Effects of a Reduction in Credit Constraints on Educational Attainment: Evidence from Chile

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Abstract

This paper analyzes the enrollment and retention effects of a student loan reform that relaxed credit constraints in Chile. The reform reduced the interest rate from an average of 6 percent to a fixed rate of 2 percent. The identification strategy follows a Difference-in-difference approach that compares the effects of this policy change among eligible and ineligible students. We find a precise null effect of the reform on overall immediate enrollment, along with a diversion effect that increased enrollment to universities in 2.5 percentage points (pp.) (7 percent), while enrollment to vocational institutions dropped in 2.5 pp. (14 percent). Moreover, we find that for female students the decrease in enrollment to vocational institutions is not fully offset by the increase in enrollment to universities. We also find that the reform increased university retention (two-year enrollment rate) by 3 percent and reduced the dropout rate by 10 percent. Our findings are mainly driven by medium-income-family students.

1 Introduction

There is a growing interest in the economics literature about the effects of financial aid on educational attainment as a result of the ongoing debate regarding the relative importance of long-run and short-run constraints.[1] On one hand, some researchers argue in favor of early-stage investments as the main drivers of long-run educational and labor outcomes (Cameron and Heckman, 2001; Carneiro and Heckman, 2002; Heckman et al., 2006). And on the other hand, some others focus on short-run credit constraints as the main obstacles for higher educational attainment, especially among low-income families (Lochner and Monge-Naranjo (2011) provide an excellent detailed review for the latter).

Empirically evaluating how credit constraints affect tertiary educational decisions is a difficult challenge for several reasons. For instance, (i) the impossibility of directly observing credit constraints, (ii) the potential endogeneity in enrollment-based regressions, and (iii) the fact that most tertiary

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[1]See Dynarski and Scott-Clayton (2013) for a review on the economics-of-education literature on financial aid.

education systems have admission processes that are highly determined by unobserved measures such as alumni status of parents and recommendation letters.[2]

In recent years, a branch of the literature has focused on studying the Chilean Higher Education system (CHES hereinafter) thanks to the similarities with the U.S. system but mostly because its institutional setting allows overcoming several of the aforementioned issues. The admission process, for example, is entirely determined by observable academic variables such as high-school GPA and the national admission test score. Moreover, the CHES has a highly centralized and standardized grants system, allowing researchers to come in hand with a rich set of administrative records. Finally, in the last decades, Chile has introduced and modified different aid programs to boost access to post-secondary education. Empiricists have exploited these programs as quasi-natural experiments to identify the effects of credit access on educational attainment and labor market outcomes (e.g. Rau et al. (2013), Solis (2017), Bucarey et al. (2018), Montoya et al. (2018)).

The main goal of this paper is to analyze post-secondary enrollment and retention effects of a Chilean reform to state-guaranteed loans that took place in 2012, loosening credit constraints for high school graduates. The reform consisted of the following changes to repayment conditions: (i) a decrease of the interest rate from approximately 6 percent 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. From these, the interest rate drop is the most relevant change since it is automatically applicable to all loans, while the two others are available upon request and only a small fraction of debtors apply for them.[3]

Our identification strategy is based in a Difference-in-differences (DiD) approach, exploiting the differences between students who were exposed and not exposed to the 2012 changes (post and pre 2012 high school graduation cohorts) and between students who were eligible and ineligible for the loan. We combine a rich set of administrative records at the individual level. Our data covers the entire population of high school graduates in Chile between 2006 and 2014 who faced their first enrollment decisions in the 2007-2015 period. We have detailed information about their enrollment and permanence choices, about the academic variables that determine loan eligibility, and other individual, school, and educational program characteristics that we use as control variables.

We contribute to the empirical literature on the effects of financial aid on educational attainment in two ways. First, while most of the research focuses on the effects of having access to student loans (i.e. on the extensive margin), this paper is the first one, to the best of our knowledge, to evaluate the effects of a reform that introduces changes in the intensive margin by loosening constraints and in a context where those changes were designed to affect repayment behavior only.[4] Analyzing the effects of these intensive margin changes to credit access is of important relevance for policymakers, specially in countries with similar student-loan systems where a reform that loosens credit constraints might be part of the future agenda.[5] Second, and in comparison to other related research, our data has the advantages of having (i) complete non-missing information about the entire population of high school graduates, (ii) large sample sizes that improve efficiency of our

[2]See Riegg (2008) for a discussion about causal inference and selection bias on the financial aid literature.

[3]In 2015, only 8% and 4% of the debtors were beneficiary of the 10%-cap and delayed repayments respectively. See Ingresa (2015) for details.

[4] See Nielsen et al. (2010) and Dynarski (2003) as examples of papers that study the effects of reforms to other types of financial aid different from student loans.

[5]Colombia, Mexico, U.S., Canada, U.K. and Australia are examples of countries with student loans as mechanisms of financial aid.

estimates, and (iii) a considerable number of cohorts.

Our results suggest that the loosening of credit constraints had no effect on overall immediate enrollment (i.e., enrollment in any CHES institution during the year immediately following high school graduation). However, we find a diversion effect: enrollment to universities increased by 2.5 percentage points (pp.) — which amounts to a 7 percent increase relative to enrollment of non-exposed eligible individuals — in detriment of enrollment to vocational institutions, which fell by 2.5 pp. — equivalent to a 14 percent decrease of enrollment relative to the same group. This effect is stable over time except for a decrease in 2015 when a new free-tuition program was announced by the government. This shift in institutional choice is explained — in line with Angrist et al. (2016) — by the implicit subsidy the reform creates toward universities relative to vocational institutions given that the former are more expensive in terms of tuition fees and program length. Moreover, the diversion effect implies welfare effects since some individuals that diverted their decision toward universities would be likely better-off had they pursued a vocational degree instead (Rodriguez et al., 2016).

Our findings are consistent with the evidence on the enrollment effects of financial aid in general (Cornwell et al. (2006), Fack and Grenet (2015), Perna and Titus (2004), van der Klaauw (2002)), and particularly with the Chilean evidence on the effects of having access to the CAE loan. With a Regression Discontinuity design, Solis (2017) and Montoya et al. (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 PSU score near 475 points. In addition, our results are smaller due to the fact that 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 in almost 1 pp. — a 3 percent increase relative to non-exposed eligible individuals — while two-year enrollment in vocational institutions fell by 0.5 pp. — equivalent to a 4 percent decrease relative to the same group. Conditional on being enrolled, we also estimate that the dropout rate from universities decreases in almost 2 pp. (a 10 percent decrease), while the effect in vocational institutions is statistically non-significant. This improvement in university retention along with a small deterioration in vocational persistence results from two mechanisms: a sorting effect in ability caused by the diversion effect in enrollment that reduces the likelihood of dropping out in universities while increasing it in vocational institutions (Rodriguez et al., 2016); and a perverse incentive from the CAE loan itself that encourages all institutions to reduce dropout rates given their guarantors role (Rau et al., 2013).

The international literature on the persistence effects of financial aid has found mixed results so far. For example, Glocker (2011) and Chatterjee and Ionescu (2012) discuss the importance of financial aid on retention and completion; while Herzog (2005), Stinebrickner and Stinebrickner (2008) and Stinebrickner and Stinebrickner (2012) find that there are other factors that are more relevant than credit constraints for persistence and graduation. Our results are consistent with recent Chilean evidence. Solis (2017) finds an increase of 16 pp. in university two-year enrollment. Our result is smaller but, again, his finding applies for selected individuals only and considering access to the loan instead of a loosening in credit constraints. Rau et al. (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 gender and family income. Regarding gender, we estimate that the only significant difference between men and women is on immediate enrollment such that for females the diversion from vocational institutions (-3.2 pp.) is not fully compensated by the increase in university enrollment (2.3 pp.). As a result, the interest rate drop has a negative impact on immediate enrollment for women (-0.9 pp.), which is explained by female students delaying their enrollment decision to better prepare for the challenge of being accepted at the university. Along the family-income dimension, which we proxy by high school financing scheme, we find that all of our results in enrollment and persistence are entirely driven by students graduating from voucher high schools (middle-income family) with no effects whatsoever on students from public schools (low-income family). This results from the fact that the student loan under analysis does not cover the full tuition costs so that students still need to finance the remaining difference along with other expenses. Then, the 2012 reform is not large enough to have an impact on low socioeconomic status high school graduates.

From a public policy perspective, our findings suggest that a reform to student loans that loosens credit constraints might have null overall effects on access to education, but it could instead have unintended consequences in the institutional composition of students in dimensions such as ability, gender and socioeconomic status; which in turn translate into nontrivial welfare effects.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of the CHES, the changes introduced to the loan in 2012, and the 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 of our outcomes, analyzes the plausibility of the identification strategy, and studies heterogeneous effects. Section 5 concludes.

2 Background and Data

The Chilean Higher Education System (CHES) 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 of length. Vocational institutions on the other hand, offer technical programs which are mainly between 3 and 4 years of 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 that 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 roughly about $CLP 2.1 million ($USD 2,970), which represents 41% of the median family income in 2015.[6] For the more than 100 vocational institutions, the mean tuition fee was around $CLP 1.1 million ($USD 1,556), representing 21% of the 2015 median family income.

This is of special relevance for students graduating from state-funded public schools and from voucher schools. In the same period of time, 39% of the students came from public schools and

[6] 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 of 12/31/2015.

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% for the vocational institutions case. Finally, for the remainder 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 will assess how the reform heterogeneously impacts graduates from public schools versus graduates from voucher schools.

Students have few options to finance tertiary education. To 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, from a total of 1,165,654 students enrolled in the CHES, 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 others such as transportation and food expenses. Student loans, on the other hand, cover tuition fees only.[7] 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.[8] 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.

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

The process of CAE loan applications and CHES enrollment is structured as follows. Students

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

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

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.

graduating from high school register for the PSU (*Prueba de Selección Universitaria*), a national college admission test that highly determines admission to the CHES and access to grants.[9] During the PSU registration process, individuals planning to apply for the CAE loan (or other grants) must fill a socioeconomic form which is used to determine income eligibility. Once test results are published, academic eligibility is determined, loans are granted and students decide to either enroll or not to their respective programs.

To become a beneficiary of the CAE loan, a high school graduate must fulfill both the academic and the family income eligibility criteria. Only students with a PSU score greater or equal to 475 or high school GPA greater or equal to 5.3 are eligible.[10][11] The socioeconomic criterion is the least relevant of the two since it has changed overtime 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 since 2014 it has been granted based on the academic criteria only, covering applicants from all socioeconomic conditions.

Initially the CAE loan was granted with 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, payments begin 18 months after graduation, and between 2006 and 2011 had an average annual interest rate

[9] 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 average score of the mandatory tests is typically used to assess eligibility for grants.

[10] GPA ranges from 1 to 7.

[11] If a student wishes to enroll to an university then she has to comply with the PSU cutoff, while if she wants to enroll to a vocational institution then she has to comply with either of the thresholds.

of 5.6 percent. In middle 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 with 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 level up the conditions between the two loans and expected to improve repayments following a report that estimated a default rate of 36% and predicted a possible increase to up to 50% (World Bank, 2011).

From a theoretical perspective, this reform accounts for 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 drop is the most relevant one given that the subsidized reduction is automatically applicable to all loans; while the 10%-of-income cap subsidy to repayments and the option to delay them in case of unemployment are available upon request and only a small fraction of debtors has applied for them since its implementation. In 2015 for example, 8% and 4% of the 242,604 CAE debtors were beneficiary 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 period of grace. With a 20-year maturity loan, this is equivalent to an annuity of CLP$ 1.3 million. With the new interest rate of 2%, she would instead owe a total of CLP$ 13.6 million (a 13 percent drop) with an annuity of CLP$ 0.8 million, which represents a non trivial decrease of 37 percent.[12]

This loosening of constraints 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 database from the Ministry of Education that comprises records of all students enrolled in primary and secondary education, from which we built our universe of high school graduates. This source contains relevant information about the student and her high school. 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 score 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

[12] 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, that the debtor does not request contingent payments nor a delay of them, etc.

institutions. Merging all the data through an individual identifier, we built a dataset consisting of every yearly cohort of high school graduates and information on their eligibility, enrollment and persistence.

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 missassigned the CAE loan due to an error in the income sorting of applicants, granting loans in the opposite order (Ingresa, 2010). And secondly, the government introduced a new program in 2016 that made available tuition-free tertiary education 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.[13]

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 by the applicants. To overcome this issue, we drop from our sample all graduates from private high schools in order to resemble as close as possible income eligibility compliance. By doing so — i.e. conditional on being socioeconomically eligible — we exploit eligibility on the academic dimension only. A second concern is related to high school graduates who do not register to take the PSU test, impeding us to determine 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, in consequence, increasing the probabilities of enrollment, persistence, and graduation.

Although the changes introduced in the 2012 reform affected the intensive margin and focused on the repayment period, the drop in the interest rate is substantial enough to motivate the investigation of the educational effects of this loosening in constraints, given that it reduced the cost associated to the investment in education. To identify these causal effects, we use a Difference-in-differences (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 the year immediately following high school graduation. Given that the CAE loan is constrained to eligible 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 those eligible students unexposed to the reform (i.e. eligible individuals from the 2007-2011 cohorts). The difference

[13] See Espinoza and Urzúa (2015) for an initial evaluation of the new tuition free program and Bucarey (2017) for an analysis on other educational effects.

in enrollment between these two groups cannot be uniquely attributed to the reform since other confounding factors could also potentially be explaining part of the difference.

In order to solve this issue, our second difference in enrollment is the one between the two groups of cohorts of ineligible individuals (2012-2015 and 2007-2011). Given that these individuals' decision is not affected by the reform, any difference between the 2007-11 and the 2012-15 cohorts will capture those potential confounders.

With this DiD model we are implicitly assuming 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 estimation base model is:

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

where $eligible_{it}$ is an indicator of CAE-eligibility for high school graduate i of cohort t and $exposed_{it}$ indicates exposure to the reform (i.e., $t \geq 2012$).[14]

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

In this model, the interaction coefficient for $eligible_{it} \times exposed_{it}$ (i.e., β_3), captures the Intention to Treat effect (ITT) of the reform on the enrollment rate. This model is also to be extended to include cohort fixed effects and other relevant covariates as robustness checks for our model specification.

A second specification of our DiD identification strategy is:

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

where the $exposed_{it}$ variable is replaced by the cohort fixed effects $cohort_{jit}$. This model is useful in that it disaggregates the overall effect into yearly effects, providing information about the dynamics. In this case, coefficients β_j of the interaction $eligible_{it} \times cohort_{jit}$ for $j = 2012, \ldots, 2015$ are those of interest, since they capture the evolution of the effect over time. Moreover, the remainder β_j coefficients (i.e. those for $j = 2007, \ldots, 2011$) are of particular interest as well since they allow us to test for the parallel trends assumption.

[14] Note that our data is not longitudinal, as each individual is considered only in the corresponding year of her immediate enrollment decision.

3.2 Two-year Enrollment

Our second and third outcomes focus on persistence decisions. Here we define two-year enrollment as a binary variable that takes the value of 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 at all. Same as with the immediate enrollment outcome, we make use of three variables: (i) overall two-year enrollment, (ii) university two-year enrollment and (iii) vocational two-year enrollment.

The first difference in our DiD setting comes from the comparison between eligible students that 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 one), since those are the first individuals whose decision of a second year of 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.[15] 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 non-exposure as our second difference.

An issue arises with the two-year-enrollment outcome. Eligibility to the CAE loan is potentially endogenous in this setting, given that initially ineligible individuals (i.e. those with PSU < 475 and GPA < 5.3) can retake the PSU test one year later and become eligible if they manage to score above the 475 threshold. For this reason, we use an Instrumental Variables approach within our DiD framework.

The endogenous variable is the overall eligibility within two years following high school graduation ($Eligible_{2it}$). It is given by the student's GPA, which does not change overtime, and the first-attempt PSU score in case she does not retake the test, or the second-attempt PSU score in case she retakes it and scores above her first score. We use as instrument the first-attempt eligibility status ($eligible_{1it}$) which is given by the student's GPA and by her first-attempt PSU score.

A similar identification assumption to that in our DiD model in the previous section (i.e., a paralell trends assumption) provides validity of the instrument in this framework. Relevance of the instrument is also straightforward in this setting: the endogenous variable $Eligible_{2it}$ and its instrument $eligible_{1i}$ are highly correlated by construction since they 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.[16]

Our two stage least squares (2SLS) base model is defined by the structural equation:

$$y_{it} = \beta_0 + \beta_1 \text{Eligible}_{2it} + \beta_2 \text{exposed}_{it} + \beta_3 \text{Eligible}_{2it} \times \text{exposed}_{it} + \epsilon_{it} \tag{3}$$

and by the first-stage equation:

$$\text{Eligible}_{2it} = \gamma_0 + \gamma_1 \text{eligible}_{1it} + \gamma_2 \text{exposed}_{it} + \eta_{it} \tag{4}$$

[15] Note that with this specification we lose one cohort of students, that of 2015, given that we do not observe their second-year decision in 2016.

[16] In this case we would have for that student that $eligible_{1it} = 0$ and $Eligible_{2it} = 1$. Also, note that all first-attempt eligible individuals are also overall eligible as well (i.e. those with $eligible_{1it} = 1$ also have $Eligible_{2it} = 1$).

where $exposed_{it}$ is the dummy variable for students affected by the reform in their second year as already discussed. In this case, the coefficient β_3 of the interaction $Eligible_{2it} \times exposed_{it}$ captures the local average treatment effect (LATE) of the reform on the two-year enrollment rate of compliers.[17] We will also extend our model to include cohort fixed effects and other relevant covariates as robustness checks.

Just as with equation 2, in order to desegregate the effect into yearly effects, we also estimate the following model:

$$y_{it} = \beta_0 + \beta_1 \text{Eligible}_{2it} + \sum_{j=2008}^{2015} \alpha_j \text{cohort}_{jit} + \sum_{j=2008}^{2015} \beta_j \text{Eligible}_{2it} \times \text{cohort}_{jit} + \epsilon_{it} \tag{5}$$

$$\text{Eligible}_{2it} = \gamma_0 + \gamma_1 \text{eligible}_{1it} + \sum_{j=2008}^{2015} \gamma_j \text{cohort}_{jit} + \eta_{it} \tag{6}$$

where Equation 5 corresponds to the 2SLS structural equation,Equation 6 is the 2SLS first-stage equation, and the exposure variable $exposed_{it}$ is replaced and disaggregated by the cohort fixed effects $cohort_{jit}$. In this model, the parameters of interest are the β_j of the interaction $Eligible_{2it} \times cohort_{jit}$ for $j = 2012, \ldots, 2015$ to capture the dynamics of the effect and for $j = 2008, \ldots, 2011$ to test for the parallel trends assumption.

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 onto the second year of enrollment. To disentangle this information and know about the marginal effect on the second year decision we make use of our third and last outcome.

3.3 Second-year Dropout

Our third outcome variable is second-year dropout. Analysis of dropout decisions is conditional on being enrolled: our subsample of study comprises all high school graduates that immediately enrolled in the CHES in the 2007-2014 period and we will be interested in their dropout decision for the following year. Any results from this model should be interpreted with caution since there might be an issue of sample selection.

Given that we only have enrollment records at the beginning of each period, we do not observe if a student completed the year or not. For this reason, we define second-year dropout as a binary outcome that takes the value of 1 if we do not observe a student's registration at the beginning of her second year, regardless of whether she completed her first academic period or not.

Because estimation is now conditional on enrollment, we no longer define three binary variables but analyze second-year dropout across types of institution (i.e., universities and vocational institutions) instead. A 2SLS model just like equations 3 and 4 is used to solve for the potential endogeneity of the overall eligibility condition. Again, the interaction coefficient β_3 captures the effect of interest.

[17] In this setting a complier is a student that enrolls for two consecutive years only if initially eligible to the loan. See Angrist et al. (1996) for details.

Equations 5 and 6 are replicated for this outcome as well to analyze dynamics and test for the parallel trends assumption where, again, the parameters of interest are those of the interactions β_j.

4 Results

This section presents and discusses our main results. For completeness and to better understand the Chilean context, Table 2 presents some descriptive information for selected cohorts.[18] Our sample consists of over 1.5 million high school graduates, 40% of which come from a public school and the remaining 60% from a voucher school. The overall female/male ratio is of 1.15. Eligibility to CAE loan has increased its coverage from 75% in 2007 to 82% in 2015.

Enrollment in the CHES has an upward trend overtime with an annual growth rate of 2.3%, mainly explained by growth in vocational enrollment (4.9% vs 0.7%). Overall, one half of our sample of high school graduates immediately enrolls to the CHES. Within our period of study, the gender gap in enrollment decreased by two thirds from 3 pp. to 1.1 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 retention in the CHES, 36% of high school graduates in our sample enrolls for two consecutive years, with an annual growth rate of 2.5% and driven, once again, by vocational permanence (6.3% vs 0.2%). The gender gap was very small in 2007 and not only it disappeared but at the end of the sample period females are more likely than males to enroll for two years.[19] The gap in the trends by type of school is very similar to that of immediate enrollment, with students from public schools close to 6 pp. less likely to enroll for two years than students from voucher schools.

Also regarding retention in tertiary education, one in every four students enrolled in the CHES drops in her second year of studies. While the dropout rate has decreased over time in vocational institutions, it has marginally increased in universities. In every year of our period of study, females are less likely to drop than males by nearly 3 pp. The gap in dropout rates by type of school has remained stable overtime at about 2 pp.

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

In order to assess the relative sizes of our estimates, we report the respective number of non-exposed eligible individuals and their outcome mean in most tables. As a robustness check, we add year effects and three types of control variables to our base models. Student level variables include gender, attendance rate, *comuna*, and number of family members at different levels in the education system. School level variables include indicators of financing scheme (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 and program duration.

[18] See Appendix A for detailed information on all our cohorts.

[19] See Becker et al. (2015) and Becker et al. (2010) for an analysis of the overtaking of men by women in higher education.

Table 2: Descriptive Statistics

	Cohort				
	2008	2010	2012	2014	Pooled
Immediate Enrollment	0.457	0.466	0.515	0.543	0.496
Two-Year Enrollment	0.330	0.346	0.355	0.398	0.364
Second Year Dropout	0.266	0.241	0.273	0.261	0.257
Eligible	0.779	0.771	0.769	0.794	0.778
PSU	475	473	475	477	475
GPA	5.6	5.6	5.6	5.6	5.6
Females	0.546	0.531	0.534	0.532	0.534
Public School	0.423	0.421	0.362	0.365	0.396
Obervations	147,480	180,306	169,824	174,789	1,527,798

4.1 Effects on Immediate Enrollment

Table 3 presents 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 from adding 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, given that eligibility is determined by academic variables, which are arguably related to ability, results suggest that more able students are more likely to enroll. However, when we disaggregate by type of CHES institution, we find that this result is driven by university enrollment: eligible students are more likely to enroll to universities and slightly less likely to enroll to vocational institutions. This could be explained by 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 to universities in detriment of vocational institutions by 2.5 pp. In absolute terms, this result implies that approximately 16,000 out of 636,760 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 non-exposed eligible individuals.

Our results are consistent with others found in the literature, although of a smaller magnitude. By means of a Regression Discontinuity Design (RDD) Solis (2017) uses cohorts 2007-09 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 et al. (2018) analyze labor market effects and within their model also estimate 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

Table 3: Immediate Enrollment

	Overall			University			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible	0.254***	0.254***	0.237***	0.286***	0.286***	0.267***	-0.032***	-0.032***	-0.030***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Exposed	0.063***	0.074***	0.080***	-0.011***	-0.025***	-0.022***	0.074***	0.099***	0.103***
	(0.003)	(0.007)	(0.006)	(0.001)	(0.007)	(0.007)	(0.003)	(0.004)	(0.004)
Eligible × exposed	0.001	0.001	-0.001	0.026***	0.026***	0.025***	-0.025***	-0.026***	-0.025***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(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	1,527,798	1,527,798	1,527,797	1,527,798	1,527,798	1,527,797	1,527,798	1,527,798	1,527,797
Control group size	636,760	636,760	636,760	636,760	636,760	636,760	636,760	636,760	636,760
Outcome mean	0.524	0.524	0.524	0.348	0.348	0.348	0.176	0.176	0.176

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 financing institution, rural area and geographical region. Student level control variables include gender, attendance rate, *comuna* and number of family members at different leves 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.

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.

Two reasons explain the difference with our results. First, we focus on a reform that introduced changes in the intensive margin (i.e. an interest rate drop 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 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 poins), while our results are interpreted as an average for the treated individuals.

The shift in institutional choice from vocational institutions to universities is explained — in line with Angrist et al. (2016) — by the implicit subsidy the interest rate drop creates for universities. Given that enrolling in this type of institutions entitles more costs both in pecuniary (i.e. tuition fees) and timely (i.e. program length) terms, the loosening of credit constraints is of a bigger scale for the choice of attending universities; which in turn, further increases the relative incentive to enroll in an university in comparison to a vocational institution.

In addition, this diversion effect implies a welfare effect that depends on the characteristics of the individuals that shifted their enrollment decision toward universities as a result of the 2012 reform. Rodriguez et al. (2016) propose a structural schooling decision model to simulate the effects of a reduction of tuition costs in Chile — which can be interpreted as a loosening in credit constraints and therefore similar to the interest rate drop — 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 lengthy 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, in consequence, (iv) that there is a non trivial 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 means that individuals deciding to enroll in an 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.

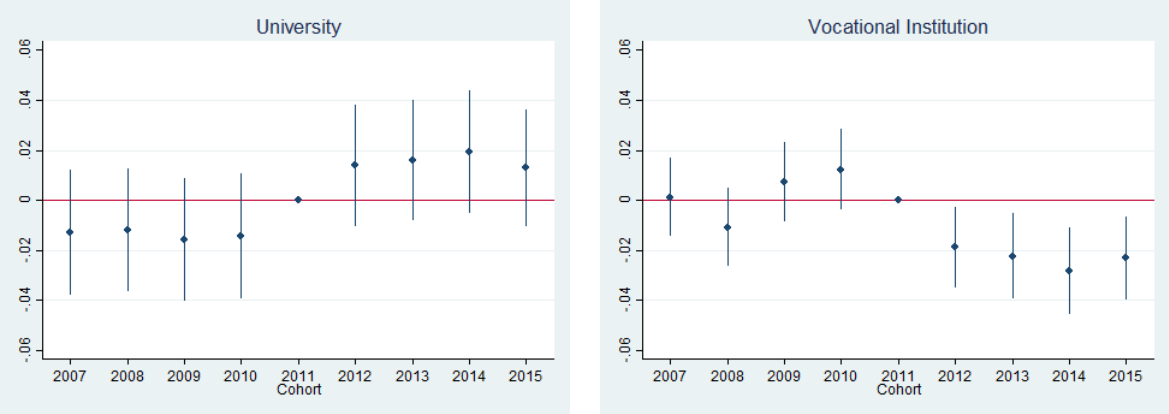

Figure 1: Dynamics of Immediate Enrollment

Figure 1 represents the dynamics of the effect on immediate enrollment by depicting the β_j interaction (i.e., $eligible \times cohort_j$) coefficient estimates described in equation 2, along with their corresponding 99% confidence intervals. Detailed estimation results and robustness checks are presented in Appendix B. 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 can see 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 strongly demanding test of the parallel trends assumption: for each year previous to the reform we cannot reject the null hypothesis of non-significance for both the university and the vocational enrollment variables.[20]

4.2 Effects on Retention

We next turn our attention to the effects on retention in tertiary education measured by our two-year enrollment and second-year dropout variables. Table 4 presents the 2SLS results for two-year enrollment. Just as with the immediate enrollment results, columns (1), (4) and (7) display the results for our base model as in equation 4. Columns (2), (5) and (8) add year fixed effects and columns (3), (6) and (9) add further control variables.

Panel B presents first-stage estimation results, showing how strong our initial-eligibility variable is as an instrument for the overall-two-year eligibility. Panel A presents the estimation results for the (LATE) effect of the reform. For this case we also find a null effect in overall persistence. However, there is a statistically significant diversion effect, similar (but smaller in magnitude) to that of immediate enrollment.

[20] Appendix C presents additional evidence in favor of the Parallel Trends assumption for all our outcomes.

Table 4: Two Year Enrollment

	General			University			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Structural Equation									
Eligible_2	0.264***	0.264***	0.246***	0.250***	0.250***	0.235***	0.014***	0.013***	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)
Exposed	0.034***	0.060***	0.064***	-0.007***	-0.011*	-0.014**	0.040***	0.072***	0.078***
	(0.002)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.002)	(0.003)	(0.003)
$\text{Eligible}_2 \times$ exposed	0.005	0.004	0.003	0.009**	0.009**	0.009*	-0.005*	-0.005**	-0.006**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)
Panel B: First-stage Equation									
eligible_1	0.963***	0.963***	0.963***	0.963***	0.963***	0.963***	0.963***	0.963***	0.963***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Exposed	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Cragg-Donald F-stat	1.4E7	1.4E7	1.3E7	1.4E7	1.4E7	1.3E7	1.4E7	1.4E7	1.3E7
Kleibergen-Paap F-stat	7.1E5	7.1E5	7.1E5	7.1E5	7.1E5	7.1E5	7.1E5	7.1E5	7.1E5
Year effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,347,837	1,347,837	1,347,837	1,347,837	1,347,837	1,347,837	1,347,837	1,347,837	1,347,837
Control group size	499,983	499,983	499,983	499,983	499,983	499,983	499,983	499,983	499,983
Outcome mean	0.399	0.399	0.399	0.275	0.275	0.275	0.124	0.124	0.124

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 financing institution, rural area and geographical region. Student level control variables include gender, attendance rate and number of family members at different leves in the education system. Control group size accounts for the number of eligible individuals in the before period, while Outcome mean refers to the mean of the dependent variable of those individuals.

The loosening of credit constraints not only leads individuals to be more likely to choose universities but it also encourages them to continue for a second year of studies while diminishing the likelihood of enrolling and staying in a vocational institution. As a result, two-year university enrollment increases in almost 1 pp. (a 3 percent increase relative to non-exposed eligible individuals) while it drops by 0.5 pp. in vocational institutions (a 4 percent relative to the same group). Solis (2017) measures persistence in universities with two-year enrollment within three years after high school graduation and finds an increase of 16 pp., equivalent to a 50 percent increase. Our result is smaller but, again, his finding applies for near-the-cutoff individuals and considering access to the loan instead of a change in repayment conditions.

Results for second-year dropout in Table 5 allow us to further investigate the effects of the reform on retention. Again, columns differ in the inclusion of year effects and other control variables. Panel A presents the effect of the reform and Panel B shows the relevance of our instrument. Among students enrolled in an university, the reform is correlated with a decrease of almost 2 pp. in the dropout rate, which amounts to a 10 percent decrease relative to non-exposed eligible individuals. For vocational institutions on the other hand, we estimate a null effect on the dropout rate.

This difference in retention effects across institutions — i.e. an improvement in university persistence with no changes in vocational institutions — results from two operating mechanisms. The first one is the sorting effect created by the reform that diverts more able individuals to enroll in universities instead of vocational institutions as already discussed in the previous section. As a result, the ability distribution ameliorates in universities while it moves in the opposite direction in

Table 5: Second-year Dropout

	University			Vocational		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Structural Equation			
Eligible_2	-0.227***	-0.227***	-0.163***	-0.178***	-0.177***	-0.150***
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Exposed	0.048***	0.037***	0.039***	-0.016***	-0.061***	-0.027***
	(0.008)	(0.009)	(0.009)	(0.005)	(0.007)	(0.007)
$\text{Eligible}_2 \times$ exposed	-0.019**	-0.019**	-0.019**	0.005	0.004	0.002
	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)
			Panel B: First-stage Equation			
eligible_1	0.989***	0.989***	0.988***	0.996***	0.996***	0.996***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Exposed	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)	(0.000)
Cragg-Donald F-stat	1.6E7	1.6E7	1.6E7	2.8E7	2.8E7	2.7E7
Kleibergen-Paap F-stat	4.0E5	4.0E5	4.9E5	4.1E6	4.1E6	4.2E6
Year effects	No	Yes	Yes	No	Yes	Yes
Control variables	No	No	Yes	No	No	Yes
Observations	386,329	386,329	375,297	273,715	273,715	272,737
Control group size	170,722	170,722	170,722	84,303	84,303	84,303
Outcome mean	0.195	0.195	0.195	0.266	0.266	0.266

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 financing institution, rural area and geographical region. Student level control variables include gender, attendance rate and number of family members at different leves in the education system. Control group size accounts for the number of eligible individuals in the before period, while Outcome mean refers to the mean of the dependent variable of those individuals.

vocational institutions. And given that ability is negatively correlated with the dropout probability as documented by Rau et al. (2013) and Rodriguez et al. (2016), then retention measures improve for universities while they worsen for vocational institutions.

The second mechanism comes from a perverse incentive originated by the CAE loan itself. Rau et al. (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.[21]. In consequence, we find that persistence improves in universities following this perverse incentive which is boosted by the sorting effect, while the two mechanisms operate in opposite directions for vocational institutions.

[21] As already discussed in Section 2.1, higher education institutions in Chile are guarantors for CAE debtors until graduation and absorb the dropout risk. Rau et al. (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 retention outcomes.[22] The top panel depicts dynamics of the effect on two-year enrollment and the bottom panel on the second-year dropout. Effects for university are shown in left panels and for vocational institutions in right panels. Out of 16 β_j interaction coefficients for $j = 2008, \ldots, 2011$, 13 are not statistically significant, supporting strong evidence of the plausibility of the parallel trends assumption.[23] Regarding the university post-2012 coefficients, we can see that the ones associated to year 2012 are very close to zero for both two-year enrollment and second-year dropout. This reinforces our argument of a sorting effect in enrollment given that the 2012-effects correspond to the 2011 cohort that was exposed to the intervention only after the first year enrollment choices had already been made. The first cohort fully exposed to the reform is that of 2012, and they had to decide in 2013 whether to enroll for a second year or drop out. It is precisely in this year that we observe the biggest effect in dropout and, at the same time, the first non-zero effect in two-year enrollment. In the case of vocational institutions, the trend in the coefficients is very flat with post-2012 coefficients even closer to zero than the pre-2012 coefficients; this suggests, once again, a null effect of the reform on vocational retention.

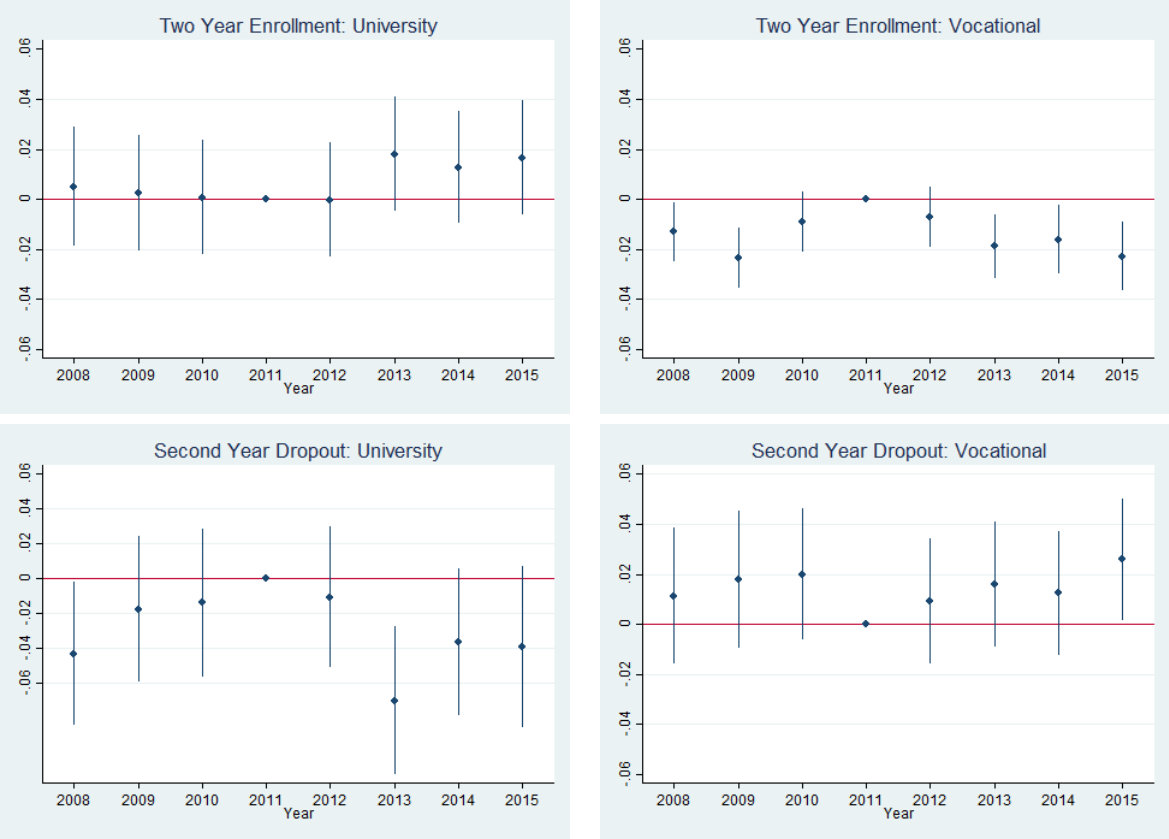

Figure 2: Dynamics of Permanence-related variables

[22] Detailed estimation results and robustness checks are presented in Appendix B.

[23] Appendix C presents additional evidence in favor of the Parallel Trends assumption for all our outcomes.

Finally, the loosening of credit constraints analyzed in this paper will likely have other unintended consequences on outcomes beyond educational attainment, such as labor market outcomes and other long term related variables. Unfortunately, at the moment of writing of this paper there is not enough available information to properly evaluate these effects since cohorts exposed to the reform are just graduating, entering the labor market and beginning to repay their debts. Nevertheless, we can link our results to the literature that focuses on access (extensive margin) to student loans in general and to the CAE loan in particular to anticipate some potential consequences of the reform (intensive margin).

Montoya et al. (2018) suggest that the university-vocational substitution implies longer time to graduation, since programs at universities are of longer length, which in turn translates into less accumulated experience. Moreover, they analyze the long term effects (11 years after high school graduation) of loan eligibility on other labor market outcomes and find that there is no difference between graduating from an university or from a vocational institution in terms of annual earnings, participation or job stability. If anything, the authors argue that the investment in university education doubles the monetary cost in comparison to vocational institutions, concluding that "*for individuals up to age 30 in Chile, college does not pay off relative to vocational education*". Bucarey et al. (2018) also follow an RDD approach to analyze the labor market returns to the CAE loan for students enrolled in universities, with similar findings: later completion, larger debt, lower experience, and no difference in wages and employment around the 475-PSU threshold. Recent literature in contexts other than the CHES has focused on several long-term outcomes such as the type of jobs chosen (Rothstein and Rouse, 2011), family planning (Kaufmann et al., 2013), homeownership (Mezza et al., 2016), retirement savings (Elliott et al., 2013), and even intergenerational effects (Kaufmann et al., 2015). Studying the long term effects of loosening credit constraints such as the CAE reform of 2012 or even the free-tuition reform of 2016 will be of great importance in the years to come.

4.3 Heterogeneity

This section analyzes the extent to which enrollment and retention effects of the reform are heterogeneous across two dimensions: gender and high school financing scheme (public versus voucher). We approach this question by estimating equation 1 in the case of enrollment and equations 3 and 4 in the case of persistence outcomes separately for female and male students (Table 6) and public and voucher schools (Table 10). Each table presents the reduced-form estimated effects for each subsample and their difference, along with the corresponding standard errors. We perform seemingly unrelated estimation (SUEST) in order to allow for correlation between subsample estimates.[24] Standard errors are clustered at the class level.

4.3.1 Female versus Male Students

Results in Table 6 suggest significant heterogeneity in enrollment decisions across the gender dimension. While there is no statistically significant difference in immediate university enrollment, the impact of the reform on vocational enrollment is stronger for female students (negative for both), with a difference of -1.3 pp. (significant at the 1% level).

[24] See Weesie (1999) for details.

Table 6: Gender Analysis

	General			University			Vocational		
	Female	Male	Difference	Female	Male	Difference	Female	Male	Difference
				Immediate Enrollment					
Eligible × after	-0.009**	0.004	-0.013**	0.023***	0.023***	0.000	-0.032***	-0.019***	-0.013***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.004)	(0.005)
				Two Year Enrollment					
Eligible × after	-0.000	0.004	-0.004	0.010**	0.006	0.005	-0.011***	-0.001	-0.009**
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.003)	(0.003)	(0.004)
				Second Year Dropout					
Eligible × after				-0.011	-0.025**	0.014	-0.002	0.005	-0.007
				(0.011)	(0.011)	(0.016)	(0.006)	(0.006)	(0.009)
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: SUEST 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 financing institution, rural area and geographical region. Student level control variables include attendance rate, *comuna* and number of family members at different leves in the education system.

In the case of males, university enrollment increases by 2.3 pp. in detriment of the vocational enrollment, which decreases by 1.9 pp. (both estimates significant at 1%). Although positive (0.4 pp.), the estimate of the overall effect is not statistically significant. In the case of females, however, the decrease of 3.2 pp. in vocational enrollment (significant at 1%) is not fully compensated by the increase of 2.3 pp. in university enrollment (significant at 1%). The overall estimated effect is a decrease of 0.9 pp. in immediate CHES enrollment for female students (significant at 5%).

The results for two-year enrollment follow a similar pattern, which is reasonable due to the way variables are defined. However, the negative overall effect for females disappears, and none of the results for males are statistically significant. The only significant result for the second-year dropout rate is a 2.5 pp. decline (significant at 5%) for male students enrolled at universities. Nevertheless, we cannot reject equality to the effect for females.

While the negative overall effect on CHES enrollment for female students might seem somewhat counterintuitive, it can be explained in light of the definition of our immediate enrollment outcome. It is possible that, while the reform induced a group of female high school graduates to switch from vocational institution enrollment to university enrollment, some of them did immediately (the year after graduation) and others delayed their enrollment decision. This delay could be an optimal response since eligibility criteria is harder to meet when enrolling in an university.[25] Evidence in Table 7 is consistent with our claim where we analyze the evolution of the proportions of female and male students delaying their PSU assessment in a DiD framework and find that the proportion of women delaying the test increases relative to the corresponding proportion of men after the reform.

[25]Recall that the academic criteria for CAE eligibility is more stringent if the student enrolls in an university: passing the PSU cutoff is required, whereas meeting either PSU or GPA cutoff is sufficient if the student enrolls in a vocational institution.

For this, we estimate the following equation

$$\text{delay}_{it} = \beta_0 + \beta_1 \text{female}_i + \beta_2 \text{after}_t + \beta_3 \text{female}_i \times \text{after}_t + \epsilon_{it}$$

where the outcome variable, delay_{it}, is defined as an indicator that the student sat the PSU test at least once, but not immediately — i.e., not during the corresponding year t of high school graduation.

Table 7: PSU Delay

	Delayed PSU
Female	-0.005***
	(0.001)
After	-0.022***
	(0.001)
Female × After	0.002**
	(0.001)
Observations	2,007,043

Notes: Clustered standard errors at the class level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Still, a question remains of why the reform induced women to delay enrollment but not men. There is a vast literature relating the life-cycle production of cognitive and noncognitive skills to a wide range of outcomes such as schooling attainment, labor market outcomes, and even some risky behaviors (Cunha and Heckman, 2007, 2008; Heckman et al., 2006). While cognitive skills seem to be similarly distributed among men and women (Bound et al., 1986), there is evidence that women tend to have higher average, and less dispersed noncognitive skills than men and that this difference can explain the recent boom in higher education of women (Becker et al., 2015, 2010). The decision to delay the timing of CHES enrollment might reasonably require certain levels of noncognitive skills. Following Becker et al. (2015) that argue that grades represent "*a crude but broad measure of noncognitive skills relevant to schooling*" we present in Table 8 evidence that female students have higher noncognitive abilities than males. Here we show yearly differences in average standardized GPA between men and women. We standardize each year's data by the corresponding sample means and standard deviations of female's GPAs. Men's average GPA is systematically below (around 0.3 standard deviations) that of women through time.[26].

Moreover, if women tend to score lower in the PSU than men, these two factors could induce them to delay their CHES enrollment: in order to enroll in an university and have access to the CAE at the new conditions, female students might need to improve their expected performance in the test and they have the patience and self-control that this task requires. Table 9 presents evidence suggesting that Chilean female students perform worse in the PSU than male students. We show yearly differences in standardized PSU scores between men and women and the corresponding standard errors. We standardize by the corresponding yearly sample means and standard deviations of

[26]This is consistent with evidence found in the U.S. (Becker et al., 2015; Conger and Long, 2010)

Table 8: GPA Gender Gap

	2007	2008	2009	2010	2011	2012	2013	2014	2015
GPA gender gap	-0.31***	-0.31***	-0.30***	-0.31***	-0.30***	-0.28***	-0.29***	-0.30***	-0.35***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)

Notes: Difference of Mean GPA (standardized by corresponding year sample mean and standard devation of female students) between male and female students and the corresponding standard errors (in parentheses). *** p<0.01, ** p<0.05, * p<0.1.

women's scores. The first column shows the difference in the average of language and mathematics scores, which determines CAE eligibility. The following columns report the differences in each individual test: mathematics, language, science, and history and social sciences. Men tend to do better than women in every dimension of the test.[27] This result is consistent with other findings in the literature documenting that, while men perform better in mathematics, women tend to do better in language (Fryer and Levitt, 2010; Marks, 2008). In our restricted sample — i.e. registered students from non-private schools — men have higher scores in language, but the gender gap in this dimension is less than half the gaps in the other dimensions. The difference in the math and language average is noteworthy: men's mean score is systematically around ten percent of a standard deviation higher than women's, which makes eligibility for university-CAE harder to achieve for female students. Appendix D presents additional evidence supporting our claim.

Table 9: PSU Scores Gender Gap

	Math and Language Average	Math	Language	Science	History and Social Sciences
2007	0.09***	0.13***	0.05***	0.11***	0.12***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2008	0.10***	0.16***	0.04***	0.10***	0.15***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2009	0.10***	0.14***	0.06***	0.13***	0.15***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)
2010	0.09***	0.15***	0.03***	0.11***	0.12***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)
2011	0.11***	0.15***	0.05***	0.12***	0.12***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)
2012	0.09***	0.16***	0.03***	0.11***	0.15***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)
2013	0.11***	0.16***	0.06***	0.12***	0.13***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)
2014	0.11***	0.15***	0.07***	0.12***	0.13***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)
2015	0.11***	0.16***	0.05***	0.10***	0.15***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)

Notes: Difference of Mean PSU scores (standardized by corresponding year sample mean and standard devation of female students) between male and female students and the corresponding standard errors (in parentheses). *** p<0.01, ** p<0.05, * p<0.1.

[27] Note that this does not necessarily imply that women have lower cognitive skills. For example, the gender gap could be driven by differential effects of a competitive test setting on men and women's performance (Niederle and Vesterlund, 2010).

4.3.2 Public school vs Voucher school students

Table 10 analyzes the differences in the enrollment and persistence effects between individuals graduating from voucher and public schools.[28][29] Results suggest that the diversion effect in immediate enrollment, the small pass-through to two-year enrollment, and the decrease in university dropout rates we found in the pooled sample are entirely driven by students graduating from voucher schools. The loosening of credit constraints had no effect whatsoever on eligible students coming from public schools. There are two differences between public and voucher schools that help us explain why public school graduates did not respond to the reform.

Table 10: High School Financing Scheme Analysis

	General			University			Vocational		
	Public	Voucher	Difference	Public	Voucher	Difference	Public	Voucher	Difference
Immediate Enrollment									
Eligible × after	0.002	-0.001	0.003	0.009	0.030***	-0.021**	-0.007	-0.031***	0.024***
	(0.006)	(0.004)	(0.007)	(0.008)	(0.005)	(0.009)	(0.005)	(0.004)	(0.006)
Two Year Enrollment									
Eligible × after	0.001	0.006	-0.005	-0.002	0.014***	-0.016*	0.004	-0.008**	0.012**
	(0.006)	(0.004)	(0.007)	(0.007)	(0.004)	(0.009)	(0.004)	(0.003)	(0.005)
Second Year Dropout									
Eligible × after				-0.019	-0.022**	0.003	0.009	-0.004	0.012
				(0.014)	(0.010)	(0.017)	(0.007)	(0.006)	(0.009)
Cohort effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: SUEST 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 rural area and geographical region. Student level control variables include gender, attendance rate, *comuna* and number of family members at different leves in the education system.

First, students in public schools tend to attain lower scores in standardized tests than students in voucher schools. Literature focused on the effects of voucher systems has found a sorting effect of the introduction of voucher schools in Chile with the "best" public school students moving to voucher schools (Hsieh and Urquiola, 2006; Urquiola, 2016). Table 11 repeats the exercise of Table 9 for voucher and public schools. Graduates of public schools score systematically lower (around 0.9 standard deviations in the math and language average) than graduates of voucher schools. Appendix E presents further evidence for this claim.

[28] Good descriptions of the Chilean secondary education system can be found, for example, in Anand et al. (2009), Hsieh and Urquiola (2006), Mizala and Romaguera (2000), Sapelli and Vial (2002), and Torche (2005).

[29] As it was discussed in Section 2.2, students from private high schools are dropped from the analysis throughout the paper.

Table 11: PSU Scores Voucher versus Public School Gap

	Math and Language Average	Math	Language	Science	History and Social Sciences
2007	0.09***	0.09***	0.08***	0.04***	0.07***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2008	0.09***	0.08***	0.08***	0.03***	0.06***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2009	0.08***	0.09***	0.07***	0.04***	0.08***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2010	0.08***	0.09***	0.08***	0.03***	0.07***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
2011	0.10***	0.10***	0.08***	0.04***	0.07***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2012	0.09***	0.10***	0.08***	0.04***	0.07***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2013	0.10***	0.12***	0.08***	0.06***	0.07***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2014	0.08***	0.10***	0.06***	0.05***	0.06***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2015	0.08***	0.11***	0.06***	0.06***	0.06***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)

Notes: Differences of Mean PSU scores (standardized by corresponding year sample mean and standard devation of public school graduates) between graduates of voucher and public schools and the corresponding standard errors (in parentheses). *** p<0.01, ** p<0.05, * p<0.1.

Second, public high 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" which is typically lower than actual tuition fees. This means that even being granted the loan, students (or their families) need sufficient liquidity to cover a non-negligible fraction of the tuition fee. It is arguably harder for poorer households to cover this expense.

Thus, as public school graduates tend to be poor and score low in the PSU, the reform does not actually affect their marginal incentives. Our evidence suggests that the 2012 intensive margin changes in credit access are not big enough to improve educational attainments among low-family-income students.

5 Conclusions

In this paper, we analyze the effects on enrollment and retention in higher education of a reform to student loans that loosened credit constrains by decreasing the interest rate from approximately 6% to a fixed rate of 2%, along with other minor changes that improved repayment conditions. By using a Difference-in-difference approach we exploit these changes to state-guaranteed CAE loans that took place in Chile in 2012.

Our results show that the reform had no effect on overall enrollment in the CHES. Interestingly, we find a diversion effect: enrollment to universities increased by 2.5 p.p. — a 7 percent increase relative to the enrollment rate of eligible students who graduated before the reform — in detriment of enrollment to vocational institutions that fell by 2.5 p.p. — equivalent to a decrease of 14 percent in enrollment relative to the same group. This institutional shift from vocational institutions to universities imply welfare effects given that some diverted individuals would be likely better off had they pursued a vocational degree instead.

In addition we find that retention in universities improves both in two-year enrollment and second-year dropout; while it slightly worsens in vocational institution as a result of a sorting effect in enrollment in conjunction with a perverse incentive to reduce dropout rates by institutions. We also find that for female students the reform had a negative effect on overall enrollment since the decrease in enrollment to vocational institution is not fully offset by the increase in enrollment to universities. We argue this result stems from female students delaying enrollment. Finally, all of our results are entirely driven by students from voucher schools, with null effects for students graduating from public schools.

Our findings constitute important lessons for policymakers — in the CHES and other similar — of the unintended consequences of reforms that loosen constraints. Future research on the long term implications will be of big importance in order to have a complete picture of the short and long run welfare effects.

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Appendix A

Table 12: Descriptive Statistics

	Cohort									
	2007	2008	2009	2010	2011	2012	2013	2014	2015	Pooled
Immediate Enrollment	0.449	0.457	0.456	0.466	0.488	0.515	0.538	0.543	0.540	0.496
by Institution										
University	0.284	0.292	0.274	0.273	0.285	0.299	0.297	0.298	0.299	0.289
Vocational	0.165	0.165	0.182	0.193	0.203	0.216	0.242	0.245	0.242	0.207
by Gender										
Females	0.436	0.443	0.451	0.463	0.488	0.517	0.539	0.540	0.535	0.492
Males	0.466	0.474	0.462	0.468	0.488	0.513	0.537	0.546	0.546	0.501
by High School										
Public	0.400	0.416	0.418	0.423	0.448	0.462	0.494	0.505	0.503	0.451
Voucher	0.489	0.487	0.483	0.497	0.516	0.545	0.563	0.565	0.562	0.526
Two-Year Enrollment		0.330	0.343	0.346	0.354	0.355	0.379	0.398	0.403	0.364
by Institution										
University		0.223	0.227	0.219	0.220	0.217	0.228	0.226	0.228	0.223
Vocational		0.107	0.115	0.127	0.134	0.138	0.151	0.172	0.175	0.141
by Gender										
Females		0.324	0.338	0.347	0.360	0.363	0.386	0.404	0.408	0.367
Males		0.336	0.347	0.345	0.348	0.346	0.370	0.391	0.398	0.361
by High School										
Public		0.289	0.309	0.311	0.315	0.319	0.333	0.357	0.367	0.324
Voucher		0.362	0.367	0.371	0.383	0.380	0.404	0.421	0.424	0.391
Second Year Dropout		0.266	0.250	0.241	0.239	0.273	0.265	0.261	0.257	0.257
by Institution										
University		0.216	0.221	0.202	0.195	0.239	0.237	0.238	0.232	0.223
Vocational		0.352	0.302	0.300	0.303	0.320	0.304	0.289	0.287	0.305
by Gender										
Females		0.256	0.235	0.231	0.224	0.256	0.254	0.250	0.244	0.244
Males		0.277	0.267	0.252	0.257	0.291	0.277	0.273	0.272	0.271
by High School										
Public		0.277	0.256	0.255	0.255	0.288	0.279	0.277	0.272	0.270
Voucher		0.259	0.246	0.232	0.230	0.264	0.258	0.253	0.249	0.249
Eligible	0.752	0.779	0.767	0.771	0.767	0.769	0.782	0.794	0.815	0.778
PSU	474.827	474.858	474.420	472.508	475.556	474.615	476.212	476.832	478.672	475.402
GPA	5.564	5.600	5.581	5.585	5.581	5.596	5.611	5.643	5.683	5.606
Females	0.541	0.546	0.537	0.531	0.527	0.534	0.531	0.532	0.529	0.534
Public School	0.443	0.423	0.422	0.421	0.405	0.362	0.363	0.365	0.367	0.396
Obervations	146,410	147,480	171,300	180,306	184,636	169,824	174,909	174,789	178,144	1,527,798

Appendix B

Table 13: Dynamics of Immediate Enrollment

	University		Vocational	
	(1)	(2)	(3)	(4)
Eligible	0.297***	0.278***	-0.034***	-0.034***
	(0.007)	(0.007)	(0.004)	(0.004)
Cohort 2007	0.013***	0.015***	-0.039***	-0.043***
	(0.003)	(0.004)	(0.005)	(0.005)
Cohort 2008	0.012***	0.011***	-0.029***	-0.032***
	(0.003)	(0.004)	(0.006)	(0.005)
Cohort 2009	0.001	0.003	-0.027***	-0.027***
	(0.003)	(0.004)	(0.006)	(0.005)
Cohort 2010	-0.002	0.001	-0.020***	-0.021***
	(0.003)	(0.004)	(0.006)	(0.005)
Cohort 2012	0.002	0.003	0.028***	0.030***
	(0.003)	(0.004)	(0.005)	(0.005)
Cohort 2013	-0.006**	-0.004	0.056***	0.057***
	(0.002)	(0.004)	(0.006)	(0.005)
Cohort 2014	-0.011***	-0.010***	0.066***	0.067***
	(0.002)	(0.004)	(0.006)	(0.006)
Cohort 2015	-0.011***	-0.010***	0.059***	0.060***
	(0.002)	(0.004)	(0.006)	(0.005)
Eligible × cohort 2007	-0.013	-0.016*	0.001	0.003
	(0.010)	(0.010)	(0.006)	(0.006)
Eligible × cohort 2008	-0.012	-0.012	-0.011*	-0.010
	(0.010)	(0.009)	(0.006)	(0.006)
Eligible × cohort 2009	-0.016*	-0.017*	0.007	0.007
	(0.009)	(0.009)	(0.006)	(0.006)
Eligible × cohort 2010	-0.014	-0.017*	0.012*	0.013**
	(0.010)	(0.010)	(0.006)	(0.006)
Eligible × cohort 2012	0.014	0.010	-0.019***	-0.017***
	(0.009)	(0.009)	(0.006)	(0.006)
Eligible × cohort 2013	0.016*	0.011	-0.022***	-0.021***
	(0.009)	(0.009)	(0.007)	(0.007)
Eligible × cohort 2014	0.019**	0.016*	-0.028***	-0.027***
	(0.010)	(0.010)	(0.007)	(0.007)
Eligible × cohort 2015	0.013	0.010	-0.023***	-0.022***
	(0.009)	(0.009)	(0.006)	(0.006)
Control variables	No	Yes	No	Yes
Observations	1,527,798	1,527,797	1,527,798	1,527,797

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 financing institution, rural area and geographical region. Student level control variables include gender, attendance rate, *comuna* and number of family members at different leves in the education system.

Table 14: Dynamics of Two Year Enrollment

	University		Vocational	
	(1)	(2)	(3)	(4)
Eligible_2	0.248***	0.233***	0.024***	0.021***
	(0.006)	(0.007)	(0.003)	(0.003)
$\text{Eligible}_2 \times$ year 2008	0.005	0.005	-0.013***	-0.012***
	(0.009)	(0.009)	(0.005)	(0.005)
$\text{Eligible}_2 \times$ year 2009	0.003	0.003	-0.024***	-0.023***
	(0.009)	(0.009)	(0.005)	(0.005)
$\text{Eligible}_2 \times$ year 2010	0.001	0.001	-0.009*	-0.009**
	(0.009)	(0.009)	(0.005)	(0.005)
$\text{Eligible}_2 \times$ year 2012	-0.000	0.002	-0.007	-0.007
	(0.009)	(0.009)	(0.005)	(0.005)
$\text{Eligible}_2 \times$ year 2013	0.018**	0.017*	-0.019***	-0.019***
	(0.009)	(0.009)	(0.005)	(0.005)
$\text{Eligible}_2 \times$ year 2014	0.013	0.010	-0.016***	-0.016***
	(0.009)	(0.009)	(0.005)	(0.005)
$\text{Eligible}_2 \times$ year 2015	0.017*	0.015*	-0.023***	-0.023***
	(0.009)	(0.009)	(0.005)	(0.005)
Year 2008	0.003*	0.004	-0.017***	-0.020***
	(0.002)	(0.003)	(0.004)	(0.004)
Year 2009	0.003*	0.001	-0.001	-0.003
	(0.002)	(0.003)	(0.004)	(0.004)
Year 2010	-0.001	-0.002	0.000	0.001
	(0.002)	(0.003)	(0.004)	(0.004)
Year 2012	-0.001	-0.004	0.009**	0.009**
	(0.002)	(0.003)	(0.004)	(0.004)
Year 2013	-0.006***	-0.005*	0.031***	0.037***
	(0.002)	(0.003)	(0.004)	(0.004)
Year 2014	-0.006***	-0.008***	0.050***	0.052***
	(0.002)	(0.003)	(0.004)	(0.004)
Year 2015	-0.010***	-0.012***	0.059***	0.062***
	(0.002)	(0.003)	(0.004)	(0.004)
Control variables	No	Yes	No	Yes
Observations	1,347,837	1,347,837	1,347,837	1,347,837

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 financing institution, rural area and geographical region. Student level control variables include gender, attendance rate and number of family members at different leves in the education system.

Table 15: Dynamics of Second Year Dropout

	University		Vocational	
	(1)	(2)	(3)	(4)
Eligible_2	-0.207***	-0.136***	-0.188***	-0.159***
	(0.011)	(0.011)	(0.007)	(0.007)
$\text{Eligible}_2 \times$ year 2008	-0.043***	-0.063***	0.011	0.005
	(0.016)	(0.016)	(0.011)	(0.010)
$\text{Eligible}_2 \times$ year 2009	-0.017	-0.027*	0.018*	0.011
	(0.016)	(0.016)	(0.011)	(0.010)
$\text{Eligible}_2 \times$ year 2010	-0.014	-0.017	0.020**	0.019*
	(0.016)	(0.016)	(0.010)	(0.010)
$\text{Eligible}_2 \times$ year 2012	-0.010	-0.031**	0.009	0.006
	(0.016)	(0.015)	(0.010)	(0.009)
$\text{Eligible}_2 \times$ year 2013	-0.070***	-0.080***	0.016*	0.012
	(0.016)	(0.016)	(0.010)	(0.009)
$\text{Eligible}_2 \times$ year 2014	-0.036**	-0.041**	0.012	0.004
	(0.016)	(0.016)	(0.010)	(0.009)
$\text{Eligible}_2 \times$ year 2015	-0.039**	-0.026	0.026***	0.019**
	(0.018)	(0.018)	(0.009)	(0.009)
Year 2008	0.059***	0.078***	0.034***	0.028***
	(0.016)	(0.016)	(0.011)	(0.010)
Year 2009	0.041**	0.057***	-0.018	-0.015
	(0.016)	(0.016)	(0.011)	(0.010)
Year 2010	0.017	0.023	-0.020*	-0.024**
	(0.017)	(0.016)	(0.010)	(0.010)
Year 2012	0.052***	0.082***	0.008	0.018*
	(0.016)	(0.015)	(0.010)	(0.010)
Year 2013	0.108***	0.111***	-0.015	-0.012
	(0.016)	(0.016)	(0.010)	(0.010)
Year 2014	0.078***	0.089***	-0.025**	0.002
	(0.017)	(0.016)	(0.010)	(0.009)
Year 2015	0.074***	0.064***	-0.034***	-0.007
	(0.018)	(0.018)	(0.010)	(0.009)
Control variables	No	Yes	No	Yes
Observations	386,329	375,297	273,715	272,737

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 financing institution, rural area and geographical region. Student level control variables include gender, attendance rate and number of family members at different leves in the education system.

Appendix C

To provide further evidence in favor of the Parallel Trends assumption, Figure 3 shows the time evolution of our three outcomes for both eligible and non-eligible individuals. In turn, each outcome is depicted throughout its three variables: general, university, and vocational institutions. Panel A presents trends for Immediate enrollment, panel B for two-year enrollment and panel C for second-year dropout, respectively. From this visual inspection we can argue that all nine variables have evolved in a parallel way before the 2012 changes between eligible and ineligible individuals, providing evidence in favor of our identification assumption.

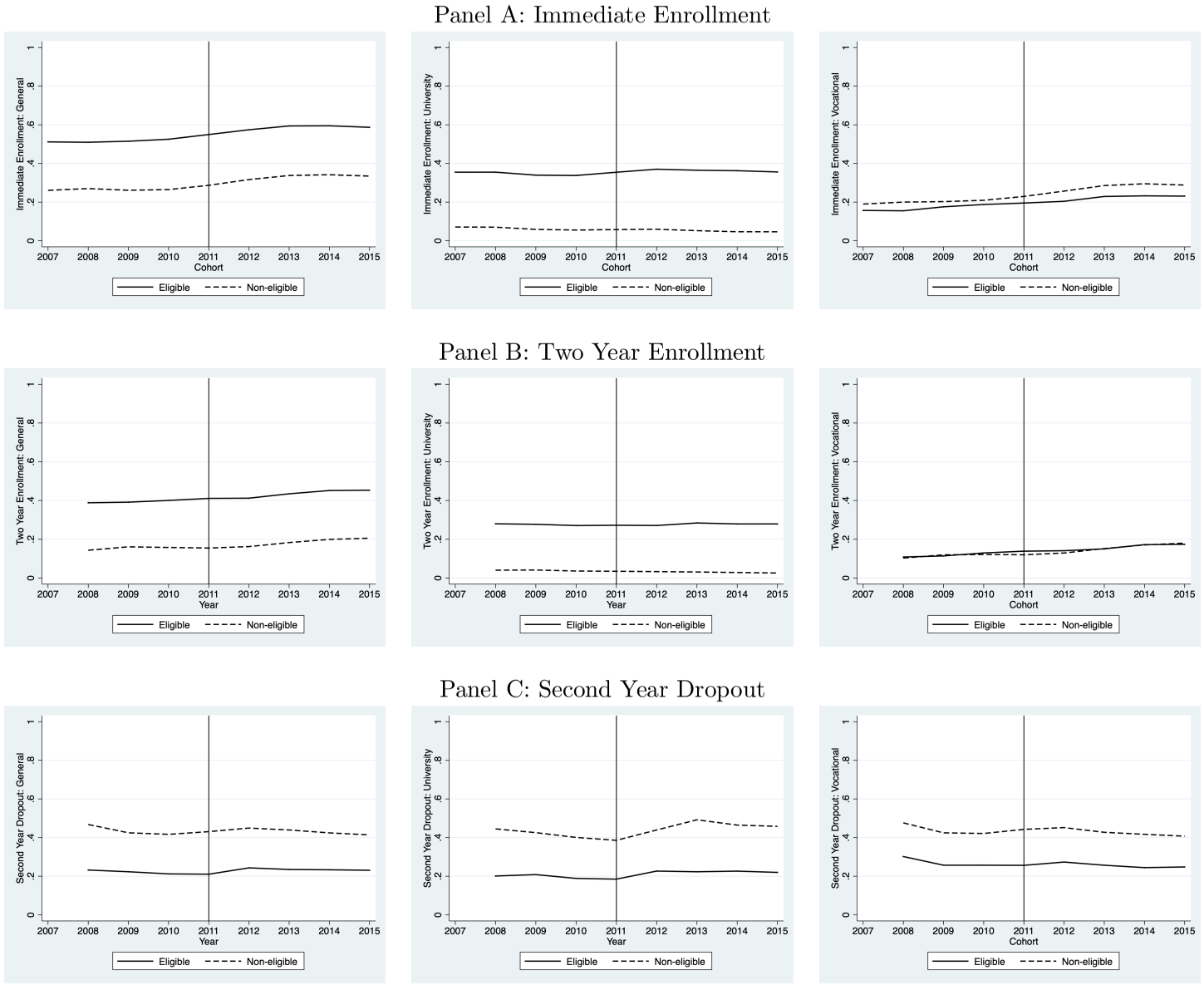

Figure 3: Outcomes over time by eligibility

Appendix D

To further elaborate the argument that women perform worse than men in PSU scores, in Table 16 we estimate the yearly differences of the proportions of male and female students scoring under 475 in the PSU. The proportion of women not meeteing the PSU cutoff for CAE eligibility is systematically higher (between 4 and 7 pp.) than the corresponding proportion of men. This evidence is consistent with our explanation for the negative effect of the reform on female overall immediate enrollment.

Table 16: Gender Difference in Proportion of PSU Scores under 475

	2007	2008	2009	2010	2011	2012	2013	2014	2015
Difference of proportions	-0.07***	-0.06***	-0.06***	-0.06***	-0.05***	-0.04***	-0.06***	-0.06***	-0.05***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)

Notes: Difference of proportions of students scoring less than 475 in PSU between male and female students and the corresponding standard errors (in parentheses). *** p<0.01, ** p<0.05, * p<0.1.

Appendix E

Table 17 mimics the exercise in Table 16. The proportion of public school students scoring under 475 in the PSU is persistently higher than the corresponding proportion in voucher schools (between 14 and 17 p.p.).

Table 17: Voucher-Public School Difference in Proportion of PSU Scores under 475

	2007	2008	2009	2010	2011	2012	2013	2014	2015
Difference of proportions	-0.14***	-0.14***	-0.14***	-0.15***	-0.14***	-0.17***	-0.16***	-0.14***	-0.16***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)

Notes: Difference of proportions of students scoring less than 475 in PSU between students of Voucher and Public schools and the corresponding standard errors (in parentheses). *** p<0.01, ** p<0.05, * p<0.1.