Rethinking Student Loan Design: Evidence from a Price-Based Reform in Chilean Higher Education*

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Abstract

This paper examines the effects of a major 2012 student loan reform in Chile that reduced interest rates from 6% to 2% and introduced more flexible repayment terms. Unlike studies of initial loan implementation, this reform offers a rare opportunity to examine how changes in the cost of borrowing affect enrollment decisions among already-eligible students. Using rich administrative data and a difference-in-differences design, we estimate the effects of the reform on immediate enrollment, second-year enrollment, and second-year dropout. To strengthen causal inference, we complement our strategy with a difference-in-discontinuities approach that leverages eligibility thresholds. We find a compositional shift in immediate enrollment: university enrollment increases by 2.5 percentage points, offset by an equal decline in vocational institutions, with no effect on overall enrollment. This shift persists into second-year outcomes, where university students exhibit slightly higher dropout and vocational students show improved persistence. These effects are concentrated among students from voucher schools and are absent among students from public schools, likely due to persistent academic and financial constraints. We also find that overall enrollment declines for female students, which may reflect greater risk aversion in response to uncertainty. These findings shed light on how price-based reforms to student loan programs can generate unequal responses across student groups and institutional sectors, offering valuable lessons for the design of equitable higher education financing.

JEL classification: I22, I24, I28

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1 Introduction

Access to tertiary education has expanded steadily in recent decades, yielding well-documented private and social benefits such as increased income, enhanced equality of opportunity, and broader economic growth (OECD, 2024; Ma and Pender, 2023; World Bank, 2018; Hill, Hoffman and Rex, 2005). However, in countries characterized by high tuition fees and limited public subsidies, this expansion has often been accompanied by rising levels of student debt, as loans serve as the primary

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mechanism for cost-sharing between students and the state (Looney and Yannelis, 2024; Garritzmann, 2016; Heller and Callender, 2013). In Chile, for instance, the state-guaranteed student loan program (*Crédito con Aval del Estado*, CAE) has become a focal point of public debate. In October 2024, the government announced a proposal to eliminate the CAE and introduce a new financing system, which includes partial debt forgiveness for existing borrowers and a shift toward income-based repayment terms (Alarcón and Brunner, 2025). Similarly, in the United States, outstanding student loan debt has surpassed $1.6 trillion, prompting discussions around debt relief and reform (Dinerstein et al., 2025; Dinerstein, Yannelis and Chen, 2024; Pew Research Center, 2024; Catherine and Yannelis, 2023). These developments underscore the growing attention to student loan systems and their long-term implications for borrowers and higher education financing.

The growing policy interest in student loan reform highlights the importance of empirical evidence on the consequences of changes to existing loan programs. While much of the literature has focused on the introduction of new financial aid schemes and their impact on outcomes such as enrollment, persistence, graduation, repayment, labor market trajectories, and even family formation (e.g., Dearden, 2019; Scott-Clayton and Zafar, 2019; Velez, Cominole and Bentz, 2019; Marx and Turner, 2019; Wiederspan, 2016; Rothstein and Rouse, 2011), fewer studies examine how changes to the terms of existing loan programs affect educational outcomes. This paper addresses this gap by analyzing a major 2012 reform to Chile's state-guaranteed loan program, which substantially reduced the interest rate and improved repayment conditions, lowering the cost of borrowing for all eligible students.

Although evaluating the reform's effect on repayment outcomes is an important policy question, we focus instead on how changes in the price of borrowing affect higher education decisions, particularly enrollment and persistence in the first two years. By making borrowing cheaper, the reform may have altered students' perceived affordability of tertiary education and, in turn, their enrollment choices. Whether there is an effect on educational attainment is ultimately an empirical question, since price reductions may elicit different behavioral responses across student populations and types of institutions. Our analysis shows that the reform did not increase overall immediate enrollment in higher education. Instead, it shifted students from vocational programs toward universities, with these differences persisting into the second year. We also find that the impacts vary by gender and by high school type, indicating heterogeneous responses across socioeconomic backgrounds and sexes. To identify these effects, we exploit variation in exposure to the reform across cohorts, together with eligibility criteria based on academic performance.

Taken together, our results contribute to the literature on the effects of financial aid on educational attainment in two main ways. First, while most prior research has focused on the impact of introducing new financial aid programs, such as expanding access to student loans, this paper evaluates a policy reform that altered the terms of existing student loans, specifically targeting repayment conditions rather than access. To the best of our knowledge, this is the first paper to provide causal evidence on how such changes to ongoing loan programs affect higher education decisions, particularly in a context where the primary policy objective was to improve repayment outcomes.

Related evidence is scarce. Mezza et al. (2020) and Black et al. (2023) analyze how changes in tuition or loan limits affect borrowing amounts, while a smaller set of studies examines borrowing costs, such as interest rates or repayment terms (e.g., Herbst, 2023; Sten-Gahmberg, 2020). Even then, much of this work relies on simulations or cross-country comparisons rather than ex post evaluations of discrete policy interventions (e.g., Abraham et al., 2020; Barr et al., 2019; Britton, van der Erve and Higgins, 2019; Armstrong et al., 2019; Chapman and Doris, 2019; Dearden et al., 2008). A few studies examine reforms to other types of financial aid (e.g., Dearden, Fitzsimons and Wyness, 2014; Nielsen, Sørensen and Taber, 2010; Dynarski, 2003), but none focus on changes to loan repayment conditions.

As Dynarski, Page and Scott-Clayton (2023) note in their literature review, even modest adjustments to financial aid design can influence educational choices, especially for present-biased students who weigh immediate costs more heavily than future benefits. The authors also emphasize the importance of examining how such changes may affect the distribution of enrollment across types of institutions or education sectors. Building on this insight, our second contribution is to document significant shifts in the composition of enrollment across higher education institutions following the reform, along with changes in policy coverage and gender inequality in access to tertiary education. By showing that adjustments to loan repayment conditions can reallocate students between universities and vocational institutions, we highlight a previously overlooked channel through which financial aid policies shape educational trajectories. These findings are particularly relevant for policymakers in settings where student loans remain a central mechanism for financing higher education and where reforms to repayment conditions are actively under discussion.

The Chilean case offers a valuable opportunity to empirically examine the effects of a reform that altered the cost of borrowing for student loans. In 2006, the Chilean government introduced a state-guaranteed loan program (CAE), which underwent a major policy reform in 2012. This reform introduced three key changes: (i) a reduction in the interest rate from an average of approximately 6 percent to a fixed rate of 2 percent, (ii) income-contingent repayments capped at 10 percent of income, and (iii) the option to postpone payments during periods of unemployment.[1] All three changes are relevant from a borrower's perspective, as they reduce the financial burden of repayment. The interest rate reduction stands out in particular, as it is applied automatically to all loans, whereas the income cap and payment deferral options become available upon request once borrowers enter repayment after completing tertiary education.

The institutional setting of Chile's higher education system (HES, hereinafter) provides a complementary reason why this context is particularly well-suited for studying student loan reforms. While sharing structural similarities with the U.S. system—such as a diverse mix of public and private institutions and widespread use of student loans—Chile features a centralized admission process based entirely on observable academic criteria, including high school GPA and scores from the national standardized test (PSU). This setup helps address common challenges to causal inference in the financial aid literature, which often arise from unobserved factors like parental alumni status or recommendation letters that influence admission decisions in other contexts (Riegg, 2008; Liu and Borden, 2019). In addition, Chile's standardized and centralized financial aid system facilitates access to detailed administrative data, making it possible to examine policy effects with greater precision. Prior research has leveraged this institutional structure to study the effects of loan availability on educational attainment and labor market outcomes (e.g., Aguirre, 2021; Bucarey, Contreras and Muñoz, 2020; Montoya, Noton and Solis, 2018; Solis, 2017; Rau, Rojas and Urzúa, 2013).

Building on this institutional setting, we use individual-level administrative data covering the full population of state-funded school graduates from 2006 to 2014, who faced immediate enrollment

[1] See Biblioteca del Congreso Nacional de Chile (2012) for details on the reform law.

decisions between 2007 and 2015. These records include detailed information on enrollment patterns, academic eligibility, and a set of individual and school characteristics. Compared to related studies, our setting offers several advantages: nationwide administrative coverage, large sample sizes that improve statistical precision, and data on multiple cohorts over time.

To evaluate the effects of the 2012 reform on short-term postsecondary outcomes, we use a difference-in-differences (DiD) empirical method. The treated group consists of eligible students from cohorts that were exposed to the reform, while the control group includes eligible students from cohorts that were not exposed. Eligibility is defined based on minimum high school GPA and national standardized test (PSU) scores. Our main outcomes are immediate enrollment, defined as enrollment in any higher education program during the year following high school graduation; second-year enrollment, defined as enrollment for two consecutive years after graduation; and second-year dropout, defined as enrollment in the first year after graduation but no enrollment in the subsequent year. We focus on these short-term outcomes because (i) the reform applied to both new and existing loans, making longer-term exposure partially ambiguous, and (ii) some vocational programs have short durations, so graduation would confound with continued enrollment. By concentrating on the first two years, we can cleanly measure behavioral responses to the reduction in borrowing costs and improved repayment conditions. In addition, we complement the DiD analysis with a difference-in-discontinuities (Diff-in-Disc) design, which uses a local comparison around eligibility thresholds to strengthen causal identification under alternative assumptions.

Our results show that the 2012 Chilean reform had no effect on overall immediate enrollment in higher education institutions, that is, enrollment in any program during the year immediately following high school graduation. However, we uncover a notable shift in institutional choice: university enrollment increased by 2.5 percentage points (pp.), representing a 7 percent rise relative to enrollment among nonexposed but eligible students. This increase was offset by an equivalent 2.5 pp. decline in vocational institution enrollment, a 14 percent decrease relative to the same comparison group. This reallocation effect remains stable over time, except for a temporary drop in 2015 coinciding with the announcement of a new tuition-free program. This shift can be interpreted as resulting from the implicit subsidy created by the reform for university education relative to vocational programs, which tend to be less expensive in both tuition and duration (Angrist et al., 2016).

These findings align with prior evidence on the enrollment effects of financial aid (e.g., Gurgand, Lorenceau and Mélonio, 2023; Carruthers and Welch, 2019; Park and Scott-Clayton, 2018; Fitzpatrick and Jones, 2016) and are consistent with previous studies of the Chilean loan system.[2] For instance, using regression discontinuity designs (RDD), Solis (2017) and Montoya, Noton and Solis (2018) find that loan eligibility increases immediate university enrollment by 18 pp. and 15.2 pp., respectively, although their estimates are local to individuals near the eligibility cutoff on the national admission test. Compared to those results, our estimated effects are smaller, which is expected given that we analyze changes in loan conditions rather than the expansion of loan access.

Regarding second-year outcomes, we find that the shift from vocational institutions to universities following the 2012 reform was accompanied by an increase in university enrollment in the second year of study. Specifically, second-year university enrollment rose by 2.1 pp., representing a 7 percent increase relative to nonexposed eligible students. This was accompanied by a 0.5 pp. increase in

[2] See also Fack and Grenet (2015), Cornwell, Mustard and Sridhar (2006), Perna and Titus (2004), and van der Klaauw (2002).

second-year dropout, equivalent to a 13 percent increase. In vocational institutions, we observe a modest 0.6 pp. decline in second-year enrollment and a more pronounced reduction of 1.5 pp. in second-year dropout, which corresponds to a 47 percent decrease relative to the baseline dropout rate of eligible nonexposed students. Overall, the increase in university continuation outweighs the decline in vocational enrollment, and the reduction in vocational dropout is not fully offset by the rise in university dropout. These findings underscore heterogeneous responses in both enrollment and persistence decisions across institutional types. While our analysis only focuses on immediate and second-year outcomes, due to complications in defining long-term treatment exposure and varying program durations, these institutional shifts may have important long-term implications given the well-documented returns to college selectivity, particularly for students at the margin of admission (e.g., Hoekstra, 2009; Zimmerman, 2014; Kirkeboen, Leuven and Mogstad, 2016; Hastings, Neilson and Zimmerman, 2014).

Evidence on the retention effects of financial aid from the international literature is mixed. While several studies highlight the importance of financial support in promoting student persistence and completion (e.g., Murphy and Wyness, 2023; Bietenbeck et al., 2023; Denning, 2019; Chatterjee and Ionescu, 2012; Glocker, 2011), other literature finds that aid can have unintended consequences depending on how it influences institutional choices (e.g., Cohodes and Goodman, 2014). For example, financial incentives may lead students to shift toward less selective institutions, which can reduce graduation rates and future earnings. Additional studies emphasize the role of academic preparation, student preferences, and other non-financial barriers to persistence and completion (e.g., Stinebrickner and Stinebrickner, 2012, 2008; Herzog, 2005). Our findings are consistent with Chilean evidence. For example, Card and Solis (2022) and Solis (2017) report increases of 16 to 20 pp. in two-year university enrollment following loan access. Our estimated effects are smaller, which is again consistent with the fact that we study adjustments to borrowing conditions rather than the introduction of loans, and our estimates apply to the full population of eligible students rather than those near an eligibility threshold.

As a robustness check, we apply the Diff-in-Disc design to both first- and second-year outcomes, comparing cohorts just above and below the PSU threshold before and after the reform. The resulting estimates are closely aligned with those from the main DiD analysis, reinforcing our findings on enrollment and retention and suggesting that the reform's effects are relatively homogeneous across the academic ability distribution.

Finally, we explore heterogeneity in the effects of the reform along two key dimensions: type of public-funded school, which proxies for socioeconomic background, and student sex. We find that the impacts on both enrollment and retention are concentrated among students from voucher high schools—typically associated with middle-income families—while no effects are detected for students from public schools, who tend to come from more disadvantaged backgrounds. This limited coverage may reflect two structural constraints: (i) the student loan does not cover full tuition or living expenses, which poses a greater barrier for lower-income students, and (ii) students from public schools systematically underperform on the national admission test, reducing their likelihood of meeting the eligibility criteria. These findings suggest that the 2012 reform was not substantial enough to affect the most disadvantaged students.

We also document gender differences in enrollment responses. For female students, the decline in vocational enrollment (–3.1 pp.) was not fully offset by a corresponding increase in university enrollment (2.2 pp.), leading to a net reduction in immediate enrollment (–0.8 pp.). By contrast, male students experienced a nearly one-to-one shift from vocational to university enrollment, with no change in total enrollment. This asymmetric response introduces a new layer of gender inequality in access to tertiary education. A possible explanation is that women may be more sensitive to uncertainty in educational returns or face additional structural constraints that influence their enrollment decisions.

In summary, this paper contributes to the understanding of how more generous loan terms—such as reductions in interest rates that lower the effective price of borrowing—can influence educational decisions in complex and heterogeneous ways. Beyond documenting enrollment and retention effects, our analysis highlights compositional shifts across institutions, differentiated impacts by gender, and limited effects for students from more disadvantaged backgrounds. These heterogeneous responses underscore the importance of anticipating behavioral reactions when designing or reforming financial aid systems. As many countries continue to revise student loan programs, understanding how seemingly neutral price adjustments affect different groups of students is essential for informed policymaking.

The remainder of the paper is organized as follows. Section 2 describes the Chilean higher education system, the 2012 loan reform, and our data. Section 3 presents the main empirical strategy. Section 4 reports the main results. Section 5 implements the complementary empirical method to assess robustness, and Section 6 examines heterogeneity. Section 7 concludes.

2 Background and Data

2.1 Institutional Setting

In Chile, students typically progress through one of three types of secondary schools: public schools, voucher-funded private schools, or fully private schools. Between 2007 and 2015, 39% of students graduated from public schools, 53% from voucher schools, and 8% from private schools. After completing secondary education, students may choose to pursue higher education, which in Chile comprises two main types of institutions: universities and vocational institutions (*Institutos Profesionales* and *Centros de Formación Técnica*). Universities offer professional programs and are the only institutions authorized to confer academic degrees, typically lasting five to six years, while vocational institutions provide technical programs, usually lasting three to four years. Admission to both types of institutions is centralized and based on observable academic criteria, including high school GPA and scores from the national standardized test (PSU, *Prueba de Selección Universitaria*).

Institutions in the Chilean Higher Education System (HES) are primarily funded through tuition fees, with public funding concentrated in universities via direct and indirect subsidies. Tuition fees represent a substantial financial burden for prospective students. During these years, the average annual tuition fee at the 62 universities was approximately CLP $2.1 million (USD $2,970), equivalent to 41% of the 2015 median family income.[3] For the more than 100 vocational institutions, the average annual tuition fee was CLP $1.1 million (USD $1,556), or 21% of median family income. This financial burden is particularly significant for students from public and voucher schools, which

[3]Median family income is calculated using the 2015 *CASEN* household survey. USD conversions use the official exchange rate as of 12/31/2015.

serve mostly students from lower- and middle-income families.

Financial options for students to cover higher education expenses are limited. Work-and-study or work-and-save strategies are often unfeasible due to time constraints and low earning potential, and access to commercial credit is restricted by formal employment and income requirements. As a result, government-sponsored financial aid, comprising loans and scholarships, serves as the primary funding source. In 2015, 62% of higher education students received government aid, with 34% holding a scholarship and 38% a loan. Scholarships typically cover tuition and, in some cases, enrollment and living expenses. Student loans, by contrast, are limited to tuition payments and only cover costs up to a program-specific ceiling, known as the "referential tuition fee," which is set annually by the Ministry of Education based on program quality.

There are two main student loan programs in Chile: the traditional university loan (FSCU, *Fondo Solidario de Crédito Universitario*) and the state-guaranteed loan (CAE, *Crédito con Aval del Estado*). The FSCU loan is available only to students enrolled at the 27 traditional universities (those established before 1980). It carries a 2% annual interest rate and features income-contingent repayments beginning two years after graduation, capped at 15 years and 5% of annual income. The CAE loan, by contrast, is available to students at all accredited institutions. It is financed by private banks and jointly guaranteed by the state and the institution of enrollment. The CAE is by far the largest financial aid instrument in Chile: in 2015, one in three higher education students held a CAE loan, accounting for 83% of all student loans. Its terms changed substantially with the 2012 reform, making it central to our analysis.

2.2 The CAE Loan and the 2012 Reform

Introduced in 2006, the CAE loan aimed to expand access to higher education regardless of institutional type. The loan system involves (i) private banks that disburse funds, (ii) the government and institutions that serve as guarantors against default and dropout, and (iii) students who borrow and repay.

Students begin the CAE loan process during the standardized national college application cycle. First, they register for the PSU and submit a socioeconomic form, which is used to assess income eligibility for financial aid.[4] Once PSU results are released and academic eligibility is established, students who meet the requirements may apply for the CAE loan. The loan is disbursed only if the student subsequently enrolls in an eligible institution.

To qualify for a CAE loan, applicants must meet both academic and income criteria. Academic eligibility depends on the type of institution the student wishes to attend. For university enrollment, students must score at least 475 points on the PSU. For vocational institutions, students may qualify either by meeting the same PSU threshold or by having a high school GPA of at least 5.3 on a 1–7 scale. Although income eligibility initially restricted access to students from the bottom four income quintiles (as in 2007), this requirement was rapidly relaxed and fully eliminated by 2014. Since then, loans have been granted based solely on academic performance.

[4] The PSU included two mandatory components (language and mathematics) and two optional components (science or history/social science). Scores ranged from 150 to 850, with a mean of 500 and a standard deviation of 110. The PSU was administered until 2020 and was later replaced in 2022 by the PAES (*Prueba de Acceso a la Educación Superior*), a new standardized national test used for admission to postsecondary education.

Before 2012, CAE loans closely resembled commercial loans: they carried market interest rates (averaging 5.6%), lacked income-contingent repayment mechanisms, and allowed banks to pursue standard collection practices. Repayment began 18 months after graduation, with a term of up to 20 years. In mid-2011, the government announced a major reform to the CAE loan system, which took effect in 2012. The reform was introduced in response to concerns about high levels of loan delinquency, with default rates exceeding 35 percent and projections suggesting they could surpass 50 percent (World Bank, 2011).[5] The reform introduced three key changes: (i) a fixed annual interest rate of 2%, subsidized by the state, (ii) an optional income-contingent repayment cap of 10%, with the state covering the difference, and (iii) an option to delay repayment during unemployment. These changes substantially reduced the cost of borrowing, particularly due to the automatic application of the lower interest rate. The income-contingent and deferment options require explicit requests and are less commonly used: by 2015, only 8% of CAE borrowers had activated the 10% repayment cap and 4% had requested deferment (Ingresa, 2015).

From a policy perspective, this reform constituted a reduction in the price of student loans, specifically, a decline in the present value of future repayment obligations. For example, a student borrowing CLP $2.1 million annually over a 6-year program would have owed CLP $15.7 million at a 5.6% interest rate by the end of the grace period. With a 20-year repayment plan, this translates to an annual payment of CLP $1.3 million. Under the new 2% rate, the total debt falls to CLP $13.6 million, with an annual payment of CLP $0.8 million—a 37% reduction.[6]

This reform represents a shift in the cost structure of existing credit—rather than expanding access to new groups (as occurred with the 2006 introduction of the CAE loan)—and thus can be interpreted as a price-based policy shock. It is important to note that the 2012 reform did not alter the academic eligibility rules for obtaining a CAE loan, which remained defined by the same PSU and GPA thresholds. Consequently, the reform operated solely through a reduction in borrowing costs for students who already qualified under the pre-existing rules. In our analysis, we define treatment based on exposure and academic eligibility while estimating effects on overall enrollment, university enrollment, and vocational enrollment using the full sample of eligible students in each case. This approach ensures that loan eligibility criteria remains exogenous to the reform, isolating the causal impact of lower borrowing costs.

2.3 Data and Sample

Chile's highly centralized financial aid application process enables the use of nationwide administrative data covering the full population of students who graduated from state-founded schools and registered for the PSU immediately afterward, including their eligibility status and postsecondary enrollment decisions. We construct our dataset by merging information from three main sources.

First, we use administrative records from the *Departamento de Evaluación, Medición y Registro Educacional* (DEMRE), the institution responsible for administering the PSU. These records include all individuals who registered for the PSU, along with their standardized test scores. Second, we obtain data from the Ministry of Education on student characteristics and the high schools from which they just graduated. Third, we use individual-level enrollment records from the Ministry of Education, which document matriculation in all universities and vocational institutions. By merging

[5] Similar default rates have been predicted in the U.S. (Scott-Clayton, 2018).

[6] This example assumes fixed annual borrowing, no inflation, and no use of repayment caps or deferments.

these three data sources through a unique individual identifier, we construct repeated cross-sectional cohorts of high school graduates, including detailed information on their academic eligibility and postsecondary enrollment trajectories.

Our analysis focuses on the 2007–2015 cohorts, that is, students who completed high school between 2006 and 2014. We exclude the 2006 cohort because the CAE loan program was misallocated during its inaugural year due to a government error in sorting applicants by income, resulting in aid being distributed in reverse order (Ingresa, 2010). We also exclude cohorts after 2015 because a major policy change in 2016 introduced tuition-free higher education for a subset of students. This reform fundamentally altered the financial aid landscape and could confound our analysis of the 2012 CAE loan reform.

3 Empirical Strategy

To estimate the causal effect of the 2012 reform to the CAE loan program, we exploit its timing and academic eligibility rules through a difference-in-differences (DiD) research design. The reform reduced the cost of borrowing by lowering the interest rate and improving repayment conditions, thereby increasing the net present value of investing in higher education. In the presence of credit constraints, such financial aid reforms are expected to influence enrollment decisions by relaxing liquidity constraints and improving returns to education (Becker, 1964; Long and Riley, 2007; Johnson, 2013). We compare outcomes before and after the reform across groups defined by eligibility, thereby isolating the effect of the reform from other time-varying confounders.

Eligibility for the CAE loan is determined by academic performance. Students are considered eligible if they meet one of two criteria: (i) a score above 475 on the PSU standardized test, or (ii) for applicants to vocational institutions only, a high school GPA greater than 5.3 on a 1-to-7 scale. Our treatment group consists of students who were both academically eligible for the loan and exposed to the reform, that is, individuals who made their enrollment decisions between 2012 and 2015. The control group comprises eligible students from earlier cohorts (2007–2011), whose enrollment decisions occurred before the reform. To further account for time-varying factors unrelated to the policy change, we also compare ineligible students across the same cohort groups. Since these individuals were not eligible for the loan, their enrollment patterns provide a benchmark to net out confounding trends affecting all students over time. Importantly, the academic eligibility rules remained unchanged throughout the reform, ensuring that exposure and eligibility criteria are exogenous to the policy change.

To estimate the average effect of the reform, we implement a standard DiD model using repeated cross-sectional data, where each individual appears only once. The treatment occurs at a single point in time, year 2012, and ineligible students serve as a reference group that is never treated. This specification compares changes in outcomes over time between eligible and ineligible students, attributing any differential change post-2012 to the effect of the reform. Our identification assumption is that, absent the reform, the difference in unobserved determinants of the outcomes between eligible and ineligible students would have remained constant over time. In other words, the average remaining difference in unobservables is assumed to be stable before and after the policy change. We assess the plausibility of this assumption through a pre-trend test in an event study specification presented later in this section.

Our base DiD model is:

$$y_{it} = \beta_0 + \beta_1 \, \text{eligible}_{it} + \beta_2 \, \text{exposed}_{it} + \beta_3 \, \text{eligible}_{it} \times \text{exposed}_{it} + \varepsilon_{it} \tag{1}$$

where y_{it} denotes the outcome for student i in cohort t; eligible_{it} is a binary variable indicating whether the student satisfies the academic criteria for loan eligibility; exposed_{it} indicates exposure to the reform, taking value 1 for cohorts 2012 and onward (2011 and onward for second-year outcomes); and the interaction term captures the average treatment effect on the treated (ATT).

To investigate the dynamics of the reform's effects and assess the plausibility of the parallel trends assumption, we also estimate an event study model:

$$y_{it} = \beta_0 + \beta_1 \, \text{eligible}_{it} + \sum_{j=2007}^{2015} \alpha_j \, \text{cohort}_{jit} + \sum_{j=2007}^{2015} \beta_j \, \text{eligible}_{it} \times \text{cohort}_{jit} + \varepsilon_{it} \tag{2}$$

where cohort_{jit} is a full set of cohort indicators and the reference year is set to 2011. The coefficients β_j for years before 2012 serve as a test for pre-trends, while those for 2012 onward capture the evolution of the treatment effect.

We examine three binary outcomes—immediate enrollment, second-year enrollment, and second-year dropout—and for each, we define three levels: (i) the entire higher education system (HES), (ii) universities, and (iii) vocational institutions. All specifications use the full sample, as we do not restrict the analysis to students who applied to or enrolled in a given institution type. Thus, the level-specific outcomes are defined over the entire population as follows:

Immediate Enrollment (HES/University/Vocational). Our primary outcome is immediate enrollment, defined as whether a student enters a postsecondary institution in the year directly following high school graduation. *HES* equals 1 if the student enrolls in any institution and 0 otherwise. *University* (resp. *Vocational*) equals 1 if the student enrolls in a university (resp. vocational institution) and 0 otherwise.

Second-year Enrollment (HES/University/Vocational). To capture student retention, reflecting both the initial enrollment and continuation decisions, we define second-year enrollment as a binary variable indicating whether the student is observed enrolled for two consecutive years following high school graduation. *HES* equals 1 if the student is observed in any program for two consecutive years (system-level persistence) and 0 otherwise. *University* (resp. *Vocational*) equals 1 if the student is enrolled in a university (resp. vocational institution) in both consecutive years (type-specific persistence) and 0 otherwise.

Second-Year Dropout (HES/University/Vocational). Our final outcome captures attrition between the first and second years of postsecondary education, providing insight into early persistence failures. *HES* equals 1 if the student enrolls in the year immediately following high school graduation but does not enroll in any HES program in the subsequent year (system-level attrition) and 0 otherwise. *University* (resp. *Vocational*) equals 1 if the student enrolls in a university (resp. vocational institution) in the first year after graduation but is not enrolled in that same institution type in the second year (type-specific attrition) and 0 otherwise.

We estimate Equations (1) and (2) for each of the three outcomes and at each level of the education system, using the full sample in all cases. The timing of exposure is adjusted accordingly. For immediate enrollment, the exposure period corresponds to cohorts 2012–2015. For second-year outcomes, it corresponds to cohorts 2011–2015, as students enrolling in 2011 made their second-year decision under the new loan conditions. As a robustness check, we also extend our baseline models to include cohort fixed effects and additional student and high-school controls.

We focus on short-term outcomes for two main reasons. First, the timing of the 2012 reform creates ambiguities in measuring longer-term outcomes. While immediate enrollment clearly occurs before or after the reform, second-year outcomes already involve some partial exposure: students in the 2011 cohort made their first-year enrollment decision in 2011 (pre-reform) but faced second-year decisions in 2012 (post-reform). In our baseline analysis, we treat the 2011 cohort as exposed for second-year outcomes, and Appendix B presents a robustness check where cohort 2011 is defined as "half-exposed", cohorts 2007–2010 as unexposed, and cohorts 2012–2015 as fully exposed. Extending the analysis to third-year or later outcomes would exacerbate these ambiguities: different cohorts would face a mix of pre- and post-reform years, making exposure inconsistent and complicating the interpretation of causal effects. Second, heterogeneity in program duration complicates longer-term outcomes. Universities and vocational institutions differ in length, with some vocational programs lasting only two years. Analyzing third-year enrollment or dropout could misclassify students who completed a shorter program as non-enrolled or as dropouts, biasing the estimates. This problem becomes even more pronounced for fourth- or fifth-year outcomes. Focusing on first- and second-year outcomes avoids these issues, providing a cleaner measure of students' behavioral responses to lower borrowing costs and improved repayment conditions.

4 Main Results

This section presents and discusses the main results. To provide context and enhance understanding of the Chilean setting, Table 1 displays descriptive statistics for exposed and unexposed cohorts, distinguishing between eligible and ineligible students. The sample comprises approximately 1.5 million high school graduates, 3.3% of whom attended rural schools. Regarding school type, 39.4% graduated from public schools, while the remaining 60.6% attended voucher schools. The overall female-to-male ratio is 1.14. By definition, eligible students have higher PSU scores and GPAs, whereas academic performance does not differ significantly between exposed and unexposed cohorts.

In terms of educational attainment, half of the high school graduates in our sample immediately enroll in the higher education system (HES), with 29.5% entering universities and 21.1% enrolling in vocational institutions. Over time, overall enrollment has increased, driven primarily by a rise in vocational institution enrollment, while university enrollment has remained stable. This trend is reflected in the higher vocational enrollment rates, which drive the overall increase, among the 2012–2015 cohorts (i.e., exposed) compared to the 2007–2011 cohorts (i.e., unexposed). As expected, eligible students have higher enrollment rates than ineligible students, except in vocational institutions, where enrollment rates are more balanced across both groups.

Regarding persistence in the HES, 43.8% of our sample remains enrolled for two consecutive years, while 6.7% exits the system after the first year. In other words, conditional on enrolling for one year, the retention rate is 87% (0.438/0.505), and the dropout rate is 13% (0.067/0.505). Among university students, the retention rate is 88% (0.259/0.295), whereas in vocational institutions, it is 78% (0.164/0.211). Eligible students exhibit higher retention rates than ineligible students (89% vs. 70%). Meanwhile, dropout rates remain similar between exposed and unexposed cohorts.

Table 1: Descriptive Statistics

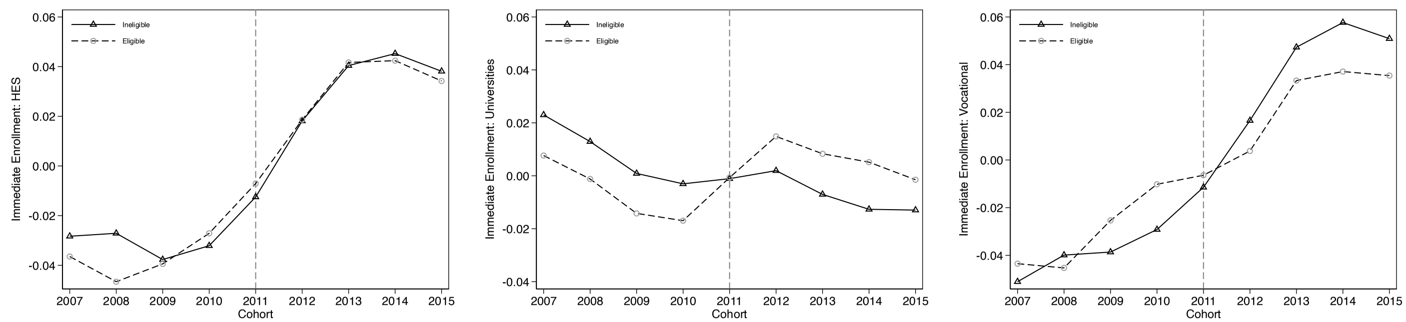
	All Students	Unexposed		Exposed	
		Ineligible	Eligible	Ineligible	Eligible
	(1)	(2)	(3)	(4)	(5)
Immediate Enrollment	0.505	0.275	0.533	0.337	0.597
by Institution					
Universities	0.295	0.066	0.356	0.053	0.368
Vocational	0.211	0.210	0.177	0.285	0.230
Second-Year Enrollment	0.438	0.192	0.470	0.228	0.526
by Institution					
Universities	0.259	0.044	0.316	0.034	0.326
Vocational	0.164	0.137	0.140	0.184	0.182
Second-Year Dropout	0.067	0.078	0.056	0.099	0.063
by Institution					
Universities	0.035	0.023	0.038	0.020	0.040
Vocational	0.047	0.067	0.032	0.089	0.041
PSU	476.263	391.620	496.094	392.313	494.395
GPA	5.605	4.939	5.777	4.946	5.814
Attendance	90.517	89.249	92.092	86.761	90.159
Female	0.533	0.467	0.555	0.454	0.552
Public School	0.394	0.454	0.411	0.398	0.354
Rural School	0.033	0.045	0.032	0.039	0.029
Observations	1,497,379	186,801	620,206	145,041	545,331

Notes: The exposure period for first-year outcomes, control variables and observtions includes cohorts from 2012 to 2015, while second-year outcomes include cohorts from 2011 to 2015.

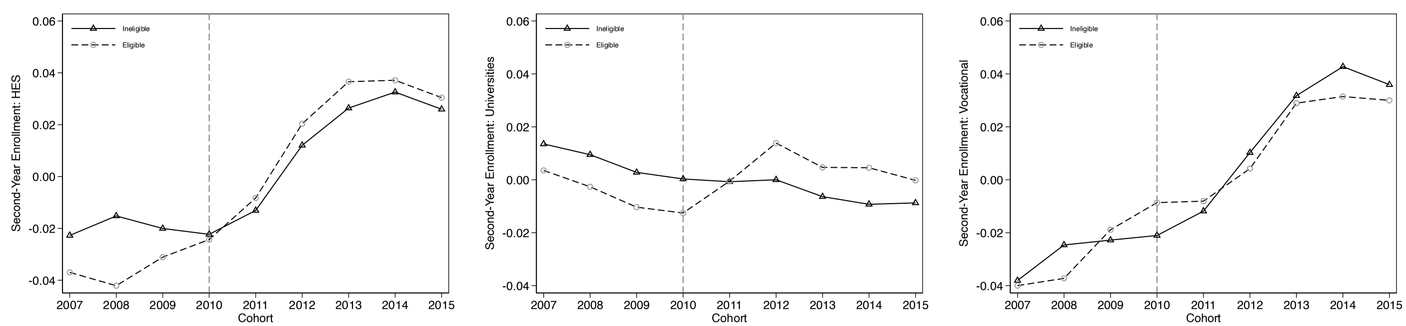
Complementing these descriptive patterns, Figure 1 presents a visual inspection of annual trends for our nine outcomes: immediate enrollment, second-year enrollment, and second-year dropout, each disaggregated by institution type (any HES institution, universities, and vocational institutions) and eligibility status. Panels A, B, and C show the corresponding time series. Prior to the 2012 reform, the trajectories for eligible and ineligible students evolve in a reasonably similar fashion, providing visual support for the parallel trends assumption that underlies our DiD design, while post-reform years display the divergence in outcomes that we analyze below.

Building on this descriptive and graphical evidence, the following subsections present the estimation results of the models discussed in the previous section. All regressions follow a linear probability model with standard errors clustered at the class level to account for intraclass correlation, where a class is defined as the cohort graduating from a specific high school in a given year. To contextualize the magnitude of our estimates, most tables report the number of unexposed eligible individuals and their mean outcome. As a robustness check for our main specification, we extend the base models by incorporating cohort effects and two sets of control variables. Student-level controls include gender, attendance rate, and municipality, while school-level controls account for school type (public or voucher) and rural location.

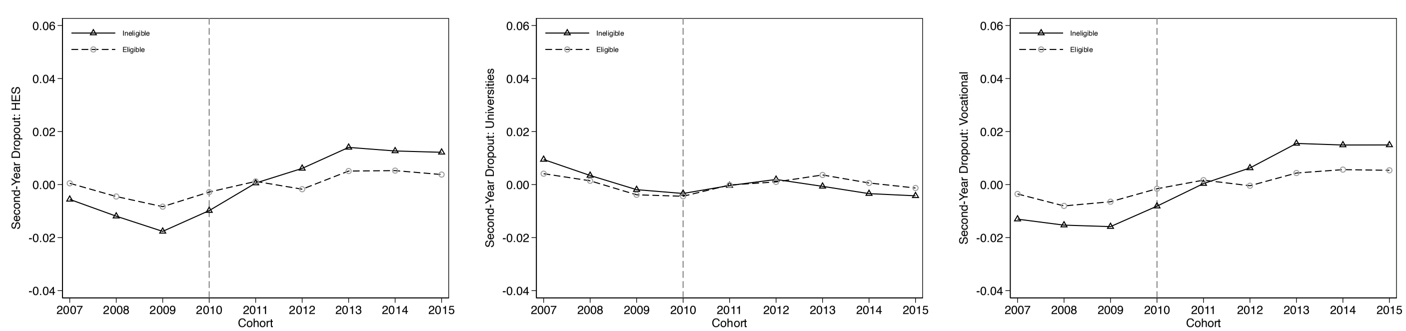
Figure 1: De-meaned Outcomes over Time by Eligibility



Panel A: Immediate Enrollment



Panel B: Second-Year Enrollment



Panel C: Second-Year Dropout

Notes: Time trends of de-meaned immediate enrollment, second-year enrollment, and second-year dropout for eligible and ineligible individuals are shown in panels A, B, and C, respectively. Within each panel, the left sub-panel represents the overall HES, the center sub-panel corresponds to universities, and the right sub-panel to vocational institutions.

4.1 Effects on Immediate Enrollment

Table 2 presents the results for our three immediate enrollment variables: overall, university, and vocational institution enrollment. Columns (1), (4), and (7) report estimates from the base model specified in Equation (1). Columns (2), (5), and (8) incorporate cohort fixed effects, while Columns (3), (6), and (9) further include student- and high school-level control variables.

Eligible students are more likely to enroll in higher education. This is not solely due to the availability of CAE but also because they may qualify for other grants and/or the FSCU loan. Moreover, since eligibility is determined by academic performance—arguably linked to ability—the results suggest that higher-ability students are more likely to enroll. However, when disaggregating by institution type, we find that this result is driven by university enrollment: eligible students are more likely to enroll in universities but slightly less likely to enroll in vocational institutions. This may reflect higher economic returns from college degrees or align with a Roy model of comparative advantage. The coefficient on the *exposed* variable captures the trend in enrollment over time, as previously discussed.

Table 2: Immediate Enrollment

	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × exposed	0.002	0.002	0.000	0.025***	0.026***	0.024***	-0.023***	-0.024***	-0.024***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Exposed	0.062***	0.068***	0.075***	-0.013***	-0.035***	-0.032***	0.075***	0.103***	0.107***
	(0.003)	(0.007)	(0.007)	(0.001)	(0.007)	(0.007)	(0.003)	(0.004)	(0.004)
Eligible	0.258***	0.258***	0.240***	0.290***	0.290***	0.271***	-0.032***	-0.032***	-0.031***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379
Control group size	620,206	620,206	620,206	620,206	620,206	620,206	620,206	620,206	620,206
Outcome mean	0.533	0.533	0.533	0.356	0.356	0.356	0.177	0.177	0.177

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. School-level controls include indicators for school type and rural location, while student-level controls include gender, attendance rate, and municipality. Control group size and outcome mean are presented for eligible students not exposed to the reform.

The overall enrollment effect of the reform is neither statistically nor economically significant, suggesting that reducing the price of loans (through lower interest rates and better repayment conditions) had no impact on immediate enrollment. However, a substitution effect emerges when analyzing enrollment by institution type: the reform increased university enrollment at the expense of vocational institutions by 2.5 percentage points (pp.). In absolute terms, this represents a shift of 15,500 students (out of 620,206) from vocational institutions to universities. This result is robust to the inclusion of different sets of covariates and corresponds to an approximate 7 percent increase in university enrollment and a 14 percent decrease in vocational enrollment relative to the enrollment rate of unexposed eligible individuals.

Our results align with prior literature but are smaller in magnitude. Using an RDD, Solis (2017) examines cohorts from 2007 to 2009 to estimate the effects of crossing the 475 PSU-score threshold, which enables loan eligibility, and finds that immediate university enrollment increases by 18 pp.—nearly doubling relative to ineligible students. Similarly, using the same RDD approach and cohorts, Montoya, Noton and Solis (2018) analyze labor market effects and also estimate the impact on different enrollment measures. Their findings indicate that scoring above the 475 cutoff increases overall immediate enrollment by 9.6 pp. and university enrollment by 15.2 pp., attributing much of this change to a shift from vocational institutions to universities. Likewise, Bucarey, Contreras and Muñoz (2020) identify a compositional effect associated with loan access, as students substitute enrollment in high-quality vocational institutions for lower-quality universities.

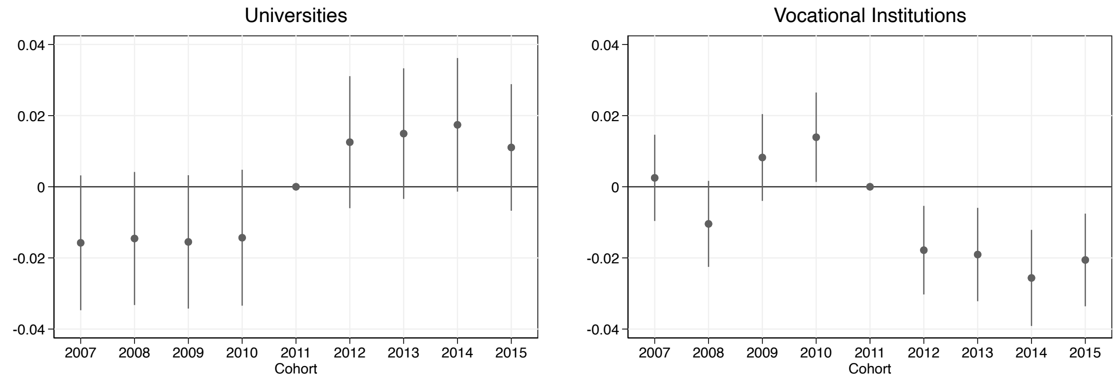
In contrast, Aguirre (2021) finds that access to loans in addition to grants for vocational education increases the likelihood of enrolling in a vocational institution while decreasing university enrollment in the short run. These seemingly contradictory results can be reconciled by noting that the running variable in Aguirre's RDD is the student's GPA, where crossing the 5.3 threshold grants CAE eligibility exclusively for vocational institutions. In comparison, the previously mentioned studies focus on the PSU-score threshold of 475, which determines CAE eligibility for both universities and vocational institutions. Our study considers eligibility based on both PSU and GPA criteria.

Our analysis differs from the existing literature in two key ways. First, we examine a reform that modified the price of existing loans through an interest rate reduction and new repayment conditions, whereas prior studies analyze the effects of gaining access to the CAE loan itself. Second, in the RDD framework, estimates capture local treatment effects for individuals near the eligibility threshold (i.e., those with a PSU score close to 475), whereas our results reflect the average impact on treated individuals.

The shift in institutional choice from vocational institutions to universities can be explained by the implicit subsidy created by the loan price reduction (Angrist et al., 2016). Since university programs are more costly both in monetary terms (i.e., tuition fees) and time commitment (i.e., program length), the financial effect of the price change is relatively larger for university enrollment, thereby increasing the incentive to choose a university over a vocational institution.

Figure 2 presents the event study estimates of immediate enrollment effects, displaying the β_j interaction coefficients (i.e., $\text{eligible}_{it} \times \text{cohort}_{jit}$) described in Equation (2), along with their corresponding 95% confidence intervals. Detailed estimation results and robustness checks are provided in Appendix A. The left panel illustrates the evolution of the effects on university enrollment, while the right panel shows the corresponding effects on vocational enrollment. In both cases, we observe a sharp shift in the β_j coefficients following the 2012 reform: university enrollment increases, while vocational enrollment decreases. These effects remain stable over time, with a slight decline in magnitude in 2015 when the tuition-free program was announced for implementation in 2016. Additionally, the estimated interaction coefficients for cohorts from 2007 to 2010 serve as a rigorous test for differential pre-trends. For pre-reform years, we cannot reject $\beta_j = 0$ for either university or vocational enrollment.

Figure 2: Event Study for Immediate Enrollment



Notes: Point estimates of the β_j coefficients from Equation (2), along with their 95% confidence intervals. The base category is the 2011 cohort. The displayed estimates correspond to the baseline specification without covariates. The left panel presents results for immediate enrollment in universities, while the right panel shows results for immediate enrollment in vocational institutions.

4.2 Effects on Second-year Enrollment and Dropout

We next examine the effects of the reform on second-year outcomes in tertiary education, as captured by our second-year enrollment and second-year dropout variables. Table 3 presents the corresponding estimation results: the upper panel reports coefficients on second-year enrollment, while the lower panel reports coefficients on second-year dropout. In this case, the exposed cohorts span from 2011 to 2015, since students enrolling in 2011 may have made their second-year decisions under the new interest rate policy introduced in 2012. To account for this partial exposure, Appendix B reports estimation results that treat the 2011 cohort separately. As in the previous analysis, Columns (1), (4), and (7) show estimates from the baseline model described in Equation (1), while Columns (2), (5), and (8) add cohort fixed effects. Columns (3), (6), and (9) include additional student- and school-level control variables.

Consistent with the descriptive statistics, eligible students are more likely to remain enrolled for two consecutive years and less likely to drop out in their second year compared to ineligible students. Over time, both second-year enrollment and second-year dropout have increased, though the rise in dropout is relatively modest. The interaction coefficients indicate that the 2012 reform to the CAE loan increased second-year enrollment by 1.7–2.0 pp. and reduced second-year dropout by 1.3–1.4 pp. These effects correspond to a 4 percent increase in second-year enrollment and a 24 percent reduction in second-year dropout, relative to the respective means for unexposed eligible students.

A closer examination reveals that the loan price reduction led to a 2.1 pp. increase in university second-year enrollment—equivalent to a 7 percent rise—as well as a 0.5 pp. increase in second-year dropout, corresponding to a 13 percent rise. This suggests that the reform-driven increase in immediate university enrollment was accompanied by gains in student retention over two years, albeit with a modest increase in dropout. In contrast, vocational institutions experienced a slight decline of 0.6 pp. (4 percent, significant at the 5% level) in second-year enrollment, alongside a more substantial decrease of 1.5 pp. (47 percent) in second-year dropout. Thus, the decline in vocational enrollment following the reform is reflected in lower rates for both second-year outcomes, particularly a sharp reduction in dropout.

Comparing both types of institutions, we find that the increase in university second-year enrollment more than offsets the decline observed in vocational institutions. In addition, the reduction in second-year dropout among vocational students is not counterbalanced by the increase in dropout among university students. These results illustrate how modifications to existing loan schemes (e.g., changes to interest rates or repayment conditions) affect both access and persistence in tertiary education. The CAE reform not only induced a shift from vocational to university enrollment but also had heterogeneous effects on second-year persistence across institution types.

Table 3: Second-Year Enrollment and Dropout

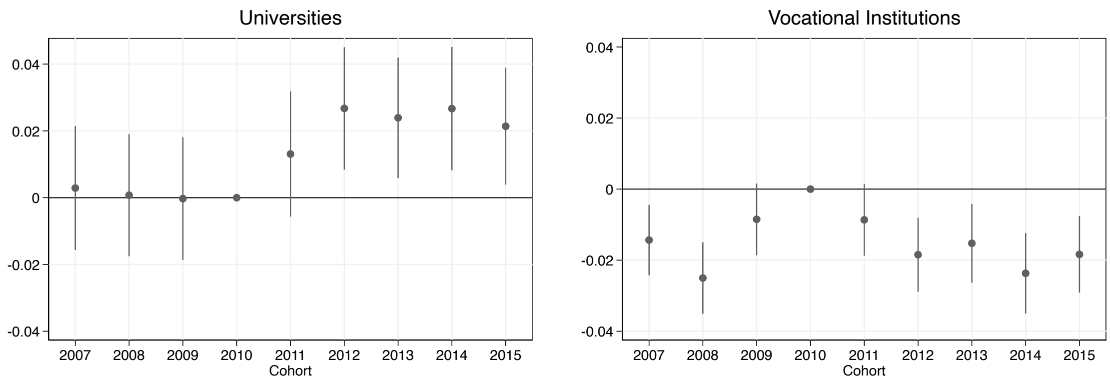
	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Second-Year Enrollment									
Eligible × exposed (2nd year)	0.020***	0.019***	0.017***	0.021***	0.021***	0.020***	-0.004	-0.005**	-0.006**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Exposed (2nd year)	0.036***	0.048***	0.057***	-0.011***	-0.024***	-0.020***	0.047***	0.075***	0.080***
	(0.003)	(0.007)	(0.006)	(0.001)	(0.007)	(0.007)	(0.002)	(0.004)	(0.003)
Eligible	0.278***	0.278***	0.255***	0.272***	0.272***	0.251***	0.003	0.003	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)
Outcome mean	0.470	0.470	0.470	0.316	0.316	0.316	0.140	0.140	0.140
Second-Year Dropout									
Eligible × exposed (2nd year)	-0.013***	-0.014***	-0.013***	0.005***	0.005***	0.005***	-0.015***	-0.015***	-0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Exposed (2nd year)	0.020***	0.017***	0.015***	-0.003***	-0.011***	-0.012***	0.023***	0.025***	0.024***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Eligible	-0.022***	-0.022***	-0.018***	0.015***	0.015***	0.016***	-0.034***	-0.034***	-0.031***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Outcome mean	0.056	0.056	0.056	0.038	0.038	0.038	0.032	0.032	0.032
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379
Control group size	481,617	481,617	481,617	481,617	481,617	481,617	481,617	481,617	481,617

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. School-level controls include indicators for school type and rural location, while student-level controls include gender, attendance rate, and municipality. Control group size and outcome mean are presented for eligible students not exposed to the reform.

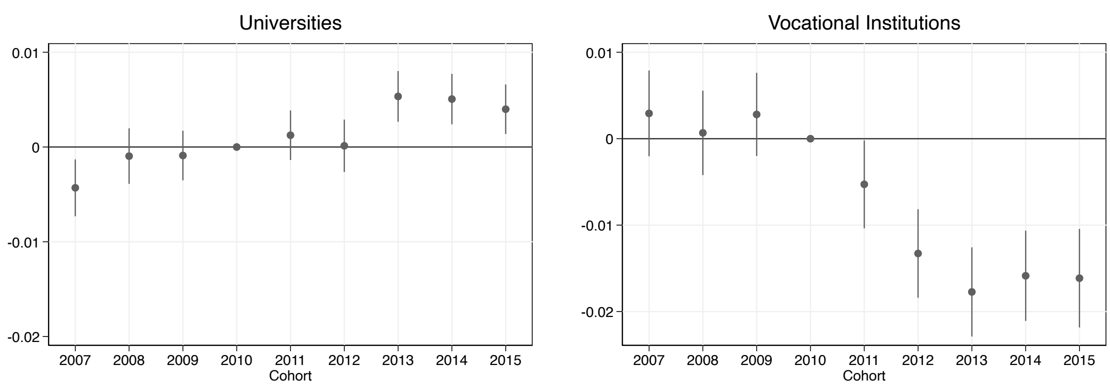
Figure 3 presents the event study estimates for our second-year outcomes, along with their corresponding 95% confidence intervals. The top panel displays the evolution of the effects on second-year enrollment, while the bottom panel does so for second-year dropout. In both cases, estimates for universities are shown in the left panels and for vocational institutions in the right panels. Among the 12 β_j interaction coefficients for $j \in 2007, 2008, 2009$, 9 are statistically indistinguishable from zero, providing strong support for the plausibility of the parallel trends assumption. See Appendix A for detailed estimation results and robustness checks for all our outcomes.

As previously discussed, the 2012 loan reform led to increases in both second-year enrollment and second-year dropout in universities, while both outcomes declined in vocational institutions. This further confirms that the reform influenced not only immediate enrollment but also student persistence in higher education. Finally, coefficients are generally stable throughout the exposure period, with the exception of the 2011 interaction terms, which are smaller in magnitude across all outcomes. This is expected, as the 2011 cohort was only partially exposed to the reform in terms of their second-year decisions.

Figure 3: Event Study for Persistence and Retention



(a) Second-year Enrollment



(b) Second-Year Dropout

Notes: Point estimates of the β_j coefficients from Equation (2), shown with their 95% confidence intervals. Cohort 2010 is the omitted (base) category. The estimates correspond to the baseline specification without covariates. Panel (a) presents the results for second-year enrollment: the left sub-panel displays estimates for universities, and the right sub-panel for vocational institutions. Panel (b) shows the results for second-year dropout, with the left and right sub-panels corresponding to universities and vocational institutions, respectively.

The observed effects on second-year enrollment and second-year dropout suggest that the loan price reduction helped promote persistence in higher education, particularly in universities. Financial factors are known to influence not only access but also continuation. By lowering borrowing costs, the reform likely reduced the perceived and actual burden of staying in the system beyond the first year, especially in universities, where programs are longer and more expensive. The increase in university second-year enrollment, together with the moderate rise in dropout, indicates that more students remained enrolled in the short run, even if some may have later discontinued. In vocational institutions, the reform led to a reduction in both second-year enrollment and second-year dropout, suggesting that those who chose to enroll were more likely to persist, potentially due to a better match between students and institutions. These results are consistent with the substitution effect documented by Montoya, Noton and Solis (2018) and Bucarey, Contreras and Muñoz (2020), where students shifted from vocational to university pathways following changes in loan availability. In our case, the shift appears to also influence persistence outcomes in distinct ways across institution types.

Our findings complement existing literature that documents how financial aid impacts retention in higher education. For instance, Castleman and Long (2016) show that need-based grants not only increase college entry but also improve continuation and completion rates, while Dynarski (2003) finds that lowering the cost of education raises college completion. Similarly, Murphy and Wyness (2023) provide evidence that an unexpected institutional financial aid award improves first-year persistence and course performance among enrolled students, highlighting the role of credit constraints in academic outcomes. In the Chilean context, Solis (2017) and Aguirre (2021) show that access to student loans can significantly boost persistence and completion. In particular, Solis (2017) shows that access to student loans increases the probability of enrolling in a second year of university by 50 percent. Unlike these studies, which focus on the introduction of financial aid, our results suggest that even adjustments to existing aid schemes—such as interest rate reductions or changes to repayment conditions of student loans—can have meaningful effects on student outcomes. Moreover, the heterogeneous effects by institution type emphasize the importance of considering how policy design interacts with institutional characteristics and student preferences.

5 Alternative Identification Strategy

A related literature examining the effects of student loan availability in Chile exploits the discontinuity around the 475-point PSU eligibility threshold (e.g., Card and Solis, 2022; Bucarey, Contreras and Muñoz, 2020; Solis, 2017), as well as the 5.3 GPA cutoff (Aguirre, 2021). Building on this approach, this section presents additional results using a Difference-in-Discontinuities (Diff-in-Disc) strategy for our three outcomes: enrollment, second-year retention, and second-year dropout.[7] We estimate separate effects for the HES, universities and vocational institutions. This analysis serves as a complementary robustness check to our main Difference-in-Differences (DiD) findings.

We implement a standard Regression Discontinuity Design (RDD) using PSU test scores as the running variable and comparing students just above and below the 475-point eligibility threshold. This strategy compares otherwise similar students who differ only in their eligibility for the CAE loan, leveraging local quasi-random variation in loan access (Lee and Card, 2008; Lee and Lemieux, 2010). Following Calonico, Cattaneo and Titiunik (2014), we select optimal bandwidths and estimate local linear regressions with triangular kernel weights.

[7] See Grembi, Nannicini and Troiano (2016) for a detailed discussion of this research design.

Formally, for each exposure group $j \in \{0, 1\}$, we estimate:

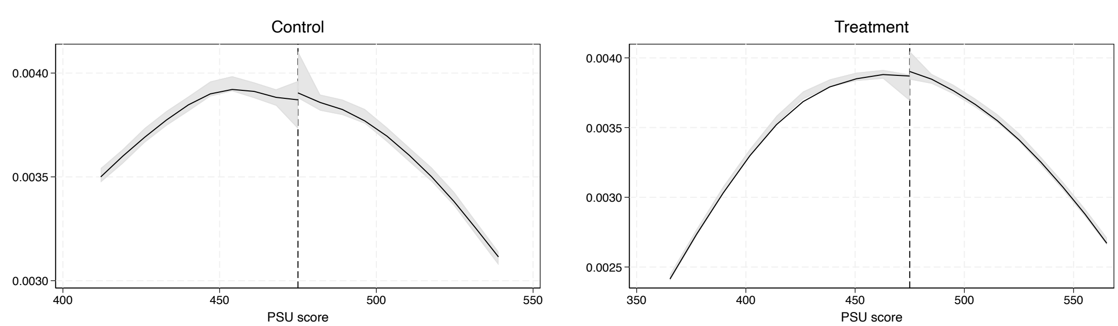
$$\min_{\boldsymbol{\alpha}^{j}} \sum_{i=1}^{n} \mathbf{1}\left(\text{exposed}_{it} = j\right) \left[y_{it} - \alpha_0^j - \alpha_1^j \cdot \mathbf{1}\left(x_{it} \geq 0\right) - \alpha_2^j x_{it} - \alpha_3^j x_{it} \cdot \mathbf{1}\left(x_{it} \geq 0\right)\right]^2 \mathbf{K}\left(x_{it}/h^j\right) \quad (3)$$

where $x_{it} = \text{PSU}_{it} - 475$ is the running variable for student i in cohort t centered at the threshold, h^j denotes the bandwidth, and $\mathbf{K}(\cdot)$ is the triangular kernel function. We estimate the model separately for the unexposed (control) and exposed (treatment) cohorts. The effect of the 2012 CAE loan reform is then identified as the difference between the two estimated discontinuities, i.e., $\tau = \alpha_1^1 - \alpha_1^0$. Class-level clustered standard errors for all parameters $\widehat{\alpha}$ and for the effect estimate $\widehat{\tau}$ are computed using Seemingly Unrelated Estimation (SUEST) to account for potential correlation across samples (Weesie, 1999).

In the case of vocational institutions, where eligibility is also determined by GPA, we first estimate the model using the full sample, as we do for universities, and then conduct an additional analysis restricted to students with GPA < 5.3, ensuring that eligibility is defined solely by the PSU threshold.

To verify the validity of our RDD approach, Figure 4 displays the results of a discontinuity test for the density of PSU scores around the 475 threshold for both unexposed and exposed cohorts (Cattaneo, Jansson and Ma, 2020; McCrary, 2008). For control cohorts, we conduct local polynomial density estimation with bandwidths of 20.971 and 21.323 to the left and right of the cutoff, respectively. The resulting test yields a t-statistic of 0.7514 (p-value = 0.4524). For treatment cohorts, the bandwidths are 36.579 and 30.071, and the test yields a t-statistic of 1.2909 (p-value = 0.1967). In both cases, we fail to reject the null hypothesis of continuity at the cutoff, suggesting an absence of manipulation in the running variable and thus supporting the validity of the RDD.

Figure 4: Density Test



Notes: Manipulation tests for PSU scores around the 475 cutoff for unexposed (left panel) and exposed (right panel) cohorts. The tests follow the procedure of Cattaneo, Jansson and Ma (2020), based on local polynomial density estimation.

Table 4 presents the Diff-in-Disc results for immediate enrollment. Columns (1)–(3) use the full sample of students, while columns (4)–(6) restrict the sample to students with GPA < 5.3, ensuring that loan eligibility for vocational institutions is determined solely by crossing the 475 PSU-score threshold. In addition to reporting the coefficients $\widehat{\alpha}_1^1$ (treatment), $\widehat{\alpha}_1^0$ (control), and $\widehat{\tau}$ (difference), we present the optimal bandwidths for each outcome and treatment group, as well as outcome means for unexposed students with $0 \leq x_i \leq$ bw, that is, for students in the control group who are marginally eligible.

For universities, where loan eligibility is determined exclusively by PSU scores, column (2) shows that passing the 475 cutoff increases the probability of enrollment by 10.2 pp. among control cohorts and by 12.7 pp. among treated cohorts. The difference between these effects indicates that the 2012 loan reform increased university enrollment by 2.5 pp. (significant at the 1% level), corresponding to an approximate 7 percent increase relative to the enrollment rate of just-above-the-cutoff students in the control cohorts. For students with GPA < 5.3, column (5) shows that loan eligibility increases enrollment by 6.1 pp. among control cohorts and by 8.4 pp. among treated cohorts, resulting in a difference of 2.3 pp. (significant at the 5% level), which corresponds to an 8 percent increase. These two estimates are not only very similar to each other but also to the effect obtained from our DiD model, suggesting that the reform's effects might be relatively homogeneous across the PSU and GPA distributions.

Table 4: Difference-in-Discontinuities Design: Immediate Enrollment

	All students			GPA < 5.3		
	HES	Universities	Vocational	HES	Universities	Vocational
	(1)	(2)	(3)	(4)	(5)	(6)
Difference	0.013**	0.025***	-0.007	0.003	0.023**	-0.022*
	(0.006)	(0.006)	(0.006)	(0.012)	(0.010)	(0.011)
Treatment	0.074***	0.127***	-0.048***	0.062***	0.084***	-0.024***
	(0.004)	(0.005)	(0.005)	(0.009)	(0.007)	(0.009)
Control	0.061***	0.102***	-0.040***	0.059***	0.061***	-0.002
	(0.004)	(0.004)	(0.004)	(0.008)	(0.007)	(0.007)
Outcome Mean	0.578	0.359	0.199	0.540	0.304	0.234
Bandwidth						
Treatment	51.257	36.629	41.201	48.882	47.259	43.601
Control	51.142	40.393	51.088	45.712	48.539	55.572

Notes: Local linear regressions with triangular kernel and optimal bandwidths (bw) are estimated separately for treatment and control cohorts. SUEST standard errors clustered at the class level in parentheses. Outcome means are presented for unexposed students with $0 \leq x_i \leq$ bw. *** p< 0.01, ** p< 0.05, * p< 0.1.

In vocational institutions, eligibility is achieved by passing either the 475-PSU threshold or the 5.3-GPA cutoff. Column (3) for the full sample shows similar increases in enrollment probabilities from passing the 475 cutoff between treatment and control cohorts, resulting in a non-significant difference of -0.7 pp. This null effect may be explained by the fact that approximately 60% of students with a PSU score below 475 points have a GPA above 5.3 and are therefore still eligible for the loan. When restricting the sample to students with GPA < 5.3 (column (6)), we find that becoming eligible has no effect on enrollment among control cohorts, while among treated cohorts it leads to a 2.4 pp. decrease in vocational enrollment (significant at the 1% level). This translates into a 2.2 pp. reduction (approximately 9 percent, significant at the 10% level) in vocational enrollment attributable to the 2012 loan reform, a result that is consistent with our DiD estimates. Overall, these findings suggest that the decline in vocational enrollment might be relatively homogeneous across the PSU distribution but concentrated among students with GPA < 5.3 who may have shifted towards universities following the reform.

Table 5 presents the Difference-in-Discontinuity estimates for our second-year outcomes. For second-year enrollment in universities, crossing the 475-PSU threshold increases the probability of consecutive enrollment by 8.2 pp. in control cohorts and by 10.4 pp. in treatment cohorts, using the full sample. Among students with GPA < 5.3, the increases are 4.2 and 6.3 pp., respectively. In both cases, the estimated effect of the 2012 loan reform is approximately 2.2 pp. (significant at the 1% level), corresponding to a 7 percent increase relative to the second-year enrollment rate of just-above-the-cutoff students in the control cohorts. For university second-year dropout, we find that loan eligibility increases the probability of dropout in both control and treatment cohorts, across both samples. Moreover, the loan reform raised second-year dropout by 1.1 pp. (21 percent) in the full sample and by 1.5 pp. (25 percent) among students with GPA < 5.3, both effects being significant at the 1% level. Compared to our DiD estimates for universities, which show a 2.1 pp. increase in second-year enrollment and a 0.5 pp. increase in dropout, these results suggest that the retention effects are relatively homogeneous across the ability distribution, while the dropout effects appear to be concentrated among marginally PSU-eligible students and are particularly pronounced among those in the lower part of the GPA distribution.

For vocational institutions, crossing the 475-PSU threshold is associated with decreases in both second-year enrollment and second-year dropout in the full sample, but with increases in both outcomes among students with GPA < 5.3. Regarding the impact of the 2012 loan price reduction, we find no statistically significant effect on second-year enrollment in either sample. However, among students with GPA < 5.3, the reform led to a 1.2 pp. reduction in second-year dropout (26 percent, significant at the 5% level). Compared to our DiD estimates—effects of -0.6 pp. on second-year enrollment and -1.5 pp. on dropout—these results suggest that the effects in vocational institutions are heterogeneous across the ability distribution in terms of magnitude, though they remain consistent in direction.

The Diff-in-Disc estimates align closely with the results from our main DiD specification, especially for university outcomes. This similarity is noteworthy given the differences in identifying assumptions and populations captured by each method. While the DiD strategy compares broader cohorts before and after the reform, the Diff-in-Disc approach focuses on students near the 475 PSU eligibility threshold, providing local estimates for marginally eligible students. The similarity in estimated effects for enrollment and retention suggests that the impact of the 2012 reform may be relatively homogeneous across the ability distribution, or that the overall effect is largely concentrated among students near the eligibility threshold. This does not imply that the two estimators should yield identical results; some divergence is expected because the marginal student captured by the Diff-in-Disc differs from the one captured by the DiD. If the effect of improved loan conditions varies across students, then discrepancies would arise naturally. That the estimates are generally similar in our case strengthens the interpretation that the reform's main consequences were shared across a broad group of eligible students.

In addition, our results align with studies using RDD to examine the effects of loan eligibility on enrollment and retention. For university students, we find that passing the 475 PSU-score threshold increases the probability of immediate enrollment by 10.2–12.7 pp. and of second-year enrollment by 8.2–10.4 pp. These estimates are comparable to those in Solis (2017), who finds a 16.2 pp. effect on immediate enrollment and a 16 pp. effect on second-year ever enrollment, and to Bucarey, Contreras and Muñoz (2020), who report a 6.8 pp. increase in ever enrollment. For vocational institutions, our estimates suggest a decrease in enrollment probabilities (–4.0 pp. and –4.8 pp.), closely matching the –5.8 pp. decline in ever enrollment found by Bucarey, Contreras and Muñoz (2020). By contrast, Aguirre (2021) documents increases of 4.2 pp. in immediate enrollment and 4.1 pp. in ever enrollment from passing the 5.3 GPA threshold.

Table 5: Difference-in-Discontinuities Design: Second-Year Outcomes

	All students			GPA < 5.3		
	HES	Universities	Vocational	HES	Universities	Vocational
	(1)	(2)	(3)	(4)	(5)	(6)
Second Year Enrollment						
Difference	0.019***	0.022***	-0.001	0.036***	0.021**	0.010
	(0.006)	(0.006)	(0.006)	(0.012)	(0.009)	(0.010)
Treatment	0.071***	0.104***	-0.038***	0.075***	0.063***	0.003
	(0.004)	(0.004)	(0.004)	(0.008)	(0.006)	(0.007)
Control	0.051***	0.082***	-0.036***	0.039***	0.042***	-0.007
	(0.005)	(0.004)	(0.004)	(0.010)	(0.007)	(0.007)
Outcome Mean	0.508	0.308	0.171	0.445	0.235	0.185
Bandwidth						
Treatment	54.855	36.385	37.827	56.684	46.439	46.449
Control	50.292	43.328	44.053	38.495	43.131	48.969
Second Year Dropout						
Difference	0.002	0.011***	-0.003	-0.003	0.015***	-0.012**
	(0.003)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)
Treatment	0.007***	0.023***	-0.007***	-0.001	0.022***	-0.013***
	(0.002)	(0.002)	(0.002)	(0.005)	(0.003)	(0.004)
Control	0.004**	0.012***	-0.004**	0.002	0.007**	-0.001
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.004)
Outcome Mean	0.060	0.052	0.031	0.079	0.060	0.046
Bandwidth						
Treatment	68.837	43.806	46.012	47.480	61.127	51.662
Control	63.992	51.955	52.965	57.989	61.913	59.837

Notes: Local linear regressions with triangular kernel and optimal bandwidths (bw) are estimated separately for treatment and control cohorts. SUEST standard errors clustered at the class level in parentheses. Outcome means are presented for unexposed students with $0 \leq x_i \leq$ bw. *** p< 0.01, ** p< 0.05, * p< 0.1.

Finally, it is important to note that our estimates of the effects of the 2012 loan reform are smaller than the effects found in previous literature because our estimates capture a fundamentally different margin: rather than estimating the effect of access to loans versus no access, we estimate the effect of reducing the price of loans (through lower interest rates and better repayment conditions) for students already eligible under pre-reform rules. This distinction is crucial, as the populations and margins differ: in prior studies, the baseline is no credit access at all, whereas in our context the baseline is access to less generous loans.

Together, these findings suggest that while access to credit can substantially alter enrollment decisions, further improvements in loan terms continue to shape student behavior, albeit with smaller effects. The convergence of findings from both the DiD and Diff-in-Disc approaches reinforces the robustness of our conclusions regarding the 2012 loan reform's impact on educational attainment in Chile. Our results not only align with existing literature on the effects of loan eligibility but also extend the discourse by highlighting the consequences of modifying loan conditions for already eligible students.

6 Heterogeneity

Building upon our findings, this section delves deeper into the heterogeneity of the reform's effects across demographic and educational backgrounds, providing a more granular understanding of the reform's outcomes. To investigate this heterogeneity, we re-estimate Equation (1) separately for female and male students (Table 6), and for students graduating from public and voucher schools (Table 7). Each table reports the estimated effects for the respective subsamples, along with their differences, including SUEST class-level clustered standard errors, outcome means, and sample sizes.

6.1 Female vs. Male Students

The results in Table 6 indicate significant heterogeneity between female and male students, particularly in the outcomes related to vocational institutions, while the effects on university-related outcomes appear more homogeneous. First, regarding immediate enrollment, we observe the previously discussed shift from vocational institutions to universities. The increase in university enrollment resulting from the loan reform is similar for female and male students. However, the decline in vocational enrollment is more pronounced among female students (-3.1 pp.) than among male students (-1.7 pp.). This differential effect leads to an overall decrease of 0.8 pp. in immediate enrollment among female students in the HES—equivalent to a 1.5 percent reduction and statistically significant at the 5% level.

Second, the results for second-year enrollment follow a similar pattern. The increase in university second-year enrollment is comparable between female and male students, indicating no significant heterogeneity along this dimension. In contrast, the effects for vocational institutions differ by sex: while the reform has no statistically significant impact on vocational second-year enrollment among male students, it leads to a negative effect of 1.3 pp. (9 percent) for female students. Third, regarding second-year dropout, we find no significant sex differences in the effects of the reform, whether considering university, vocational, or overall dropout. As previously discussed, the loan price reduction led to lower second-year dropout in vocational institutions and a slight increase in universities, and these results hold similarly for both female and male students.

These heterogeneous effects suggest that female students may respond more cautiously to a reduction in the cost of borrowing, particularly when this change shifts the perceived tradeoff between vocational and university education. While the reform encouraged university enrollment, it also reduced the relative attractiveness of vocational programs, which are typically shorter and more affordable. For some female students, this shift may have introduced new academic or financial barriers, leading to a net decline in immediate enrollment. This result is consistent with evidence that women, on average, are more risk-averse in financial decisions and may be more sensitive to perceived debt burdens, even when borrowing conditions improve (Croson and Gneezy, 2009). Additionally, female students may face structural constraints—such as caregiving responsibilities or labor market expectations—that make longer university programs less feasible. In this context, the 2012 CAE reform may have inadvertently discouraged some female students from enrolling in the HES, highlighting the importance of aligning policy incentives with the specific needs and constraints of different student groups.

Table 6: Heterogeneity of Main Results by Student Sex

	HES			Universities			Vocational		
	Female	Male	Difference	Female	Male	Difference	Female	Male	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Immediate Enrollment	-0.008**	0.004	-0.012**	0.022***	0.021***	0.001	-0.031***	-0.017***	-0.013***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)	(0.004)	(0.004)	(0.005)
	[0.52]	[0.55]		[0.34]	[0.37]		[0.18]	[0.18]	
Second-Year Enrollment	0.009**	0.021***	-0.011*	0.019***	0.017***	0.002	-0.013***	0.001	-0.014***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)	(0.003)	(0.003)	(0.004)
	[0.46]	[0.49]		[0.30]	[0.33]		[0.14]	[0.14]	
Second-Year Dropout	-0.014***	-0.013***	-0.001	0.004***	0.004***	-0.000	-0.015***	-0.014***	-0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
	[0.05]	[0.06]		[0.03]	[0.04]		[0.03]	[0.03]	
Observations	798,437	698,942		798,437	698,942		798,437	698,942	
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: SUEST standard errors clustered at the class level in parentheses. Outcome means for eligible unexposed students in square brackets. *** p<0.01, ** p<0.05, * p<0.1. School-level controls include indicators of school type and rural location, while student-level controls include attendance rate and municipality.

6.2 Public-School vs. Voucher-School Students

Table 7 presents the heterogeneity analysis by high school type, comparing individuals graduating from voucher and public schools. The results suggest that the observed substitution in enrollment between vocational and university programs is primarily driven by students from voucher schools. Among these students, we observe a 3 pp. (18 percent) decrease in immediate enrollment in vocational institutions, which is almost entirely offset by a 2.9 pp. (7 percent) increase in university enrollment. As a result, the net effect on immediate enrollment in the HES is close to zero for this group. In contrast, among students from public schools, we find no evidence of a substitution between institutions, and the reform's immediate enrollment effects are statistically null for both universities and vocational institutions.

When we analyze second-year enrollment, we again find that the effects are concentrated among voucher-school students. Specifically, there is a significant increase of 2.5 pp. (7 percent) in second-year university enrollment for this group, while no significant change is observed for students from public schools. Similarly, the decline in second-year vocational enrollment is driven entirely by voucher-school students, who experience a reduction of 0.8 pp. (6 percent), whereas no effect is detected among public-school students.

Finally, second-year dropout effects also differ by school type, especially in vocational institutions. The decrease in vocational dropout is more pronounced among voucher-school students, reaching 1.7 pp. (57 percent), compared to a 1.0 pp. (25 percent) decrease for public-school students. In the case of university dropout, differences are weaker and only marginally significant (at the 10% level), but the overall reduction in second-year dropout remains larger among voucher-school students (-1.5 pp.) relative to public-school students (-0.9 pp.).

Table 7: Heterogeneity of Main Results by School Type

	HES			Universities			Vocational		
	Public	Voucher	Difference	Public	Voucher	Difference	Public	Voucher	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Immediate Enrollment	0.003	-0.000	0.003	0.008	0.029***	-0.021**	-0.005	-0.030***	0.024***
	(0.006)	(0.004)	(0.007)	(0.008)	(0.005)	(0.009)	(0.005)	(0.004)	(0.006)
	[0.49]	[0.56]		[0.30]	[0.39]		[0.19]	[0.17]	
Second-Year Enrollment	0.013**	0.021***	-0.009	0.006	0.025***	-0.019**	0.004	-0.008**	0.012**
	(0.006)	(0.004)	(0.007)	(0.008)	(0.005)	(0.009)	(0.004)	(0.003)	(0.005)
	[0.43]	[0.50]		[0.27]	[0.35]		[0.15]	[0.13]	
Second-Year Dropout	-0.009***	-0.015***	0.006**	0.003***	0.006***	-0.003*	-0.010***	-0.017***	0.007***
	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
	[0.06]	[0.05]		[0.04]	[0.04]		[0.04]	[0.03]	
Observations	590,563	906,816		590,563	906,816		590,563	906,816	
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: SUEST standard errors clustered at the class level in parentheses. Outcome means for eligible unexposed students in square brackets. *** p<0.01, ** p<0.05, * p<0.1. School-level controls include indicators of school type and rural location, while student-level controls include attendance rate and municipality.

The heterogeneous effects by school type likely reflect underlying differences in both academic preparedness and financial constraints between public- and voucher-school students. Public-school students in Chile generally achieve lower scores on standardized tests and tend to come from more disadvantaged socioeconomic backgrounds compared to their voucher-school peers (Torche, 2005; Hsieh and Urquiola, 2006; Urquiola, 2016). These factors may limit their access to university programs regardless of financial aid reforms.

Furthermore, the design of the CAE loan program—covering only a fixed tuition cap—means that students are still responsible for covering any remaining costs. For public-school students, the 2012 loan price reduction may not have sufficiently altered the affordability of tertiary education, especially if they faced larger funding gaps or higher non-tuition-related barriers such as learning materials or living expenses. In contrast, voucher-school students, who may have faced fewer academic or financial constraints at the margin, appear to have responded more readily to the reform. This interpretation is consistent with our findings that increases in university enrollment and reductions in dropout were primarily concentrated among voucher-school graduates, suggesting that the reform, while effective for some, may have failed to reach the most vulnerable students in the system.

7 Conclusions

In this paper, we analyze the effects of a student loan reform on enrollment, retention and dropout in higher education. The 2012 reform to Chile's state-guaranteed student loan program (CAE) substantially reduced the interest rate from approximately 6% to a fixed rate of 2%, alongside other, less prominent improvements to repayment conditions. We exploit this policy change using a difference-in-differences (DiD) approach, and our main results are robust to an alternative "difference-in-discontinuities" (Diff-in-Disc) strategy, which applies a regression discontinuity design (RDD) under different identification assumptions. The consistency across methods lends additional credibility to our findings and supports the use of DiD to evaluate the educational consequences of reduced borrowing costs introduced by the reform.

Our results show that the reform had no effect on overall immediate enrollment in higher education. However, we find a compositional shift: university enrollment increased by 2.5 percentage points (pp.)—a 7 percent rise relative to the enrollment rate of eligible cohorts before the reform—while enrollment in vocational institutions declined by an equivalent 2.5 pp., representing a 14 percent drop relative to the same group. These findings highlight the importance of considering institutional heterogeneity in the effects of financial aid reforms, as policy changes may influence not only whether students enroll but also where they choose to enroll.

While the reform did not expand overall access to tertiary education, it did produce a notable reallocation of students across institutional types. This shift toward universities, where programs tend to be longer and more expensive, may result in higher individual debt burdens, partially offsetting the intended financial relief from lower interest rates. This potential trade-off between improved loan terms and increased borrowing is particularly relevant in light of persistent concerns about repayment; for instance, Ingresa (2023) reports a default rate of 54% among all CAE borrowers in 2023.

Regarding persistence, we find improvements concentrated among university students, with a 2 pp. (7%) increase in second-year enrollment. In contrast, second-year enrollment in vocational institutions declined by 0.6 pp. Patterns of second-year dropout also diverged across institutional types: while dropout increased by 0.5 pp. (13%) among university students, it declined by 1.5 pp. (47%) among vocational students. These heterogeneous effects in persistence—together with the observed shifts in enrollment—highlight the importance of institutional type, as they may have meaningful long-term implications for students' educational trajectories and repayment outcomes.

Virtually all of the effects on enrollment and persistence are concentrated among students graduating from voucher schools, with no detectable response among students from public schools, who likely face additional academic and economic constraints. We also find that overall enrollment decreased slightly among female students, which may reflect greater sensitivity to debt or institutional risk.

This paper sheds light on the heterogeneous effects of student loan reforms that modify borrowing conditions without expanding eligibility. By documenting compositional shifts in enrollment and differential responses across institution types, school backgrounds, and gender, it advances the research agenda recently outlined by Dynarski, Page and Scott-Clayton (2023), which emphasizes the need to unpack the distributional consequences of student aid. In doing so, the paper contributes to a growing body of evidence on how price-based reforms affect student behavior and offers timely insights for policymakers engaged in the ongoing debates about the structure and effectiveness of loan programs. These findings underscore the importance of accounting for institutional and demographic heterogeneity when designing or reforming student financing systems.

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Both authors contributed equally to this work.

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A Parallel Trends Assumption

This appendix examines the validity of the parallel trends assumption underlying our difference-in-differences (DiD) identification strategy and provides the detailed estimation results used to construct Figures 2 and 3, which display the dynamic treatment effects of the reform. To this end, we estimate the event study specification described in Equation (2) separately for each outcome.

The results are presented in Table A.1. Columns (1)–(4) use the 2011 cohort as the reference category, while Columns (5)–(12) use 2010 instead. The coefficients on the interaction terms 'Eligible × cohort j' for $j \in \{2007, 2008, 2009, 2010, 2012, 2013, 2014, 2015\}$ in Columns (1)–(4), and $j \in \{2007, 2008, 2009, 2011, 2012, 2013, 2014, 2015\}$ in Columns (5)–(12), are the β_j coefficients in Equation (2) and are plotted in the corresponding figures along with their 95% confidence intervals.

The bottom panel of Table A.1 reports the p-values of F-tests for the null hypothesis $H_0 : \boldsymbol{\beta}^{\text{pre}} = \mathbf{0}$, where $\boldsymbol{\beta}^{\text{pre}}$ denotes the vector of pre-reform interaction coefficients. For most outcomes, we fail to reject the null at conventional significance levels, providing formal support for the assumption of no differential pre-trends between eligible and ineligible cohorts. Taken together with the visual evidence in Figure 1, these results reinforce the credibility of our DiD estimates and support the validity of our identification strategy.

Table A.1: Event Study

	Immediate Enrollment				Second-Year Enrollment				Second-Year Dropout			
	Universities		Vocational		Universities		Vocational		Universities		Vocational	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eligible × cohort 2007	-0.016	-0.017*	0.003	0.004	0.003	0.002	-0.014***	-0.014***	-0.004***	-0.004***	0.003	0.003
	(0.010)	(0.010)	(0.006)	(0.006)	(0.009)	(0.010)	(0.005)	(0.005)	(0.002)	(0.001)	(0.003)	(0.003)
Eligible × cohort 2008	-0.015	-0.013	-0.010*	-0.010	0.001	0.003	-0.025***	-0.025***	-0.001	-0.001	0.001	0.000
	(0.010)	(0.010)	(0.006)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.001)	(0.001)	(0.002)	(0.002)
Eligible × cohort 2009	-0.015	-0.015	0.008	0.009	-0.000	0.000	-0.009*	-0.009*	-0.001	-0.001	0.003	0.003
	(0.010)	(0.010)	(0.006)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.001)	(0.001)	(0.002)	(0.002)
Eligible × cohort 2010	-0.014	-0.015	0.014**	0.015**								
	(0.010)	(0.010)	(0.006)	(0.006)								
Eligible × cohort 2011					0.013	0.013	-0.009*	-0.010*	0.001	0.001	-0.005**	-0.005**
					(0.010)	(0.010)	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.003)
Eligible × cohort 2012	0.013	0.011	-0.018***	-0.016***	0.027***	0.025***	-0.018***	-0.018***	0.000	-0.000	-0.013***	-0.013***
	(0.009)	(0.009)	(0.006)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.003)
Eligible × cohort 2013	0.015	0.012	-0.019***	-0.018***	0.024***	0.021**	-0.015***	-0.015***	0.005***	0.005***	-0.018***	-0.017***
	(0.009)	(0.009)	(0.007)	(0.007)	(0.009)	(0.009)	(0.006)	(0.006)	(0.001)	(0.001)	(0.003)	(0.003)
Eligible × cohort 2014	0.017*	0.016	-0.026***	-0.025***	0.027***	0.025***	-0.024***	-0.024***	0.005***	0.005***	-0.016***	-0.015***
	(0.010)	(0.010)	(0.007)	(0.007)	(0.009)	(0.010)	(0.006)	(0.006)	(0.001)	(0.001)	(0.003)	(0.003)
Eligible × cohort 2015	0.011	0.009	-0.021***	-0.020***	0.021**	0.019**	-0.018***	-0.019***	0.004***	0.005***	-0.016***	-0.016***
	(0.009)	(0.009)	(0.007)	(0.007)	(0.009)	(0.009)	(0.006)	(0.005)	(0.001)	(0.001)	(0.003)	(0.003)
Eligible	0.302***	0.283***	-0.036***	-0.035***	0.271***	0.250***	0.014***	0.012***	0.017***	0.018***	-0.036***	-0.033***
	(0.007)	(0.007)	(0.004)	(0.004)	(0.007)	(0.007)	(0.004)	(0.004)	(0.001)	(0.001)	(0.002)	(0.002)
Cohort 2007	0.024***	0.024***	-0.040***	-0.043***	0.013***	0.011***	-0.017***	-0.020***	0.013***	0.013***	-0.005*	-0.005*
	(0.003)	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.003)	(0.002)
Cohort 2008	0.014***	0.012***	-0.028***	-0.031***	0.009***	0.004	-0.004	-0.006	0.007***	0.008***	-0.007***	-0.006***
	(0.003)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.003)	(0.002)
Cohort 2009	0.002	0.003	-0.027***	-0.028***	0.002	0.001	-0.002	-0.001	0.001	0.001	-0.008***	-0.008***
	(0.003)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.002)	(0.002)
Cohort 2010	-0.002	0.000	-0.018***	-0.019***								
	(0.003)	(0.003)	(0.006)	(0.005)								
Cohort 2011					-0.001	-0.003	0.009**	0.010**	0.003**	0.003***	0.009***	0.009***
					(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.003)	(0.003)
Cohort 2012	0.003	0.005	0.028***	0.031***	-0.000	0.002	0.031***	0.037***	0.005***	0.003***	0.014***	0.013***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.003)	(0.003)
Cohort 2013	-0.006**	-0.006	0.059***	0.059***	-0.007***	-0.008**	0.053***	0.055***	0.003**	0.002*	0.024***	0.024***
	(0.002)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.005)	(0.004)	(0.001)	(0.001)	(0.003)	(0.003)
Cohort 2014	-0.012***	-0.010***	0.069***	0.071***	-0.010***	-0.009***	0.064***	0.067***	-0.000	-0.002	0.023***	0.022***
	(0.002)	(0.003)	(0.006)	(0.006)	(0.002)	(0.003)	(0.005)	(0.005)	(0.001)	(0.001)	(0.003)	(0.003)
Cohort 2015	-0.012***	-0.010***	0.062***	0.064***	-0.009***	-0.008**	0.057***	0.060***	-0.001	-0.002**	0.023***	0.022***
	(0.002)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.001)	(0.003)	(0.003)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379
Pre-trends p-value	0.423	0.401	0.001	0.001	0.987	0.988	0.000	0.000	0.039	0.023	0.531	0.439

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. School-level control variables include indicators for school type and rural location, while student-level controls include gender, attendance rate and municipality. The pre-trends p-value corresponds to the null hypothesis that interaction coefficients are equal to zero for unexposed cohorts.

B Cohort 2011

As discussed in Section 3 and Subsection 4.2, students from cohort 2011 differ from later cohorts in terms of their exposure to the 2012 CAE loan reform. These students made their first-year enrollment decisions before the reform—under the original, higher interest rate—but made their second-year decisions after the reform, once loan conditions had become more favorable. Thus, their exposure lies between that of pre-reform and fully treated cohorts. To account for this difference, we estimate separate interaction coefficients for the 2011 (*Half-exposed*) cohort and for cohorts 2012–2015 (*Fully exposed*) in all second-year outcome specifications.

This distinction is conceptually relevant because, for the 2011 cohort, the reform could only affect continuation decisions (persistence or dropout) after students had already enrolled. Following the literature on the effects of cost changes on continuing students (e.g., Bietenbeck et al., 2023; Denning, 2019), these effects can be interpreted as operating at the intensive margin. In contrast, cohorts 2012–2015 were fully exposed to the reform when making both their first- and second-year decisions, implying treatment at the extensive margin.

Table B.1 presents the results. For second-year enrollment, the fully exposed cohorts show a 1.7–2.0 percentage point (pp.) increase in overall enrollment (significant at the 1% level), driven by a 2.2–2.4 pp. increase in universities and a 0.7–0.8 pp. decline in vocational institutions. The half-exposed cohort exhibits a similar overall effect (1.7–1.8 pp., significant at 1%) but smaller coefficients at the type level: a 1.2 pp. increase in universities and no significant change in vocational institutions. For second-year dropout, the fully exposed cohorts display an overall decline of 1.5–1.6 pp. (significant at 1%), with a 0.5 pp. rise in universities and a 1.7 pp. decrease in vocational institutions. Among half-exposed students, the corresponding effects are more muted: a 0.7 pp. decline in overall dropout (significant at 1%), a 0.3 pp. rise in universities, and a 0.7 pp. decrease in vocational institutions.

Overall, the estimates confirm the robustness of our main findings. The effects for fully exposed cohorts align closely with the baseline results in Table 3, reinforcing our interpretation of the reform as generating compositional shifts toward universities and improved short-term persistence within the higher education system. The smaller coefficients for the 2011 cohort suggest that our main estimates combine both intensive and extensive margin effects, although we cannot statistically reject equality between half- and fully-exposed coefficients.

This analysis further supports our focus on first- and second-year outcomes in the main paper. Extending the analysis to third- or later-year outcomes would complicate identification since exposure and enrollment decisions would increasingly overlap across cohorts and treatment statuses.

Table B.1: Second-Year Enrollment and Dropout, Separating Cohort 2011

	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Second-Year Enrollment									
Eligible × Fully-exposed	0.020***	0.020***	0.017***	0.024***	0.024***	0.022***	-0.007**	-0.008***	-0.008***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Eligible × Half-exposed	0.018***	0.018***	0.017***	0.012*	0.012	0.012	0.002	0.003	0.002
	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Fully-exposed	0.044***	0.048***	0.057***	-0.012***	-0.027***	-0.022***	0.056***	0.077***	0.082***
	(0.003)	(0.007)	(0.006)	(0.001)	(0.007)	(0.007)	(0.002)	(0.004)	(0.004)
Half-exposed	0.007	0.011	0.015**	-0.007***	-0.016***	-0.015***	0.014***	0.029***	0.032***
	(0.005)	(0.007)	(0.006)	(0.002)	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)
Eligible	0.278***	0.278***	0.255***	0.272***	0.272***	0.251***	0.003	0.003	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)
Outcome mean	0.470	0.470	0.470	0.316	0.316	0.316	0.140	0.140	0.140
Second-Year Dropout									
Eligible × Fully-exposed	-0.015***	-0.016***	-0.015***	0.005***	0.005***	0.005***	-0.017***	-0.017***	-0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Eligible × Half-exposed	-0.007***	-0.007***	-0.007***	0.003**	0.003**	0.003***	-0.007***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Fully-exposed	0.023***	0.019***	0.017***	-0.003***	-0.011***	-0.013***	0.026***	0.027***	0.025***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Half-exposed	0.012***	0.007***	0.007***	-0.002**	-0.008***	-0.008***	0.013***	0.012***	0.012***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Eligible	-0.022***	-0.022***	-0.018***	0.015***	0.015***	0.016***	-0.034***	-0.034***	-0.031***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379	1,497,379
Control group size	481,617	481,617	481,617	481,617	481,617	481,617	481,617	481,617	481,617

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. School-level controls include indicators for school type and rural location, while student-level controls include gender, attendance rate, and municipality. Control group size and outcome mean are presented for eligible students not exposed to the reform.

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