The Unintended Effects of an Intensive Margin Reform to Student Loans on Educational Attainment*

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Abstract

This paper documents the unintended consequences of a Chilean student-loan reform that reduced the interest rate by two-thirds in 2012, along with other changes. This reform provides a unique opportunity to assess changes at the intensive margin of ongoing student loans, as opposed to the extensive margin of initial implementation. In contrast with the intended objective of improving repayment, we find unexpected effects on the composition of enrollment across institutions, limited coverage for low-income students, and gender inequality in education decisions. Using rich administrative records, we exploit differences in exposure and eligibility with a difference-in-differences approach. We further complement our identification strategy with a difference-in-discontinuities design, providing as-good-as-random local variation in eligibility. Our results show a diversion effect in immediate enrollment from vocational institutions to universities with no effect on overall enrollment, diversion that permeates into two-year enrollment and second-year dropout. Virtually all our results are driven by middle-income students, with no effects on low-income students. Moreover, female overall immediate enrollment falls, in contrast with a fully compensated diversion effect for males that leaves their overall enrollment unchanged.

1 Introduction

There has been a steady increase in tertiary education access in recent decades (OECD, 2022). The private and social benefits of attaining higher levels of education are well known, including increased income, equality of opportunity, social mobility, economic growth, and well-being, among others (Ma and Pender, 2023; OECD, 2022; World Bank, 2018; Hill, Hoffman and Rex, 2005). However, in countries with a combination of high tuition fees and low subsidy regimes, this upward trend in access to higher education has been accompanied by growing levels of student debt since loans are typically used as the main cost-sharing mechanism between students and the state (Garritzmann, 2016; Heller and Callender, 2013). In recent years, the increased financial burden of students has led to higher default rates, sparking public debates around student debt relief plans such as the Public Service Loan Forgiveness program in the U.S. or a recent proposal for debt remission in

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Chile. These are examples of recent initiatives to reform existing financial aid programs, years after implementation.[1]

The need to revise student loans has now become evident, but successful reforms require sound empirical evidence on the effects of intensive margin changes to ongoing loan programs. The literature has mainly focused on the extensive margin of these programs, evaluating the effects of introducing an entirely new financial aid program on several short- and long-term outcomes such as enrollment, dropout, graduation, repayment, labor market outcomes, or even family planning (e.g., Black et al., 2023; Mezza et al., 2020; Dearden, 2019; Scott-Clayton and Zafar, 2019; Velez, Cominole and Bentz, 2019; Marx and Turner, 2019; Wiederspan, 2016; Rothstein and Rouse, 2011). However, to the best of our knowledge, evidence on intensive margin changes is scarce, in part because there are few such reforms (e.g., Herbst, 2023; Sten-Gahmberg, 2020). In addition, most of this literature focuses on predicting the potential effects of reforming loan programs through simulated and comparative analysis, as opposed to ex post impact evaluation (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).

The Chilean case provides a unique opportunity to empirically assess the effects of a reform on the intensive margin. In 2006, the Chilean government introduced a state-guaranteed loan program that was later reformed in 2012. This reform consisted of the following changes to repayment conditions: (i) a decrease in the interest rate from approximately 6 percent on average to a fixed interest rate of 2 percent, (ii) repayments were made contingent on income with a cap of 10 percent, and (iii) the possibility to delay repayments in case of unemployment. In the Chilean context, the interest rate decrease is the most salient change since it is automatically applicable to all loans, while the other two are available upon request, and only a small fraction of debtors apply for them.[2]

With these changes, the government intended to improve repayments in a context of high default rates over 35% that were predicted to increase beyond 50% at the time (World Bank, 2011).[3] While it would be natural to investigate whether the reform achieved its declared goal, this paper focuses on its effects on enrollment and retention in tertiary education. The 2012 changes, especially the interest rate decrease, constitute a relaxation of financial constraints for high school graduates, which could in turn alter their marginal incentives and, therefore, impact their enrollment decisions. Furthermore, the overall effect might not necessarily be an improvement in educational attainment, since the interaction of multiple incentives throughout a student's life cycle could yield unintended consequences.

The institutional setting of Chile's higher education system (HES hereinafter) is relevant for the student loan literature because of its similarities with the U.S. system while simultaneously featuring an admission process that is entirely determined by observable academic variables such as high-school GPA and a national admission test score. This allows us to overcome the challenges to causal inference posed by the fact that admission processes are highly determined by unobserved measures such as alumni status of parents and recommendation letters in most countries.[4] Moreover,

[1] See https://studentaid.gov/ and https://www.bcn.cl/ as references of initiatives in the U.S. and Chile respectively. Other countries using student loans include Colombia, Mexico, Canada, the U.K. and Australia, among others.

[2] In 2021, nine years after the reform, only 4% and 5% of debtors were beneficiaries of the 10% cap and delayed repayments, respectively. See Ingresa (2021) for details.

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

[4] See Riegg (2008) and Liu and Borden (2019) for discussions of causal inference and selection bias in the financial aid literature.

the Chilean HES has a highly centralized and standardized grants system, allowing researchers to access a rich set of administrative records. In fact, previous studies have exploited the quasi-natural experiments provided by this setting to identify the causal effects of the extensive margin —i.e., credit access— of student loan programs 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).

To evaluate the causal effects of the 2012 reform on immediate and two-year enrollment and second-year dropout, we follow a difference-in-differences (DiD) approach as our main identification strategy, using the differences between exposed and nonexposed students and between students who were eligible and ineligible for the loan. We combine a rich set of administrative records at the individual level, covering the entire population of high school graduates in Chile between 2006 and 2014 who faced immediate enrollment decisions in the 2007–2015 period. We have detailed information about their enrollment and permanence choices, the academic variables that determine loan eligibility, and other individual, school, and educational program characteristics that we use as control variables. We complement our identification strategy with a difference-in-discontinuities (Diff-in-Disc) design, exploiting the same source of exogenous variation under different identification assumptions that provide as-good-as-random local variation in eligibility. In comparison to related research, our data offer the advantages of having (i) complete administrative records for the entire population of high school graduates, (ii) large sample sizes that improve the efficiency of our estimates, and (iii) a considerable number of cohorts.

We contribute to the literature on the effects of financial aid on educational attainment in two ways. First, while most research focuses on the effects of the extensive margin of these policies (e.g., granting access to student loans), this paper is one of the first to evaluate a reform that loosens financial constraints by introducing changes at the intensive margin of student loans in a context where those changes were mainly intended to improve repayment rates.[5] As Dynarski, Page and Scott-Clayton (2022) point out in a recent literature review, even small changes to financial aid programs can have large effects on the educational decisions of present-minded students since the associated costs are upfront while benefit flows are distributed over a longer, distant horizon. Moreover, the authors urge researchers and research consumers to ponder the potential compositional effects in enrollment across educational institutions or sectors. This is precisely our second contribution, where we document striking unintended consequences for enrollment composition, policy coverage, and gender inequality in access to tertiary education. Both our contributions are also relevant for public policy design in education, especially in countries with similar financial aid mechanisms where student loan reforms are part of the agenda, as mentioned above.

Our results reveal that the 2012 Chilean reform had no effect on overall immediate enrollment (i.e., enrollment in any HES institution during the year immediately following high school graduation). However, and surprisingly, we find a diversion effect whereby enrollment in universities increased by 2.5 percentage points (pp.)—which amounts to a 7 percent increase relative to enrollment of nonexposed eligible individuals—to the detriment of enrollment in vocational institutions, which fell by 2.5 pp.—equivalent to a 14 percent decrease in enrollment relative to the same group. This effect is stable over time except for a decrease in 2015 when the government announced a new tuition-free program. This shift in institutional choice is explained—in line with Angrist et al.

[5]See Dearden, Fitzsimons and Wyness (2014), Nielsen, Sørensen and Taber (2010) and Dynarski (2003) as examples of papers that study the effects of reforms on types of financial aid other than student loans.

(2016)—by the implicit subsidy the reform creates for universities relative to vocational institutions given that the former are more expensive in terms of tuition fees and program length. Moreover, the diversion effect might entail welfare effects since some individuals who diverted their decision toward universities would be likely better off had they pursued a vocational degree instead (Rodriguez, Urzua and Reyes, 2016).

Our findings are consistent with the evidence on the enrollment effects of financial aid in general (e.g., Gurgand, Lorenceau and Mélonio, 2023; Carruthers and Welch, 2019; Park and Scott-Clayton, 2018; Fitzpatrick and Jones, 2016) and particularly with the Chilean evidence on the effects of granting access to student loans.[6] Using a regression discontinuity design (RDD), Solis (2017) and Montoya, Noton and Solis (2018) find that loan eligibility increases university immediate enrollment by 18 pp. and 15.2 pp., respectively, although these results apply only for individuals with scores around the eligibility threshold on the national admission test. In addition, our results are smaller because we analyze a reform on the intensive rather than the extensive margin.

Regarding retention, we find that as a result of the 2012 changes, the diversion from vocational institutions to universities also encouraged enrollment in universities for a second consecutive year, increasing it by almost 2 pp.—a 7 percent increase relative to nonexposed eligible individuals—with no significant changes in two-year enrollment for vocational institutions. Conditional on being enrolled, we also estimate that the second-year dropout rate from universities decreased by approximately 3.5 pp. (a 32 percent decrease), in contrast with a considerably smaller effect of almost -1 pp. (-4%) for vocational institutions. These asymmetric findings across institutions result from two mechanisms: a sorting effect in ability, caused by the diversion effect in enrollment that reduces the likelihood of dropping out of universities while increasing dropout in vocational institutions (Rodriguez, Urzua and Reyes, 2016), and a perverse incentive from the student loan program itself that encourages all institutions to reduce dropout rates given their guarantor role (Rau, Rojas and Urzúa, 2013).

The evidence from the international literature on the retention effects of financial aid is mixed. For example, Denning (2019), Chatterjee and Ionescu (2012), and Glocker (2011) discuss the importance of financial aid for retention and completion; however, Cohodes and Goodman (2014), Stinebrickner and Stinebrickner (2012), Stinebrickner and Stinebrickner (2008), and Herzog (2005) find that there are other factors that are more relevant than financial constraints for retention and graduation. Our results are consistent with Chilean evidence. Card and Solis (2022) and Solis (2017) find an increase between 16 and 20 pp. in university two-year enrollment. Our result is smaller, but again, their findings apply for selected individuals only and considering access to the loan instead of changes to program parameters. Rau, Rojas and Urzúa (2013) build a structural model for sequential schooling decisions and find that access to this particular loan reduces dropout rates in both universities and vocational institutions.

Finally, we also examine the possibility of heterogeneous effects across two dimensions, namely type of school, which proxies for socioeconomic background, and sex. We find that virtually all our results in enrollment and retention are entirely driven by (middle-income) students graduating from voucher high schools with no effects whatsoever on (low-income) students from public schools. This low reform coverage, in the sense that it does not reach the most disadvantaged students, might result from the facts that (i) the loan does not cover full tuition costs, so that students still need

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

to finance the remaining difference along with other expenses, which is arguably harder for poorer students, and (ii) public-school students perform systematically worse on the national admission test that also determines loan eligibility. Ultimately, the 2012 reform is not large enough to have an impact on low-income students.

Regarding sex, we find a significant difference between men and women in enrollment, such that female diversion from vocational institutions (-3 pp.) is not fully compensated by the increase in female university enrollment (2.2 pp.), in contrast with full compensation for males. This introduces a new dimension of gender inequality. As a result, the 2012 reform had a negative impact on immediate enrollment for women (-0.9 pp.), which seems to be explained by them delaying their enrollment decisions. This behavior might be an optimal response since eligibility criteria are harder to meet for university enrollment and female students obtain systematically lower scores on the national admission test.

In summary, our findings suggest that a reform that loosens financial constraints through the introduction of intensive margin changes to student loans might have important unintended consequences in terms of the composition of enrollment across institutions, limited coverage for low-income students, and gender inequality in education decisions. These unexpected effects could in turn translate into nontrivial welfare effects and even backfire on the reform's intended objective of improving repayment rates because of the diversion to longer and more expensive university programs.

The remainder of this paper is organized as follows. Section 2 describes the institutional background of the Chilean HES, the changes introduced to the loan in 2012, and our data. The empirical strategy for identification of the effects on educational outcomes is presented in Section 3, while Section 4 presents the results for all our outcomes and analyzes the plausibility of the identification strategy. Section 5 implements an alternative identification strategy to support our main results, while Section 6 explores heterogeneous effects. Finally, Section 7 concludes the paper.

2 Background and Data

The Chilean Higher Education System (HES) comprises two types of institutions: universities and vocational institutions (*Institutos Profesionales* and *Centros de Formación Técnica*). Universities offer professional programs and are the only institutions entitled to confer academic degrees. Programs at universities are usually between 5 and 6 years in length. Vocational institutions, on the other hand, offer technical programs that are mainly between 3 and 4 years in duration. Both types of institutions are financed primarily through tuition fees, with the state providing complementary funding by direct and indirect mechanisms assigned almost entirely to universities.

Tuition fees imply an important financial burden for high school graduates who decide to enroll, since they represent a large fraction of family income. Between 2007 and 2015, the period of analysis in this paper, the mean tuition fee in the 62 Chilean universities was approximately $CLP 2.1 million ($USD 2,970), which represents 41% of the median family income in 2015.[7] For the more than 100 vocational institutions, the mean tuition fee was approximately $CLP 1.1 million ($USD 1,556), representing 21% of the 2015 median family income.

[7] Median family income is calculated in all cases using the household survey *Caracterización Socioeconómica Nacional* CASEN 2015. Conversion from $CLP to $USD uses the official exchange rate as of 12/31/2015.

This is of special relevance for students graduating from state-funded public schools and from voucher schools.[8] In the same time period, 39% of the students came from public schools, and the mean tuition fee represented 42% and 22% of the median family income for universities and vocational institutions, respectively. Similarly, 53% of the students graduated from a voucher school, and the mean tuition fee for universities represented 34% of the median family income and 18% in the case of vocational institutions. Finally, for the remaining 8% of students graduating from private high schools, the mean tuition fee represented 10% and 5% of the median family income for universities and vocational institutions, respectively. In the results section, we assess how the reform heterogeneously impacts graduates from public schools versus graduates from voucher schools.

Students have few options to finance tertiary education. Work-and-study or work-and-save are usually very demanding alternatives, and access to the conventional financial market is typically limited by restrictive conditions on income and job formality. That is why students rely on government grants as their principal source of funding, where eligibility is mostly determined by academic performance and socioeconomic characteristics such as family income. In 2015 for example, of a total of 1,165,654 students enrolled in the Chilean HES, 723,216 (58%) had some form of government financial aid. That same year, the government granted 443,299 loans (38%) and 397,386 scholarships (34%) (Ministry of Education, 2016).

Scholarships cover tuition and, in some cases, enrollment fees and other costs such as transportation and food expenses. Student loans, on the other hand, cover tuition fees only.[9] Students have access to two types of loan: the traditional university loan or FSCU (*Fondo Solidario de Crédito Universitario*) and the state-guaranteed loan or CAE (*Crédito con Aval del Estado*). The FSCU loan is granted by the state only to students who enroll in the so-called "traditional" universities, has an annual interest rate of 2% with payments that begin two years after graduation, and contemplates a maximum of 15 years of payments with a cap of 5% of total income.[10] The CAE loan is provided, administered, and collected by private banks and guaranteed by the state and the higher education institution where the student is enrolled. Payment conditions, such as the interest rate, changed in the 2012 reform and are described in detail below.[11]

Of all the types of financial aid the government grants to students, the CAE loan is the most important, both in number of beneficiaries and amount granted, as shown in Table 1. In fact, one in every three tertiary education students has a CAE loan to pay for tuition fees. These figures hint at the public policy relevance of analyzing the effects of the 2012 reform to CAE.

2.1 The CAE Loan and the 2012 Reform

The CAE loan was introduced in 2006 as an alternative to the conventional FSCU loan that was granted only to students enrolled in traditional universities. The main goal of the policy was to broaden access to the HES regardless of the chosen institution (i.e., university or vocational institution). Participants in the CAE system are (i) private banks lending the money, (ii) the

[8] See Aguirre (2022) for details on the Chilean voucher system.

[9] Moreover, loans only cover tuition fees up to a maximum amount called the "referential tuition fee," which is annually determined by the Ministry of Education for each program based on its quality.

[10] "Traditional" universities, or more formally *Universidades del Consejo de Rectores*, are the group of 27 universities created before 1980.

[11] See Beyer et al. (2015) for additional information on the structure of the Chilean student loan system.

Table 1: Government Grants in 2015

	Quantity		Total Amount	
Scholarships	**397,386**	**47.27%**	**483,597**	**49.80%**
Beca Centenario	99,930	11.89%	240,974	24.81%
Beca Nuevo Milenio	171,576	20.41%	96,362	9.92%
Beca de Articulación	5,557	0.66%	3,892	0.40%
Beca Juan Gómez Millas	63,474	7.55%	70,545	7.26%
Beca Excelencia Académica y PSU	24,946	2.97%	26,859	2.77%
Beca de Nivelación Académica	3,466	0.41%	2,850	0.29%
Beca Hijos de Profesionales de la Educación	10,360	1.23%	5,104	0.53%
Beca Vocación de Profesor	9,555	1.14%	21,715	2.24%
Beca de Reparación	3,858	0.46%	6,222	0.64%
Beca de Reubicación U. del Mar	4,664	0.55%	9,074	0.93%
Loans	**443,299**	**52.73%**	**487,494**	**50.20%**
CAE	369,253	43.92%	415,951	42.83%
FSCU	74,046	8.81%	71,543	7.37%
Total	**840,685**	**100.00%**	**971,091**	**100.00%**

Notes: Ministry of Education, *Memoria Financiamiento Estudiantil 2016*. Quantity refers to the number of grants. Total Amount in CLP $MM.

government and educational institutions as guarantors absorbing the default and dropout risks, respectively, and (iii) the students/debtors who borrow and make repayments accordingly.

The process of CAE loan applications and HES enrollment is structured as follows. Students graduating from high school register for the PSU (*Prueba de Selección Universitaria*), a national college admission test that highly determines admission to the HES and access to grants.[12] During the PSU registration process, individuals planning to apply for the CAE loan (or other grants) must complete a socioeconomic form that is used to determine income eligibility. Once test results are published, academic eligibility is determined, loans are granted, and students decide whether to enroll in their respective programs.

To become a beneficiary of the CAE loan, a high school graduate must fulfill both the academic and family income eligibility criteria. Only students with a PSU score greater than or equal to 475 or a high school GPA greater than or equal to 5.3 are eligible.[13][14] The socioeconomic criterion is the less relevant of the two since it has changed over time and students do not ex ante know what the cutoff is because the state sorts applicants by income and grants the loans up to the available budget. In 2007, the first year of analysis in this paper, the CAE loan covered up to the fourth income quintile, and it has been granted solely based on academic criteria since 2014, covering applicants from all socioeconomic conditions.

[12] The PSU is administered once per academic year and consists of two mandatory (language and mathematics) and two optional tests (science and history/social science; one must be chosen). PSU scores range from 150 to 850 points and are normalized to have a mean of 500 and standard deviation of 110 points. The student's average score on the mandatory tests is typically used to assess eligibility for grants.

[13] GPA ranges from 1 to 7.

[14] If a student wishes to enroll in a university, they have to comply with the PSU cutoff, but if they wish to enroll in a vocational institution, they have to comply with either of the thresholds.

Initially, the CAE loan was granted under conditions similar to those of a conventional loan in the financial sector with market interest rates, payments not contingent on income, and banks legally entitled to use mechanisms to collect debts. CAE loans have maturity up to 20 years with payments beginning 18 months after graduation, and between 2006 and 2011, they had an average annual interest rate of 5.6 percent. In mid-2011, the government announced a reform to the CAE loan that came into effect in 2012. The changes introduced were (i) a new fixed annual interest rate of 2 percent, similar to that of the FSCU and with the government subsidizing the difference from the market interest rate; (ii) repayments contingent on income upon request, with a cap of 10% and the government subsidizing the remaining difference; and (iii) the possibility, upon request, to delay payments in case of unemployment. With these changes, the government intended to align the conditions between the two loans and expected to improve repayment following a report that estimated a default rate of 36% and predicted a possible increase to a 50% rate (World Bank, 2011), similar to Scott-Clayton (2018)'s default rate predictions for the U.S.

From a theoretical perspective, this reform represents a loosening of credit constraints since individuals initially faced tighter repayment conditions that were relaxed in 2012 and implied a reduction of educational costs (in present value). Of the three changes introduced, the interest rate decrease is the most relevant given that the subsidized reduction is automatically applicable to all loans, while the 10%-of-income cap subsidy for repayments and the option to delay them in the event of unemployment are available upon request, and only a small fraction of debtors have applied for them since its implementation. In 2015, for example, 8% and 4% of the 242,604 CAE debtors were beneficiaries of the 10% cap and delayed repayments, respectively (Ingresa, 2015). Moreover, the decrease in the interest rate is considerable in terms of the present value of repayment flows. To illustrate its potential implications, consider the following scenario. A student applying for a CLP$ 2.1 million annual loan at the former 5.6% interest rate would owe a total of CLP$ 15.7 million at the end of a 6-year program and after the 18-month grace period. With a 20-year maturity loan, this is equivalent to an annuity of CLP$ 1.3 million. With the new interest rate of 2%, they would instead owe a total of CLP$ 13.6 million (a 13 percent decrease) with an annuity of CLP$ 0.8 million, which represents a nontrivial decrease of 37 percent.[15]

This reform constitutes a change in the intensive margin of credit access rather than an extensive margin change such as the introduction of the CAE loan itself. It is important to analyze the potential effects of such intensive margin changes on educational attainment, especially when these changes are substantial as in the 2012 reform.

2.2 Data and Sample

The application process for financial aid is highly centralized in Chile, allowing us to use nationwide administrative records that contain information about the entire population of high school graduates, along with their eligibility status and enrollment choices in any given year. We obtained information from three sources.

The first is the student performance and graduation databases from the Ministry of Education that comprise records of all students enrolled in secondary education, from which we build our

[15] Several assumptions are implicit in this example for the sake of simplicity. To name a few, we assume that the student requests the same amount every year, that the loan is granted on an annual basis along with the future repayments, that there is no inflation, and that the debtor does not request contingent nor delayed payments.

universe of high school graduates. This source contains relevant information about the students and their high schools. Our second source of information is DEMRE (*Departamento de Evaluación, Medición y Registro Educacional*), the institution in charge of the PSU process. They provided us with the PSU scores for all test takers in our period of analysis. Our third data source from the Ministry of Education provides individual information about enrollment decisions in all universities and vocational institutions. Merging all the data through an individual identifier, we build a dataset consisting of every cohort of high school graduates and information on their eligibility and enrollment histories.

We limit our analysis to the 2007–2015 cohorts (i.e., high school students graduating between 2006 and 2014) for two reasons. In 2006, the first year of implementation, the government misassigned the CAE loan due to an error in the income sorting of applicants, granting loans in the opposite order (Ingresa, 2010). Second, the government introduced a new program in 2016 that made tuition-free tertiary education available for some individuals. The 2016 reform entirely changed the scenario for students regarding financial restrictions, which in turn could introduce a confounding factor into our analysis of the 2012 reform.[16]

In addition, care must be taken in using the entire population of high school graduates. As already discussed, income eligibility changes over time, and its threshold is not observed by the researchers nor the applicants. To overcome this issue, we drop all graduates from private high schools from our sample to match income eligibility compliance as closely as possible, implying the exclusion of only 8% of the entire population. By doing so—i.e., conditional on being socioeconomically eligible—we exploit eligibility on the academic dimension only. A second concern pertains to high school graduates who do not register to take the PSU test, preventing us from determining their eligibility through the PSU score channel. For this reason, we additionally restrict our sample to registered students only.

3 Empirical Strategy

Following a simple model of human capital accumulation with imperfect credit markets, state-funded programs such as scholarships and loans increase the net present value of investment in the education project by reducing the associated costs and, therefore, raise the enrollment choice probability (Johnson, 2013; Long and Riley, 2007; Becker, 1964). Although the changes introduced in the 2012 reform affected the intensive margin and focused on the repayment period, the decrease in the interest rate is substantial enough to motivate the investigation of the enrollment effects, given the implied reduction in the costs associated with the educational project. To identify these causal effects, we use a DiD approach exploiting the timing of the reform and the loan's academic eligibility conditions.

3.1 Immediate Enrollment

Our first and main outcome of interest is immediate enrollment, defined as the choice of enrollment in the year immediately following high school graduation. As the CAE loan is constrained to eligible

[16]See Espinoza and Urzúa (2015) for an initial evaluation of the new tuition free program and Castro-Zarzur, Espinoza and Sarzosa (2022), Bucarey (2017) for an analysis of other educational effects.

individuals only, our treatment group is the sample of eligible individuals from cohorts 2012–2015 since they are the only ones exposed to the reform.

Our first difference is the comparison between the treatment group and nonexposed eligible students (i.e., eligible individuals from the 2007–2011 cohorts). The difference in enrollment between these two groups cannot be uniquely attributed to the reform since it could be partially explained by other confounding factors. To solve this issue, our second difference in enrollment is that between the corresponding two groups of cohorts of ineligible individuals. As their decision is not affected by the reform, any difference between the 2007–2011 and the 2012–2015 cohorts will capture those potential confounders.

With this DiD model, we implicitly assume that the average remaining difference in unobservables between eligible and ineligible individuals is the same before and after the 2012 changes; this assumption is commonly known as the parallel trends condition. In the results section, we present evidence of the plausibility of this assumption.

Following standard practice, our base estimation 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 eligible_{it} is an indicator of CAE eligibility for high school graduate i in cohort t and exposed_{it} indicates exposure to the reform (i.e., $t \geq 2012$). Note that our empirical design corresponds to a standard difference-in-differences model for repeated cross-sectional data since each individual is considered only in the corresponding year of their immediate enrollment decision. Moreover, treatment occurs at a single point in time—i.e., year 2012—and control units—i.e., ineligible students—are never treated.

Immediate enrollment, y_{it}, is to be captured by three binary variables. The first is overall enrollment, which equals 1 when individual i enrolls in the HES, regardless of the type of institution chosen, and 0 if they do not enroll. Our second binary variable is university enrollment, taking value 1 when the individual enrolls in a university and 0 otherwise (i.e., if they enroll in a vocational institution or do not enroll at all). Similarly, our third variable is vocational enrollment, an indicator activated when the high school graduate enrolls in a vocational institution. We follow this strategy to capture any compositional effects in enrollment between these two types of institutions.

In this model, the interaction coefficient for $\text{eligible}_{it} \times \text{exposed}_{it}$ (i.e., β_3) captures the intention to treat (ITT) effect of the reform on the enrollment rate. This model will also be extended to include cohort fixed effects and other relevant covariates as robustness checks for our baseline model.

A second specification of our DiD identification strategy is:

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

where the exposed_{it} variable is replaced by the cohort fixed effects cohort_{jit}. This model is useful because it disaggregates the overall effect into yearly effects, providing information about the dynamics. In this case, the coefficients β_j of the interaction $\text{eligible}_{it} \times \text{cohort}_{jit}$ for $j \in \{2012, \ldots, 2015\}$

are those of interest since they capture the evolution of the effect over time. Moreover, the remaining β_j coefficients (i.e. those for $j \in \{2007, \ldots, 2011\}$) are also of particular interest since they allow us to test for differential pre-trends to assess the plausibility of the parallel trends assumption.

3.2 Two-year Persistence and Retention

Our second and third outcomes focus on persistence and retention decisions, respectively. We define two-year enrollment as a binary variable that takes value 1 if the high school graduate immediately enrolls for two consecutive years and 0 otherwise, which includes the scenarios of enrollment for one year only or no enrollment. We restrict our sample to programs with a length of at least one year, since two-year enrollment is undefined otherwise. Two-year enrollment provides a measure of persistence that comprises information about the immediate first-year decision to enroll along with information on the decision to continue into the second year of enrollment. To disentangle this information and learn about the marginal effect on the second-year decision, we employ our third and last outcome, second-year dropout.

Analysis of dropout decisions is conditional on being enrolled: our subsample comprises all high school graduates who immediately enrolled in the HES in the 2007–2014 period, and we are interested in their dropout decision for the following year. As we only have enrollment records at the beginning of each period, we do not observe whether a student completed the year. For this reason, we define second-year dropout as a binary outcome that takes value 1 if we do not observe a student's registration at the beginning of their second year, regardless of whether they completed their first academic period.[17]

As with the immediate enrollment outcome, we use the DiD framework presented in Equation (1) and focus on three variables—overall, university, and vocational—for both two-year enrollment and second-year dropout. These models are also extended to include cohort fixed effects and other student- and program-specific control variables. Furthermore, we implement specifications as in Equation (2) to learn about the dynamics of the effects and to validate our identification strategy.

The first difference in our DiD setting comes from the comparison between eligible students who were exposed and those who were not exposed to the reform. Note that in this case, the first cohort that was exposed is the 2011 cohort (and not the 2012 cohort), since these are the first individuals whose decision regarding second-year enrollment is made under the new loan conditions. For this reason, exposed cohorts are now those from 2011 to 2014, while unexposed cohorts are those from 2007 to 2010.[18] To isolate the potential confounding differences between these two groups of cohorts, we use the difference in enrollment for ineligible students between periods of exposure and nonexposure as our second difference.[19]

[17] Persistence and retention are defined at the system level—i.e., HES, universities, or vocational institutions—not at the program level. For instance, a student from cohort t who enrolls in university A in year t and moves to university B in year $t+1$ is not considered a dropout in our definition, even when they dropped out of their initial program.

[18] Note that for this specification, we exclude the 2015 cohort since their second-year decisions might also be affected by the free tuition program introduced in 2016.

[19] A concern may arise regarding the potential endogeneity of our eligibility variable in this two-year setting. To address this issue, we run IV specifications in Appendix B and present evidence consistent with a negligible presence of endogeneity.

4 Main Results

This section presents and discusses our main results. For completeness and to better understand the Chilean context, Table 2 presents descriptive information for all cohorts. Our sample consists of approximately 1.5 million high school graduates, 40% of whom come from a public school and the remaining 60% come from a voucher school. The overall female/male ratio is 1.14. Eligibility for CAE loans has increased its coverage from 75% in 2007 to 81% in 2015.

Enrollment in the HES has an upward trend over time with an annual growth rate of 2.1%, mainly explained by growth in vocational enrollment (5.1% vs 0.1%). Overall, one-half of our sample of high school graduates immediately enrolls in the HES. Within our period of study, the gender gap in enrollment decreased from 2.5 pp. to 1.4 pp. A more subtle decrease is found in the enrollment gap between students from public high schools vs. students from voucher schools. The gap decreased from close to 9 pp. to nearly 6 pp.

In terms of persistence in the HES, 44% of high school graduates in our sample enroll for two consecutive years, with an annual growth rate of 2.3% and driven, once again, by vocational permanence (5.9% vs 0.3%). The gender gap was very small in 2007 and not only did it disappear, but at the end of the sample period, females are more likely than males to enroll for two years.[20] The trend in the gap by type of school is very similar to that of immediate enrollment, with students from public schools being nearly 8 pp. less likely to enroll for two years than students from voucher schools.

Regarding retention in tertiary education, 13% of students enrolled in the HES drop out in their second year of studies, with the dropout rate decreasing over time. In every year of our period of study, females are less likely to drop out than males. The gap in dropout rates by type of school has remained stable over time at approximately 3 pp.

The following subsections present the estimation results of the models discussed in the previous sections. All regressions follow a linear probability model with standard errors clustered at the class level to account for intraclass correlation. In this setting, a class is defined as the corresponding cohort graduating from a specific high school in a given year.

[20] See Dao, Dávila and Greulich (2021), Bertocchi and Bozzano (2020), and Becker, Hubbard and Murphy (2010) for an analysis of the overtaking of men by women in higher education.

Table 2: Descriptive Statistics

	HES Application Process Cohort									
	2007	2008	2009	2010	2011	2012	2013	2014	2015	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Immediate Enrollment	0.464	0.463	0.464	0.475	0.494	0.521	0.547	0.552	0.549	0.505
by Institution										
Universities	0.299	0.297	0.281	0.279	0.290	0.304	0.301	0.301	0.302	0.295
Vocational	0.167	0.167	0.183	0.197	0.204	0.218	0.247	0.252	0.248	0.211
by Gender										
Females	0.478	0.480	0.469	0.477	0.493	0.518	0.546	0.556	0.557	0.510
Males	0.453	0.449	0.459	0.474	0.494	0.524	0.548	0.549	0.543	0.501
by High School										
Public	0.503	0.493	0.492	0.504	0.521	0.551	0.572	0.574	0.571	0.534
Voucher	0.416	0.422	0.425	0.435	0.453	0.468	0.503	0.514	0.513	0.460
Two-Year Enrollment	0.398	0.402	0.406	0.412	0.425	0.454	0.473	0.478		0.432
by Institution										
Universities	0.258	0.259	0.248	0.247	0.255	0.267	0.262	0.264		0.257
Vocational	0.125	0.130	0.145	0.153	0.155	0.170	0.194	0.198		0.160
by Gender										
Females	0.406	0.413	0.407	0.408	0.419	0.447	0.469	0.477		0.432
Males	0.391	0.394	0.405	0.416	0.431	0.459	0.477	0.479		0.433
by High School										
Public	0.438	0.433	0.437	0.444	0.456	0.486	0.502	0.504		0.465
Voucher	0.347	0.360	0.364	0.368	0.379	0.397	0.422	0.433		0.383
Second-Year Dropout	0.143	0.131	0.122	0.131	0.138	0.129	0.135	0.132		0.132
by Institution										
Universities	0.135	0.125	0.113	0.111	0.120	0.120	0.127	0.117		0.121
Vocational	0.250	0.221	0.209	0.222	0.237	0.220	0.215	0.212		0.222
by Gender										
Females	0.150	0.140	0.129	0.143	0.150	0.136	0.141	0.140		0.141
Males	0.137	0.122	0.116	0.122	0.126	0.122	0.129	0.126		0.125
by High School										
Public	0.128	0.120	0.110	0.118	0.124	0.118	0.122	0.120		0.120
Voucher	0.166	0.147	0.141	0.153	0.161	0.151	0.160	0.156		0.155
Eligible	0.755	0.780	0.768	0.772	0.767	0.769	0.781	0.794	0.814	0.778
PSU	475.759	475.829	475.638	473.877	476.538	475.305	476.784	477.304	479.135	476.263
GPA	5.567	5.601	5.582	5.584	5.579	5.593	5.609	5.641	5.681	5.605
Female	0.540	0.546	0.536	0.531	0.526	0.534	0.531	0.532	0.528	0.533
Public School	0.442	0.422	0.422	0.420	0.405	0.360	0.362	0.364	0.365	0.394
Observations	140,142	143,399	167,166	175,526	180,774	167,409	173,111	173,168	176,684	1,497,379

Notes: Cohort 2015 is not considered for two-year enrollment and second-year dropout as discussed in Footnote 18. For the same reason, the pooled-sample statistics for these variables are computed excluding cohort 2015.

To assess the relative sizes of our estimates, we report the respective number of nonexposed eligible individuals and their outcome mean in most tables. As a robustness check for our main specification, we add year effects and three types of control variables to our base models. Student-level variables include gender, attendance rate, district of residence, and number of family members at different levels in the education system. School-level variables include indicators of school type (public or voucher), rural area, and geographical region. Finally, program-level covariates—which are included only in the regressions for second-year dropout—include tuition fee, accreditation status, and program length.

4.1 Effects on Immediate Enrollment

Table 3 presents the results for our three immediate enrollment variables: overall, university, and vocational institution enrollment. Columns (1), (4) and (7) show the results for our base model as presented in Equation (1). Estimation results including cohort fixed effects are displayed in Columns (2), (5) and (8), while Columns (3), (6) and (9) also include student and high school control variables.

Eligible students are more likely to enroll. This is not only due to CAE's availability but also because they are potentially eligible for other grants and/or the FSCU loan. Moreover, as eligibility is determined by academic variables, which are arguably related to ability, the results suggest that more able students are more likely to enroll. However, when we disaggregate by type of HES institution, we find that this result is driven by university enrollment: eligible students are more likely to enroll in universities and slightly less likely to enroll in vocational institutions. This could be explained by the higher economic returns associated to college degrees but could also be understood in a comparative advantage framework in a Roy selection model. The coefficient on the *exposed* variable captures the trend in enrollment over time, as already discussed.

The overall enrollment effect of the reform is neither statistically nor economically significant, suggesting that the loosening of credit constraints had no impact on immediate enrollment. Interestingly, we find a diversion effect when we conduct our analysis separately by type of institution: the reform increased enrollment in universities at the expense of vocational institutions by 2.5 pp. In absolute terms, this result implies that approximately 15,500 of 620,206 individuals shifted their enrollment decision toward universities instead of vocational institutions. This finding is robust to the inclusion of different sets of covariates and roughly amounts to a 7 percent increase in university enrollment and a 14 percent decrease in vocational enrollment relative to the enrollment rate of nonexposed eligible individuals.

Our results are consistent with others found in the literature, although of a smaller magnitude. By means of an RDD, Solis (2017) uses cohorts 2007–2009 to estimate the effects of crossing the 475 PSU-score threshold, which enables loan eligibility, and finds that immediate enrollment in universities increases by 18 pp., close to a 100 percent increase relative to ineligibles. Following a similar RDD with the same three cohorts, Montoya, Noton and Solis (2018) analyze the labor market effects, and in their model also estimate the effects on different measures of enrollment. The authors find that scoring above the 475 cutoff has a positive effect of 9.6 pp. on overall immediate enrollment and 15.2 pp. on university immediate enrollment, arguing that most of this variation is a reflection of a vocational-to-university substitution. Similarly, Bucarey, Contreras and Muñoz (2020) also find a compositional effect for the extensive margin of loan access, with students switching from high-quality vocational institutions to universities of a lower quality.

In contrast, Aguirre (2021) finds that access to loans on top of grants for vocational education increases the probability of enrolling in a vocational institution while decreasing the probability of enrolling in a university in the short run. These apparently conflicting results can be reconciled by noting that the running variable in the author's RDD is the student's GPA, so that crossing the 5.3 threshold enables CAE eligibility for vocational institutions only. The papers mentioned above focus on the PSU-score 475 threshold, enabling CAE eligibility for any institution, while here we consider eligibility as determined by both the PSU and GPA dimensions.

Table 3: 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***	-0.031***	0.075***	0.103***	0.106***
	(0.003)	(0.007)	(0.006)	(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 control variables include indicators of school type, rural area and geographical region. Student level control variables include gender, attendance rate, district and number of family members at different levels in the education system. Control group size accounts for the number of ineligible individuals in the exposure period, while Outcome mean refers to the mean of the dependent variable of those individuals.

Our analysis differs from the existing literature in two ways. First, we focus on a reform that introduced changes on the intensive margin (i.e., an interest rate reduction that loosens credit constraints), while others analyze the effects of having access to the CAE loan itself (i.e., the extensive margin). Second, in the RDD framework, the results are local in the sense that they are interpreted as treatment effects for individuals near the threshold (i.e., those with a PSU score close to 475 points), while our results are interpreted as an average for the treated individuals.

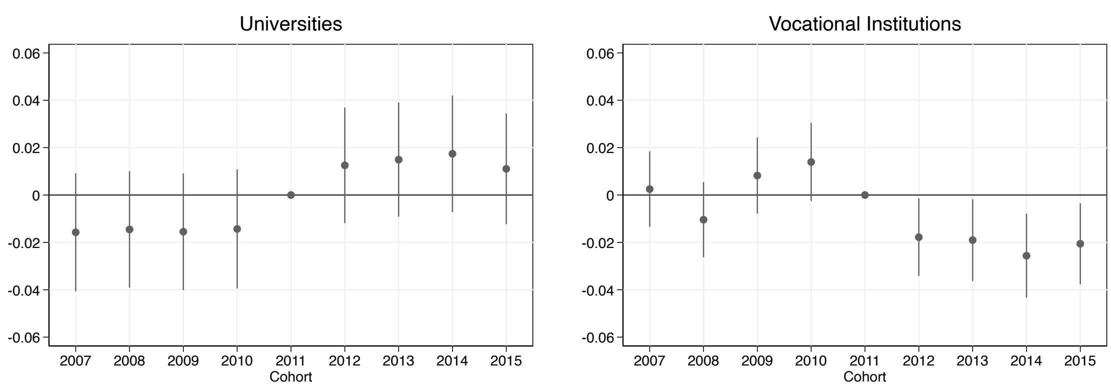
In line with Angrist et al. (2016), the shift in institutional choice from vocational institutions to universities is explained by the implicit subsidy that the interest rate reduction creates for universities. As enrolling in this type of institution is costlier both in pecuniary (i.e., tuition fees) and time (i.e., program length) terms, the financial effect of the interest rate decrease is proportionately larger for the choice of attending universities, which in turn further increases the relative incentive to enroll in a university in comparison to a vocational institution.

In addition, this institutional diversion might entail a welfare effect that depends on the characteristics of the individuals who shifted their enrollment decision toward universities as a result of the 2012 reform. Rodriguez, Urzua and Reyes (2016) propose a structural schooling decision model to simulate the effects of reduced tuition costs in Chile—which can be interpreted as a loosening of credit constraints and therefore similar to the interest rate decrease—and find negligible effects on overall enrollment, which is consistent with our results. Moreover, the authors find for Chile that

(i) more able students obtain lengthier degrees (i.e., pursue degrees at universities instead of vocational institutions); (ii) economic returns (annual earnings) are increasing in ability and are larger for students graduating from university than for those graduating from vocational institutions; (iii) the ability-earnings gradient is steeper for vocational degrees than for university degrees; and, as a consequence, (iv) that there is a nontrivial likelihood of obtaining negative returns for university graduates since a large fraction of them would have received higher earnings had they chosen a vocational institution instead. In our setting, this suggests that individuals deciding to enroll in a university instead of a vocational institution as a consequence of the 2012 reform are marginally more able (a sorting effect), but some of them would be likely better off had they pursued a vocational degree instead. See the cost-benefit analysis in Aguirre (2021) for an illustration of the potential negative impact of institutional diversion on expected earnings.

Figure 1 presents the dynamics of the effect on immediate enrollment by displaying the β_j interaction (i.e., eligible$_{it}$ $\times$ cohort$_{jit}$) coefficient estimates described in Equation (2), along with their corresponding 99% confidence intervals. Detailed estimation results and robustness checks are presented in Appendix A. The left panel depicts the evolution of the effects on university enrollment, while the right panel does the same with vocational enrollment. In both cases, we observe a sharp change in the signs of β_j following the 2012 reform: university enrollment increases, while vocational enrollment decreases. These effects are stable over time, with a small decrease in magnitude in 2015 when the new tuition-free program was announced for 2016. In addition, the estimated interaction coefficients for cohorts 2007 to 2011 provide a highly demanding test for differential pre-trends. For each year prior to the reform, we cannot reject the null hypothesis that $\beta_j = 0$ for both the university and vocational enrollment variables.[21]

Figure 1: Dynamics of Immediate Enrollment



Notes: Point estimates of the β_j coefficients in Equation (2) with their 99% confidence intervals. The base category is cohort 2011. The estimation results in display correspond to the baseline specification without covariates. The results for immediate enrollment in universities are displayed on the left panel. The results for immediate enrollment in vocational institutions are displayed on the right panel.

[21] Appendix A presents additional evidence in favor of the parallel trends assumption for all our outcomes.

4.2 Effects on Persistence and Retention

We next turn our attention to the effects on second-year persistence and retention in tertiary education as measured by our two-year enrollment and second-year dropout variables, respectively. Table 4 presents estimation results for two-year enrollment.[22] Just as with the immediate enrollment results, Columns (1), (4) and (7) display the results for our base model as in Equation (1). Columns (2), (5) and (8) add cohort fixed effects, and Columns (3), (6) and (9) add further control variables. In this case, we find a positive effect on overall two-year enrollment of approximately 2 pp., virtually entirely driven by university two-year enrollment.

The 2 pp. effect on university two-year enrollment results from the combination of the immediate enrollment increase discussed in Subsection 4.1 and the decrease in second-year dropout that we present and discuss below. These two effects boost university two-year persistence, producing a relative effect of 7%. In contrast, for vocational institutions, the immediate enrollment effect that diverted students from these institutions to universities is offset by a reduction in second-year dropout. Thus, the combination of these opposing effects results in a null or slightly negative effect on vocational two-year enrollment.

Table 4: Two-Year Enrollment

	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × exposed (2nd year)	0.021***	0.020***	0.018***	0.022***	0.021***	0.020***	-0.004	-0.005*	-0.005**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Exposed (2nd year)	0.034***	0.054***	0.063***	-0.010***	-0.022***	-0.017**	0.043***	0.077***	0.082***
	(0.003)	(0.007)	(0.007)	(0.001)	(0.007)	(0.007)	(0.002)	(0.004)	(0.004)
Eligible	0.277***	0.277***	0.255***	0.271***	0.271***	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)
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,318,892	1,318,892	1,318,892	1,318,910	1,318,910	1,318,910	1,320,677	1,320,677	1,320,677
Control group size	480,876	480,876	480,876	480,879	480,879	480,879	481,614	481,614	481,614
Outcome mean	0.469	0.469	0.469	0.315	0.315	0.315	0.140	0.140	0.140

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 of school type, rural area and geographical region. Student level control variables include gender, attendance rate, district and number of family members at different levels in the education system. Control group size accounts for the number of ineligible individuals in the exposure period, while Outcome mean refers to the mean of the dependent variable of those individuals.

The results for second-year dropout in Table 5 allow us to further investigate the effects of the reform on two-year retention. Again, the columns differ in the inclusion of cohort effects and other control variables that now include program characteristics. Conditional on HES immediate enrollment, we find an overall effect of -1.3 pp. (-12%) on second-year dropout, suggesting that the reform increases overall retention. Among students enrolled in a university, the reform is correlated with a decrease of approximately 3.5 pp. in the dropout rate, which amounts to a 32% decrease relative

[22] See Appendix B for similar 2SLS results.

to nonexposed eligible individuals. For vocational institutions, on the other hand, we estimate a considerably smaller effect on the dropout rate of approximately -0.8 pp. (-4%).

This difference in retention effects across institutions—i.e., a sizable improvement in university retention but a substantially weaker improvement in vocational institutions—might be explained by two operating mechanisms. The first is the sorting effect created by the reform that diverts more able individuals to enroll in universities instead of vocational institutions, as discussed in the previous section. As a result, the ability distribution improves in universities but moves in the opposite direction in vocational institutions. Moreover, as ability is negatively correlated with the dropout probability as documented by Rau, Rojas and Urzúa (2013) and Rodriguez, Urzua and Reyes (2016), our retention measure improves more strongly for universities than for vocational institutions.

The second mechanism comes from a perverse incentive originated by the CAE loan itself. Rau, Rojas and Urzúa (2013) build a structural model with unobserved heterogeneity for sequential schooling decisions and find that access to this particular loan reduces dropout rates in both universities and vocational institutions; a reduction that the authors discuss is explained by the fact that the CAE loan creates incentives for institutions to reduce dropout rates given their role as guarantors.[23] As a result, we find that retention improves more in universities following this perverse incentive, which is boosted by the sorting effect, while the two mechanisms operate in opposite directions for vocational institutions.

Table 5: Second-Year Dropout

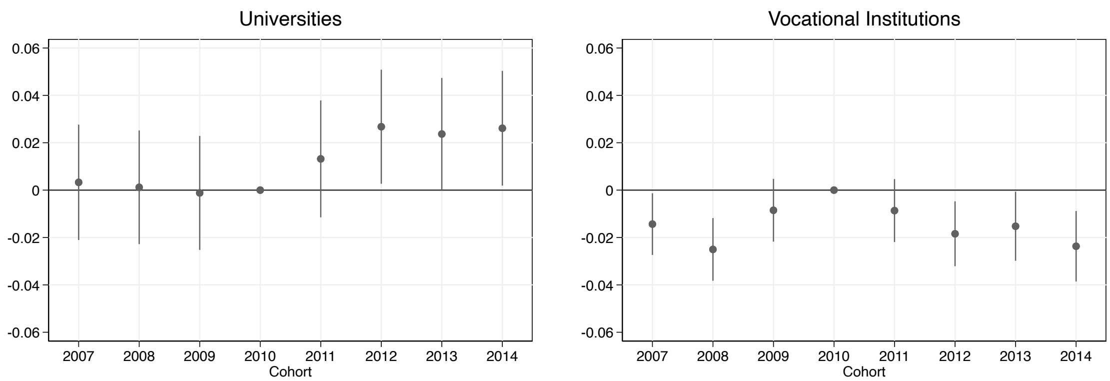
	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × exposed (2nd year)	-0.013***	-0.013***	-0.009**	-0.035***	-0.036***	-0.031***	-0.007	-0.007*	-0.010**
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Exposed (2nd year)	0.012***	0.003	0.013***	0.037***	0.026***	0.047***	0.002	-0.027***	0.006
	(0.004)	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)	(0.004)	(0.006)	(0.006)
Eligible	-0.183***	-0.183***	-0.130***	-0.235***	-0.233***	-0.175***	-0.139***	-0.139***	-0.115***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	657,479	657,479	644,831	386,140	386,140	374,422	272,124	272,124	271,154
Control group size	252,544	252,544	252,544	169,930	169,930	169,930	82,957	82,957	82,957
Outcome mean	0.107	0.107	0.107	0.108	0.108	0.108	0.188	0.188	0.188

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 of school type, rural area and geographical region. Student level control variables include gender, attendance rate, district and number of family members at different levels in the education system. Program characteristics include duration, annual fee, and an indicator for accreditation. Control group size accounts for the number of ineligible individuals in the exposure period, while Outcome mean refers to the mean of the dependent variable of those individuals.

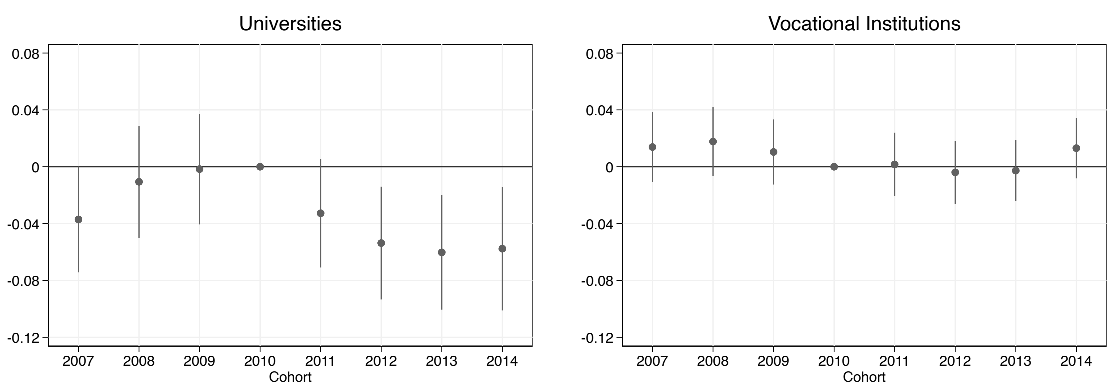
[23] As discussed in Section 2.1, higher education institutions in Chile are guarantors for CAE debtors until graduation and absorb the dropout risk. Rau, Rojas and Urzúa (2013) argue that " *[CAE loan] creates incentives for [institutions] to reduce dropout rates since they are obliged to repay if the lender drops out. In order to prevent students from dropping out, some [institutions] may lower their standards and shift resources to activities that are less successful at producing human capital but more attractive to students on the margin between continuing their education and dropping out.*"

Figure 2 presents the dynamics of the effects on our persistence and retention outcomes.[24] The top panel depicts the dynamics of the effect on two-year enrollment, and the bottom panel does so for second-year dropout. The effects for universities are shown in the left panels, and those for vocational institutions appear in the right panels. Of the 12 β_j interaction coefficients for $j \in \{2007, \ldots, 2010\}$, 10 are not statistically significant, providing strong evidence for the plausibility of the parallel trends assumption.[25] Regarding the university post-2010 coefficients, note that they are remarkably stable for cohorts 2012 to 2014 and of smaller magnitude in 2011 since that cohort is only partially exposed. This figure clearly shows the positive effect of the reform on persistence and retention in universities. For vocational institutions, in contrast, the effect is statistically null or at most slightly negative, as previously discussed.

Figure 2: Dynamics of Persistence and Retention



(a) Two-Year Enrollment



(b) Second-Year Dropout

Notes: Point estimates of the β_j coefficients in Equation (2) with their 99% confidence intervals. The base category is cohort 2010. The estimation results in display correspond to the baseline specification without covariates. Panel (a) displays the results for two-year enrollment in universities and vocational institution on the left and right sub-panels, respectively. The results for second-year dropout from universities and vocational institutions are displayed on the left and right sub-panels of panel (b), respectively.

[24] Detailed estimation results and robustness checks are presented in Appendix A.

[25] Appendix A presents additional evidence in favor of the parallel trends assumption for all our outcomes.

5 Alternative Identification Strategy

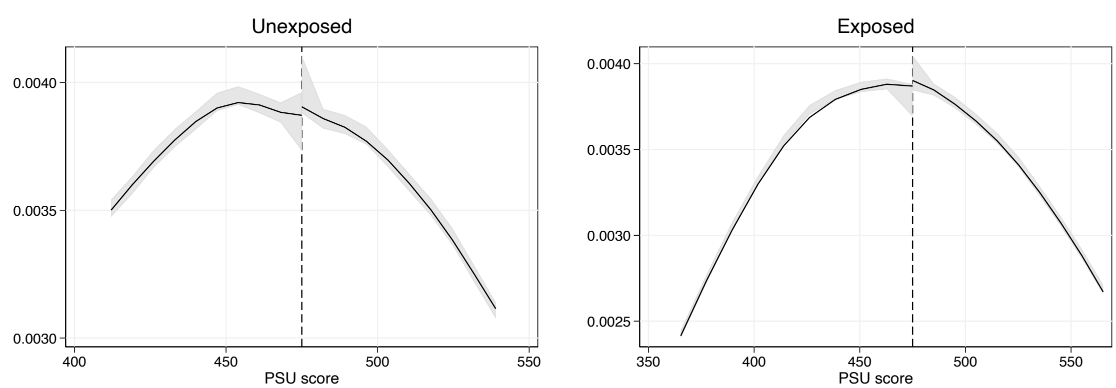
A related literature analyzing the extensive margin effects of student loans in Chile exploits the discontinuity around the 475 PSU threshold (e.g., Card and Solis, 2022; Solis, 2017; Bucarey, Contreras and Muñoz, 2020) or around the 5.3 GPA cutoff (Aguirre, 2021). This section presents additional Diff-in-Disc results for our enrollment, persistence, and retention outcomes for HES and for universities and vocational institutions separately as an alternative to our main DiD strategy.[26]

Following a standard RDD, we use PSU test scores as our running variable and compare eligible and ineligible individuals in a neighborhood around the 475 threshold. By doing so, we are essentially comparing similar students who only differ in their eligibility for the CAE loan, providing as-good-as-random local variation in eligibility (Lee and Lemieux, 2010; Lee and Card, 2008). Following Calonico, Cattaneo and Titiunik (2014), the local vicinity is determined by using optimally chosen bandwidths, and estimation is conducted by local linear regressions with triangular kernels.

We implement this RDD separately on exposed and unexposed cohorts and estimate the effect of the 2012 CAE loan reform as the difference between these two discontinuities. Because, in the case of vocational institutions, eligibility is also determined by GPA, we additionally conduct this analysis on the subsample of students with GPA < 5.3 to ensure that eligibility is only defined by the PSU threshold.

To verify the validity of our RDD approach, Figure 3 presents 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). As can be seen, we cannot reject the null hypothesis of continuity at the cutoff, suggesting an absence of manipulation of our running variable and, therefore, validating the strategy.

Figure 3: Density Test



Notes: Manipulation tests for PSU scores of unexposed (left panel) and exposed (right panel) students. The Cattaneo, Jansson and Ma (2020) test is based on their local polynomial density estimator.

Table 6 presents the Diff-in-Disc results on immediate enrollment. Column (2) shows a difference in the discontinuities in university enrollment for exposed and unexposed cohorts of 2.5 pp. in our full

[26]See Grembi, Nannicini and Troiano (2016) for details on this research design.

sample. In contrast, Column (6) shows a -2.2 pp. difference in vocational enrollment for students with GPA < 5.3. This diversion effect from vocational institutions to universities is remarkably similar to our main DiD results presented in Table 3 above.

Table 6: 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)
Exposed	0.074***	0.127***	-0.048***	0.062***	0.084***	-0.024***
	(0.004)	(0.005)	(0.005)	(0.009)	(0.007)	(0.009)
Unexposed	0.061***	0.102***	-0.040***	0.059***	0.061***	-0.002
	(0.004)	(0.004)	(0.004)	(0.008)	(0.007)	(0.007)
Bandwidth						
Exposed	51.257	36.629	41.201	48.882	47.259	43.601
Unexposed	51.142	40.393	51.088	45.712	48.539	55.572
Observations						
Exposed	117,087	84,280	94,582	27,136	26,254	24,260
Unexposed	140,696	111,784	140,552	35,855	37,995	43,254

Notes: Optimal bandwidths separately selected by exposure. Triangular kernel is used for local linear regressions. SUEST standard errors clustered at the class level in parentheses. *** p< 0.01, ** p< 0.05, * p< 0.1.

The results for two-year enrollment are presented in Table 7. The effect on HES persistence, represented by the difference in discontinuities in Column (1), is a 2.5 pp. increase, entirely driven by a 2.5 pp. increase in universities as displayed in Column (2). In Column (6), we find a statistically nonsignificant effect for vocational institutions. These results are consistent with our DiD findings presented in Table 4.

Table 7: Difference-in-Discontinuities Design: Two-Year Enrollment

	All students			GPA < 5.3		
	HES	Universities	Vocational	HES	Universities	Vocational
	(1)	(2)	(3)	(4)	(5)	(6)
Difference	0.025***	0.025***	-0.002	0.042***	0.030***	0.007
	(0.006)	(0.006)	(0.006)	(0.013)	(0.009)	(0.011)
Exposed	0.076***	0.107***	-0.038***	0.080***	0.072***	0.000
	(0.004)	(0.004)	(0.005)	(0.008)	(0.006)	(0.008)
Unexposed	0.051***	0.082***	-0.036***	0.039***	0.042***	-0.007
	(0.005)	(0.004)	(0.004)	(0.010)	(0.007)	(0.007)
Bandwidth						
Exposed	58.077	37.934	38.461	64.812	51.634	48.703
Unexposed	50.111	43.226	44.051	38.730	43.832	48.974
Observations						
Exposed	133,494	88,264	89,627	38,607	31,115	29,424
Unexposed	107,266	92,890	94,706	23,653	26,707	29,759

Notes: Optimal bandwidths separately selected by exposure. Triangular kernel is used for local linear regressions. SUEST standard errors clustered at the class level in parentheses. *** p< 0.01, ** p< 0.05, * p< 0.1.

Finally, Table 8 presents our Diff-in-Disc estimates for second-year dropout. In this case, all our point estimates are statistically null except for those in Column (6), which shows a 4.3 pp. decrease (significant at the 10% level only) in vocational dropout. These results should be interpreted with caution. First, these estimates are less precise due to the important reduction in sample size resulting from (i) conditioning on immediate enrollment, (ii) restricting to a small neighborhood around 475, and (iii) excluding students with GPA $\geq$ 5.3. Second, for reason (i) above, the sample is now self-selected, and these correlations are not necessarily causal effects, as mentioned in Subsection 4.2. Last, for reason (ii) above and as in a standard RDD approach, we are estimating the local effect only for students in a vicinity of the 475 cutoff, which might differ from the average effect across the whole distribution of test scores that we estimate with our main DiD approach.

The evidence presented in this section, where we exploit the same source of exogenous variation but employ different identification assumptions and a different empirical method, is remarkably consistent with our main findings in Section 4. These results reinforce the DiD design as a valid empirical strategy for evaluating the educational effects of the intensive margin changes introduced by the 2012 CAE reform. We now turn to heterogeneity analysis based on our main DiD strategy.

Table 8: Difference-in-Discontinuities Design: Second-Year Dropout

	All students			GPA < 5.3		
	HES	Universities	Vocational	HES	Universities	Vocational
	(1)	(2)	(3)	(4)	(5)	(6)
Difference	-0.002	0.003	-0.010	-0.017	-0.000	-0.043*
	(0.006)	(0.011)	(0.009)	(0.014)	(0.025)	(0.023)
Exposed	-0.008*	-0.017**	0.002	-0.029***	-0.009	-0.038**
	(0.004)	(0.008)	(0.006)	(0.010)	(0.018)	(0.016)
Unexposed	-0.005	-0.020***	0.012	-0.012	-0.009	0.005
	(0.004)	(0.008)	(0.007)	(0.010)	(0.017)	(0.016)
Bandwidth						
Exposed	54.348	51.297	46.782	50.644	54.499	31.156
Unexposed	59.024	53.361	50.914	51.482	45.883	40.089
Observations						
Exposed	69,669	30,248	32,749	15,517	6,386	5,968
Unexposed	61,304	27,849	26,649	14,229	5,728	6,197

Notes: Optimal bandwidths separately selected by exposure. Triangular kernel is used for local linear regressions. SUEST standard errors clustered at the class level in parentheses. *** p< 0.01, ** p< 0.05, * p< 0.1.

6 Heterogeneity

This section analyzes the extent to which the enrollment, persistence and retention effects of the reform are heterogeneous across two dimensions: student sex and school type. We approach this question by separately estimating Equation (1) for female and male students (Table 9) and for public and voucher schools (Table 10). Each table presents the estimated effects for each subsample and their difference, along with the corresponding class-level clustered standard errors, sample sizes, and outcome means. We perform seemingly unrelated estimation (SUEST) to allow for correlation between subsample estimates.[27]

6.1 Female vs. Male Students

The results in Table 9 suggest significant heterogeneity in immediate enrollment decisions across the sex dimension. While there is no significant difference in immediate university enrollment between female and male students, the impact of the reform on vocational enrollment is stronger for female students (negative for both males and females), with a difference of -1.3 pp. (significant at the 1% level).

[27]See Weesie (1999) for details.

Table 9: Heterogeneity of Main Results by Student Sex

	HES			Universities			Vocational		
	Female (1)	Male (2)	Difference (3)	Female (4)	Male (5)	Difference (6)	Female (7)	Male (8)	Difference (9)
Immediate Enrollment	-0.009**	0.005	-0.013**	0.022***	0.022***	-0.000	-0.030***	-0.017***	-0.013***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.004)	(0.005)
	[798,437]	[698,942]	[1,497,379]	[798,437]	[698,942]	[1,497,379]	[798,437]	[698,942]	[1,497,379]
	{0.50}	{0.51}	{-.01}	{0.29}	{0.30}	{-.01}	{0.21}	{0.22}	{-.01}
Two-Year Enrollment	0.010**	0.021***	-0.011*	0.020***	0.017**	0.003	-0.012***	0.001	-0.013***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.007)	(0.007)	(0.003)	(0.003)	(0.004)
	[704,170]	[614,722]	[1,318,892]	[704,185]	[614,725]	[1,318,910]	[705,057]	[615,620]	[1,320,677]
	{0.44}	{0.44}	{0}	{0.26}	{0.26}	{0}	{0.16}	{0.17}	{-.01}
Second-Year Dropout	-0.006	-0.012**	0.006	-0.021*	-0.041***	0.021	-0.007	-0.012**	0.004
	(0.005)	(0.005)	(0.007)	(0.011)	(0.011)	(0.015)	(0.006)	(0.006)	(0.008)
	[340,807]	[304,024]	[644,831]	[198,692]	[175,730]	[374,422]	[142,543]	[128,611]	[271,154]
	{0.12}	{0.14}	{-.02}	{0.11}	{0.13}	{-.02}	{0.21}	{0.23}	{-.02}
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. Sample sizes in square brackets. Outcome sample means in curly braces. *** p<0.01, ** p<0.05, * p<0.1. School level control variables include indicators of school type, rural area and geographical region. Student level control variables include attendance rate, district and number of family members at different levels in the education system.

In the case of males, university enrollment increases by 2.2 pp. at the expense of vocational enrollment, which decreases by 1.7 pp. Although positive (0.5 pp.), the estimate of the overall effect is not significant. In the case of females, however, the decrease of 3.0 pp. in vocational enrollment is not fully compensated by the increase of 2.2 pp. in university enrollment. The overall estimated effect is a decrease of 0.9 pp. in immediate HES enrollment for female students (significant at the 5% level).

While this negative overall effect for females might appear somewhat counterintuitive, it can be explained in light of the definition of our immediate enrollment outcome. It is possible that some of the female students who were induced by the reform to switch from vocational institutions to universities did so immediately (the year after graduation), but others delayed their enrollment decision. This delay could be an optimal response since eligibility criteria are more difficult to meet when enrolling in a university, and female students obtain systematically lower scores on the PSU test. Appendix C presents supporting evidence for this hypothesis.

In terms of retention, we find no evidence of sex heterogeneity in the effects of the reform on second-year dropout. The point estimates in Column (6) for universities, Column (9) for vocational institutions and Column (3) for the overall system are not significant. For persistence, measured by the two-year enrollment outcome, sex heterogeneity is entirely driven by the heterogeneity in the immediate enrollment effects, as evidenced by the similarity of the point estimates in Columns (3), (6) and (9).

These results suggest that the responses of females and males to the reform differ mainly in terms of immediate enrollment. In the following subsection, we analyze heterogeneity by type of school.

6.2 Public-School vs. Voucher-School Students

Table 10 analyzes the differences in the enrollment, persistence, and retention effects between individuals graduating from voucher and public schools. This exercise suggests that the diversion effect in immediate enrollment from our main results is entirely driven by voucher-school students. The intensive margin changes to this student-loan program had no effect whatsoever on eligible public-school students. Moreover, their lack of response extends to persistence and retention, as can be seen in the nonsignificant coefficients in Columns (1), (4) and (7)—with the exception of university second-year dropout.

The 3.3 pp. decrease in university second-year dropout, significant at the 5% level, is statistically equivalent to voucher-school students' response. Appendix D presents additional evidence that the PSU score gap between public- and voucher-school students is smaller conditional on university enrollment, possibly reflecting an ability sorting of public-school students into universities. Therefore, this odd result could be driven by more able public-school students who are somewhat closer to their voucher-school counterparts in the dimensions that are relevant for the dropout decision.

Table 10: Heterogeneity of Main Results by School Type

	HES			Universities			Vocational		
	Public (1)	Voucher (2)	Difference (3)	Public (4)	Voucher (5)	Difference (6)	Public (7)	Voucher (8)	Difference (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)
	[590,563]	[906,816]	[1,497,379]	[590,563]	[906,816]	[1,497,379]	[590,563]	[906,816]	[1,497,379]
	{0.46}	{0.53}	{-.07}	{0.24}	{0.33}	{-.09}	{0.22}	{0.21}	{.01}
Two-Year Enrollment	0.012*	0.023***	-0.011	0.005	0.026***	-0.021**	0.004	-0.007**	0.011**
	(0.006)	(0.004)	(0.008)	(0.008)	(0.005)	(0.010)	(0.004)	(0.003)	(0.005)
	[525,289]	[793,603]	[1,318,892]	[525,295]	[793,615]	[1,318,910]	[525,986]	[794,691]	[1,320,677]
	{0.39}	{0.47}	{-.08}	{0.21}	{0.29}	{-.08}	{0.17}	{0.16}	{.01}
Second-Year Dropout	-0.001	-0.016***	0.015*	-0.033**	-0.034***	0.001	0.001	-0.018***	0.018**
	(0.006)	(0.005)	(0.008)	(0.014)	(0.009)	(0.017)	(0.007)	(0.005)	(0.009)
	[234,013]	[410,818]	[644,831]	[122,173]	[252,249]	[374,422]	[112,153]	[159,001]	[271,154]
	{0.15}	{0.12}	{.03}	{0.14}	{0.11}	{.03}	{0.23}	{0.21}	{.02}
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. Sample sizes in square brackets. Outcome sample means in curly braces. *** p<0.01, ** p<0.05, * p<0.1. School level control variables include indicators of school type, rural area and geographical region. Student level control variables include attendance rate, district and number of family members at different levels in the education system.

All things considered, our finding of null results in public schools can be explained by two facts. First, public-school students tend to attain lower scores on standardized tests than voucher-school students. The literature on the effects of voucher systems documents a sorting effect of the introduction of voucher schools in Chile, with students from the right tail of the test-score distribution in public schools moving to voucher schools (Hsieh and Urquiola, 2006; Urquiola, 2016). Appendix D presents further evidence that graduates from public schools score systematically lower than graduates from voucher schools on the PSU test.

Second, public-school students tend to be poorer than voucher school-students. It has been documented that while public schools serve mostly students coming from low-income households, voucher schools concentrate on the lower-middle and middle-income sectors (Torche, 2005). As discussed in Section 2, the CAE only covers up to a "referential tuition fee" that is typically lower than actual tuition fees. This means that even if receiving the loan, students need sufficient liquidity to cover a nonnegligible fraction of the tuition fee. It is arguably more difficult for poorer students to cover this expense. Thus, as public-school graduates tend to be poorer and score lower on the PSU test, the reform might not actually affect their marginal incentives. Our evidence suggests that the 2012 intensive margin changes are not large enough to improve access to tertiary education among public-school students.

7 Conclusions

In this paper, we analyze the effects on enrollment, persistence and retention in higher education of a reform to student loans on the intensive margin that decreased the interest rate from approximately 6% to a fixed rate of 2%, along with other less salient changes that improved repayment conditions. We exploit these changes to the Chilean state-guaranteed CAE loan that took place in 2012 by using a DiD approach. Our main findings are remarkably robust to an alternative Diff-in-Disc identification strategy, where we implement an RDD approach to exploit the same source of exogenous variation under different identification assumptions. This lends additional credibility to DiD as a suitable method for evaluating the educational effects of the intensive margin changes introduced by the 2012 reform.

Our results show that the reform had no effect on overall HES immediate enrollment. Interestingly, we find a diversion effect whereby enrollment in universities increased by 2.5 pp.—a 7 percent increase relative to the enrollment rate of eligible cohorts before the reform—at the expense of enrollment in vocational institutions that fell by 2.5 pp.—equivalent to a decrease of 14 percent in enrollment relative to the same group. This institutional shift from vocational institutions to universities might entail welfare effects given that some diverted individuals would be likely better off had they pursued a vocational degree instead.

Ultimately, this student loan reform does not enhance access to tertiary education beyond a compositional effect across institutions; if anything, access for female students worsens since they appear to delay their enrollment decisions. Moreover, the diversion effect could backfire on the intended objective of improving repayment rates by increasing students' debt burden. While the interest rate decrease directly reduces loan payments, it also has an indirect opposite effect of raising debt levels for students moving from vocational institutions to universities, where programs are longer and more expensive. In fact, Ingresa (2021) reports a concerning default rate of 48.4% among all CAE debtors in 2021.

Regarding persistence, we find an improvement only for students enrolled in universities with a 2 pp. (7%) increase in two-year enrollment. In terms of retention, second-year dropout falls by 1.3 pp. (12%) in the overall system, consisting of a stronger -3.5 pp. (-32%) effect for students enrolled in universities and a weaker -0.8 pp. (-4%) effect in vocational institutions. Both findings might result from a sorting effect in enrollment in conjunction with HES institutions' perverse incentive to reduce dropout rates.

Virtually all of our results on immediate enrollment, persistence, and retention are driven primarily by students from voucher schools, with no response among students graduating from public schools. This constitutes another unintended effect, where a policy reform that in principle should benefit economically disadvantaged students ends up not reaching them.

This paper constitutes a step forward in the research roadmap recently proposed by Dynarski, Page and Scott-Clayton (2022) by paying close attention to the multilayered heterogeneity of the effects of a student aid program reform. Our findings suggest important lessons for policymakers on the unexpected consequences of reforms introducing intensive margin changes to student loan programs. As stressed by Aguirre (2021), compositional effects such as the ones documented in this paper should be a primary concern in the design of educational policy. This is a cautionary tale warning of the unintended effects that result from altering incentives throughout a student's life cycle.

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A Parallel Trends Assumption

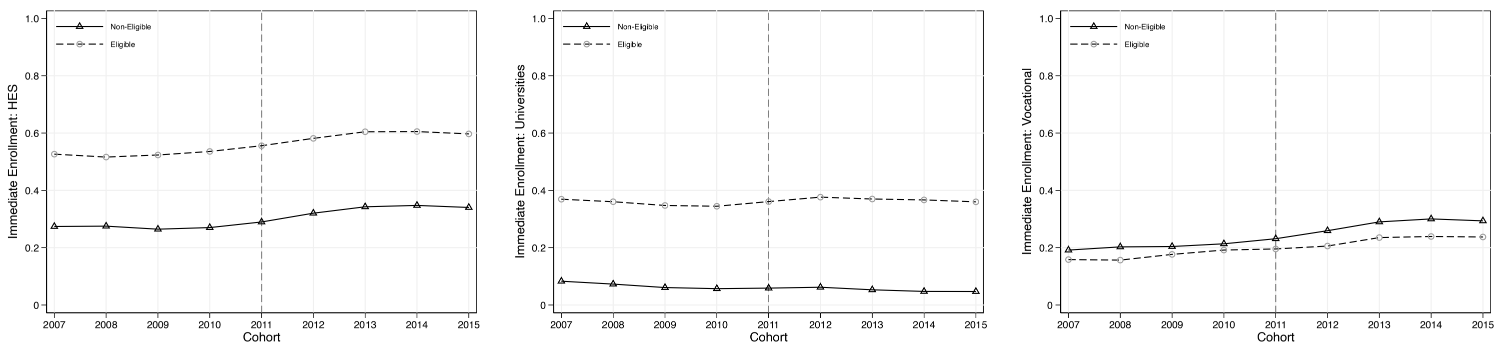
This appendix examines the plausibility of the parallel trends assumption underlying our DiD research design, and provides the detailed estimation results used to construct Figures 1 and 2 on the dynamics of the treatment effects. We start with a visual inspection of the time trends of our nine outcomes—immediate enrollment, two-year enrollment, and second-year dropout for any HES institution, universities, and vocational institutions—for eligible and non-eligible individuals in Figure A.1. Panels A, B, and C show the corresponding trends of immediate enrollment, two-year enrollment, and second-year dropout, respectively. From this visual inspection we can reasonably conclude that all nine variables evolved in a parallel fashion between eligible and ineligible students before the 2012 reform. This evidence of parallel pre-treatment trends is consistent with the identification assumption of parallel counterfactual trends of the potential outcomes in the absence of treatment.

To analyze the dynamics of the enrollment, persistence, and retention effects of the reform, we estimate Equation (2) separately for each of our outcomes. The results for immediate enrollment, two-year enrollment, and second-year dropout are reported in Table A.1. The reference categories for the cohort dummies are cohort 2011 in Columns (1)–(4) and cohort 2010 in Columns (5)–(12). As explained in the main text, cohort 2015 is excluded in the analysis of two-year enrollment and second-year dropout. The coefficients on 'Eligible × cohort j' for $j \in \{2007, 2008, 2009, 2010, 2012, 2013, 2014, 2015\}$ in Columns (1)–(4) and $j \in \{2007, 2008, 2009, 2011, 2012, 2013, 2014\}$ in Columns (5)–(12) correspond to the β_j in Equation (2) and are plotted in Figures 1 and 2 in the main text along with their 99% confidence intervals.

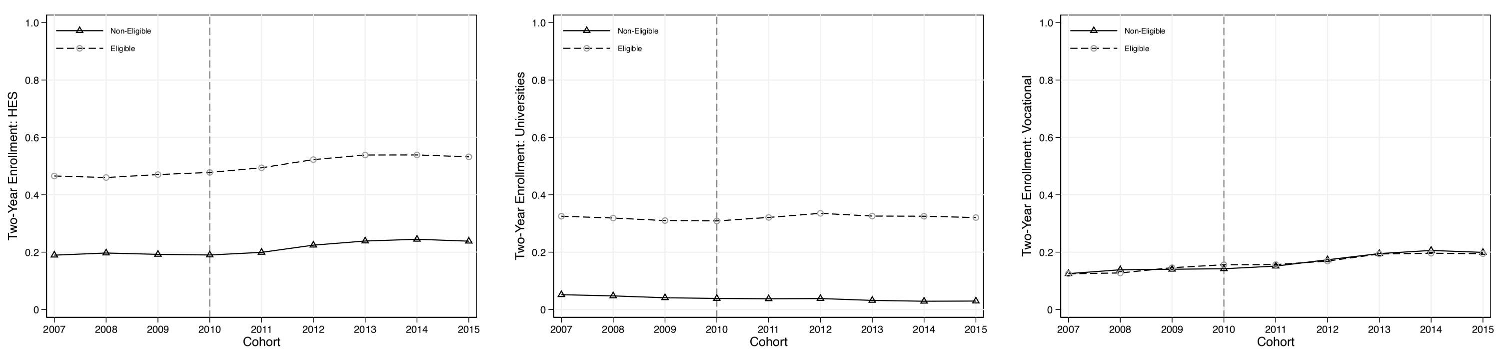
The bottom panel of Table A.1 presents the p-values of the F statistic for the null hypothesis $H_0 : \boldsymbol{\beta}^{\text{pre}} = \mathbf{0}$, where $\boldsymbol{\beta}^{\text{pre}}$ is the vector of the β_j coefficients for the unexposed cohorts. These results indicate that we cannot reject the null hypothesis at conventional significance levels for most of our outcomes. Finally, Table A.2 reports the results of a falsification exercise where we run our main DiD specifications on the subsample of unexposed cohorts $j \in \{2007, \ldots, 2009\}$ under a placebo reform hypothetically occurring in year 2009. That is, we estimate the corresponding specifications of Equation (1) with $\text{exposed}_{it} = \mathbb{1}\left[t \geq 2009\right]$. Reassuringly, the point estimates are not statistically significant in most cases.[28]

[28]While the null of no pre-trends is rejected for a few outcomes in Table A.1, differential pre-trends can be statistically ruled out when excluding individuals making immediate decisions in year 2008 and two-year decisions in years 2007–2008, as shown in the corresponding table A.1 in the Online Appendix. Similarly, we cannot reject that all placebo treatment effects are zero when excluding students making decisions in year 2008 in the corresponding Table A.2 in the Online Appendix. As a robustness check, we replicate all our analysis excluding these cohorts, with our main results remaining virtually unchanged.

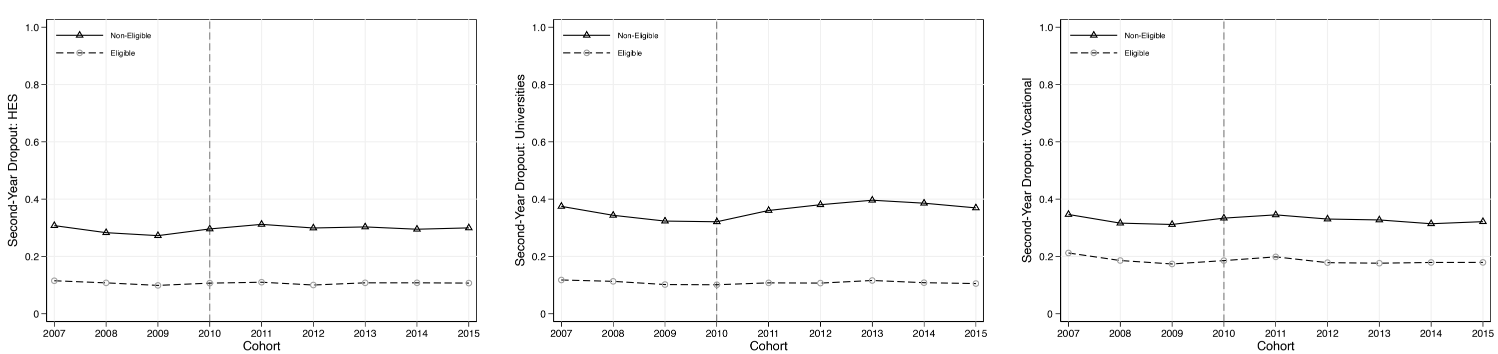
Figure A.1: Outcomes over Time by Eligibility



Panel A: Immediate Enrollment



Panel B: Two-Year Enrollment



Panel C: Second-Year Dropout

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

Table A.1: Dynamics

	Immediate Enrollment				**Two-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	0.003	-0.014***	-0.014***	-0.037**	-0.050***	0.014	0.009
	(0.010)	(0.010)	(0.006)	(0.006)	(0.009)	(0.010)	(0.005)	(0.005)	(0.014)	(0.015)	(0.010)	(0.009)
Eligible × cohort 2008	-0.015	-0.013	-0.010*	-0.010	0.001	0.003	-0.025***	-0.025***	-0.011	-0.023	0.018*	0.011
	(0.010)	(0.010)	(0.006)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.015)	(0.015)	(0.009)	(0.009)
Eligible × cohort 2009	-0.015	-0.015	0.008	0.009	-0.001	-0.000	-0.009*	-0.009*	-0.002	-0.003	0.010	0.010
	(0.010)	(0.010)	(0.006)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.015)	(0.015)	(0.009)	(0.009)
Eligible × cohort 2010	-0.014	-0.015	0.014**	0.015**								
	(0.010)	(0.010)	(0.006)	(0.006)								
Eligible × cohort 2011					0.013	0.014	-0.009*	-0.010*	-0.033**	-0.048***	0.002	-0.002
					(0.010)	(0.010)	(0.005)	(0.005)	(0.015)	(0.015)	(0.009)	(0.008)
Eligible × cohort 2012	0.013	0.011	-0.018***	-0.016***	0.027***	0.026***	-0.018***	-0.018***	-0.054***	-0.056***	-0.004	-0.006
	(0.009)	(0.009)	(0.006)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.015)	(0.015)	(0.009)	(0.008)
Eligible × cohort 2013	0.015	0.012	-0.019***	-0.018***	0.024***	0.021**	-0.015***	-0.015***	-0.060***	-0.056***	-0.003	-0.010
	(0.009)	(0.009)	(0.007)	(0.007)	(0.009)	(0.009)	(0.006)	(0.006)	(0.016)	(0.015)	(0.008)	(0.008)
Eligible × cohort 2014	0.017*	0.016	-0.026***	-0.025***	0.026***	0.025**	-0.024***	-0.024***	-0.058***	-0.038**	0.013	0.007
	(0.010)	(0.010)	(0.007)	(0.007)	(0.009)	(0.010)	(0.006)	(0.006)	(0.017)	(0.017)	(0.008)	(0.008)
Eligible × cohort 2015	0.011	0.009	-0.021***	-0.020***								
	(0.009)	(0.009)	(0.007)	(0.007)								
Eligible	0.302***	0.283***	-0.036***	-0.035***	0.270***	0.249***	0.014***	0.012***	-0.220***	-0.157***	-0.148***	-0.122***
	(0.007)	(0.007)	(0.004)	(0.004)	(0.007)	(0.007)	(0.004)	(0.004)	(0.011)	(0.011)	(0.006)	(0.006)
Cohort 2007	0.024***	0.024***	-0.040***	-0.043***	0.013***	0.010***	-0.017***	-0.020***	0.054***	0.055***	0.013	0.007
	(0.003)	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.015)	(0.015)	(0.010)	(0.009)
Cohort 2008	0.014***	0.012***	-0.028***	-0.031***	0.009***	0.004	-0.004	-0.006	0.023	0.032**	-0.017*	-0.016*
	(0.003)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.015)	(0.015)	(0.009)	(0.009)
Cohort 2009	0.002	0.002	-0.027***	-0.028***	0.002	0.001	-0.002	-0.001	0.002	0.003	-0.022**	-0.026***
	(0.003)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.015)	(0.015)	(0.009)	(0.008)
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**	0.039***	0.065***	0.011	0.021**
					(0.002)	(0.003)	(0.004)	(0.004)	(0.015)	(0.015)	(0.009)	(0.008)
Cohort 2012	0.003	0.005	0.028***	0.031***	-0.000	0.001	0.031***	0.036***	0.060***	0.065***	-0.003	0.001
	(0.003)	(0.003)	(0.005)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)	(0.016)	(0.015)	(0.009)	(0.009)
Cohort 2013	-0.006**	-0.005	0.059***	0.059***	-0.007***	-0.008**	0.053***	0.054***	0.075***	0.087***	-0.006	0.018**
	(0.002)	(0.003)	(0.006)	(0.005)	(0.002)	(0.003)	(0.005)	(0.004)	(0.016)	(0.015)	(0.009)	(0.008)
Cohort 2014	-0.012***	-0.010***	0.069***	0.071***	-0.010***	-0.009***	0.064***	0.067***	0.065***	0.061***	-0.020**	0.007
	(0.002)	(0.003)	(0.006)	(0.006)	(0.002)	(0.003)	(0.005)	(0.005)	(0.017)	(0.017)	(0.009)	(0.008)
Cohort 2015	-0.012***	-0.010***	0.062***	0.063***								
	(0.002)	(0.003)	(0.006)	(0.005)								
Student district fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
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	1,318,910	1,318,910	1,320,677	1,320,677	386,140	374,422	272,124	271,154
Pre-trends p-value	0.423	0.404	0.001	0.001	0.969	0.972	0.000	0.000	0.030	0.003	0.260	0.561

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 of school type, rural area and geographical region. Student level control variables include gender, attendance rate, district and number of family members at different levels in the education system. Control group size accounts for the number of ineligible individuals in the exposure period, while Outcome mean refers to the mean of the dependent variable of those individuals.

Table A.2: Placebo Reform

	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Immediate Enrollment									
Eligible × exposed (placebo)	0.012*	0.012*	0.012*	-0.000	-0.000	0.000	0.012**	0.012**	0.011**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)
Observations	450,707	450,707	450,707	450,707	450,707	450,707	450,707	450,707	450,707
Two-Year Enrollment									
Eligible × exposed (placebo)	-0.005	-0.005	-0.003	-0.003	-0.003	-0.001	-0.002	-0.002	-0.003
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Observations	450,131	450,131	450,131	450,132	450,132	450,132	450,706	450,706	450,706
Second-Year Dropout									
Eligible × exposed (placebo)	0.011	0.010	0.018***	0.024*	0.024*	0.036***	-0.005	-0.005	0.001
	(0.007)	(0.007)	(0.007)	(0.013)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Observations	208,438	208,438	199,456	130,909	130,909	122,616	77,854	77,854	77,130
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables are the same as in Tables 3, 4, and 5.

B Endogeneity of Two-Year Eligibility

An issue arises with our two-year outcomes capturing persistence and retention in higher education. Loan eligibility is potentially endogenous in this setting since individuals that were ineligible in the year immediately following high school graduation—i.e., those with PSU < 475 and GPA < 5.3—can resit the test and become eligible in later years, in particular, in the second year after graduation. In this appendix, we follow Solis (2017) in using an instrumental variables (IV) approach to deal with this potential endogeneity problem.

The endogenous variable in our IV setup is the student's overall eligibility within two years following high school graduation, eligible_{2it}. It is given by the student's GPA, which does not vary over time, and their first-attempt PSU score in case they do not retake the test or their second-attempt score in case they do resit and score above their first-attempt. We instrument this variable with the student's eligibility in the first year after graduation, eligible_{1it} , which is determined by their GPA and first-attempt PSU score.

To be more precise, let d_{jit} be an indicator equal to 1 if student i in cohort t attempted the PSU in year $t + (j - 1)$ for $j \in \{1, 2\}$—i.e., immediately after graduation and the following year—and ψ_{jit} the corresponding PSU score if $d_{jit} = 1$. We define the two-year-best PSU score as the best of any attempts within 2 years after graduation. That is,

$$\Psi_{it} = \begin{cases} \max\left(\psi_{1it}, \psi_{2it}\right) & \text{if } \ d_{1it} = 1 \text{ and } d_{2it} = 1 \\ \psi_{1it} & \text{if } \ d_{1it} = 1 \text{ and } d_{2it} = 0 \\ \psi_{2it} & \text{if } \ d_{1it} = 0 \text{ and } d_{2it} = 1. \end{cases}$$

Then, two-year eligibility—that is, ever being eligible within the first two years since high school graduation—can be defined as

$$\text{eligible}_{2it} \equiv \mathbb{1}\left[\Psi_{it} \geq 475 \text{ or } \text{GPA}_{it} \geq 5.3\right],$$

whereas immediate eligibility—the one that defines the treatment and control groups in our main analysis—is defined as

$$\text{eligible}_{1it} \equiv \mathbb{1}\left[\psi_{1it} \geq 475 \text{ or } \text{GPA}_{it} \geq 5.3\right].$$

The endogeneity problem arises because the reform could induce some immediately-ineligible students to resit the test with the aspiration of becoming second-year eligible, thus self-selecting into treatment.[29]

Our DiD-IV linear regression model is given by the structural equation

$$y_{it} = \boldsymbol{x}'_{it}\boldsymbol{\lambda} + \eta_{it} \tag{B.1}$$

and the first stage

$$\boldsymbol{x}_{2it} = \boldsymbol{\Gamma}\, \boldsymbol{z}_{it} + \boldsymbol{\nu}_{it} \tag{B.2}$$

[29] The interaction $\text{eligible}_{2it} \times \text{exposed}_{it}$ becomes endogenous by extension and we instrument it with the corresponding interaction, $\text{eligible}_{1it} \times \text{exposed}_{it}$. The relevance, independence, and exclusion restriction requirements for the full vector of excluded instruments $\boldsymbol{z}_{2it}$ is essentially determined by the stochastic properties of eligible_{1it}, and therefore, we focus our discussion on this variable.

where

$$\boldsymbol{x}_{it} = \begin{pmatrix} \boldsymbol{x}_{1it} \\ \boldsymbol{x}_{2it} \end{pmatrix}, \quad \boldsymbol{x}_{2it} = \begin{pmatrix} \text{eligible}_{2it} \\ \text{eligible}_{2it} \times \text{exposed}_{it} \end{pmatrix}, \quad \boldsymbol{z}_{it} = \begin{pmatrix} \boldsymbol{x}_{1it} \\ \boldsymbol{z}_{2it} \end{pmatrix} = \begin{pmatrix} \boldsymbol{x}_{1it} \\ \text{eligible}_{1it} \\ \text{eligible}_{1it} \times \text{exposed}_{it} \end{pmatrix},$$

and $\boldsymbol{x}_{1it}$ is a vector of control variables—the included instruments—containing exposed_{it} and the scalar 1 in our baseline specifications, and including the same covariates as our main-text analysis in additional specifications. Under the standard IV assumptions within the potential outcomes framework with heterogeneous treatment effects—namely the first stage, independence, exclusion restriction, and monotonicity assumptions—the λ coefficient on $\text{eligible}_{2it} \times \text{exposed}_{it}$ in Equation (B.1) captures the local average treatment effect (LATE) of the reform on the two-year enrollment/second-year dropout rate of compliers.[30]

The relevance condition for our instrument is straightforward since the endogenous regressor, eligible_{2it}, and its instrument, eligible_{1it}, are strongly correlated by construction: both build on the GPA and the first-attempt PSU score. In fact, they will only differ in the scenario of a formerly ineligible student retaking the test and scoring above 475 points, and it is impossible—again, by construction—for an immediately-eligible student to become two-year ineligible.[31] Therefore, the monotonicity assumption—i.e., that the instrument affects the endogenous regressor in one direction only, so there are no defiers—is also trivially satisfied.

Regarding the validity of our instrument, notice that while it is hard to argue that immediate eligibility is as good as randomly assigned, the parallel trends assumption underlying the DiD design implies that the regressors in Equation (1) are exogenous—i.e., the linear regression in Equation (1) is equivalent to the population DiD. Since these regressors are our instruments in equation system (B.1)–(B.2), we conclude that, if the parallel trends assumption holds, we must have $\mathbb{E}[\boldsymbol{z}_{it}\varepsilon_{it}] = \mathbf{0}$, which in turn immediately implies $\mathbb{E}[\boldsymbol{z}_{it}\eta_{it}] = \mathbf{0}$ since Equation (B.2) is a linear projection.[32] Therefore, under the parallel trends assumption, our instruments satisfy the independence condition for a valid IV.

To address the concern that the parallel trends assumption may not hold away from the eligibility threshold, we combine this IV strategy with our Diff-in-Disc design from Section 5, replacing the OLS estimator at each side of the threshold with the corresponding 2SLS estimator. Here, identification relies on a local randomization argument: immediate eligibility is as good as randomly assigned in a neighborhood of the threshold. Hence, our instruments are locally valid across the threshold even if the parallel trends assumption does not hold over the entire support of the PSU scores.

Finally, the exclusion restriction requires that immediate eligibility affects the corresponding two-year outcome only through its effect on two-year eligibility—the first stage. It may be argued that immediate eligibility affects the probabilities of (i) enrollment for two consecutive years and (ii) dropping out in the second year through its direct effect on immediate enrollment—which we document in our main analysis—, rendering the exclusion restriction invalid. However, our

[30] In this setting, compliers are students who would enroll for two consecutive years/drop out in the second year only if initially eligible for the student loan. See Angrist, Imbens and Rubin (1996) for details.

[31] In the former case, the student would have $\text{eligible}_{1it} = 0$ and $\text{eligible}_{2it} = 1$. Note that the latter case is mechanically ruled out since all immediately-eligible individuals are two-year eligible as well: $\text{eligible}_{1it} = 1 \implies \text{eligible}_{2it} = 1$.

[32] See the Online Appendix for details.

analysis of second-year dropout is conditional on immediate enrollment, so, to the extent that there is no other causal mechanism from immediate eligibility to second-year dropout, the exclusion restriction is satisfied conditional on immediate enrollment.[33] Moreover, the exclusion restriction is not necessary for a causal interpretation of the reduced form regression capturing the effect of the instrument on the outcome (Angrist, Imbens and Rubin, 1996). Notice that our main specification in Equation (1) corresponds to the reduced form of IV system (B.1)–(B.2), and therefore, a violation of the exclusion restriction is not a concern for our main results.

The estimation results of our IV-DiD regressions for two-year enrollment and second-year dropout are reported in Table B.1 below. Reassuringly, these results are remarkably similar to our main reduced-form results in Tables 4 and 5. As expected from our discussion above, immediate eligibility is a strong instrument for two-year eligibility, as can be seen from the large Cragg and Donald (1993) statistics. Our IV-Diff-in-Disc results in Table B.2 are also similar to the corresponding Diff-in-Disc results in Tables 7 and 8—virtually identical for second-year dropout and close but somewhat larger for two-year enrollment.

Figure B.1 illustrates the strength of the instrument. Panel (a) displays the superimposed histograms—constructed with common bins—of immediate (ψ_{1it}) and two-year-best (Ψ_{it}) PSU scores for students in our sample who attempted the test at least once within two years from high school graduation. Panel (b) shows a scatter plot of Ψ_{it} against ψ_{1it} with partially transparent markers so that higher-density points are darker. The overall picture, confirmed by the cross-tabulation in Table B.3, is that only a few students retake the test and score higher than their first attempt. In fact, only 0.78% of the students in our sample change eligibility status by resitting the PSU, representing 3.45% of the 22.64% of immediately ineligible students. It is reassuring in our context that the endogenous regressor, two-year eligibility, closely resembles the excluded instrument, immediate eligibility. This means that there is little response to the reform on this margin to begin with, so the endogeneity problem, if any, turns out to be small. We conclude that the findings in this appendix lend credibility to our reduced-form approach to the DiD/Diff-in-Disc design in our main analysis.

[33] In contrast, our two-year enrollment variable does not condition on immediate enrollment, so it is open to this critique. Therefore, the IV results for two-year enrollment presented in this section should be interpreted with caution in the sense of attributing them a causal interpretation. However, the fact that these results are consistently similar to our main reduced-form results is reassuring.

Table B.1: IV-DiD Regressions for Two-Year Outcomes

	HES			Universities			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Two-Year Enrollment									
Eligible × exposed (2nd year)	0.019***	0.018***	0.017***	0.020***	0.020***	0.019***	-0.004	-0.005*	-0.006**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,318,892	1,318,892	1,318,892	1,318,910	1,318,910	1,318,910	1,320,677	1,320,677	1,320,677
Cragg-Donald	12,978,617	12,974,252	12,685,634	12,978,958	12,974,594	12,685,961	12,992,452	12,988,078	12,699,156
Second-Year Dropout									
Eligible × exposed (2nd year)	-0.012***	-0.013***	-0.009**	-0.035***	-0.037***	-0.032***	-0.007	-0.007*	-0.010**
	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Cohort effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	657,479	657,479	644,831	386,140	386,140	374,422	272,124	272,124	271,154
Cragg-Donald	50,099,142	50,084,633	46,329,066	15,740,421	15,720,617	14,367,298	27,011,830	26,989,479	25,954,683

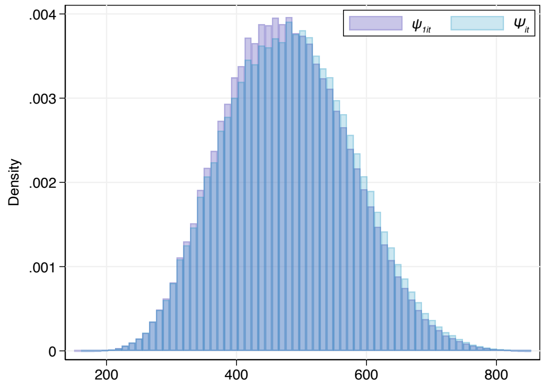
Notes: 2SLS estimates instrumenting eligible_{it} with eligible_{2it}. Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Control variables are the same as in Tables 3, 4, and 5.

Table B.2: IV-Diff-in-Disc Design for Two-Year Outcomes

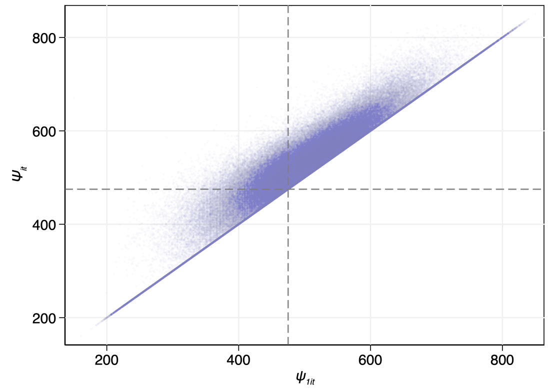
	Two-Year Enrollment						**Second-Year Dropout**					
	All students			GPA < 5.3			All students			GPA < 5.3		
	HES	Universities	Vocational	HES	Universities	Vocational	HES	Universities	Vocational	HES	Universities	Vocational
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Difference	0.031***	0.035***	-0.005	0.047***	0.035***	0.006	-0.002	0.003	-0.010	-0.017	-0.000	-0.043*
	(0.008)	(0.007)	(0.007)	(0.015)	(0.011)	(0.012)	(0.006)	(0.012)	(0.010)	(0.014)	(0.026)	(0.023)
Exposed	0.097***	0.142***	-0.051***	0.095***	0.088***	-0.002	-0.008**	-0.019**	0.001	-0.029***	-0.009	-0.038**
	(0.005)	(0.005)	(0.005)	(0.009)	(0.007)	(0.009)	(0.004)	(0.008)	(0.006)	(0.010)	(0.019)	(0.016)
Unexposed	0.067***	0.107***	-0.046***	0.048***	0.052***	-0.008	-0.006	-0.022***	0.012	-0.012	-0.009	0.005
	(0.006)	(0.005)	(0.005)	(0.012)	(0.009)	(0.008)	(0.004)	(0.008)	(0.007)	(0.010)	(0.018)	(0.016)
Bandwidth												
Exposed	58.077	37.934	38.461	64.812	51.634	48.703	54.348	51.297	46.782	50.644	54.499	31.156
Unexposed	50.111	43.226	44.051	38.730	43.832	48.974	59.024	53.361	50.914	51.482	45.883	40.089
Observations												
Exposed	133,494	88,264	89,627	38,607	31,115	29,424	69,669	30,248	32,749	15,517	6,386	5,968
Unexposed	107,266	92,890	94,706	23,653	26,707	29,759	61,304	27,849	26,649	14,229	5,728	6,197

Notes: Optimal bandwidths separately selected by exposure. Triangular kernel is used in local linear regressions. Standard errors clustered at the class level in parentheses. *** p< 0.01, ** p< 0.05, * p< 0.1.

Figure B.1: Immediate and Two-Year-Best PSU Scores



(a) Histograms



(b) Scatter Plot

Notes: Immediate PSU scores, ψ_{1it}, and two-year-best PSU scores, Ψ_{it}, for students in our sample who attempted the PSU at least once within two years from high school graduation in panel (a). The sample is further restricted to students who attempted the test in the first year after graduation (neither ψ_{1it} or Ψ_{it} are missing). Panel (a) displays superimposed histograms constructed with common bins. Panel (b) shows a scatter plot with partially transparent markers so that higher-density points are darker.

Table B.3: Immediate vs Two-Year Eligibility

	Two-year eligibility		
	Yes (1)	No (2)	Total (3)
Immediate eligibility			
Yes	77.36%	0%	77.36%
No	0.78%	21.87%	22.64%
Total	78.13%	21.87%	100.00%

Notes: Cells report the percentage of students in our sample that fall in each category.

C Heterogeneity: Sex

Table C.1 presents evidence that Chilean female students perform systematically worse in the PSU than male students. We standardize PSU scores by cohort and present yearly differences between men and women with the corresponding standard errors. There is a persistent and sizable gender gap in detriment of female students ranging from approximately 10% to 17% of a standard deviation. This result is consistent with other findings in the literature documenting that men tend to perform better in mathematics (Marks, 2008; Fryer and Levitt, 2010; Bharadwaj et al., 2016). The PSU gender gap makes it harder for women to meet the eligibility criteria for both university admission and CAE access since they are mainly determined by PSU scores.

In this context, the reduction in the cost of financing the educational investment project, resulting from the interest rate drop of the 2012 reform, might induce some female students to resit the PSU hoping to improve their scores. We explore this hypothesis by estimating the following equation

$$y_{it} = \beta_0 + \beta_1 \text{female}_i + \beta_2 \text{after}_t + \beta_3 \text{female}_i \times \text{after}_t + \varepsilon_{it} \tag{C.1}$$

where y_{it} represents one of two outcomes. Our first outcome, repetition, is an indicator variable for students sitting the PSU immediately and also the following year. The second outcome, improvement, is also a binary variable indicating that, conditional on repetition, the second-attempt PSU score is higher than the first one. Table C.2 presents the estimation results for two different groups of students. Columns (1) and (3) include all students who sat the PSU immediately after high school graduation, while columns (2) and (4) comprise the subset of those students that did not enroll immediately.

Results in columns (1) and (2) indicate that females are more likely to resit the test, and that the reform widens this difference. This finding confirms our hypothesis that the reform induces higher repetition rates among female students, which could cause a negative effect in their immediate enrollment. Moreover, results in column (3) show that women are not only more likely to improve their scores through repetition but also that this likelihood roughly doubles after the reform. This result is not driven by non-enrolled students, as shown in column (4).

Table C.1: PSU Scores Gender Gap

	Cohort								
	2007	2008	2009	2010	2011	2012	2013	2014	2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male	0.0940	0.0959	0.0787	0.0779	0.0667	0.0521	0.0787	0.0792	0.0674
Female	-0.0785	-0.0785	-0.0677	-0.0680	-0.0590	-0.0446	-0.0686	-0.0694	-0.0598
Difference	0.1725***	0.1744***	0.1465***	0.1459***	0.1256***	0.0968***	0.1473***	0.1486***	0.1272***
	(0.0057)	(0.0055)	(0.0052)	(0.0051)	(0.0051)	(0.0053)	(0.0052)	(0.0052)	(0.0051)

Notes: Average PSU scores, standardized by cohort, for male and female students and the their difference. Standard errors of the corresponding t test for the equality of means in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.2: Evidence of Female Delay

	Repetition		Improvement	
	All students	Non-enrolled	All students	Non-enrolled
	(1)	(2)	(3)	(4)
Female × exposed	0.007**	0.036***	0.013***	-0.007
	(0.003)	(0.007)	(0.005)	(0.005)
Female	0.030***	0.058***	0.015***	-0.000
	(0.002)	(0.004)	(0.003)	(0.003)
Exposed	-0.015***	-0.006	-0.082***	-0.024***
	(0.003)	(0.006)	(0.004)	(0.004)
Observations	1,155,228	518,399	228,696	181,391

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Repetition and Improvement are indicator variables. All students comprises the sample of students who sat the PSU immediately after high school graduation. Non-enrolled is the subsample of students that did not enroll immediately. Cohort 2015 is excluded because we do not have access to PSU scores for year 2016.

D Heterogeneity: School Type

Table D.1 presents yearly PSU scores standardized at the cohort level for public- and voucher-school students separately along with the mean difference test and the corresponding standard errors. For our full sample, the gap favoring voucher-school students is approximately 32% of a standard deviation and persistent over time. In contrast, conditional on university enrollment, the gap drops to around 10% of a standard deviation.

Table D.1: PSU Scores School-Type Gap

	Cohort								
	2007	2008	2009	2010	2011	2012	2013	2014	2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Full Sample									
Voucher	0.1270	0.1220	0.1215	0.1250	0.1168	0.1207	0.1220	0.1024	0.1119
Public	-0.1690	-0.1730	-0.1740	-0.1813	-0.1801	-0.2279	-0.2311	-0.1916	-0.2087
Difference	0.2959***	0.2950***	0.2955***	0.3063***	0.2969***	0.3486***	0.3531***	0.2939***	0.3206***
	(0.0057)	(0.0055)	(0.0052)	(0.0051)	(0.0051)	(0.0055)	(0.0054)	(0.0054)	(0.0053)
Conditional on University Enrollment									
Voucher	0.7853	0.8152	0.8426	0.8673	0.8355	0.8202	0.8281	0.8117	0.8326
Public	0.7009	0.7440	0.7634	0.7698	0.7599	0.6852	0.6787	0.7215	0.7173
Difference	0.0844***	0.0712***	0.0792***	0.0975***	0.0756***	0.1350***	0.1494***	0.0902***	0.1153***
	(0.0090)	(0.0086)	(0.0081)	(0.0080)	(0.0079)	(0.0083)	(0.0080)	(0.0079)	(0.0078)
Conditional on Vocational Enrollment									
Voucher	-0.2776	-0.3006	-0.2709	-0.2692	-0.3455	-0.3833	-0.3826	-0.4033	-0.3969
Public	-0.4962	-0.5244	-0.5029	-0.5064	-0.5635	-0.6171	-0.6053	-0.6242	-0.6302
Difference	0.2186***	0.2238***	0.2320***	0.2371***	0.2180***	0.2338***	0.2227***	0.2209***	0.2333***
	(0.0097)	(0.0096)	(0.0084)	(0.0079)	(0.0077)	(0.0078)	(0.0074)	(0.0074)	(0.0073)

Notes: Average PSU scores, standardized by cohort, for voucher and public school students and the their difference. Standard errors of the corresponding t test for the equality of means in parentheses. *** p<0.01, ** p<0.05, * p<0.1.