Credit Access and College Enrollment*

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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 who score just above the cutoff have nearly 20 percentage points higher enrollment than students who score just below the cutoff, which represent a 100% increase in the enrollment rate. More importantly, 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 20 percentage points higher enrollment rates in the second and third years of college around the cutoff score, representing relative increases of 213% and 446% respectively, and also eliminating the enrollment gap between the richest and poorest income quintiles. These findings suggest that differential access to credit is an important factor behind the intergenerational transmission of education and income.

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1 Introduction

Students from richer families are more likely to attend, persist at, and graduate from 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 [2001]; 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., Lang [1993]; Kane [1994, 1996]; Card [1999]; Belley and Lochner [2007]; Lochner and Monge-Naranjo [2011a]; and Brown, Scholz and Seshadri [2012]).[1]

Measuring the effects of credit constraints on college enrollment is a difficult task because determining whether a family has 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 to credit, and that affect college enrollment, leading to biased estimates.[2] 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 unobserved by the econometrician. Moreover, Van der Klauuw (2002) 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 used to assist students from low income families. In addition, the admission process relies on unobserved and subjective measures, such as recommendation letters, parental alumni status, etc. Recognizing the problem, tests of the credit constraint hypothesis have relied mainly on indirect measures of credit access (Cameron and Taber [2004]) 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

[1]See Lochner and Monge-Naranjo (2011b) for a detailed review of the literature.

[2]This econometric problem has also been documented in the literature that estimate the price elasticity of demand for college education (e.g. Manski and Wise [1983], McPherson and Schapiro [1991], Van der Klaauw [2002], Dynarski [2003] and Nielsen, Sorensen and Taber [2010]).

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 selection, and allows for a direct and unbiased estimate of the causal effect of credit access on college enrollment and college progress.[3]

A key feature of my analysis is the availability of a detailed student-level data that present 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, preferences, etc.). 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. Third, the two loan programs provide access to *standardized loans* (i.e. that do not depend on family characteristics) to eligible students, offered by the government and private banks, eliminating potential endogeneity of loan offers designed to attract better students. The nature of the loan programs, that gives credit access as good as randomly around the threshold, the admission system characteristics, and the availability of these data, allow a reliable evaluation of the causal effects of credit access on college enrollment and college progress.

My analysis shows that access to the loan programs 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 threshold. 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 cutoff).

More importantly, access to the loan programs appears to eliminate the relatively large income gradient in college enrollment. Among those who are barely ineligible for loans, students from the richest quintiles are twice as likely to enroll as students from the poorest quintile. Among students who are barely eligible, the enrollment gap is statistically zero.

[3] In terms of the methodology, Canton and Blom (2010) and Gurgand, Lorenceau and Melonio (2011) perform an RDD analysis using information on Mexican and South African students.

The literature has focused mainly on college enrollment, but programs that promote enrollment would not have any significant effect on educational attainment if they attract students who are unable to graduate. For this reason, a different strand of literature examines the impact of aid on persistence, dropout and graduation rates (e.g. Dynarski [2003]; DesJardins, Ahlburg and McCall [2002]; Bettinger [2004]; Singell [2004]; and Stinebricker and Stinebrickner [2008]), with a similar level of disagreement on the conclusions.[4]

This strand of the literature faces similar econometric problems. Enrolled students constitute a self-selected sample of individuals, and therefore the analysis on the effects of credit constraints on persistence and dropout rates may not lead to causal estimations. Furthermore, in most cases, the analysis is performed using information from a single institution or restricted group of institutions. That implies two more concerns. First, the analysis depends critically on the characteristics of the analyzed institution. Second, in many cases, transferred students are mistakenly considered dropouts.

I use the same exogenous variation on access to credit markets to estimate the causal effect on college progress, defined as enrollment in the second and the third year. Moreover, using the population of students graduating from high school in a given year eliminates the selection bias in the analysis of college progress, and using all institutions eliminates the bias associated with transferred students and presents general evidence not contingent on one institution. To the best of my knowledge, this is the first paper that uses an exogenous source of access to loans and the entire population of students and institutions that participate in the college admission process.

In this context, my estimates indicate that the causal effect of access to credit markets on college progress is much greater than suggested by previous studies. For each student who enrolls in second year of college without access to credit, 3.1 enroll in the second year when access to credit is available. Moreover, for every student who enrolls in the third year of college without access to credit markets, 5.5 do so when they have access to loans.

Additionally, access to the loan programs eliminates the income gradient in second and third year college enrollment. Among those barely ineligible for loans, students from the poorest income

[4]See Chen (2008) and Hossler et al (2009) for a survey of the literature.

quintile enroll at 6% and 3% in the second and third years respectively, while students from the richest quintile enroll at 20% and 24% respectively. On the contrary, among those barely eligible for loans, there is no statistical difference in the enrollment rate in the second and third year between the richest and the poorest students.

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" (Dynarski [2003]; and Lochner and Monge-Naranjo [2011a]). I address this concern by estimating the price effect directly, using exogenous access to a grant program that gives access to dramatically reduced tuition costs. Additionally, I present a couple of tests that use the differences in interest rates and enforceability of the two loan programs to decompose the enrollment effect into price and access effects. I find that the price effect is small and conclude that the overall effect is driven by credit access.

The paper is organized as follows. Section 2 discusses the empirical strategy. Section 3 describes the background and the data. Section 4 presents the empirical evidence for the effects of credit access on college enrollment and progress, and the enrollment gap by family income. Section 5 presents the decomposition of the effect into access and price effects. Section 6 shows additional validity checks and section 7 concludes.

2 Empirical Strategy

Two financing programs in Chile offer college tuition loans to students who: first, complete a socioeconomic verification form before taking the College Admission Test (Prueba de Selección Universitaria, hereafter PSU); second, are classified in one of the poorest four income quintiles by the tax authority; and third, score at least 475 points on the PSU test.

This last requirement enables a sharp regression discontinuity design. Students receive access to loans as good as randomized around the cutoff (Lee, 2008) and, therefore comparing college enrollment rates for the group at or just above the cutoff (the "treatment" group) and the group just below (the "control" group) gives the causal effect of credit access on college enrollment.

Hahn, Todd and Van der Klaauw (2001), Van der Klaauw (2008), Lee (2008), and Lee and

Lemieux (2010) describe the conditions under which a RDD gives a causal estimation. The intuition is simple. If we assume that each individual's score (the running or assignment variable) has a random component with a continuous density, then the probability of being ϵ above the cutoff or ϵ below is the same (for a sufficiently small ϵ). Therefore, even though the score depends on the individual characteristics (selection), being eligible for treatment in this small neighborhood of the cutoff is as good as random assignment. Students barely below the cutoff can be used as a counterfactual to students at or barely above the cutoff, because the only difference between these two groups is that students at or above the cutoff receive the treatment.

Ideally, we would compare the average outcome for students at a small neighborhood of the threshold, but usually there is not enough data in this small vicinity, and thus the estimation suffers from small sample bias. Lee and Lemieux (2010) suggest the following equation as an equivalent specification to estimate the RDD.

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

Where $\mathbf{1}(T_i \geqslant \tau)$ is an indicator function for whether the student i's PSU score T_i is equal to or greater than the eligibility threshold τ; the term $(T_i - \tau)$ accounts for the influence of the running variable on Y_i in a flexible nonlinear function $f(\cdot)$; and ξ_i is, a mean zero error. The parameter β_0 captures the expected value of Y_i for students barely below the cutoff and β_1 captures the increase in the expected value of Y_i for individuals ϵ above the cutoff.

Equation (1) allows using students who are not necessarily close to the cutoff. The advantage is the increased statistical power due to adding more data to the estimation. The disadvantage is the bias produced by individuals who are farther from the cutoff when f is not correctly specified. Imbens and Kalyanaraman (2012) propose a method to calculate an asymptotically optimal bandwidth to use a local linear regression in equation (1), where they use a squared error loss function to weigh these two biases.

The results shown in this paper are based on a local linear regression using the optimal bandwidth of Imbens and Kalyanaraman, which in this case gives a bandwidth of 44 PSU points around the cutoff ($w^* = 44$). Nevertheless, the results are highly robust to different bandwidths and functional specifications.

Moreover, the assumption of a continuous density for the random component of the assignment variable is directly testable. First, if the probability of being at either side of the cutoff is the same for each individual, we should not observe individuals bunching at one side of the cutoff (McCrary, 2008). Secondly, if the random component is truly random at the cutoff, students in a small neighborhood of the threshold should have the same baseline characteristics; equation (1) can be used to perform this test.

2.1 Enrollment in Second and Third year

One concern from the policy maker's perspective is that access to loans may have 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. Hence, it is not sufficient to observe an effect in the first year enrollment rate to reduce the education attainment gap.

I estimate the causal effects of access to credit markets on enrollment in second and third year of college, using the same exogenous variation in access to loans. In this case, I deal with the problem of selection into treatment using a fuzzy RDD.

In the previous case, eligibility for loans was determined sharply by the score in one PSU attempt. For second and third year enrollment, students have the chance to retake the PSU test once or twice respectively (since the test is written once a year). Non-eligible students may enroll in college for the first year, expecting that they can get access to loans to enroll in the second or third year if they score at least 475 in subsequent attempts, thus self-selecting into treatment. For this case, eligibility is not fully determined by the score of the first attempt (the probability of being eligible for loans for second and third year enrollment is not zero for the control group). Nevertheless, the probability of being eligible still jumps discontinuously at the threshold, because not all students who scored below 475 in their first attempt retake the test, and only a portion of those succeed in scoring 475 or more in subsequent attempts. This allows a fuzzy RDD, where eligibility in the second and third year is instrumented by being eligible in the first, i.e. a dummy for scoring greater than or equal to 475 in the first year.

Specifically, I perform a two stage least square regression as follows:

$$Y_i = \beta_0 + \beta_1 \cdot Elig_i + \beta_2 \cdot f(T_i - \tau) + \nu_i \tag{2}$$

$$Elig_i = \gamma_0 + \gamma_1 \cdot \mathbf{1}(T_i \geqslant \tau) + \gamma_2 \cdot f(T_i - \tau) + \eta_i \tag{3}$$

Where Y_i corresponds to the outcome of interest: enrollment in the second year, enrollment in the third year, or dropout status. $Elig_i$ takes on the value 1 if student i is eligible for college loans in the year of analysis, and zero otherwise.

The term $\mathbf{1}(T_i \geqslant \tau)$, the indicator function for scoring greater than or equal to the cutoff in the first attempt, is used as instrument for being eligible for loans. All the other variables are defined as in equation (1).

Now, the parameter β_1 measures the effect of having access to college loans on enrollment in the second and third year for those for whom the treatment status does not change in the following years, after taking the PSU test for the first time.

3 Background and Data

One key feature of this paper is the possibility to observe every aspect of a partially centralized college admission process and the entire population of students graduating from high school that participate in the admission process nationwide. The admission process is based on the PSU Test,[5] which is taken by 96% of all students graduating from high school each year.[6] 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. The test is taken simultaneously across the country only once a

[5]The PSU was created in 1966 and was known as PAA (Prueba de Aptitud Académica: academic aptitude test) before 2003.

[6]Source: Ministry of Education.

year, and can be taken as many times as wanted after paying a fee.[7]

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 college placement and for loans and grants eligibility.[8]

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). Therefore, it is not subject to manipulation by students or graders. PSU scores are normalized to a distribution with mean 500 and standard deviation of 100 to make them comparable between years. The scores range from 150 to 850 points.

The PSU test is implemented by the Council of Chancellors of Chilean Universities (Consejo de Rectores de las Universidades Chilenas: CRUCH), which organizes the traditional universities that are as described below.

The first data source in this paper is the registry of students who enroll for the PSU test. It contains individual data on PSU scores, high school GPA, which determine placement in universities; and a rich set of socioeconomic characteristics, such as self-reported family income, parent education, school of graduation, etc. It includes PSU test data sets for the years 2006 to 2009.

Once students know their PSU scores, they can apply to two types of universities, called "traditional" and "private". The "traditional" universities correspond to 25 institutions that were founded before the educational reform of 1981. Some are public and others are privately funded, but all receive direct funding from the government (Aporte Fiscal Directo).

The 33 universities created after the reform of 1981 are called "private" universities. They do not receive direct funding from the government, and before 2006, their students were excluded from the credit system with public funds. Their growth has been rapid and steady, increasing enrollment from a handful of students in 1991 to nearly half of the student body in 2009.

Both types of universities use the PSU test score to select students. Traditional universities

[7] The fee is about $50 or CLP 24,000 in 2010 and is waived for all students graduating from public and voucher schools who apply for a waiver.

[8] The optional tests are (1) History and Social Sciences and (2) Sciences, which includes modules on biology, chemistry, and physics. They are not considered for loan eligibility, but they are considered in the placement score that is a factor in admission to college programs.

use the test as a mechanism to centrally allocate applicants: After knowing their scores, students apply to up to 8 programs, and the allocation process ranks all the students applying to any given program using the PSU score and high school GPA. Seats are offered to the best scoring students in each program and the rest are put on a wait list. If a student is accepted into more than one program, she is placed in her highest preference and is eliminated from all other rankings.

Private universities receive applications independently, but they also select applicants considering the PSU test score and high school GPA. They prefer students with higher PSU scores for three reasons. First, to distinguish the quality of the students, it is the best ability measure available. Second, all the universities in the country compete to get indirect governmental funding (Aporte Fiscal Indirecto), which is calculated based on PSU scores from the students enrolled in each institution every year. This funding is the second source of earnings for private universities. Third, the PSU scores of the student body are used to publicize the quality of the programs to attract more students. Each year, before the PSU test, universities disclose the PSU score of the last student enrolled in each program (program cutoff score or puntaje de corte) to signal how much in demand they are.

The PSU data set described previously also contains information on the application preferences to traditional universities and the placement results from the centralized mechanism. After students know the results from the centralized placement process, they can enroll in the assigned program; wait for a spot in a program where they are wait listed, or simultaneously, apply to private universities, which select the best scoring applicants.

After the whole enrollment process is finished, universities inform the ministry about the enrollment in all their programs, and the ministry assigns loans, grants and scholarships.

The second source of data used in this paper is the enrollment in higher education data set from the Ministry of Education. It includes the enrollment outcome of the process described above (for all programs and institutions) for the period from 2006 through 2009. This data set is merged with the PSU data set using the national identification number, RUN (Rol Único Nacional).

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

Financial aid from the Education Ministry is assigned according to the information provided by students before the PSU test, in the economic status verification form (Formulario Único de Acreditación Socioeconómica, FUAS). This information is sent by the ministry to the Chilean tax authority (Servicio de Impuestos Internos or SII) to verify the information and classify students in income quintiles. Table 1 summarizes all college financial aid given or managed by the Ministry of Education. All aid is given to students based on their PSU scores (except for the Excellence Scholarship) and their income quintiles.

I use the FUAS application form data set as a third source of information. The key element in this data set is the income quintile reported by the tax authority that determines eligibility for the two loan programs and for six scholarship programs. Moreover this data set contains the assignment to benefits and take up for the traditional loan, which is described below. This information has been collected by the ministry since 2006, but I only have data from 2007 onward. The data is also merged at the individual level using the national id number RUN.

The last set of information used in this paper corresponds to loan take up for the State Guaranteed Loan Program from the INGRESA commission, the organization created to manage this credit program in 2006. As before, the data was merged using RUN.

As described in the previous section, scoring 475 or more in the PSU test determines eligibility for the group of students that have been preselected for two loan programs, and therefore determines access to credit markets. These programs are described here.

3.1 The loan programs

The two loan programs are (i) the Traditional Loan Program (Crédito Solidario) and (ii) the State Guaranteed Loan Program. The same eligibility criteria are used in both programs, namely, students are required to complete the FUAS socioeconomic form, be classified by the tax authority in one of the four poorest income quintiles, and score at least 475 points in the PSU test. The only difference in terms of eligibility is that the former loan 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 in the country (all the traditional universities and 18 private universities).

3.1.1 The Traditional University Loan Program

The Traditional University Loan Program is managed by the universities, which determine the amount to lend and are in charge of the collection process.[9]

This loan has special conditions that make it very attractive to students. The real interest rate on this loan is about 2% per year with a maximum of 15 years of payments - after that, the debt is written off. Repayment starts two years after the student's graduation and the installments correspond to 5% of the borrower's income. Moreover, any portion of tuition not covered by this loan can be covered by the State Guaranteed Loan.

Despite these special characteristics, the loan has a low repayment rate (from 52 to 60% for the years considered). One possible reason is that the universities are in charge of collecting loan payments in the first stage and a central organization, named Fondo Solidario, is responsible in a second stage; neither are specialists in collecting loans. In recent years, the Chilean government has made some modifications that allow the tax authority to retain tax refunds and publicize names of defaulting students; this has increased the repayment rate to 80% (in some cases) of all reprogrammed loans.[10] The low enforceability and the low interest rate indicate the existence of a subsidy component in this loan scheme.

3.1.2 State Guaranteed Loan program

The State Guaranteed Loan program (Crédito con Aval del Estado, SGL hereafter) allows private banks to provide college tuition loans to eligible students. These loans 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.

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

[9] It was introduced in 1981 as part of an educational reform.
[10] Source: Fondo Solidario de Crédito Universitario.

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.

This loan scheme is very similar to loans currently available in the conventional financial market. First, the real interest rate was about 6% per year in the years considered, which corresponds to the government long-run interest rate,[11] and is slightly higher than the mortgage rate for the same period. Anecdotally, this loan and its interest rate led to massive street protests in 2011 and 2012. It was considered too expensive, because some graduates had to pay up to 17% of their income after graduation.

Second, private banks make the loans and are in charge of the repayment process. Private banks can use all available legal mechanisms to recover the debt, including release of information to credit score institutions, asset impoundment, and judicial collection. Releasing information is important in the labor market in Chile, because usually firms request that potential employees not appear as defaulters in credit score records.

Third, installments do not depend on the borrower's income. The SGL program requires students to start repayment 18 months after graduation in monthly installments for 20 years.

Fourth, to increase the enforceability of the debt, the loan contract has special clauses that involve the tax authority and employers. Employers are mandated to deduct repayments directly from payroll and to make payments directly to banks. The law also establishes penalties to employers who do not comply with this process. Additionally, the loan contract allows the tax authority to retain tax refunds in case the former student does not pay the lending bank. This last characteristic has proven to be an efficient measure, increasing repayment for these traditional loans since 2002.

In the case of dropouts, the higher education institution guarantees the loan: 90% of the capital plus interest for the first year, 70% for the second, and 60% for the third year onward. 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

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

responsible for enforcing collection from the student.

For all these reasons, I argue that this loan scheme can be used as a market benchmark.[12]

3.1.3 Other Loans Available

In order to have a broad picture of what type of loans students have available in the conventional financial market, here I briefly describe other sources of financing. First, some colleges offer scholarships or loans to complement the loans described above, mainly to attract the best students. As a consequence, all these scholarships and loans require much higher PSU scores than 475, hence do not confound the effects of the two loan programs.

There are two types of loans given by private banks: the Corfo loans ("crédito Corfo"),[13] and private bank loans. To get any of these loans, students need a guarantor, who needs to certify a good credit record, be employed, have a regular income source, and have a minimum family income or assets to use as collateral.

Corfo loans are offered by private banks, which manage the entire process, using resources coming from the Corfo development office. These loans have interest rates that vary among banks, ranging from 6.8% to 8.5% (real annual), and minimum guarantor monthly 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 the definition of the income quintiles).

Secondly, banks also offer loans with their own resources. The most relevant is the one given by BancoEstado.[14] This loan is aimed at lower income families, but the two poorest income quintiles are excluded. The minimum family monthly 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 $1,225 (CLP600,000).

Both of these loans depend on family characteristics that exclude students from the poorest

[12] This program was designed to give a market alternative to students who did not have access to traditional loans: students in private universities and vocational schools.

[13] Corfo (Corporación de Fomento a la Producción) is a development office from the government.

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

families. The income requirement is the main source of exclusion, but some families are excluded when they do not have a stable income source or have bad credit records. This is especially important in a country with high levels of labor market informality. According to the national household survey CASEN, in 2006, 36% of all workers are in the informal sector (self-employed or without a contract), and therefore students from those families were excluded from getting college loans in the regular market. Moreover, students need to rely on family altruism to get support when asking for loans.[15] In contrast, the two loan programs analyzed in this paper do not depend in family characteristics for 80% of the population (the four poorest income quintiles).

3.2 Sample

The data present two sources of selection that may be problematic. First, students that do not complete the FUAS socioeconomic form before the PSU test are not eligible, and therefore they are not affected by the cutoff. Second, because students can take the PSU test as many times as desired, a student may try repeatedly until getting a score equal to or greater than 475, self-selecting to be eligible for loans

I address the first problem by restricting the analysis to students who comply with all the requirements to get the Traditional or the SGL loan before the PSU test (*preselected students*, hereafter). For this sample of students, crossing the threshold implies a sharp change in their eligibility condition. To address the second problem (to eliminate the self-selection into treatment), I restrict the sample to first-time test takers.

Moreover, the assignment rule was fulfilled for all years except 2006, the first year of implementation. Anecdotally, in 2006 the Chilean tax authority gave the information on income by ranking students from 1 to N. This ranking was misinterpreted by the commission managing the SGL program, which 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.[16] In all other years, the assignment rule was fulfilled perfectly; hence I will only use the PSU processes

[15] Brown, Scholz and Seshadri (2012) have indicated this factor as an important source of credit constraints.
[16] Additionally, some loans were given to students below the cutoff

for 2007 to 2009.

4 Results

This section presents the empirical evidence organized as follows. Section 4.1 tests the conditions for a valid RDD: random loan assignment, absence of manipulation of PSU scores, and balance on baseline characteristics between the eligible and non-eligible students around the cutoff. 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. Section 4.4 presents the effects on college progress, the family income gap on progress, and dropout rates.

All the following RD results are restricted to the group of *preselected* students, took the PSU test for the first time, and scored 44 PSU points around the loan program cutoff (see section 3.1).

4.1 Conditions for a valid RD design

4.1.1 Loan Eligibility

Figure 2 shows the probability of being preselected for a loan among all students in the data set with respect to the PSU score, for all years separately and pooled together. Each dot represents the average preselection in an interval of 2 PSU points. The dashed lines represent fitted values from a fourth order spline and 95% confidence intervals for each side.[17]

On average 57% of all students crossing the cutoff fulfilled all of the conditions and became eligible. The figure shows that preselection is continuous at the cutoff indicating that students cannot precisely control their score, confirming the random nature of the PSU score around the cutoff.

For preselected students, loan eligibility changed from 0 to 1 at 475. Figure 3 shows that a third of these students took up any of the two loans around the threshold, while nobody from below the cutoff as expected. The second discontinuity that appears in that figure occurs at 550 with the

[17] All figures have the same structure, i.e. dots represent the average among students on bins 2 points wide; lines are fitted values for fourth order polynomial splines and 95% confidence intervals.

introduction of the Bicentenario scholarship. This change in loans take up will be used in Section 5 to disentangle the price and the access effect.

4.1.2 Local Continuity Assumption: Manipulation of the Assignment variable.

Because the eligibility conditions are public knowledge, students may try to self-select into treatment manipulating their score to be just above the cutoff. This would violate the assumption that the assignment variable density is continuous at the cutoff (McCrary, 2008).

I argue that manipulation is not feasible in this context. First, As explained in section 2.2, the PSU test contains only multiple choice questions that are graded by an optical device, and therefore it is an objective measure of the test outcome (see Figure 1 for an example of an answer sheet).

Second, to verify this formally, Figure 4 shows the estimate of the density function of PSU scores using fourth order splines for the assignment variable at each side of the cutoff, plus 95% confidence intervals. Dots in Figure 4 correspond to the empirical density. The test shows that the density function is statistically continuous at the cutoff, 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 test for the random nature of the scores in a small neighborhood around the threshold, I show that baseline characteristics are balanced at the cutoff.

First, as mentioned in section 3, no other aid or loan program influences the financial conditions for students in the vicinity of 475 (see Table 1). Secondly, I use equation (1) to show the balance on covariates at the discontinuity, where Y_i is now a covariate.

Table 3 shows the estimation of β_1 (when f is linear using the optimal bandwidth of $w^* = 44$ PSU points), for all observables in the PSU data set (t-stats in parentheses). The first 6 columns show the estimation for the three years separately. The last two columns show the results for all three years pooled together. In some years, there are differences in some characteristics, but they are reversed in the other years. Importantly, the pooled data presents perfect balance for all the observed characteristics available. Figure 5 confirms this, for 11 of the covariates presented in Table

3. All variables appear perfectly balanced at the cutoff.

These figures also explain the characteristics of the sample around the cutoff. The average income quintile is slightly below two, showing that the majority of the sample comes from the lowest income quintile. About 60% of the students are females, 19 years old with parent education of about 10 years. Most of them comes from public schools, are single, do not work previous entering college, and come from household of 4 members.

All the conditions for a valid RDD are satisfied. Therefore, the comparison between students barely eligible and barely ineligible will give the causal effects of access to loans on enrollment and progress.

4.2 Effect on College enrollment

The main result is shown in Figure 6, which shows the effect of crossing the cutoff on college enrollment for the entire population of preselected students. 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. 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 at least 475, and therefore become eligible for a loan, increase their enrollment rate significantly. The last figure, shows that the enrollment rate jumps from 18 to 37 percent. These numbers can be better appreciated in Figure 7 that shows the same graphs for a narrower window of 50 points around the cutoff.

Table 4 shows the estimation of β_0 and β_1 from equation (1), where Y_i is $Pr(Enroll_i = 1)$ an indicator function that takes on the value one if student i enrolled in college and zero otherwise. Column (1) shows the estimation for all years together, while columns (2), (3), and (4) show regression results for 2007, 2008, and 2009 respectively. Each year is shown separately to show robustness, because, for the sample of first time takers, in each year the natural experiment is independent of other years.

Having access to college tuition loans increases significantly 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 relative importance of these effects, the row labeled "Increase w/r to Ineligible Enrollment" shows the relative increase in the enrollment probability, i.e. the increase in the enrollment rate for barely eligible students as a fraction of the enrollment rate for barely ineligible student 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. In other words, for each student enrolling without access to loans, two students enroll when they have access to these programs.

To give a sense of how insensitive these results are to the chosen bandwidth and specification, Figure 8 presents estimates of β_1 and β_1/β_0 for different bandwidths (from 2 to 80 PSU points) comparing the linear with a fourth order spline specification. The upper left graph shows estimations of β_1 for the linear specification with a vertical line for the optimal bandwidth used throughout the paper ($w^* = 44$), while the upper right graph shows it for a fourth order polynomial 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 roughly 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 (standard errors calculated using the delta method). The relative increase is very close to 100% for both specifications and for all bandwidth, except for some small ones.

These results allow the conclusion that access to loans has a causal effect on college enrollment that is highly robust, and these effects are larger in magnitude than elsewhere reported.

4.3 Enrollment Gap by Family Income

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

I estimate the effect on $P(Enroll_i = 1)$ by income quintiles interacting equation (1) with dummies for the income quintiles. Therefore the analysis is equivalent to the one in section 4.2 comparing individuals with and without access to loans, but within income quintiles.

Table 5 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. The enrollment rate for students without access to loans at the cutoff in this quintile is 13.3 percent, this implies that having access to tuition loans led to a 151% increase in the enrollment rate.

The effects are slightly smaller but not significantly different for quintiles 2 and 3 (an increase of 17 and 16 percentage points in each case). The enrollment rate for the students barely below the threshold are 17% and 15% respectively, while the treated group reaches enrollment rates of 34% and 39%. This implies a relative 103% and 106% increase in the probability of enrollment for quintiles 2 and 3 respectively.

For the fourth quintile, the effects are weaker: the effect is not statistically different than zero in 2008 and only significant at the 5% level for 2007. For all the years pooled together, having access to loans causes an increment in the enrollment probability of 7 percentage points.

To see what happened with the enrollment gap by family income, Figure 9 shows these results in perspective for all years pooled together.[18] The graph on the left reproduce the results of Table 5, showing the jump in enrollment at the discontinuity by quintile by year, plus 95% confidence intervals, while the graph on the right shows the estimates for the enrollment rate separated for ineligible and eligible students at the threshold, and 95% confidence intervals.

The figure on the right shows that the college enrollment rate increases with family income for the group without access to tuition loans, and thus the enrollment gap by family income is very similar to the enrollment gap found in the whole population - 15 percentage points, and is statistically different than zero. The enrollment rate for the poorest quintile is 15% percent, while the richest income quintile has an enrollment rate of 30%.[19]

[18]Equivalent figures for each year show the same patterns. These results are not shown but are available upon request.

[19]The population enrollment gap (poorest to richest) calculated with this data may present biases if students from the richest income quintile do not apply for benefits, because they know they are not eligible. In that case, quintile information would be missing, and therefore those students wouldn't be considered in the calculations of Table 5

On the contrary, among students with access to loans, the enrollment gap by income quintile is statistically zero. The enrollment rate is 35% for the poorest quintile and 33% for the richest.

In conclusion, conditional on being around the cutoff (i.e. graduating from high school, taking the PSU test, and scoring around 475), access to these programs eliminates the college enrollment gap by family income.

4.4 College Progress and Dropout Rates.

In the previous section, I showed that access to loans has an important effect in the first year enrollment, and reducing the enrollment gap by family income. Nevertheless, these effects are not sufficient to reduce the educational attainment gap, if the students receiving these loans are not prepared to succeed in college and drop out easily. To assess the effects of the educational gap I estimate the effects of access to loans in college progress (defined as enrollment in the second and third year), and the dropout rate.

4.4.1 Effect on Enrollment Progress

I first estimate the effects of having access to these two loan programs on enrollment in the second and third years of college.[20]

Panel A in Table 6 presents the difference in enrollment in the second and third years between eligible and non-eligible students using the linear specification described before.[21,22]

Column (1) shows the enrollment in the second year for the cohort that took the test in 2007, and column (2) for those who took the PSU in 2008. Column (3) shows these two cohorts pooled together.

and Figure 9. I address that problem calculating the population enrollment gap using the Chilean household survey: CASEN 2006 and 2009, finding similar estimates than those calculated using this data.

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

[21] The results are highly robust to different functional specifications and different bandwidths. These results are not shown but are available upon request.

[22] Panel B in Table 6 shows the first stages of the different definitions of eligibility used in the 2SLS setting for enrollment in second and third year. 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 dummy for the cutoff corresponds to the portion of compliers.

Column (3) shows that having access to college tuition loans increases the probability of reaching the second year of college by 20.2 percentage points. In relative terms, the effect at the discontinuity is equivalent to a 213% increase in the second year enrollment. Only 9.5 percent of the ineligible students at the cutoff enroll in a second year, while 29.7 percent do so of the eligible students.

The difference in the enrollment rate between eligible and ineligible students increased from the first to the second year, in absolute and relative terms, 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 have to work less, to finance educational investments and consumption.

Column (4) confirms this. It shows the effects of third year enrollment, defined as having enrolled in three consecutive years, regardless of true advancement in coursework. Here, we observe the behavior of the cohort that took the PSU for the first time in 2007. Third year enrollment increases by 20.5 percentage points for those eligible for loans. Relatively to the enrollment rate for the control group (4.6 percent), this implies a 445% 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, 5.5 students do so when they have access.

4.4.2 College Progress by Income Quintile

Table 7 shows the effect on college progress by income quintile. As before, the dummy for being eligible on the first attempt is used as instrument for being eligible in the second and the third year, using the linear specification described before.[23]

As before, columns (1) and (2) show enrollment in the second year for cohorts 2007 and 2008 respectively and column (3) summarizes second year enrollment for both cohorts. Column (4) shows the effects for the third year for 2007 test-takers.

Column (3) shows that the effects are stronger for the first 3 income quintiles. Students that have access to the loan programs increase their second year enrollment rate by 22 percentage points compared to the enrollment rates for ineligibles: 6%, 10% and 11% for quintiles 1, 2 and 3

[23] The results are also highly robust to different specifications and bandwidths. Those results are available upon request.

respectively. This represents relative increases of 346%, 223% and 202% respectively. The fourth income quintile also benefits from the loan programs; eligible students increase their enrollment probability in the second year by 6 percentage points. Students in the fifth income quintile have statistically the same second year enrollment rate around the cutoff.

Similar changes occur for the third year enrollment. The poorest 3 income quintiles are the ones that increase their progress most. The first quintile increases its enrollment by 21 percentage points, while the baseline enrollment is only 3 percent. This implies that for each student enrolling in third year without access to loans, 8 students do so when they have access. For the second income quintile, the relative increase is equivalent to 9 students enrolling in the third year for each one with no access to tuition loans. For the third quintile, this number is 6 students. The 2 richest income quintiles present no statistical difference around the cutoff.

Regarding the enrollment gap by family income, Figure 10 shows the enrollment rate separated for students barely eligible and ineligible by income quintile. The left graph shows enrollment in second year and the right shows enrollment in the third. Those figures show a strong income gradient in college progress for students that have no access to tuition loans: the second year enrollment rate among students from the richest income quintile is 3 times the rate for the poorest. This gap increases to 8 times (richest to poorest) enrolling in the third year. Strikingly, on the contrary, the gap between the poorest and the richest quintiles is statistically zero when access to the loans is granted.

In conclusion, access to loans not only reduces the gap in initial enrollment, the effects are persistent in later years, allowing students from poor backgrounds have the same enrollment rate as the richest of the country.

4.4.3 Dropout Rates

Few papers try to document the causal effects of financial aid on retention conditional on enrollment and dropout rates. Exceptions are Dynarski (2003), Bettinger (2004), Singell (2004) and Stinebrickner and Stinebrickner (2008), with mixed conclusions.

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 test, while in the previous analysis the comparison was made with respect to all students around the cutoff. This analysis shed light on the potential problems faced by the literature.

Because this section compares only students who self-selected into college around the cutoff, the estimates are no longer causal effects, but rather are correlations that can be illustrative in comparison to the previous results. First I check what types of biases are underlying the correlations by comparing observable characteristics. Table 8 shows the estimation of equation (1) for the same group of covariates for students around the cutoff, with each column corresponding to those who enrolled in 2007 through 2009. As expected, 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 better 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 preference for college education.

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 will therefore use different definitions of dropout. The first definition, named "dropout after 1 year" is an indicator function that takes the value 1 if a student enrolls after taking the PSU test for the first time, but is not enrolled the year immediately after. Similarly, "dropout after 2 years" takes the value of 1 if somebody enrolled in 2007, but was not enrolled in 2009. Because these definitions do not capture the possibility of coming back after some years of absence, I define a third measure of attrition. The variable "dropout in 2009" is an indicator variable that takes the value of 1 for any student who enrolled in 2007 or 2008 but was not enrolled in 2009, the last year I observe the enrolling behavior of these students. This measure allow those who enroll in 2007 to miss 2008 but come back in 2009, but do not capture those who may come back in later years. Finally, to distinguish between the first two definitions, I define a variable named "dropout in 2nd year" to equal 1 if a student enrolled the first 2 years but did not show up in the third.

Table 9 reports the estimate of the same linear specification in a 2 stage least square setting to control for self-selection on the eligibility condition, adding as covariates all the observable characteristics to control for any selection on observables. Column (1) shows the dropout rate after the first year of college. Students barely eligible for loans drop out 6 percentage points less frequently than ineligibles. The dropout rate for students without access to these loans is 19%; this implies that the probability of dropping out after the first year of college experiences a 31% decrease.

Column (2) shows the estimates for the definition "dropout in 2009" indicating that eligible students drop out 5 percentage points less than constrained ones. Column (3) shows dropout rates over a longer span of time: "dropout after 2 years of college." Eligible students dropped out less frequently, by 11 percentage points. In relative terms, this effect implies a 38% decrease in this dropout rate. Column (4) shows that the dropout rate does not fall significantly for the students enrolled in a second year, but the relative magnitudes confirm the previous result: there is a 26% reduction in the dropout probability.

These correlations help explain the long run enrollment results previously reported but they also help to reconcile results from the literature. One important issue is the selection process that is evident here. Students who enroll in college are different than the underlying population, and therefore the effects estimated from the sample conditional on enrollment present a bias towards zero. In this case, the dropout results are much smaller than the results from enrollment and progress which are due to family income compensating for access to loans. Nevertheless, they are relatively large and highly significant.

5 Price versus access effect

A natural question is whether these effects in enrollment are consequence of access to credit markets (the "access effect") or is due to a change in the price of higher education (the "price effect"), if these tuition loans present an implicit subsidy component when the interest rate is lower than the market interest rate or when the repayment is not strongly enforced.[24]

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

To decompose the total effect into price and access effects, I present the following three tests.

5.1 Estimating the price effect directly

The first test estimates the price effect directly, i.e., compares the college enrollment rate among students facing different tuition costs. Specifically, I compare the enrollment rate of students who must finance the full tuition cost to those who receive a scholarship or grant that covers part of the tuition, in a context where access to credit is available for everybody (no access effect). As usual, students who receive scholarships or grants are different from those who do not. To address the endogeneity problems, I use another natural experiment of the same type as exogenous variation in access to scholarships.

Table 1 presented the requirements to qualify for grants delivered by the Ministry of Education. Most of them depend on PSU score cutoffs, enabling the use of a regression discontinuity design, as discussed above. In particular, the Bicentenario grant is available to students who comply with the following requirements. First, complete the FUAS socioeconomic form. Second, belong to the two lowest income quintiles. Third, enroll in traditional universities, and fourth, obtain a PSU score above 550.[25] This grant program is the third in importance in the country. It is given to 4.7% of the universe of students and 55% of eligible students. Moreover, the benefit covers in full the reference tuition[26] (90% of the total tuition cost), hence constitutes a significant drop in the cost of university education.

Because all students scoring 475 PSU points or higher have access to credit markets (secured by the existence of the two loan programs discussed above), there is no credit access effect around the Bicentenario grant cutoff. Therefore, the comparison between the college enrollment rates of those who score barely above the grant cutoff of 550 PSU points, with those slightly below gives the causal effect of the reduction in tuition cost on the enrollment rate, the price effect.

[25] The same tests are made to check the validity of RDD: Balance on observed characteristics, bunching at the cutoff, and a discontinuous number of recipients around the cutoff. Those tests are not presented here but are available upon request.

[26] Reference tuition is a value determined by the Ministry of Education. It calculates what the tuition at a given university should be based on its characteristics (quality of education, demand for programs, prestige, etc). On average, reference tuition corresponds to 90% of effective tuition fixed by universities.

The upper left graph in Figure 11 shows the rate of students receiving the Bicentenario grant among students who meet all requirements around the grant cutoff point (as before the sample is restricted to preselected students that take the PSU for the first time to avoid selection). By definition, nobody below the threshold received the benefit, while 38% of students above the threshold received the grant, and therefore their tuition was reduced by approximately 90%. A close-up is shown in the figure in the middle.

The bottom left figure also shows a close-up of figure 6 to enlarge the effects in enrollment around 550. More formally, Column (2) of Table 10 presents the linear regression for equation (1) around the cutoff of 550.[27]

The reduction in the costs of college did not affect enrollment. This indicates that investment in college education has returns that largely compensate the cost of loans, and therefore students with access to financing are able to invest optimally. On the contrary, the enrollment rate for students below 475 shows that with no access, many students underinvest in college education.

If this scenario were true for those around 475, we could conclude that the effects for enrollment and progress are mainly due to the difference in access to credit markets. However, there may be differences between the enrollment decisions of students receiving this grant and those who get scores around 475. To eliminate in part this problem, I perform the same analysis using another scholarship with a cutoff point closer to 475: the scholarship for teachers' children, which considers a cutoff of 500 points. The main disadvantage is that this program generates much less variation in the rate of beneficiaries than the previous program (see table 1).

To gain access to this scholarship, students need to satisfy the following requirements. First, complete the FUAS form, second, belong to one of the four poorest income quintiles. Third, be children of teachers or employees of public and voucher schools, and fourth, score 500 points or more in the PSU. This scholarship provides a benefit level similar to the reference tuition discussed in the previous section and the Bicentenario grant.

The upper right graph of Figure 11 shows the change in the rate of beneficiaries from this scholarship relative to eligible students. Being a child of a teacher or employee of an eligible school

[27] Column (1) shows the change in the rate of beneficiaries of the Bicentenario scholarship around the cutoff of 550.

is not observable in the data; therefore, this graph shows the rate relative to all those who meet all the other requirements.[28]

The lower right figure shows that there is no change in college enrollment, despite the significant drop in the price of education. These results are confirmed in Table 10. Column (4) shows the change in the probability of receiving the scholarship and column (5) shows the change in the enrollment rate at the cutoff. The price effect is again statistically zero, so it can be concluded that the effect on enrollment and progress corresponds to a change resulting from access to credit markets.

The price effects measured by both of these scholarships use students that have higher PSU scores than 475, and therefore they may not fully reflect the population of interest: the students around the threshold. To address that problem, the following two tests shed light on what it happens around 475.

5.2 A Placebo Test

The SGL has very similar characteristics to conventional market loans (see section 3.1.2) and students that enroll in private universities can only have access to this loan. These characteristics can be used to test the hypothesis of the existence of credit constraints and to assess the importance of the access effect.

Under the hypothesis of no credit constraints, the inclusion of this loan program should not cause any change in the enrollment rate in private universities, because this loan scheme is equivalent to those already in the market, and student enrolling in private universities cannot have access to traditional loans (which we assume have a significant price effect). Thus, if the hypothesis is true this program is acting as a placebo for the group of students that enroll in private universities. Hence, the test measures the change in enrollment in private universities.

Table 11 shows this test using equation (1) with a dummy for enrolling in private universities as dependent variable. As before, the first column shows the effect for all the year pooled together

[28]Even though the industry in which parents work is observable in the data, it does not mention whether those working in education correspond to eligible schools or other educational institutions. In addition, many students do not report industry.

and the rest shows each year separately.

The test rejects the hypothesis of no credit constraints with a t-stat of 17. Moreover, this test indicates that the enrollment in private universities increased in 8.7 percentage points, half of the enrollment effect analyzed previously. The other half is a mix between access and price effects, since happen in traditional universities where both loan programs are available. Therefore at least 50% of the effect is due to credit access.

Figure 12 confirms this, it shows the regression discontinuity for enrollment in private universities. The first three graphs show the effects for 2007, 2008, and 2009 respectively to show robustness, and the lower right graph shows the effect for all years pooled together.

5.3 Responses for Differentiated Access to Credit Markets

The third test analyzes the different response of groups with differentiated access to credit markets. The price and the access effects can be decomposed by observing the different aggregate behavior of these groups. The price effect can be obtained decomposed from the access effect by observing the behavior of students from families that traditionally have had access to loans in the Chilean market.

As discussed in section 3, the fourth and fifth quintiles have access to private loans with interest rates similar to the SGL. The fourth quintile is also eligible for this loan, so the total effect for that group is composed only of the price effect. Under the hypothesis that the price effect is zero, the inclusion of the SGL should not produce any change in the enrollment rate for the fourth quintile.

This hypothesis is tested using the enrollment rate in private universities where SGL is the only program available. Table 12 shows that there is no change in enrollment for the fourth quintile in private universities in 2007, 2009 and for all the sample pooled together and in 2008 the effects is marginally significant. This confirms that SGL is equivalent to the loans in the conventional credit market (without price effect). Additionally, the larger response for the first two quintiles, which do not have access to other loans in the formal market, indicates that these effects are driven by the accessibility of loans rather than a change in the price of education.[29]

[29]Panel B of Table 12 gives the relative importance of these effects, the change in enrollment relative to the rate of

Having access to the SGL implies a 105% increase in the probability of enrollment for the first quintile, while the increment is 61% and 44% for quintiles two and three respectively, indicating that those with less financial opportunities are the ones driven the effects.

None of the three tests presented here are a perfect measure of how important the access effect is, but taken together they are evidence indicating that the results shown in the previous sections are mainly driven by the partial elimination of credit access restrictions for the lowest income quintiles.

6 Validity Checks

Finally, I perform validity checks for 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.), and thus the discontinuity shown before may be artificially driven by colleges placement rules. In the second case, if students above the cutoff have more programs to choose from, then more students are willing to attend college and the discontinuity arises as a consequence.

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 students who belong to the highest income quintile, and therefore are ineligible, while the second uses information on applications and placement in traditional universities.

those barely ineligible.

6.1.1 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,[30] so they cannot discriminate based on whether a student has access to loans. If colleges are discriminating against students below 475, we should see a discontinuity at the threshold for all income groups, including those from the highest quintile.

The row labeled "$\mathbf{1}(PSU \geqslant \tau)$ x q5" in Table 5 shows the effects of crossing the cutoff for the richest income quintile. The results indicate that there is no discontinuity around 475 for this income group.

To have a broader picture, Figure 13 depicts the regression discontinuities for the different years and for all years together, using the same type of graphs as before for the richest quintile only. This figure shows that there is no difference in enrollment around the cutoff. These graphs confirm that the positive and significant effect at the 10% confidence level effect for 2009 shown in column (4) of Table 5 is the consequence of a type I error rather than a discontinuity. This evidence shows that colleges are not selecting students based on their financial condition.

6.1.2 Applications and Placement for Traditional Universities

The centralized process in traditional universities requires that students apply to and rank at most eight programs. I use the information on students' applications to show that college placement offers are locally continuous at the cutoff, thus colleges are not offering placement depending on the student financial status. Moreover I show that the discontinuity is driven by students who score at least 475 who apply more often after they became eligible for these loan programs.

The same regression discontinuities are run for the probability of submitting a valid application and for student's placement on a program conditional on having applied as dependent variables. Panel A of Table 13 shows results using all preselected students in the optimal bandwidth neighborhood around the cutoff.

[30] The only information universities have is the student's self-reported income category, which has a 0.4 correlation with the income quintiles reported by the tax authority.

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. Column (1) of panel A shows that the probability of application 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 13 shows the probability of being placed conditional on having applied to a program in these universities. Panel-B's Column (1) in this table 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 shown, indicating that traditional universities did not discriminate based on loan eligibility.

To show that these results are not sensitive to bandwidth or functional forms, Figure 14 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 placement conditional on having applied.[31]

6.2 Program Cutoffs on Private Universities

The previous analysis do not rule out the possibility that private universities are discriminating due to loan eligibility, but the same analysis cannot be performed for these universities, because there is no information on preferences for their programs. Instead, I present evidence that the enrollment discontinuity is not driven by a larger availability of programs in private universities 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. Because the concern is for student at the threshold I restrict the sample to students 2 PSU points around 475.

Panel A of Table 14 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 also accept students from the control group. If a substantial number of colleges were selecting only

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

students with scores above 475, we should observe a bigger difference in the availability of programs.

Even though the percentages in Table 14 are high, they are statistically different from 100%. To see the effect of the difference in program availability between groups, Panel B of Table 14 shows the same regression discontinuity as before in a 2 point window around the cutoff, eliminating from the sample all the students in the treatment group who enrolled in a program with a program cutoff higher than 475.

Column (1) of Table 14 shows the effect for all three years pooled together. Again, the effect of access to college loans on college enrollment is highly significant and equal to 14 percentage points, which is equivalent to an 105% increase relative to the average enrollment for the control group.

To see if these results depend on the chosen bandwidth or functional form, Figure 15 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 form.

7 Conclusions

In this paper, I exploit the sharp eligibility rules of two programs in Chile that give access to tuition loans to students who score above a given threshold on the national college admission test. This enables a regression discontinuity design which, combined with full information on students' enrollment and financial aid benefits, and an admission system that weights only two observed variables, allow the unbiased estimation of the causal effect of credit access on college enrollment and progress.

The results around the cutoff show that, for every student who enrolls in college with no access to loans, 2 students enroll when these loans are available, mainly from families of the poorest three income quintiles. This enrollment effect is enhanced over time in the second and third years of college. For each student without access to these loans who persists in the second and in third years, 3.1 and 5.5 students enroll in the second and third year respectively when they do have access to these financial programs.

The results above are interpreted as the effects of access to credit markets on college enrollment and college progress, because three different tests that decompose the total effect into price and access effects indicate that price component is very small to non-significantly different than zero.

Most strikingly, this paper shows that access to the loan programs appears to eliminate the relatively large income gradient in college enrollment and progress. Among students who are barely ineligible for loans, those coming from the richest quintile are twice as likely to enroll as students from the poorest income quintile, an enrollment gap very similar to the whole population. In contrast, among students who are barely eligible, the gap is statistically zero.

The same is true for enrollment in the second and third years. Student from the richest income quintile are 3 and 8 times as likely to enroll as students from the poorest income quintile in second and third year respectively when this loan programs are barely not available. On the contrary, among students that are barely eligible, there is no statistical difference in the enrollment rate between richest and poorest.

Around the threshold all baseline characteristics are balanced, including ability and family income, therefore, I conclude that access to credit explains almost entirely the enrollment and the educational attainment gap between students with different family income (conditional on participating in the admissions process and being at the cutoff).

The evidence shows that incomplete credit markets prevent students from low income families from investing in human capital. Credit access therefore has implications for the intergenerational distribution of education and income.

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9 Tables and Figures

Table 1: Requirement for scholarships

	% Recipients[1] with respect to:		Requirements:			
	Population	Eligibles	Income quintiles	PSU Cutoff	Institution type	Cover
	(1)	(2)	(3)	(4)	(5)	(6)
Loans						
State Guaranteed	9.46%	27.90%	1 to 4	**475**	Accredited[+]	(a)
Traditional Loan	8.58%	21.92%	1 to 4	**475**	Traditional[++]	(a)
Scholarships and Grants						
Bicentenario	4.70%	55.14%	1 and 2	550	Traditional	(a)
Juan Gomez Millas	0.02%	0.87%	1 and 2	640	Accredited[2]	(a)
PSU Score grant	0.02%	0.05%	1 to 4	-	Accredited[3]	(b)
Exellence	2.32%	4.78%	1 to 4	-	Accredited[2,4]	(a)
Teacher's children: BHDP	1.02%	3.98%	1 to 4	500	All[5,6]	(c)
Pedagogy: BPED	0.07%	0.74%	all	600	Accredited[5]	(b)

(1): Column (1) reports the ratio of recipients over students taking the test for the first time. Column (2) correspond to the ratio of recipients over those that take the PSU test for the first time, have applied to the benefit, belong to eligible quintiles and score more than the respective cutoff.
(2): Only students graduating from voucher and public high schools.
(3): National or regional best PSU score.
(4): Only for students in the top 5% of their graduating high school.
(5): Only student with high school GPA greater than 5.5 are eligible for BHDP, and only GPA greater than 6.0 for BPED. High School GPA goes from 1 to 7 points.
(6): Only for children of teachers and employees from voucher of public schools.
($^{+}$): "Accredited" refers to all accredited colleges (traditional and private) and accredited vocational institutions.
($^{++}$): "Traditional" refers to traditional universities which are all accredited.
(a): Funds up to reference cost.
(b): Funds up to fixed value, about the same magnitude than reference tuition US$2,250 for univ., which corresponds to the average reference tuition, and US$1,000 for vocational).
(c): Funds up to US$1,000 which correspond to a third of the university average tuition or total vocational school tuition.

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: 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		25,114		29,899		77,646	

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 4: 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)
$\mathbf{1}(\text{PSU} \geqslant \tau)$	.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%.

Table 5: 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)
$\mathbf{1}(\text{PSU} \geqslant \tau)$ x q1	.201	.210	.186	.203
	(.008)***	(.015)***	(.016)***	(.013)***
$\mathbf{1}(\text{PSU} \geqslant \tau)$ x q2	.171	.211	.160	.157
	(.013)***	(.026)***	(.022)***	(.022)***
$\mathbf{1}(\text{PSU} \geqslant \tau)$ x q3	.164	.210	.162	.134
	(.017)***	(.031)***	(.028)***	(.029)***
$\mathbf{1}(\text{PSU} \geqslant \tau)$ x q4	.070	.064	.033	.110
	(.020)***	(.032)**	(.036)	(.034)***
$\mathbf{1}(\text{PSU} \geqslant \tau)$ x q5	.031	-.091	.042	.085
	(.022)	(.058)	(.030)	(.044)*
q1	.133	.164	.142	.146
	(.008)***	(.009)***	(.007)***	(.005)***
q2	.166	.219	.190	.195
	(.016)***	(.014)***	(.014)***	(.009)***
q3	.154	.242	.257	.224
	(.020)***	(.019)***	(.020)***	(.011)***
q4	.232	.345	.290	.287
	(.023)***	(.026)***	(.024)***	(.014)***
q5	.318	.248	.372	.292
	(.038)***	(.019)***	(.032)***	(.015)***
Obs.	84605	24126	28536	31943
R^2	.378	.377	.379	.384

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

Table 6: 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)
$\mathbf{1}(\text{PSU}\geqslant\tau)$	.788	.753	.769	.757
	(.007)***	(.007)***	(.005)***	(.008)***
Const.	.271	.286	.281	.326
	(.030)***	(.027)***	(.020)***	(.033)***
Obs.	22633	25114	47747	22633
R^2	.745	.729	.737	.709

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

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

2SLS IV Regression				
Dependent Var.:	Enrollment 2nd year in 2008	Enrollment 2nd year in 2009	Enrollment 2nd year pooled	Enrollment 3rd year in 2009
	(1)	(2)	(3)	(4)
Eligible x q1	.228	.207	.218	.212
	(.017)***	(.019)***	(.013)***	(.017)***
Eligible x q2	.261	.198	.223	.256
	(.033)***	(.031)***	(.023)***	(.033)***
Eligible x q3	.266	.189	.222	.236
	(.038)***	(.037)***	(.027)***	(.039)***
Eligible x q4	.084	.029	.059	.061
	(.038)**	(.045)	(.030)**	(.038)
Eligible x q5	-.054	.062	.032	-.048
	(.062)	(.039)	(.033)	(.062)
q1	.044	.083	.063	.030
	(.010)***	(.012)***	(.007)***	(.010)***
q2	.058	.128	.100	.028
	(.021)***	(.021)***	(.016)***	(.021)
q3	.060	.147	.110	.049
	(.026)**	(.026)***	(.019)***	(.026)*
q4	.152	.278	.212	.142
	(.027)***	(.034)***	(.022)***	(.027)***
q5	.275	.171	.198	.244
	(.049)***	(.026)***	(.023)***	(.049)***
Obs.	24172	28583	52755	24172

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 1 year of college	Dropout in 2009	Dropout after 2 year of college	Dropout in 2nd year 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.	14750	14750	6728	6728
R^2	.032	.04	.067	.018

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%. All regressions are based on equation (1), using 44 point around the cutoff and a 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.

Table 10: Measuring the price effects from exogenous change in college tuition.

	Recipients Bicentenario grant at 550	College Enrollment at 550	Recipients of any benefit at 550	Recipients Teacher's Children at 500	College Enrollment at 500
	(1)	(2)	(3)	(4)	(5)
1(PSU⩾550)	.378	.012	.009		
	(.006)***	(.009)	(.009)		
1(PSU⩾500)				.021	-.008
				(.002)***	(.006)
Const.	.00005	.633	.625	-2.99e-14	.468
	(.004)	(.006)***	(.006)***	(.001)	(.004)***
Obs.	48370	48370	48370	82991	82991
R^2	.327	.03	.015	.013	.109

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

All regressions are based on equation (1), using 44 point around the cutoff and a linear specification.

Table 11: RD for enrollment in private universities. Preselected, first-time-takers and $w = 44$ PSU points

Dependent Var.:	Private College Enrollment in year:			
	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
$\mathbf{1}(\text{PSU} \geqslant \tau)$	.087	.087	.065	.106
	(.005)***	(.009)***	(.009)***	(.009)***
Const.	.132	.092	.152	.146
	(.003)***	(.005)***	(.006)***	(.006)***
% Increase wr Control Enr.	66%	95%	43%	73%
Obs.	77646	22633	25114	29899
R^2	.023	.023	.012	.035

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

Table 12: RD Private 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)
$\mathbf{1}(\text{PSU}\geqslant\tau)$ x q1	.105	.107	.073	.125
	(.007)***	(.011)***	(.013)***	(.011)***
$\mathbf{1}(\text{PSU}\geqslant\tau)$ x q2	.086	.097	.059	.112
	(.011)***	(.020)***	(.019)***	(.020)***
$\mathbf{1}(\text{PSU}\geqslant\tau)$ x q3	.071	.085	.076	.060
	(.015)***	(.025)***	(.025)***	(.027)**
$\mathbf{1}(\text{PSU}\geqslant\tau)$ x q4	.020	-.012	.019	.053
	(.018)	(.026)	(.032)	(.032)*
$\mathbf{1}(\text{PSU}\geqslant\tau)$ x q5	.018	-.056	.048	.019
	(.020)	(.040)	(.028)*	(.042)
q1	.100	.074	.120	.106
	(.004)***	(.007)***	(.008)***	(.007)***
q2	.140	.092	.159	.152
	(.008)***	(.013)***	(.013)***	(.012)***
q3	.173	.109	.175	.219
	(.010)***	(.017)***	(.018)***	(.018)***
q4	.219	.158	.242	.260
	(.013)***	(.020)***	(.024)***	(.023)***
q5	.231	.189	.209	.316
	(.014)***	(.032)***	(.018)***	(.031)***
Obs.	86463	24889	29404	32170
R^2	.206	.167	.204	.243
Quintile	PANEL B: Relative increase			
q1	105%	145%	61%	118%
q2	61%	105%	37%	74%
q3	41%	78%	43%	27%
q4	9%	-8%	8%	20%
q5	8%	-30%	23%	6%

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

Table 13: 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			
	.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			
	-.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%

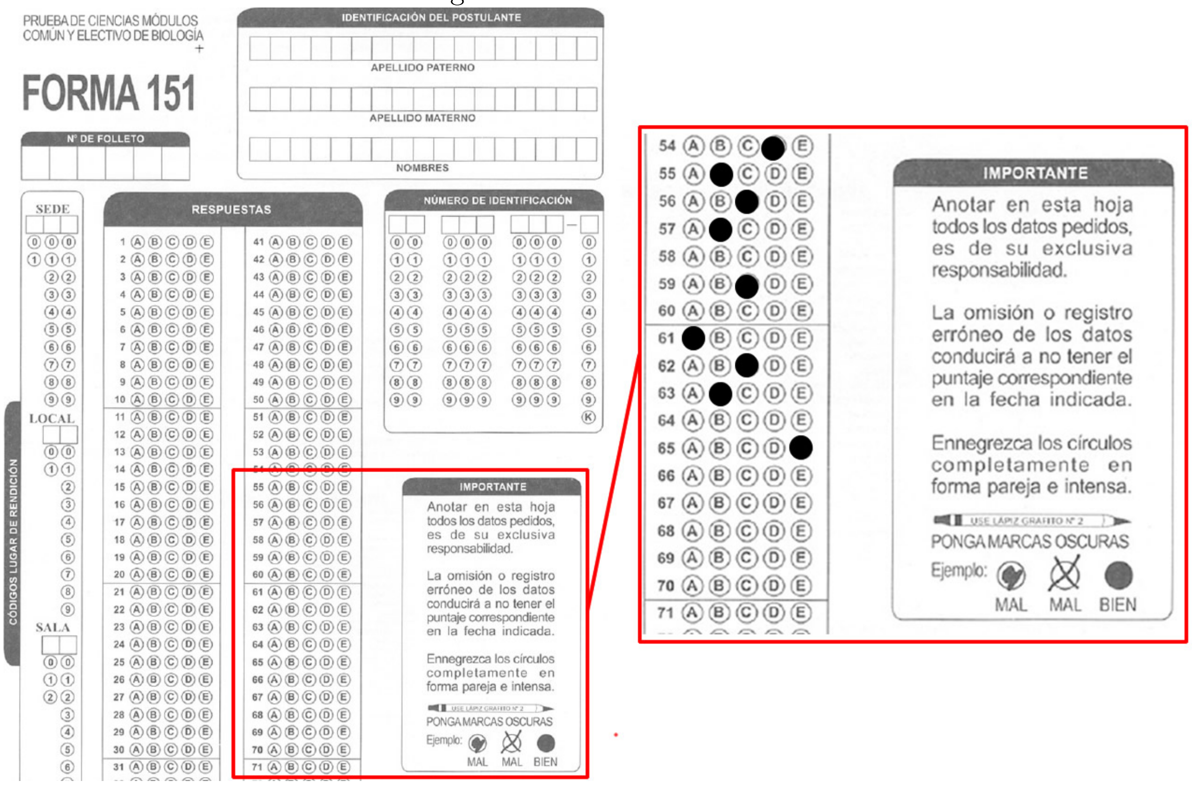
Table 14: RD eliminating all students that enrolled in programs that were not available for students below the cutoff. $w = 2$

	Pooled 07-09	2007	2008	2009
	(1)	(2)	(3)	(4)
PANEL A: % of programs with cutoff below 475				
% of programs with cutoffs below 475	96.23%	95.38%	98.02%	92.53%
PANEL B: Enrollment in programs with cutoff below 475				
Dependent Var.:	College Enrollment in year:			
$\mathbf{1}(\text{PSU}\geqslant\tau)$	.139	.121	.163	.161
	(.028)***	(.052)**	(.052)***	(.043)***
Const.	.133	.079	.175	.115
	(.021)***	(.041)*	(.039)***	(.034)***
Increase w/r Control Enr.	105%	153%	93%	140%
Obs.	3244	966	1067	1211
R^2	.022	.018	.027	.026

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

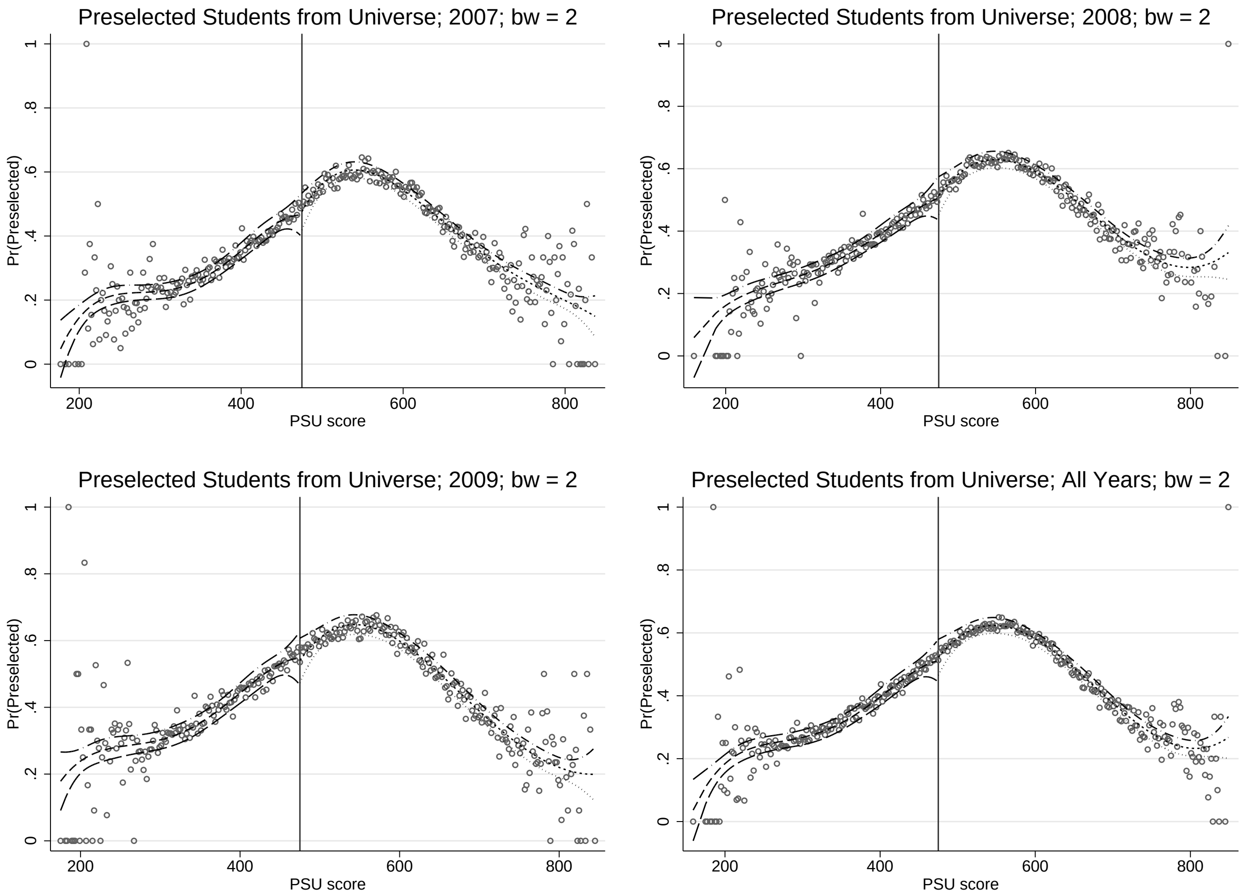
Panel A considers the percentage of programs that have cutoffs below 475 for all students in the treatment group when the window is $PSU_i \in [475, 477)$.

Figure 1: PSU Answer sheet



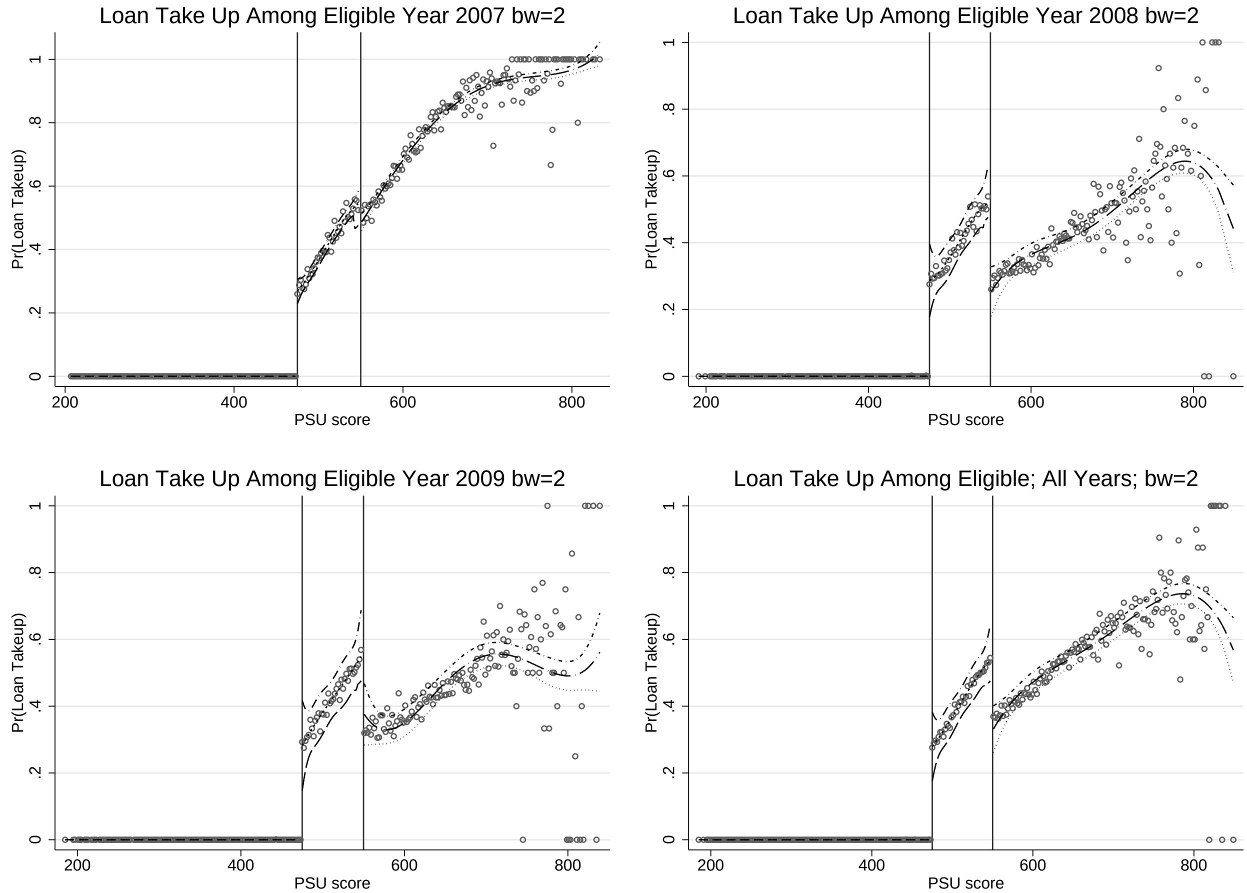
Note: To answer the PSU test students need to bold the circle with the correct answer. Optical devices grade this sheets.

Figure 2: Probability of being *Preselected* for loans.



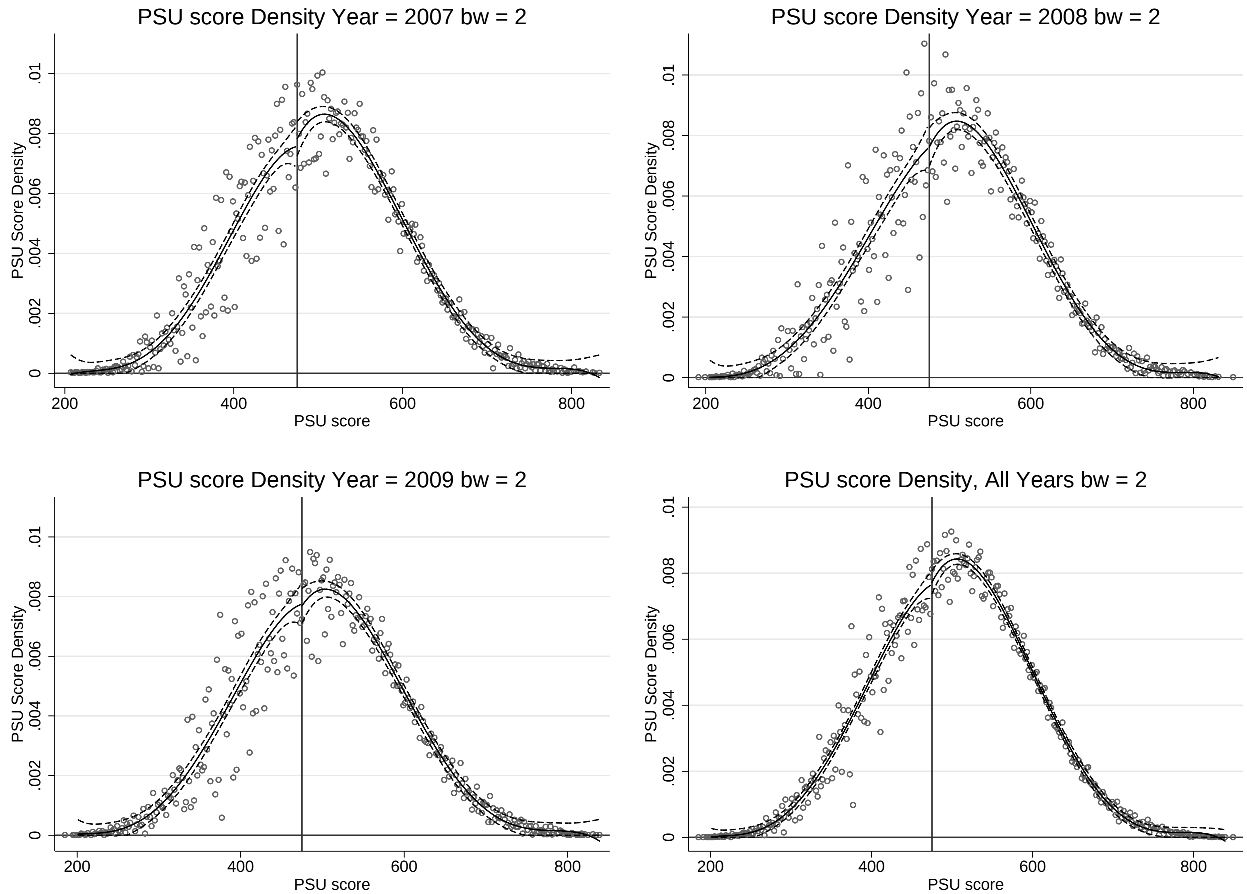
Note: Each dot indicates the preselection rate of students with scores in an interval of 2 PSU points (all students included). On average each dot contains 670 students. The dashed lines represent fitted values from a 4th order polynomial spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475).

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



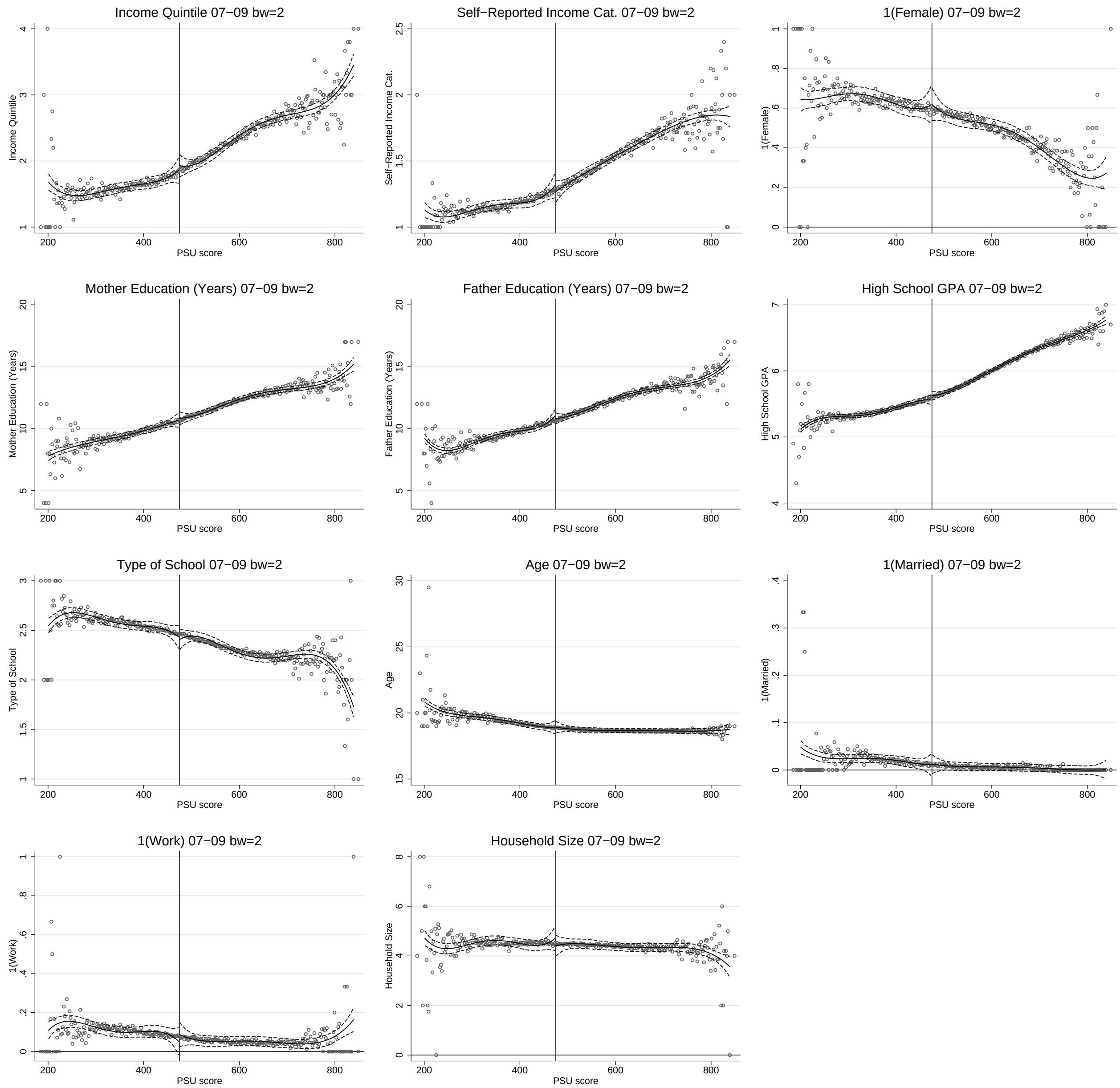
Note: Each dot represents average loan take-up relative to eligible students, in an interval of 2 PSU points. To the right of tye cutoff, each dot contains on avergae roughly 441 students receiving the loans. The dashed lines represent fitted values from a 4th order spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475).

Figure 4: RD for PSU scores frequency distribution.



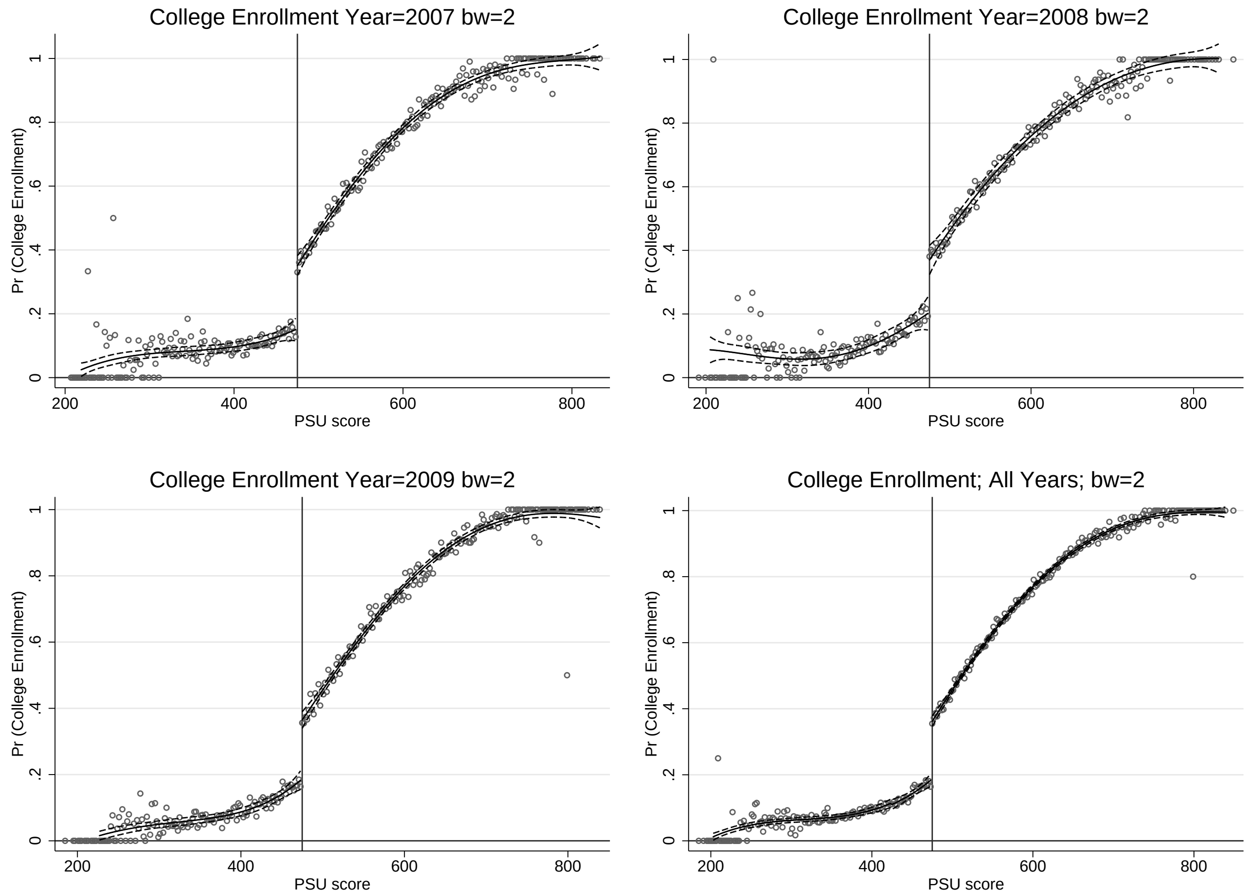
Note: Each dot represents the density of PSU scores in an interval of 2 points. The sample considers only students who satisfy all requirements to be eligible for college loans and take the PSU immediately after graduating from high school.

Figure 5: RD for base line characteristics. Full sample.



Note: Each dot represents the average of the variable 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 preselected first-time takers students.
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.

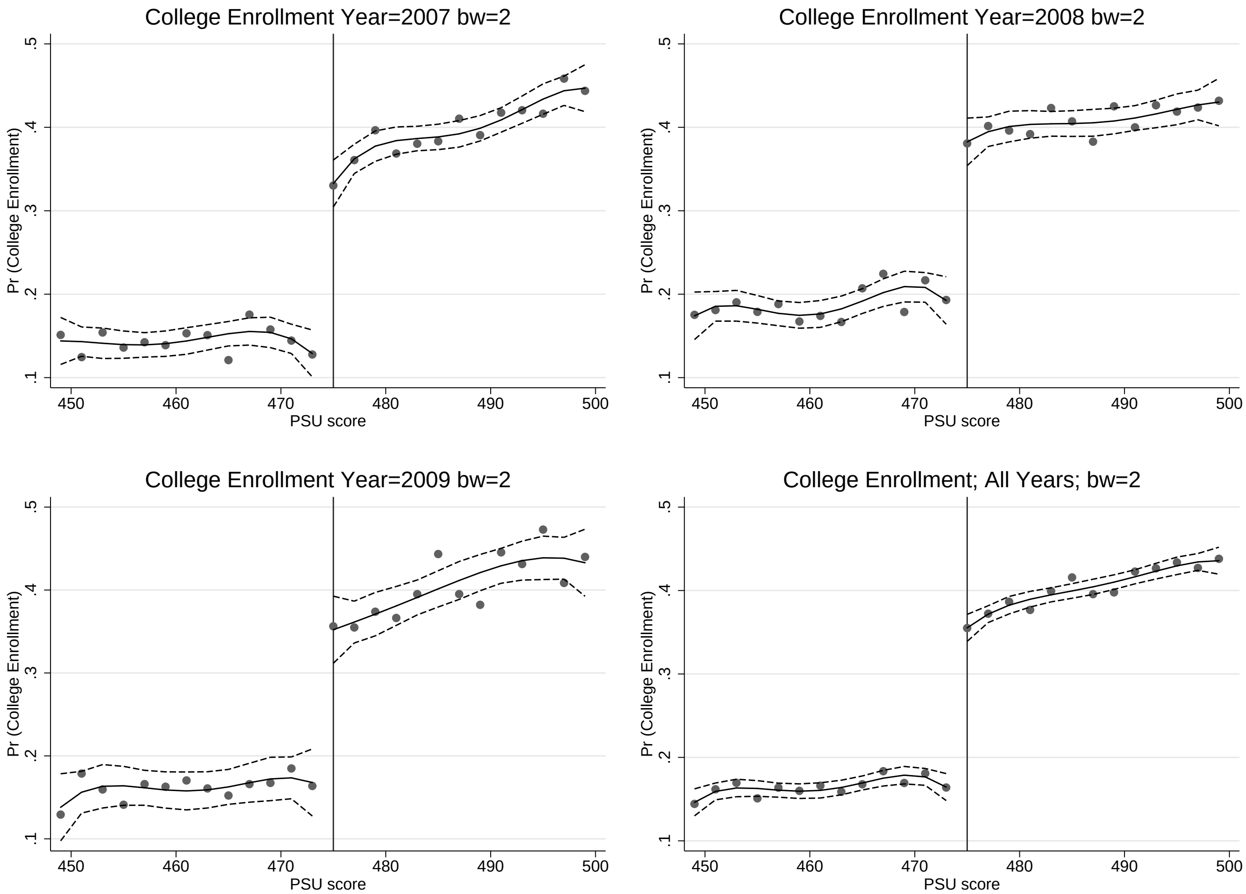
Figure 6: RD for College enrollment. Full sample.



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).

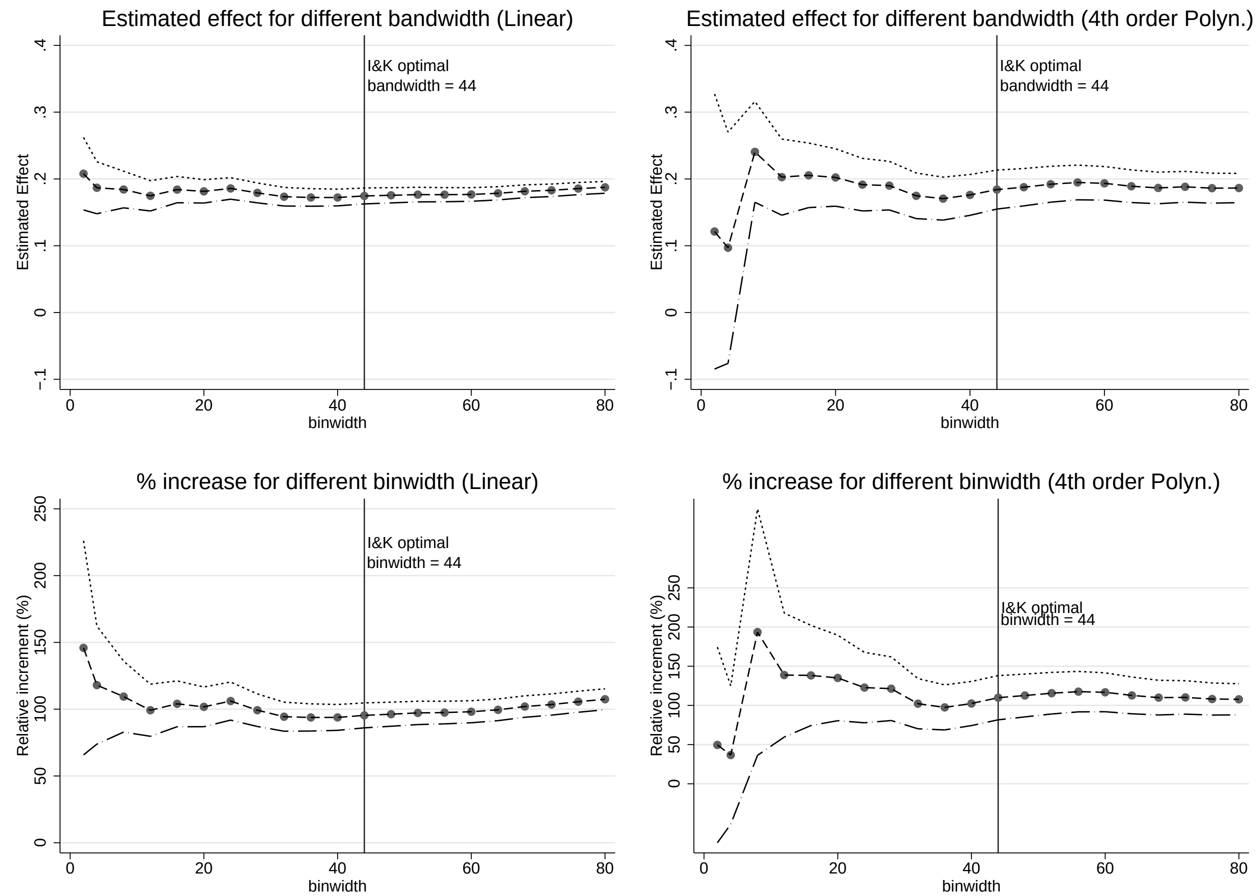
These graphs show the full sample of students fulfilling all requirements to be eligible for college loans and taking the PSU immediately after graduating from high school.

Figure 7: 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 8: Comparison of different bandwidth in the estimation of the effect of loan access on college enrollment.



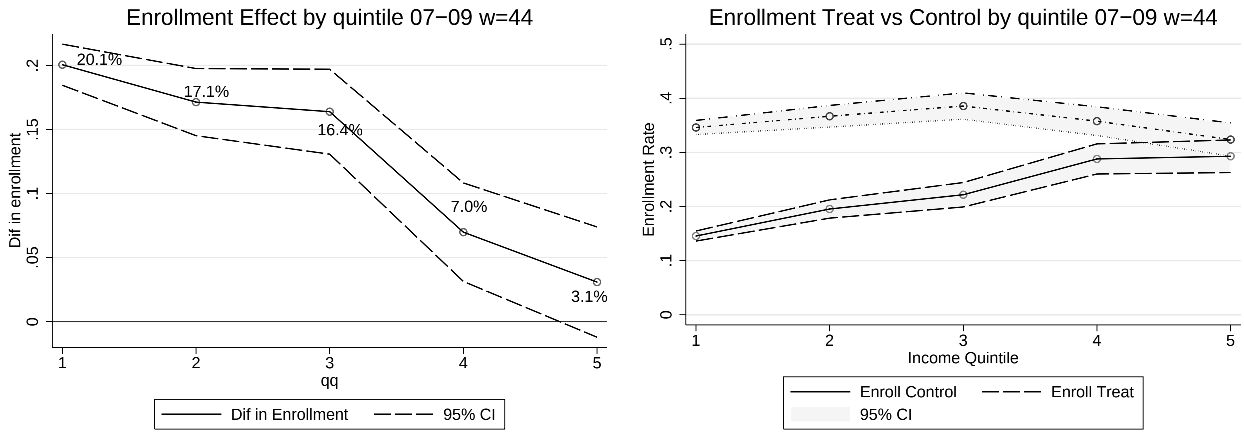
Note: The graphs on the top show the RD estimation of the effect of being eligible for loans on college enrollment using different bandwiths and 95% confidence intervals constructed using robust standard errors. The graph on the bottom show the relative increase in enrollment:

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

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 (1)), and 95% confidence interval using delta method standard errors.

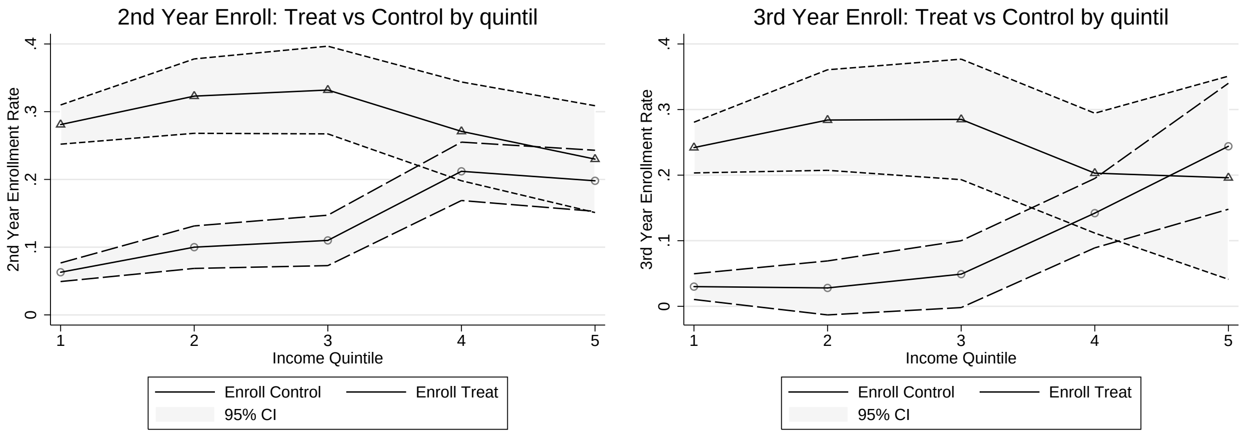
"I&K optimal bandwidth" refers to the optimal bandwidth $w = 44$, estimated using Imbens and Kalyanaraman (2009).

Figure 9: Enrollment rate by quintile years 2007 to 2009 pooled together.



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).

Figure 10: Income Gap for Second and Third year Enrollment Persistence.



Note: Both graphs show the estimation of the enrollment rate at each side of the cutoff by income quintile (and 95% confidence interval).

Figure 11: Estimating the price effects using grants.

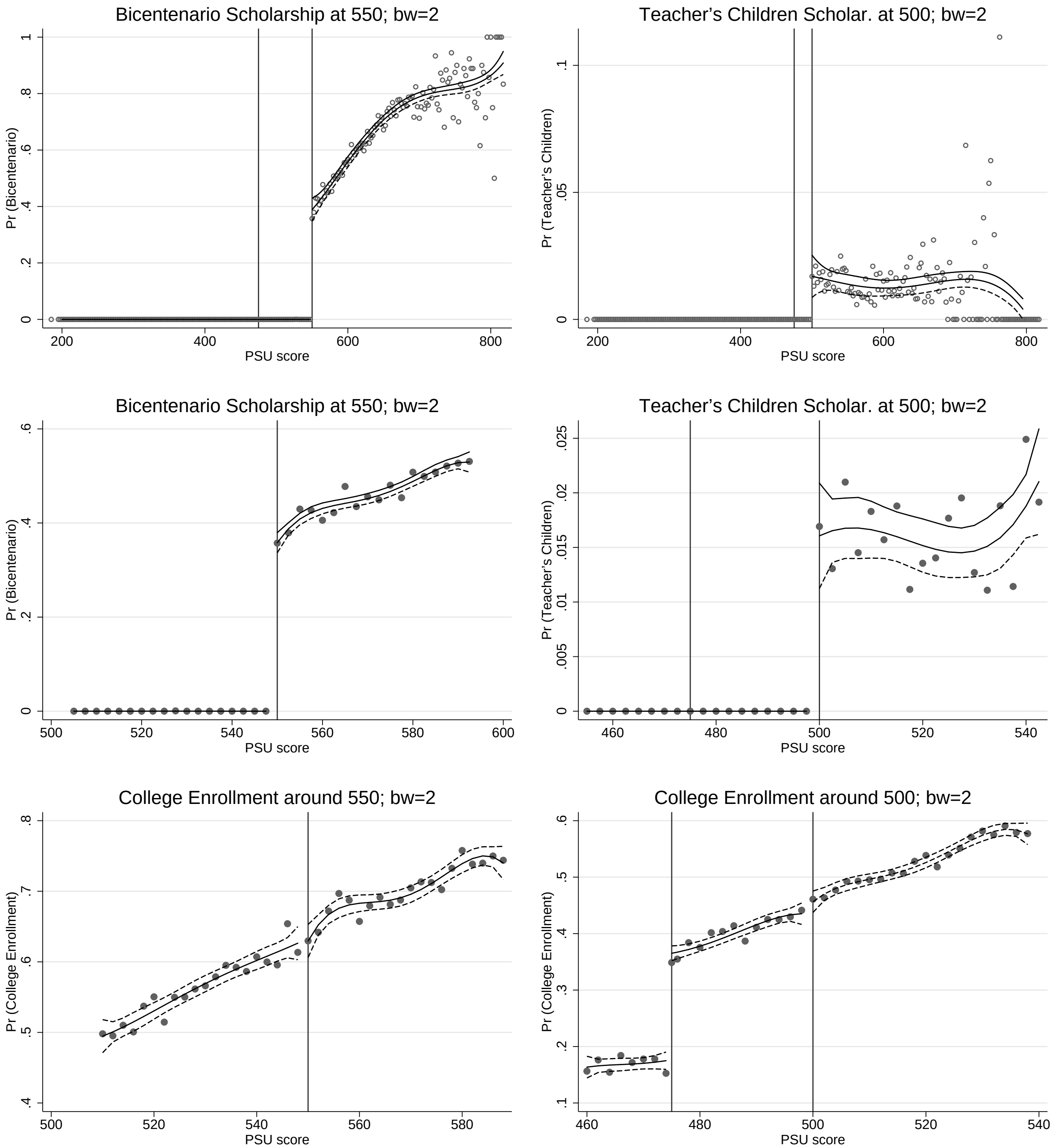
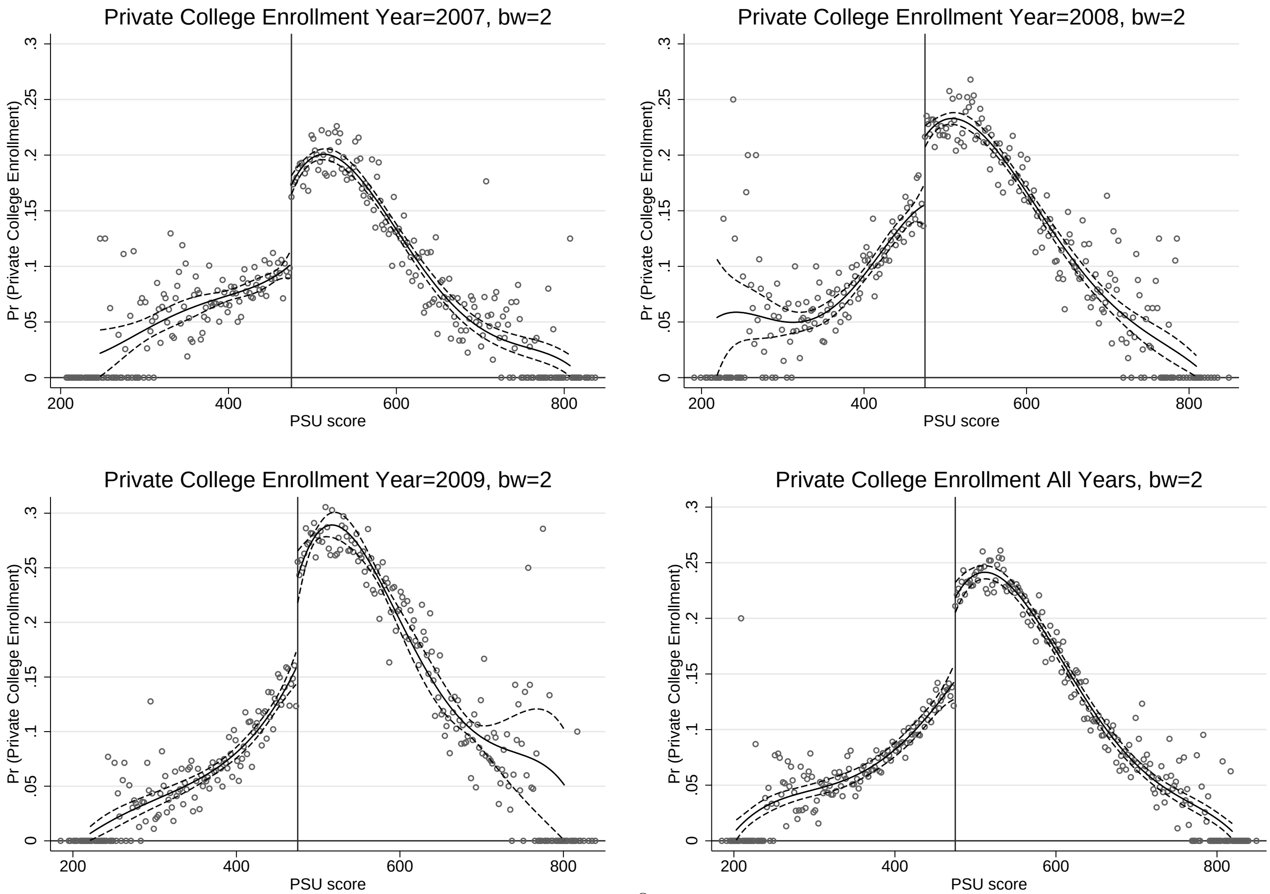


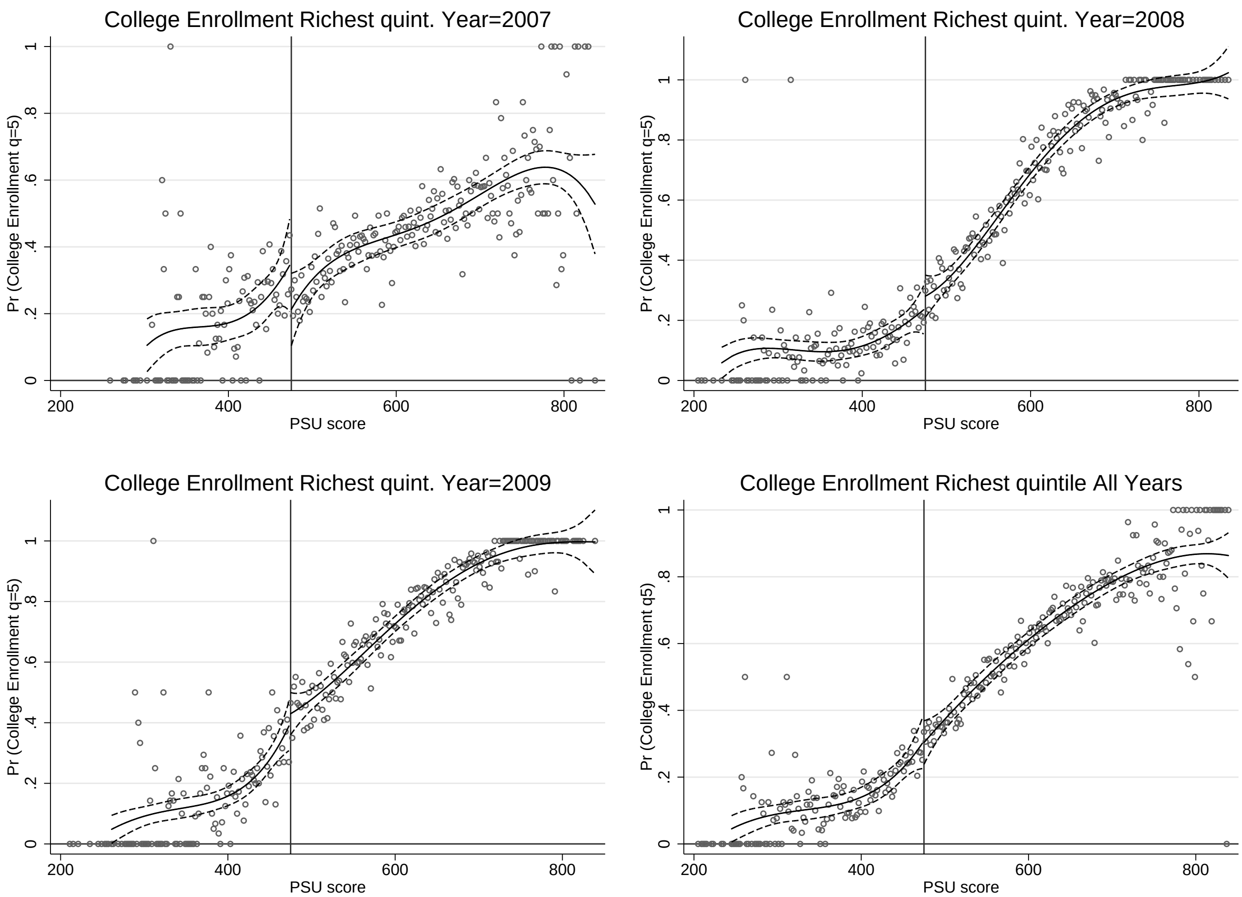
Figure 12: Enrollment Probability on private universities around the cutoff.



x^o

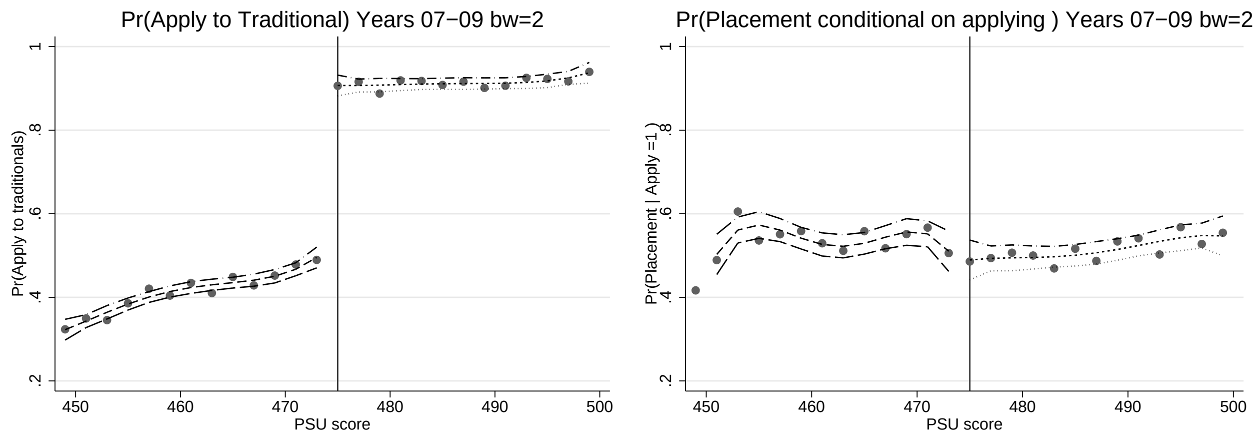
Note: Each dot represents average college enrollment in private universities in an interval of 2 PSU points. Each dot have on average 376 students who satisfy all requirements to be eligible for college loans and take the PSU immediately after graduating from high school. The dashed lines represent fitted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

Figure 13: Probability of college enrollment around the cutoff for students from the highest income quintile.



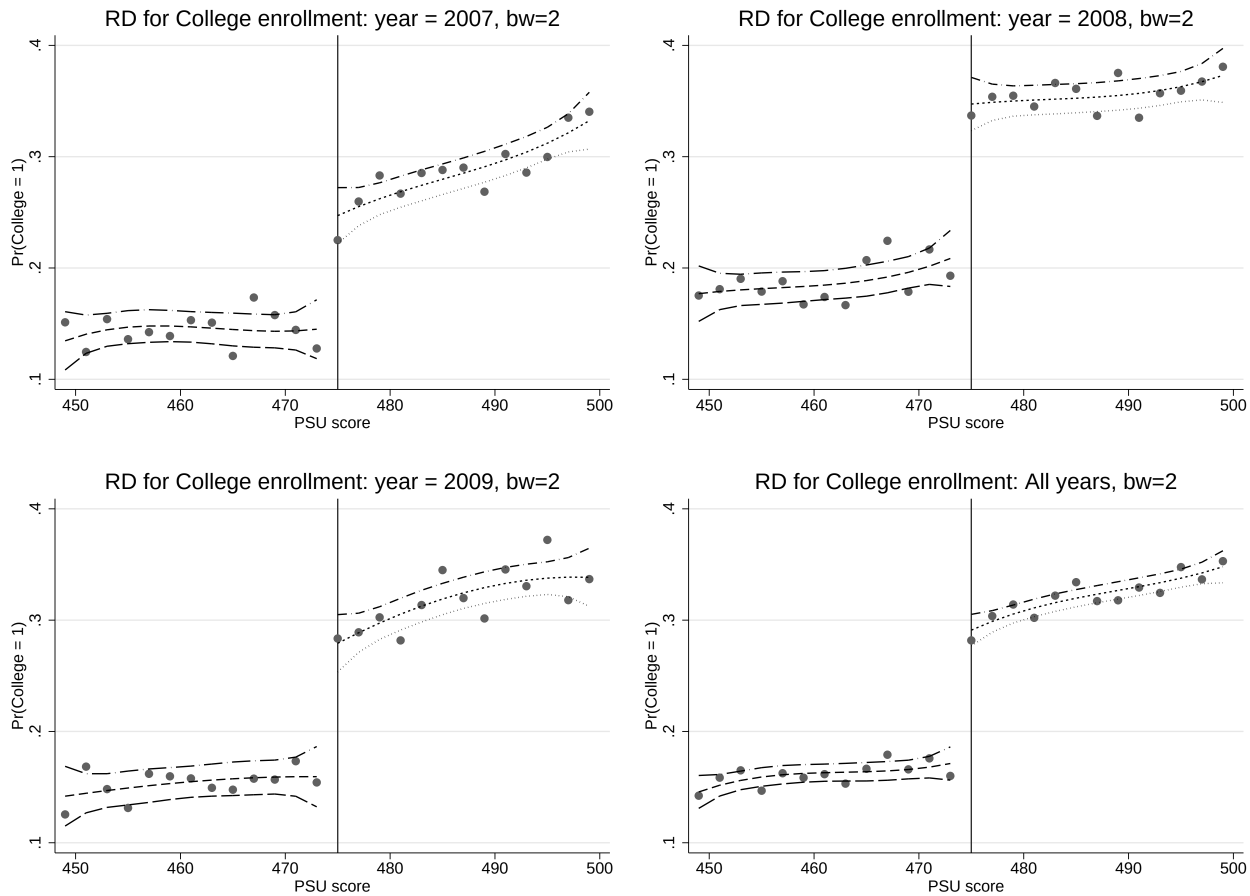
Note: Each dot represents average college enrollment for students in the fifth quintile in intervals of 2 PSU points. The dashed lines represent fitted values from a 4th order polynomial spline and 95% confidence intervals for each side. The vertical line indicates the cutoff (475).

Figure 14: 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 15: 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.