Credit Constraints for Higher Education

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Abstract

This paper addresses the importance of credit constraints explaining the gap on college enrollment between students coming from rich and poor families. Measuring the effect of credit constraints on college enrollment is a complex task due to the unobserved nature of credit constraints and the existence of other variables that affect college enrollment that are unobserved as well. This paper exploits a natural experiment that produces variation on credit constraints directly, analyzing two programs that give college tuition loans to students who score above a given cut-off in the national college admission test. This enables a regression discontinuity design that addresses the problems of unobserved omitted variables and self-selection. Moreover, the paper uses a rich and detailed data set from a nationwide admission process that does not rely on unobserved subjective variables to select students, eliminating potential biases from the supply side. With this exogenous variation on loan access, I estimate the causal effect of credit constraints on college enrollment. The college enrollment rate increases significantly for students that are eligible for loans, and is statistically the same for all income groups after the elimination of credit constrains.

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1 INTRODUCTION

There is a vast literature that attempts to explain the relationship between college enrollment and family income. Two points of view are used to explain this phenomenon. The first argues that the observed gap is a consequence of a long run differences in educational investment, both at home and in schools, in which higher-income parents give their children habits, preferences for education, better quality schools, books, and even better genes that increase their readiness for college (Carneiro and Heckman, (2002), Cameron and Taber (2004), Nielsen, Sorensen and Taber (2010) and others). The second argues there are short run credit constraints that affect students from poor families, thus preventing them from enrolling in higher education. (McPherson and Schapiro (1991), Kane (1996), Card (1999, 2001), Dynarski (2003), Belley and Lochner (2007), among others).

Measuring the effects of credit constraints on college enrollment is a difficult task mainly because credit status is unobserved. Most of the previous literature has relied on indirect methods that present at least one of the following limitations: they fail to identify the credit constrained subpopulation and they do not control for all unobservable characteristics that may explain enrollment.

Since credit constraints status is not observed, the literature has used changes in tuition cost, student aid, and other measures that affect returns to college education (they are not a loan) to measure the effects on college enrollment. These changes affect all members of the population and therefore identify changes on choices among those who were initially indifferent (or close to be) between college education and an outside option, without regard on credit constraints. The identification relies on the assumption that these changes affect credit constrained population differently: a change in college education costs should have a larger impact in college enrollment rate among poor students. These indirect methods find evidence that may be consistent with the hypothesis of credit constraints, but they cannot identify the effects of complete credit markets in college enrollment. Moreover, these conclusions may be driven by other factors that are not considered, such as the population density affected by the changes in college costs.

A second difficulty comes from the existence of unobserved characteristics that affects college enrollment. Unobserved variation from students and the admission processes. Students that enroll into college differ from those who don't in variables such as ability, family background, performance expectations, preferences for college education, etc. On the other hand, to select students, colleges ask requirements that are not observed by the econometrician such as, letters of recommendation, statement of purpose, etc., and moreover, colleges use financial aid to capture better students. All of these unobserved variables introduce potential biases to the estimation of the causal effect of credit constraints on college enrollment.

Additionally, the literature faces another limitation: detailed individual level data on enrollment and aid is limited and sometimes only available for a few colleges. Therefore, it is not possible to discriminate whether a student did not enroll because of either low aid or enrollment in another college.

As a consequence, the evidence shown in the literature is heterogeneous, with some papers showing evidence consistent with credit constraints and others showing little to nothing.

This work contributes to the literature in three ways. First, it uses a natural experiment that produces variation in credit constraints for college enrollment giving loans (not aid) to students in a market context, and therefore it measure the effects of credit constraints on college enrolment directly.

Second, the programs to be evaluated determines a cut-off score for college loans eligibility, lifting credit constraints as good as randomly around the cut-off, and therefore it deals with the omitted variables and selection into treatment biases present in some studies. Moreover the admission process analyzed depends exclusively on observed students' characteristics, avoiding potential biases from admission processes that weight subjective characteristics.

Third, it uses a rich, unexplored, individual level data set that 1) gives full information on enrollment decisions and loan assignment for all individuals and for all higher education programs in the country, 2) The financial programs offers a standard loan to eligible students who decide whether to accepted it, reducing the potential endogeneity of loan offers made to attract better students, and 3) the data contains full information on recipients of tuition loans as well as scholarships given by the main source of financial aid.

Assessing the importance of credit constraints is important from an educational policy perspective. Studies concluding there are no credit constraints suggest it is not necessary to implement programs that alleviate the financial burden for lower income students (Carneiro and Heckman, 2002). If the effects of credit constraints are not measurable due to the lack of reliable data and credible research designs, then the absence of policies would affect the performance of lower income students and their social mobility, and the effects would be transmitted to future generations.

The paper is organized as follows. Section 2 reviews previous literature and discusses the ideal scenario to measure the causal effect of credit constraints on college enrollment. Section 3 addresses the identification strategy. Section 4 presents the data, Section 5 displays the results and section 6 concludes.

2 BACKGROUND

Measuring the effects of credit constraints on college enrollment is a very difficult task because we need to deal with the influence of several unobserved variables. Credit constrained status is unobserved at the first place and enrollment is determined by several factors such as preferences, expectations, student's ability, etc. which are unobserved as well.

In the following subsections I review briefly how the literature has dealt with this problems and an ideal scenario to measure these effects are described.

2.1 Previous Literature

Since credit constraints are unobserved, the literature has come out with indirect methods that show evidence consistent with the credit constraints hypothesis, explaining the gap on college enrollment between high and low income families.

Some studies use reduced form econometrics to exploit variation in tuition costs and aid regimes. Generally, these attempts assume that students from previous years or from different regions that were not affected by the new aid scheme or change in tuition costs are comparable with those affected.

McPherson and Schapiro (1991) use time series to account for the relation between tuition costs and enrollment, comparing students before and after the introduction of the Pell Grant program in 1974; Kane (1996) interprets the delayed college entry in high tuition states among blacks and poor whites as evidence of credit constraints; Carneiro and Heckman (2002) introduce a measure of ability into the estimated equation using the National Longitudinal Survey of Youth of 1979 (NLSY79); Dynarski (2003) considers the effects of the elimination of the Social Security Student Benefit Program, which provided subsidies for students of deceased, disabled, or retired parents and compares students before and after the reform; and Belley and Lochner (2007) compare NLSY79 with NLSY97, discovering that previous findings are inconsistent with the new survey.

Another important indirect method attempts to estimate structural models. The researcher calibrates a model of choice using observational data and then simulates the decisions made by students, changing parameters such as tuition cost, parental bequest, etc. Outstanding examples of this are Keane and Wolpin (2001) and Cameron and Heckman (2001). Keane and Wolpin (2001) use NLSY79 to conduct counterfactual experiments to assess the effects of the credit constraints and parental transfers on access to higher education. Cameron and Heckman (2001) estimate a dynamic sequential model of schooling attainment using NLSY79 to improve measures of parental background; they then use it to simulate policies that reduce credit constraints.

A separate strand of the literature attempts to use individual level information, on financial aid given directly to students enrolling in college, nevertheless the evidence shown is indirect since aid implies a change in college education returns.

Nielsen, Sorensen and Taber (2010) use the variation due to an aid reform in Denmark that increases stipends for students coming from richer families to measure the effects on tuition cost in enrollment and use data on assets to measure potential biases from borrowing constrained population. They rank individuals according to observable income and match pre- and post-reform individuals at the same place on the income distribution and compare the enrollment rate. This approach assumes that enrollment depends only on the position of the income distribution (and then the level of the subsidy) and not on unobservable characteristics. Moreover, it is quite possible that their identification will not capture effects of credit constraints on enrollment since in Denmark colleges are tuition-free, stipends are very high, and the larger increases are applied to richer students as they clearly state in their paper.

A very important related paper is Van der Klaauw (2002), addressing the question on how important aid offers are attracting students to an East Coast college. He argues that colleges' aid is increasingly based on academic merit and is used to encourage the best admitted students to enroll in a given college, (which compete with other colleges for the best students), rather than being a tool to make college more accessible to students from low income families. This point adds a new source of endogeneity, because we cannot observe all the aspects of the admission process which may explain in part the difference in enrollment rate. In particular, he points out that the admission process depends on subjective variables that are unobserved by the econometrician, such as recommendation letters, statements of purpose, and extracurricular activities. He also brings out one of the problems faced by studies that use information from only one institution: there is missing information about other colleges' offers, outside opportunities, and whether a student decided to enroll in another institution or not to enroll at all.

Some of these attempts present at least one of two problems: they do not identify directly credit-constrained students (they don't use a variation in the credit constraint condition of students), and/or they cannot identify the proper counterfactual since they cannot capture the effect of all unobservable variables. As a consequence, the estimated parameters do not measure the effects of credit constrains on college enrollment or present omitted variable biases.

The main problem of indirect methods is that using changes in tuition costs, financial aid, etc. affect returns to college for all individuals in the income distribution. The observed change in enrollment comes from students that initially were indifferent (or very close to be) between college and the outside option, i.e. their returns to college education were zero (or close to zero), and a change in tuition, financial aid, alternative wage, etc., change that return to be positive. This literature relies on the fact that changes should be larger for credit constrained students, but this differential changes could also happen as consequence of differences in the population densities, i.e. More students from poor families are indifferent between college and the outside option.

Another way to find indirect evidence consistent with the hypothesis of credit constraints is comparing OLS and IV estimations of Mincer returns to education (Card 1999, 2001). Instrumental variables measure the effect on students which treatment status is affected by the instrument (Imbens and Angrist, 1994), but not necessarily the subpopulation that is constrained.

A second problem is the presence of unobserved characteristics that influence decision on college enrollment. Studies that compare outcomes in different years or different states are assuming that the subpopulations affected by the change in aid or tuition cost are comparable. Students from different years may be different because the introduction of a program may incentivize different type of students to participate in the enrollment process. Student's population on different states may reflect migration patterns looking for more affordable college institutions, in ways that are not captured by household surveys. If relevant unobserved variables are omitted, then the estimation is biased. Structural models that use observational data to calibrate the parameters suffer from the same problems mentioned above.

2.2 The ideal scenario to measure the causal effect.

The ideal scenario to measure the causal effect of credit constraints on college enrollment would be a randomized control trial (RCT) of credit constraints on a sample of students since randomization allows controlling for unobserved characteristics. The treatment group would receive loans to enroll in college while the control group would need to finance college by their own resources. The difference in enrollment rate between treatment and control groups gives the average treatment effect (ATE) of credit constraints on college enrollment.

An experiment randomizing credit constraints would be difficult to implement in reality, because the experiment would need to finance a significant amount of resources for several years and would need to implement an enforceable repayment method. If the promises to receive resources for all college years and/or the enforceable repayment method are not credible, students may alter their choices. In the first case, students would not enroll even though they are offered a loan, because they may not believe in the availability of fund for all years, and in the second case, if they don't believe in the enforcement method, they may interpret the experiment as a reduction in the education cost, which pushes them to enroll because of an increase in the returns to college education. Thus, an experiment like this is not free of identification problems and implies a long lasting and expensive effort.

Moreover, it is difficult to control and observe what students do outside the experiment. It is likely that students from high income families, assigned to the control group, get financing by their families, which would invalidate the results.

If these difficulties on implementing a RCT are true in reality, even an experiment would not be a feasible way to measure the effects of credit constraints in college enrollment.

3 IDENTIFICATION STRATEGY

The natural experiment exploited in this paper corresponds to two financial programs given in Chile that offers college tuition loans.

These two programs are available to any student who belong to the lowest four income quintiles[1] scoring at least 475 points in the national College Admission Test (Prueba de Selection Universitaria, PSU hereafter) which allows the implementation of a regression discontinuity design.

For each individual, a random term ξ is revealed the day of the test; some students get a ξ that put them just above the cut-off and some get a ξ that make them score just below 475. Since the realization of ξ is random, unobserved (and observed) characteristics are balanced in a neighborhood of the threshold. Finally, comparing college enrollment rates in this neighborhood of the cut-off gives the causal effect of credit constraints on college enrollment for these students from the lowest four income quintiles.

3.1 The Admission test, PSU

The PSU test consists in two mandatory tests on language and mathematics and two optional tests. The average on the mandatory tests is referred as *PSU score* which is considered for loan eligibility. The optional tests: History and Social Sciences, and Sciences (which includes modules on biology, chemistry, and physics) are not considered for loan eligibility, but they are considered for the placement score depending on each college program.

The tests contain only multiple choice questions which are answered on a special sheet that is graded automatically by a photo optical device. PSU scores are normalized each year to make them comparable with other years to a distribution with mean 500 and standard deviation of 110. The scores range from 150 to 850 points.

[1] The income quantiles are defined using percapita autonomous income defined by the last available National Household Survey (CASEN). CASEN 2009 defined the income limits between quintiles, which expressed in annual US dollar would be: poorest quintile (quintile 1) lower than \$1,443 (CH\$707,196); quintile 2, \$2,469 (CH\$1,209,768); quintile 3, \$3,914 (CH\$1,917,660); quintile 4, \$7,014 (CH\$3,436,788). Calculated using March 2011 exchange rate (490CH\$/\$). Source CASEN 2009, MIDEPLAN (Ministry of Planification).

The test is taken simultaneously in all of the country once a year and is used as a selection mechanism for almost all higher education institutions in the country.

To discourage random answers in the test, for each four incorrect answers one correct is deducted.

There is a fee to take the test (about \$50 or CH\$24,000 in 2010) which is waived for all students graduating from public and voucher schools that apply for. The test can be taken as many times as wanted. To avoid the learning effects that may imply score manipulation, we only consider students that take the test for the first time.

3.2 The loan programs

The first loan program is the State Guaranteed Loan program (Credito con Aval del Estado), which allows private banks to give loans to eligible students that are guaranteed by the State of Chile and by higher education institutions. Additionally students need to be accepted by an accredited institution.

From all 58 institutions that provide college education in Chile, 77.6% participate in the program. For the remainder, 19% are not accredited institutions and 3.4% have dropped out of the program. Some institutions ask for higher PSU scores to guarantee the loan as shown in table 5, but 85% of all programs requires 475 PSU score to be eligible.

The second loan program, the Solidarity Credit Fund (Fondo de credito solidario) was introduced in 1981 as part of an education reform. To be eligible for this loan, in addition to the two conditions mentioned before, students must enroll in one of the 25 traditional universities[2]. The loans are given by the university and could be complemented by the

[2]The colleges in Chile are classified in two types, traditional and private. Traditional universities are those that were providing higher education before the educational reform of 1981, some of them belong to the State and others are privately-funded. The 1981 educational reform allowed the creation of new

State Guaranteed Loan.

Table 7 shows all other scholarships given by the Ministry of Education. All of them are given to students depending on PSU scores, but none of them affect students scoring in a neighborhood of 475.

Individual colleges offer scholarships to attract best students. All of them require higher PSU scores than 475. For Example UDD offers discounts starting on 10% for students scoring 620 or more.

To be eligible of any benefits given by the Ministry of Education, the students apply using a unique application form (Formulario Único de Acreditación SocioEconómica, FUAS) before the PSU test. The family income information given to FUAS is contrasted with information from the Chilean IRS to rank students' family income and to determine eligibility.

3.3 Repayment Enforcement

The State Guaranteed Loan program determines that students start repayment 18 month after graduation in monthly installment for 20 years divided in three installment periods (low, medium and high). Private Banks gives the loans and they are in charge to the repayment process. The loan contract establishes that employers are mandated to deduct repayments directly from the payroll, and the law considers penalties to employers that do not comply with this process. The contract also allows the IRS to retain tax refunds in case the former student does not pay to the lending Bank. The interest rate is about 6% per year.

The higher education institution guarantees the loan in case of dropout: 90% of the capital plus interest for the first year, 70% for the second, and 60% for the third year

higher education institutions, which are known as "private universities".

onwards. The State guarantees up to 90% when the educational institution covers less than that percentage. In the event that a student stop paying after all mechanisms used by banks to collect debt, the guarantors pay to the bank and they become responsible to enforce the repayment.

The Solidarity Credit Fund is managed by the universities which are in charge of the collecting process. Repayment starts after 2 years of the student's graduation and installments, calculated each year, correspond to 5% of borrower's income. The cost of this loan is about 2% per year with a maximum of 15 years of payments. This loan scheme has a lower repayment rate of about 52-60%, but recently some effort has been made to increase it with measure such as a new law that allows the IRS to keep borrower tax refunds, and publicizing names of defaulter students.

4 DATA

The data comes from four data sets from three different institutions.[3] The first data set contain individual level PSU scores and socioeconomic characteristics that are self-reported by the students when they register for the test, such as family income, parent education, household size, city of residence, etc. It also includes high school GPA, school of graduation, and other school characteristics. The data comes from The Council of Chancellors of Chilean Universities (Consejo de Rectores de las Universidades Chilenas: CRUCH), which is the organization that implements the PSU process. It includes eight different PSU processes for the eight years from 2003 to 2010.

The second data set includes data at the individual level on enrollment. It comes from the Ministry of Education and includes enrollment program, and institutions for

[3]To process the data, I asked for the coordination of these institutions who merged the data. This coordination process took more than one year.

the period from 2006 through 2009.

The third data corresponds to the FUAS application form which gives individual level information on application to benefits given by the Ministry of Education of Chile, eligibility, income quintile and assignment to eight scholarship programs and the Solidarity Credit. The information has been collected by the Ministry since 2006 but I only have from 2007 onwards.

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

5 RESULTS

The results are organized in this section as follow. Subsection 5.1 test the condition for a valid RD: loan assignment around the cutoff, manipulation of PSU scores, characteristics balanced between the eligible and non-eligible students to test the local continuity assumption. Subsection 5.2 the results on the estimation of the causal effect are presented. Subsection 5.3 addresses some potential problems with the identification and finally, 5.4 present results by income groups.

On each year, an average of 211,000 students took the PSU test. Therefore, to estimate the RD parameters I am able to use a very small window around the threshold. To be conservative, all the RD results shown in the following sections would consider 2 PSU points around the threshold and to show that these results are not sensitive to bandwidth or functional form, a graph with fourth order splines will be given for all students scoring from 450 to 500 points.

5.1 Conditions for a valid RD design

The RD conditions for a causal estimation are reviewed in this sub section. In section 5.1.1 is shown the fuzzy nature of the loan assignment. Section 5.1.2 addresses the possibility of manipulation on the assignment variable, and Section 5.1.3 compares the balance among other covariates between control and treated groups to shed light about the local continuity assumption for the expected potential outcomes around the cut-off. (see Lee and Lemieux, (2009), Hahn et al (2001), and Van der Klaauw (2008))

5.1.1 Loan Eligibility

Figures 1 and 2 show that the probability of receiving a loan jumps discontinuously at 475.

Figure 1 shows assignment on the 2 programs for students around 475. The assignment rule was fulfilled for all years except 2006, the year of implementation.

Some problems happened in the year of implementation. Anecdotally, in that year, the Chilean IRS gave the information on income ranking students from 1 to N. This information was misinterpreted by the commission, who assigned loans beginning with the richest. When they figured out the mistake, loans were already announced and they had to assign a new number of loans for the poorest.

In all other years, the assignment rule has been fulfilled perfectly: no student scoring below 475 has received a tuition loan.

Figure 2 shows the probability of being eligible for a loan and 95% confidence intervals, with respect the PSU score for all years when the information about application for loans and income quintile are available (2007 through 2009). We can observe that scoring above the cutoff increase the probability of being eligible for loans from 0 to 55% for all years.

5.1.2 Local Continuity Assumption: Manipulation of the Assignment variable.

The local continuity assumption for the outcome expectation requires that the assignment variable is not manipulated. As explained in Section 3 the PSU test consider only multiple choice questions which are graded by an optical device, which imply that manipulation would be unfeasible. To verify the latter, Figure 3 shows the frequency distribution of PSU scores, a predicted value from a regression using a fourth order spline and 95% confidence intervals. Each dot indicates the number of students with scores in an interval of 2 PSU points, for instance, the first dot to the right of 475 indicates the number of students that scored on the interval [475, 477). The number of students scoring above and below 475 are statistically the same as shown by the intersection of the confidence intervals and also shown by the last row in table 9 which confirms that PSU scores are not subject to manipulation.

Nevertheless, the test can be taken as many times as wanted, and therefore students may try over until they get a score over 475. As a consequence in the all sections, only students taking the test for the first time are considered.

5.1.3 Local Continuity Assumption: Balance Among Covariates.

As a second check for the local continuity assumption we need to show that there is no other variable that is causing a discontinuity in the outcome around the cut-off. Section 3 mentioned that no other aid or loan program was influencing the financial conditions for students in the vicinity of 475, which is shown in Table 7. Here we check the influence of other variables to verify that the loan assignment is as good as random.

In Table 9, we see t-test for the difference in means of selected covariates and the t-values in parentheses. In 2006, all characteristics are not significantly different between both groups, except for high school GPA, which show that students above 475 have a greater high school GPA at 10% significance. No other year shows these characteristics being different between groups. In 2007, all covariates are balanced. In 2008, we see that students scoring above work less than their counterparts. Finally, in 2009, we observe that there are 4% less women above the cutoff and they have a higher probability of being married, but the significance is 10%. All other characteristics are equal between groups.

5.2 MAIN RESULTS

In figure 4, we observe the effect of crossing the cut-off on college enrollment. We observe college enrollment rate by year around the cut-off using bins 2 points wide with fitted values and 95% confidence intervals using fourth order splines. In the four years, students who score more than 475 and become eligible for a loan enroll with a higher probability in college.

The interpretation of this figure is that the elimination of credit constraints for students who cross the threshold caused an increase in the enrollment rate of roughly 50%: from 15% to 25% in 2007, from 19% to 30% in 2008, and from 17% to 25% in 2009.The only exception is 2006, when the increment was from 28% to 32%.

In Table 10 we observe results for the RD estimation suggested by Imbens and Lemieux (2008):

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

Where T_i is the PSU score for student i and τ is the cut-off. The parameter of interest,

β_1, is highly significant in all years, except the year of implementation, 2006.

This regression considers only students in a window of 2 PSU score. The results are robust to the inclusion of all covariates and the inclusion of region fixed effect as indicated in panel B and C respectively.

In 2006 we observe a positive effect but not significantly larger than zero. This result may be consider an placebo test, since in this year loans were given to students below the cutoff and to students from the highest quintile, as explained before.

From 2007 to 2009, when the loan assignment was done correctly, we observe a highly significant parameter around 12%. The overall effect for all the years with correct implementation is in column (6), and correspond to an increase in 11.4% en the probability of enrollment

In the last row we can see the average enrollment rate for the control group, i.e. students with PSU scores in the interval [473, 475). The effect shown in panel C column (6) corresponds to an increase of 56% of the baseline probability of enrollment.

This difference in enrollment is the average treatment effect of credit constraint on college enrollment for students around the cut-off.

5.3 ROBUSTNESS

This section analyses two possible scenarios that may invalidate the expected outcome local continuity assumption and therefore previous results: colleges may have incentives to select students based of their loan eligibility, and these financial programs may be considered as a reduction in college cost affecting returns to college education.

In the first case, colleges may offer more places to students above the cut-off, because these financial opportunities imply that they are more likely to finish a degree: Students can avoid working while studying, they will have secure financial resources for the whole period, etc. In the second case, these conclusions wouldn't be valid if students consider these loans as mere subsidies or conditional transfers: If becoming eligible for these loans imply a reduction in college cost (an increase in returns to college), then the observed discontinuity may not be a consequence of credit constraints elimination. This may happen if the repayment rateis low, the enforcement promises are not credible, or if the interest rate does not correspond to a complete market scenario.

5.3.1 Are colleges selecting students differently around the cut-off?

The conclusions would be wrong if colleges select students depending on their loan eligibility condition. To rule out that possibility I present three robustness checks. The first use information on applications and placement on traditional universities to show that these programs cause a discontinuity on students' application to college rather than a discontinuity in the probability of being admitted to a specific program. The second, using enrollment for private colleges, shows program's cut-offs scores for those college programs chosen for students above 475 to check that the availability of programs is the same for both groups. The third, uses information on both types of colleges but only for students that belong to the highest income quintile, to show that colleges, who do not observe student's income, are not differentiating students based on their eligibility for these loan programs.

Applications and placement for traditional colleges

The first data set mentioned on Section 4 comes from the organization in charge of the PSU process, the Council of Chancellors of Chilean Universities (traditionals). This organization originally implemented the PSU process as a centralized mechanism to offer positions to their programs, and later all other higher education institutions engaged using this test to offer placement as well. The centralized process collects information on students' characteristics, as mentioned before, but also contains student's application forms, when they apply to this type of universities. In the student's application they rank up to a maximum of 8 programs, and the data set contains information on students' ranks, and their placement condition for each preference.

The centralized placement process starts offering a position to the best PSU score student, on her highest preference. The process continues with the following students until a program is complete. All other applicants become part of the program's wait-list and the process continues by placing best score students on their highest preferences until all programs are complete or all students are assigned.

I use this information to show that college placement is locally continuous at the cut-off and the discontinuity is driven by students who score more than 475 and became eligible for one or both loan programs. After students apply, the probability of being placed will depend on the relative position on the list of applicants which, around the cut-off, is not discontinuous, if colleges are not considering the cut-off to offer placement.

To show that, the same regression discontinuities are run with valid applications and placement conditional on having applied to traditional colleges as a dependent variables.

Table 12 shows results for the following regression using all student in a 2-PSU-points neighborhood around the cut-off:

$$Pr(Apply_i^{Trad} = 1) = \gamma_0 + \gamma_1 1(T_i \geqslant \tau) + \gamma_2(T_i - \tau) + \gamma_3 1(T_i \geqslant \tau) \cdot (T_i - \tau) + \zeta_i \quad (2)$$

Where $Apply_i^{Trad}$ takes value 1 if a student i applied to any program from a traditional university. T_i is student i PSU score, τ is the cut-off of 475 and ζ_i a mean zero error term.

The first 3 columns show the result for each year from 2007 to 2009 and columns 4 to 6 show all years together including covariates and years dummies. In column 4, we observe that the probability of application increase in $\hat{\gamma}_1 = 17.2\%$, for those who are eligible for loans and in the last column the increase is $\hat{\gamma}_1 = 15.2\%$, when 10 covariates and year dummies are included.

To show that traditional colleges are not selecting students depending on their loan eligibility, Table 13 shows the probability of being placed conditional on having applied to a program. Specifically Table 13 is showing the regression discontinuity for students within a 2-points window around the threshold:

$$Pr(Placed_i = 1 | Apply_i^{Trad} = 1) = \phi_0 + \phi_1 1(T_i \geqslant \tau) + \phi_2 (T_i - \tau) + \phi_3 1(T_i \geqslant \tau) \cdot (T_i - \tau) + \xi_i \quad (3)$$

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

Columns 1 to 3 show the results for each year and columns 4 to 6 show all years pooled together adding covariates and year dummies as in the previous table. We can see that there is no discontinuity around the cut-off for any regression, the parameter $\hat{\phi}_1$ is not significantly different than zero.

To show that these results are not sensitive to bandwidth or the inclusion on high order splines, Figure 6 shows the result of these two tables adding a fourth order spline. The figure on the left shows the discontinuity on applications around the cut-off, while the figure on the right shows the probability of placement conditional on having applied.[4]

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

Program's cut-off on private colleges

To extend the analysis to private colleges, I present a second exercise. The data available gives information about all enrolled students by program. This information can be used to compute the score for last student enrolled in a program (program cut-off) to see if the programs chosen by students above the cut-off are available for students below the cutoff.

Table 14 shows the percentage of programs chosen for the treatment group that have a program cut-off below 475, i.e. are available for students in the control group. The treatment group corresponds to all students that scored in the interval $[475, 477)$, and the control group all those who scored in $[473, 475)$. Therefore this table is showing that more than 92.5% of the students in the treatment group are enrolling in programs that would place a student in the control group in the event she would decide to apply. This table shows that, if colleges are selecting students around 475 based on their financial condition, this action have no significant effect on program availability for students around the threshold. Some colleges may have decided to only accept students with PSU score above 475 (or any other score), but students always have the chance to apply to programs that are available to both groups

Even though the percentages in Table 14 are very high, they are statistically different than 100%. Therefore, to see the effect of this different program availability between groups, Table 16 shows the same regression discontinuity shown before, but eliminating from the sample, all students in the treatment group that enrolled in a program with a program cut-off larger than 475, i.e. not available for the control group.

Column 1 shows the effect for all three years pooled together. Again, the effect of the partial elimination of the credit constraints on college enrollment is significant, and equal to 8.2% which correspond to an increase of 41% in the average enrollment for the control group.

To see if these results depend on the bandwidth chosen or the functional form, figure 7 shows the regression discontinuity for each year separated and the years pooled together including a fourth order polynomial spline. The results are the same and they do not depend on the bandwidth neither on the functional form.

Placement for non-eligible

Another way to rule out that colleges discriminate students around the cut-off based on their financial condition is observing the enrollment rate for students from the highest income quintile. Since they are not eligible for loans, college enrollment rate should be the same for students above and below the threshold. Universities do not observe student's income[5] when they offer placement, therefore they cannot discriminate whether a student is credit constrained or not. If colleges are discriminating out students below 475, we should see a discontinuity on 475 for all income groups, including those from the highest quintile.

Table 18 shows the same regression discontinuity but only for students from the richest income quintile. The results indicates that there is no discontinuity around 475 for this income group, but the number of observations is low because, only few students from high income families apply for benefits since they know a priori the quintiles limits to be eligible.

To have a broader picture, Figure 8 depicts the regression discontinuities for the different years and for all years together using bins 2 points wide and fourth order splines. This figure shows that there is no difference in enrollment around the cut-off. The large, positive significant at 10% effect for 2009 shown in Table 18 is a small-sample consequence

[5]Beyond a self reported income category which have a correlation of .4 with the income quintiles reported by the IRS

rather than a discontinuity. This is evidence of colleges not selecting students based on their financial condition.

5.3.2 Subsidy vs. Loan

Another potential identification problem would be if these loan programs are perceived by students as subsidies or conditional transfers that lower educational costs and therefore imply a larger return to college education. Therefore being eligible for these loans also implies higher returns to college.

If students believe they do not have to pay back, or they expect that the debt will be forgive, or the interest underlying these loans is significantly lower than a market alternative, then the enrolling decision may be driven by a perceived increase in the returns to college education.

To rule out this possibility, I use the fact that both programs have different cost and different expected repayment rates.

The solidarity credit, given only to student enrolling in traditional universities, is managed by each university using different criteria to select the amount given to the beneficiary. The universities in the first step and a central organization (Fondo solidario) in the second are in charge of collecting debts. Since neither the universities nor the central organization are specialists in collecting loans, this scheme ended with a low repayment rate which goes from 52 to 60% for the years here considered. Nevertheless, in the last years, the Chilean Government has run some modifications that allow the Chilean IRS to retain tax refunds of defaulter students, which has increased the repayment rate since this change in 2002 to 80% (in some cases) of all reprogrammed loans.

As mentioned in Section 3.3 this repayment starts after 2 year of the student graduation who pays monthly installments that correspond to the 5% of her income for 15 years, and after that period the debt is written off.

All of these characteristics indicate that there is a subsidy component in this loan scheme that may confound the effects mentioned in the previous section, because the students scoring above the cut-off would face a reduction in the cost of college education, moving them to apply and enroll more often.

On the contrary, the second program is very similar to loans that exist in the market, it has an interest rate of roughly 6% which corresponds to the government long run interest rate[6]. Specialized private banks are responsible for lending and collection of loans. Moreover, it was created with some legal clauses to ensure a higher repayment rate. The two most important are, first, employers are mandated to discount from the pay check the monthly amount indebted and to pay directly to banks; second, the Chilean IRS can deduct from tax refunds any amount indebted by a defaulter student. This last characteristic has proved to be an efficient measure increasing the repayment rate in the solidarity from 2002.

Additionally to the previously mentioned measures, private banks can use all the mechanisms that exist in the law to recover the debt which include releasing information to credit score institutions, asset impound, and judicial collection. Releasing information is important for the labor market in Chile, because many firms ask the potential employee not to have defaulted debts or not to appear as a defaulter in the credit score files in order to be hired for a job.

If all the mechanisms fail, the State pays to the bank and now the State starts the collection process again.

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

In order to compare the State Guaranteed Loan with available market loans, I briefly describe the latter here. There are two types of loans given by private banks: the first is called Corfo loan ("credito Corfo") and is lent by private banks to students with collateral assets. The resources come from a development corporation in Chile, Corfo, who lend money for this purposes to private banks which managed the process. The second is given by Banestado, a private bank with partial ownership by the State of Chile.

The Corfo loan has an interest rate that varies among banks, going from 6.8% to 8.5% annually. It requires a bank guarantor person who needs to certify a good credit record, to be employed, to have a regular income source, to have assets to use as collateral and to have a minimum family income of $1,225 or CH$600,000, corresponding to an average family on the third income quintile. The repayment period is 10 years, and the loan size depends on the program and the college enrolled.

The loan given by Banestado is aimed to lower income families, starting from incomes in the middle of the second income quintile ($714 or CH$350,000))[7]. It has an interest rate between 6.6% and 6.8% annually, requires a bank guarantor person who, as before, need to certify employment, good credit record, a steady income source, and assets to use as collateral. It has a maximum repayment period of 15 years and offers different grace periods up to 2 years.

The State Guaranteed Loan program ask for a similar costs but there is no need of a guarantor, with income and assets, since that role is played by the State of Chile and the educational institutions while the student is studying. This program was designed to give a market alternative to students that did not have access to the solidarity loan, especially those in private institutions. This suggests that this program is a very similar

[7]Using the quintile limits mentioned before, and the average family size for each income quintile (4.5 member), gives the following monthly income thresholds: first quintile, 0 and 540 (CH$265,000); second quintile, lower than $925 (CH$ 454,000); etc. The minimum family income required to apply to this loan is $714 (CH$350,000).

to a market loan therefore cannot be considered as a subsidy or conditional transfer.

If students are enrolling because an increase in college education returns we should not observe a discontinuity around the threshold for those who have access only to the State Guaranteed Loan program.

The following table and figure show the regression discontinuity for college enrolment analyzing students only receiving the State Guaranteed Loan in private colleges. The following analysis will exclude all students enrolled in traditional universities because the Solidarity loan was available on these colleges. As a consequence, we should observe a smaller effect since we are only eliminating students enrolled in college, at both sides of the cut-off, however above the threshold the enrollment probability is larger.

Table 17 shows the effects of trespassing the loan cut-off only for students on private colleges. The first column shows the effect for all years since 2007 through 2009 pooled together, and the following columns show each year separately. The results show that the partial lifting of credit constraints for these students represented an increase in the enrollment probability of 7.6 percent points which compared with the enrollment rate for the control group implies an increment of 76% on the probability of enrollment.

Finally, to show the effects in perspective, Figure 9 shows the same regression discontinuity for all scores between 450 and 500 using a fourth order polynomial. The results seem significant and robust.

Considering only this program that is very similar to a market loan, but without the guarantor requirement, we observe a positive and highly significant effect on the enrollment rate. This evidence indicates that the effects mentioned in the previous section are not due to increments on college education returns, but to the elimination (partial) of credit constraints.

In the next section, similar results will be shown by income quintile, and the conclusions will follow the same line. The income groups that did not have access to credit market before the introduction of these programs, are who benefit more, while those who did have access to a credit market only benefit marginally.

5.4 HETEROGENEOUS EFFECTS

5.4.1 By Income Quintile

Table 19 shows the effects by income quintile for years from 2007 to 2009.

The information on income quintiles is available only for students who applied to the program (self-select), which allow the determination of LATE (Hahn et al, 2001; Battistina and Rettore, 2008).

The lowest income quintile is the one that benefit most, as expected, since there is no option in the credit market for this group. The LATE effects for this income quintile range between 19% and 24%. Column (4) shows the overall effect for all years from 2007 to 2009, the elimination of credit constraints caused an increase on the probability of enrollment of 20.9%. The enrollment probability for the control group by income are 13,9%, 16.2%, 20.4% 27.8% and 26.5% for the poorest to the richest quintile respectively. This means for the first quintile that without access to tuition loans, college enrollment rate is 13.9 percent points, with the elimination of the credit constraint that rate jumps to 34.8 percent points, an increase of 150% increase in the enrollment rate.

The enrollment effects are large and significant for all the eligible quintiles, decreasing from the lowest quintile to the fourth. The fifth quintile is not eligible for loans and, as expected, shows no significant effect of the introduction of the programs.

Similar results are shown in figure 10 and 11. This figures show the difference in college enrollment rate for students in different income quintiles. The figure on the left shows the jump on enrollment at the discontinuity by quintile, while in the figure on the right the enrollment rate for treatment and control. It is necessary to consider that the number of students in the fifth quintile is very low ranging from 67 in 2007 to 91 in 2008. This happens because students from this income quintile knew a priori that they were not eligible for loans, they did not apply, and therefore they are not considered in the data set.

In the graph on the right, we can observe that the enrollment rate is very steady among income groups for eligible students. Roughly 40% of unconstrained students (above 475) enrolled into college.

The most striking result on this paper is that the enrollment rates for all income quintiles are not statistically different, after the inclusion of these programs that eliminates credit constraints, which indicates that the difference in the college enrollment rate between students from poor and rich backgrounds is highly explained by problems in the access to credit markets of poor students.

Figure 11 describe the results for all years together (from 2007 to 2009) showing that the effects are highly significant for the 3 first quintiles, and significant at 5% for to the fourth. These results are consistent with the fact that the pooorest income quintile did not have access to credit markets for students enrolling in private colleges.

Again, figure on the right shows that the enrollment rate is not different among income groups if credit constraits are eliminated, around 35%, while for the control group, the richest quintile more than doubles the enrollment rate from the poorest.

6 CONCLUSION

Measuring the effects of credit constraints on college enrollment is a very difficult task, because the unobserved nature of credit constraints and other variables that influence enrollment decisions. This paper exploited a natural experiment that eliminates credit constraints to students using an assignment rule which enables random variation, allowing measuring the causal effects of credit constraints on college enrollment directly and using a rich individual data set in a national admission process that uses only observed characteristics to place students in the different programs.

The programs State Guaranteed Loan and Solidarity Loan offer college tuition loans to students coming from the lowest four income quintiles that score more than 475 points in the national college admission test (PSU), enrolling in accredited colleges. This test cut-off is used to implement a regression discontinuity designs to estimate the causal effect of credit constraints on college enrollment.

Section 5.1.1 shows that the financial programs in Chile analyzed here are a natural experiment that assigns students to college tuition loans as good as random. The control group is not eligible to receive college tuition loans; therefore they must rely on their own resources into enroll to college. Meanwhile the treatment group receives access to loans.

In section 5.2 we observed that the elimination of the credit restriction has a significant effect on college enrollment. Students who are eligible for tuition loans increase their enrollment rate in 56% (from 20.5% to 31.9% in the year 2007 through 2009).

Section 5.3 dealt with two potential problems: if colleges select students based on their financial condition and if the loan contains a subsidy component.

To show that colleges were not selecting students depending on their loan eligibility, three exercises were applied. First, it was shown that the inclusion of these programs only affected students' choices: students apply more after knowing they were eligible for loans, while the probability of being placed conditional on having applied was the same for the treatment and the control group. Secondly, it was shown that more than 92.5% of the programs chosen by students on the treatment groups (on private colleges) were also available for students in the control group around the cut-off. Moreover, running a RD for the subsample of programs that were available for both groups, gives a high, positive and highly significant effect. Lastly, it was shown that colleges were not selecting students based on their financial condition by contradiction, using students from the highest income quintile, who were not eligible, and the fact that colleges do not observe student's income: If colleges select based on loan eligibility, we should observe a discontinuity around the cut-off for all income groups, but as shown, there is no jump for students from the highest income quintile.

To rule out the potential confounding of a reduction in college costs, the same regression was run considering only private universities and State Guaranteed Loan because its similarities with market loans available in Chile, which implies a small to no subsidy component. These results again indicate an important increase in the enrollment probability when the credit constraint is eliminated: Student above the cut-off increased their enrollment probability in 76% with respect to the control enrollment rate.

Section 5.4 shows that these effects are stronger among the poorest, they increase their enrollment rate in 150% with respect to the control group enrollment rate.

More importantly, when credit constraints are eliminated for students above the cut-off, enrollment srate for all income groups are the same, about 35%, while for the control group, the enrollment rate for the richest more than doubles the rate for the poorest. This evidence suggest that the gap in college enrollment observed in many countries, between students from poor and rich families, is a consequence of imperfect access to credit markets among the poorest.

All these results are strong evidence of the importance of credit constraints in college enrollment.

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8 TABLES

8.1 Comparing Evidence from Chile with the rest of the world.

Table 1: Higher education enrollement rates and tuition cost relative to per capita GDP

	I	II	III	IV
	(1)	(2)	(3)	(4)
College Cost/GDPpc	-.052	-.043		
	(.013)***	(.013)***		
Av tuition/GDPpc			-.188	-.183
			(.052)***	(.046)***
Av life expens/GDPpc			.011	.020
			(.026)	(.023)
Tert Expend/GDP		.172		.174
		(.067)**		(.060)***
Const.	.564	.341	.597	.373
	(.040)***	(.103)***	(.039)***	(.092)***
Obs.	39	36	39	36
R^2	.286	.434	.407	.569

Dependent Var: Enrollment rate(Enrollment/tertiary pop age).
Data Sources: Tuition and other college expenses from The International Comparative Higher Education Finance and Accessibility Project. Enrollment, Population on Tertiaty education age from UNESCO Institute for Statistic. Per capita GDP from World Development Indicators database, World Bank.

$$Enrollrate_j = \beta_0 + \beta_1 \left(\frac{Tuition}{GDPpc}\right)_j + \beta_2 \left(\frac{LivExpenses}{GDPpc}\right)_j + \beta_3 \left(\frac{ExpendOnTertEd}{GDP}\right)_j$$

Table 2: Mincer Returns to Education

	Social		
Region	Primary	Secondary	Higher
Asia	16.2	11.1	11
Europe*/Middle East/North Africa	15.6	9.7	9.9
Latin America/Caribbean	17.4	12.9	12.3
OECD	8.5	9.4	8.5
Sub-Saharan Africa	25.4	18.4	11.3
World	18.9	13.1	10.8
Chile (1989) (**)	8.1	11.1	14

Source: Psacharopoulos and Patrinos (2002)
(**) Psacharopoulos (1994)
(*) Non-OECD.

Table 4: Different rates of return to education: High School, Vocational and College. OLS CASEN 2006

	I	II	III	IV
	(1)	(2)	(3)	(4)
Years of Educ	.085	.088	.064	.064
	(.0006)***	(.0008)***	(.001)***	(.001)***
edsup x Years Educ.			.130	
			(.004)***	
college x Years Educ.				.146
				(.005)***
voca x Years Educ.				.041
				(.008)***
Exper, Exper2	N	Y	Y	Y
Covariates	N	Y	Y	Y
Obs.	106360	106360	106360	106360
R^2	.149	.311	.323	.324

8.2 Admission Process Characteristics

Table 5: Colleges Extra requirement for the State Guaranteed Loan.

cut-off	475	500	520	550	570	580	600	625	650	660	TOTAL
Traditional	12	3	0	0	1	1	1	1	2	1	22
Max no of loans	7,572	393	-	-	100	100	150	500	500	285	9,600
%	79%	4%	0%	0%	1%	1%	2%	5%	5%	3%	100%
privates	11	6	2	2	0	1	0	0	1	0	23
Max no of loans	19,750	1,620	400	513	-	120	-	-	50	-	22,453
%	88%	7%	2%	2%	0%	1%	0%	0%	0%	0%	100%
Total Institu	23	9	2	2	1	2	1	1	3	1	45
Max no of loans	27,322	2,013	400	513	100	220	150	500	550	285	32,053
%	85%	6%	1%	2%	0%	1%	0%	2%	2%	1%	100%

Table 7: Requirement for scholarships

	PSU cutoff	Type of U	Income Quintil	HS GPA	Loan Cost
College credit	**475**	Traditional	1, 2, 3 & 4		2% year
State Guar. Loan	**475**	All Accred.	1, 2, 3 & 4		apx6% year
Bicentenario	550	Traditional	1 & 2		
Juan Gomez	640	Traditional	1 & 2		
Teacher's children	500	Traditional	1, 2, 3 & 4	5.5	
Pedagogy Students	600	All		6.0	
Excellence		All	1, 2, 3 & 4	best 5%	
PSU score	Nat'l/Reg'l	All	1, 2, 3 & 4		

8.3 RD preliminaries

Table 9: Balance among covariates

Window: 2 PSU points around the cut-off

Year(dif/(t))	2006	2007	2008	2009
income	0.01	-0.02	-0.14	0.03
	(0.37)	(-0.47)	(-1.5)	(0.46)
1(married)	-0.01	0.01	0	0.01
	(-1.09)	(1.22)	(-0.56)	(1.83)*
1(female)	-0.01	0.03	0.02	-0.04
	(-0.55)	(1.53)	(0.8)	(-1.88)*
1(work)	-0.01	-0.01	-0.03	0.00
	(-0.28)	(-0.44)	(-2.03)**	(0.13)
HS GPA	0.89	-0.02	0.24	-0.58
	(1.87)*	(-0.06)	(0.57)	(-1.42)
Fat. Educ	0.08	0.07	-0.05	-0.2
	(0.6)	(0.61)	(-0.35)	(-1.52)
Mot. Educ	-0.02	0.02	-0.09	-0.15
	(-0.17)	(0.23)	(-0.84)	(-1.44)
School Type	-0.04	-0.02	-0.01	0.03
	(-1.1)	(-0.64)	(-0.4)	(1.25)
No HH members	-0.11	0.11	0.03	0.02
	(-1.06)	(1.29)	(0.36)	(0.54)
N	1682	2150	2187	2285

8.4 Reduced Form Result

Table 10: Main result: RD College Enrollment. By year and all sample. $\epsilon = 2$ PSU points.

Dependent Var. : College Enrollement	2006	2007	2008	2009	All	07-09
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without covariates						
1(PSU>=475)	.054	.152	.126	.129	.099	.126
	(.042)	(.038)***	(.035)***	(.030)***	(.018)***	(.019)***
Cons.	.261	.102	.168	.104	.168	.129
	(.033)***	(.031)***	(.026)***	(.024)***	(.014)***	(.015)***
R^2	.002	.017	.015	.022	.012	.017
Panel B: With covariates						
1(PSU>=475)	.066	.162	.128	.123	.105	.134
	(.042)	(.038)***	(.035)***	(.030)***	(.018)***	(.019)***
R^2	.047	.038	.044	.056	.038	.044
Panel C: With covariates and region Fixed effects						
1(PSU>=475)	.067	.159	.124	.114	.099	.128
	(.041)	(.038)***	(.034)***	(.029)***	(.017)***	(.018)***
R^2	.118	.071	.096	.104	.083	.083
Obs.	1682	2150	2187	2280	8299	6617
Mean enroll. control	.283	.14	.181	.141	.181	.155

Covariates: 1(work), household size, income category, health insurance type, father education, mother education, 1(father work), 1(mother work), high school GPA, and 1(female). Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 11: RD for college enrollment for students preselected for loans.

Dependent Var. : College Enrollement	c2007	c2008	c2009	c0709
	(1)	(2)	(3)	(4)
Panel A: Without covariates				
1(PSU>=475)	.172	.203	.216	.213
	(.047)***	(.046)***	(.043)***	(.028)***
R^2	.05	.045	.05	.047
Panel B: With covariates				
1(PSU>=475)	.188	.211	.211	.221
	(.047)***	(.046)***	(.044)***	(.028)***
R^2	.071	.064	.071	.064
Panel C: With covariates and region Fixed effects				
1(PSU>=475)	.186	.200	.191	.208
	(.047)***	(.044)***	(.043)***	(.027)***
R^2	.102	.124	.119	.11
Obs.	1475	1504	1268	3437
Mean enroll. control	.128	.193	.154	.16

Covariates: 1(work), household size, income category, health insurance type, father education, mother education, 1(father work), 1(mother work), high school GPA, and 1(female). Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

8.5 Robustness Checks

Table 12: Applications and Acceptation conditional on application

	ap07	ap08	ap09	ap0709	ap07092	ap07093
	(1)	(2)	(3)	(4)	(5)	(6)
1(PSU>=475)	.096	.168	.165	.172	.168	.152
	(.043)**	(.030)***	(.025)***	(.017)***	(.017)***	(.018)***
PSU-475	.033	.008	.011	-.0003	.001	.010
	(.022)	(.015)	(.014)	(.009)	(.009)	(.010)
1(PSU>=475)x[PSU-475]	-.035	-.036	-.010	-.011	-.013	-.016
	(.032)	(.026)	(.020)	(.014)	(.014)	(.014)
Const.	.207	.121	.089	.111	-.228	-.198
	(.037)***	(.022)***	(.018)***	(.013)***	(.030)***	(.031)***
Covariates	N	N	N	N	Y	Y
Year FE	N	N	N	N	N	Y
Obs.	2252	2321	2677	7250	7250	7250
R^2	.027	.04	.052	.039	.081	.084

Dependent Variable: 1(Application to college)
Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%Covariates: 1(work), household size, income category, health insurance type, father education, mother education, 1(father work), 1(mother work), high school GPA, and 1(female). Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 13: Applications and Placement conditional on application

	ac07	ac08	ac09	ac0709	ac07092	ac07093
	(1)	(2)	(3)	(4)	(5)	(6)
1(PSU>=475)	.107	-.003	.067	.039	.054	.056
	(.115)	(.108)	(.137)	(.067)	(.067)	(.067)
PSU-475	-.129	.001	-.129	-.066	-.071	-.073
	(.068)*	(.069)	(.112)	(.043)	(.044)	(.044)*
1(PSU>=475)x[PSU-475]	.133	.013	.102	.050	.053	.061
	(.083)	(.082)	(.117)	(.049)	(.049)	(.049)
Const.	.316	.550	.383	.437	-.169	-.176
	(.107)***	(.100)***	(.132)***	(.063)***	(.128)	(.127)
Covariates	N	N	N	N	Y	Y
Year FE	N	N	N	N	N	Y
Obs.	556	456	489	1501	1501	1501
R^2	.011	.0003	.009	.004	.04	.049

Dependent Variable: 1(College Placement | application to college = 1)
Covariates: 1(work), household size, income category, health insurance type, father education, mother education, 1(father work), 1(mother work), high school GPA, and 1(female). Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%

Table 14: Percentage of programs with cut-off below 475 for the treatment group. $w = 2$

Year	2007	2008	2009
% of programs	95.38%	98.02%	92.53%

This table consider the percentage of programs that have cut-off below 475 for all students in the treatment group when the window considered is 2 PSU points ($PSU_i \in [475, 477)$), i.e. programs that are available for students in the control group $PSU_i \in [473, 475)$

Table 16: RD eliminating all students that enrolled in programs that were not available for students below the cut-off

	y0709pc	y07pc	y08pc	y09pc
	(1)	(2)	(3)	(4)
1(PSU>=475)	.082	.087	.087	.098
	(.019)***	(.038)**	(.035)**	(.030)***
PSU-475	-.020	-.025	-.010	-.034
	(.011)*	(.020)	(.018)	(.021)
1(PSU>=475)x[PSU-475]	.033	.024	.011	.059
	(.018)*	(.036)	(.036)	(.029)**
Const.	.129	.102	.168	.104
	(.015)***	(.031)***	(.026)***	(.024)***
Obs.	6407	2051	2137	2219
R^2	.007	.005	.008	.012

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

Table 17: RD for college enrollment using only the State Guaranteed loan for private universities.

	cae0709	cae07	cae08	cae09
	(1)	(2)	(3)	(4)
1(PSU>=475)	.076	.101	.069	.096
	(.018)***	(.035)***	(.033)**	(.028)***
PSU-475	-.018	-.018	-.013	-.034
	(.010)*	(.018)	(.017)	(.019)*
1(PSU>=475)x[PSU-475]	.036	-.008	.014	.075
	(.017)**	(.033)	(.034)	(.028)***
Const.	.099	.075	.133	.075
	(.014)***	(.029)***	(.024)***	(.022)***
Obs.	6166	1982	2030	2154
R^2	.008	.008	.005	.017

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

Table 18: College Enrollment for students in Quintile 5.

	q507	q508	q509	q50709
	(1)	(2)	(3)	(4)
1(PSU>=475)	-.162	.037	.182	.034
	(.125)	(.098)	(.107)*	(.062)
Obs.	67	91	79	237
R^2	.027	.002	.036	.001

8.6 Heterogeneous Effects

Table 19: RD College Enrollment by income quintile. By year and all sample. $\epsilon = 2$ PSU points.

	c2007	c2008	c2009	c0709
	(1)	(2)	(3)	(4)
1(PSU>475) x Quintil1	.236	.214	.191	.209
	(.032)***	(.037)***	(.031)***	(.019)***
1(PSU>475) x Quintil2	.221	.158	.197	.190
	(.062)***	(.054)***	(.049)***	(.031)***
1(PSU>475) x Quintil3	.185	.201	.234	.193
	(.064)***	(.065)***	(.072)***	(.039)***
1(PSU>475) x Quintil4	.055	.140	.144	.105
	(.078)	(.083)*	(.081)*	(.047)**
1(PSU>475) x Quintil5	-.162	.037	.182	.034
	(.124)	(.097)	(.106)*	(.061)
Obs.	2150	2187	2280	6617
R^2	.197	.226	.231	.214

Robust standard errors in parenthesis. (***): p⩽1%, (**): p⩽5%, (*):p⩽10%
$P(Coll_i = 1) = \sum_{k=1}^{5} \theta_k q_i^k + \sum_{k=1}^{5} \beta_k q_i^k \cdot 1(PSU \geqslant 475) + \sum_{k=1}^{5} \pi_k q_i^k \cdot [PSU - 475] +$
$+ \sum_k \phi_k q_i^k \cdot [PSU - 475] \cdot 1(PSU \geqslant 475)$, where $k \in$ {1,...5}

Table 20: RD College Enrollment Father Education. By year and all sample. $\epsilon = 2$ PSU points.

Dependent Var. : College Enrollement						
	2006	2007	2008	2009	All	07-09
	(1)	(2)	(3)	(4)	(5)	(6)
1(PSU>=475) x FPrim	.082	.077	.058	.054	.052	.049
	(.076)	(.056)	(.059)	(.048)	(.029)*	(.030)
1(PSU>=475) x FHS	.082	.132	.132	.178	.119	.145
	(.062)	(.054)**	(.051)***	(.042)***	(.025)***	(.027)***
1(PSU>=475) x FVoca	.042	.156	.146	.196	.116	.164
	(.162)	(.196)	(.123)	(.116)*	(.071)	(.077)**
1(PSU>=475) x FColle	-.042	.097	.220	.119	.060	.110
	(.105)	(.119)	(.112)**	(.119)	(.055)	(.064)*
Obs.	1682	2150	2187	2280	8299	6617
R^2	.321	.194	.254	.221	.238	.219

Dependent Variable: 1(College enrollment)
Robust standard errors in parenthesis. (***): p$\leqslant$1%, (**): p$\leqslant$5%, (*):p$\leqslant$10%
$P(Coll_i = 1) = \sum_k \theta_k FatherEdu_i^k + \sum_k \beta_k FatherEdu_i^k \cdot 1(PSU \geqslant 475) +$
$+ \sum_k \pi_k FatherEdu_i^k \cdot [PSU - 475] + \sum_k \phi_k FatherEdu_i^k \cdot [PSU - 475] \cdot 1(PSU \geqslant 475)$
where $k \in$\{Primary, High School, Vocational, College\}

9 FIGURES

9.1 Loan Assignment.

Figure 1: Credit allocation by PSU score. Only students graduating the previous year.

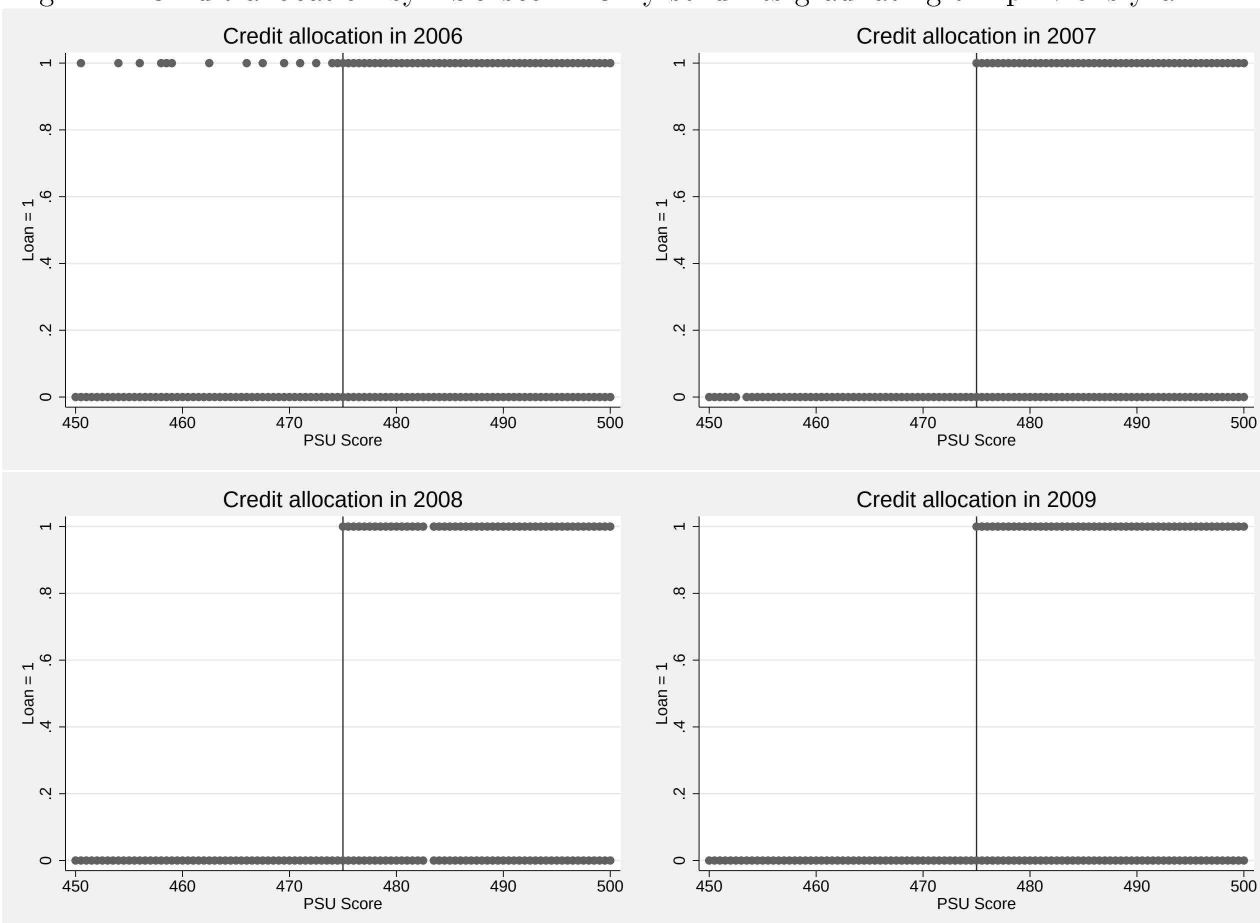
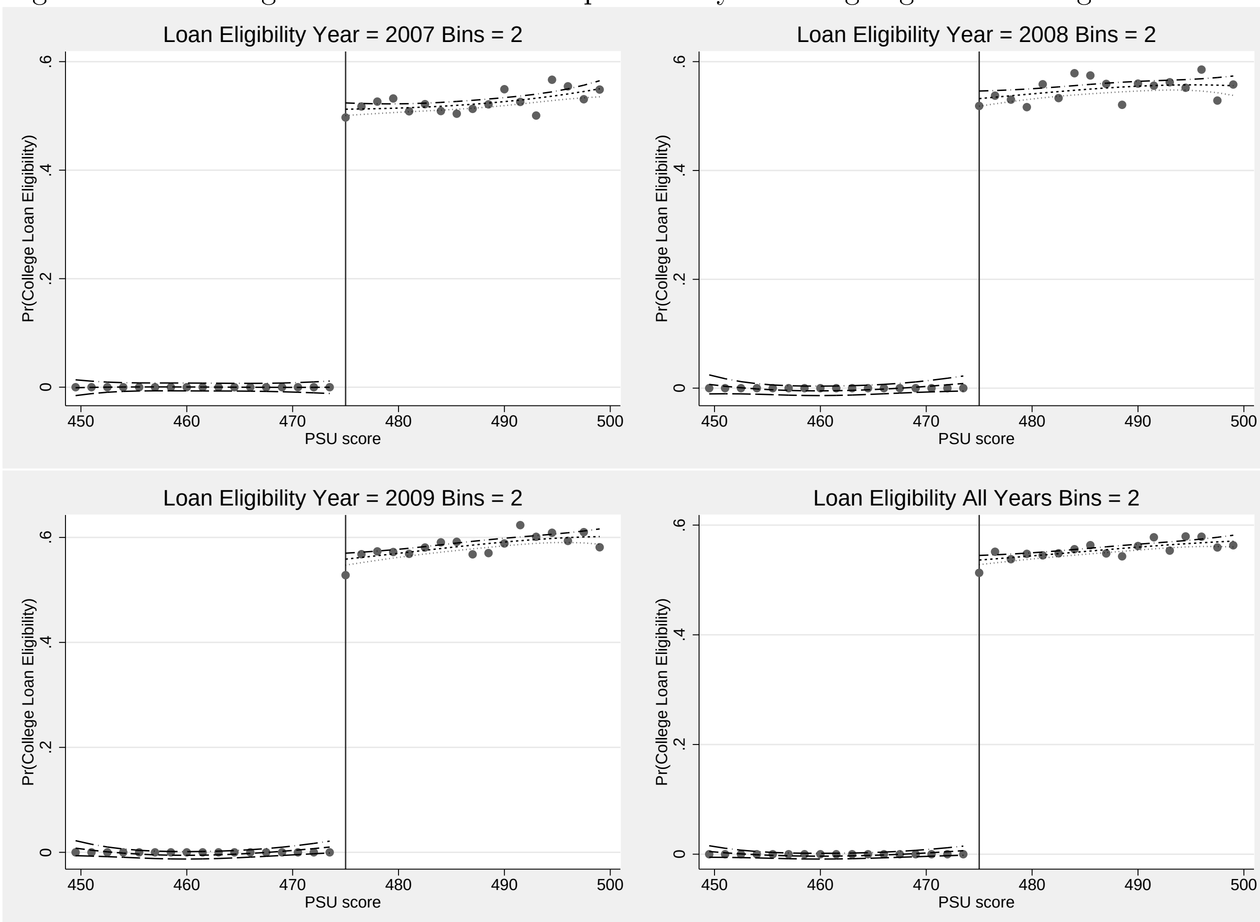
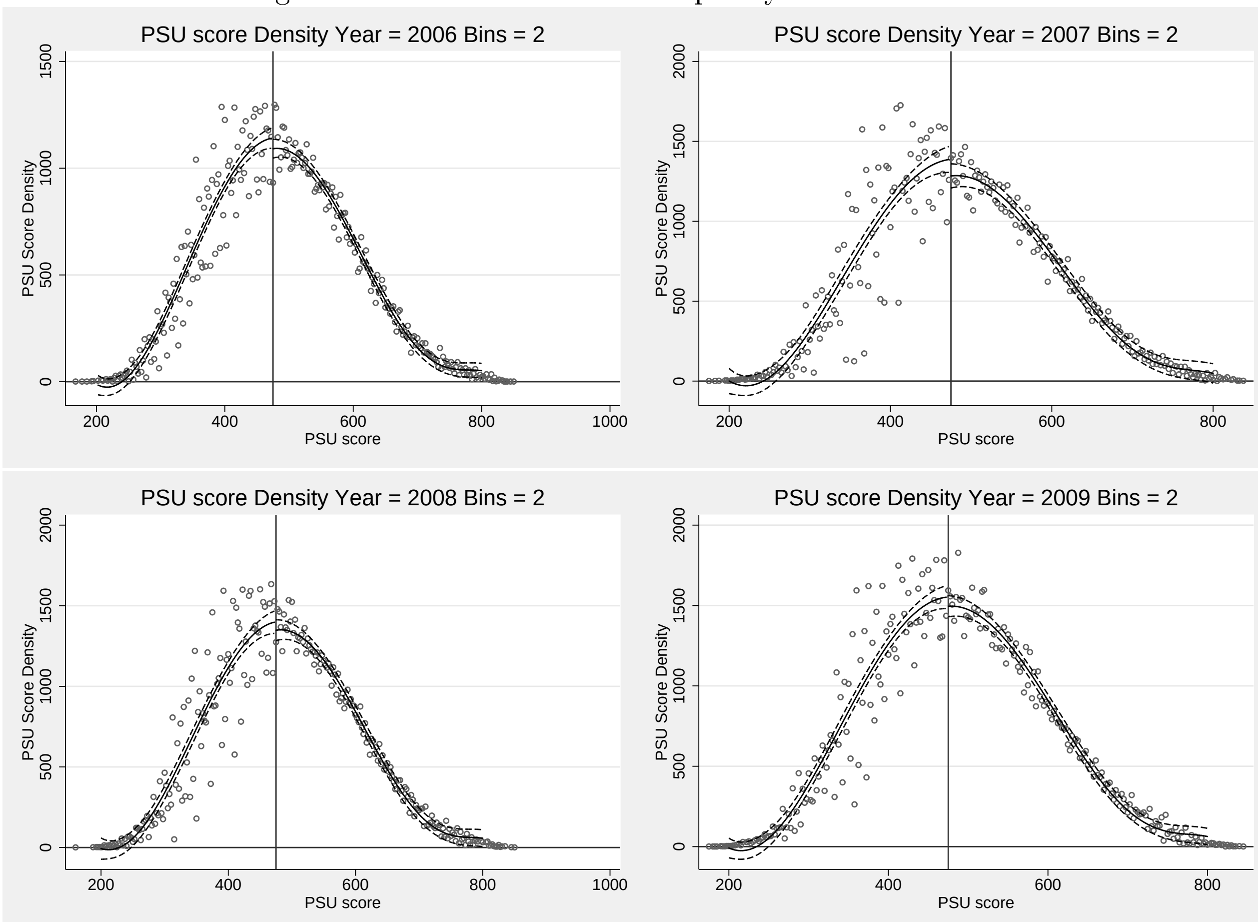


Figure 2: Loan assignment. Unconditional probability for being eligible to College Loans.



9.2 Manipulation of the running variable.

Figure 3: RD for PSU scores frequency distribution.



9.3 Reduced Form.

Figure 4: Reduced form. Probability of college enrollment around the cut-off. $\epsilon = 2$

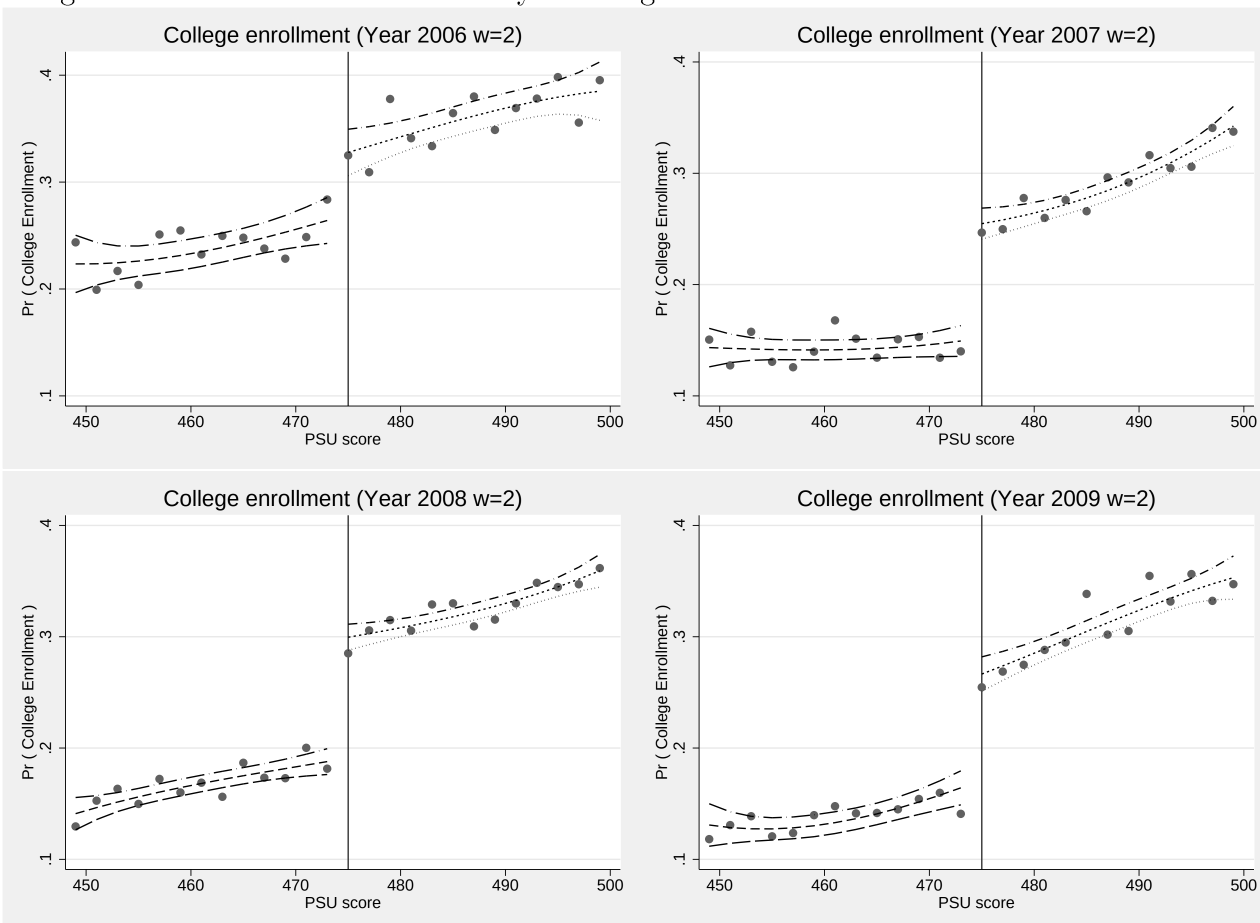
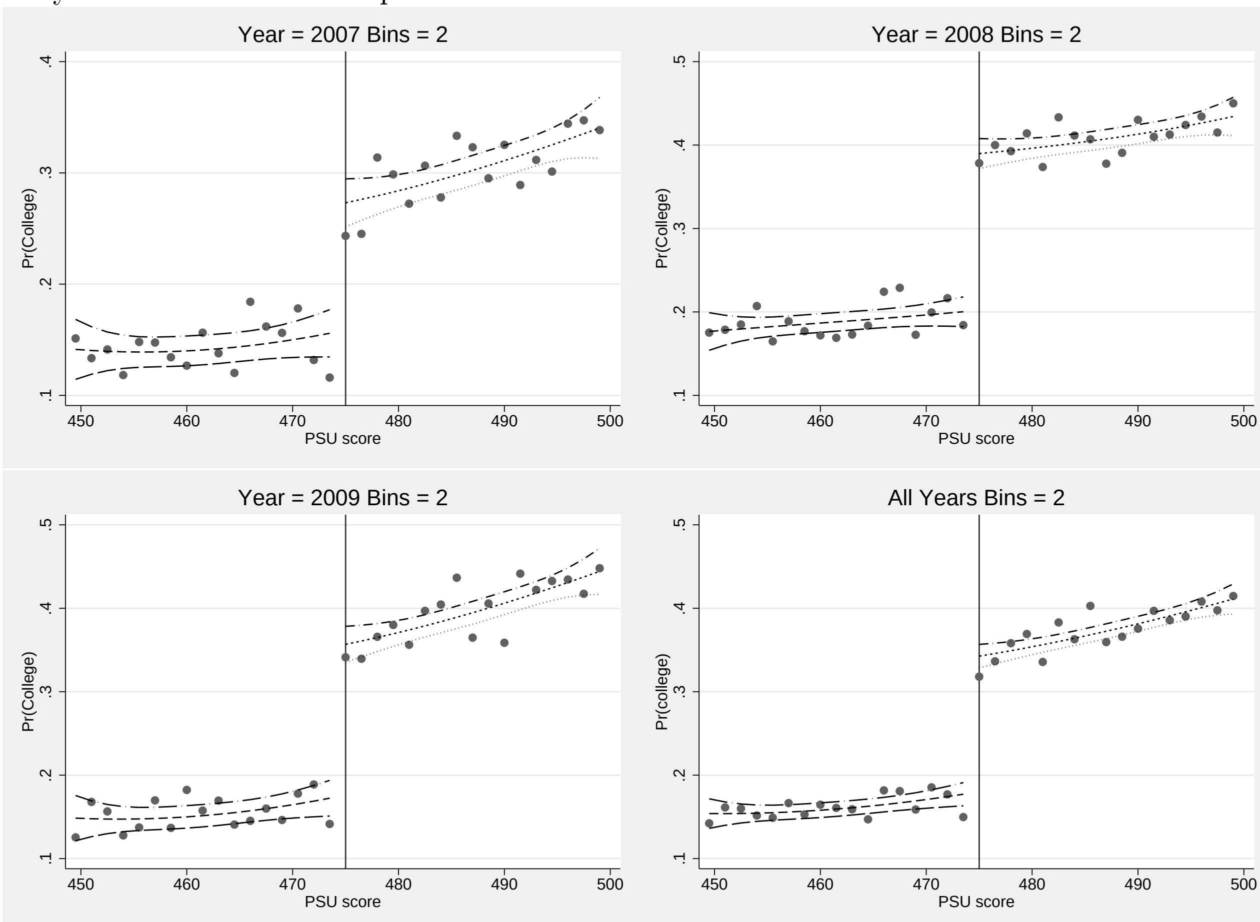


Figure 5: Reduced form. Probability of college enrollment around the cut-off. $\epsilon = 2$. Only PRESELECTED sample.



9.4 Robustness Checks

Figure 6: RD for application to traditional universities and being accepted conditional on being applied All years from 2007 through 2009.

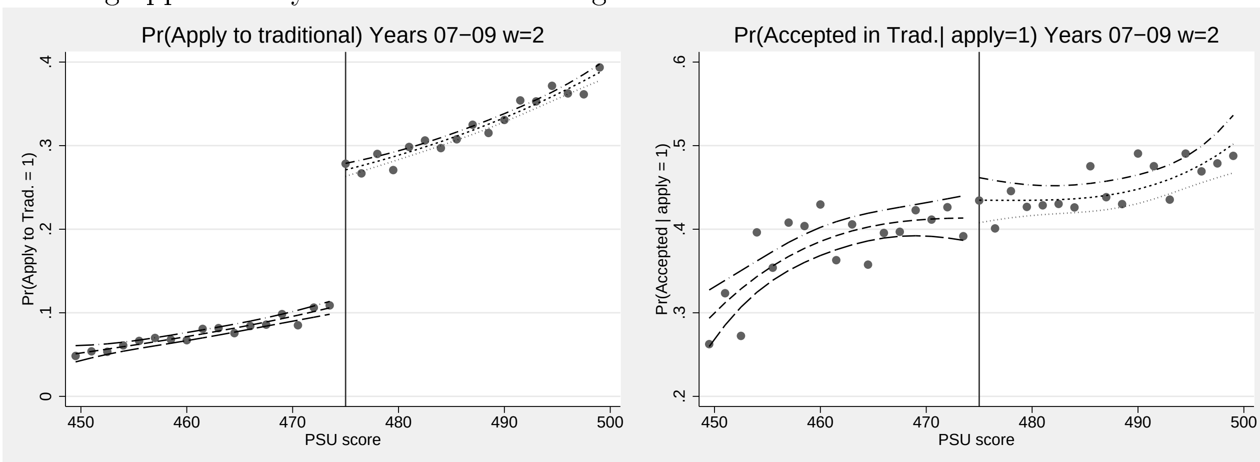


Figure 7: RD for college enrollment without considering enrolled students with program cut-offs below 475. $\epsilon = 2$

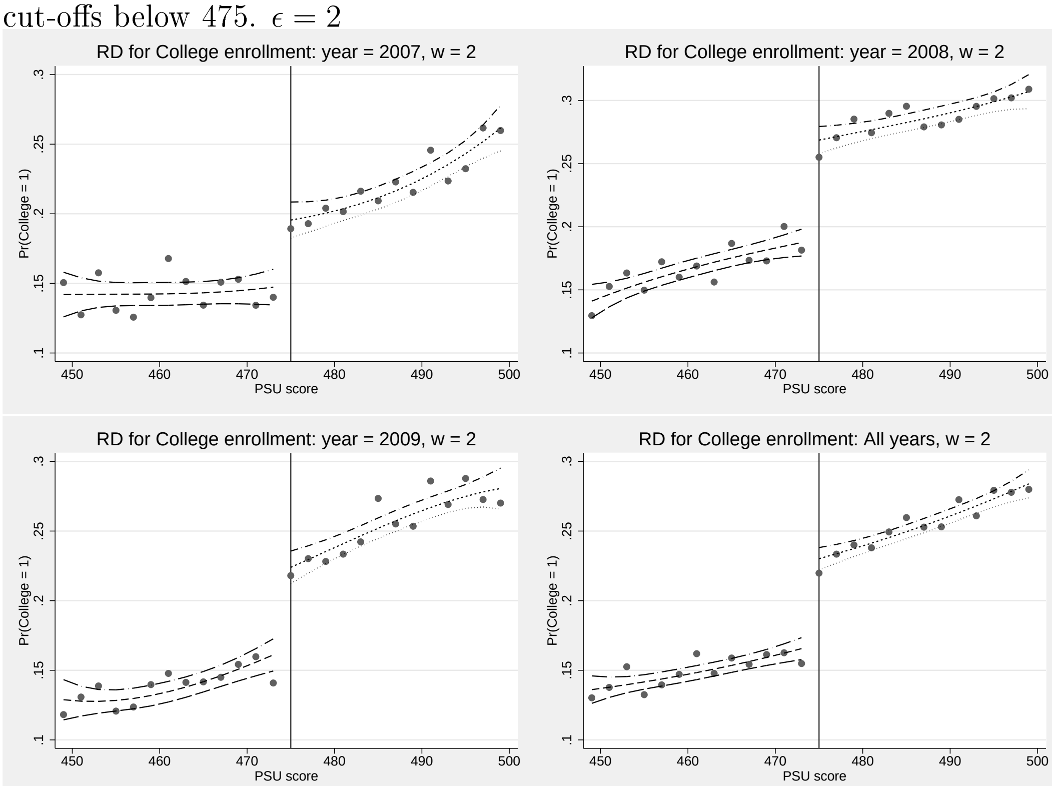


Figure 8: Probability of college enrollment around the cut-off for students from the highest income quintile. $\epsilon = 2$

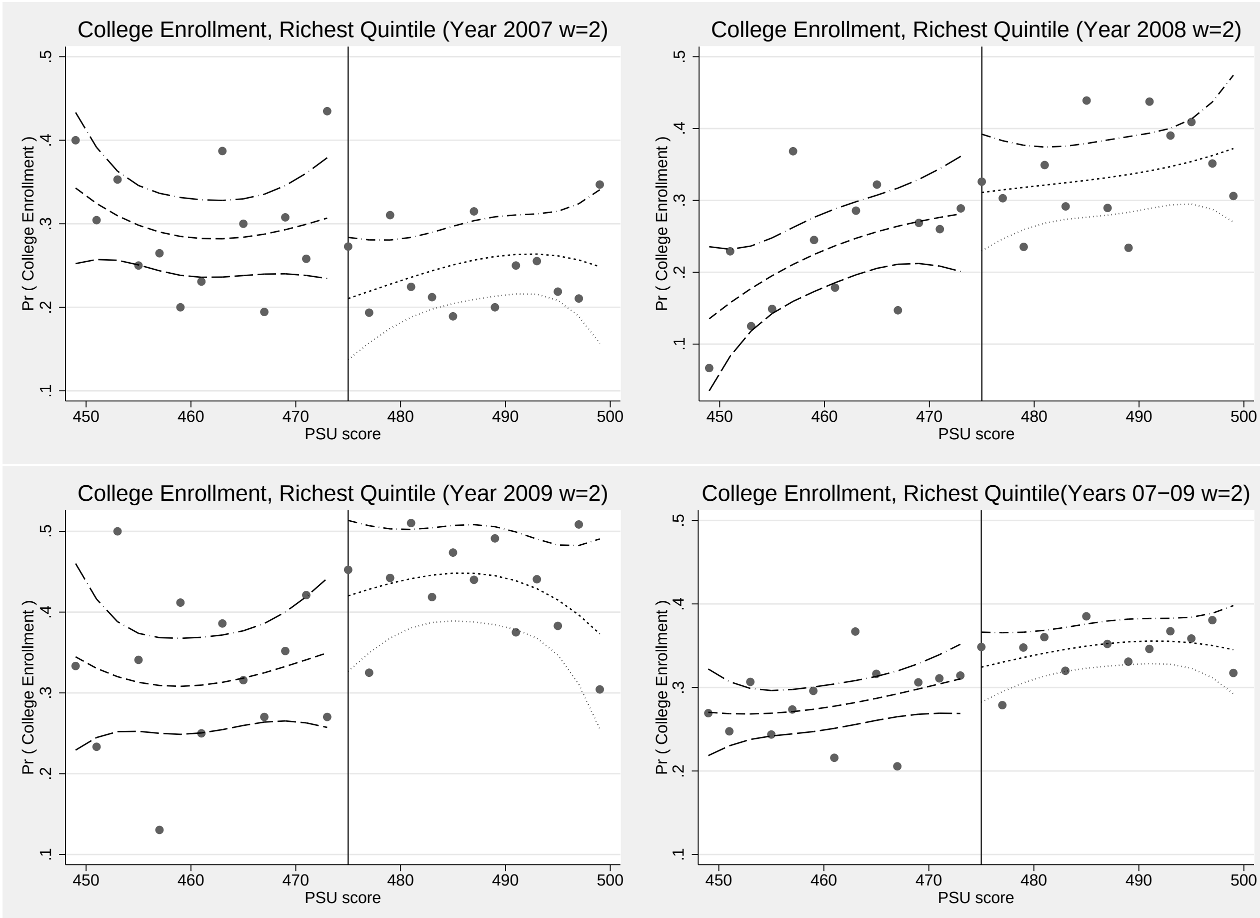
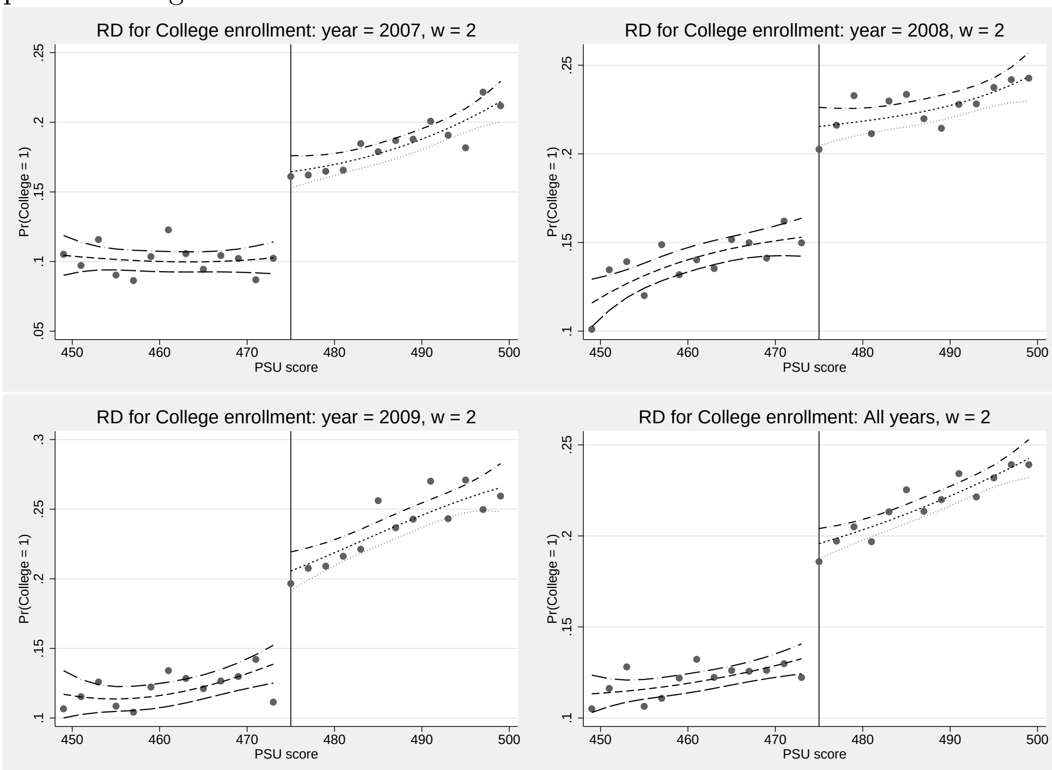


Figure 9: RD for college enrollment considering only the State Guaranteed Loan in private colleges. $\epsilon = 2$



9.5 Heterogeneous Effects

Figure 10: Comparison in enrollment rate by quintile years 2007 to 2009. $\epsilon = 2$

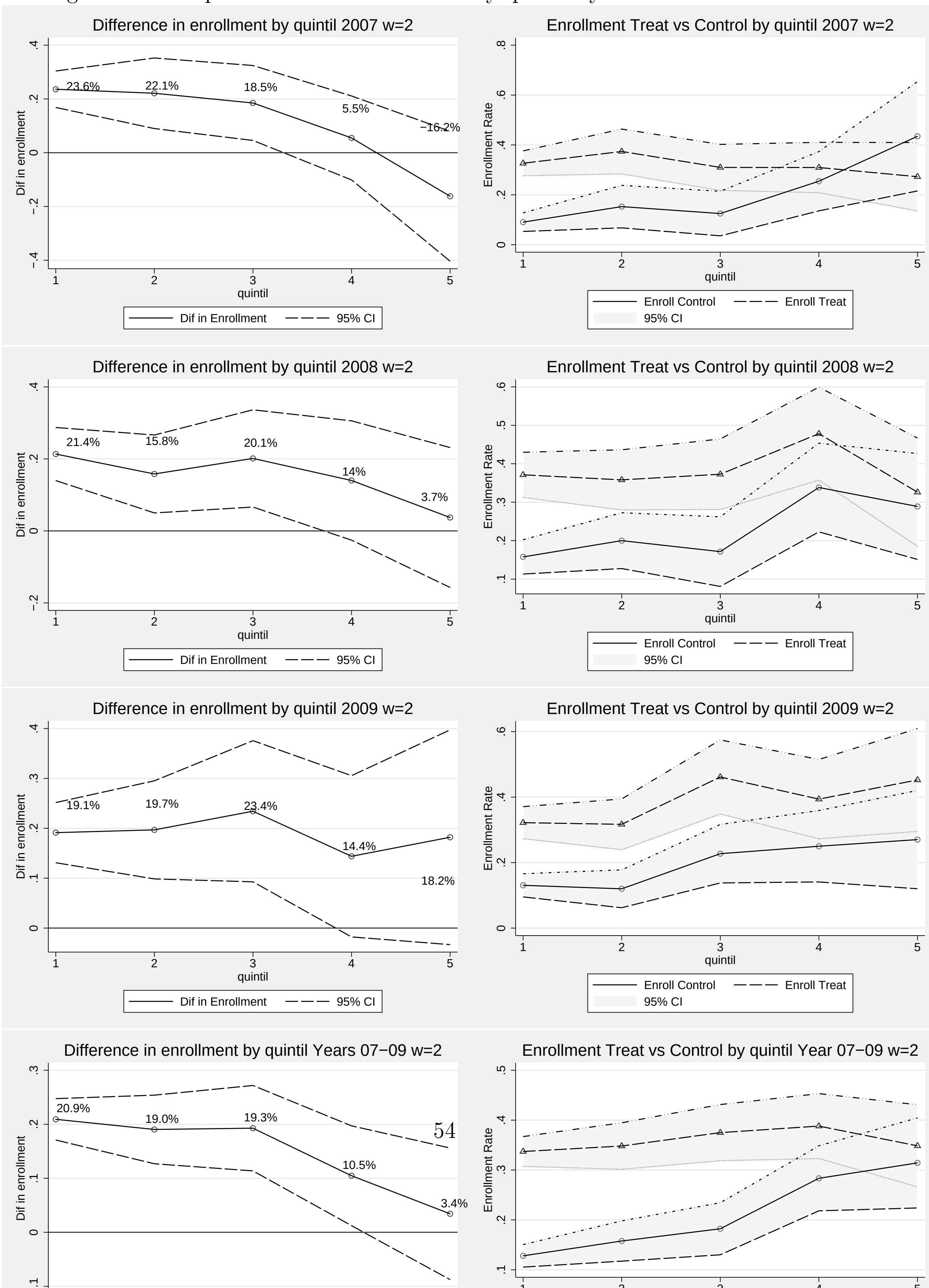


Figure 11: Enrollment rate by quintile years 2007 to 2009 pooled together. $\epsilon = 2$