	Fals. Test 1	Fals. Test 2	Fals. Test 3
Cutoff Date	Fals. Cutoff Mar30, 2022	Fals. Cutoff Apr29, 2022	Official Cutoff May30, 2022	Fals. Cutoff Mar30, 2022	Fals. Cutoff Apr29, 2022	Official Cutoff May30, 2022	Fals. Cutoff Mar30, 2022	Fals. Cutoff Apr29, 2022	Official Cutoff May30, 2022
Sample	Mar30 Falsif Sample	Apr29 Falsif. Sample	May30 Transfer Stud. Sample	Mar30 Falsif. Sample	Apr29 Falsif. Sample	May30 Transfer Stud. Sample	Mar30 Falsif. Sample	Apr29 Falsif. Sample	May30 Transfer Stud. Sample
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)
W. Date $\leq$ Cutoff Date	-0.035	-0.023	0.043	-0.015	-0.039	-0.027	0.018	0.008	-0.045
	(0.034)	(0.040)	(0.052)	(0.040)	(0.046)	(0.178)	(0.011)	(0.017)	(0.093)
Counterfactual Mean	0.831	0.793	0.987	0.711	0.666	0.874	0.015	0.028	0.016
Observations	5,039	3,415	690	5,039	3,415	690	5,039	3,415	690
Schools	1238	949	466	1238	949	466	1238	949	466
Optimal Bandwidth	12	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (=1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are 14-17 aged students who disenrolled from two-sided schools. Regression discontinuity estimates of the effect of withdrawing before or after the indicated cutoff date on whether the disenrolled student re-enrolled by August 31st (two months after the report – delivered on June 28th, 2022), on whether graduated from school by the end of the year (Dec 31st, 2022), and on crime outcomes. We count only crime events with pretrial injunctions (typically sanctioned within 24–48 hours) committed between July 1, 2022, and April 30, 2024, that is, starting immediately after the dropout reports' release (June 28th, 2022). Estimates correspond to the β parameter from Equation 1. All models include school fixed effects, day-of-the-week fixed effects, and a dummy on whether the dropout student graduated in the previous year. Columns (1), (4), and (7) use March 30^{th}, 2022 (the penultimate school day of March) as a placebo cutoff. Columns (2), (5), and (8) use April 29^{th}, 2022 (the penultimate school day of April) as a placebo cutoff. For these, the sample includes students who withdrew within $\pm$12 school days of the respective placebo date. Columns (3), (6), and (9) restrict the sample to transfer students who withdrew within $\pm$12 school days from the official cutoff date of may 30^{th}, 2022, i.e., students who disenrolled but re-enrolled in another school within 24 hours of withdrawal and thus are not eligible to be included the dropout report. All models apply triangular kernel weights. Standard errors clustered at the school-group level are reported in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.X: Robustness V: 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	-0.0579*	-0.0679	-0.0873**
	(0.0339)	(0.0423)	(0.0419)
Ineligible Mean	0.3375	0.3375	0.3375
Observations	2,928	2,928	2,928
Schools	840	840	840
Optimal Bandwidth	12	12	12
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 aged 14-17 who dropped out from two-sided schools. 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 $\pm$ 12 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 FEs, day-of-weeks FEs, and the outcome at baseline, i.e., a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year) or not. Columns (1) estimates a local linear polynomial model (Calonico et al., 2014, command `rdrobust` in `Stata`). Regression model in column (2) show the RD estimate from a linear OLS model. Regression model in column (3) additionally control for a set of pre-treatment covariates, gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. 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.XI: Robustness VI: Treat. Effects on Re-enrollment, Graduation, and Crime: Std.Err. clustered at the school-level

	If Re-enrolled by Aug31,2022		If Graduated by Dec31,2022		If committed Crime PI Cases July1,2022 to Apr30,2024	
	OLS Linear	2SLS	OLS Linear	2SLS	OLS Linear	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
W.D. $\leq$ May 30^{th}, 2022 ($B_{isd} = 1$)	0.094**		0.105**		-0.038**	
	(0.043)		(0.049)		(0.019)	
If listed in the Report ($T_{isd} = 1$)		0.187**		0.209**		-0.076**
		(0.087)		(0.098)		(0.039)
Ineligible Mean	0.751	0.751	0.678	0.678	0.032	0.032
Observations	2,928	2,928	2,928	2,928	2,928	2,928
Schools	840	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FE	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (= 1)	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
1^{st} Stage Estimate		0.501		0.501		0.501
Cragg-Donald F-Statistic		335.379		335.379		335.379
Kleibergen-Paap F-Statistic		153.047		153.047		153.047

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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); on whether graduated from school by the end of the year (Dec 31^{st}, 2022); and on whether 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). Optimal bandwidth is fixed at $\pm$ 12 school days, which is consistent with the optimal bandwidth used for estimating crime effects. 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, a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year), and a set of pre-treatment covariates, including gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. Ineligible Mean is the mean outcome for students who disenrolled after May 30^{th}, 2022. Regression models in odds columns show linear OLS estimates. Regression columns in 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 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 full set of covariates. 2SLS coefficient is equivalent to the reduced form estimate divided by the first stage estimate. Standard errors clustered at the school level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.XII: Treatment Effects on School Re-enrollment, by Type of Re-enrollment

	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	0.094***	0.031	0.063	0.045	0.009	0.008
	(0.036)	(0.025)	(0.041)	(0.043)	(0.023)	(0.017)
Observations	2,928	2,928	2,928	2,928	2,928	2,928
Ineligible Mean	0.751	0.077	0.674	0.589	0.058	0.027
Schools	840	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (=1)	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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 dropout student re-enrolled in the same school from which the student dropped out. Different School is a dummy that equals 1 if the dropout student re-enrolled in a school other than the school of origin. Free Exams is a dummy that equals 1 if the dropout 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 dropout 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 $\pm$ 13 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 gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. Standard errors clustered at the school-group level are shown in parentheses. *p<0.10; **p<0.05; ***p<0.01.

Table A.XIII: Role of Spatial Networks on Treatment Effects on Re-enrollment

	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}, 2022	0.094***	0.044	0.088**	-0.044**	0.030	0.026	0.004	0.010	0.008
	(0.036)	(0.042)	(0.040)	(0.021)	(0.043)	(0.041)	(0.019)	(0.023)	(0.017)
Ineligible Mean	0.751	0.358	0.299	0.059	0.313	0.285	0.028	0.058	0.027
Observations	2,928	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	840
Optimal Bandwidth	12	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (=1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

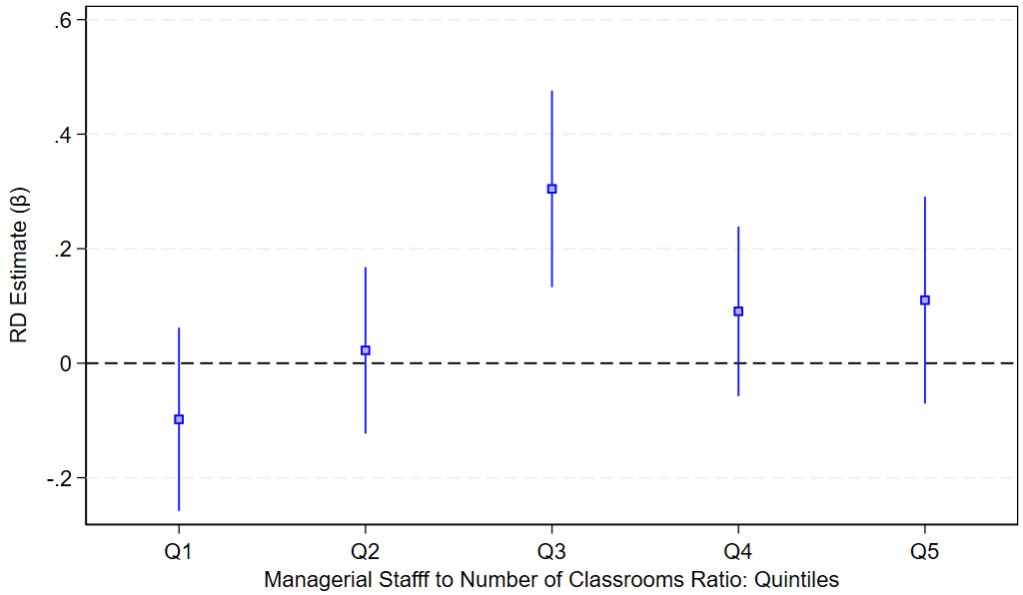
Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st} (two months after the report – delivered on June 28th, 2022). Optimal bandwidth is fixed at $\pm$ 12 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 gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. 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). The other 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)). The columns "Total" in columns (2) and (5) sums regular and non-regular schools under each heading. 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). 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.XIV: Role of Institutional Networks on Treatment Effects on Re-enrollment

	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}, 2022	0.044	0.045	0.060*	-0.015	-0.009*	-0.005	-0.004
	(0.042)	(0.034)	(0.033)	(0.011)	(0.005)	(0.004)	(0.003)
Ineligible Mean	0.358	0.154	0.135	0.019	0.003	0.002	0.001
Observations	2,928	2,928	2,928	2,928	2,928	2,928	2,928
Schools	840	840	840	840	840	840	840
Optimal Bandwidth	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1
School FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduated in 2021 (=1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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 $\pm$ 12 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 gender, age dummies, lag-age dummies, parents' schooling dummies, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. Outcome in column 1 is a dummy that equals 1 if the dropout re-enrolled in a school located within the same municipality of the origin school, and zero otherwise. Conditional on re-enrolling in the same municipality, the next outcomes are dummies defined based on whether the dropout re-enrolled 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)). 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. Column (1) estimate sums the total estimate in columns (2) and (5), plus the effects on those who re-enrolled in schools within the same municipality but with no information on institutional networks (not shown). Students who re-enrolled in the origin school are coded as if re-enrolled in the origin school's institutional network. 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.XII: Re-enrollment effects across Quintiles of Managerial Staff to Classrooms Ratio in Origin School



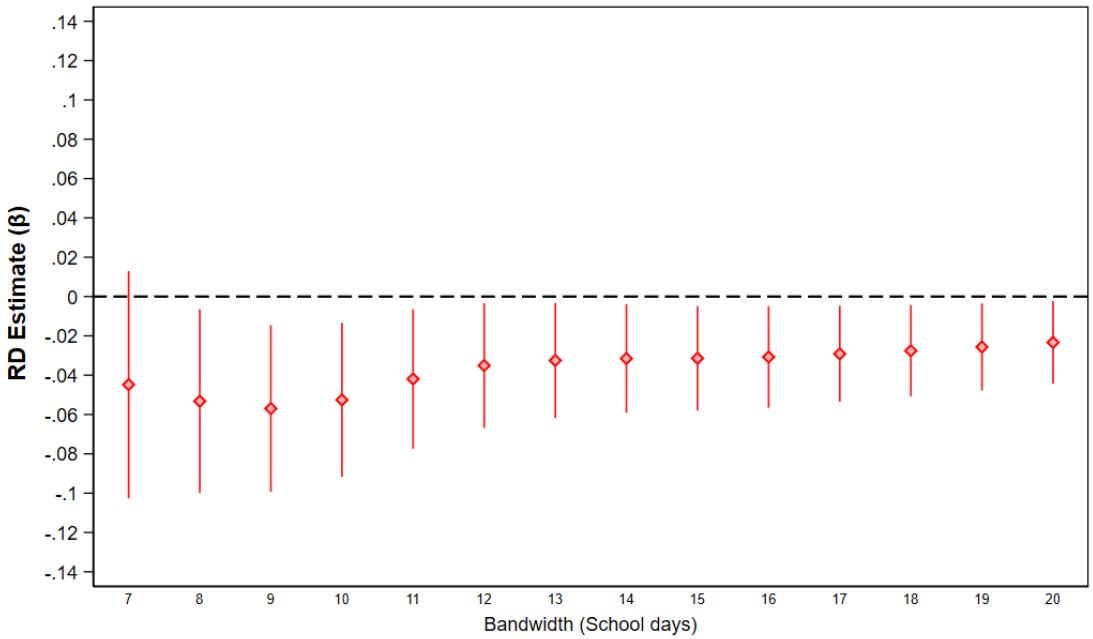
Notes: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools and within the optimal bandwidth of $\pm$ 12 school days from the cutoff date, May 30^{th}, 2022. Regression discontinuity estimates of the effect of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on whether the disenrolled student re-enrolled by August 31^{st}, controlling for school fixed effects, day-of-week fixed effects, and a whether the withdrawn student graduated in 2021 or not. Regressions are estimated separately for the quantile of Managerial Staff to Classrooms Ratio of the school of origin from which the student dropped out. For each quintile, each dot represents the estimate of the parameter β (and its associated 95% confidence interval) from equation 1. 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.XV: 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	OLS	OLS
	(1)	(2)	(3)
W.D $\leq$ May 30^{th}, 2022	0.078**	0.152***	0.011
	(0.036)	(0.052)	(0.050)
Ineligible Mean	0.805	0.814	0.795
Observations	2,928	1,451	1,477
Schools	840	439	401
Optimal Bandwidth	12	12	12
Polynomial Degree	1	1	1
School FEs	Yes	Yes	Yes
Day-of-week FEs	Yes	Yes	Yes
Graduated in 2021 (=1)	Yes	Yes	Yes

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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 $\pm$ 12 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 dummy for whether the withdrawn student graduated in 2021 or not. 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.XIII: Robustness: Crime Effects across multiple alternative bandwidths



Note: Sample of analysis are 14-17 aged dropouts who dropped out from two-sided schools. Point estimates and their associated 95% confidence intervals for the β parameter from estimating our main equation 1 on whether the dropout student committed a PI crime between July 1st, 2022 and April 30th, 2024. There is a range of different bandwidths around the cutoff date (May 30^{th}, 2022), from 7 to 20 school days, i.e., one month in total. 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 bandwidth 15 indicates the difference in crime rates 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. All models control for school fixed effects, day-of-the-week fixed-effects, and a set of additional pre-treatment covariates 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, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies. 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.

Table A.XVI: Robustness: Treatment Effects on Crime using PI + Not-PI Crimes

	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 ($B_{isd} = 1$)	-0.0413***	-0.0331**	-0.0382**		-0.0268*	-0.0297*	-0.0354**	
	(0.0125)	(0.0160)	(0.0161)		(0.0143)	(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				0.502				0.502
Cragg-Donald F-Statistic				240.940				240.940
Kleibergen-Paap F-Statistic				175.635				175.635

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from two-sided schools. 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, day-of-week fixed effects, and a dummy on whether the dropout student committed crime before the reports were disbursed and a dummy of the associated gravosity of the committed crime (1-5). Columns (1) and (5) compute the robust estimate using a local linear polynomial model (Calonico et al., 2014, command `rdrobust` in `Stata`). Regression models in columns (2) and (6) show estimates from a linear OLS model. 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.

Table A.XVII: Intensive Margin Effects on Crime: Peers-in-School-of-Destination Attributes

	Crime Rate in School-of-Destination: All peers						Crime Rate in School-of-Destination: Only Boys peers					
	All Crimes 2016-2021		Severe Crimes 2016-2021		Not Severe Crimes 2016-2021		All Crimes 2016-2021		Severe Crimes 2016-2021		Not Severe Crimes 2016-2021	
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)	OLS (10)	OLS (11)	OLS (12)
Upper: W.D. $\leq$ May30	0.015		0.016		-0.003		0.005		0.013		-0.015*	
	(0.011)		(0.011)		(0.003)		(0.013)		(0.012)		(0.009)	
Lower: W.D. $\leq$ May30		-0.000		-0.001		-0.006**		-0.015*		-0.006		-0.020***
		(0.006)		(0.006)		(0.003)		(0.009)		(0.008)		(0.006)
Observations	1,923	1,922	1,923	1,923	1,919	1,920	1,923	1,920	1,921	1,923	1,921	1,920
Ineligible Mean	0.019	0.019	0.014	0.014	0.017	0.017	0.046	0.046	0.025	0.025	0.039	0.039
Optimal Bandwidth	12	12	12	12	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grad. in 2021 (= 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from “high frequency" dropout (two-sided) schools within the optimal bandwidth of $\pm$ 12 school days from the cutoff date May 30^{th}, 2022, and re-enrolled in the origin school or a different school (either regular or non-regular) by August 31^{st}, 2022. Dropouts re-enrolled in Free Exam Schools (<7%) are not considered as no school-of-destination information is available. Regression discontinuity estimates of the upper and lower bound effects of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on the share of the grade peers in the school of destination who committed crime in the period 2016-2021. All crimes rate accounts for severe and not-severe crimes. Severe crimes include homicides, rapes, aggravated assaults, and recidivism. Not-severe crimes include assaults, property damage, thefts, and drug-related offenses. Re-enrollment rate in treatment and control group is 70.09% and 65.56%, respectively, for a difference in re-enrollment of 4.53 pp in favor of treatments. For estimating upper bound effects, we follow Lee (2009) and trim the treatment group observations that are in the lower end of the school-of-destination outcome distribution such that re-enrollment rate is the same across treatment and control groups. Likewise, for estimating lower bound effects, we trim observations in the treatment group that are in the upper end of the school-of-destination outcome distribution. For dummy outcomes, trimming is through random assignment. 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, a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year) or not, and 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.

Table A.XVIII: Intensive Margin Effects on Schooling: Peers-in-School-of-Destination Attributes

	Academic Performance and Socio-demographics of peers in School-of-Destination										School-of-Destination's Instruction Time				School-of-Destination's Modality Program	
	Mean SIMCE (Z)		School Attendance Rate		School Graduation Rate		Mean Age-Grade Lag		Share of Parents with Secondary Educ.		If Full Day School Schedule (JEC)		If Evening School Schedule		If Adult School Modality	
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)	OLS (10)	OLS (11)	OLS (12)	OLS (13)	OLS (14)	OLS (15)	OLS (16)
Upper: W.D. $\leq$ May30	0.035		0.022*		0.026*		-0.145		0.045**		0.111**		-0.031		-0.073*	
	(0.045)		(0.013)		(0.015)		(0.091)		(0.019)		(0.056)		(0.033)		(0.042)	
Lower: W.D. $\leq$ May30		-0.018		0.002		0.009		-0.262***		0.002		0.024		-0.081***		-0.093**
		(0.039)		(0.015)		(0.016)		(0.082)		(0.019)		(0.060)		(0.026)		(0.037)
Observations	1,924	1,921	1,921	1,919	1,921	1,920	1,920	1,922	1,919	1,920	1,922	1,919	1,923	1,925	1,922	1,922
Ineligible Mean	-0.302	-0.302	0.770	0.770	0.816	0.816	-0.474	-0.474	0.570	0.570	0.752	0.752	0.076	0.076	0.126	0.126
Optimal Bandwidth	12	12	12	12	12	12	12	12	12	12	12	12	12	12	12	12
Polynomial Degree	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-the-week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grad. in 2021 (= 1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

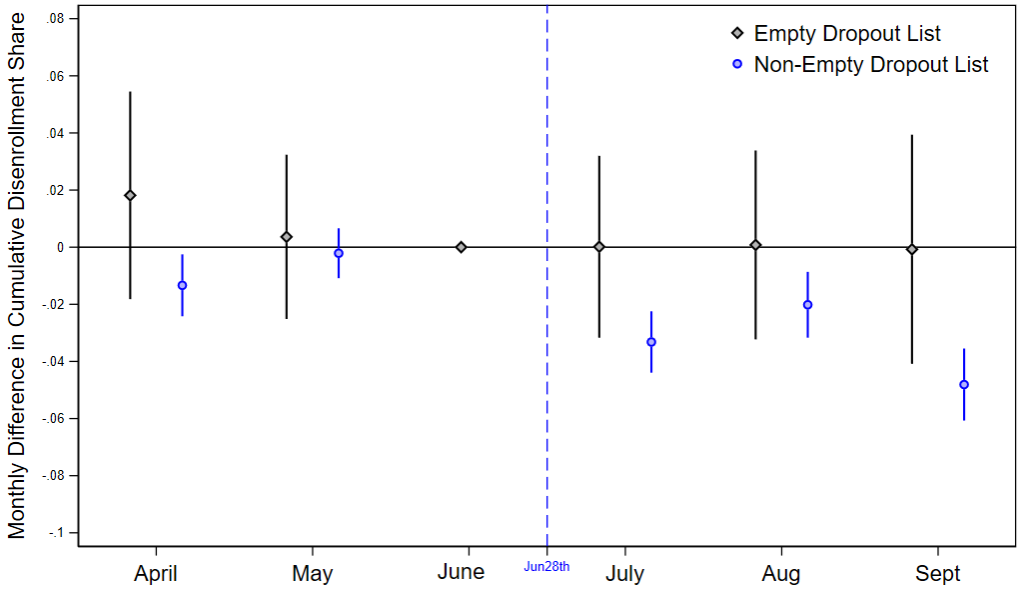
Note: Sample of analysis are evaluation sample students aged 14-17 who dropped out from "high frequency" dropout (two-sided) schools within the optimal bandwidth of $\pm$ 12 school days from the cutoff date May 30^{th}, 2022, and re-enrolled in the origin school or a different school (either regular or non-regular) by August 31^{st}, 2022. Dropouts re-enrolled in Free Exam Schools (<7%) are not considered as no school-of-destination information is available. Regression discontinuity estimates of the upper and lower bound effects of disenrolling before the cutoff date May 30^{th}, 2022 ($B_{isd} = 1$) on the average characteristics of the grade peers in the school of destination. Each regression outcome indicates the mean of the variable across grade peers in the school of destination where the dropout re-enrolled. Re-enrollment rate in treatment and control group is 70.09% and 65.56%, respectively, for a difference in re-enrollment of 4.53 pp in favor of treatments. For estimating upper bound effects, we follow Lee (2009) and trim the treatment group observations that are in the lower end of the school-of-destination outcome distribution such that re-enrollment rate is the same across treatment and control groups. Likewise, for estimating lower bound effects, we trim observations in the treatment group that are in the upper end of the school-of-destination outcome distribution. For dummy outcomes, trimming is through random assignment. 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, a dummy on whether the disenrolled student graduated in 2021 (pre-treatment year) or not, and 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.

Table A.XIX: 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	$ 9,600
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.	$ 72.14
Dropout Reports	Identification and listing of dropout students, and production and delivery of dropout reports.	$ 0.62
School Agency Costs	Expenditures schools incur in efforts to reintegrate listed 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.	$25.00
Unit Cost	SIGE + Dropout Reports + School Monitoring, per High-School dropout student	$ 97.76
Cost Effectiveness		
# 14-17 aged Dropouts in 2022	Number of 14-17 aged dropout students in High Dropout Schools in 2022	13,862
Total Implementation Costs	# 14-17 aged Dropouts in 2022 $\times$ per-student cost	$1,355,149
# 14-17 aged Dropouts in 2022 (would-be criminals)	Estimated number of 14-17 aged dropout students in High Dropout Schools in 2022 that would have committed crime had not been listed in the report.	1,056
Total (would-be) Crime Costs (Savings)	# 14-17 aged Dropouts in 2022 (would-be criminals) $\times$ per-crime cost	$10,137,600
Total Savings	Total (would-be) Crime Costs (Savings) - Total Implementation Costs	$ 8,782,451
Share of GDP in 2022 (%)	Total Savings divided by GDP in 2022	0.0029%
Return of Investment (ROI)	Total Savings divided by Total Implementation Costs	6.48

Note: Yearly costs in US dollars of 2022 (873.17 Chilean pesos per 1 USD) assuming only 14-17 aged dropout students (for which we observe crime) in high dropout schools (for which we do find crime effects) are listed in the report. The costs of crime are taken from Alfaro et al. (2024), which considers violent and non-violent "Crimes of High Social Connotation", 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.XIV: 2022-2016 Monthly Differences in Cumulative Disenrollment Share Ready



Notes: Unit of analysis are schools. Mean differences in the monthly cumulative disenrollment share of students aged 14-17 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 1,071 schools whose dropout report included a list of dropout students and 291 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[59].

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). LRPA processes severe crimes, including homicides, rapes, aggravated assaults, and recidivism, as well as non-severe crimes including assaults, property damage, thefts, and drug-related offenses. 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.

[59]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.

C. Online Appendix

C.1. 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 9-12th grade disenrolled students, who collectively contribute to two-thirds of the total dropout rate, and most of the upward trend in dropout rates in recent years are explained by high-school students (see Section 2 in the main text). 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.

In this section, we expand the analysis considering the full sample of dropout students regardless of age, and examine heterogeneous effects by school grade. Online Appendix Table A.I presents treatment effects across separate sub-samples of students in primary (1-4 grades), middle (5-8), and high-school (9-12), while Appendix Table A.II show the effects using the full sample all together[60]. 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. 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[61]. This is not 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[62].

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

[60] In order to check for internal validity, a balance test is performed in Appendix Table A.III, which shows that treatment and control groups are generally well balanced in a large number of education and sociodemographic characteristics.

[61] 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.

[62] 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.

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[63]. 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.

[63]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).

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

	If Re-enrolled by August 31st, 2022						If Graduated by December 31st, 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 30th, 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
1st 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: Sample of analysis are dropout students who dropped out from two-sided schools. Regression discontinuity estimates of the effect of withdrawing before the cutoff date (May 30th, 2022) on re-enrollment and graduation rates, separated by school grades (1-4; 5-8; 9-12). Optimal bandwidth is fixed at $\pm$ 14 school days, which is selected following MSE-optimal bandwidth choices using `rdbwselect` command in `Stata`. 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, grade dummies, GPA tercile dummies, school attendance's tercile dummies, a dummy if the student is classified as preferential, a dummy if the student is in high school, a dummy for whether the student committed crime, and crime severity dummies, as in main equation 1. Ineligible Mean is the mean outcome for students who disenrolled after May 30th, 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.II: Treatment Effects on Re-enrollment and Graduation – Full Sample

	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 dropout students who dropped out from two-sided schools. 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 (Calonico et al., 2014, 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 A.III: Internal Validity: Balance Checks – Full 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.