Credit Access and College Enrollment

Alex Solis*

December 5, 2011

Abstract

Does limited access to credit explain some of the gap in schooling attainment between children from richer and poorer families? I present new evidence on this important question using data from two loan programs for college students in Chile. Both programs offer loans to students who score above a threshold on the national college admission test, providing the basis for a regression discontinuity evaluation design. I find that students from relatively poor families who score just above the cutoff have nearly 20 percentage points higher enrollment than students who score just below the cutoff. Access to the loan program effectively eliminates the family-income gradient in enrollment among students with similar test scores. Moreover, access to loans also leads to higher enrollment in the second and third years of college. These findings suggest that differential access to credit is an important factor behind the intergenerational transmission of income in Chile.

1 Introduction

Students from richer families are more likely to attend college than students from poor families. Whether the gap is due entirely to differences in tastes and abilities, or is partially driven by credit constraints faced by lower income families, is a matter of much debate. Some analysts argue that the gap is mainly a reflection of long-run differences in educational investment, both at home and in schools, that affect the readiness for college (e.g., Cameron and Heckman (1998), Keane and Wolpin (2001), and Carneiro and Heckman (2002)). Others have argued that liquidity constraints prevent some relatively able poor students from enrolling in college(e.g., Kane (1994, 1996), Belley and Lochner (2007), and Lochner and Monge-Naranjo (2011a)).

*PhD Candidate, Department of Agricultural and Resource Economics, University of California, Berkeley. Email: alexsolis@berkeley.edu.
I would like to thank Elisabeth Sadoulet, Frederico Finan, David Card, Alain De Janvry, Jeremy Magruder, Edward Miguel, Sofia Villas-Boas, Ethan Ligon, Emmanuel Saez, Peter Berck, Patrick Kline, Gianmarco Leon, Mauricio Larrain, Owen Ozier, and participants of UC Berkeley ARE Development Workshop, Development Lunch, Development Seminar, ARE Seminar, 2011 PacDev Conference, 2011 MOOD workshop, II workshop on Economics of Education and World Bank social mobility workshop for all their valuable help and comments. I would like to thank Gonzalo Sanhueza and Humberto Vergara for providing the data and especially Francisco Meneses for helping me with the data and for his comments. I gratefully acknowledge financial support from the Confederacion Andina de Fomento CAF and from the Center for Equitable Growth at the University of California, Berkeley. A previous version of this paper circulated under the title "Credit Constraints for Higher Education." All errors are my own.

Measuring the effects of credit constraints on college enrollment is a difficult task because determining whether a family actually has (or does not have) access to credit is difficult or impossible. Even if access to credit were directly observed, there are many other unobserved variables that are likely to be correlated with access, and that affect college enrollment. For example, students from high income families may have better access to credit markets, but also may have stronger preferences for college education, better academic preparation, and superior cognitive and non-cognitive skills. Recognizing the problem, tests of the credit constraint hypothesis have mainly relied on indirect measures of credit access (e.g., Cameron and Taber (2004), and Stinebrickner and Stinebrickner (2008)) that lead to mixed – and sometimes inconsistent – findings.

In this paper, I exploit the sharp eligibility rules of a pair of student loan programs recently introduced in Chile that give access to college tuition loans for students who score above a certain threshold on the national college admission test. Around the eligibility cutoff these programs provide college tuition loans which are as good as randomly assigned (Lee, 2008), enabling a regression discontinuity design that addresses the problems of unobserved omitted variables and self-selection, allowing for an unbiased estimate of the causal effect of credit access on college enrollment.

A key feature of my analysis is the availability of a detailed student-level data set that presents several advantages over the samples used in earlier studies. First, I observe the *entire population* of individuals who participate in the national college admission process, including full information on their enrollment (institutions, programs, rankings, etc.) for three consecutive years, loan eligibility, loan take-up, scholarships, objectively-measured family income, and socio-economic characteristics. Second, I observe the two variables that *completely determine* college admission: the score on the national college admission test and high school GPA, ruling out potential biases from admission processes that weight subjective characteristics (e.g., letters of recommendation and parental alumni status). I also observe complete information on the financial programs offered in Chile (including program rules and program recipiency). Finally, the two loan programs of interest offer standardized loans to eligible students, eliminating potential endogeneity of loan offers designed to attract better students.

The nature of the loan programs and the availability of these data allow a highly credible evaluation of the causal effects of credit access on college enrollment and college persistence.

My analysis shows that access to the loan program increases the college enrollment probability by 18 percentage points – equivalent to a nearly 100% increase in the enrollment rate of the group with test scores just below the eligibility cutoff. Students from the lowest family income quintile benefit the most: for these students access to the loans causes a 140% increase in the probability of enrollment (on a baseline enrollment rate of 15% for students just below the test score cutoff). Importantly, access to the loan program appears to effectively eliminate the relatively large income gradient in enrollment for students with scores just below the eligibility threshold. Among students who are barely *ineligible* for loans the relative enrollment rate for students from families with incomes in the first and fifth quintiles is 2:1. Among students who are barely *eligible*, the gap is statistically zero.

Access to loans not only increases enrollment, but also improves progress in college. Eligible students are 20 percentage points more likely to enroll for a second year and 21 percentage points more likely to enroll for a third year of college. Those numbers are equivalent to a 213% and 445% increase, respectively, when compared with the enrollment probability for the groups without access. Students with access to loans are 6 percentage points less likely to drop out after their first year and 11 percentage points less likely to drop out after their second year, which are equivalent to a 31% and 38% decrease, respectively.

One concern with the interpretation of these results might be that they are due to the presence of lower than market interest rates and low enforceability (a price effect). I address this concern using the differences in interest rates and enforceability of the two loan programs. I decompose the enrollment effect into a price effect and an access effect. I find that the price effect is close to zero and conclude that the overall effect is driven by the credit constraints.

To deal with the local validity of the regression discontinuity design, I identify all 3,608 sets of twins and triplets in the sample and run regressions with family fixed effects as a second identification strategy to control for unobserved family characteristics. Thus, I can estimate the effects of loan access on college enrollment in other parts of the college admission test distribution, and my results are roughly of the same magnitude.

The paper is organized as follows. Section 2 reviews previous literature and, discusses the context and the natural experiment. Section 3 describes the data. Section 4 presents the results, and section 5 concludes.

2 Background

Measuring the effects of credit constraints on college enrollment is a difficult because credit access is unobserved and enrollment is also affected by unobserved factors that are correlated with credit constraints. In this section, I review briefly how the literature has addressed these problems.

2.1 Previous Literature

Enrollment

The question of whether access to credit markets affects human capital investments has a long history (see Becker (1967)).

The literature on credit constraints for higher education is a subset of the work on the relationship between student aid and college enrollment.[1][2] Due to the problem of unobserved variables, the

[1] See Ehrenberg (2004) for a survey of the literature of higher education.

[2] For theoretical approaches see Lochner and Monge-Naranjo (2011) and Brown et al (2011). The first paper derives a model with endogenous constraints using the design of college tuition loans and the repayment incentives, predicting cross-sectional evidence consistent with U.S. data. The second uses models transfer from parents to children to predict that students with relatively high ability, egoistic parents, or relatively poor parents will underinvest in education because of borrowing constraints.

methods are mixed and the conclusions – on the strength of the relationship and even on its existence – are controversial (Lochner and Monge-Naranjo (2011b) present a complete survey). Among the papers that find an effect, McPherson and Schapiro (1991) use tuition cost time series to explain variation in enrollment before and after the introduction of the Pell Grant program in 1974 in the US. They find that low income families are more likely to respond to these incentives. Manski and Wise (1983) estimate a 23% increase in the enrollment rate with the introduction of the Pell grant program as it existed in 1979-80. Dynarski (2003) finds that the elimination of the Social Security program, which gave an average annual payment of $6,700 to eligible students when tuition and fees averaged $1,900, led to a 33% reduction in the probability of enrollment (18.2 percentage points). Nielsen, Sorensen and Taber (2010) use the variation due to an aid reform in Denmark that increased stipends for students coming from richer families to measure the effects on enrollment in a tuition-free environment, and they use data on assets to measure potential biases from borrowing constraints, finding no effects. My paper contributes to this literature by showing consistent estimates that poor students are the ones who benefit most from loan programs and documenting a substantially larger increase in the enrollment probability (96%) as a consequence of the programs.

Few papers try to show evidence on the existence of credit constraints. Most use indirect methods and present results that are consistent (or not) with the hypothesis of credit market imperfections. Kane (1996) interprets the delayed college entry in high tuition states among blacks and poor whites as evidence of credit constraints. Carneiro and Heckman (2002) show that the enrollment gap by family income becomes much smaller once family background and a measure of student ability are included, concluding that family income (and therefore credit constraints) do not explain the gap.[3] Belley and Lochner (2007) compare NLSY79 with NLSY97, finding that the results from Carneiro and Heckman (2002) are inconsistent with the newer survey. Card (1999 and 2001) uses another indirect approach consistent with the hypothesis of credit constraints. He compares OLS and instrumental variables (IV) estimations of Mincer returns to education. Because of ability bias, OLS estimation is expected to be upwardly biased; nevertheless he shows that different instruments give larger estimates than OLS for the returns to education. This anomaly may indicate that students affected by the instruments have a large rate of return (discount rate bias according to Lang (1993)), which is consistent with credit constraints. Cameron and Taber (2004) use direct cost and foregone wages as instruments for education, reasoning that direct costs affect the credit constrained population differently while foregone earnings do not. They find that both instruments give similar estimates and conclude that credit constraints are not important. Carneiro and Heckman (2002) discuss the validity of the instruments and present a model to explain the IV-OLS puzzle when credit constraints are not important. [4]

My paper contributes to this strand of the literature by using random variation in credit access

[3] Using the National Longitudinal Survey of Youth of 1979 (NLSY79) in the U.S.

[4] A different approach if given by Attanasio and Kaufmann (2009) and Kaufmann (2010), they use differences in the expected returns and information sets between students from high and low income families to explain the college enrollment differences in Mexico, concluding that the sensitivity of low income students to change in direct costs suggests the presence of credit constraints.

directly: eligible students have access to loans (not grants). Moreover, by using RD I am able to neutralize the effect of family background and ability. I am also able to use the variation in the costs of the two programs to show that the enrollment effect is mainly driven by access to credit markets rather than the effects of below market interest rates or low enforceability. My results show that students with access to loans increase their college enrollment significantly.

A separate strand of the literature estimates structural models. The researcher calibrates a model of choice using observational data and then simulates the decisions made by students, changing parameters such as tuition cost, parental bequest, etc. Prominent examples of this are Keane and Wolpin (2001), Cameron and Heckman (2001) and Cameron and Taber (2004). Keane and Wolpin (2001) use NLSY79 to conduct counterfactual experiments to assess the effects of the credit constraints and parental transfers on access to higher education. Cameron and Heckman (2001) estimate a dynamic sequential model of schooling attainment using NLSY79 to improve measures of parental background, which they then use to simulate policies that reduce credit constraints. Cameron and Taber (2004) estimate a structural model using the instruments mentioned above as exclusion restrictions for the estimation of the structural parameters. These three papers conclude that credit constraints have no effect on college attendance.

More direct approaches use regression discontinuity designs. Van der Klaauw (2002) addresses the question of how important grant offers are in attracting students to an East Coast college. However, he argues that colleges' grants are increasingly based on academic merit and are used to encourage the best admitted students to enroll in a given college, rather than being a tool to make college more accessible to students from low income families. In addition, the admission process relies on unobserved and subjective measures, such as recommendation letters, statements of purpose, and extracurricular activities, which appear as another source of endogeneity. He also highlights one of the problems faced by studies that use information from only one institution: there is missing information about other colleges' offers, outside opportunities, and whether a student decides to enroll in another institution or not to enroll at all. Gurgand, Lorenceau and Melonio (2011) analyze the effects of loan access on college enrollment using a RD with data from a private program in South Africa that gives loans depending on parent's credit score. In terms of the methodology, this paper is the closest to mine, neverthelees, they only observe public colleges, so they assumed that students not enrolled in this type of colleges did not enrolled at all.

I address all of these concerns since first, this paper uses variation in credit availability rather than financial aid generally; second, I use a nationwide college admission process with full information, i.e. I observe all of students' educational institutions' decisions; third, the admission process relies exclusively on observed characteristics; and last, loans are offered to all eligible students from the four lowest income quintiles who score more than the cutoff and as such are not used as a mechanism to attract better students to a given college.

Progress in College and Dropout Rate

Much less work has been done about the effects of financial aid or credit constraints on college progress and dropout rates (see Chen (2008) and Hossler et al (2009) for excellent surveys). Dynarski (2003) uses the elimination of a social benefit program to show the effects of financial aid on completion of one year of college and completed years of college. She finds a positive but not significant effect for persistence and years completed.

Bettinger (2004) uses data for students from all public colleges in Ohio and exploits small discontinuities in the assignment rule of the Pell grant program to find that a $1,000 increase in the grant leads to a 3.7 percentage point reduction in the probability of dropout.

Singell (2004) uses administrative data from the University of Oregon to estimate the effect of aid on retention for freshman students. He controls for selection into college education by estimating retention and initial enrollment jointly and using a rich set of student's characteristics. He estimates that the most effective form of aid to retain students is scholarships, and only subsidized loans have an effect in retention.

DesJardins et al (2002) use the estimates from a hazard model to simulate the effects of aid on college dropout for students at the University of Minnesota. They find that not all forms of aid are effective: need based grants have no effects on retention, loans have a small effect, and scholarships are the most effective. They estimate that – relative to a situation in which no aid is provided – the survival rate after four years of college increases by 10 percent.

Lastly, Stinebricker and Stinebrickner (2008) estimate how important credit constraints are in explaining dropout decisions using a survey of students enrolled in a tuition free college (Berea College). They find that credit constraints explain only a small fraction of the observed dropout rate.

The difficulty of obtaining reliable data to determine the relationship between aid or access to credit markets on enrollment and persistence in college is evident from these last three papers. All of them rely on information from a single institution. My paper contributes to this strand of the literature because, to the best of my knowledge, this is the first paper that uses the universe of all available institutions and students attending college to account for these effects, thereby eliminating potential bias from students who are considered dropouts when they actually switch institutions, and it presents evidence that is not contingent on the characteristics of a single institution.

Additionally, I show that students who enroll without access to loans come from higher income families. This indicates that comparisons between groups with and without access to aid or loans are biased towards zero. This is especially important for papers that compare students with and without aid conditional on enrollment that do not address the problem of selection into college. I find that access to loans has a larger impact on progress in college and dropout rates than previous evidence.

2.2 The Natural Experiment

Two financing programs in Chile offer college tuition loans to students who: first, apply for benefits; second, belong to the lowest four income quintiles; and third, score at least 475 points on the national College Admission Test (Prueba de Selección Universitaria, PSU hereafter). This enables a sharp regression discontinuity design to study the effects of access to college tuition loans on college enrollment[5] [6]

Being barely above or below the cutoff is random in a small vicinity of the threshold: for each individual, a random term ξ is revealed the day of the test. Some students get a ξ that puts them just above the cutoff and some get a ξ that makes them score just below. Since the realization of ξ is random, unobserved (and observed) characteristics are balanced in a neighborhood of the threshold. As a consequence, students above the cutoff have access to college loans while students below do not. Comparing college enrollment rates for the group just above (the treatment group) and the group just below (the control group) gives the causal effect of credit access on college enrollment.

2.3 Universities and the Admission Test, PSU

There are two types of universities in Chile: traditional and private. Traditional universities are those that were founded before the reform of 1981. Some of them are public and others are privately-funded, but mainly they are not-for-profits. There are 25 of these universities and they serve roughly 50% of all students in college education.[7] The 1981 educational reform allowed the creation of new higher education institutions, which are known as "private universities". There are 33 universities of this type varying in quality and size. Their growth has been rapid and steady, increasing enrollment from a handful of students in 1991 to 274,000 in 2009.

The PSU test was created in 1966 by the traditional universities to have a centralized placement mechanism.[8] The PSU test consists of two mandatory tests on language and mathematics and two optional tests. The average on the mandatory tests is referred to as the *PSU score*, and is used for loan eligibility.[9]

The tests have only multiple choice questions which are answered on a special sheet that is graded automatically by a photo optical device (figure 1 shows an example of the answer sheet). PSU scores are normalized to a distribution with mean 500 and standard deviation of 110 to make them comparable between years. The scores range from 150 to 850 points.

The test is taken simultaneously across the country once a year and is used as a selection mechanism

[5] All the students that fulfill the first two conditions become eligible if they score more than 475 in the PSU, therefore the probability of receiving treatment jumps from 0 to 1 after 475.

[6] Dinkelman and Martinez (2011) perform an experiment randomly releasing information about the eligibility criterion of this loan programs to students in 8th grade finding that students reduce absenteeism.

[7] (54% in 2008 and 52% in 2009)

[8] The name was changed from PAA (Prueba de Aptitud Académica: academic aptitude test) to PSU in 2003.

[9] The optional tests: History and Social Sciences, and Sciences (which includes modules on biology, chemistry, and physics) are not considered for loan eligibility, but they are considered for the placement score depending on each college program.

for almost all higher education institutions in the country. Roughly 95% of all high school graduates take the PSU each year. Some students take it even when they do not plan to enroll in tertiary education, because sometimes it is required as a high school graduation certificate. There is a fee to take the test (about $50 or CLP24,000 in 2010), which is waived for all students graduating from public and voucher schools that apply for the waiver. The test can be taken as many times as wanted, but is only offered once a year.

2.4 The loan programs

The two loan programs that give tuition loans to eligible students are (i) the Traditional Loan Program and (ii) the State Guaranteed Loan Program. The same eligibility criteria, used by both programs, stipulate that students need to: apply for benefits, be classified in one of the poorest four income quintiles, and score more than 475 in the PSU test. The only difference in terms of eligibility is that the former is given to students enrolling in one of the 25 traditional universities, while the latter can be used at any of the 44 accredited universities.

Table 1 summarizes all financial aid given by the Chilean Ministry of Education. All aid is given to students based on their PSU scores (except for the Excellence Scholarship),[10] but only the two loan programs are given to students in the neighborhood of 475. To be eligible for any benefits given by the Ministry of Education, students apply using a unique application form (Formulario Único de Acreditación Socioeconómica, FUAS) before the PSU test. The family income information given to FUAS is contrasted with information from the Chilean internal revenue service (Servicio de Impuestos Internos, SII), which ranks students' family income to determine if they belong to the first four income quintiles, and thus determines eligibility.

2.4.1 The Traditional University Loan program

The Traditional University Loan Program (Fondo de Crédito Solidario) was introduced in 1981 as part of an educational reform, and only applies to students who enroll in a traditional university. These loans are managed by the universities, which decide the amount to lend to the student and are in charge of the collecting process. Repayment starts 2 years after the student's graduation and the installments are calculated each year, corresponding to 5% of the borrower's income. The interest rate on this loan is about 2% per year with a maximum of 15 years of payments, and after that the debt is written off. This loan can be complemented with the State Guaranteed Loan. The universities in the first stage, and a central organization (Fondo Solidario) in the second are in charge of collecting loan payments. Since neither the universities nor the central organization are specialists in collecting loans, this scheme has a low repayment rate (from 52 to 60% for the years considered). In recent years, the Chilean government has made some modifications that allow the SII to retain tax refunds and publicize names of defaulting students, which has increased the repayment rate to 80% (in some

[10] The Excellence Scholarship is given to students in the top 5% in the GPA distribution.

cases) of all reprogrammed loans.[11] Nevertheless, all of these characteristics taken together indicate that there is a subsidy component to this loan scheme.

2.4.2 State Guaranteed Loan program

The State Guaranteed Loan program (Crédito con Aval del Estado) allows private banks to provide loans to eligible students that are guaranteed by the state and by higher education institutions. To be eligible, students need to fulfill the three requirements mentioned above and enroll in one of the 44 accredited universities. The interest rate is about 6% per year, which corresponds to the government long-run interest rate,[12] and is slightly higher than a mortgage rate for the same period.

The higher education institution guarantees the loan in case of dropout: 90% of the capital plus interest for the first year, 70% for the second, and 60% for the third year onwards. The state guarantees up to 90% when the educational institution covers less than that percentage. In the event that a student stops paying, after the bank implements all mechanisms used to collect the loans, the guarantors (the state and/or the educational institution) must pay the bank and become responsible for enforcing the collection from the student.

Out of the 58 institutions that provide college education in Chile, 77.6% participate in the program. Of the remainder, 19% are not accredited institutions and 3.4% have dropped out of the program. Some institutions ask for higher PSU scores to guarantee the loan, but 85% of all programs require the standard 475 PSU score to be eligible.

The State Guaranteed Loan program requires students to start repayment 18 months after graduation in monthly installments for 20 years divided into three installment periods (low, medium and high). Private banks give the loans, and they are in charge of the repayment process. The loan contract establishes that employers are mandated to deduct repayments directly from the payroll and to pay directly to banks (the law also establishes penalties to employers who do not comply with this process). Additionally, the loan contract allows the SII to retain tax refunds in case the former student does not pay the lending bank. This last characteristic has proved to be an efficient measure, increasing the repayment rate since 2002. In addition to the previously mentioned measures, private banks can use all relevant legal mechanisms to recover the debt, which include information release to credit score institutions, asset impoundment, and judicial collection. Releasing information is important in the labor market in Chile, since usually firms request that potential employees not appear as defaulters in credit score records. The State Guaranteed Loan program asks for a similar interest rate as the loans in the market, but there is no need of a guarantor with income and assets, since that role is played by the state and the educational institutions while the individual is studying. This program was designed to give a market alternative to students that did not have access to traditional loans, especially those in private educational institutions and vocational schools. Since this loan is run by private banks with several proven mechanisms to collect debt and the interest rate is similar to other

[11] Source: Fondo Solidario de Crédito Universitario

[12] Source: International Comparative Higher Education and Finance Project. State University of New York at Buffalo.

market loans (see next section), it is expected that this loan will have similar repayment rates to an ordinary loan. Because of the similarities in interest rates, I will use these loans in section 4.5 as a market benchmark to disentangle the effects due to lower interest rates and the pure access to credit market effect.

2.5 Other Loans Available

In order to have a broad picture of the degree to which students are credit constrained, I briefly describe other sources of financing here. First, some colleges offer scholarships or loans to complement the loans described above, mainly to attract the best students, and therefore all these scholarships and loans require much higher PSU scores than 475.

There are two types of loans given by private banks: 1) the Corfo[13] loan ("crédito Corfo") and 2) private bank loans. The development office, Corfo, lends money for this purpose to private banks which manage the process. Both of these loans require a bank guarantor, who needs to certify a good credit record, be employed, have a regular income source, have assets to use as collateral, and have a minimum family income. Also both loan types offer a maximum repayment period different grace periods.

Corfo loans have interest rates that varied among banks, ranging from 6.8% to 8.5% (real annual), and minimum guarantor income of $1,225 or CLP600,000, corresponding to a family income in the bottom part of the fourth income quintile (see table 2).

For private bank loans, the most important is the one given by BancoEstado.[14] This loan is aimed at lower income families, starting with incomes close to the top of the second quintile. The minimum family income required to apply for this loan is $714 (CLP350,000). The real interest rate lies between 6.6% and 6.8% annually. All other loans from private banks have very similar requirements but ask for higher minimum family income, starting at $1,224 (CLP600,000).

Credit access and the cost of higher education in Chile are comparable to those in the U.S. In financial development, Chile ranks 30th among 52 countries (the first in Latin America) in the financial development index elaborated by the World Economic Forum in 2008, while the U.S. is first. Nevertheless, Chile ranks 11th in the category of "ease to access to credit" and 22nd in "ease to access to loans," while the U.S. is 21st and 7th respectively. Another important difference may be income per capita: Chile has an income per capita of $15,040,[15] less than the US but similar to other developed countries like Portugal ($23,200) and higher than some eastern countries of the European Union.[16] However, in terms of average college cost relative to GDP per capita, Chile ranked 32nd and the U.S. 27th among 45 countries.[17] An average Chilean college student pays 125% of GDP per capita for

[13] Corfo: Corporación de Fomento a la Producción

[14] A private bank with partial ownership by the government of Chile

[15] PPP estimated for 2010 Source: IMF.

[16] Bulgaria, Latvia, and Romania.

[17] Relative cost to GDP per capita = College cost / GDP per capita. Where college cost is the sum of average tuition and average living expenses. Source: International Comparative Higher Education and Finance Project from State University of New York at Buffalo. GDP per capita source: IMF).

tuition, fees and living expenses, while an average American student pays 75% of GDP per capita.

3 Data

The data come from four data sets from three different institutions. The first data set contains individual PSU scores and socioeconomic characteristics that are self-reported by the students when they register for the test, such as family income, parent education, household size, city of residence, etc. It also includes high school GPA, school of graduation, and other school characteristics. This data set comes from the Council of Chancellors of Chilean Universities (Consejo de Rectores de las Universidades Chilenas: CRUCH), which is the organization that implements the PSU process. It includes PSU processes for y years, 2006 to 2009.

The second data set includes information at the individual level on enrollment. It comes from the Ministry of Education and includes full enrollment data for all programs and institutions for the period from 2006 through 2009.

The third data set corresponds to the FUAS application form, which contains individual level information on applications for benefits given by the Ministry of Education of Chile, eligibility for benefits, income quintile reported by the IRS, and assignment to eight scholarship programs and the traditional loan program. This information has been collected by the Ministry since 2006, but I only have data from 2007 onwards.

The last data set corresponds to individual data on State Guaranteed Loans from the INGRESA commission from 2006 to 2009. This commission was created in 2006 to manage this credit system.

The data allow me to address several problems found in the literature. First, the information comes from a centralized national admission process, and therefore contains enrollment status, program, and higher education institutions for all students attending college in the country. It also provides an objective measure of family income, provided by the tax authority for the group of students that applies for benefits.

Second, for admission to traditional universities, the centralized process uses only PSU score and high school GPA to determine the rank of students applying to a given program. Whereas private universities are not mandated to use these criteria to determine placement, they do use them to ascertain the quality of the students, and they rely on the PSU score when the demand for a given program is larger than the number of seats. Importantly, the admission process does not weight subjective variables such as letters of recommendation or a statement of purpose, which could complicate empirical analysis by adding another source of unobserved variation.

Lastly, loan amounts are determined exclusively by family income for the traditional loan and defined by the student in the State Guaranteed Loan programs. In neither case is the loan used as an instrument to encourage students to enroll in a given institution. In the US, in contrast, loans and financial aid in general serve to attract better students (see Van der Klaauw, 2002), introducing another source of endogeneity since the actual rule to determine financial aid is not observed by the econometrician and may be correlated with unobserved variation.

Loan eligibility is pre-determined for the Ministry of Education before the PSU test results are known.[18] Students fill out the FUAS form with socioeconomic characteristics, which is sent to the Chilean IRS to determine eligibility. Of all students who took the PSU test, 56, 60, and 62% applied for benefits in the years 2007, 2008, and 2009 respectively.

Table 3 shows the total number of students taking the test each year, roughly 220,000. The large sample size allows me to compare a very narrow window around the cutoff. The following section will show that the estimation for a window of only two points around the cutoff has enough power while still being conservative. Eligible and non-eligible students are still comparable in observables in a window of four points around the cutoff.

The PSU test can be taken as many times as a student wants. Therefore, students may try repeatedly until they get a score above 475, self-selecting to be eligible for loans. To avoid the effects of "learning how to take the test" and self-selection into treatment, in all the following sections, only students taking the test for the first time are considered.

Restricting the sample to first-time takers and preselected students, i.e. those who applied for benefits, were classified in one of the four poorest income quintiles, and score 44 point around the threshold, gives a sample size of 77,646 students, which corresponds to 12% of the universe or .43 of the standard deviation from the score distribution., as shown in table 3.

4 Results

This section is organized as follows. Section 4.1 tests the conditions for a valid RD: random loan assignment around the cutoff, absence of manipulation of PSU scores, and balanced characteristics between the eligible and non-eligible students to test the local continuity assumption. Section 4.2 shows results for the estimation of the causal effect of loan access on college enrollment. Section 4.3 presents results by income groups and revisits the college enrollment gap. The decomposition of the total effect between price and pure access effects is presented in section 4.5. In section 4.4 the effects on progress in college and dropout rates are presented, and section 4.6 addresses some potential problems with the identification.

All of the RD results shown in the following sections focus on the optimal bandwidth, which correspond to 44 PSU points around the threshold. The following analysis also focuses on all students that applied for benefits, were classified in one of the four poorest income quintiles, and took the PSU test for the first time. To show that these results are not sensitive to bandwidth or functional form, a graph with fitted fourth order splines and 95% confidence intervals will be given for all students scoring from 450 to 500 points. Each dot in the graph represents the average outcome for students in

[18] The Education Ministry is the main source of aid, scholarships, and tuition loans in the country. The programs given by the ministry are in part explained in table 1. Only recently, universities have started individual scholarship and loans programs to complement those given by the ministry.

bins of 2 points. For instance, the first dot to the right of 475 indicates the number of students that scored on the interval [475, 477).

4.1 Conditions for a valid RD design

The RD conditions for a causal estimation are reviewed in this section (see Hahn et al (2001), Lee and Lemieux, (2010), and Van der Klaauw (2008)).

4.1.1 Loan Eligibility

The assignment rule was fulfilled for all years except 2006, the first year of implementation,[19] hence, I will only use the PSU processes for 2007 to 2009. Figure 2 shows the probability of being eligible for a loan among all students in the data set and 95% confidence intervals, with respect to the PSU score for all years separately and pooled together. On average 57% of all students crossing the cutoff fulfilled all of the conditions. Among students that became eligible, figure 3 shows that half of them took up a loan. From these figures the sharp nature of the treatment is evident: no student below the threshold was eligible to receive these loans and none received for the years from 2007 to 2009.

4.1.2 Local Continuity Assumption: Manipulation of the Assignment variable.

The local continuity assumption for the outcome expectation requires the assignment variable not to be manipulated. As explained in section 2.2, the PSU test contains only multiple choice questions that are graded by an optical device, which implies that manipulation would be infeasible (see figure 1 for an example of an answer sheet). To verify this, figure 4 shows the frequency distribution of PSU scores plus a predicted value from a regression using a fourth order spline and 95% confidence intervals. The intersection of the confidence intervals shows that the number of students scoring above and below 475 is not statistically different, which confirms that PSU scores are not subject to manipulation around the cutoff.

4.1.3 Local Continuity Assumption: Balance of Covariates.

As a second check for the local continuity assumption I need to show that there is no other variable that is causing a discontinuity in the outcome around the cutoff. As mentioned in section 2.2, no other aid or loan program influences the financial conditions for students in the vicinity of 475, which is shown in table 1. Here I check the influence of other variables to verify that the loan assignment is as good as random in the neighborhood around the threshold. Following Imbens and Lemieux (2008):

[19] Anecdotally, in 2006 the Chilean IRS gave the information on income by ranking students from 1 to N. This information was misinterpreted by the commission managing the State Guaranteed Loan program, who assigned loans beginning with the richest student. When they figured out the mistake, loans were already announced and they had to assign a new number of loans to the poorest. Additionally, some loans were given to students below the cutoff. In all other years, the assignment rule was fulfilled perfectly: no student scoring below 475 received a tuition loan.

$$Y_i = \beta_0 + \beta_1 \mathbf{1}(T_i \geqslant \tau) + \beta_2(T_i - \tau) + \beta_3(T_i - \tau) \cdot \mathbf{1}(T_i \geqslant \tau) + \epsilon_i \tag{1}$$

Where $\mathbf{1}(T_i \geqslant \tau)$ is an indicator function for whether the student i's PSU score T_i is larger than the cutoff τ. $(T_i - \tau)$ account for the influence of the running variable on Y_i, $(T_i - \tau) \cdot \mathbf{1}(T_i \geqslant \tau)$ allows this influence to vary differently above and below τ, and ϵ_i a mean zero error. The parameter β_1 captures the increase in the expected value of Y_i arriving to the cutoff from above.

Table 4 shows the estimation of β_1 for the optimal bandwidth, for selected covariates, and t-values in parentheses. Panel A shows that all the covariates are balanced when the sample is pooled together. The first two columns show the results for a bandwidth of 2 PSU score. Column 3 and 4 for a bandwidth of 4 points and the last two columns the estimation using the optimal bandwidth (w^*) using Imbens and Kalyanarama (2011). Panel B shows the estimation for the three years separately using $w^* = 44$. In 2007, high school GPA is higher for the group below the cutoff, but the difference is very small.[20] In 2008, there are differences in high school type and household size. In 2009, there are slight differences in self-reported income category, being lower for the group below the cutoff, and high school GPA, now being larger for students above.

All the conditions for a natural experiment are satisfied and therefore I can apply a regression discontinuity design. The following section shows the results for the causal effects.

4.2 Effect on College enrollment

The main result is shown in figure 5, which shows the effect of crossing the cutoff on college enrollment for all the sample. Each dot represents the average enrollment for the students that belong to a bin of 2 points. Fitted values from a 4th order spline and 95% confidence intervals for each side of the cutoff are shown for scores between 450 and 500. The first three figures represent years 2007 through 2009 separately and the fourth figure all three years pooled together.

In all years, students who score more than 475, and therefore become eligible for a loan, increase their enrollment rate significantly. Access to loans causes an increase in the enrollment rate from 18 to 38 percent in 2007, from 20 to 39 percent in 2008 and from 19 to 39 percent in 2009. For all years pooled together, the average enrollment rate jumps from 18 to 37 percent.

To have a better look of what is happening at the cutoff I present in figure 6 a the same graph for a narrow window of 25 points around the cutoff. The effects on access to loans are about of the same size of the enrollment rate for the control group.

Formally, to determine the effects of access to credit on college enrollment, I run the following regression around the cutoff, suggested by Imbens and Lemieux (2008):

$$P(Enroll_i = 1) = \beta_0 + \beta_1 \mathbf{1}(T_i \geqslant \tau) + \beta_2 f(T_i - \tau) + \beta_3 f(T_i - \tau) \cdot \mathbf{1}(T_i \geqslant \tau) + \epsilon_i \tag{2}$$

[20] GPA is in the same unit of measure of the PSU test, i.e. from 160 to 850 points

The variables are defined as in equation (1), and $P(Enroll_i = 1)$ takes on the value one if students i enrolled in college. The parameter of interest β_1, is highly significant in all years as shown below.

Table 5 shows the estimation. Column (1) shows the estimation for all years together, while columns (2), (3), and (4) show regression results for 2007, 2008, and 2009 respectively. Having access to college tuition loans increases the probability of attending college by 19, 16, and 18 percentage points for 2007, 2008, and 2009 respectively and 18 percentage points for the full sample.

To assess the importance of these effects, the row labeled "Control Mean Enrollment" shows the relative increase in the enrollment probability, i.e. the increase in enrollment as a fraction of the enrollment for the control group, measured at the cutoff (β_1/β_0). Thus, access to loans implies a 96% increase in the probability of college enrollment for all years together (see row labeled "Increase w/r to Control Enr."). In other words, for each student enrolling without access to loans, 2 students enroll when they have access to these programs.

To give a sense of how sensible these results are to the chosen bandwidth, I present the magnitude of the enrollment discontinuity shown in table 5 for different bandwidths from 2 to 80 PSU points in figure 7. The graph on the left shows the linear specification of equation (2) with a vertical line for the optimal bandwidth ($w = 44$). In the graph on the right we see a fourth order spline. We can observe that the results are not sensitive to bandwidth and are almost the same for the two specifications, estimating an effect of 18 percentage points.

The graphs on the bottom present the relative increase in the enrollment probability, i.e. the ratio β_1/β_0, with a 95% confidence interval, where standard errors are calculated using the delta method. The relative increase is very close to 100%. Again the relative benefit is not sensible to the chosen bandwidth of functional specification.

This result addresses the bias from loan access that is correlated to family income. Control for the bias from unobserved variables correlated with enrollment and access to loans, such as preferences, expectations, etc., but more importantly ability. It uses a placement process that relies exclusively on the running variable and the observed high school GPA eliminating the potential biases from endogenous financial aid offers. Includes all the students that are participating in the college placement process nation-wide, and all higher education institutions, therefore avoids the bias from not observed enrollment status of some students. The result is free form the influence of other financial programs given in the country. The set of information allows restricting the analysis to new applicants to avoid the effects of self-selection into receiving aid and the effects of learning of the process to get aid. It is very robust to functional form specification and bandwidth.

To the best of my knowledge, this is the first paper that estimates the effect of loan access on college enrollment with all these characteristics. The result shows that loan access have a causal effect on boosting college enrollment and the effects are larger than any other previous piece of evidence.

Given these results, I will show now how the enrollment gap is affected by the inclusion of these two financial programs.

4.3 Enrollment Gap By Family Income

This section explores the college enrollment gap by family income for the students in this quasi experiment. The question is whether access to tuition loans helps to reduce the existing gap or not.

I estimate the effect by income quintiles with the following equation:

$$\begin{aligned} P(Enroll_i = 1) = \quad & \textstyle\sum_{q=1}^{5}[\phi_q Q_i^q + \beta_1^q \cdot Q_i^q \cdot \mathbf{1}(T_i \geqslant \tau) + \\ & + \gamma_1^q \cdot Q_i^q \cdot (T_i - \tau) + \gamma_1^q \cdot Q_i^q \cdot \mathbf{1}(T_i \geqslant \tau) \cdot (T_i - \tau)] + \epsilon_i \end{aligned} \tag{3}$$

Here all variables are the same as in equation (2), but Q_i^q corresponds to dummies that take on the value 1 if student i belongs to income quintile q. Equation (3) is therefore equivalent to the main regression (equation 2) comparing individuals with and without access to loans within income quintiles.

Table 6 shows the estimation results. The table is organized as before with the first column presenting results for the pooled sample and the following columns for each year separately. Focusing on all years together we observe that the effect is stronger for the poorest quintile. The access to these loans caused an increase in the enrollment probability of 20 percentage points for the first quintile, while the enrollment for students without access to loan at the cutoff in this quintile is 14.6%. This implies that having access to tuition loans led to a 137% increase in the enrollment rate.

The effects are slightly smaller but not significantly different for quintiles 2 and 3 (17 and 16 percentage points in each case), which compared with the mean enrollment in the control group (20% and 22% respectively) implies a relative 85% and 73% increase in the probability of enrollment.

For the fourth quintile, the effect is lower but significantly different from zero, (7 percentage points). Nevertheless, the effects are weak for the estimation in some years, columns (2) to (4): the effect is not statistically different than zero in 2008 and only significant at 5% for 2007.

Figures 8 and 9 show these results in perspective, to see what happened with the college enrollment gap by family income. Figure 8 shows the effects for each year separately and figure 9 for all years pooled together. The graphs on the left reproduce the results of table 6, showing the jump in enrollment at the discontinuity by quintile by year, plus 95% confidence intervals, while the graphs on the right show the enrollment rate separated for treated and control groups, plus 95% confidence intervals.

We observe that without access to loans the enrollment gap in very similar to the enrollment gap found in the whole population, 15 percentage points, students from higher income families enroll more than twice than students from families in the pooorest quintile.[21] The graphs on the right in both figures show that the enrollment rate increases with family income for the group without access to tuition loans. The enrollment rate for the poorest quintile is in the 10 to 16 percent range, while the highest income quintile has an enrollment rate in the range of 20 to 41 percent. More precisely, focusing on the effects for the three year pooled together, the right graph of figure 9 shows that the

[21] Source: own calculation using the Chilean household survey: CASEN 2006.

enrollment rate is 13 percent for the poorest and 31 percent for the richest, and the difference is statistically significant.

On the other hand, the enrollment rate by income quintile is the same for the group with access to loans. The enrollment rate is roughly 35%, and the difference between income quintiles is not significantly different from zero.

This implies that conditional on being around the cutoff (i.e. graduating from high school, taking the PSU test, and scoring around 475), inclusion in these programs eliminates the college enrollment gap by family income. The unconditional college enrollment gap by family income is still significantly different than zero.

4.4 Enrollment and Dropout Rates in the Following Years

One concern from the policy maker's perspective is that access to loans has an effect only on initial enrollment, but not on the graduation rate. If loans are given to students without the proper preparation for college education, graduation rates on both sides of the cutoff would be similar with no positive effect on degree attainment.

Enrollment data for 2008 and 2009 also contain information about the college enrollment status of students enrolled initially in 2007 and 2008. I use this data to measure the effect of credit access on medium run enrollment (enrollment in the second and third year of college) and on dropout rates.

Since students can retake the PSU exam every year after high school graduation, the sample of first time takers that scored less than 475 in their first attempt may become eligible for college tuition loans in later years if they score more than the cutoff. A student is eligible for college tuition loans for all years after she scores more than 475 in one attempt. This self-selection into treatment prevents the estimation of the causal effect using a sharp RD. Nevertheless, not all students below the cutoff retake the test, and only a portion of them succeed in scoring more than 475, therefore the probability of being eligible for college loans do not go from 0 to 1, but still jumps discontinuously at the cutoff. Thus, we can estimate the local average treatment effect (LATE) using a fuzzy RD design, where eligibility is instrumented by being eligible in the first year, i.e. a dummy for scoring more than 475 in the first year.

As before, all regressions in this section use the sample of first-time takers that were preselected for loans and score 44 points around the cutoff. Specifically the regressions are the following:

$$P(Y_i = 1) = \beta_0 + \beta_1 \hat{Elig}_i + \beta_2(T_i - \tau) + \beta_3(T_i - \tau) \cdot \mathbf{1}(T_i \geqslant \tau) + \epsilon_i \tag{4}$$

$$Elig_i = \gamma_0 + \gamma_1 \mathbf{1}(T_i \geqslant \tau) + \gamma_2(T_i - \tau) + \gamma_3(T_i - \tau) \cdot \mathbf{1}(T_i \geqslant \tau) + \epsilon_i \tag{5}$$

Where Y_i could be enrollment in second or third year, or dropout status. $Elig_i$ takes on the value 1 if student i is eligible for college loans in any year after she takes the PSU for the first time, and zero otherwise. T_i is a dummy for scoring more than the cutoff in the first attempt. Crossing the threshold in the first attempt ($\mathbf{1}(T_i \geqslant \tau)$) is random as shown in section 4.1 and is highly correlated with being eligible for college loans; hence, it is a valid instrument. The parameter β_1 measures the effect of having access to college loans for the compliers, i.e. those for whom the treatment status does not change in the following year after taking the PSU test for the first time.

4.4.1 Medium Run Enrollment

I first estimate the effects of having access to these two loan programs on enrollment in the second and third years of college.[22]

Panel A in table 7 presents the difference in the second year enrollment rate between eligible and non-eligible students.[23] Column (1) shows the enrollment in the second year in 2008 among those who took the test in 2007; column (2) shows the effects on enrollment in the second year in 2009 for those who took the PSU in 2008; and column (3) shows these two groups pooled together. To allow a comparison, the row labeled "Control Mean Enrollment" indicates the proportion of students below the cutoff who reached the second year of college.[24]

Column (3) shows that having access to college tuition loans increases the probability of reaching the second year of college by 20 percentage points. Relative to the enrollment in the second year at the discontinuity for the control group (9.5%), the effect is equivalent to a 213% increase.

The difference in college enrollment increased, indicating that credit access not only matters for initial enrollment, but also helps students stay in college, probably because they do not have to work or work less in part time jobs to finance educational investments and consumption.

Next I estimate the effects for third year enrollment. As before, "third year enrollment" is defined as having enrolled in three consecutive years, regardless of true advancement in coursework. For this case we can observe the behavior of only one cohort, the group of students that took the PSU for the first time in 2007. Column (4) of table 7 shows that the probability of enrolling in the third year increases by 21 percentage points for those eligible for loans, whereas the enrollment rate for the control group is 4.6 percent as shown in the row for the constants term. This implies a 455% increase in the probability of attending the third year, i.e. for each student who reaches the third year of college without access to tuition loans, 5.6 students do so when they have access.

Taken together, these results show that access to college loans is not only important for initial enrollment, but is also an important determinant of progress in college.

[22] I do not observe college performance for these students while in college. Being in second year is defined as enrolling in two consecutive years, regardless of their true advancement in coursework.

[23] Panel B in table 7 shows the first stages of the different definitions of eligibility used in the 2 stage least squares regressions for medium run enrollment. For instance, "Being eligible for loans in 2nd year (2008)" takes on the value 1 for any student who is around the cutoff in 2007 and is eligible for loans at the beginning of 2008. The constant corresponds to the proportion of students that was in the control group in 2007 and became eligible in the next year by retaking the test. The jump corresponds to the portion of compliers.

[24] Using the group of students below the cutoff as a benchmark is a conservative, since some of them do not comply. They took the test in the following years and became eligible for loans.

4.4.2 Dropout Rates

Few papers have documented the effects of financial aid on retention conditional on enrollment and dropout rates. Exceptions are Bettinger (2004), Singell (2004) and Stinebrickner and Stinebrickner (2008). The latter studies the relationship between credit constraints and dropout using a survey where students self-report the main causes of their dropout. They find that credit constraints are not an important reason for dropout.

In this section, I study the effects of college loans on the dropout status of the students in this quasi experiment. The main difference between these results and those in the previous section is that here the sample is restricted to students who enrolled in college when they first took the PSU, while in the previous analysis the comparison was made with respect to all students around the cutoff.

Since this section compares only students who enrolled (self-selected) into college around the cutoff, we need to check if they are comparable in observables to determine if the control group is the true counterfactual. Table 8 the estimation of equation (1)for the same group of covariates for students around the cutoff, with each columns corresponding to those who enrolled in 2007 through 2009. Students without access to loans who enroll in college are different from those eligible for loans: they come from higher income families (indicated by the income quintile and the self-reported income), suggesting that these students relied on family resources to enroll; they have more educated parents and come from higher quality schools (more students from public schools above the cutoff, and more students from voucher schools below), which may indicate a higher preferences for college education; etc. Therefore, the following results represent the lower bound of the true causal effect.

The available data does not provide a definitive measure of dropout status for the students. Some students may not enroll in one year but go back to finish their programs after some time out. I rely on the enrollment situation for the years after first enrollment. I will therefore use different definitions of dropout, as explained in the following table.

	Variable	Definition
1	Dropout after 1 year	Enrolled in college in 07, but did not in 08, or Enrolled in college in 08, but did not in 09.
2	Dropout in 2009	Enrolled in college in 07, but did not in 09, or Enrolled in college in 08, but did not in 09.
3	Dropout after 1 or 2 years	Enrolled in college in 07, but did not in 09.
4	Dropout in 2nd year	Enrolled in college in 07 and in 08, but did not in 09.

The first definition captures the dropout after the first year of college, i.e. those students who enrolled when they first took the PSU test but did not appear in the enrollment list the year after. This definition considers two groups of students: those who took the test and enrolled in 2007 but did not enroll in 2008; and those who took the test and enrolled in 2008 but did not attend the second year in 2009. The second definition considers all students who enrolled when they took the test for the first time in 2007 or 2008 but did not enroll in 2009. The third definition measures the dropout rate after two years of college; it takes on the value 1 if a student enrolled in 2007 but did not enroll in 2009, regardless of what happened in the second year. Lastly, to distinguish between second and third year dropouts, the fourth definition takes on the value 1 if a student enrolled in college for two consecutive years but did not enroll in the third.

Table 9, column (1) shows the dropout rate after the first year of college. Eligible students drop out 6 percentage points less frequently than ineligibles. The dropout rate for students without access to these loans 19%; this implies that the probability of dropping out after the first year of college experiences a 32% decrease.

The second definition of dropout relies on the most recent information of enrollment available in this data, the information in 2009. It considers two cases: students who enrolled in 2007 and did not enroll in 2009, without taking into account what happened in 2008; and students who enrolled in 2008 and did not enroll in 2009. Column (2) indicates that eligible students drop out 5 percentage points less than constrained ones. Once again, comparing this number with the dropout rate for the control group of 20 percent indicates that having access to loans reduces the dropout rate by 25%.

Now I move to dropout rates over a longer span of time: the dropout rate after two years of initial enrollment and the dropout rate in the second year of college. Column (3) shows that eligible students dropped out less frequently by 11 percentage points. The size of the coefficient implies a large reduction in the dropout rate: the dropout of all those below 475 is 29%, implying a 38% decrease in this rate. Column (4) shows that the dropout rate do not fall significantly for the students enrolled in a second year, but the magnitudes confirm previous conclusions, the reduction in the drop out probability suffers a 26% decrease. Since students below the cutoff come from higher income families, we can conclude that access to college tuition loans also has a role in explaining dropout rates.

All of this evidence put together indicates that credit access plays an important role in explaining progress in college. The differences appear from the first year of college and get larger with more years of college. In the dropout rate case, the differences in family income compensate for the differences in loan access then the effects are smaller (they are the lower bound of the true effect), nevertheless they are large ans highly significant. If these results were extrapolated to the following years, it is highly probable that we would find a positive and significant effect in graduation rates.

4.5 Price versus access effect

Now I turn to a different question, are these findings a consequence of pure access to credit markets or they are due to the presence of subsidies. College tuition loans may have an implicit subsidy component when the interest rate is lower than the market interest rate or when the repayment is not strongly enforced. This means that students who receive these loans experience a decrease in their college education costs and therefore an increase in the internal rate of return for college education, inducing an increase in college enrollment: the "price effect." Alternatively, these loans may constitute the only source of financing for human capital investments for families without access to credit markets: the "access effect."[25]

To differentiate the price and access effects I run the following two tests. First, if students are enrolling because of an increase in college education returns, we should not observe a discontinuity around the threshold for those who have access only to the State Guaranteed Loan program, because this program has very similar characteristics to market loans (see section 2.2).

Second, the price and access effect can be decomposed by observing different subgroups with and without credit constraints. The price effect can be obtained by observing the behavior of students from families without credit access restrictions around the discontinuity. Students from this type of family have access to their own resources or credit markets on both sides of the cutoff, but only those who score more than 475 on the PSU have access to the two financial programs studied here. The price effect would be the difference in college enrollment between students from above and below the threshold that belong to non-constrained families. On the other hand, the access effect would be the difference between the total and the price effect for a family with credit constraints. Since access to credit markets is highly correlated with income,[26] I will show the different responses for different income groups. Specifically, I will focus on the fourth income quintile, since it has access to credit markets and is also eligible for these two programs. Therefore the difference in enrollment between students above and below 475 gives the price effect.

Finally, I will combine the predictions of the two tests to see the different responses by income quintiles using only the State Guaranteed Loan program, the loan program that is similar to a market.

For the first test, I need to observe exogenous variation in the access to the State Guaranteed Loan. The natural candidate is comparing students around the cutoff. The problem is that around the cutoff I observe the influence of both programs at the same time. Therefore I run a bounding exercise excluding from the sample all students who enrolled in traditional universities (in these universities both programs were available). What is left are students who did not enroll and those who decided (self-selected) to go to private universities, where the State Guaranteed Loan program is the only option. I can assume that there are four types of students that enroll in the presence of these loans depending on the interaction of two categories: returns to college and access to credit. On the one hand, students with high returns (who enroll at higher than the market interest rates) and students with low returns (who enroll only if the interest rate is subsidized). On the other hand, students with access to loans from the market, and students that cannot borrow from the market, and these two loan programs are their only source of finance.

Therefore, the first group with high returns and with access to loans enrolls in college disregarding their position around the threshold. The second group with access to loans but with a low returns, would enroll only if is offered a subsidize loan. Thus, students from this group above the cutoff enroll in traditional universities where they can get the subsidized loan. The third group behaves

[25] Dynarski (2003) called these effects the subsidy and liquidity effects respectively.

[26] Section 2.2 shows that private bank loans were available only for student for the top two quintiles.

similarly, students without access to market loans with low returns from above the cutoff enroll only in traditional universities where they have access to the less expensive loan. For this group, scoring more than 475 for this group implies a change in both conditions, access and access to loans that are cheap enough for their ability level. Finally, the last group that has high returns and no access to market loans, for these students scoring more than 475 changes their access to loans and therefore they can enroll in any type of university.

Hence, the effect of subsidized loans can be neutralized analyzing what is the change in enrollment in private universities, where there is no access to the subsidized loan. In these universities we should not observe students coming from low returns to college groups (groups 2 and 3), and assuming that high returns students (constrained or not) do not choose university types for reasons that are correlated with access to loans. I test this assumption in table 10. Therefore, excluding from the sample to all students enrolled in traditional universities eliminates students from low returns groups and randomly from the groups with high returns.

Table 10 shows the estimation of equation (1) for several covariates for the subsample that excludes students from traditional universities. For the pooled sample there are only two variables that are not balanced: Income, measured by the income quintile and high school GPA. Income is slightly significant at the 10% and students from above the threshold have a significantly lower high school GPA. So the assumption is only true weakly.[27]

Figure 10 show the regression discontinuity for college enrollment analyzing the difference in enrollment in private universities, where the only source of financing is the State Guaranteed Loan. As before, the first three graphs show the effects for 2007, 2008, and 2009 respectively, while the graph in the lower right corner shows the effect for all years pooled together. This last graph shows that the probability of enrollment increases by 12 percentage points and is highly significant.

More formally, table 11 shows the regression discontinuity. Focusing on the results for all years pooled together, I observe an increase of 11 percentage points in the probability of enrollment, while the mean enrollment rate for the group without access to loans is 14 percent (see the row labeled "Control mean enrollment"). Thus, having access to loans represents a 78% increase over the baseline enrollment rate. As before, having access to loans increases significantly the number of students that enroll in college: for each student below the cutoff, 1.8 students enroll when college tuition loans are available. Moreover, the relative increase in enrollment is very similar to the previous results with both programs, which indicates that the price effect is small.

The second test requires exploring different responses for different income groups, so I return to the estimation of equation (3). As seen in section 4.2. As discussed in section 2.2, the fourth quintile has access to private loans similar in interest rate to the State Guaranteed Loan, so the weaker enrollment discontinuity indicates that the inclusion of these financing programs does not have a significant price

[27] This result may reflect the fact that private universities are more expensive, their students do not have access to subsidized transportation, are located not randomly, oftentimes in exclusive neighborhoods, etc. But those margins are not going to be explored here.

effect. Additionally, the big response for the first three quintiles, which do not have access to other loans, indicates that these effects are driven by the accessibility of loans rather than a change in the returns to college.

To confirm this, I combine the two tests, considering only students who enroll in private universities where the more expensive loan is the only available and the interaction with the income quintiles. Panel A of table 12 shows that for the first three quintiles the effects are strong and significantly different from zero while they are small and almost insignificant for the fourth quintile for all years separately and pooled together, confirming that the price effect for the State Guaranteed Loan is close to zero.

To assess the importance of these effects, panel B of table 12 gives the mean enrollment rate for students below the cutoff, and panel C the relative increments, i.e. the ratio of the enrollment jump at the cutoff shown in table 12 to the enrollment in the control group. Having access to the State Guaranteed Loan implies a 125% increase in the probability of enrollment for the first quintile on average, while the increment is 74% and 55% for quintiles two and three respectively.

With this information in hand, I can draw conclusions about the access to credit markets effects by looking at the responses for the first three quintiles. The evidence shows that the (partial) elimination of credit constraints implies a significant increase in the probability of college enrollment: for each student enrolled without access to loans, more than two enroll when these credit constraints are lifted.

4.6 Validity Checks

This section tests two key assumptions of the regression discontinuity approach. First, I explore whether colleges are able to select students based on their loan eligibility. Second, I look at whether the programs chosen for students above the threshold were also available for students below the cutoff.

In the first case, colleges may offer more places to students above the cutoff, because these financial opportunities imply that they are more likely to finish a degree (students can avoid working while studying, they will have secure financial resources for the whole period, etc.). In the second case, the RD may not be valid if students above the cutoff have more programs to choose from.

4.6.1 Are colleges selecting students differently around the cutoff?

To rule out the possibility that colleges are observing or inferring the financial status of the applicants and selecting based on that information, I present two validity checks. The first shows placement for non eligible students (students that belong to the highest income quintile), while the second uses information on applications and placement in traditional universities.

Placement for non-eligible

Students from the highest income quintile are not eligible for loans, so the college enrollment rate should be the same for students above and below the threshold. Universities do not observe student income when they offer placement,[28] so they cannot discriminate based on whether a student has access to loans or not. If colleges are discriminating against students below 475, we should see a discontinuity at 475 for all income groups, including those from the highest quintile.

Panel A in table 13 shows the same regression discontinuity, but only for students from the richest income quintile. The results indicate that there is no discontinuity around 475 for this income group.

To have a broader picture, figure 11 depicts the regression discontinuities for the different years and for all years together using the same set of graphs as before. This figure shows that there is no difference in enrollment around the cutoff. These graphs confirms that the positive and significant at 10% effect for 2009 shown in column (4) of table 13 is consequence of an type I error rather than a discontinuity. This evidence shows that colleges are not selecting students based on their financial condition.

Applications and placement for traditional colleges

The centralized process requires that students apply and rank at most 8 programs. The placement process starts by offering a position to the student with the best PSU score for her highest preference. The process continues with the following students until all programs are full or all students are assigned.

I use the information on students' applications to show that college placement offers are locally continuous at the cutoff, and the discontinuity is driven by students who score more than 475 who apply more often after they became eligible for these loan programs. After students apply, the probability of being placed will depend on their relative position in the list of applicants, which is not discontinuous around the cutoff.

The same regression discontinuities are run with valid applications and placement conditional on having applied to traditional colleges as dependent variables. Panel A of table 14 shows results for the following regression using all students in optimal bandwidth neighborhood around the cutoff:

$$Pr(Apply_i^{Trad} = 1) = \gamma_0 + \gamma_1 1(T_i \geqslant \tau) + \gamma_2(T_i - \tau) + \gamma_3 1(T_i \geqslant \tau) \cdot (T_i - \tau) + \zeta_i \tag{6}$$

$Apply_i^{Trad}$ takes on value 1 if a student i applied to any program from a traditional university. As before, T_i is student i's PSU score, τ is the cutoff of 475 and ζ_i a mean zero error term.

The first column shows the results for all years together, while columns (2) to (4) show the results for each year from 2007 to 2009. In column (1), the probability of application, $\hat{\gamma_1}$, increases by 33 percentage points for those who are eligible for loans.

To show that traditional colleges are not selecting students depending on their loan eligibility, panel B of table 14 shows the probability of being placed conditional on having applied to a program in these universities. Specifically, it shows the following regression discontinuity for students around the threshold:

[28] Beyond students' self-reported income category, which has a correlation of only .4 with the income quintiles reported by the IRS.

$$Pr(Placed_i = 1|Apply_i^{Trad} = 1) = \phi_0 + \phi_1 1(T_i \geqslant \tau) + \phi_2(T_i - \tau) + \phi_3 1(T_i \geqslant \tau) \cdot (T_i - \tau) + \xi_i \quad (7)$$

Therefore this regression only considers students who applied to traditional colleges, and $Placed_i$ takes on value 1 if the student i was placed in one of the programs.

Column (1) of Panel B in table 14 shows all years pooled together, and columns (2) to (4) show the results for each year separately. There is no discontinuity around the cutoff for any of the regressions since the parameter $\hat{\phi_1}$ is not significantly different than zero.

To show that these results are not sensitive to bandwidth or functional forms, figure 12 shows the results of these two tables adding a fourth order spline for all students between 450 and 500 PSU points. The figure on the left shows the discontinuity in applications around the cutoff, while the figure on the right shows the probability of placement conditional on having applied.[29]

4.6.2 Program cutoff on private colleges

To rule out the possibility that the enrollment discontinuity is driven a larger availability of programs for students above the cutoff, I compute the score for the last student enrolled in each program (program cutoff) to see if the programs chosen by students above the cutoff are available for students below.

Panel B of table 15 shows the percentage of programs chosen for students in the treatment group that have a program cutoff below 475 and thus are available for students in the control group. In the worst case (year 2009) 92.5% of the students in the treatment group enrolled in programs that would accept students from the control group. If an important part of colleges were selecting only students with scores above 475 we should observe a bigger difference in the availability of programs.

Even though the percentages in table 15 are high, they are statistically different from 100%. To see the effect of the difference in program availability between groups, Panel A of table 15 shows the same regression discontinuity as before in a 2 point window around the cutoff, eliminating from the sample all students in the treatment group that enrolled in a program with a program cutoff larger than 475.

Column (1) of table 15 shows the effect for all three years pooled together for the regression discontinuity that eliminates students enrolled in programs not available for the control group. Again, the effect of access to college loans on college enrollment is significant and equal to 14 percentage points, which is equivalent to an 87% increase with respect to the average enrollment for the control group.

To see if these results depend on the chosen bandwidth or functional form, figure 13 shows the regression discontinuity for each year separated and all years pooled together including a fitted fourth order polynomial spline. The results are the same and do not depend on the bandwidth or functional

[29] As before, each dot represent the average outcome within students in a 2-points wide bin.

form.

4.7 External validity

Regression discontinuity estimates are very reliable for the effect of college loan access on college enrollment for students around the cutoff, but do not give information about what happens in other points of the score distribution. To shed light on what happens elsewhere, I propose a second identification strategy that neutralizes the effects of family background to test the influence of access to loans on college enrollment: using the sample of twins to run family fixed effects regressions.

The motivation of this strategy is that siblings, and especially twins, are exposed to the same family characteristics throughout their lives. They receive the same parental influence about preferences for college education and the same information set about expected earnings and the difficulty of college. Moreover, they receive the same educational inputs at home, the same genes in the case of monozygotic twins, and in most cases they go to the same school and they share the same classroom and therefore the same teachers and peers.

As a consequence, I can estimate the causal effect comparing how different credit access status explains variation on college enrollment within twins.

Consider the following model. Suppose the probability of college enrollment for an individual i that belongs to family j is $P_{ij} = Prob(College_{ij} = 1)$, is a function as follows:

$$P_{ij} = f(C_{ij,}\mathbf{X}_{ij}, \mathbf{Y}_j, f_j) \tag{8}$$

$\mathbf{X}_{ij}$is a vector of individual characteristics such as ability (maybe measured as PSU score or high school GPA), educational inputs, school quality, etc. that affects the probability of enrolling in college. $\mathbf{Y}_j$ and f_j correspond to vectors of observed and unobserved family characteristics respectively, which are available for each member of the family: information about college returns and college difficulty, educational assets at home, parental education, parental genes, etc. C_{ij} is a dummy that takes on the value of 1 if student i has access to college tuition loans.

One way to test (8) would be to use a linear probability model (following Ashenfelter and Rouse, 1998):

$$P_{ij} = \alpha + \beta C_{ij} + \mathbf{X}'_{ij}\delta + \mathbf{Y}'_j\gamma + f_j + \epsilon_{ij} \tag{9}$$

The problem with this estimation is the existence of unobserved variables in both individual and family components that enter the error term, producing bias. To deal with this situation, I use the sample of twins and include family fixed effects that control for family characteristics that affect the enrollment decision.

The fixed effects estimation would be the following:

$$P_{ij} - \overline{P}_j = \beta(C_{ij} - \overline{C}_j) + (\mathbf{X}'_{ij} - \overline{\mathbf{X}}'_j)\delta + (\epsilon_{ij} - \bar{\epsilon}_j) \tag{10}$$

This estimation differences out all family characteristics. The proper estimation of the parameter of interest β, depends on the availability of individual characteristics $\mathbf{X}_{ij}$. The observed part of the difference $(\mathbf{X}_{ij}^{'} - \overline{\mathbf{X}}_{j}^{'})$, such as the difference in the PSU score, school quality, or high school GPA are easily introduced in the estimation, while the unobserved elements, are assumed to be equal or sufficiently similar among twins (preferences for college may be quite similar if they depends on family influence, educational assets given specifically to a twin also do not differ much among them, etc.).

The main confounding problem is the presence of unobserved individual characteristics that are not similar between twins that explain the enrollment decision and are correlated with credit status. For example, one twin may be highly motivated to go to college, which enables him to get a higher PSU score and therefore get access to college tuition loans, while the second twin, not interested in college, simply scores low and does not become eligible.

Since the variation in credit access is still given by being eligible or not for college tuition loans (i.e. scoring above or below the cutoff), considering small windows around the cutoff may solve the problem of unobserved differences. The problem now is to determine the window around the cutoff, because we do not know if these differences are due to shocks from the test or the true influences of different unobserved variables.

To estimate β from equation (10) I will show different score windows, and I will show that the results are very robust to different windows and functional specifications, suggesting that the influences of these unobserved variables are not important.

4.7.1 Implementation using twins

There are 6,269 individuals in the sample of twins, triplets and quadruplets from 2007 to 2009, but for the following analysis, I consider only students who take the test for the first time just after graduating from high school (for the same reasons mentioned before), which reduces the sample to 5058 individuals. Moreover, in order to compare twins that are similar in abilities I consider twins pairs that do not differ more than S_k PSU points, where $S_k = 50 + 25 \cdot k$ $(k = 0, ..., 4)$. Table 16 shows the estimation of equation (10). It shows different specifications from linear to a polynomial of fourth order. Table 17 adds to this specification a measure of ability (high school GPA) and restricts the sample to twins of the same sex (to account for differences in genes) and twins who graduate from the same high school (to account for unobserved differences in the quality of the education received).

The parameter of interest is an indicator for whether the students score more than the cutoff. We observe two main things: first, the probability of college enrollment rises between 18 and 33 percentage points, depending on the specification and score window used, for students that became eligible for loans; and second, results are very robust among specifications and score windows. The average college enrollment for non-eligible students in this sample is 14 percent, which implies that college enrollment increases in the range of 113% to 205%.

Moreover, we observe that these results are very close to those obtained using the regression discontinuity (see table 5 column 3), which suggests that the estimation is also valid for a wider range around the cutoff.

One interesting result in table 17 is that the PSU score has no influence on the twins' enrollment decisions, which suggests that twins behave almost identically even though they may have large differences in the college admission test score and therefore in ability.

With these results, I argue that access to loans has approximately the same effect for all of the students in the ability distribution.

5 Conclusions

In this paper I present evidence of the effects of access to college loans on enrollment, progress in college, and dropout decisions using a natural experiment that gives access to loans to eligible students that score more than a given cutoff on the college admission test in Chile, thus enabling a regression discontinuity design.

This paper offers several advantages over the previous literature addressing the effects of aid and credit constraints on college enrollment: (1) it uses random variation on access to loans directly since loans are assigned to students randomly around the cutoff, (2) it uses the universe of students that participate in the admission process with full information on enrollment and aid access, (3) it analyses a centralized admission process that relies exclusively on observed student characteristics, and (4) this admission process does not use aid as a mechanism to attract better students. These features of the experiment overcome the issues of unobservable access to aid (loans specifically), omitted variables bias, measurement problems of enrollment status, endogeneity of the placement process and endogeneity of aid offers.

I estimate the causal effect of access to these loans on college enrollment. I find that the enrollment rate for eligible students increases by 18 percentage points, which is equivalent to a 95% increase in the enrollment rate. The effect is robust to a variety of functional forms, bandwidths around the discontinuity and sample periods. This result has three salient features: First, it shows that aid programs have a positive causal effect on college enrollment. Second, the estimated causal effect is larger in magnitude compared to those found in the developed country context. Finally, and maybe most importantly, access to these loan programs closes the enrollment gap for the lowest income quintile households with access to these programs. Among students who are barely *ineligible* for loans the relative enrollment rate for students from families with incomes in the first and fifth quintiles is 2:1, which is about the same enrollment gap as that for the whole population. Strikingly, among students who are barely *eligible*, the gap is statistically zero. To my knowledge, this is the first paper that shows this type of evidence with a very credible identification strategy and with a sample that addresses all the problems indicated in the literature. However, this result is conditional on being around the cutoff - the gap is still present for the whole population, because poor students are less likely to graduate from high school and are therefore more likely to be below the cutoff.

Using the enrollment status data of subsequent years, I estimate the effect of access to loans on enrollment in the second and third year of college. Access to the loan programs increases the enrollment rate significantly (20 and 21 percentage points respectively). Moreover, the relative increase rises with more years of college: the increase in enrollment relative to students in the control group is 213% and 445% for students attending the second and third year respectively. These results are consistent with previous evidence for grants, but relatively much larger. Dynarski (2003) shows a positive but not significant effect for completion of at least one year of college (14 percentage points), even though the program she analyses is much more generous.

Conditional on enrollment in college, I estimate the effects on dropout rates. First I show that students who enroll without access to loans come from higher income families. This indicates that comparisons between the two groups are biased towards zero. This is especially important for papers that compare students with and without aid conditional on enrollment that do not address the selection into college. Nevertheless, I find that students with access to loans are 6 percentage points less likely to drop out from college after the first year, reducing the dropout rate in 31%. Meanwhile, students with access to loans are 11 percentage points less likely to drop out after two years of college, which translates into a 38% reduction in the dropout rate.

Previous results have found little or no effect of loans on persistence or dropout rates (see Singell (2004), DesJardins et al (2002), and Stinebricker and Stinebrickner (2008)). All of these papers use information from a single institution. To the best of my knowledge, my paper is the first one using the universe of all available institutions and students participating in the higher education application process to account for the effect of aid on dropout rates, thereby eliminating potential bias from students that are considered dropouts when they actually switch institutions. In addition, my results are substantially larger than those from the US.

I also present suggestive evidence from two tests that my results are driven by the effect of access to credit markets instead of being caused by lower than market interest rates and low enforceability. The first test restricts the analysis to students that enrolled in private universities where the only loan available was the State Guaranteed Loan, which presents similar interest rates and enforceability as the loans currently available in the market. Thus if students are enrolling because of a decrease in the cost of loans, we should not observe any discontinuity in enrollment for private universities. The second test compares the expected response in enrollment for different income quintiles. On the one hand, the lowest two income quintiles do not fulfill the bank's requirements of having a minimum family income, so they did not have access to credit markets until the creation of these programs. On the other hand, the fourth income quintile was eligible for loans already in the market, so the creation of these programs did not change their access to credit markets, but it may have changed the loan price. Thus, the fourth quintile only faces a price effect.

For the first test, having access to the State Guaranteed Loan increases the probability of enrollment in private colleges by 11%, representing an 80% increase in the enrollment rate, compared to a 95% increase when both programs were analyzed together. Since the relative effects are very similar, I conclude that the price effect is not important and the effects are mainly driven by partial elimination of credit constraints. For the second test, the enrollment discontinuity for the fourth quintile is not significantly different form zero, which indicates that the price effect is not relevant.

These results based on the regression discontinuity result in strong internal validity but say nothing about what happens in other parts of the score distribution. To deal with this problem I present a second identification strategy that neutralizes family background: I show regressions using family fixed effects for the sample of twins. The results are statistically indistinguishable from the results using RD, which indicates that the effect of loan access on enrollment is similar in other parts of the distribution.

Taken together, this evidence demonstrates that differential credit access plays an important role in explaining the college enrollment gap between high and low income families. Incomplete credit markets prevent students from low income families from investing in human capital.

In future work, together with the providers of this data, I will estimate the effects of access to loans on college graduation.[30] The long run objective of this project is to estimate the effects of access to loans on other labor outcomes, especially the returns to college education.

6 References

1. Ashenfelter, O and C. Rouse (1998), "Income, Schooling and Ability: Evidence From a New Sample of Identical Twins." *Quarterly Journal of Economics*, 113: pp. 253-284.

2. Attanasio O. and K. M. Kaufmann (2009), "Educational Choices, Subjective Expectations, and Credit Constraints," NBER Working Papers #15087, National Bureau of Economic Research.

3. Battistin, E. and E. Rettore (2008). "Ineligibles and Eligible Non-participants as a Double Comparison Group in Regression-Discontinuity Designs." *Journal of Econometrics,* 142 pp. 715–730.

4. Becker, G. S. (1967). "Human Capital and the Personal Distribution of Income". (W. S. Woytinski Lecture) Ann Arbor Michigan: University of Michigan Press.

5. Belley, P., and L. Lochner (2007). "The Changing Role of Family Income and Ability in Determining Educational Achievement." *Journal of Human Capital*, vol1. 1, no. 1, pp. 37–89.

6. Bettinger, E. (2004). "How Financial Aid Affects Persistence." NBER Working Paper #10242, National Bureau of Economic Research.

7. Brown M., J. K. Scholz and A. Seshadri (Forthcoming). *"A New Test of Borrowing Constraints for Education".* The Review of Economic Studies.

[30] Students enrolled in college programs in 2007 should start graduating in December 2011.

8. Card, D. (1999). "The Causal Effect of Education on Earnings." In (O. Ashenfelter, and D. Card, eds.). Handbook of Labor Economics, Vol 3A. Amsterdam: Elsevier Science, North-Holland, pp. 1801–63.

9. Card, D. (2001). "Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems." *Econometrica*, vol. 69(5). pp. 1127–60.

10. Carneiro, P. and J. J. Heckman (2002). "The Evidence on Credit Constraints in Post-secondary Schooling." *Economic Journal,* 112, 482, pp. 705-34.

11. Cameron, S. and J. J. Heckman (2001). "The Dynamics of Educational Attainment for Black, Hispanic, and White Males." *Journal of Political Economy*, vol. 109, pp. 455–99.

12. Cameron, S. and C. Taber (2004). "Estimation of Educational Borrowing Constraints Using Returns to Schooling." *Journal of Political Economy*, vol. 112, no.1. pp. 132-182.

13. Chen, R. (2008) "Financial Aid and Student Dropout in Higher Education: A Heterogeneous Research Approach" in (J. C. Smart, editor) Higher Education: Handbook of Theory and Research, Volume 23, 209-239.

14. DesJardins, S.L., D.A. Ahlburg, and B.P. McCall (2002). "Simulating the Longitudinal Effects of Changes in Financial Aid on Student Departure from College." *The Journal of Human Resources.* 37 (3): pp. 653-679.

15. Dinkelman T. & Martínez C. A. (2011). "Investing in Schooling in Chile: The Role of Information about Financial Aid for Higher Education," Working Papers 1296, Princeton University, Department of Economics, Center for Economic Policy Studies.

16. Dynarski, S. (2003). "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion." *American Economic Review,* 93, pp. 279-288.

17. Gurgand, M. , A. Lorenceau and T. Melonio (2011), "Student Loans: Liquidity Constraint and Higher Education in South Africa." Paris School of Economics working paper 2011-20.

18. Hahn, J., P. E. Todd and W. Van der Klaauw (2001). "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica,* vol. 69, no. 1, pp. 201-209.

19. Heckman, J. J., T. M. Lyons, and P. E. Todd (2000). "Understanding Black-White Wage Differentials, 1960-1990." *The American Economic Review*, vol. 90, No 2, pp. 344-349.

20. Hossler, D., M. Ziskin, J. P.K. Gross, S. Kim, and O. Cekic (2009) "Student Aid and Its Role in Encouraging Persistence" in (J. Smart, editor) Higher Education: Handbook of Theory and Research, Volume 24, 389-425.

21. Imbens, G. W. and J. D. Angrist (1994). "Identification and Estimation of Local Average Treatment Effects." *Econometrica*, vol. 62, no. 2 pp. 467-475.

22. Imbens, G., and K. Kalyanaraman (2011), "Optimal Bandwidth Choice for the Regression Discontinuity Estimator," *The Review of Economic Studies*, forthcoming.

23. Imbens, G. W. and T. Lemieux (2008). "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics*, 142 pp. 615–635.

24. Kane, T. (1994). "College Entry by Blacks since 1970: The Role of College Costs, Family Background, and the Returns to Education." *Journal of Political Economy*, vol 105, no. 5, pp. 878-911.

25. Kane, T. (1996). "College Costs, Borrowing Constraints and the Timing of College Entry." *Eastern Economic Journal*, vol. 22(2). pp. 181–94.

26. Kaufmann, K. M. (2010). "Understanding the Income Gradient in College Attendance in Mexico: The Role of Heterogeneity in Expected Returns," Working Papers 362, IGIER (Innocenzo Gasparini Institute for Economic Research), Bocconi University.

27. Keane, M. P. and K. I. Wolpin (2001). "The Effect of Parental Transfers and Borrowing Constraints on Educational Attainment." *International Economic Review* 42, 4, pp. 1051- 1103.

28. Lang, K. (1993). "Ability Bias, Discount Rate Bias, and the Return to Education." Manuscript. Department of Economics, Boston University.

29. Lee, D. S. (2008). "Randomized Experiments from Non-random Selection in U.S. House Elections." *Journal of Econometrics*, 142(2): pp. 675–97

30. Lee, D. S., and T. Lemieux. (2010). "Regression Discontinuity Designs in Economics." *Journal of Economic Literature*, 48(2): pp. 281–355.

31. Lochner, L. and A. Monge-Naranjo (2011), "The nature of credit constraints and human capital", *American Economic Review* 101(6): 2487–2529.

32. Lochner, L. and A. Monge-Naranjo (2011), "Credit Constraints In Education" NBER Working Paper #17435, National Bureau of Economic Research.

33. Manski, C. F. and D. A. Wise (1983), "College Choice in America", Cambridge, MA. Harvard University Press.

34. McPherson, M. S. and M. O. Schapiro (1991). "Does Student Aid Affect College Enrollment? New Evidence on a Persistent Controversy." *American Economic Review*, 81, 1, pp. 309-18.

35. Nielsen, H. S., T. Sørensen, and C. Taber (2010). "Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform." *American Economic Journal: Economic Policy*, 2, pp. 185–215.

36. Singell, L.D. (2004). "Come and Stay a While: Does Financial Aid Effect Retention Conditioned on Enrollment at a Large Public University?" *Economics of Education Review*, 23, no. 5 pp. 459–71.

37. Stinebrickner R. & Stinebrickner T. (2008). "The Effect of Credit Constraints on the College Drop-Out Decision: A Direct Approach Using a New Panel Study." *The American Economic Review*, Vol. 98, No. 5, pp. 2163-2184.

38. Van Der Klaauw, W. (2002). "Estimating the Effect of Financial Aid Offers on College enrollment: A Regression–Discontinuity Approach." *International Economic Review*, vol. 43, no. 4.

39. Van Der Klaauw, W. (2008). "Regression–Discontinuity Analysis: A Survey of Recent Developments in Economics." *LABOUR*, Volume 22, Issue 2, pp. 219–245.

7 Tables

7.1 Admission Process Characteristics

Table 1: Requirement for scholarships

	College Type	PSU Score Cutoff	Income Quintile	High School GPA(*)
Loans				
- Traditional	Traditional	475	1 to 4	
- Guaranteed by the State	All Accredited	475	1 to 4	
Scholarships and Grants				
- Bicentenario	Traditional	550	1 and 2	
- Juan Gomez Millas	Traditional	640	1 and 2	
- Teacher's children	Traditional	500	1 to 4	5.5
- Pedagogy Students	All	600	-	6.0
- Excellence	All	-	1 to 4	best 5%
- PSU score	All	National or regional score	1 to 4	

(*): High School GPA goes from 1 to 7 points

Table 2: Income quintiles upper bounds definitions.

Income Quintile	Monthly Family income in CLP$	Monthly Family income in US$
I	208,575	426
II	356,800	728
III	565,580	1,154
IV	1,013,620	2,069
V	∞	∞

Source: CASEN 2009. Calculated from per capita upper bounds multiplied by the mean family size 3.54

Table 3: Sample Characteristics and size of the RD sample.

	Years			Pooled
	2007	2008	2009	07 to 09
Mean	500.5	500.6	500.5	500.5
Minimum	177	160.5	175.5	160.5
Maximum	838	850	845.5	850
Standard deviation (sd)	102.0	102.9	102.7	102.6
Observations (N)	211,258	214,494	240,783	666,535
$w = 44$	+ Around cutoff + Preselected + 1st time takers			
Observations (n)	22,633	25,114	29,899	77,646
n/N	11%	12%	12%	12%
n/sd	.43	.43	.43	.43

w correspond to the bandwidth used in the RD regression. $w = 44$ was calculated with the algorithm given by Imbens and Kalyanaraman (2009).
$^n/_N$ refers to the percentage of the full sample used in the estimation when $w = 44$, $^n/_{sd}$ refers to the percentage relative to the standard deviation of the full sample.

7.2 RD preliminaries

Table 4: Balance among covariates. First time takers, 44 points around cutoff, and preselected for loan before taking the admission test.

bandwidth w=44								
Year Process							Pooled	
	2007		2008		2009		07-09	
Variable	dif	abs(t)	dif	abs(t)	dif	abs(t)	dif	abs(t)
Self reported income	0.00	(0.19)	0.01	(1.01)	-0.02	(1.69)*	0.00	(0.43)
Quintile	0.03	(1.21)	0.03	(1.06)	-0.01	(0.35)	0.02	(1.1)
Mother education	0.02	(0.31)	-0.06	(0.96)	0.05	(0.86)	0.00	(0.11)
Father education	-0.03	(0.43)	0.04	(0.52)	0.04	(0.62)	0.02	(0.46)
1(female)	0.00	(0.2)	-0.01	(0.53)	0.01	(0.48)	0.00	(0.13)
High school GPA	-0.47	(2.32)**	-0.15	(0.67)	0.34	(1.8)*	-0.04	(0.37)
H. school type	0.01	(0.9)	0.04	(3)***	-0.01	(0.9)	0.01	(1.57)
1(married)	0.00	(0.67)	0.00	(0.07)	0.00	(0.73)	0.00	(0.15)
1(work)	0.01	(1.48)	-0.01	(0.66)	0.01	(0.72)	0.00	(0.92)
HH Size	0.01	(0.23)	-0.09	(1.97)**	0.01	(0.31)	-0.02	(0.74)
1(mother works)	-0.04	(0.64)	0.06	(0.99)	0.02	(0.39)	0.02	(0.45)
1(father works)	0.05	(0.91)	-0.02	(0.51)	0.02	(0.44)	0.01	(0.45)
Who finance college	0.01	(0.13)	0.04	(0.93)	-0.01	(0.22)	0.01	(0.45)
Will live outside HH	0.02	(1.57)	-0.01	(1.25)	-0.01	(0.63)	0.00	(0.32)
Expect aid to finance	0.00	(0.9)	0.00	(0.36)	0.00	(0.16)	0.00	(0.4)
Observations (n)	22,633		29,899		25,114		77,646	

Note: Dif refers to the $\beta_1$of equation (1). t-values in parenthesis (in absolute values). (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Self-reported income is classified in three categories, 1 being the lowest. School type is classified in three categories, 1 for private, 2 for voucher, and 3 for public schools.

7.3 Effect on College enrollment

Table 5: RD for college enrollment for students preselected for loans. $w = 44$ PSU points

Dependent Var.:	College Enrollment in year:			
	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
1(PSU>=475)	.175	.192	.159	.176
	(.006)***	(.011)***	(.011)***	(.010)***
Const.	.183	.155	.209	.182
	(.004)***	(.007)***	(.007)***	(.006)***
% Increase w/r Control	95.7%	124%	76.2%	96.7%
Obs.	77646	22633	25114	29899
R^2	.107	.118	.096	.109

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

7.4 College Enrollment Gap by family income

Table 6: RD College Enrollment by income quintile. By year and full sample. $w = 44$ PSU points.

Dependent Var.:	College Enrollment in year			
	Pooled 07 to 09	2007	2008	2009
	(1)	(2)	(3)	(4)
1(PSU>=1) x q1	.201	.210	.186	.203
	(.008)***	(.015)***	(.016)***	(.013)***
1(PSU>=1) x q2	.171	.211	.160	.157
	(.013)***	(.026)***	(.022)***	(.022)***
1(PSU>=1) x q3	.164	.210	.162	.134
	(.017)***	(.031)***	(.028)***	(.029)***
1(PSU>=1) x q4	.070	.064	.033	.110
	(.020)***	(.032)**	(.036)	(.034)***
1(PSU>=1) x q5	.031	-.091	.042	.085
	(.022)	(.058)	(.030)	(.044)*
Obs.	84605	24126	28536	31943
R^2	.378	.377	.379	.384

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

7.5 Enrollment and Dropout Rate in the following years

Table 7: Enrollment in Second and third years of college for all students around the cutoff in 2007 and 2008. $w = 44$

	(1)	(2)	(3)	(4)
PANEL A: 2SLS IV Regression				
Dependent Var.:	Enrollment 2nd year in 2008	Enrollment 2nd year in 2009	Enrollment 2nd year pooled	Enrollment 3rd year in 2009
Eligible	.222	.183	.202	.205
	(.013)***	(.014)***	(.010)***	(.013)***
Const.	.062	.126	.095	.046
	(.008)***	(.009)***	(.006)***	(.008)***
% increase	358%	145%	213%	445%
PSU Process	2007	2008	07-08	2007
R^2	.091	.058	.072	.088
PANEL B: First Stages				
Instrumented Var.:	Eligible for loans in 2nd year (2008)	Eligible for loans in 2nd year (2009)	Eligible for loans in 2nd year (pooled)	Eligible for loans in 3rd year (2009)
1(PSU>475)	.788	.753	.769	.757
	(.007)***	(.007)***	(.005)***	(.008)***
Const.	.271	.286	.281	.326
	(.030)***	(.027)***	(.020)***	(.033)***
Obs.	22693	25253	47946	22693
R^2	.745	.729	.737	.709

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 8: Balance among covariates for Students that choose college in their first PSU test. $w = 44$

Year Process (w=44)	2007		2008		2009	
Variable	dif	abs(t)	dif	abs(t)	dif	abs(t)
Self reported income	-0.14	(4.79)***	-0.07	(2.38)**	-0.13	(4.96)***
Quintile	-0.20	(2.91)***	-0.19	(3.31)***	-0.25	(4.71)***
Mother education	-0.22	(1.68)*	-0.35	(2.71)***	-0.17	(1.35)
Father education	-0.51	(3.33)***	-0.30	(2.01)**	-0.12	(0.80)
1(female)	0.08	(2.68)***	-0.03	(1.41)	0.02	(0.90)
High school GPA	0.90	(1.91)*	0.49	(1.10)	0.40	(1.0)
H. school type	0.11	(3.31)***	0.10	(3.82)***	0.06	(2.27)**
1(married)	-0.01	(1.23)	0.02	(1.86)*	0.01	(1.46)
1(work)	0.01	(0.46)	0.01	(0.35)	0.01	(0.58)
HH Size	0.13	(1.16)	0.02	(0.18)	0.12	(1.39)
1(mother works)	-0.04	(0.28)	0.25	(1.99)**	0.15	(1.21)
1(father works)	0.06	(0.52)	0.01	(0.15)	0.11	(1.18)
Will live outside HH	0.02	(0.64)	-0.03	(1.23)	-0.01	(0.60)
Who finance college	0.14	(1.43)	0.04	(0.55)	0.13	(1.72)*
Expect aid to finance	0.01	(0.69)	0.01	(0.87)	-0.01	(0.76)
Both Parents live	0.00	(0.05)	0.01	(0.44)	-0.01	(0.27)
Obs (N)	6,728		8,022		8,980	

Note: Dif refers to the β_1 of equation (1). t-values in parenthesis (in absolute values). (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 9: Dropout rate in 2nd and 3rd years of college around the cutoff. $w = 44$

Dep. Variable :	Dropout after 1y of college	Not in 2009	Dropout after 2y of college	Dropout in 2nd y of college
	(1)	(2)	(3)	(4)
Eligible	-.058	-.051	-.110	-.010
	(.017)***	(.018)***	(.033)***	(.024)
Const.	.188	.201	.293	.038
	(.099)*	(.105)*	(.155)*	(.124)
Covar	Y	Y	Y	Y
% Decrease	31%	25%	38%	26%
Obs.	14801	14801	6749	6749
R^2	.032	.04	.067	.018

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%. All regressions 2 point around the cutoff and linear specification.
Covariates are "self reported income", income quintile, mother education, father education, age, female dummy, high school GPA, health insurance system, married dummy, work dummy, dummy for public schools, dummy for voucher schools, household size.

7.6 Price vs. access effects

Table 10: Balance among covariates. Excluding students from traditional universities. $w = 44$

Year Process	2007		2008		2009		Pooled	
(w=44)				07-09				
Variable	dif	abs(t)	dif	abs(t)	dif	abs(t)	dif	abs(t)
Self reported income	0.01	(0.8)	0.02	(1.85)*	-0.02	(1.83)*	0.00	(0.3)
Quintile	0.04	(1.21)	0.06	(2.06)**	0.00	(0.09)	0.03	(1.76)*
Mother education	0.01	(0.22)	-0.01	(0.1)	0.08	(1.4)	0.03	(0.95)
Father education	-0.02	(0.29)	0.05	(0.65)	0.06	(0.79)	0.03	(0.79)
1(female)	0.01	(0.65)	0.00	(0.08)	0.00	(0.3)	0.00	(0.58)
High school GPA	-0.92	(4.15)***	-0.37	(1.56)	0.09	(0.46)	-0.34	(2.7)***
H. school type	-0.01	(0.84)	0.02	(1.64)	-0.02	(1.44)	0.00	(0.5)
1(married)	0.00	(0.63)	0.00	(0.13)	0.00	(0.69)	0.00	(0.06)
1(work)	0.01	(0.91)	-0.01	(0.69)	0.01	(1.39)	0.01	(1.04)
HH Size	-0.01	(0.21)	-0.10	(2.18)**	0.02	(0.51)	-0.03	(0.96)
1(mother works)	-0.07	(0.93)	0.01	(0.21)	0.01	(0.22)	-0.01	(0.25)
1(father works)	0.03	(0.55)	-0.03	(0.63)	0.01	(0.15)	0.00	(0.03)
Will live outside HH	0.00	(0.33)	-0.02	(1.93)*	-0.01	(0.96)	-0.01	(1.56)
Who finance college	-0.02	(0.49)	0.02	(0.46)	-0.01	(0.38)	-0.01	(0.25)
Expect aid to finance	0.00	(0.37)	0.00	(0.73)	0.00	(0.16)	0.00	(0.51)
Obs (N)	19,202		21,705		26,956		67,863	

Note: Dif refers to the β_1 of equation (1). t-values in parenthesis (in absolute values). (***): p$\leqslant$1%, (**): p$\leqslant$5%, (*):p$\leqslant$10%

Self-reported income is classified in three categories, 1 being the lowest. School type is classified in three categories, 1 for private, 2 for voucher, and 3 for public schools.

Table 11: RD for college enrollment for students preselected for loans. Restricting the sample to enrolled into Private Colleges. $w = 44$ PSU points

Dependent Var.:	College Enrollment in year:			
	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
1(PSU>=475)	.112	.116	.091	.127
	(.006)***	(.010)***	(.011)***	(.010)***
Const.	.140	.100	.162	.152
	(.004)***	(.006)***	(.007)***	(.006)***
% Increase wr Control Enr.	80%	116%	56%	84%
Obs.	67863	19202	21705	26956
R^2	.048	.049	.034	.061

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 12: RD College Enrollment by income quintile. By year and full sample. $w = 44$ PSU points.

	PANEL A: Regression by quintile			
Dependent Var.:	College Enrollment in year			
	Pooled 07 to 09	2007	2008	2009
	(1)	(2)	(3)	(4)
1(PSU>=475) x q1	.131	.139	.100	.146
	(.008)***	(.014)***	(.015)***	(.012)***
1(PSU>=475) x q2	.111	.129	.089	.126
	(.013)***	(.025)***	(.022)***	(.021)***
1(PSU>=475) x q3	.100	.118	.108	.081
	(.017)***	(.029)***	(.028)***	(.029)***
1(PSU>=475) x q4	.032	-.0008	.017	.074
	(.019)*	(.030)	(.036)	(.034)**
1(PSU>=475) x q5	.021	-.079	.050	.044
	(.022)	(.044)*	(.029)*	(.045)
q1	.105	.079	.126	.111
	(.004)***	(.007)***	(.008)***	(.007)***
q2	.149	.102	.171	.156
	(.008)***	(.014)***	(.014)***	(.013)***
q3	.182	.116	.184	.229
	(.011)***	(.018)***	(.019)***	(.019)***
q4	.237	.171	.272	.270
	(.014)***	(.022)***	(.026)***	(.023)***
q5	.248	.219	.220	.334
	(.015)***	(.036)***	(.019)***	(.032)***
Obs.	74297	20537	24970	28790
R^2	.25	.214	.246	.283
Quintile	PANEL B: Relative increase			
q1	125%	176%	79%	132%
q2	74%	126%	52%	81%
q3	55%	102%	59%	35%
q4	14%	0%	6%	27%
q5	8%	-36%	23%	13%

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

7.7 Validity Checks

Are colleges choosing by financing status?

Table 13: College Enrollment for students in Quintile 5. $w = 44$

Dependent Var.:	5th Quintile College Enrollment in year:			
	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
1(PSU>=1)	.031	-.091	.042	.085
	(.022)	(.058)	(.030)	(.044)*
Obs.	6959	2044	3422	1493
R^2	.016	.026	.022	.00004

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 14: Applications to traditional colleges and placement conditional on application, around the cutoff. $w = 44$

	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
		PANEL A		
Dependent Variable:		Application to traditional universities		
1(PSU>=475)	.334	.175	.369	.463
	(.010)***	(.017)***	(.018)***	(.017)***
Const.	.563	.734	.538	.414
	(.009)***	(.016)***	(.017)***	(.015)***
Obs.	30653	10074	9830	10749
R^2	.486	.431	.512	.531
		PANEL B		
Dependent Variable:		Placement conditional on application		
1(PSU>=475)	-.036	-.027	-.040	-.042
	(.023)	(.031)	(.036)	(.038)
Const.	.544	.501	.592	.558
	(.019)***	(.028)***	(.034)***	(.036)***
Obs.	23339	7996	7506	7837
R^2	.004	.004	.003	.005

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%
Panel C considers the percentage of programs that have cutoffs below 475 for all students in the treatment group when the window considered is 2 PSU points ($PSU_i \in [475, 477)$), i.e. programs that are available for students in the control group $PSU_i \in [473, 475)$

Table 15: RD eliminating all students that enrolled in programs that were not available for students below the cutoff. $w = 2$

PANEL A: Enrollment in programs with cutoff below 475				
Dependent Var.:	College Enrollment in year:			
	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
1(PSU>=475)	.139	.121	.163	.161
	(.028)***	(.052)**	(.052)***	(.043)***
Const.	.133	.079	.175	.115
	(.021)***	(.041)*	(.039)***	(.034)***
Control Mean Enrollment	.160	.128	.193	.154
Increase w/r Control Enr.	87%	95%	84%	105%
Obs.	3244	966	1067	1211
R^2	.022	.018	.027	.026
PANEL B: % of programs with cutoff below 475				
% of programs with cutoffs below 475		95.38%	98.02%	92.53%

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

7.8 Twins tables

Table 16: Probability of college enrollment using twins fixed effects. Year 2007 to 2009. Only first time takers. Different Specifications.

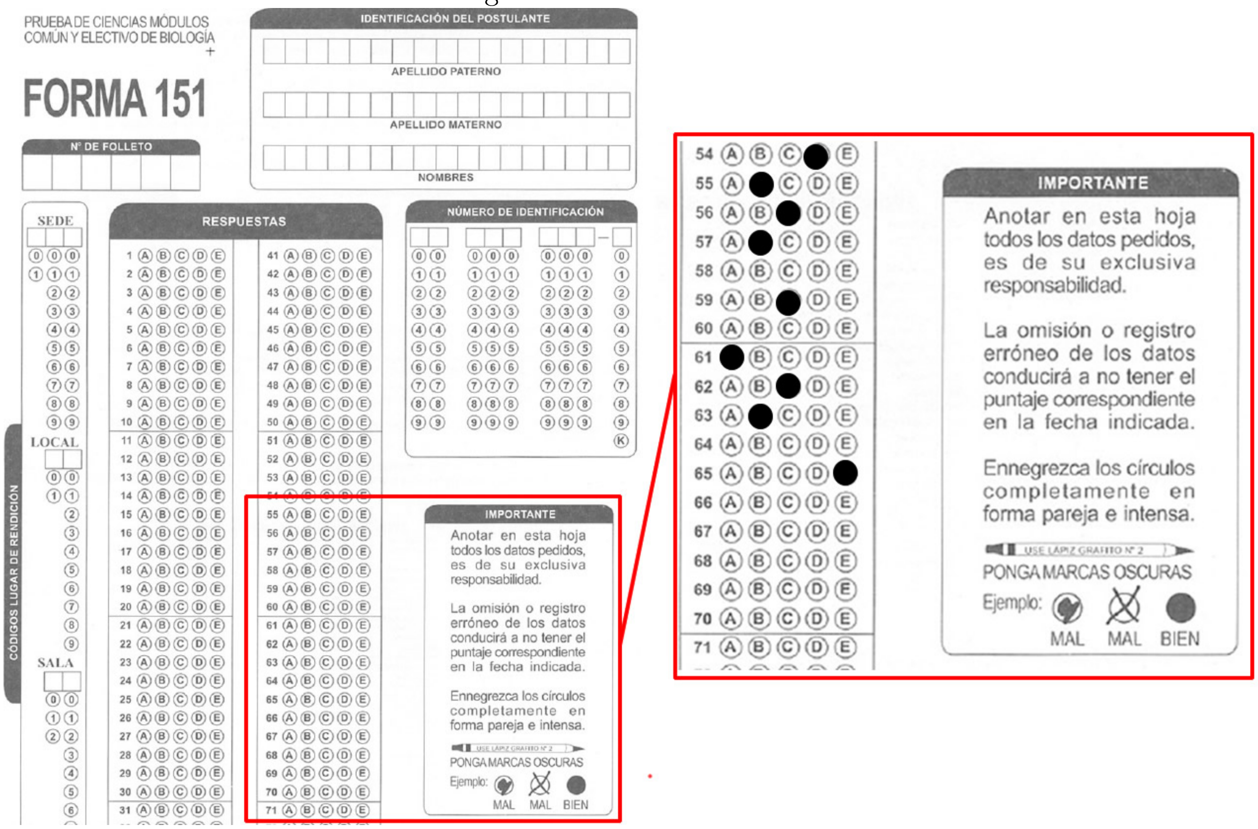
	Window 50 pts	Window 75 pts	Window 100 pts	Window 125 pts	Window 150 pts
	(1)	(2)	(3)	(4)	(5)
Specification: Linear					
1(PSU>475)	.330	.282	.258	.236	.263
	(.086)***	(.066)***	(.061)***	(.059)***	(.057)***
R^2	.106	.13	.142	.173	.18
Specification: Quadratic polynomial					
1(PSU>475)	.300	.229	.197	.183	.219
	(.102)***	(.082)***	(.077)**	(.076)**	(.075)***
R^2	.113	.133	.147	.177	.183
Specification: 3rd order polynomial					
1(PSU>475)	.319	.286	.250	.234	.266
	(.127)**	(.104)***	(.098)**	(.096)**	(.095)***
R^2	.113	.135	.148	.178	.184
Specification: 4th order polynomial					
1(PSU>475)	.301	.272	.324	.266	.305
	(.159)*	(.131)**	(.121)***	(.119)**	(.116)***
R^2	.114	.136	.151	.179	.185
Only Same Sex	N	N	N	N	N
Only Same School	N	N	N	N	N
Obs.	609	777	861	922	962

Table 17: Probability of college enrollment using twins fixed effects. Year 2007 to 2009. Only first time takers. Different specifications. Only twins from same sex and attending the same school.

	Window 50 pts	Window 75 pts	Window 100 pts	Window 125 pts	Window 150 pts
	(1)	(2)	(3)	(4)	(5)
Specification: Linear					
1(PSU>475)	.307	.270	.239	.221	.238
	(.094)***	(.072)***	(.067)***	(.067)***	(.066)***
HSGPA	-.0002	.0003	.0003	.0005	.0004
	(.0006)	(.0005)	(.0005)	(.0005)	(.0005)
R^2	.093	.111	.123	.133	.128
Specification: Quadratic polynomial					
1(PSU>475)	.300	.236	.189	.199	.235
	(.111)***	(.091)***	(.086)**	(.087)**	(.086)***
HSGPA	-.0002	.0002	.0003	.0004	.0004
	(.0006)	(.0005)	(.0005)	(.0005)	(.0005)
R^2	.101	.113	.126	.134	.128
Specification: 3rd order polynomial					
1(PSU>475)	.351	.324	.274	.317	.358
	(.138)**	(.115)***	(.109)**	(.110)***	(.108)***
HSGPA	-.0001	.0003	.0003	.0005	.0005
	(.0006)	(.0005)	(.0005)	(.0005)	(.0005)
R^2	.103	.118	.131	.143	.138
Specification: 4th order polynomial					
1(PSU>475)	.301	.315	.363	.394	.454
	(.170)*	(.144)**	(.133)***	(.134)***	(.131)***
HSGPA	-.00009	.0003	.0003	.0005	.0005
	(.0006)	(.0005)	(.0005)	(.0005)	(.0005)
R^2	.108	.119	.135	.145	.142
Only Same Sex	Y	Y	Y	Y	Y
Only Same School	Y	Y	Y	Y	Y
Obs.	481	603	660	675	691

8 Figures

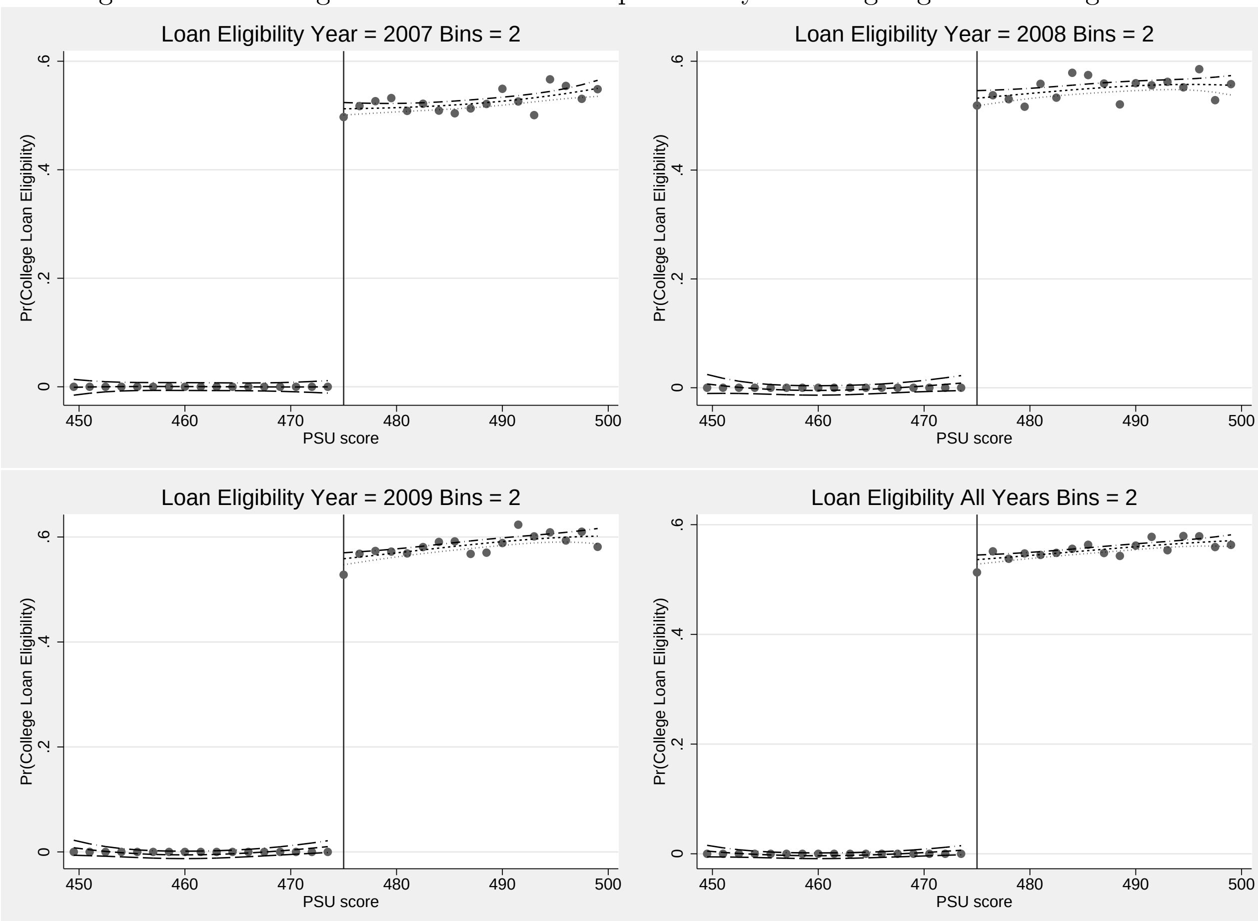
Figure 1: PSU Answer sheet



Note: To answer the PSU test students need to bold the circle with the correct answer. Optical devices grade this sheets.

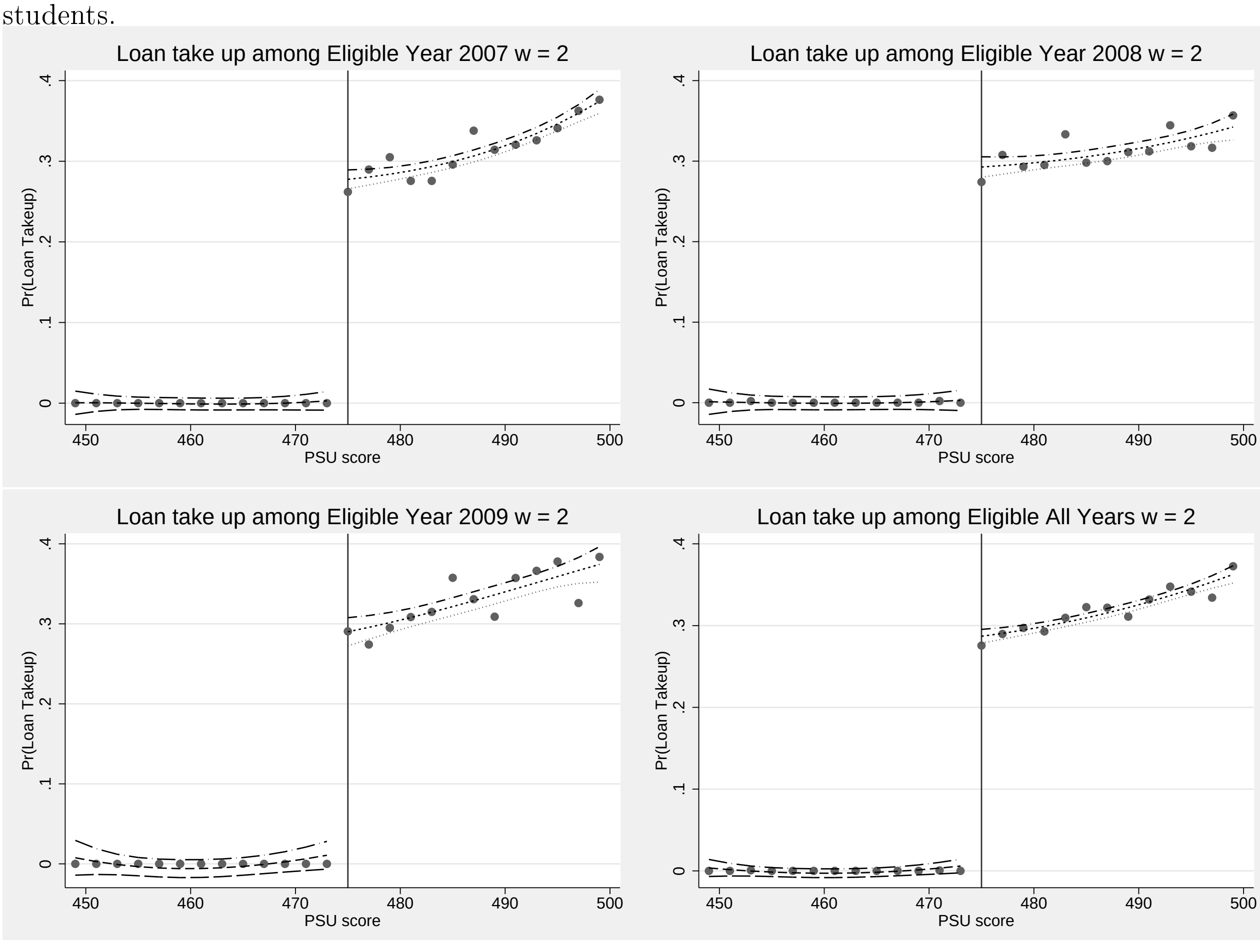
8.1 Loan Assignment.

Figure 2: Loan assignment. Unconditional probability for being eligible to College Loans.



Note: Each dot indicates average eligibility of students with scores in an interval of 2 PSU points (all students included). On average each dot contains 1,500 students (approximately 38,000 students per graph). The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475). These graphs show a window of 50 points around the discontinuity to stress the magnitude of the jump.

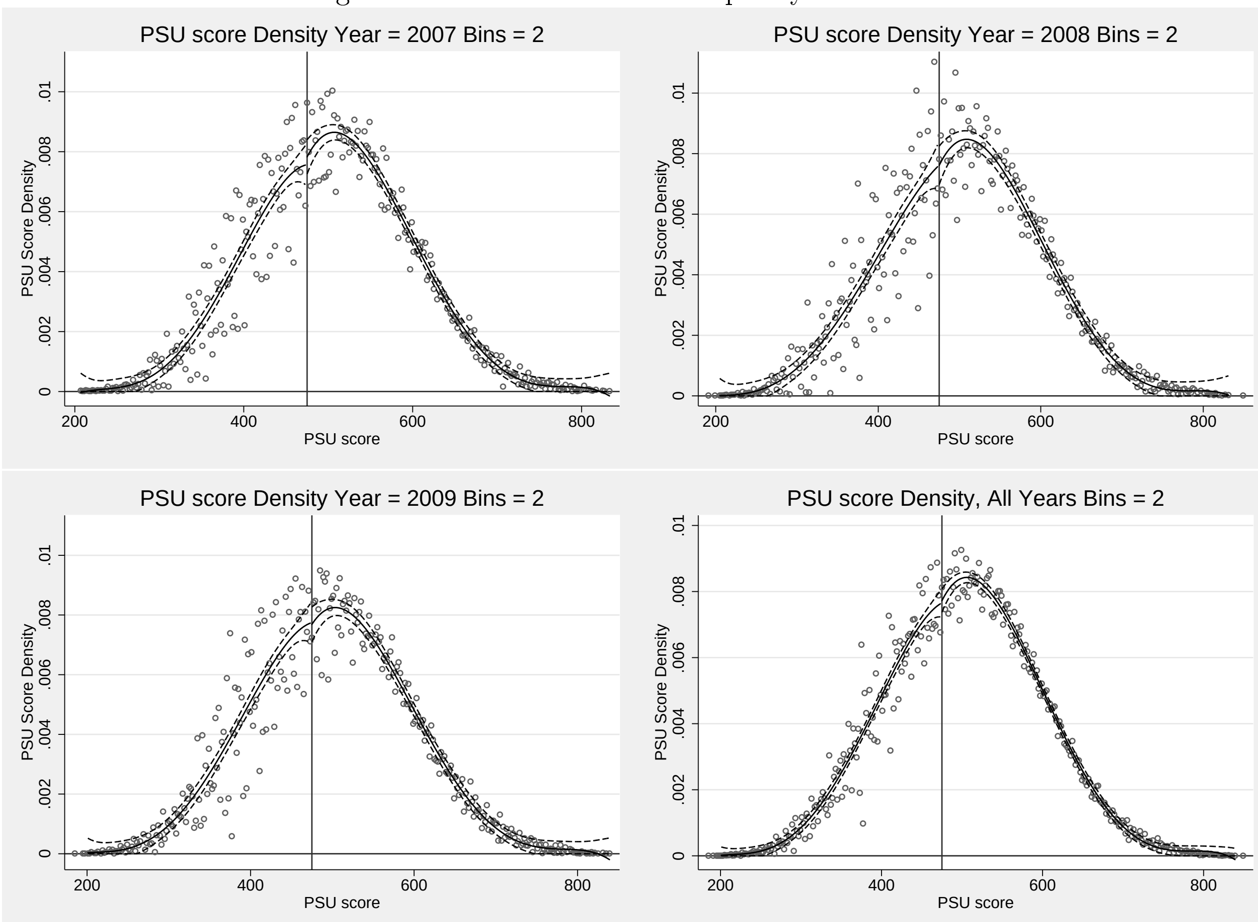
Figure 3: Loan take up. Probability of taking up a college tuition loan among preselected eligible students.



Note: Each dot represents average loan take-up relative to eligible students, in an interval of 2 PSU points. On average each dot contains 441 students fulfilling all the requirements to be eligible for college loans. The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475). These graphs show a window of 50 points around the discontinuity to stress the magnitude of the jump.

8.2 No Manipulation of the running variable.

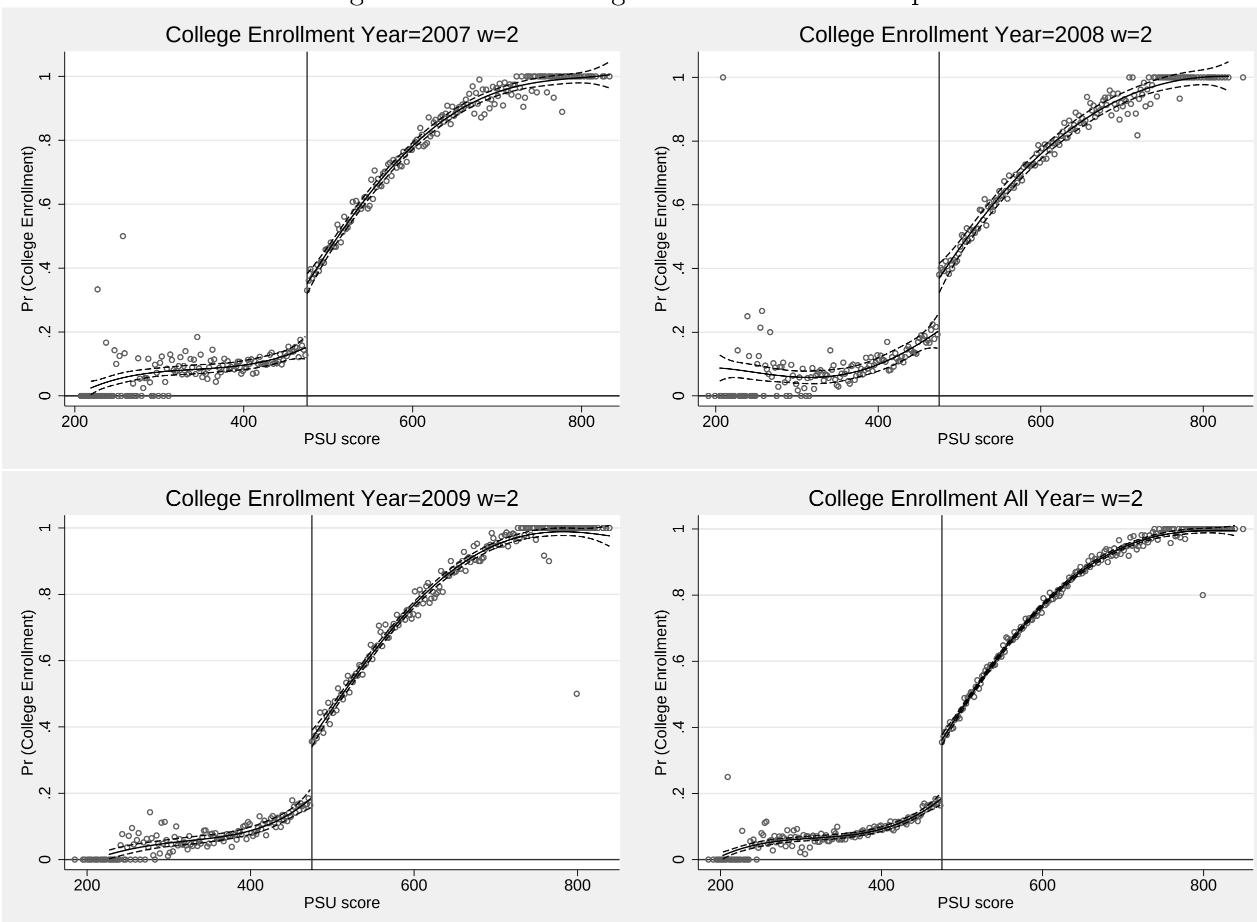
Figure 4: RD for PSU scores frequency distribution.



Note: Each dot represents the density of PSU scores in an interval of 2 points. The sample considers only students who satisfy all requirements to be eligible for college loans and take the PSU immediately after graduating from high school.

8.3 Effect on College Enrollment

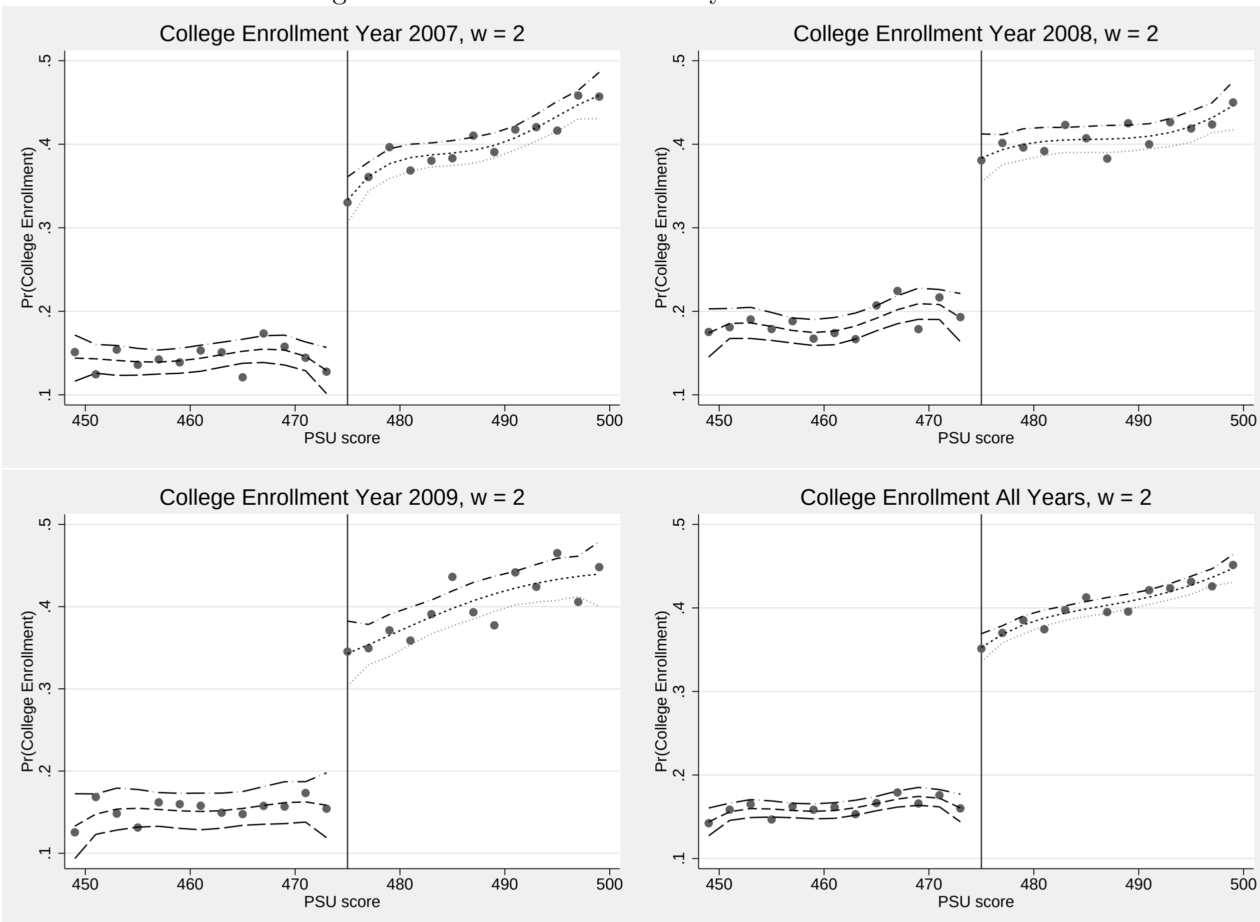
Figure 5: RD for College enrollment. Full sample.



Note: Each dot represents average college enrollment in an interval of 2 PSU points.
The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side.
The vertical line indicates the cutoff (475).

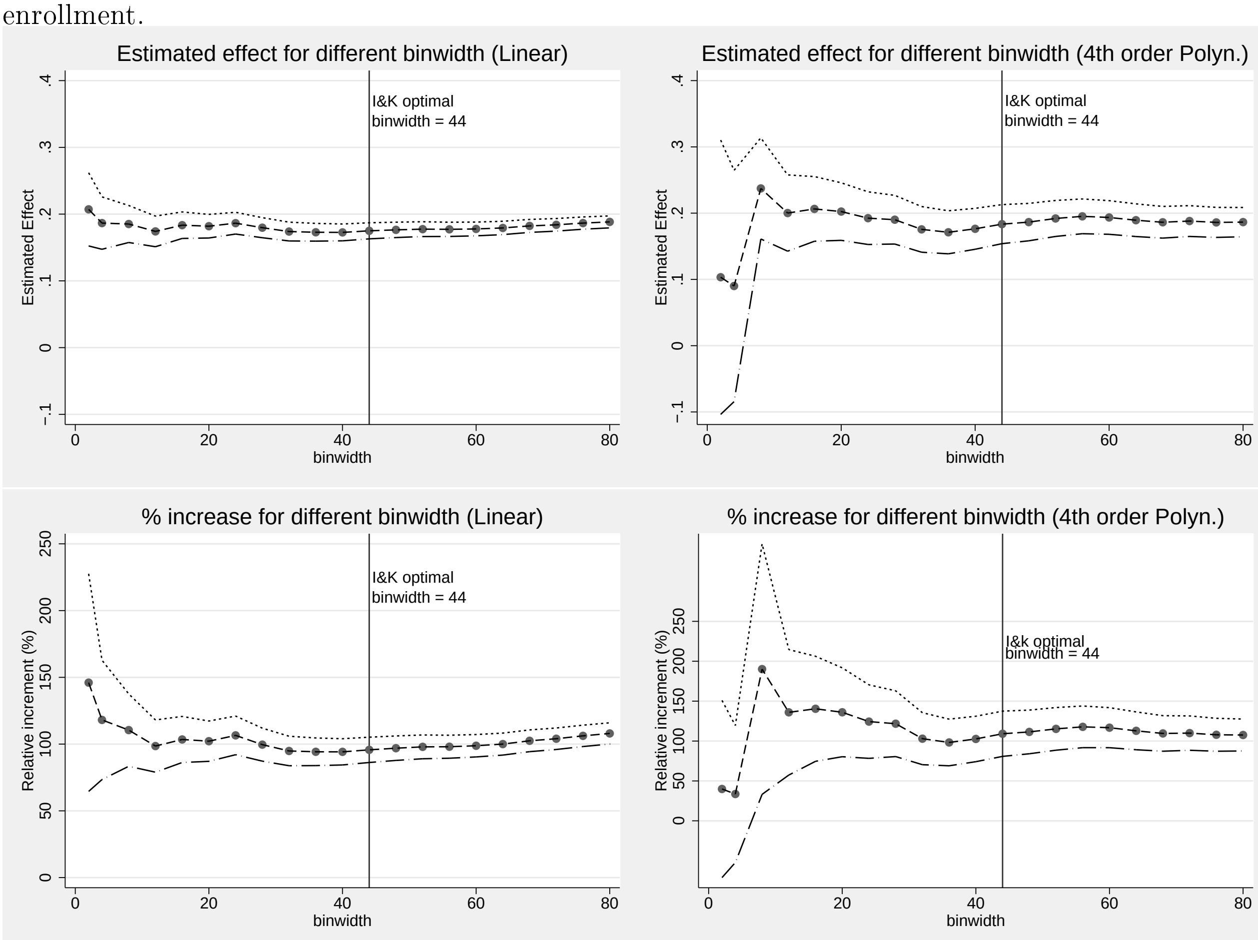
These graphs show the full sample of students fulfilling all requirements to be eligible for college loans and taking the PSU immediately after graduating from high school.

Figure 6: Enrollment Probability around the cutoff.



Note: Each dot represents average college enrollment in an interval of 2 PSU points. Each dot have on average 441 students who satisfy all requirements to be eligible for college loans and take the PSU immediately after graduating from high school. The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475). These graphs show a window of 50 points around the discontinuity to stress the magnitude of the jump.

Figure 7: Comparison of different bandwidth in the estimation of the effect of loan access on college enrollment.



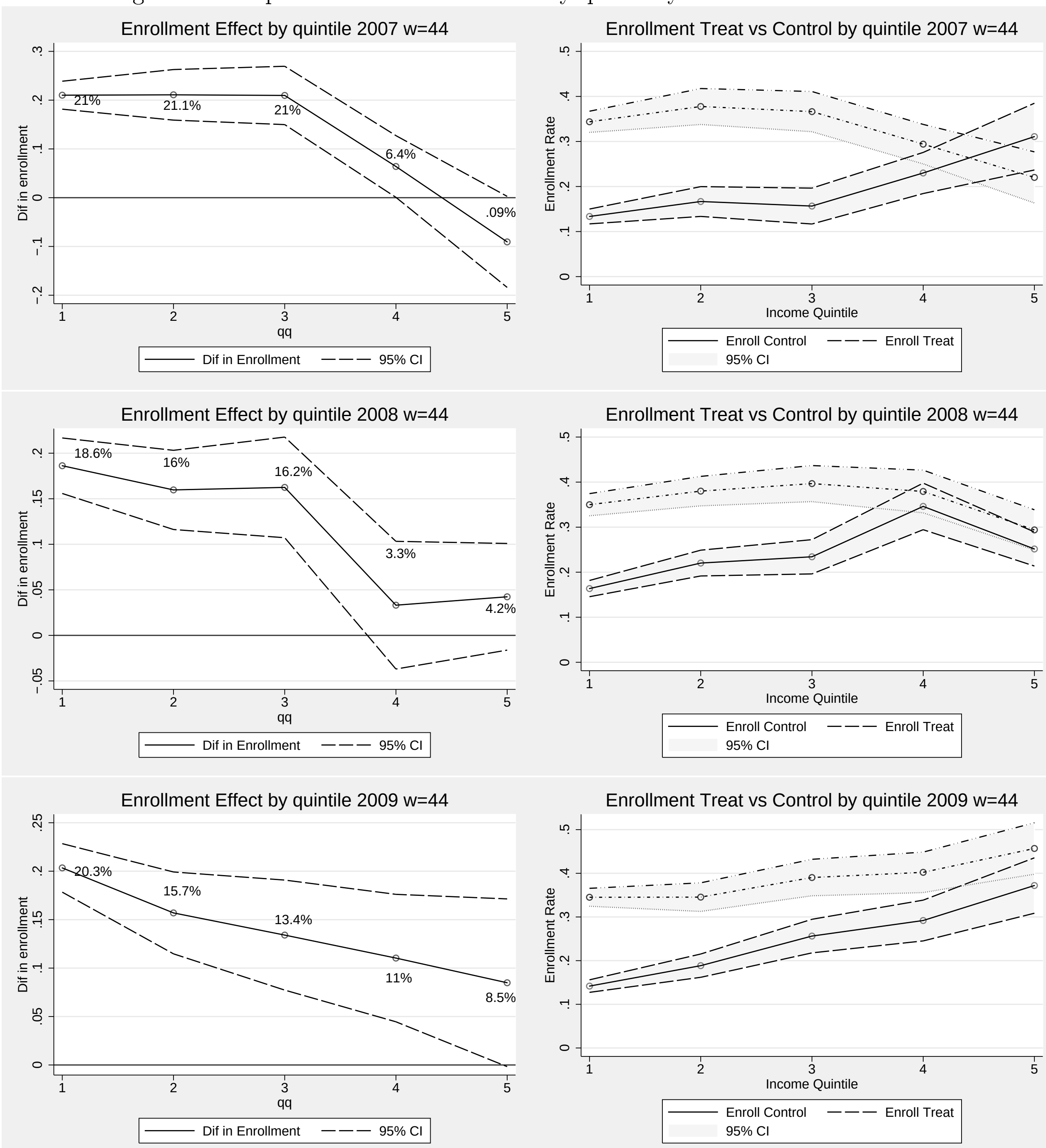
Note: The graphs on the top show the RD estimation of the effect of being eligible for loans on college enrollment using different bandwiths and 95% confidence intervals constructed using robust standard errors.
The graph on the bottom show the relative increase in enrollment:

$$(\lim_{T\downarrow\tau} \Delta Enrollment)/(\lim_{T\uparrow\tau} Enrollment) = \beta_1/\beta_0$$

Where β_0 is the enrollment rate for students without access to loans at the cutoff and β_1 the loans access effect (see equation (2)), and 95% confidence interval using delta method standard errors.
"I&K optimal bandwidth" refers to the optimal bandwidth $w = 44$, estimated using Imbens and Kalyanaraman (2009).

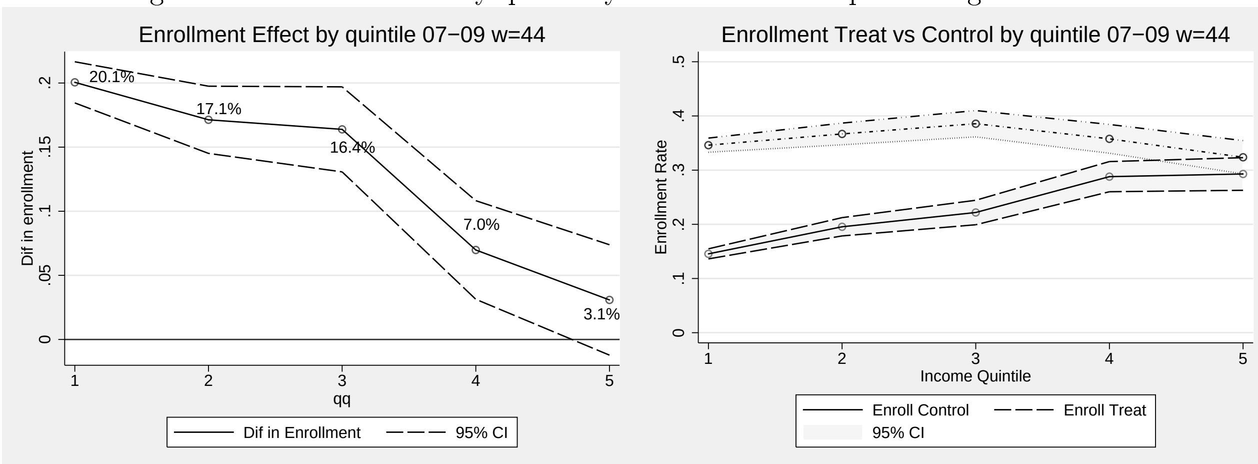
8.4 College Enrollment Gap by family income

Figure 8: Comparison in enrollment rate by quintile years 2007 to 2009. $w = 44$



Note: On the left, each point represents the effect of access to college loans on enrollment by income quintile (and 95% confidence intervals from robust standard errors) for each year of the sample. The graphs on the right show the estimation of the enrollment rate at each side of the cutoff by income quintile (and 95% confidence interval).

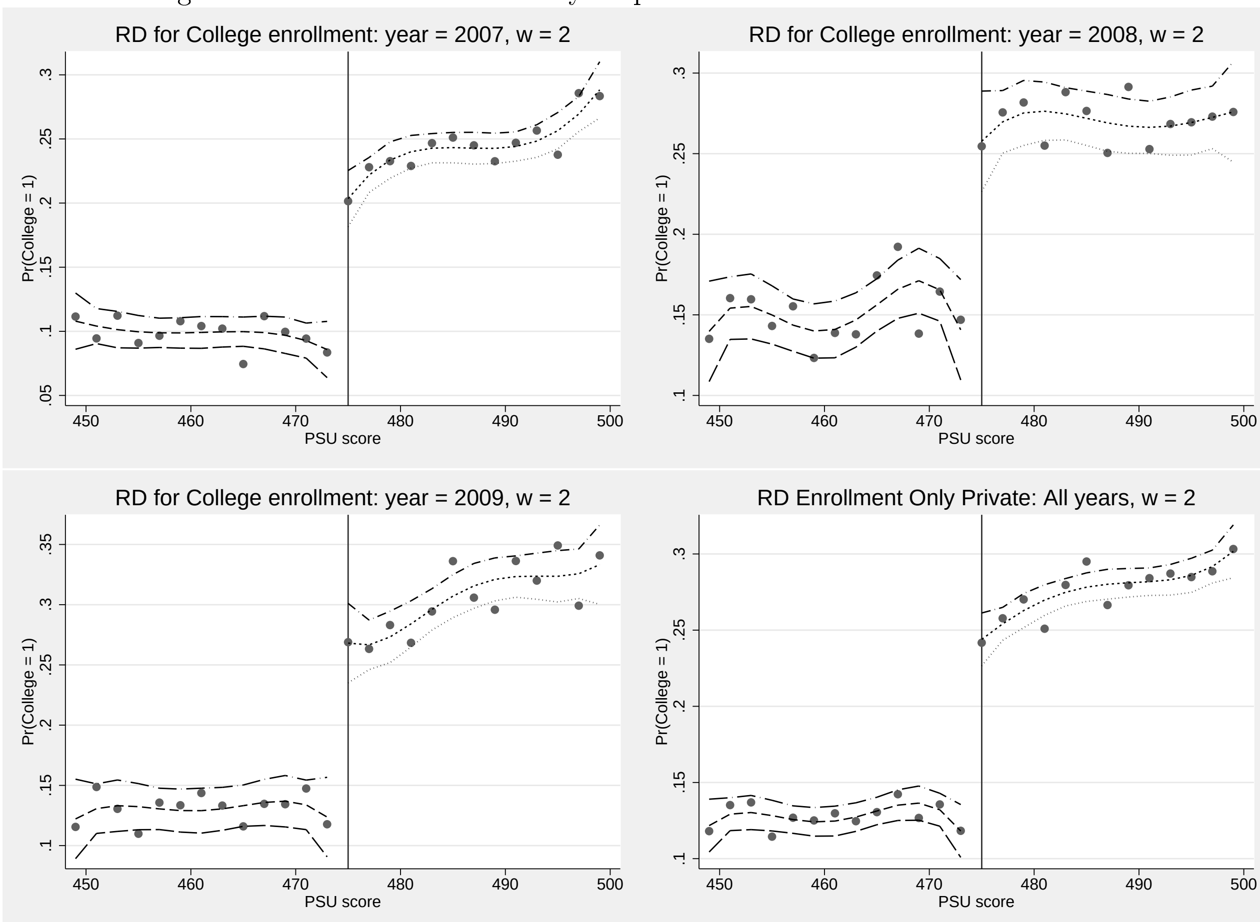
Figure 9: Enrollment rate by quintile years 2007 to 2009 pooled together. $w = 44$



Note: On the left, each point represents the effect of access to college loans on enrollment by income quintile (and 95% confidence intervals from robust standard errors) for all years of the sample pooled together. The graphs on the right show the estimation of the enrollment rate at each side of the cutoff by income quintile (and 95% confidence interval).

8.5 Price vs. access effects

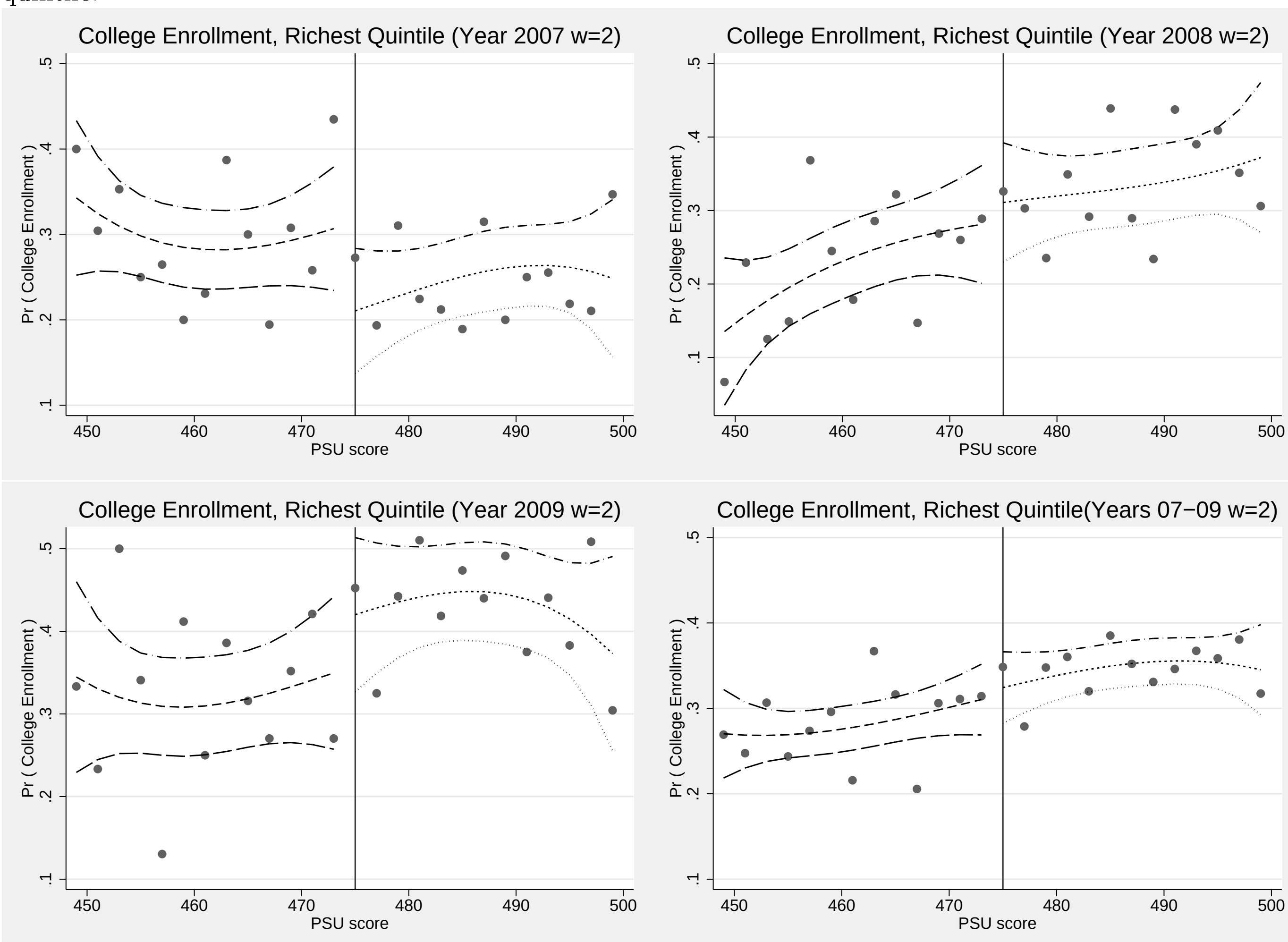
Figure 10: Enrollment Probability on private universities around the cutoff.



Note: Each dot represents average college enrollment in private universities in an interval of 2 PSU points. Each dot have on average 376 students who satisfy all requirements to be eligible for college loans and take the PSU immediately after graduating from high school. The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475). These graphs show a window of 50 points around the discontinuity to stress the magnitude of the jump.

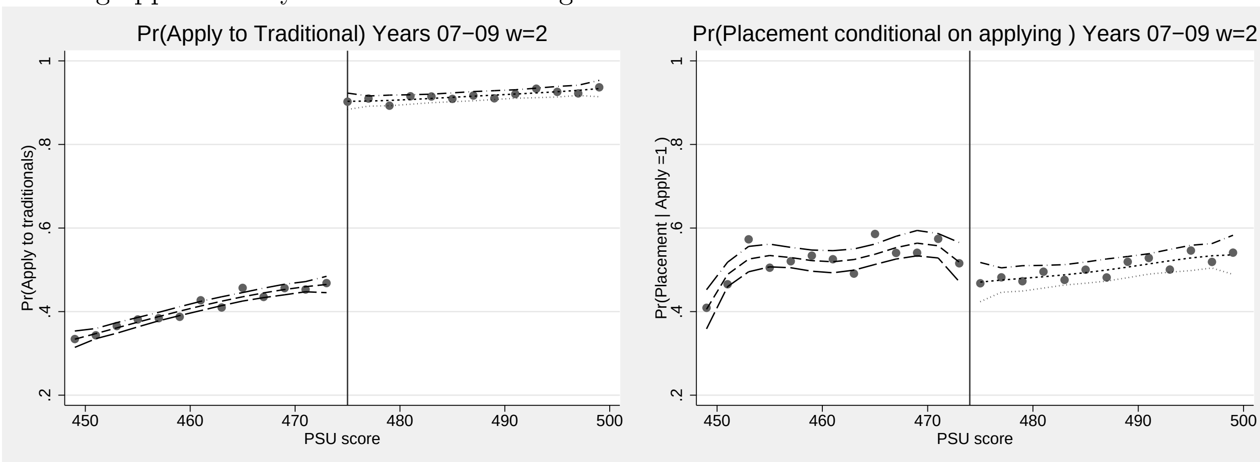
8.6 Validity Checks

Figure 11: Probability of college enrollment around the cutoff for students from the highest income quintile.



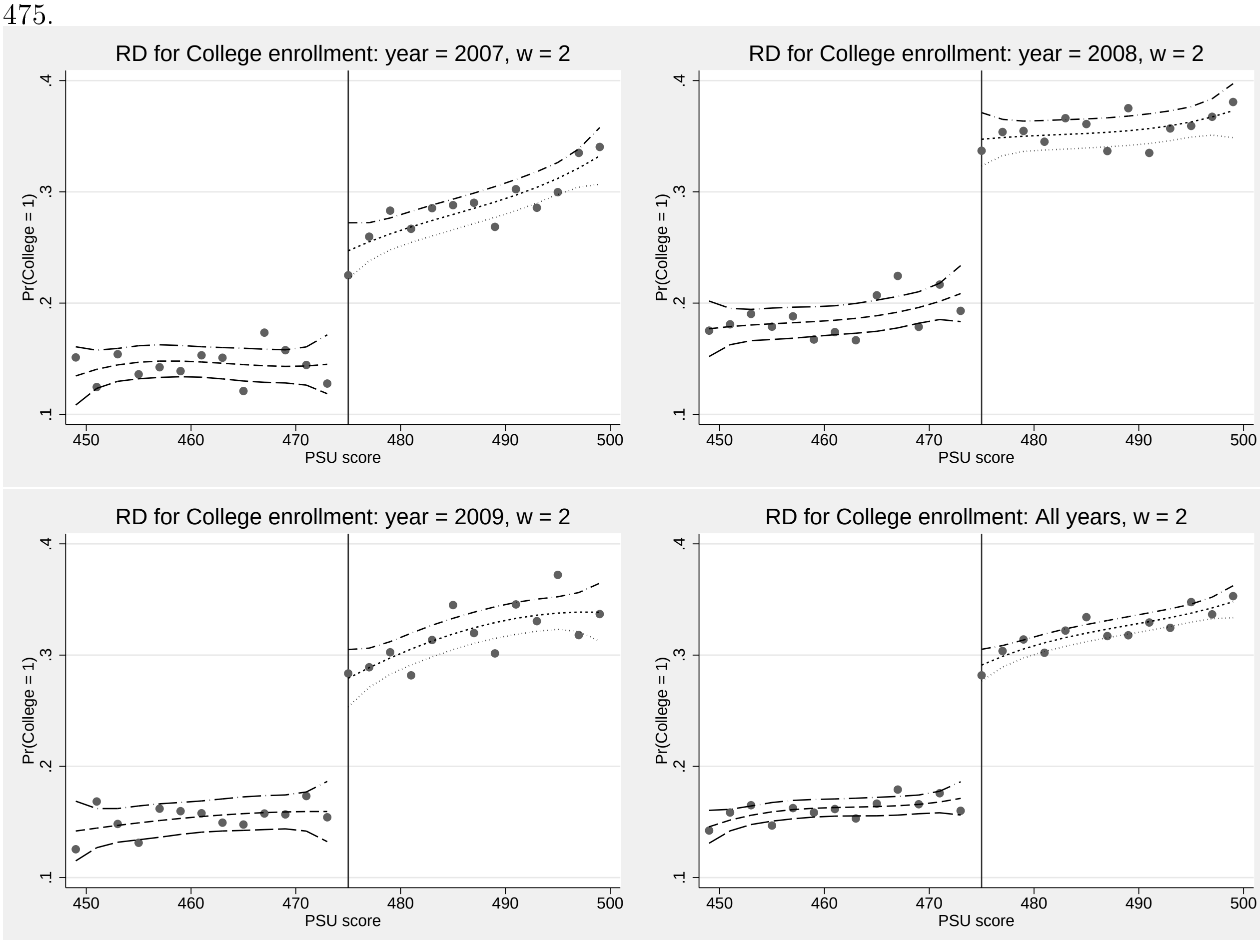
Note: Each dot represents average college enrollment for students in the fifth quintile in intervals of 2 PSU points. The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475).

Figure 12: RD for application to traditional universities and placement in a traditional conditional on being applied. All years from 2007 through 2009.



Note: Each dot represents average application to traditional universities and placement in traditional univrsities conditional of having applied in a interval of 2 PSU points. The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475).

Figure 13: RD for college enrollment without considering enrolled students with program cutoffs below 475.



Note: Each dot represents average college enrollment for students in an interval of 2 PSU points. Dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475).

The sample exclude students enrolled in programs where the last enrolled students had a score above the cutoff, thus, the program was not available for students below the threshold.