Credit Access and College Enrollment

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Does access to credit explain the gap in schooling attainment between children from richer and poorer families? I present new evidence on this important question based on the causal effects of two college loan programs in Chile that are available to students scoring above a threshold on the national college admission test, enabling a regression discontinuity design. I find that credit access leads to a 100 percent increase in immediate college enrollment and a 50 percent increase in the probability of ever enrolling. Moreover, access to loans effectively eliminates the income gap in enrollment and number of years of college attainment.

I. Introduction

Students from richer families are more likely to attend, persist at, and graduate from college than students from poorer families. Whether the gap is due entirely to differences in tastes and abilities or is partially driven by

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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 readiness for college (e.g., Cameron and Heckman 2001; Keane and Wolpin 2001; Carneiro and Heckman 2002; Cameron and Taber 2004). Others have argued that liquidity constraints prevent some relatively able but poor students from enrolling in college (e.g., Lang 1993; Kane 1994, 1996; Card 1999, 2001; Belley and Lochner 2007; Lochner and Monge-Naranjo 2011; Brown, Scholz, and Seshadri 2012).[1]

Measuring the effects of credit constraints on college enrollment is a challenging task because determining whether a family has access to credit is difficult or impossible. Moreover, even if access to credit were directly observed, there are many other unobserved variables that affect college enrollment and are likely to be correlated with access to credit, leading to biased estimates.[2] It is possible, for example, that students from high-income families have not only better access to credit markets but also stronger preferences for higher education, better academic preparation, and superior cognitive and noncognitive skills that are unobserved by the econometrician. On the supply side, access to loans may also be related to ability. For instance, van der Klaauw (2002) argues that college grants are increasingly based on academic merit and are often used by colleges to compete for the best students rather than to aid low-income families. In addition, the admission process often considers unobserved and subjective measures such as recommendation letters and the alumni status of parents. As a result of these problems, tests of the credit constraint hypothesis have relied mainly on indirect measures of credit access, with mixed—and sometimes inconsistent—findings.

The literature so far has focused on developed countries with relatively generous aid programs (mainly the United States), but little is known about what happens in other parts of the world where financial aid and loan programs are less extensive and policies could have a greater impact. This paper helps fill that gap by exploiting the sharp eligibility rules for two college loan programs recently introduced in Chile. These programs provide access to loans to students who score above a certain threshold on the national college admission test. A comparison of students with scores just above and just below the eligibility cutoff provides a direct measure of ac-

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[1] See Lochner and Monge-Naranjo (2012) for a detailed review of the literature.

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

cess to credit, enabling a regression discontinuity design (RD) that addresses the problems of unobserved omitted variables and self-selection in loan availability. Thus, these loan programs allow for credible estimates of the causal effect of credit access on college enrollment and persistence.[3]

The analysis of these loan programs is greatly facilitated by the availability of detailed student-level data for the entire population of students who participate in the national college admission process, including complete information on subsequent enrollment outcomes at all universities in the country. The available data include students' ranking of choices for the "traditional" Chilean universities (a category that is described below), their admission to individual programs, and their actual enrollment. Moreover, college admission decisions in Chile are completely determined by two observed variables—scores on the national college admission tests and high school grade point average (GPA)—ruling out potential biases due to unobserved characteristics that may affect the admission process in other contexts. Third, the loan programs provide eligible students access to standardized loans from the government and private banks, eliminating potential endogeneity of loan offers designed to attract better students. 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.

My analysis shows that access to the two loan programs increases the probability of college enrollment in the year immediately after high school graduation (immediate enrollment) by 18 percentage points—equivalent to a nearly 100 percent increase in the enrollment rate relative to the group with test scores just below the eligibility threshold. The gains are largest for students from the lowest family income quintile: access to the loans leads to a 140 percent increase in the probability of immediate enrollment measured for these students, relative to a baseline enrollment rate of 15 percent for students just below the cutoff.

I find a similar impact of loan eligibility on the probability of enrollment in the 3 years following the last year of high school—an expanded horizon that could capture other strategies to finance college, such as delaying enrollment to work for 1 or 2 years. Specifically, I find a 16 percentage point increase in the probability of ever enrolling within 3 years of high school graduation—equivalent to a 50 percent increase in the 3-year enrollment rate.

Remarkably, I also find that access to loan programs appears to essentially eliminate the relatively large gap in enrollment rates between stu-

[3] In terms of the methodology, Canton and Blom (2009) and Gurgand, Lorenceau, and Melonio (2011) perform an RD analysis using information on Mexican and South African students. Rau, Rojas, and Urzúa (2013) analyze enrollment, dropout rates, and earnings for one of the two loans analyzed here, the State Guaranteed Loan program, using a sequential schooling decision model with unobserved heterogeneity.

dents from different family income quintiles. Among those who are just below the eligibility threshold for loans, students from the richest quintile are twice as likely to enroll as students from the poorest quintile. In contrast, among students who are just above the threshold, the enrollment gap between the highest and lowest quintiles is statistically zero.

The literature on liquidity constraints has focused mainly on college enrollment. Programs that encourage enrollment may have little or no effect on long-run educational attainment—and could even end up harming students—if they attract students who are unable to successfully complete college-level work. For this reason, a different strand of literature examines the impact of aid on persistence (i.e., dropout and graduation rates), including DesJardins, Ahlburg, and McCall (2002), Dynarski (2003), Bettinger (2004), Singell (2004), and Stinebrickner and Stinebrickner (2008). As in the enrollment literature, there is wide variation in findings across studies, with some researchers finding positive effects and others reporting no significant impact of aid on college persistence.[4]

The literature on persistence faces additional econometric problems. Enrolled students constitute a self-selected sample of individuals, so it is difficult to infer causality from samples that condition on enrollment. Furthermore, in most cases, the analysis is performed using information from a single institution or a restricted group of institutions. That implies two more concerns. First, the analysis depends critically on the characteristics of the analyzed institution(s). Second, in many cases, transfer students are mistakenly considered dropouts.

This paper also contributes to this literature, using the same exogenous variation in access to loans to estimate the causal effect on two simple measures that capture college persistence: enrollment for at least 2 years and the total number of years of college completed. Using the entire population of students who participate in the admission process eliminates the selection bias in the analysis of college progress, and using all institutions eliminates the bias associated with transfer students and presents general evidence not contingent on one institution.

In this context, I estimate that the availability of loans leads to a 50 percent increase in the probability of enrolling in a second year of college within 3 years of high school graduation. Moreover, access to loans is associated with a rise of 0.5 year of completed college in the first 3 years after high school, relative to a baseline attainment rate of 0.8 year, representing a relative increase of 64 percent in human capital accumulation. For the eligible income quintiles, I also find that access to the loan programs eliminates the family income gradient in the two measures of persistence.

This setting allows me to determine average characteristics for the compliers induced to enroll in college by the two loan programs and

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

compare them with the characteristics of college enrollees in the absence of credit. I find that the loans allow relatively high-achieving students from relatively lower-income families to enroll in college. Moreover, students who enroll in college just below the cutoff come from families with more educated parents, while enrollees just above the cutoff are not different from the overall population. This suggests that these loans help reduce the enrollment gap in other dimensions.

The paper is organized as follows. Section II describes the background and the data. Section III discusses the empirical strategy. Section IV presents the main findings of the paper. Section V explores two possible mechanisms that explain these findings. Section VI describes situations in other parts of the world, and Section VII presents conclusions.

II. Background and Data

In terms of its basic structure, the Chilean university system closely resembles the American case: there is a mix of public and privately owned universities with an overlapping distribution of quality and prestige. There are two basic types of institutions: The so-called "traditional" universities are a set of 25 institutions that were founded before 1981, some of which are public (e.g., University of Chile) and some of which are private (e.g., Catholic University of Chile). All of these traditional universities receive substantial direct funding from the government. The other 33 so-called "private" universities were founded after 1981. These schools receive no direct aid from the government and are mainly financed by student tuition.

Tuition fees in Chile are high on average (about 2.1 million Chilean pesos, equivalent to 47 percent of the median family income, in 2009) and are also relatively similar across institutions.[5] Even at low-cost public universities, a family in the poorest income quintile would have to pay at least 84 percent of its available income to cover tuition just for 1 year (50 percent and 32 percent for families in the second and third quintiles, respectively). Given that the standard college program is scheduled to last 5 years and students take an average of 6.5 years to graduate, this implies a large financial burden.

There are limited options for students who cannot depend on their family to finance college education out of pocket. Even if their parents were willing to take out a student loan in the conventional financial mar-

[5] Average tuition is equivalent to US$4,200. Median family income is calculated using the household survey Caracterización Socioeconómica Nacional (CASEN) 2009. Per capita income (purchasing power parity) was approximately $14,000 (using conversion rates of 2009). Appendix C compares tuition in an international context. In terms of per capita income, Chile has tuition similar to that of other Latin American countries and the United States.

ket, they would be subject to strict income eligibility criteria. Between 2007 and 2009, the years of analysis in this paper, the lowest minimum income requirement for a college loan was offered by Banco Estado.[6] This loan required at least CLP 350,000 (US$714) in monthly family income to apply, which disqualified all families in the two lowest-income quintiles of the country, as well as some families in the third quintile (see table 1 for the definition of the income quintiles). Additionally, families are excluded if they do not have a job in the formal sector, which is especially restrictive in a country like Chile, where there are high degrees of labor market informality.[7]

To work and save to pay for college does not seem to be a plausible strategy either. The average monthly income for graduates from high school (between 18 and 20 years old) was about CLP 151,000 in 2009.[8] At this wage, it would take 1 year of full-time employment to earn the tuition for 1 year of a typical college program.

Faced with these restrictions, most students have to rely on government grants and loans to finance their college education. By far, the most important source of higher education funding is the loans and grants given by the Ministry of Education (see table 2 for details).[9] The assignment of these grants and loans is highly centralized and closely linked to performance on the national college admission test, the PSU, which is taken by all students at the same time and only once per admission process. The PSU test contains two mandatory tests in mathematics and language (comparable to mathematics and critical reading on the Scholastic Aptitude Test [SAT]), as well as two optional tests.[10] The average score on the mandatory tests is referred to as the *PSU score*. PSU scores are normalized to have a mean of 500 and a standard deviation of 110.[11] These scores are used by the Ministry of Education to determine financial aid eligibility. Additionally, PSU scores are the only variables other than high school GPA that factor into college admission decisions.

[6] This is a private bank with partial ownership by the government of Chile.

[7] According to the national household survey CASEN, in 2006, 36 percent of all workers were in the informal sector (self-employed or without a contract).

[8] Source: CASEN 2009. This figure was calculated using individuals who declare not to be enrolled in higher education. The minimum wage is CLP 165,000 (of 2009; US$330).

[9] Some universities offer loans or grants to attract better students, but those aid programs aim at students with much higher scores on the Prueba de Selección Universitaria (PSU) and thus do not confound the results here.

[10] The optional tests are (1) history and social sciences and (2) sciences. They are not considered for loan eligibility but are considered for ranking applicants in traditional universities.

[11] The PSU resembles the SAT in many dimensions. For example, the SAT has the same mean and standard deviation as the PSU. PSU scores range from 150 to 850 points, while SAT scores range from 200 to 800. The registration fee for PSU is CLP 25,000 (pesos of 2012)—equivalent to $50—while the SAT has a fee of $49. The PSU registration fee is waived for all students graduating from public and voucher schools who apply for a waiver.

TABLE 1
Income Quintile Definitions

	Income Quintile 1	2	3	4
	Chile: Income Distribution			
Upper-bound monthly family income (CLP)	178,366	306,000	469,625	777,218
Upper-bound monthly family income (US$)	364	624	958	1,586
	United States: Income Distribution			
Upper-bound monthly family income (US$)	725	1,307	2,029	3,258

Source.—For Chile: CASEN 2009. Calculated using autonomous income per family, which includes salaries, rents, subsidies from the governments, pensions, etc., for all members of the family. For the United States: 2010 American Community Survey from IPUMS. Calculated from total personal income, INCTOT (in nominal terms).

In brief, the process can be summarized as follows. Before graduating from high school in November, students must register for the PSU test. Additionally, those who want to receive aid or loans from the Ministry of Education need to submit a socioeconomic verification form (Formulario Único de Acreditación Socioeconómica [FUAS]), which is used to determine each family income quintile. Students take the PSU test in the second week of December and receive their score in the first week of January. On the basis of their PSU score, students know whether they are eligible for aid or loans (assuming they satisfy the other criteria listed in table 2). From the second week of January, students apply to the different college programs available in the country and then enroll. Institutions inform the Ministry of Education about the enrollment for all programs in order to directly receive the payments of loans and grants; only at that point in time do institutions receive information about students' income quintile classification.

The administrative data used in this paper are created as part of this highly centralized process, which ensures that I have information on all students who participate in the national test and all their subsequent enrollment activity.

A. The Loan Programs

The two most important college financing programs offered by the Ministry of Education are the Traditional University Loan (TUL) and the State Guaranteed Loan (SGL). These loans provide an amount up to the so-called "reference tuition" level, which is about 90 percent of the tuition

TABLE 2
REQUIREMENTS FOR LOANS AND SCHOLARSHIPS

	Recipients with Respect to		Requirements			
	Population (%) (1)	Eligibles (%) (2)	Income Quintiles (3)	PSU Cutoff (4)	Institution Type (5)	Cover (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	.02	.87	1 and 2	640	Accredited	[a]
PSU score grant	.02	.05	1 to 4	. . .	Accredited	[b]
Excellence	2.32	4.78	1 to 4	. . .	Accredited[1,3]	[a]
Teacher's children:						
BHDP	1.02	3.98	1 to 4	500	All[4,5]	[c]
Pedagogy: BPED	.07	.74	All	600	Accredited[4]	[b]

NOTE.—Column 1 reports the ratio of recipients over students taking the test for the first time. Column 2 corresponds to the ratio of recipients over those who take the PSU test for the first time, have applied to the benefit, belong to eligible quintiles, and score more than the respective cutoff. "Accredited" refers to all accredited colleges (traditional and private) and accredited vocational institutions. "Traditional" refers to traditional universities, all of which are accredited.

[1] Only students graduating from voucher and public high schools.

[2] National or regional best PSU score.

[3] Only for students in the top 5 percent of their graduating high school.

[4] Only students 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.

[5] Only for children of teachers and employees at voucher and public schools.

[a] Funds up to reference tuition.

[b] Funds up to fixed value: US$2,250 for universities and US$1,000 for vocational programs, which are about the same magnitude as the average reference tuition.

[c] Funds up to US$1,000, which corresponds to a quarter of the university average tuition.

costs for the years considered here.[12] The loans do not cover living expenses or any other expenses associated with attending college (books, transportation, etc.). To be eligible for either of these loans, students who complete the FUAS form need to (1) be classified by the tax authority among the four poorest income quintiles and (2) have a PSU score of at least 475 points. The identification strategy in this paper exploits this latter characteristic: among students in the eligible income quintiles, the assignment of loan eligibility is "as good as random" (Lee 2008), enabling an RD design (see Sec. III). The 475-point cutoff on the PSU test for loan eligibility is roughly equivalent to 950 SAT points, which in the United

[12] Reference tuition for each program corresponds to a fixed amount determined by the Ministry of Education that can be financed with loans and grants. This value depends on the quality of institutional assets and the labor market prospects of graduates of each program.

States would grant admission to research universities ranked roughly 175 or lower or to liberal arts colleges ranked 125 or lower.[13]

There are some differences between the two loan programs. The TUL program is an income-contingent loan with the minimum repayment set at 5 percent of the borrower's income. TUL loans are provided only to students who enroll in traditional universities, which are in charge of both determining how much to lend to each student and collecting loan repayments.[14] The real interest rate on this loan is about 2 percent per year. It has a grace period of 2 years after graduation and a maximum of 15 years of payments; after that, the debt is written off. Moreover, it can be complemented with SGL to cover an amount up to the reference tuition.

Under the SGL program, private banks provide college tuition loans to eligible students who enroll in accredited universities. These loans are guaranteed by the state and by higher-education institutions. Students decide the amount to request to meet their financial needs up to the reference tuition. The SGL program is larger than the TUL (serving 29 percent of eligible students vs. 22 percent for TUL), and its average loan amount is 1.56 times the amount given by TUL, which makes its total value 2.2 times the size of the TUL.[15]

A key feature of the SGL program is that, for the period analyzed in this paper, it is very similar to other loans available in the conventional financial market with regard to the conditions of the loan (interest rate and installment calculation) and the enforceability of the repayments.[16] First, the loan had an interest rate of about 6 percent per year (in real terms), which is slightly higher than the average mortgage rate for the same period.[17] Repayment is scheduled in fixed monthly installments for 20 years

[13] A usual measure of selectiveness used by colleges in the United States is the 25th–75th SAT percentile range (see, e.g., http://www.satscores.us/sat_scores.asp), which is calculated using the scores in math and critical reading among enrolled students only. I ranked universities on the basis of the 25th percentile, and 950 SAT points corresponds to a research university ranked 175th. For example, the 25th–75th SAT ranges at the University of Colorado–Denver are 470–600 and 480–590 in math and critical reading, respectively. There were 2,968 Title IV degree-granting 4-year colleges in the academic year 2011–12 (source: US Department of Education, National Center for Education Statistics, 2013; *Digest of Education Statistics*, 2012 [NCES 2014–15], table 306).

[14] TUL was introduced in 1981 as part of an educational reform. It was the main source of college funding for students up to the introduction of SGL. Previous to 2006, eligibility was determined independently by each university, on the basis of the amount granted to each institution.

[15] Out of the 58 institutions that provide college education in Chile, 77.6 percent participate in the program. Of the remainder, 19 percent are not accredited institutions and therefore are not eligible, and 3.4 percent have dropped out of the SGL program.

[16] This program was specifically designed to give a market alternative to students in private universities and vocational schools who did not have access to TUL.

[17] 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 percent of their income after graduation.

(not contingent on income), with a grace period of 18 months after graduation. Second, private banks are in charge of the whole process; they make the payments to institutions, give the debt information to students, and collect repayments. Therefore, they are entitled to use all available legal mechanisms to recover the debt, including the release of information to credit score institutions, asset impoundment, and judicial collection.[18] To increase the enforceability of repayment, employers are mandated to deduct repayments directly from payroll and to make payments directly to banks,[19] and the tax authority may retain tax refunds in case of default. In the event that a bank cannot collect the loans, the guarantors (the state and/or the educational institution) must pay the bank and become responsible for enforcing collection from the student.[20]

These differences have led to different repayment rates for the two programs. Despite the special characteristics of TUL, the loan has a high default rate of around 52 percent, according to Fondo Solidario de Crédito Universitario. One possible reason is that universities are not particularly effective in collecting loans. The low enforceability and the low interest rate suggest the existence of a subsidy component in this loan scheme.

On the other hand, the default rate for the SGL (evaluated in 2011) is estimated at 36 percent (World Bank 2011). Moreover, the World Bank report argues that "by design, CAE's [the Spanish acronym for SGL] terms of lending should lead to high recovery. With lending rates that exceed the Government's cost of capital by two hundred basis points, the program does not explicitly contain an embedded subsidy" (30). Nevertheless, the default rate has been higher than the default rate on conventional loans, and the World Bank predicts that it could increase to as much as 50 percent if certain recommendations are not followed.[21] Although the interest rate does not contain an implicit subsidy, a high default rate may give the wrong incentives to students who may consider it a grant instead of a loan, raising issues in this study of separating the credit access effect from a subsidy effect (see more in Sec. V).

[18] Releasing information to credit score institutions is important in the labor market in Chile because usually firms request that potential employees not appear as defaulters in credit score records.

[19] The law establishes penalties on employers who do not comply with this process.

[20] If a student drops out in the first/second/third year or later, the educational institution is responsible for repaying the bank 90 percent/70 percent/60 percent of the capital and interest accumulated and the state the difference up to 90 percent. After the student graduates, the state guarantees 90 percent.

[21] According to the World Bank's report, the high default rate is caused mainly by "suboptimal program administration, rather than excessive debt burden" (11). The main cause of the low collection rate is the lack of effective communication between lenders and students.

B. *Data and Sample*

I use four main sources of data from different administrative files to analyze the effects of eligibility for TUL and SGL loans. The first data source is the registry of students who enroll for the PSU test. It contains individual data on PSU scores and high school GPAs, as well as a rich set of socioeconomic characteristics, such as self-reported family income, parent education, school of graduation, and so forth, for the years 2007–12.

The second source of data is an administrative file from the Ministry of Education that captures enrollment in all higher-education institutions in the country. In particular, I use a version of this file that has information on enrollment in the years 2007–9.

The third source of information is the FUAS application data set for the years 2007–9. The key element in this data set is the income quintile reported by the tax authority, which determines eligibility for the two loan programs and for six scholarship programs. Moreover, this data set contains the assignment to financial aid programs and the take-up for the traditional loan (TUL).

The fourth data set contains information on loans generated under the SGL program. This data set is from the INGRESA commission, an organization created in 2006 to manage this credit program.[22]

In addition to these four main sources, I make use of student performance data and the SIMCE 2004 data set from the Ministry of Education in order to assess the representativeness of the sample.[23] The performance data set is the registry from the Ministry of Education of all students enrolled in primary and secondary education. From this data set, it is possible to determine who graduated from high school. The SIMCE data set contains test scores from annual student testing programs in Chile, as well as data on self-reported income.

There are two potential issues in using data on students who take part in the college admission system in Chile. First, students who do not complete the FUAS socioeconomic form before the PSU test are not eligible

[22] The assignment rule for SGL was fulfilled for all years except 2006, the first year of implementation. The commission managing the SGL program misassigned part of the loans. The tax authority ranked students from 1 to N, with 1 being the richest. The commission mistakenly considered the list in the opposite order and assigned all loans starting from the student ranked first. When the problem was realized, the loans were already announced and a new set of loans were issued in the correct order. Moreover, the data show that some students received this loan despite scoring less than the cutoff. Because of these problems, I do not consider 2006 in the analysis. In all other years, the assignment rule was fulfilled perfectly.

[23] SIMCE stands for System for Measurement of Education Quality (in Spanish, Sistema de Medición de la Calidad de la Educación).

for either loan program. Second, because students can choose to retake the PSU test in later years if they want to try to improve their score, there is a potential concern about the manipulation of scores around the loan eligibility threshold.

I address the first problem by restricting the main analysis to students who comply with all the requirements to be potentially eligible for a TUL or the SGL loan before they take the PSU test. For simplicity, I refer to these as "preselected" students in the remainder of the paper. For this sample of students, crossing the 475-point PSU test threshold implies a sharp change in access to tuition loans. To address the second problem, I restrict the sample to students who are first-time test takers and graduated from high school the same year they took the PSU test. I refer to the students who graduated from high school in November 2006 and took the PSU test that same month as the "2007 cohort." In all, I have information on three consecutive cohorts of students, from 2007 to 2009.

An important descriptive question is how the sample of students who participate in the college admission process differs from the overall population of students in Chile. To address this question, I use the administrative records from the Ministry of Education to track all students who graduated from eighth grade in 2004 through high school, until they participate in one admission process (if any), classifying them according to self-reported income in eighth grade (from SIMCE 2004).[24] Roughly 80 percent of students observed in eighth grade in 2004 graduated from high school sometime between 2008 and 2011, and conditional on high school graduation, just over 80 percent took the PSU admission test between 2008 and 2012.[25]

Appendix A describes the rate of participation in the admission process by income quintile.[26] As expected, students from lower-income families have a relatively high dropout rate (around 30 percent) and are less likely to take the PSU after high school graduation if they complete high school. Nevertheless, about 50 percent of the students from poor backgrounds end up participating in the admission process. Finally, among all students who took the PSU test, 60 percent applied for loans, suggesting that the admission process in Chile is a good scenario to test the importance of short-run credit constraints.

[24] According to the household survey CASEN 2009, 98.7 percent of the population finish eighth grade; therefore, this sample constitutes (almost) the universe of students for this cohort.

[25] See App. A for more details.

[26] This is based on self-reported income in the census test that took place during the students' eighth-grade year (SIMCE 2004). The correlation of this self-reported income with other measures of family income, such as self-reported income category in the PSU data set or the income quintile classification made by the tax authority, is between .45 and .65.

III. Empirical Strategy

A simple human capital model predicts that, in the absence of credit restrictions, the optimal decision is to enter college either immediately after high school or not at all. Thus, delays in college enrollment correlated with family income are suggestive of credit market failures (see Kane 1996). For this reason, the main variable of interest in this paper is college enrollment immediately after high school graduation (henceforth, immediate enrollment).

In a richer model of human capital accumulation with borrowing constraints, students without enough family resources could postpone college enrollment to work and save to finance the costs. This would result in differences in the time of initial enrollment between students from high- and low-income families, but not as much difference in the long-run enrollment rate. These differences could be potentially reduced by an effective student loan program. For this reason, I also analyze the probability of ever being enrolled in college (henceforth, ever enrolled) as the second variable of interest.

Finally, in models in which students differ in ability and may or may not know whether they can successfully complete college-level work (e.g., Stange 2012), loan programs may affect college enrollment but have little or no effect on the accumulation of advanced human capital. In such models, it is important to understand how a loan program affects not just entry to college but also persistence. Therefore, I also present evidence on ever being enrolled for 2 years and on the number of years enrolled in college.

A. *Immediate Enrollment*

My empirical strategy for measuring the effect of loan accessibility on college enrollment is to conduct an RD analysis on outcomes of students who score just above and just below the 475-point eligibility cutoff for the SGL and TUL loan programs. Hahn, Todd, and van der Klaauw (2001), van der Klaauw (2008), and Lee and Lemieux (2010) describe the conditions under which RD gives a causal estimation. The intuition is simple. If we assume that each individual's PSU score (the running or assignment variable) has a random component with a continuous density, then the probability of scoring ϵ above the cutoff or scoring ϵ below is the same (for a sufficiently small ϵ). Even if the expected PSU score depends on individual characteristics such as family background or latent ability, eligibility for treatment in the small neighborhood around the cutoff will be as good as randomly assigned (Lee 2008). In other words, students just below the cutoff can be used as a counterfactual for students just above the cutoff because the only difference between these two groups is that students above the cutoff receive the treatment.

Ideally, we would compare the average outcome for students in a small neighborhood around the threshold, but usually there are 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 RD that includes individuals away from the cutoff:

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

where Y_i is college enrollment;[27] $\mathbf{1}(T_i \geq \tau)$ is an indicator function for whether 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 admission test score on Y_i in a flexible nonlinear function $f(\cdot)$;[28] and ξ_i is a mean zero error. The parameter β_0 captures the expected value of Y_i for students just below the cutoff, and β_1 captures the increase in the expected value of Y_i for individuals just above the cutoff.

Including students away from the cutoff has the advantage of increased statistical power, achieved by 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).[29] The results shown in this paper are based on a local linear regression using the optimal bandwidth of Imbens and Kalyanaraman.[30]

Alternatively, to use the whole population of students, the following specification interacts the condition of being preselected for loans with the indicator for scoring at least at the cutoff:

$$\begin{aligned} Y_i = {} & \beta_0 + \beta_1 \cdot \mathbf{1}(T_i \geq \tau) + \beta_2 \cdot \text{PreSel}_i + \beta_3 \cdot \mathbf{1}(T_i \geq \tau) \cdot \text{PreSel}_i \\ & + f(T_i - \tau) + \xi_i. \end{aligned} \tag{2}$$

The variable PreSel_i is equal to one if student i was classified into one of the eligible income quintiles after filling out the FUAS form. The coef-

[27] This specification will also be used for testing the balance in baseline characteristics, in which case Y_i will be each characteristic.

[28] For instance, in the linear case, $f(T_i - \tau)$ estimates a linear function at each side of the cutoff:

$$f(T_i - \tau) = \phi_0 \cdot (T_i - \tau) + \phi_1 \cdot (T_i - \tau) \cdot \mathbf{1}(T_i \geq \tau).$$

For a polynomial specification, $f(\cdot)$ estimates a different polynomial for each side.

[29] They use a squared error loss function to weight these two biases.

[30] The bandwidth is calculated using the edge kernel. A uniform kernel gives a higher bandwidth, but results do not differ significantly. Bandwidth, point estimates, and standard errors calculated using Imbens and Kalyanaraman's optimal bandwidth are very similar to those calculated using the robust nonparametric confidence intervals proposed by Calonico, Cattaneo, and Titiunik (2014).

ficient β_2 captures whether there is any difference in the probability of enrollment between those who complete the FUAS and those who do not. Those who complete the socioeconomic form may be more interested in the loans because they have higher preferences either for college or for the terms of the loans. In this specification, the parameter of interest (β_3) measures the change in college enrollment rate for preselected students who score at or above the cutoff, which implies a change in their access to loans.

In this case, β_1 is the change in the probability of college enrollment at the cutoff for students not preselected for loans (nonselected hereafter), that is, those who did not complete the FUAS or were classified in the richest quintile. Because nonselected students do not experience any change in credit access if they score at or above the cutoff, it acts as a placebo test. This parameter captures whether scoring above the cutoff plays the role of a signal, either for students or for college admissions officers. The fact that the government offers financing to students scoring at least at the cutoff may be interpreted by students as a signal that they are suitable for college. Therefore, students' expectations about their own ability may not be continuous at the cutoff. On the other hand, admissions officers may discriminate in favor of students scoring at least 475 because they may expect that students with access to loans have a lower probability of dropping out, which translates into higher expected earnings for the institutions.

More importantly, this placebo test helps assess the importance of enrollment restrictions that use the same PSU cutoff. Some university programs accept applications only from students scoring 475 or more, and therefore, students with access to loans may face a larger choice set, which could in turn lead to a higher enrollment rate. This issue is discussed in detail in the online appendix.

B. *Ever Enrolled in College*

Estimating the effect of access to credit on enrollment in a longer horizon faces an additional problem: students can self-select into treatment by retaking the PSU test in subsequent years and scoring at or above the eligibility cutoff in those later attempts. I deal with this problem by using a fuzzy RD in which a student's PSU score on the first attempt serves as an instrument for ever being eligible for loans. All students above the cutoff in the first attempt are immediately eligible for a loan, while only some students below the cutoff will be eligible for loans in the following years if they retake the test and succeed in scoring 475 or more. Assuming that not all students who score just below the threshold retake the test and are able to score 475 or more points, there will still be a discontinuous jump in the ultimate availability of loans at the threshold.

This strategy measures a dynamic effect on so-called "compliers"—the subgroup of students whose eligibility status is fixed over time. The validity of the instrument is straightforward: it is clearly correlated with loan eligibility and is as good as random across the threshold.

Specifically, I perform a two-stage least squares (2SLS) regression as follows:

$$\text{Elig}_i = \gamma_0 + \gamma_1 \cdot \mathbf{1}(T_i \geq \tau) + f(T_i - \tau) + \eta_i, \tag{3}$$

$$Y_i = \beta_0 + \beta_1 \cdot \text{Elig}_i + f(T_i - \tau) + \nu_i. \tag{4}$$

The term $\mathbf{1}(T_i \geq \tau)$ is used as an instrument for ever being eligible for loans. The term Elig_i takes on the value one if student i is eligible for loans in any admission process (i.e., if a student scores above the cutoff in any PSU attempt after being classified in one of the four poorest income quintiles), and zero otherwise. The outcome of interest, Y_i, corresponds to ever enrolled in college. The control function f is defined as in equation (1).

Now, the parameter β_1 measures the effect of having access to loans on ever enrolling in college for those students for whom treatment status does not change after the first PSU attempt.

This strategy can also be applied to students who did not complete the FUAS form prior to the first time they took the PSU test or were classified in the richest income quintile, in order to perform a placebo test of the same nature as described in the previous section.

Finally, to increase efficiency of the estimation and to make sure that the group of preselected students does not constitute a strange sample, I will perform the same analysis for the whole population of test takers, interacting equations (3) and (4) with an indicator variable for preselected students as in equation (2).

C. *College Progress and Years of College*

To assess the effects of loan eligibility on longer-run human capital accumulation, I analyze both enrollment in the second year of college and the total number of college years. Owing to the limitations of my sample, I limit analysis of the second-year enrollment variable to students in the 2007 cohort of high school graduates, who are observed for up to 3 years after the time they took the PSU test.[31] The outcome variable takes the value of one if the student enrolls in any two of the three years since first writing the test.

[31] For the 2008 cohort, enrolling for 2 years would be equivalent to enrolling in two consecutive years, and therefore, the estimation would have a different interpretation.

As explained in the previous section, access to loans is not necessarily fixed over time because students can keep retaking the test until they become eligible for loans. To simplify the analysis, I will measure the effect of ever being eligible for loans on enrollment for 2 years in a 2SLS estimation as described before. The resulting 2SLS (or "treatment on the treated") effects show the impact of having at least 1 year of access to loans relative to the students who were never eligible. As in the previous discussion, I will instrument ever being eligible for loans with an indicator of whether the student scored at least the cutoff in the first attempt.

My second measure of progress in college is the number of years of enrollment. Again, I will present results for the 2007 cohort because it is the only one for which I have three years of data in all dimensions. Moreover, because eligibility for loans can change over time, I will measure the effect of ever being eligible for loans relative to students who never got access, using a 2SLS framework.

These definitions of progress in college are a consequence of data availability and do not allow us to distinguish between students who drop out permanently and students who suspend their studies temporarily to build up savings and ultimately return to college.[32]

IV. Results

This section presents the main findings. All of the following RD results are restricted to the group that took the PSU test for the first time immediately after they graduated from high school (see Sec. II.B for details). All regressions, unless otherwise specified, use a linear control function (f in eq. [1]) for students within the Imbens and Kalyanaraman optimal bandwidth (see Sec. III.A).

A. *Effect on College Enrollment*

The top panel of figure 1 shows the effect of loan eligibility on immediate enrollment. It shows the enrollment rate for the whole PSU domain of preselected students, that is, those who completed the FUAS form and were classified in one of the four eligible income quintiles. Each dot is the average enrollment rate for all students in bins of 2 PSU points. At the eligibility cutoff, where access to loans changes sharply for preselected students, we observe that the enrollment rate for barely eligible students is twice the rate for barely ineligible students.

Table 3 presents the corresponding RD estimates. Column 1, the preferred specification, shows the estimation of equation (1) for preselected

[32] Additionally, I do not observe class performance for these students while in college; therefore, this definition is agnostic about students' true advancement in coursework.

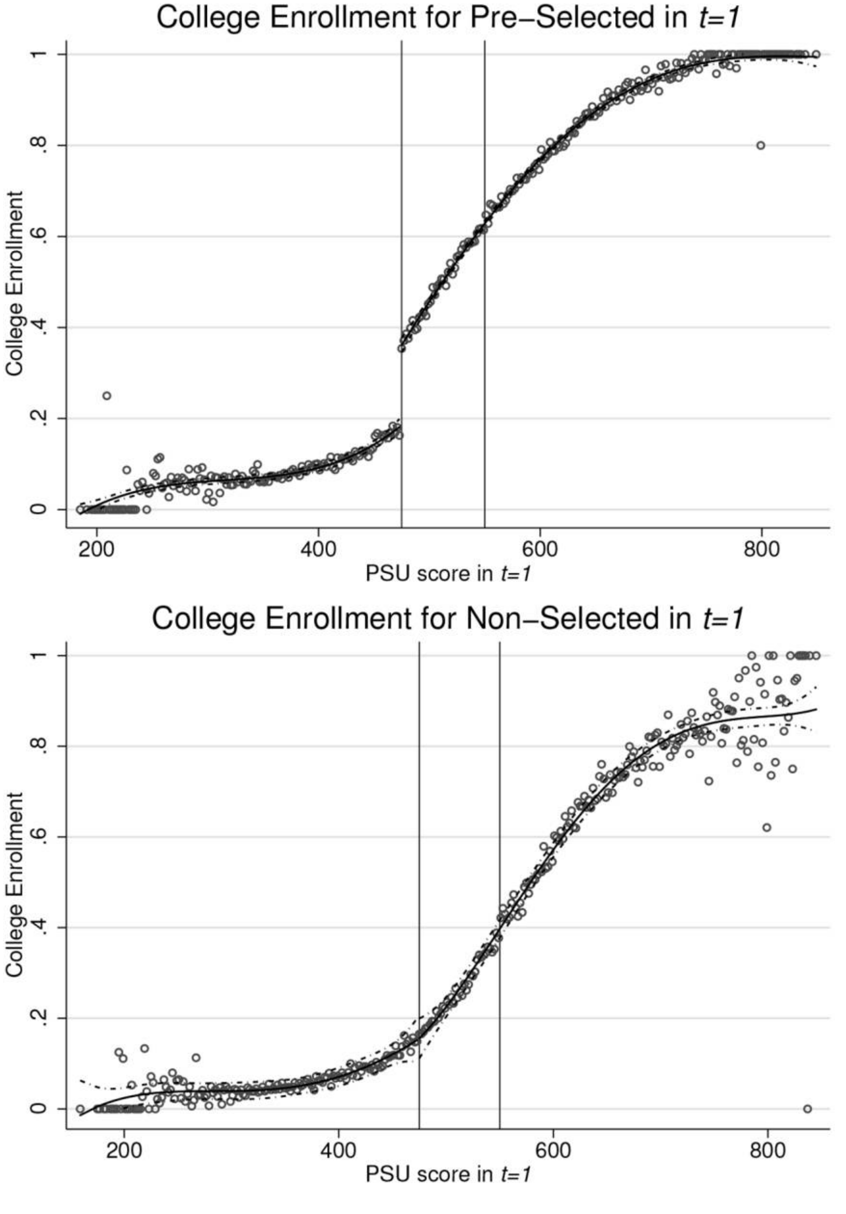

FIG. 1.—RD for immediate college enrollment. Each dot represents average college enrollment within bins of 2 PSU points. The top figure considers PSU first-time takers who applied for benefits and were classified as eligible for loans by the tax authority (preselected students). The bottom figure considers students who did not complete the FUAS or were classified in the richest income quintile (nonselected). Cohorts 2007–9 are pooled together. The vertical lines at 475 and 550 correspond to the loan cutoff and the Bicentenario scholarship, respectively. The dashed lines represent fitted values from the estimation of equation (1) where $f(\cdot)$ is a fourth-order polynomial at each side of the cutoff and 95 percent confidence intervals.

TABLE 3

Immediate College Enrollment: Dependent Variable

	Preselected	Nonselected	Population, 2007–9 Pooled			Population, by Year		
						2007	2008	2009
	Linear (1)	Linear (2)	Linear (3)	4th-Order Polynomial (4)	Linear (5)	Linear (6)	Linear (7)	Linear (8)
$\mathbf{1}(T \geq \tau) \times$ Preselected			.172	.162	.162	.201	.149	.166
			(.008)***	(.008)***	(.026)***	(.014)***	(.015)***	(.014)***
Preselected			.024	.024	.021	.001	.029	.039
			(.005)***	(.006)***	(.019)	(.009)	(.010)***	(.009)***
$\mathbf{1}(T \geq \tau)$	.175	.003	.003	.007	.025	−.010	.010	.010
	(.006)***	(.006)	(.006)	(.006)	(.018)	(.009)	(.010)	(.009)
Constant	.182	.159	.159	.159	.135	.154	.180	.141
	(.004)***	(.004)***	(.004)***	(.004)***	(.015)***	(.006)***	(.007)***	(.006)***
Observations	78,072	69,566	147,638	475,165	14,438	46,153	48,081	53,404
R^2	.108	.02	.102	.351	.05	.104	.094	.109
Bandwidth	44	44	44	All	4	44	44	44

Note.—This table shows regression results for immediate college enrollment for PSU first-time takers (cohorts 2007, 2008, and 2009). Column 1 reports the estimation of eq. (1) ($f(\cdot)$ not shown and 44 points around the cutoff) for students who applied for benefits and were classified as eligible for loans for the tax authority (preselected students). Column 2 reports the same estimation for students who did not apply for benefits or were classified in the richest income quintile (nonselected students). Columns 3–8 consider the whole population of first-time takers (eq. [2]). The control function, f, of eq. (1) or (2) is linear in all columns except col. 4, which uses a fourth-order polynomial. Columns 6–8 show the regression for each year separately. Robust standard errors are in parentheses.

* $p \leq 10$ percent.

** $p \leq 5$ percent.

*** $p \leq 1$ percent.

students. It shows that scoring above the cutoff implies an increase of 17.5 percentage points in the probability of enrolling in college immediately after the test. This represents a relative increase of nearly 100 percent: the enrollment rate for barely eligible students is 36 percent compared to a rate of 18 percent for ineligible students. The results are statistically the same using different bandwidths and different functional forms (see the online appendix). This is also evident from figure 1, which depicts fitted values for a regression using a fourth-order polynomial on each side of the cutoff. Both the fitted values and their 95 percent confidence intervals can hardly be seen, indicating the robustness of the effects.

As discussed in Section III.A, it is possible that the estimates in column 1 are confounded by an effect of passing the 475-point threshold that is not purely due to loan access, for example, if students are more likely to be offered admission if they score above the loan threshold. One way to detect this kind of bias is to perform a placebo test by running the same regression for "nonselected" students: those who are not eligible for TUL or SGL loans because they did not complete the requisite forms prior to the PSU test or were classified in the richest income quintile. In the presence of such biases, we should also observe an increase in enrollment at the cutoff for this group; the bottom panel of figure 1 suggests that there is no jump. This is confirmed in column 2, which shows that enrollment for nonselected students does not increase, and therefore, none of these behavioral responses is biasing the effect of loan access.

To show how preselected students differ from the overall population, I combined them with the nonselected students to estimate equation (2) in column 3. The pooled results show that preselected students are more prone to enroll, suggesting that they have a higher preference for college. They also confirm the previous findings and are essentially the same regardless of either the chosen bandwidth or the specification of the control function. Column 4 estimates the same equation using the whole domain of PSU scores and a fourth-order polynomial for each of the four groups of students (preselected and nonselected, below and above the cutoff), while column 5 uses local linear regression for a very small window (4 PSU points) around the eligibility cutoff.[33] In all cases, scoring at least 475 implies a relative increase of roughly 100 percent in the probability of enrollment for preselected students. Reassuringly, the estimated effects do not appear to be driven by any specific cohort: columns 6–8 exploit the fact that the cutoff creates an independent natural experiment each year, estimating equation (2) for each cohort separately and finding similar results.

[33] Four PSU points is the maximum bandwidth where simple *t*-tests of difference of means for baseline characteristics fail to reject the null hypothesis of no difference.

Next, I turn to results using "ever enrolled in college" as the outcome of interest. The upper panel in figure 2 shows the means of this outcome for preselected students and suggests that there is a large reduced-form effect on enrollment from passing the eligibility threshold. Column 1 in table 4 shows the corresponding 2SLS estimation (see Sec. III.B). The probability of ever going to college increases by 16 percentage points, representing a relative increment of 50 percent. The effect is slightly smaller than the equivalent parameter for immediate enrollment shown in table 3, but not statistically different. Moreover, as was true for the measure of immediate enrollment, there is no corresponding effect on the longer-run enrollment behavior of nonselected students (see col. 2). Column 3 fits the same specification to a pooled sample of preselected and nonselected students, in a fully interacted regression with an indicator of being preselected (i.e., the interaction of eqq. [3] and [4] with an indicator of being preselected for loans in the first attempt). Ever being eligible is instrumented by the interaction of the indicator of being preselected and the indicator for scoring more than the threshold in the first PSU attempt. The results are very similar to those in table 3. Scoring above the threshold leads to a 15 percentage point increase in the probability of ever enrolling (a 46 percent gain relative to the rate for students just below the threshold). Interestingly, this effect is only a little smaller than (and is not statistically different from) the effect on immediate enrollment shown in table 3.[34] Columns 4 and 5 present different specifications and bandwidths and confirm the robustness of the baseline specification.

The path of enrollment dynamics in the years after the first attempt to write the PSU test can be inferred by comparing the estimated impacts for different cohorts in columns 6–8 of table 4 to the corresponding estimates in table 3. Given the way the variables are measured, ever enrolled for the 2009 cohort is equivalent to immediate enrollment; ever enrollment for the 2008 cohort is equivalent to enrolling immediately or 1 year later; and ever enrollment for the 2007 cohort is equivalent to enrolling in any of the 3 years after first writing the test. Notice that for the 2007 cohort, the jump in ever enrollment at the cutoff (15.2 percent) is about three-quarters as large as the jump in initial enrollment (20.1 percent). Thus, some of the students who failed to score above the threshold on their first attempt and did not go to college the next year eventually enter college. For the 2008 cohort, the jump in ever enrollment at the cutoff (9.6 percent) is about two-thirds as large as the jump in initial enrollment (14.9 percent), suggesting that most of the convergence observed over a 3-year period occurs relatively quickly.

[34] Moreover, col. 3 shows that preselected students have a higher enrollment rate than nonselected.

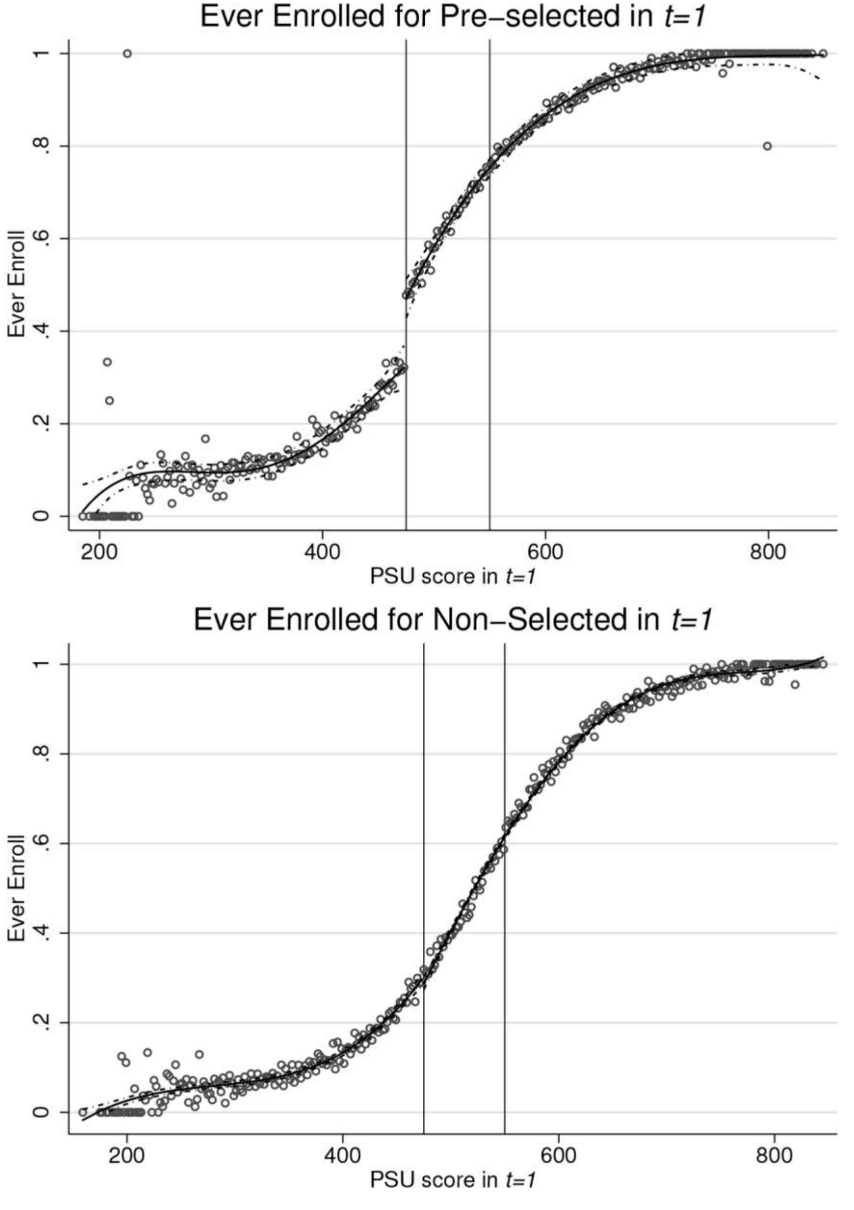

Fig. 2.—Ever enrolled in college. Each dot represents average ever enrollment (enrollment in any year between 2007 and 2009) within bins of 2 PSU points. The top graph shows enrollment for preselected students and the bottom for nonselected students (see note on fig. 1). All cohorts are pooled together. The vertical lines at 475 and 550 correspond to the loan cutoff and the Bicentenario scholarship, respectively. The dashed lines represent fitted values from the estimation of equation (1) where $f(\cdot)$ is a fourth-order polynomial at each side of the cutoff and 95 percent confidence intervals.

TABLE 4

EVER ENROLLMENT IN COLLEGE: DEPENDENT VARIABLE

	Preselected	Nonselected	Population, 2007–9 Pooled			Population, by Year		
						2007	2008	2009
	Linear (1)	Linear (2)	Linear (3)	4th-Order Polynomial (4)	Linear (5)	Linear (6)	Linear (7)	Linear (8)
Ever eligible	.155		.146	.151	.128	.152	.096	.167
	(.008)***		(.011)***	(.011)***	(.035)***	(.023)***	(.023)***	(.014)***
Preselected			.030	.027	.040	.027	.071	.039
			(.007)***	(.007)***	(.024)	(.014)*	(.013)***	(.009)***
$\mathbf{1}(T \geq \tau)$		.009	.007	−.004	.034	−.007	.023	.009
		(.007)	(.007)	(.007)	(.022)	(.013)	(.012)*	(.009)
Constant	.316	.300	.287	.290	.256	.393	.328	.142
	(.005)***	(.005)***	(.004)***	(.004)***	(.017)***	(.008)***	(.007)***	(.006)***
Observations	78,072	69,566	147,638	475,165	14,438	46,153	48,081	53,404
R^2	.119	.033	.113	.387	.059	.114	.114	.109
Bandwidth	44	44	44	All	4	44	44	44

NOTE.—This table shows regression results for ever enrolled in college for PSU first-time takers (cohorts 2007, 2008, and 2009). Column 1 reports 2SLS estimations of eq. (3) ($f(\cdot)$ not shown and 44 points around the cutoff) for students who applied for benefits and were classified as eligible for loans for the tax authority (preselected students). Column 2 reports the same estimations for students who did not apply for benefits or were classified in the richest income quintile (nonselected students). Columns 3–8 consider the whole population of first-time takers and are calculated by interacting eqq. (3) and (4) with the indicator for whether the student was preselected for loans. The specification of the control function is linear in all columns except col. 4, which uses a fourth-order polynomial. Columns 6–8 show the regression for each year separately. Robust standard errors are in parentheses.

* $p \leq 10$ percent.

** $p \leq 5$ percent.

*** $p \leq 1$ percent.

B. *Enrollment Gap by Family Income*

To explore whether increased credit access helps reduce the existing enrollment gap between students from high- and low-income families, I estimate the effect of access to loans on the probability of enrollment by income quintile. This analysis is equivalent to that in Section IV.A, comparing individuals with and without access to loans, but within income quintiles.[35]

Panel A in table 5 shows the estimation for immediate enrollment. Each column shows the estimation results for a different income quintile, pooling all three available cohorts together. As might be expected, the effects of loan eligibility are largest for the poorest quintile—a relative increment of 138 percent—declining monotonically with income, though the differences between the second and third quintiles are minor. For the fourth quintile, the impacts of loan eligibility are relatively modest. Finally, and reassuringly, the (ineligible) richest income quintile shows no effect, confirming the absence of behavioral responses associated with simply passing the 475-point threshold.

To explore the robustness of these results, figure 3 shows the estimations in graphical form. We clearly observe the effect for the three poorest quintiles; arguably, the effects are not driven by the specification of the control function or by bandwidth selection.

Panel B in table 5 presents the 2SLS estimations of ever being eligible for loans on ever enrolling in college (eqq. [3] and [4]) for each income quintile. The first-stage estimation results, shown in panel C, indicate that some students who were initially ineligible for loans ultimately become eligible (this fraction is indicated by the constant in the first-stage model). Interestingly, 14.6 percent of the students originally assigned to the richest income quintile ultimately become eligible for a loan, presumably because of income changes that lead them to be reclassified into lower-income quintiles. Comparing the 2SLS estimates for ever enrolled in college with the estimates in panel A for immediate enrollment, we see that in all four eligible income quintiles, the longer-run enrollment effect is slightly smaller than the immediate effect. Again, for the highest-income quintile, there is no impact of initial loan eligibility on longer-run enrollment, suggesting that the impact results for the other quintiles are due to loan eligibility and not to other factors that might be linked to the 475 cutoff score.

Figure 4 shows the reduced-form estimation by income quintile of equations (3) and (4) in graphical form. It shows that the effects are sizable and do not depend on functional form or bandwidth selections.

[35] Information on income quintiles is missing for all the students who did not complete the FUAS form, and therefore, the placebo test using the whole population of test takers cannot be performed, except for those students classified in the richest income quintile.

TABLE 5
Immediate and Ever Enrollment by Income Quintile

	q_1 (1)	q_2 (2)	q_3 (3)	q_4 (4)	q_5 (5)
	A. Dependent Variable: Immediate Enrollment				
$\mathbf{1}(T \geq \tau)$	.199	.169	.149	.069	.015
	(.008)***	(.013)***	(.015)***	(.017)***	(.016)
Constant	.144	.196	.228	.289	.287
	(.005)***	(.009)***	(.010)***	(.012)***	(.012)***
	B. Dependent Variable: Ever Enrolled (2SLS)				
Ever eligible	.183	.136	.129	.066	.028
	(.010)***	(.017)***	(.019)***	(.020)***	(.018)
Constant	.257	.337	.392	.468	.491
	(.007)***	(.012)***	(.014)***	(.016)***	(.013)***
	C. Dependent Variable: Ever Eligible (First Stage)				
$\mathbf{1}(T \geq \tau)$	.878	.844	.858	.867	−.004
	(.004)***	(.008)***	(.008)***	(.009)***	(.012)
Constant	.122	.156	.142	.133	.146
	(.004)***	(.008)***	(.008)***	(.009)***	(.009)***
Observations	42,120	17,007	14,447	12,550	12,225
Imbens and Kalyanaraman bandwidth	46	44	57	61	79

Note.—The sample corresponds to PSU first-time test takers with income quintile information (cohorts 2007, 2008, and 2009 pooled together). Income quintile information is not available for students who do not complete the FUAS form. Columns report the estimation by quintile (linear $f(\cdot)$ not shown and Imbens and Kalyanaraman optimal bandwidth for each quintile). Panel A reports the ordinary least squares estimates of eq. (1) by quintile for first-year enrollment. Panel B shows 2SLS estimates of eqq. (3) and (4) on ever enrolled in college. Panel C shows the first stage on the estimation of panel B. Robust standard errors are in parentheses.

* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

Figure 5 presents a graphical summary of the impacts of loan availability on different income groups, the income gradient. The left figure in panel A shows the impact estimates of loan availability on immediate enrollment by quintile group, while the right figure shows the immediate enrollment rates of students just below and just above the 475-point threshold in each quintile group. Note that enrollment rates for the group without access to loans increase with family income, from 15 percent for those in quintile 1 to 30 percent for those in quintile 5. By contrast, among students with access to loans, the enrollment gap disappears: the enrollment rate is 34.3 percent for the poorest quintile and 30.5 percent for the richest.[36]

[36] The difference is statistically different from zero at the 10 percent level, but with the sign opposite of that expected. One might be concerned that the enrollment behavior of high-income students is biased because some students from the richest income quintile will

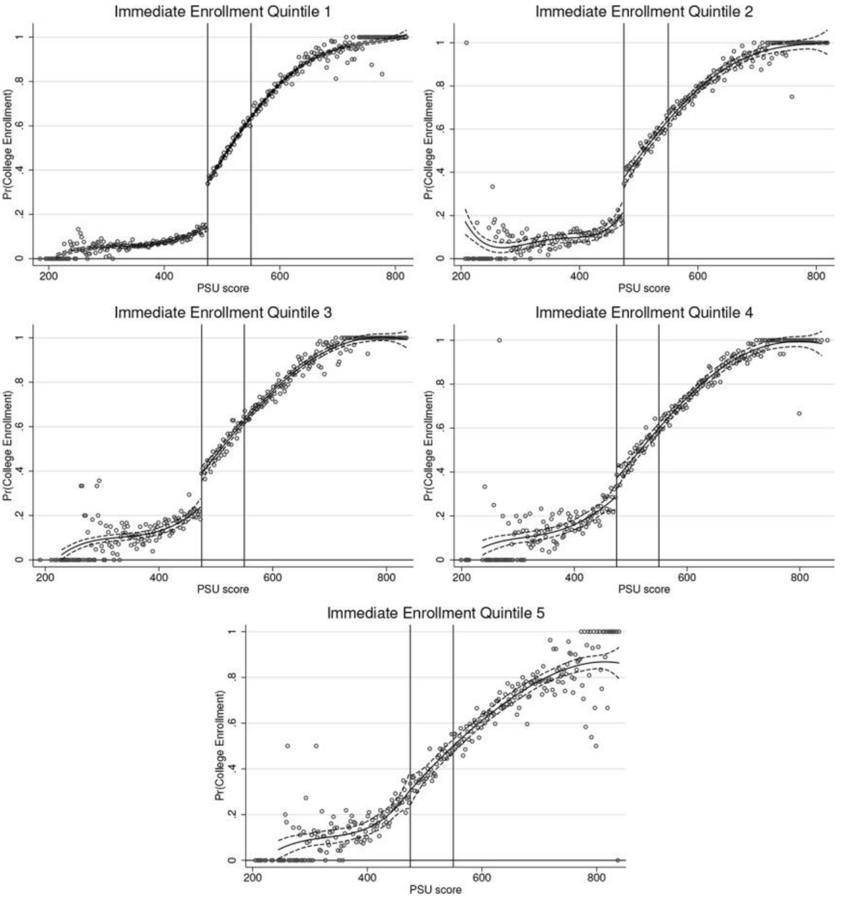

FIG. 3.—RD for immediate college enrollment by income quintile. Graphs show immediate enrollment by income quintile (quintile 1 being the poorest). Each dot represents average college enrollment within bins of 2 PSU points. The figures consider PSU first-time takers who applied for benefits and were classified as eligible for loans by the tax authority (preselected). Cohorts 2007–9 are pooled together. The vertical lines at 475 and 550 correspond to the loan cutoff and the Bicentenario scholarship, respectively. The dashed lines represent fitted values from the estimation of equation (1) where $f(\cdot)$ is a fourth-order polynomial spline and 95 percent confidence intervals for each side.

For eligible students around the test score cutoff (i.e., those who have graduated from high school, taken the PSU test, and scored in the neighborhood of 475 points), access to these loan programs eliminates the family income gap in immediate college enrollment.

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not apply for benefits, knowing that they are ineligible. This may account for the dip in enrollment rates for the fifth quintile group. Nevertheless, the conclusion is the same if we just compared the four eligible quintiles. The enrollment rate for the poorest (34.3 percent) is statistically the same as the enrollment rate for the fourth quintile (35.8 percent).

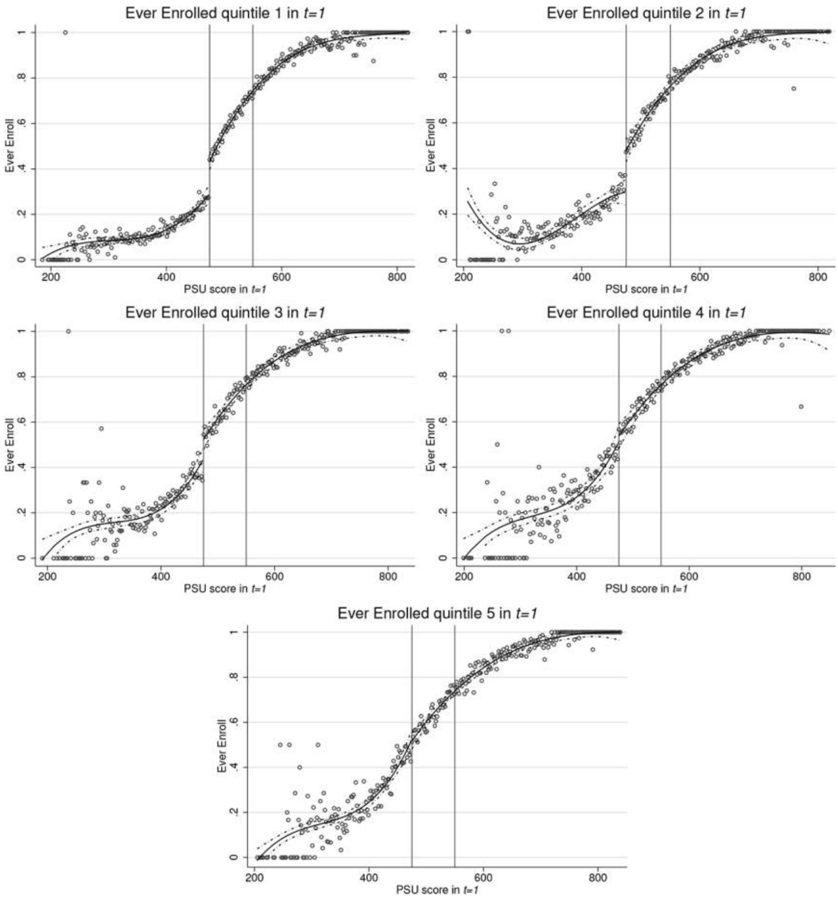
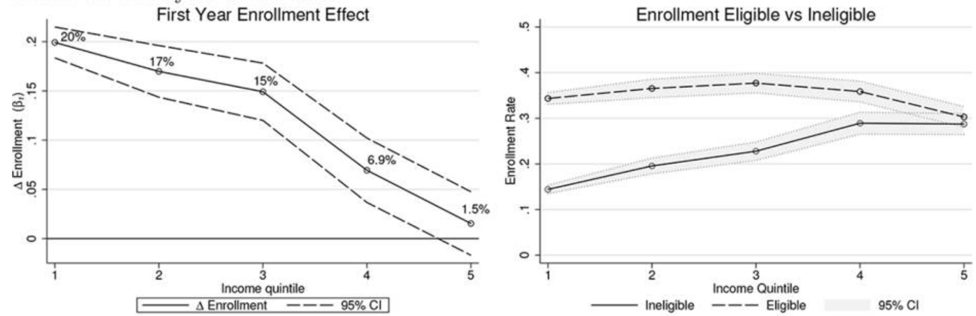


FIG. 4.—Ever enrolled by income quintile. Graphs show ever enrollment by income quintile. Each dot represents the average enrollment within bins of 2 PSU points. The figures consider PSU first-time takers who applied for benefits and were classified as eligible for loans in $t = 1$ by the tax authority (preselected). The vertical lines at 475 and 550 correspond to the loan cutoff and Bicentenario scholarship, respectively. The dashed lines represent fitted values from the estimation of equation (1) where $f(\cdot)$ is a fourth-order polynomial spline and 95 percent confidence intervals for each side.

Panel B of figure 5 repeats this analysis using the probability of ever enrolling in the period up to 3 years after high school. As before, the left figure shows how the impacts of loan eligibility vary across the income quintiles, while the right panel shows how the probability of ever enrolling varies across income groups for students who are just above and just below the 475-point threshold. For students who score just below the threshold, there is a relatively large gradient in enrollment, rising from 25 percent for the poorest quintile to 50 percent for the richest. In contrast,

Panel A: First year enrollment.



Panel B: Ever enroll in college

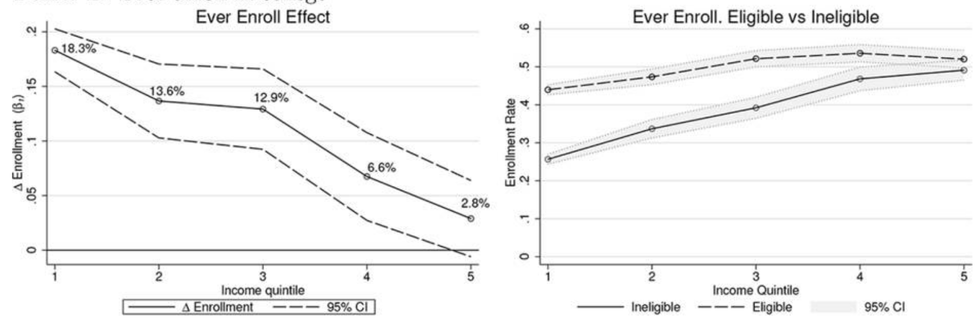


Fig. 5.—Enrollment gap by family income quintile. Panel A: First-year enrollment; panel B: ever enrolled in college. All figures are constructed using PSU first-time takers who applied for benefits (and, thus, with information on income quintile). All cohorts are pooled together. In the graphs on the left, each dot represents the magnitude of the effect of being eligible for loans on first-year college enrollment (panel A) and ever enrolling in college (panel B) by income quintile and 95 percent confidence intervals from robust standard errors (estimations of β_1 in eq. [1]), as in table 5, panels A and B. The right figures show the estimated enrollment (first year in panel A and ever enroll in panel B) for students at both sides of the cutoff: Ineligible corresponds to the enrollment rate for barely ineligible students (estimation of β_0 in eq. [1]); eligible corresponds to the enrollment rate for barely eligible for loans (estimation of $\beta_0 + \beta_1$ in eq. [1]).

for students who score just above, the gradient is substantially reduced, ranging from 44 percent for the poorest quintile to 52 percent for the top quintile. The reduced impact of loan access on the gradient in the probability of ever enrolling is consistent with the fact that students from richer quintiles are more likely to retake the PSU test, as is discussed in more detail in Section IV.F below.

C. *Internal Validity and the Characteristics of Compliers*

In order to have a valid RD design, loan assignment should be random at the cutoff. Imbens and Lemieux (2008) propose several tests of the validity of the RD design that also allow a researcher to identify the marginal students affected by access to loans. Building on their suggestions, I will first test whether scoring just above the eligibility threshold leads to a change in the probability of receiving access to loans. Second, I will look for evidence on whether the scores on the PSU test have been manipulated. Third, I will check whether assignment to loan eligibility is correlated with any baseline characteristic. Finally, I will describe the characteristics of the compliers.

First, among preselected students, the probability of being eligible for loans jumps sharply from zero to one at the cutoff by definition of the program. The top graphs in figure 6 confirm this. Nobody scoring less than the threshold receives the loan, while roughly one-third of preselected students scoring at or just above 475 points take up the loan. This holds for both the two poorest income quintiles and the next two quintiles, which are shown in the left-hand graph and right-hand graph, respectively. The left figure shows how students from the two poorest income quintiles replace the loan by a scholarship that becomes available when they score 550 or more PSU points and enroll in traditional universities (see more in Sec. V).

Second, because the eligibility conditions are public knowledge, students may try to self-select into treatment by manipulating their score to be just above the cutoff. If this were the case, we should observe a change in the density ("bunching") just above the cutoff (McCrary 2008). The bottom graph of figure 6 shows that the empirical density appears to be continuous across the threshold, confirming that the PSU scores are not subject to manipulation.[37] This graph also shows that the cutoff is slightly below the mean PSU score, which, used as an ability measure, indicates that the policy is affecting more or less average students. The loan eligibil-

[37] During the test, all students have incentives to exert maximum effort because scores will be used to determine who is accepted or wait-listed. Moreover, the tests have only multiple-choice questions graded automatically by a photo-optical device; therefore, the scores are not subject to manipulation by students or graders.

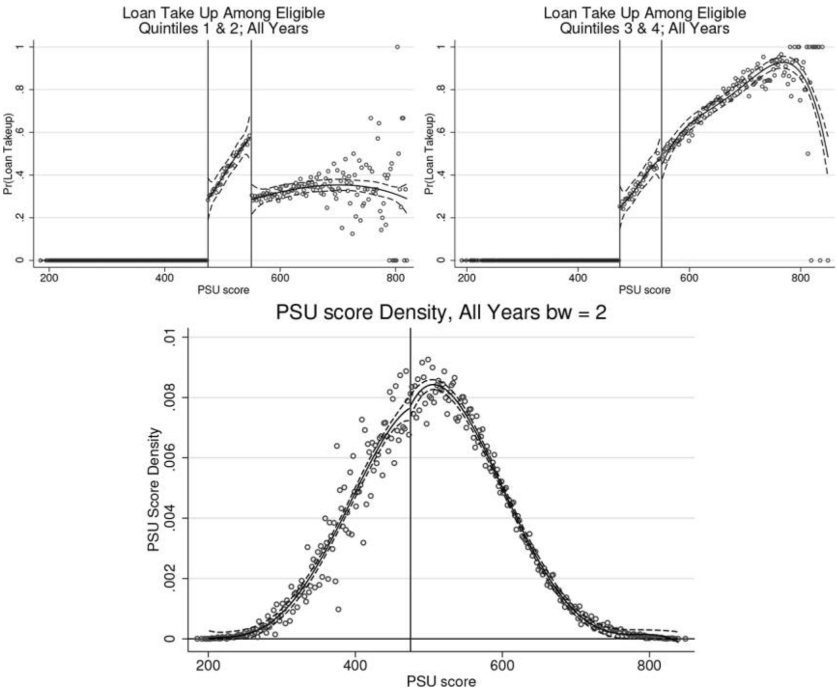


FIG. 6.—Loan take-up and PSU score density. The top-left figure shows loan take-up for the two poorest income quintiles, which are also eligible for the Bicentenario scholarship. The top-right figure shows loan take-up for the next two income quintiles, which are not eligible for the Bicentenario scholarship. The bottom figure shows the empirical density of PSU scores. Each dot indicates the average rate of each characteristic for students with scores within intervals of 2 PSU points. The solid lines represent fitted values from the estimation of equation (1) where $f(\cdot)$ is a fourth-order polynomial for each side of the cutoff. The dashed lines represent 95 percent confidence intervals. The vertical lines indicate the loan eligibility cutoff (475), and in the graphs on top, the second vertical line corresponds to the Bicentenario scholarship cutoff (550). The three cohorts (2007, 2008, and 2009) of first-time takers are pooled together.

ity threshold corresponds to roughly the 41st percentile in the ability distribution and, for the lowest income quintile, coincides with the average student.[38]

Table 6 confirms that there are no discontinuities in the mean characteristics of students just below and just above the eligibility threshold. As a point of reference, columns 1 and 2 show the means and standard deviations of the characteristics identified in the left-hand column for the entire population of first-time test takers in the 2007–9 cohorts. Columns 3,

[38] The score distribution has a mean of 500 and a standard deviation of 110. Appendix A shows the PSU score densities by income quintile for all students who applied for benefits.

TABLE 6
BALANCE OF COVARIATES: POPULATION AND PRESELECTED STUDENTS

	Population					Preselected			Enrolled in College			
Variable	Mean (1)	SD (2)	Level (3)	Diff. (4)	SE (5)	Level (6)	Diff. (7)	SE (8)	Level (9)	Diff. (10)	SE (11)	Compliers (12)
Student characteristics:												
High school GPA	56.30	8.54	54.0	−.191	(.100)*	55.15	−.046	(.118)	54.99	.620	(.620)**	56.25
Public school	.45	.50	.44	.005	(.005)	.48	.011	(.007)	.41	.079	(.079)***	.57
Voucher school	.52	.50	.52	−.005	(.005)	.51	−.010	(.007)	.56	−.066	(.066)***	.42
Private school	.03	.17	.04	.000	(.002)	.01	−.002	(.002)	.02	−.012	(.012)***	.00
Female	.57	.50	.54	.009	(.005)*	.60	.001	(.007)	.60	.016	(.016)	.64
Parent education:												
Mother years of education	11.09	3.51	10.95	−.013	(.038)	10.67	−.004	(.051)	11.37	−.456	(.456)***	10.44
Father years of education	11.12	3.73	11.02	−.003	(.042)	10.64	−.013	(.056)	11.49	−.665	(.665)***	10.13
Mother dropout high school	.41	.49	.44	−.001	(.005)	.46	−.002	(.007)	.38	.051	(.051)***	.48
Father dropout high school	.44	.50	.45	−.002	(.005)	.48	−.001	(.007)	.38	.073	(.073)***	.53
Mother college graduate	.07	.25	.06	.003	(.003)	.04	.001	(.003)	.07	−.017	(.017)**	.03
Father college graduate	.07	.26	.07	.000	(.003)	.05	.000	(.003)	.08	−.029	(.029)***	.02

Family background:												
Income quintile	2.00	1.12	2.09	.007	(.018)	1.83	.017	(.015)	2.10	−.221	(.221)***	1.64
Self-reported income	1.36	.53	1.39	.002	(.006)	1.28	−.004	(.007)	1.41	−.114	(.114)***	1.18
Household size	4.46	1.79	4.47	−.001	(.020)	4.49	−.019	(.026)	4.46	.075	(.075)	4.61
Mother has formal work	.30	.46	.29	.002	(.005)	.28	−.002	(.006)	.31	−.031	(.031)**	.25
Father has formal work	.53	.50	.55	.005	(.005)	.52	.006	(.007)	.54	−.026	(.026)*	.49
Mother housewife	.50	.50	.50	−.002	(.005)	.51	.003	(.007)	.49	.026	(.026)*	.54
Both parents work	.15	.36	.16	.002	(.004)	.14	−.001	(.005)	.17	−.033	(.033)***	.10
Observations	230,653			146,755			77,544			23,668		

Note.—Columns 1 and 2 correspond to the mean and standard deviation for the whole population of first-time test takers. Columns 3 onward report estimates from RD regressions. Level and Diff. refer to β_0 and β_1 in eq. (1), respectively (the average characteristic for the control group at the cutoff and the jump in the average for the treatment group at the cutoff, respectively). All regressions use a linear control function restricted to students within the optimal bandwidth (44 points). Columns 3–5 include the whole population of students within the optimal bandwidth, while cols. 6–8 restrict the sample to preselected students. Columns 9–11 restrict the sample to students enrolled in college in the first attempt. Column 12 shows the average characteristic at the cutoff for the compliers who enroll in college (see fn. 40). Self-reported income: family income reported by the students (three categories, 1 being the lowest). High school GPA: ranges from 40 (minimum passing grade) to 70. Public, voucher, and private high school: indicators for the type of high school. Mother/father has formal work: indicator for whether parents have a job in the formal sector. Mother housewife: indicator for whether the student's mother stays at home (*dueña de casa*). Robust standard errors are in parentheses.

* $p \leq 10$ percent.

** $p \leq 5$ percent.

*** $p \leq 1$ percent.

4, and 5 show the intercept, change in the intercept at the cutoff, and its standard error from local linear RD models fit to samples of students within the optimal Imbens and Kalyanaraman bandwidth around the 475 cutoff.[39] Columns 6, 7, and 8 report similar statistics for local linear RD models fit to the subsample of preselected students whose scores fall within the optimal bandwidth of the cutoff. Importantly, only two of the 18 RD estimates reported in column 4 are marginally significant, and none of the RD estimates reported in column 7 are significant at even the 10 percent level. These results confirm that there is "balance" between the characteristics of students just below and just above the cutoff, as would be expected for a valid RD design. This same balance is also true for each income quintile considered separately and for students who retake the test in subsequent admission years (see App. B).

Finally, inspection of table 6 also shows that the marginal students affected by the eligibility cutoff are very similar to the average students in the pool of test takers. This can be seen by comparing the mean characteristics in the population (col. 1) to the estimated mean characteristics of students just to the left of the cutoff (in col. 3). On the other hand, comparisons of column 1 with the estimated intercepts in the RD models for preselected students (in col. 6) reveal that preselected students come from slightly poorer backgrounds than the average student. This follows because students in the fifth income quintile are excluded from the preselected sample. However, these differences are presumably of little economic relevance; for mother's education, for instance, the difference is 0.42 year, about 0.11 standard deviations. These comparisons indicate that the local average treatment effect is calculated on a sample of students who are almost the same as the average student in the population.

Finally, columns 9–11 report estimates for RD models fit to the population of students who enrolled in college. These estimates—many of which are statistically significant—indicate how the characteristics of enrollees change as we move from just below the 475 threshold to just above. For example, the entries in row 6, columns 9 and 10, show that enrollees with scores just under 475 points have mean maternal education of 11.37 years, while enrollees just above the threshold have mean maternal education of $11.37 - 0.456 = 10.91$ years. Borrowing the nomenclature from the instrumental variable literature, students with scores just below the threshold are always-takers, while those with scores just above the threshold are a combination of always-takers and compliers. Comparisons of columns 9 and 10 show that the compliers have lower socioeconomic status (e.g.,

[39] The average income quintiles calculated in cols. 1–4 use the sample of students who have information on income quintile (individuals who completed the FUAS form), while the value for this variable in col. 6 is restricted to preselected students only (excluding students from the richest income quintile and with no information).

mother's and father's education, family income) than the always-takers but higher high school GPAs than the always-takers. In other words, students who attend college as a result of the loan program are from less advantaged families, but have higher precollege achievement, than those with similar test scores who would have attended regardless of the loans.

Because we know the share of students who enroll at each side of the cutoff (18 percent and 36 percent according to col. 1 in table 3), we can use the data in columns 9 and 10 to derive the average characteristics of the compliers who were induced to enroll after they received access to loans.[40] The resulting means, shown in column 12, suggest that compliers are relatively "smarter" but "poorer" than the always-takers. This is in line with the argument of Card (1999, 2001), which suggested that the marginal students affected by policies that relax credit constraints could have relatively high abilities.

D. *College Progress and Years of College*

In this section, I explore the effects of loan eligibility on two measures of college persistence: enrollment in a second year of college and the number of years of college enrollment. As noted above, I restrict the analysis to the 2007 cohort, which is observed for 3 years after graduating from high school and writing the PSU test for the first time. Panels A and B in table 7 present 2SLS estimation of fuzzy RD models for these outcomes, using initial eligibility for a loan (based on the first PSU test) as an instrumental variable for loan eligibility status.[41]

The estimates in panel A show that the probability of enrolling in a second year of college increases by 16 percentage points for preselected students who are ever eligible for loans, equivalent to a 53 percent increase in enrollment, while there is no effect for nonselected students (cols. 1 and 2, respectively). In columns 3–5, I make use of the whole population of test takers and estimate specifications that include an indicator for preselected status and an indicator for whether the first PSU score was above the threshold. The (endogenous) indicator for loan eligibility at any time over the 3-year horizon is again instrumented with a dummy indicating whether the student was eligible for a loan on the basis

[40] Let m_0, m_1, and m_c be the average characteristic for enrolled students below the cutoff, above the cutoff, and compliers (c), respectively. The average characteristic for enrolled students above the cutoff, m_1, is a weighted average between always-takers (at), m_0, and compliers, m_c, $m_1 = (p_{at} \cdot m_0 + p_c \cdot m_c)/(p_{at} + p_c)$ (p_j is the share of group, $j = at$ or c). Therefore, we can compute the average characteristic for compliers as $m_c = (1/p_c) \cdot [m_1 \cdot (p_{at} + p_c) - m_0 \cdot p_{at}]$.

[41] The bandwidth used is the one for the sharp RD case for the sample of preselected students. The Calonico et al. (2014) bandwidth selection method for a fuzzy RD suggests a slightly larger one, though the results are almost the same (the difference is at the third decimal place).

TABLE 7
Ever Enrolled in 2 Years and Years of College

	Preselected (1)	Nonselected (2)	Population, Cohort 2007 (3)	(4)	(5)
	A. Dependent Variable: Ever Enrolled in 2 Years				
Ever eligible	.158		.169	.169	.165
	(.016)**		(.022)***	(.022)***	(.077)**
Preselected			.043	.045	.044
			(.013)***	(.014)***	(.056)
$\mathbf{1}(T_1 \geq \tau)$		−.006	−.009	−.019	.015
		(.012)	(.012)	(.011)*	(.043)
Constant	.299	.275	.254	.258	.233
	(.011)***	(.008)***	(.007)***	(.007)***	(.033)***
R^2	.112	.039	.116	.408	.059
	B. Dependent Variable: Years of College				
Ever eligible	.492		.522	.509	.499
	(.037)***		(.051)***	(.052)***	(.175)***
Preselected			.063	.074	.008
			(.029)**	(.032)**	(.124)
$\mathbf{1}(T_1 \geq \tau)$		−.016	−.024	−.040	.024
		(.027)	(.027)	(.026)	(.097)
Constant	.769	.765	.700	.706	.690
	(.026)***	(.018)***	(.016)***	(.016)***	(.075)***
R^2	.145	.043	.145	.456	.076
Observations	22,819	23,334	46,153	149,069	4,596
f specification	Linear	Linear	Linear	4th polynomial	Linear
Bandwidth	44	44	44	All	4

Note.—The sample corresponds to cohorts 2007 and 2008. Column 1 reports the estimation of eqq. (3) and (4) ($f(\cdot)$ not shown and 44 points around the cutoff) for students who applied for benefits and were classified as eligible for loans for the tax authority in the first year (preselected students). Column 2 reports the same estimations for students who did not apply for benefits or were classified in the richest income quintile in $t = 1$ (nonselected students). Columns 3–6 consider the whole population of first-time takers. The specification of the control function (f) is linear in all columns except col. 4, which uses a fourth-order polynomial. Columns 4 and 5 show the regression for each year separately. Robust standard errors are in parentheses.

* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

of his first PSU score (which is just the interaction of being preselected and the indicator for having a first test above the threshold). In these models, the estimated effect for the dummy on having a PSU score above 475 points provides a test that there is no direct effect of the score on enrollment in a second year of college. The model in column 3 uses the Imbens and Kalyanaraman optimal bandwidth as a benchmark, the model in column 4 uses a global fourth-order polynomial, and the model in column 5 uses a very small (four-point) bandwidth. The results show that the effects of ever being eligible for loans are slightly higher than in the basic specification in column 1 and highly robust.[42] Moreover, the estimated effects are comparable in magnitude to the effects of loan eligibility on the probability of ever enrolling in college or of enrolling immediately after high school, suggesting that students who entered college as a consequence of being eligible for loans are very likely to persist to a second year.

Panel B of table 7 presents a parallel set of models using as the outcome the number of years that the student is observed enrolled in college. The entry in column 1 shows that loan eligibility leads to a 0.49 increase in the number of years of college enrollment, equivalent to roughly one semester of university. Relative to the years of college enrollment among students just below the threshold (1.6 years), this represents a 64 percent relative increment in college education attainment.[43] Column 2 shows again that there is no effect for nonselected students, and columns 3–5 confirm that there is no direct effect of scoring above the 475-point threshold and also show the robustness of the estimates to changes in bandwidth. Although the 0.5-year impact of loan eligibility may appear relatively large, it is worth noting that the effect is valid only for the population of compliers with ability close to 475 PSU points (slighter lower than the average students). Presumably, the effects of loan eligibility would be smaller for higher-ability or richer students. Nevertheless, they shed light on the importance of credit access to college attainment.

E. College Progress by Income Quintile

Table 8 shows the effect of access to loans on college progress by income quintile, using the same strategy as in the previous section. Panel A shows the effects of ever enrolling in a second year, and panel B shows the number of years of college enrollment. Each column shows the effect for a different income quintile. The likelihood of enrolling in a second year increases nearly 20 percentage points for the poorest income quintile, while for quintiles 4 and 5, the increments are not statistically different from zero. In relative terms, the increment for the poorest income quintile represents an increase of 83 percent in the rate of enrollment in a second year. Panel B shows that, for the poorest income quintile, attainment increases by 0.56 year (an 88 percent increase in the base rate for students just below the cutoff), and the effects decrease until becoming insignificant for the richest quintile, as expected.[44]

[42] Also, it shows that preselected students are more likely to enroll (there is selection on who completes the FUAS form).

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

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

TABLE 8
EVER ENROLLED IN A SECOND YEAR AND YEARS OF COLLEGE BY INCOME QUINTILE

	q_1 (1)	q_2 (2)	q_3 (3)	q_4 (4)	q_5 (5)
	A. Dependent Variable: Ever Enrolled in Second Year				
Ever eligible	.194	.108	.165	.053	−.020
	(.020)***	(.038)***	(.039)***	(.040)	(.040)
Constant	.234	.349	.381	.454	.577
	(.013)***	(.028)***	(.030)***	(.031)***	(.032)***
R^2	.133	.106	.124	.074	.05
	B. Dependent Variable: Years of College				
Ever eligible	.557	.418	.520	.220	−.111
	(.048)***	(.091)***	(.092)***	(.092)**	(.091)
Constant	.630	.864	.943	1.129	1.491
	(.030)***	(.063)***	(.068)***	(.070)***	(.074)***
R^2	.169	.147	.15	.099	.056
Observations	13,089	4,069	4,048	4,204	2,849
Imbens and Kalyanaraman bandwidth	46	44	57	61	79

NOTE.—The sample corresponds to PSU first-time takers with income quintile information (cohorts 2007 and 2008 pooled together). Columns report 2SLS estimation of eqq. (3) and (4) by quintile (linear $f(\cdot)$ not shown and Imbens and Kalyanaraman optimal bandwidth for each quintile). Panel A shows estimates for ever enrolled in second year and panel B for years of college. Robust standard errors are in parentheses.
* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

To compare the results by quintile, figure 7 shows how access to loans significantly reduces the income gradient in persistence and attainment. The graphs on the right show that barely ineligible students exhibit a strong income gradient in both variables, while among those who are barely eligible for loans, the gradient is substantially reduced.

F. *Dynamics*

As discussed above, one issue in interpreting the effect of loan eligibility is that students whose initial PSU test is below 475 points—or who failed to fill out the requisite paperwork prior to the test to become eligible for loans—can retake the test in subsequent years and become eligible for loans if they score more than the cutoff. This dynamic process may influence the longer-run impacts of the loan program. Eventually, if low-scoring students retake the test enough times, they could potentially become eligible and the effects on enrollment may disappear, even if all the incentives to retake the test are driven by restrictions on access to financial markets. To assess the importance of this argument, I describe the retaking process to explain the difference between immediate enrollment

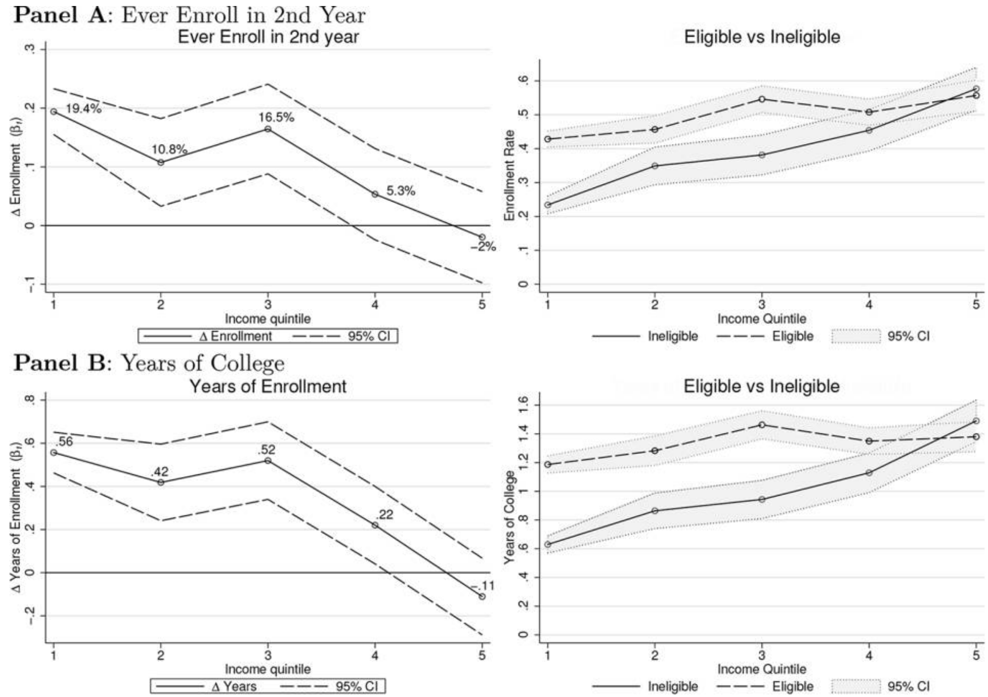


FIG. 7.—College persistence and attainment gap by family income. Panel A: Ever enrolled in second year; panel B: years of college. All figures are constructed using PSU first-time takers who applied for benefits (and, thus, with information on income quintile) for cohort 2007. In the figures on the left, each dot represents the effect of being eligible for loans on ever enrolled in 2 years of college (panel A) and years of college (panel B) by income quintile and 95 percent confidence intervals from robust standard errors (estimations of β_1 in eq. [1]), as in table 8, panels A and B. The right figures show the estimated rate of ever enrolled in second year and years of college for students at both sides of the cutoff: Ineligible corresponds to the value for barely ineligible students (estimation of β_0 in eq. [1]); eligible corresponds to the value for barely eligible for loans (estimation of $\beta_0 + \beta_1$ in eq. [1]).

and ever enrollment shown in Section IV.A. Moreover, because students cannot perfectly control their score in these new attempts, the score and the eligibility cutoff generate new natural experiments in the following years, enabling causal estimation of enrollment effects among retakers. First, I will describe how retakers differ from the overall population; second, I will show how the characteristics of retakers differ between income groups in the way that would be expected if lower-income students are mainly motivated to retake the test in order to achieve loan eligibility.

Table 9 presents estimation results from a series of local linear regression models relating the student characteristics in the row headings of the table to a constant (coefficient reported in col. 1), a dummy for having an initial score over 475 points (col. 2), a dummy if the student retakes the PSU test in later years (col. 4), and an interaction between the retake dummy and the dummy for having an initial score over 475 points (col. 6).[45] The coefficient estimates in column 4 show that among students who initially scored in an interval around the 475 cutoff, retakers have better-educated parents and are more likely to come from higher-income quintiles. Such a pattern would be expected if more advantaged families have a stronger preference for college and are willing to support their children for an extra year after high school while they do additional work to improve their scores. The estimates in column 6 show how the characteristics of retakers shift at precisely the 475-point threshold. Since students with scores above 475 are already eligible for loans, any jump here indicates the effect of removing the incentive of loan eligibility from the motives to retake the test. The results show a significant upward shift in family backgrounds at the threshold, implying that many children from poorer families retake the test only if their scores are below the loan eligibility threshold. This is consistent with the hypothesis of credit restrictions: poorer families have a strong demand for loans that are available only to students who score above the threshold. Richer students, on the other hand, have less need for loans and often retake the test to increase their chances of admission to elite programs.

I explore the variation in retaking rates across family income groups in more detail in table 10, which presents RD type models for the probability of retaking the PSU test in the first ($t = 2$), second ($t = 3$), and third years ($t = 4$) after completing high school for the whole population of students who score within the Imbens and Kalyanaraman optimal bandwidth around the threshold and for students in the five income quintile groups. The coefficients in panel A, column 1, show that roughly 29 percent of students who score less than the cutoff retake the test in $t = 2$,

[45] This estimation corresponds to the estimation of eq. (1) fully interacted with an indicator of whether the students retook the test in $t = 2$. The models are estimated on the sample within a 44-point bandwidth of the loan eligibility threshold.

TABLE 9

DIFFERENCE IN BASELINE CHARACTERISTICS FOR RETAKERS AND NONRETAKERS

	Level (1)	$\mathbf{1}(T_1 \geq \tau)$ (2)	SE (3)	Retake (4)	SE (5)	Retake × $\mathbf{1}(T_1 \geq \tau)$ (6)	SE (7)
Self-reported income	1.27	−.016	(.008)**	.018	(.011)	.062	(.016)***
Income quintile	1.81	−.012	(.017)	.055	(.024)**	.149	(.035)***
$\mathbf{1}(q_1 = 1)$	.55	−.002	(.008)	−.044	(.012)***	−.042	(.017)**
$\mathbf{1}(q_2 = 1)$	.21	.009	(.007)	.035	(.010)***	−.023	(.014)*
$\mathbf{1}(q_3 = 1)$	.14	.001	(.006)	.006	(.008)	.025	(.012)**
$\mathbf{1}(q_4 = 1)$	.11	−.007	(.005)	.002	(.007)	.041	(.011)***
Mother years of education	10.49	.012	(.06)	.599	(.080)***	.103	(.113)
Father years of education	10.49	−.064	(.065)	.512	(.089)***	.384	(.125)***
$\mathbf{1}$(female)	.57	.007	(.008)	.071	(.011)***	−.007	(.016)
High school GPA	54.94	−.005	(.143)	.691	(.182)***	.016	(.244)
Public school	.48	.015	(.008)*	−.013	(.012)	−.022	(.017)
Voucher school	.50	−.013	(.008)	.016	(.012)	.021	(.017)
Private school	.01	−.003	(.002)	−.002	(.003)	.003	(.004)
$\mathbf{1}$(married)	1.02	.003	(.003)	.000	(.004)	−.011	(.005)**
$\mathbf{1}$(work)	.03	−.001	(.003)	−.011	(.004)***	.002	(.005)
Household size	4.50	−.021	(.031)	−.021	(.041)	.003	(.057)
Both parents live	.77	.001	(.007)	.006	(.01)	.018	(.014)
Mother has formal work	.26	.006	(.007)	.051	(.011)***	−.023	(.015)
Father has formal work	.51	.001	(.008)	.013	(.012)	.026	(.017)
Mother housewife	.52	−.002	(.008)	−.028	(.012)**	.017	(.017)
Both parents work	.13	−.001	(.006)	.017	(.008)**	.003	(.012)
Mother dropout high school	.48	−.004	(.008)	−.084	(.012)***	−.013	(.016)
Father dropout high school	.50	−.001	(.008)	−.076	(.012)***	−.022	(.016)
Mother college graduate	.04	.000	(.003)	.010	(.005)**	.007	(.007)
Father college graduate	.04	−.004	(.003)	.006	(.005)	.020	(.008)***

NOTE.—Comparison in baseline characteristics for retakers vs. nonretakers. Each line shows the estimation of the following regression: $y_i = \beta_0 + \beta_1 \mathbf{1}(T_{i,t=1} \geq \tau) + \beta_2 \text{Retake}_i + \beta_3 \mathbf{1}(T_{i,t=1} \geq \tau) \times \text{Retake}_i + f(T_{i,t=1} - \tau) + \epsilon_i$, where y_i is each variable in the left-hand column, Retake_i is a dummy whether student i retook the PSU test in $t = 2$, and $T_{i,t=1}$ is her score on the test in $t = 1$. The function f is a linear control function for each side of the cutoff and for each group. The estimation is restricted to students in a window of 44 PSU points around the cutoff (Imbens and Kalyanaraman optimal bandwidth). Robust standard errors are in parentheses.

* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

TABLE 10
RETAKING PSU

	All (1)	q_1 (2)	q_2 (3)	q_3 (4)	q_4 (5)	q_5 (6)
	A. Retake in $t = 2$					
$\mathbf{1}(T \geq \tau)$	−.067	−.075	−.091	−.039	−.008	−.012
	(.006)***	(.008)***	(.014)***	(.015)***	(.016)	(.017)
Constant	.286	.267	.320	.298	.293	.323
	(.005)***	(.006)***	(.010)***	(.011)***	(.012)***	(.012)***
	B. Retake in $t = 3$					
$\mathbf{1}(T \geq \tau)$	−.011	−.019	−.009	.004	−.016	−.007
	(.003)***	(.005)***	(.007)	(.008)	(.008)*	(.008)
Constant	.067	.068	.067	.064	.067	.058
	(.003)***	(.003)***	(.005)***	(.006)***	(.007)***	(.006)***
	C. Retake in $t = 4$					
$\mathbf{1}(T \geq \tau)$	−.005	−.005	−.005	−.008	−.002	−.001
	(.003)*	(.004)	(.006)	(.006)	(.006)	(.006)
Constant	.037	.037	.038	.042	.030	.029
	(.002)***	(.003)***	(.004)***	(.005)***	(.005)***	(.005)***
	D. Ever Retake in $t = 2, 3, 4$					
$\mathbf{1}(T \geq \tau)$	−.068	−.083	−.083	−.037	−.013	−.019
	(.007)***	(.009)***	(.014)***	(.016)**	(.017)	(.017)
Constant	.344	.328	.372	.355	.341	.369
	(.005)***	(.006)***	(.011)***	(.012)***	(.013)***	(.013)***
Observations	78,072	42,120	17,007	14,447	12,550	12,225
Imbens and Kalyanaraman bandwidth	44	46	44	57	61	79

NOTE.—The table shows the estimation of eq. (1), where the dependent variable is indicated in the title of each panel (relative to their score in $t = 1$). For example, panel A shows, relative to the score in $t = 1$, the share of students who retake the PSU in $t = 2$ (retake in $t = 2$), i.e., 1 year after they graduated from high school and participated for the first time in the PSU process ($t = 1$). In panel D, the dependent variable, ever retake in $t = 2, 3, 4$, is an indicator that takes the value of one if a student retook the PSU in either $t = 2, 3$, or 4 and is equal to zero otherwise. Column 1 shows the estimation for the whole sample (the three cohorts pooled together), and the following columns by income quintile. All estimations are local linear regressions in which f is linear within the optimal Imbens and Kalyanaraman bandwidth. Robust standard errors are in parentheses.
* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

with a drop of 7 percentage points precisely at the 475-point threshold. Given that all the other reasons for delaying college should be balanced across the cutoff, 7 percentage points can be interpreted as the effect of loan eligibility among students who can afford to retake the test.

Columns 2–6 in panel A show that the effects are stronger for poorer students: the likelihood of retaking the test is 8–9 percentage points larger for students who were barely ineligible in $t = 1$ for the two poorest quintiles while not significantly different from zero for the two richest. Comparing the poorest and the two richest income quintiles (cols. 2, 5, and 6, respectively), we observe that students in these richest quintiles are about 30 percent more likely to retake the test regardless of their position across the threshold. For students in the poorest quintile, their likelihood of retaking is significantly lower (27 percent) if they did not become eligible in the first attempt and even lower (19 percent) if they did.

Panel B examines retaking in the second year after high school graduation. It shows that 7 percent of the students with scores just below the 475-point threshold retake the test, with a small but significant drop (1.1 percentage points) at the threshold. Panel C shows that the probability of retaking the test in $t = 4$ is similar across the cutoff for all the groups, with a retaking rate of about 4 percent. Finally, panel D summarizes the whole process and shows the probability of ever retaking the test (following the students for four admission rounds). It shows that the differences across the cutoff are mainly driven by students who retake the PSU at their first opportunity (year $t = 2$, shown in panel A), and therefore, the results in this paper are unlikely to be affected by students who retake the test in years for which there are no available data.

Figure 8 shows the retaking process in graphical form. It confirms that students who are barely ineligible in the first attempt are more likely to retake the test in the following PSU process; but in later years, the rate of retaking the test is much smaller and almost balanced across the cutoff.

Finally, I analyze the enrollment effects generated in subsequent PSU years. Because students cannot completely control their score in subsequent PSU attempts, retakers are as good as randomly assigned across the cutoff in those attempts, enabling an RD analysis.[46] Table 11 shows the effect of loan eligibility on college enrollment in $t = 2$ and $t = 3$ (regardless of whether a student was enrolled in previous years) relative to the scores obtained in those years. Given data limitations, enrollment in $t = 2$ can be analyzed only for the 2007 and 2008 cohorts (panel A), and enrollment in $t = 3$ can be analyzed only for the 2007 cohort (panel B).[47]

Panel A of table 11 shows the enrollment effects among retakers in $t = 2$. The conclusions are similar to those found in previous sections: becoming eligible for loans in $t = 2$ implies a sizable increase in the probability

[46] Appendix B confirms that students are as good as randomly assigned to loan eligibility across the cutoff in these attempts. It shows that baseline characteristics are balanced among those who retake the test and are preselected in $t = 2$ and $t = 3$.

[47] As before, the RD regressions use a linear f, within the Imbens and Kalyanaraman optimal bandwidth.

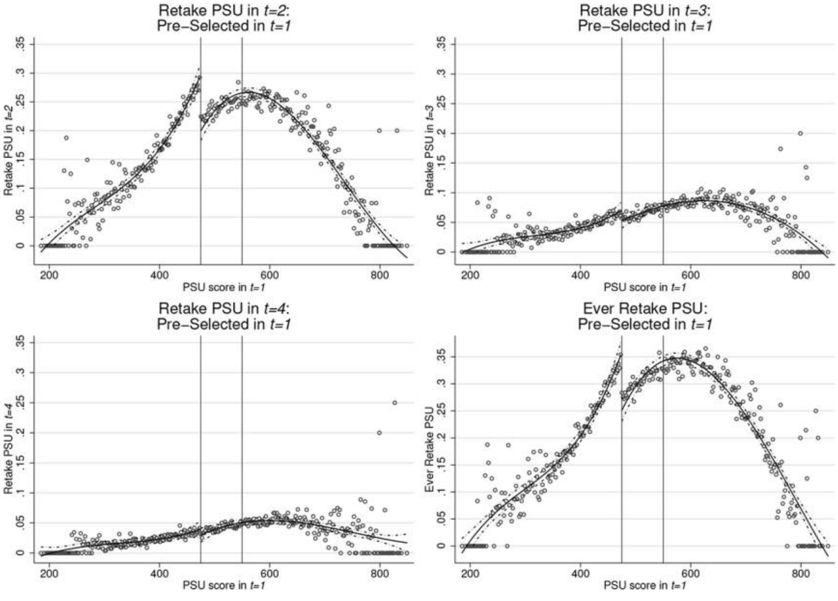


FIG. 8.—Retaking the PSU test. These figures show the share of students who retake the PSU test in the years after the first PSU attempt ($t = 2$, 3, and 4, respectively) relative to the first PSU score ($t = 1$), among students who were classified as preselected in $t = 1$ (classified in one of the four poorest income quintiles after applying for benefits [completed the FUAS form]). The bottom-right graph shows the share of students who ever retake the PSU in the 4 years. Each dot represents the average rate among students in bins of 2 PSU points. Solid lines represent fitted values from the estimation of equation (1) and a fourth-order polynomial to control for $T_i - \tau$ for each side (the function $f(\cdot)$ in eq. [1]). The dashed lines represent 95 percent confidence intervals from the same estimation. The vertical lines at 475 and 550 show the loan eligibility cutoff and the Bicentenario cutoff, respectively.

of enrollment in that year, and there is no behavioral effect for nonselected students. Column 1 shows the effect restricted to students who were preselected for loans in both $t = 1$ and $t = 2$ (those for whom scoring at least the cutoff in $t = 2$ implies a change in loan eligibility). Column 2 expands the analysis to any student preselected in the second year (including students who did not apply for benefits in $t = 1$, the great majority, or students who were classified in the richest quintile in $t = 1$ and now are reclassified in a lower quintile). Column 3 shows the placebo test among nonselected students in $t = 2$ (those who either did not apply for benefits in $t = 2$ or were classified in the richest quintile), showing no effect for crossing the cutoff.

Columns 4–6 further restrict the sample to potential delayers, that is, students who did not enroll in $t = 1$, in order to test different enrollment strategies that may confound the effects: first, if unconstrained students

TABLE 11

ENROLLMENT IN $t = 2, 3$

	A. Enrollment in $t=2$ among Retakers in $t=2$					
	Any Enrollment in $t = 2$			Enrollment in First Year in $t = 2$		
	Preselected in $t = 1$ and $t = 2$ (1)	Preselected in $t = 2$ (2)	Nonselected in $t = 2$ (3)	Preselected in $t = 1$ and $t = 2$ (4)	Preselected in $t = 2$ (5)	Nonselected in $t = 2$ (6)
$\mathbf{1}(T_2 \geq \tau)$	.274	.230	−.0007	.282	.233	−.001
	(.017)***	(.015)***	(.021)	(.018)***	(.016)***	(.022)
Constant	.339	.357	.451	.328	.353	.423
	(.013)***	(.011)***	(.015)***	(.014)***	(.012)***	(.016)***
Observations	12,475	17,187	8,773	10,340	13,689	7,464
	B. Enrollment in $t=3$ among Retakers in $t=3$					
	Any Enrollment in $t = 3$			Enrollment in First Year in $t = 3$		
	Preselected in $t = 1$ and $t = 3$ (1)	Preselected in $t = 3$ (2)	Nonselected in $t = 3$ (3)	Preselected in $t = 1$ and $t = 3$ (4)	Preselected in $t = 3$ (5)	Nonselected in $t = 3$ (6)
$\mathbf{1}(T_3 \geq \tau)$	.282	.224	.138	.204	.163	.059
	(.049)***	(.037)***	(.055)**	(.055)***	(.042)***	(.052)
Constant	.311	.331	.299	.273	.302	.179
	(.037)***	(.029)***	(.041)***	(.039)***	(.030)***	(.036)***
Observations	1,616	2,620	1,220	1,096	1,986	994

NOTE.—Panel A (panel B) shows the estimation of eq. (1) for college enrollment in $t = 2$ ($t = 3$) among students who retake the PSU in $t = 2$ ($t = 3$). The running variable is the PSU score in $t = 2$ ($t = 3$). Columns 1–3 pertain to any enrollment (regardless if the student enrolled in $t = 1$), and cols. 4–6 restrict the sample to students who did not enroll in $t = 1$ (delayers). Columns 1 and 4 are for students preselected in both periods. Columns 2 and 5 are for students preselected only in the period ($t = 2$ and $t = 3$ in panels A and B, respectively); and cols. 3 and 6 are for nonselected students. All estimations are local linear regressions in which f is linear within the optimal Imbens and Kalyanaraman bandwidth. Robust standard errors are in parentheses.

* $p \leq 10$ percent.

** $p \leq 5$ percent.

*** $p \leq 1$ percent.

are delaying 1 year to benefit from the terms of the loans, students at both sides of the cutoff should enroll at a similar rate in $t = 2$, assuming that they realize that delaying is no longer profitable. Second, this sample restriction allows us to test if working and saving for a year is a plausible strategy; that is, not enrolled students might work and save for a year so that those assigned to the group without access to loans in $t = 2$ could still enroll using their savings.

The results show little support for the mechanisms suggested by these alternative strategies. Becoming eligible for loans in $t = 2$ implies a significant increase in enrollment, with proportional increases of between 66 percent and 86 percent. Column 4 shows the results when the sample is restricted to students who were preselected in both periods, column 5 expands the analysis to any student categorized as preselected in $t = 2$, and column 6 shows the effect among nonselected students.

Panel B repeats the analysis for students who retake the PSU test 2 years after high school graduation ($t = 3$), finding essentially the same results.[48] Column 1 restricts the sample to those students who were preselected in $t = 1$ and $t = 3$; column 2 analyzes all preselected students in $t = 3$; column 3 analyzes nonselected students;[49] and, as in panel A, columns 4–6 restrict the sample to students who do not enroll in either of the first 2 years after the first PSU attempt.

Figure 9 replicates graphically the results presented in table 11. The figures on the left replicate panel A, and the figures on the right replicate panel B. The figures show exactly the same patterns as immediate enrollment and ever enrolled, despite the fact that the sample size is dramatically smaller. The conclusion, once again, is that access to credit implies sizable and significant increases in college enrollment and that the evidence is robust to bandwidth choice and specification of the control function.

Table 12 shows similar enrollment models, fit to students in different income quintiles. Panel A (panel B) shows enrollment in $t = 2$ ($t = 3$), restricted to students who were preselected in both $t = 1$ and $t = 2$ ($t = 3$). Both panels confirm previous findings. First, access to loans significantly increases enrollment for all eligible quintiles: again, the relative change is over 100 percent for the poorest income quintile. Second, in the absence of college loans, there is a significant income gradient in enrollment. Third, access to loans significantly reduces the enrollment gap by family income: conditional on falling within a narrow window of the PSU thresh-

[48] Sample sizes are smaller because only 7 percent of the students retake the test in $t = 3$, and because of enrollment data restrictions, only the 2007 cohort can be followed up to $t = 3$.

[49] Column 3 reports the test for nonselected students, showing an inconsistent effect. In the online appendix, I show evidence that this result is a consequence of small sample size; e.g., there are only 13 students (on each side) within two points of the cutoff.

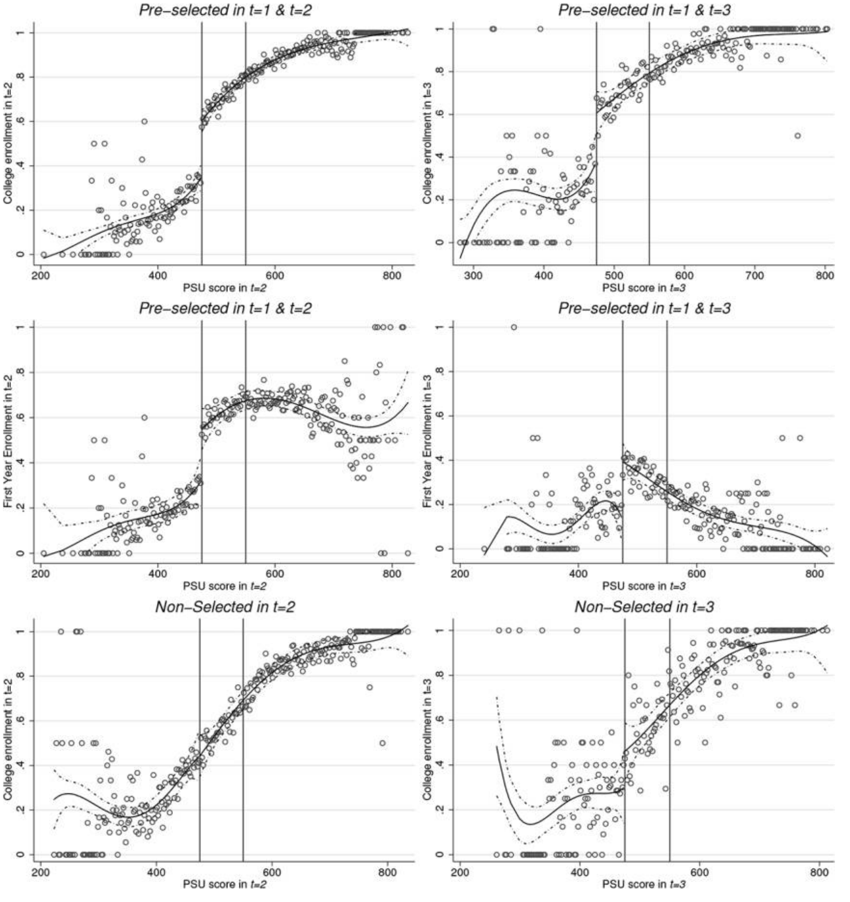


FIG. 9.—Dynamics: enrollment in $t = 2$ and $t = 3$. The dots represent average enrollment for students within 2 PSU point bins. The solid lines represent fitted values from equation (1) ($f(\cdot)$ is a fourth-order polynomial), and the dashed lines represent 95 percent confidence intervals. The top-left (top-right) graph shows any enrollment in $t = 2$ ($t = 3$), that is, first-time or continuing enrollment after $t = 1$, for preselected students in $t = 1$ and in $t = 2$ ($t = 3$). The middle-left and middle-right graphs show first-year enrollment in $t = 2$ and $t = 3$, respectively, for preselected students in both years. The bottom graphs show any enrollment in $t = 2$ (left) and $t = 3$ (right), respectively, for nonselected students. The vertical lines at 475 and 550 show the loan eligibility cutoff and the Bicentenario cutoff, respectively.

old, enrollment is statistically the same for all eligible income quintiles. Fourth, there is no change in enrollment for the ineligible fifth income quintile, ruling out other behavioral effects.[50]

[50] The sample is restricted to students who were preselected in $t = 1$ and also in the second period considered ($t = 2$ in panel A or $t = 3$ in panel B). Therefore, those who appear classified in the fifth quintile in the second period were reclassified in this quintile after being considered preselected in $t = 1$. This explains in part their lower overall enrollment rate in both periods.

TABLE 12
Enrollment in $t = 2, 3$ by Income Quintile

	q_1 (1)	q_2 (2)	q_3 (3)	q_4 (4)	q_5 (5)
	A. Any Enrollment in $t = 2$ by Quintile (Preselected in $t = 2$)				
$\mathbf{1}(T_2 \geq \tau)$	.306	.275	.197	.137	.002
	(.025)***	(.037)***	(.051)***	(.070)*	(.060)
Constant	.295	.342	.410	.537	.413
	(.018)***	(.028)***	(.040)***	(.057)***	(.043)***
Observations	5,259	2,593	1,497	888	1,081
	B. Any Enrollment in $t = 3$ by Quintile (Preselected in $t = 3$)				
$\mathbf{1}(T_3 \geq \tau)$	.290	.124	.327	.246	.038
	(.066)***	(.101)	(.141)**	(.188)	(.217)
Constant	.288	.366	.332	.417	.479
	(.050)***	(.073)***	(.117)***	(.158)***	(.176)***
Observations	837	368	208	109	107

Note.—Panel A (panel B) shows the estimation of eq. (1) for college (any) enrollment in $t = 2$ ($t = 3$) by income quintile, among students who retake the PSU in $t = 2$ ($t = 3$). The sample is restricted to students who were preselected in $t = 1$ and also in $t = s$, $s = 2$ or 3. The running variable is the PSU score in $t = 2$ ($t = 3$). All estimations are local linear regressions in which f is linear within the optimal Imbens and Kalyanaraman bandwidth for that given year. Robust standard errors are in parentheses.
* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

V. Mechanisms

One question raised by my analysis is through which mechanisms the loan programs in Chile have an effect on college enrollment. These loans may constitute a partial elimination of binding credit constraints (an "access effect") or, alternatively, the response may be attributed to a combination of income and substitution effects arising because the loans are partially subsidized by lower interest rates (at least in the case of the TUL program) and/or by the actual or perceived differences in the probability of having to pay the loans (a "price effect").

In the online appendix, I briefly explore these mechanisms, presenting two tests. The first test shows enrollment in private universities by income quintile. Given that conventional loans require minimum levels of parental income, they are not available for any student from the two poorest income quintiles, but they do not exclude students from the fourth (see Sec. II.A), suggesting that this latter group is not credit constrained. If SGL does not contain an embedded subsidy, students from the fourth income quintile should not change their enrollment in private universities after crossing the loan cutoff, because SGL is the only option on those institutions. The results in the online appendix show that enrollment of students from the fourth income quintile is not affected across the cutoff, suggesting that SGL is perceived as equivalent to loans in the conventional market, and therefore, the subsidy effect is negligible for this loan. On the other hand, the estimated effects for the first two quintiles are, in relative terms, similar to the main results in table 3.

Second, the online appendix presents evidence that college enrollment in Chile is not strongly affected by the direct costs of attendance using the variation induced by the Bicentenario scholarship program—a scholarship that covers nearly all tuition costs in traditional universities for students from the two poorest quintiles who score above 550 points on the PSU test. The analysis in figure 6 shows that the take-up rate for the two loan programs by eligible students drops sharply at the 550-point threshold, from around 60 percent for students with scores just below 550 to around 30 percent for students with scores just above 550, consistent with the idea that many of these students would prefer not to borrow if they had lower tuition costs. Despite the sharp reduction in tuition costs for students who score above 550, however, there is no evidence of any increase in college attendance rates. This is illustrated by examining the patterns in figures 1–4 around the second vertical line in each figure, which is placed at the 550-point threshold. In no case is there any indication of a shift in enrollment rates at the threshold for scholarship eligibility. To the extent that the same conclusion applies to students with scores around the 475-point threshold, one could interpret the effects of loan eligibility as mainly reflecting credit access rather than a behavioral reaction to the cost of college induced by the nature of the loan programs.

On the basis of this evidence, I conclude that college enrollment in Chile is not strongly influenced by variation in the present value of the costs of college, suggesting that the main mechanism underlying the enrollment effect is access to credit.

VI. External Validity

A final issue raised by my analysis is to what extent the findings of strong responses to credit access in Chile translate to other settings. A first observation is that unlike many other countries, Chile has no free or essentially free public universities. Even public universities in Chile set relatively high tuition rates. This contrasts with many European countries and many middle-income countries such as Mexico and Brazil, where there are at least some public institutions that have very low tuition costs. Even in the United States, where average tuition "list prices" are around \$15,000–\$20,000 per year, there are low-cost alternatives such as community colleges. A second and related observation is that universities in Chile do not "price-discriminate" among students. In contrast, many private and public universities in the United States have programs that offer substantial discounts to lower-income families. These two factors suggest that loan access may be particularly important in Chile. To place the tuition costs in context, imagine a family with income equal to the national median income in 2006. The costs of 1 year of tuition in that year ranged from 1.7 to 2.7 million CLP from low-cost public to high-cost public universities, equal to 46–76 percent of their annual income.[51] This would impose a very large barrier to most families, in the absence of a readily available loan.

While the institutional setting in Chile arguably leads to a relatively large impact of the two loan programs analyzed here (and certainly makes it much easier to measure these impacts), it is important to note that the lessons from Chile may apply to large subsets of the population in other countries. For example, most of the free public universities in Brazil are highly selective and admit only a small percentage of students. Many other students attend relatively high-cost private universities. A similar situation prevails in Mexico, Philippines, Indonesia, and many African countries.[52] Even in the United States, a number of authors have argued that the relative generosity of the complex system of grants, loans, and tuition discounts has decreased over time, making credit constraints more salient in the present (e.g., Brown et al. 2012; Lochner and Monge-Naranjo 2012).

The consequences of institutional differences in aid programs in different countries are potentially revealed by differences in the income gradient in college attendance rates across countries. Figure 10 plots college enrollment rates for students in the top and bottom family income quintiles in six countries. Despite the different funding systems, all countries show an enrollment gap. On one extreme, the enrollment ratio between students from the poorest and the richest quintiles is lowest in Sweden at a value of 1.5, arguably because college is free and students receive grants and loans to cover overhead costs.[53] On the other extreme, the enrollment ratio ranges from 3 to 10 in developing countries such as Brazil and Chile, all of which have relatively expensive tuition and relatively restrictive aid policies. These differences could be a consequence of differential access to credit, aid, or public financing of higher education. In addition, enrollment levels for the poorest quintile are much lower in developing countries, at only 5–10 percent in Latin American countries compared to 35–40 percent in developed countries. Particularly in Chile, scholarships and loans fund only a portion of the direct cost (on average,

[51] Tuition figures come from the International Comparative Higher Education and Finance Project from State University of New York at Buffalo. Income figures come from the household survey CASEN 2006. Median family income is equal to CLP 3.7 million per year.

[52] For instance, in the Philippines, private higher education accounts for about 75 percent of enrollment and relies mostly on tuition fees for funding (UNESCO 2012). See App. C for more information.

[53] Given the generosity of college funding policies, the gap in Sweden is arguably not related to access to credit at the age of entering college.

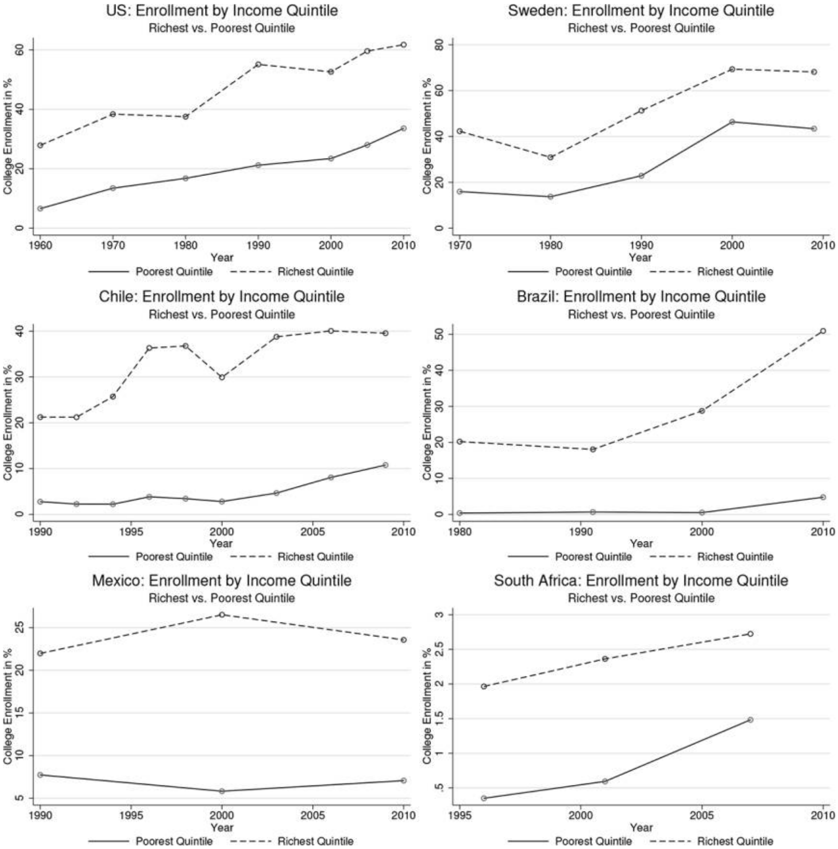


FIG. 10.—Enrollment gap by family income in different countries. All figures show the college enrollment rate for students from the richest and poorest income quintiles, restricted to individuals between 18 and 24 years old who live with their parents. Income quintiles were calculated using family income. All figures, except for Sweden, were made using census data from the Integrated Public Use Microdata Series (IPUMS). Per capita income is given in local currency. For Sweden, family income comes from tax records. College enrollment and family structure from administrative records are from Statistic Sweden, in all cases for the whole population.

up to 90 percent of tuition) and cover at most 60 percent of students from the four poorest income quintiles who choose to take the admission test.[54]

The small number of countries analyzed in figure 10 provides merely suggestive evidence on the importance of credit constraints at the international level. Nevertheless, using different indicators, we can observe consistent patterns for other countries: in countries with low education

[54] The loan eligibility cutoff, 475, corresponds to the 41st percentile of the score distribution.

expenditure on higher education, universities finance their operations through tuition directly paid by families, restricting the number of students from low-income quintiles. That shortage of college-educated individuals implies larger returns to higher education. In this scenario, Chile is not an outlier: it has high relative tuition and low government spending, presenting a situation that is similar to or better than the one in other countries in Latin America, Africa, and Southeast Asia (see App. C).[55]

VII. Conclusions

In this paper, I exploit the sharp eligibility rules for two loan programs in Chile to study the effects of access to credit on college enrollment and persistence.

The results show that access to these loan programs leads to a large increase in the fraction of students who enroll in college in the year after completing high school, effectively doubling the short-term enrollment rate in the absence of loan availability. Importantly, I find that access to loans also leads to large increases in enrollment in the second year of college and to gains in the total number of years of enrollment over the 3 years following high school graduation. An analysis of the "compliers" who are induced to enroll in college immediately after high school as a result of being eligible for the loan programs shows that these students are from relatively lower-income families but have relatively strong high school grades. This relatively strong academic background helps to explain the success of the compliers in completing their first years of college.

Most strikingly, I find that access to loans appears to nearly eliminate the relatively large income gradient in college enrollment and progress for students who score around the cutoff for loan eligibility. Among those with scores just below the eligibility threshold, students from the top income quintile are twice as likely to immediately enroll in college as students from the bottom quintile. In contrast, among students who score just above the threshold and are eligible for loans, there is no significant difference in enrollment rates between the top and bottom quintile groups.

The same is true for the effect of the loan programs on years of college enrollment. Among students who are just below the loan eligibility threshold, students from the richest income quintile have, on average, 2.4 times more years in college than students from the poorest quintile. In contrast, among students who are just at or above the eligibility cutoff, students from the poorest income quintile have (statistically) the same total years of enrollment as the richest 20 percent of the population.

[55] These points are reinforced by a few papers that present causal evidence of the effects of credit constraints on enrollment in developing countries. See, e.g., Gurgand et al. (2011) for South Africa and Rau et al. (2013), which uses similar data for Chile.

I show evidence that these effects are not driven by behavioral responses from students or university officials or by rules that restrict the application of nonselected students. Moreover, I briefly explore the mechanisms underlying the effects and conclude that the effects are driven by access to loans rather than implicit subsidies.

Comparing these results with the previous literature is difficult, mainly because the literature has focused on developed countries. These studies analyze the effects of marginal changes in existing aid or loan programs, most of which are already generous on both the extensive margins (student coverage) and intensive margins (the amounts of money involved); thus, they are less likely to provide evidence of the role of credit constraints. By contrast, the programs analyzed in this paper constitute a shift of regime from one with no access to funding to one that offers tuition loans. Although the comparison is complex, the findings help shed light on what can happen with college enrollment when aid programs fall behind increases in tuition cost, as seen in the United States in recent years (see, e.g., Belley and Lochner 2007). The evidence presented here is of special interest in middle- to low-income economies in which there are fewer programs to alleviate financial constraints and tuition costs are high (relative to per capita income). The elimination of this type of imperfection in financial markets may have substantial consequences on access to college education and the development of highly skilled workers, with the obvious implications for growth and economic development.

Appendix A

Background Information

It is important to know what type of student participates in the admission process in order to understand who is affected by the loans analyzed in this paper. The alternative hypothesis of credit constraints points to long-run factors to explain the difference in educational attainment between rich and poor students. If students from poor backgrounds do not participate in the admission process, then the effects presented here may not affect the overall enrollment gap by family income. In this appendix I will verify whether the sample of test takers also includes individuals from the poorest background.

I merged information from the universe of students who graduated from eighth grade in 2004 and followed them through high school, until they take the PSU test in subsequent years. According to official records, 98.7 percent of the population finish eighth grade,[56] and therefore, this tracking allows the analysis of the income composition of test takers relative to the whole population. One drawback is that the income information is self-reported in a 2004 census test (SIMCE 2004) that may include a high level of noise.

[56] According to the household survey CASEN 2009.

The data used here are a combination of three data sets. The first one is the data on academic performance from the Ministry of Education for the years 2008–11, including GPA, graduation status, attendance, school identifiers, and so forth, for the universe of students in all levels of primary and secondary education in Chile. These data make it possible to follow each student who graduated from eighth grade in 2004 up to high school graduation. The second data set corresponds to the SIMCE test taken by eighth graders in 2004. SIMCE is a census test performed every year at different levels (fourth, eighth, and tenth grades).[57] In parallel, this census collects information about family income that I will use to classify students in income quintiles.[58] The third data set corresponds to the PSU data between 2008 and 2012. Eighth-grade students in 2004 should be part of the admission process of 2009 if they do not repeat any year and decide to take the test immediately after graduation.

From the population of students who graduated from eighth grade in 2004, slightly over 80 percent graduated from high school in the years considered, and just over 80 percent of those who graduated took the college admission test. Given that academic performance data are available only up to 2011, students who graduate after that year are considered as dropouts; thus, the share of high school graduates and the share who take the PSU are lower-bound estimates of the true participation rates.

Figure A1 describes the situation by family income self-reported categories in the eighth-grade SIMCE 2004. The top-left graph shows that about one-third of the students in the poorest income quintile do not graduate from high school. Nevertheless, the share of students in this income quintile that write the PSU is still high at 40 percent. For the second-poorest income quintile, the situation is better. Less than 25 percent drop out from high school and 60 percent take the PSU.

For those who took the PSU in 2009 (the last year with FUAS information available), the top-right graph shows the share of students in each income quintile in 2004 who completed the FUAS form. This figure represents a lower bound of the true participation because, to be included, students need to graduate from high school without repeating grades and take the PSU immediately after graduation. Nevertheless, 60 percent of the poorest income quintile (among those who take the PSU) applied for benefits. This implies that at least 36 percent of the students from the poorest income quintile are considered in the results of the paper.

Finally, to confirm that the eligibility cutoff is not affecting a strange part of the population, the bottom graph in figure A1 shows the distribution of scores by official income quintile. The figure shows that the eligibility cutoff is slightly below the average PSU score of the poorest income quintile (479); the average by income quintile increases monotonically about 25 PSU points; thus the cutoff captures a large part of the population in each quintile and allows us to identify effects for individuals similar to the average student.

Again, this evidence reinforces the idea that Chile is a good place to investigate the effect of credit access on college enrollment.

[57] The SIMCE test (up to 2006) was applied to only one level per year; the SIMCE in 2004 is the only time that coincides with the sample of PSU takers between 2007 and 2009.

[58] SIMCE is written by about 92 percent of the population.

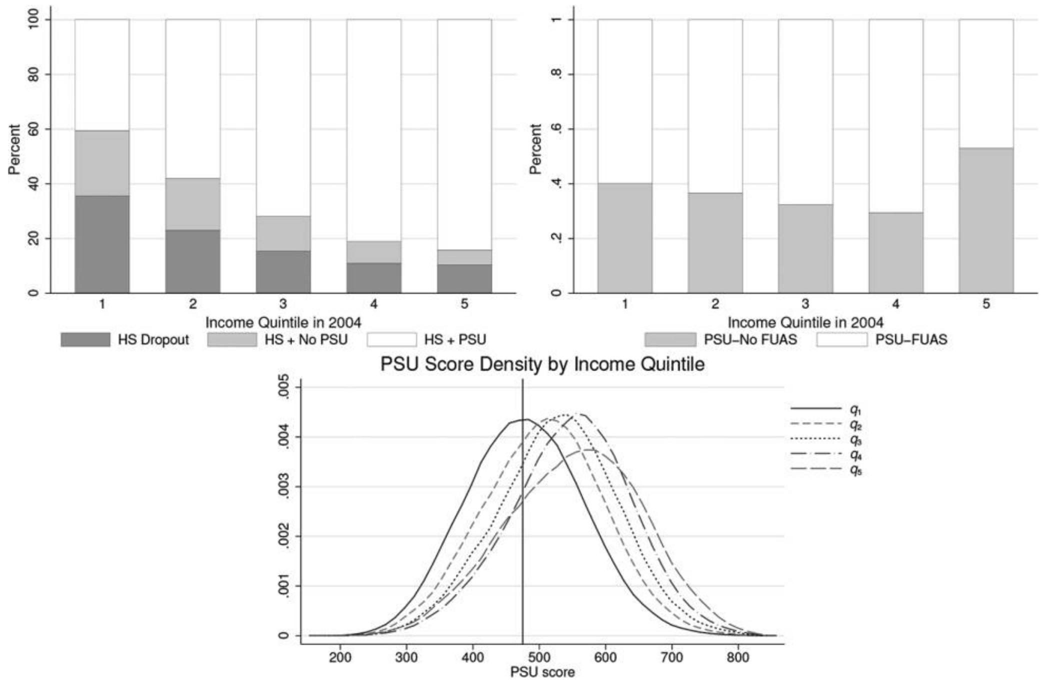


FIG. A1.—Who participates in the admission process. The upper-left graph decomposes the population of eighth-grade graduates in 2004 into high school dropouts (HS Dropout), graduates from high school who did not take the PSU (HS + No PSU), and graduates from high school who take the PSU (HS + PSU) by income quintile. The upper-right graph shows the shares of students who graduated from eighth grade in 2004 who applied for benefits from the Ministry of Education in 2009 (completed the FUAS form), conditional on taking the PSU test. For the upper graphs, income quintiles are constructed using self-reported information in the eighth-grade SIMCE test of 2004. The bottom graph shows PSU score densities by official income quintile (vertical line indicates the eligibility cutoff, q_1 being the poorest income quintile). Color version available as an online enhancement.

Appendix B

Validity of RD

This appendix shows additional tests for the internal validity of the different RD designs used in the paper.

The results of the enrollment gap by family income rely on RD regressions in which the sample is split by the income quintile classification. In order to interpret those effects as causal, table B1 shows balance of covariates by income quintiles. This table shows that, in general, students are not sorting to one side of the cutoff.

The most problematic subgroup is the third income quintile, which shows imbalance in four variables (three variables at the 5 percent significance level and one at the 10 percent). Three of these variables are collinear since they represent the type of high schools from which students graduated. Thus, the imbalance of one variable should imply the imbalance of the others. For this quintile, there are more students graduating from public high schools above the cutoff. Since these students tend to come from families with lower socioeconomic backgrounds, they may have lower preferences for college, and therefore, any potential bias is likely of the opposite sign. For all the other groups, observable baseline characteristics appear to be balanced, except for a few exceptions in line with the error of type I.[59]

Finally, table B2 shows balance of covariates for students who retake the test in the following years ($t = 2$ and 3). Students who retake the test are not able to manipulate their score with precision to be above the cutoff and are again (as good as) randomly allocated across the cutoff.

[59] Of the 100 *t*-tests reported in these tables, nine, five, and zero reject the null of no difference across the threshold at the significance levels of 10 percent, 5 percent, and 1 percent, respectively, below the hypothetical levels of 10, five, and one.

TABLE B1

BALANCE OF COVARIATES: PRESELECTED STUDENTS BY INCOME QUINTILE

Variable	Quintile 1 Level (1)	Quintile 1 Diff. (2)	Quintile 1 Abs(t) (3)	Quintile 2 Level (4)	Quintile 2 Diff. (5)	Quintile 2 Abs(t) (6)	Quintile 3 Level (7)	Quintile 3 Diff. (8)	Quintile 3 Abs(t) (9)	Quintile 4 Level (10)	Quintile 4 Diff. (11)	Quintile 4 Abs(t) (12)
Self-reported income	1.12	−.006	(.93)	1.29	−.004	(.3)	1.54	−.015	(.72)	1.71	−.003	(.11)
Mother years of education	9.9	−.102	(1.46)	10.9	.100	(1)	11.7	.128	(1.08)	12.7	−.131	(.95)
Father years of education	9.8	−.076	(.97)	10.8	.022	(.2)	11.7	.205	(1.58)	12.8	−.185	(1.29)
1(female)	.60	.015	(1.53)	.61	−.037	(2.41)**	.58	.019	(1.03)	.58	−.017	(.81)
High school GPA	55.6	−.120	(.73)	54.9	.060	(.24)	54.4	.245	(.77)	54.3	−.133	(.4)
Public school	.55	.005	(.52)	.45	.015	(.96)	.35	.047	(2.56)**	.30	.005	(.26)
Voucher school	.44	−.005	(.52)	.53	−.011	(.73)	.62	−.037	(1.97)**	.66	−.010	(.48)
Private school	.01	.000	(.13)	.01	−.003	(1.1)	.02	−.010	(1.94)*	.03	−.001	(.12)
1(married)	1.02	−.002	(.7)	1.02	−.001	(.24)	1.02	.009	(1.33)	1.01	.003	(.54)
1(work)	.03	−.003	(1.11)	.02	.010	(1.98)**	.04	−.007	(1.13)	.02	.008	(1.21)
Household size	4.59	.002	(.06)	4.45	−.027	(.49)	4.33	.012	(.19)	4.33	−.132	(1.95)*
Both parents live	.76	.003	(.37)	.77	.001	(.07)	.80	.008	(.52)	.80	.012	(.74)
Mother has formal work	.22	−.012	(1.45)	.31	−.013	(.89)	.35	.037	(2.01)**	.42	.006	(.29)
Father has formal work	.44	−.003	(.29)	.55	.023	(1.48)	.63	.008	(.44)	.67	−.004	(.21)
Mother housewife	.55	.009	(.89)	.48	.016	(1.05)	.47	−.016	(.87)	.45	−.027	(1.31)
Both parents work	.09	−.009	(1.57)	.15	−.001	(.09)	.21	.015	(.92)	.27	.005	(.26)
Mother dropout high school	.55	.016	(1.68)*	.42	−.015	(.99)	.34	−.022	(1.25)	.25	−.017	(.9)
Father dropout high school	.57	.016	(1.66)*	.46	−.022	(1.43)	.35	−.022	(1.21)	.26	.005	(.26)
Mother college graduate	.02	.002	(.76)	.03	.004	(.77)	.07	−.005	(.51)	.14	−.007	(.49)
Father college graduate	.02	.003	(1.08)	.04	.001	(.16)	.08	−.011	(1.06)	.14	−.011	(.74)
Observations		40,317			16,876			11,244			9,107	

NOTE.—Diff. refers to differences at the cutoff for each variable in the left-hand column: β_1 in eq. (1). Level corresponds to β_0 in eq. (1). All regressions use a linear control function for each side of the cutoff and are restricted to students in a window of 44 points around the cutoff. All variables except income quintile are from PSU data set. Income quintile is from the FUAS data set. For variable definitions, see the note in table 6. Absolute t-values are in parentheses.

* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

TABLE B2
Balance of Baseline Characteristics for Retakers in $t = 2$ and $t = 3$

	Level (1)	$\mathbf{1}(T_2 \geq \tau)$ (2)	SE (3)	Level (4)	$\mathbf{1}(T_3 \geq \tau)$ (5)	SE (6)
Self-reported income	1.29	−.001	(.014)	1.23	.059	(.029)**
Income quintile	1.83	−.002	(.03)	1.73	.052	(.063)
$\mathbf{1}(q_1 = 1)$	.50	.019	(.015)	.55	.001	(.032)
$\mathbf{1}(q_2 = 1)$	.26	−.030	(.013)**	.24	−.026	(.027)
$\mathbf{1}(q_3 = 1)$	.15	.006	(.011)	.14	−.002	(.022)
$\mathbf{1}(q_4 = 1)$	.09	.006	(.009)	.07	.028	(.018)
Mother years of education	10.93	−.009	(.101)	10.71	.176	(.222)
Father years of education	10.87	.034	(.111)	10.56	.371	(.243)
$\mathbf{1}$(female)	.65	.007	(.014)	.62	−.010	(.031)
High school GPA	54.02	−.272	(.263)	53.39	−.488	(.635)
Public school	.51	−.022	(.015)	.54	−.023	(.032)
Voucher school	.48	.022	(.015)	.44	.023	(.032)
Private school	.01	−.001	(.003)	.01	.003	(.006)
$\mathbf{1}$(married)	1.01	.007	(.004)	1.01	.009	(.009)
$\mathbf{1}$(work)	.02	.004	(.004)	.02	.000	(.01)
Household size	4.49	−.027	(.053)	4.56	.045	(.117)
Both parents live	.77	.014	(.012)	.83	−.064	(.027)**
Mother has formal work	.31	−.021	(.014)	.29	.006	(.029)
Father has formal work	.53	.005	(.015)	.46	.064	(.032)**
Mother housewife	.51	.003	(.015)	.51	−.001	(.032)
Both parents work	.16	−.019	(.011)*	.12	.032	(.022)
Mother dropout high school	.43	.010	(.015)	.45	−.006	(.032)
Father dropout high school	.45	.008	(.015)	.49	−.046	(.032)
Mother college graduate	.05	−.008	(.006)	.04	.006	(.013)
Father college graduate	.05	.004	(.007)	.05	.011	(.014)

Note.—Balance of covariates for retakers in $t = 2$ (cols. 1–3) and in $t = 3$ (cols. 4–6). Each line shows the estimation of eq. (1) where Y_i is each variable in the left-hand column and $\mathbf{1}(T_{i,t} \geq \tau)$ is an indicator whether the student scored at least the cutoff in $t = 2$ or 3. The function f is a linear control function for each side of the cutoff. The estimation is restricted to students in a window of 44 PSU points around the cutoff (Imbens and Kalyanaraman optimal bandwidth). Robust standard errors are in parentheses.

* $p \leq 10$ percent.
** $p \leq 5$ percent.
*** $p \leq 1$ percent.

Appendix C

International Evidence

In this appendix I explore credit restrictions in an international context. Ideally, we would like to see the relation on the family income gap relative to different levels of college credit restrictions. However, such data are not available for enough countries. Instead, I will argue that some proxies of credit restrictions can be used to explore the relationship to the family income gap.

I will test a standard prediction from human capital models with credit constraints (see, e.g., Lochner and Monge-Naranjo 2011, 2012) that establish that, in the presence of credit constraints, students stop investing in education when returns are still high. Therefore, countries with higher credit restrictions should have higher returns to education.

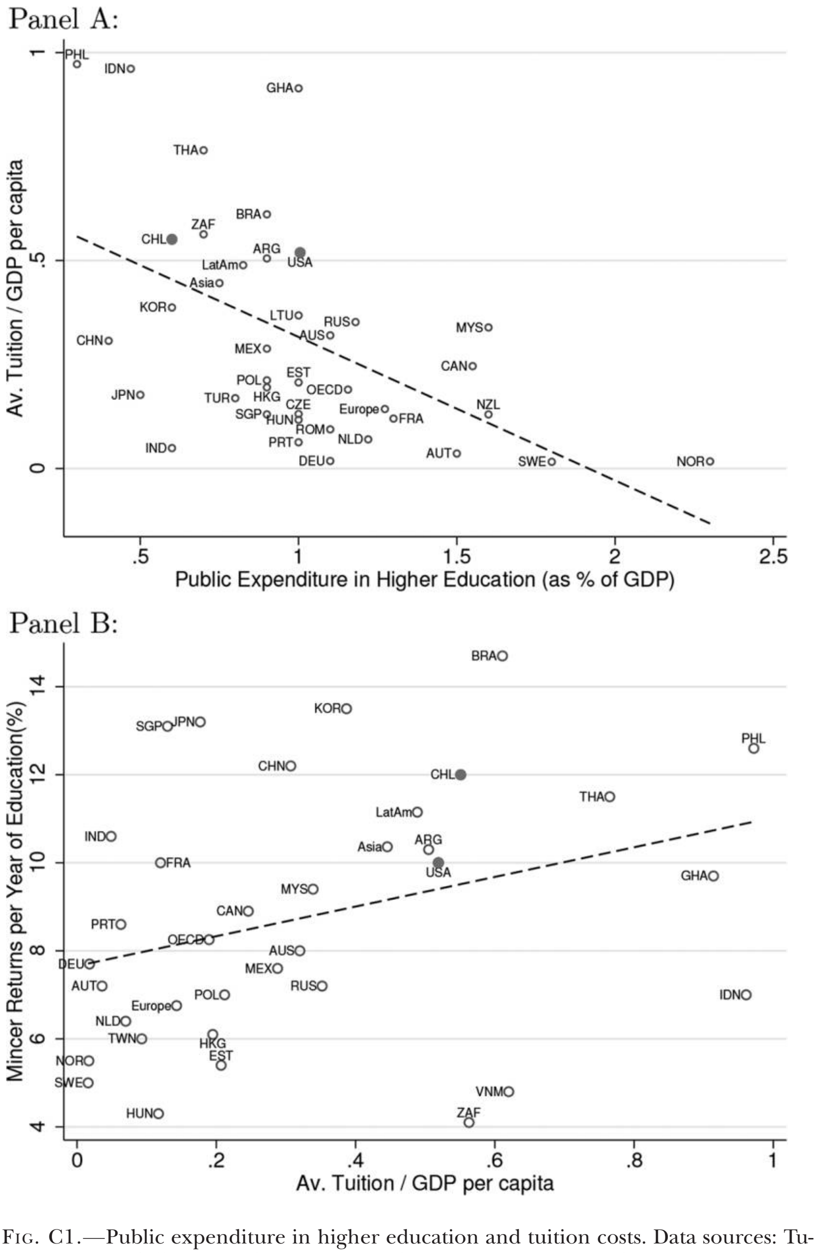


Fig. C1.—Public expenditure in higher education and tuition costs. Data sources: Tuition in 2009 US dollars from the International Comparative Higher Education and Finance Project, State University of New York at Buffalo. Average tuition computed as a simple average of all available categories (e.g., private or public). GDP per capita in 2009 US dollars from World Data Bank. Public expenditure in higher education (as a percentage of GDP) from UNESCO Statistics on Higher Education and OECD Education at a Glance 2012. Values are for 2008 or the closest year. Mincer returns to education are from Psacharopoulos and Patrinos (2004). For exposition purposes, the figures are truncated at (average tuition)/(GDP per capita) ≤ 1. Kenya, Morocco, Tanzania, and Uganda are excluded with the following coordinates (Public Expenditure, Av. Tuition/GDP per capita, Mincer return): (1.1 percent, 3.8, 16), (0.74 percent, 2.25, 15.8), (1.62 percent, 2.75, 11.9), (0.4 percent, 4.03, —). Color version available as an online enhancement.

High public spending on higher education could imply two things: (1) direct funding to universities, which might allow them to charge lower tuition costs, or (2) the availability of grants and loans, both of which would alleviate financial restrictions. Panel A in figure C1 confirms that there is a negative relation between public spending and tuition costs (relative to per capita income). On one extreme, Norway and Sweden have zero relative tuition and high public spending on higher education; on the other, African, Latin American, and Southeast Asian countries have higher relative tuition (yearly tuition between 50 percent and 100 percent of their per capita income) and low public spending (lower than 1 percent). This evidence suggests that universities finance their operations through higher tuition, and therefore, students are in charge of funding college.

Panel B shows evidence consistent with human capital models with credit constraints. The returns to education (Mincer) should be higher in countries that rely on tuition paid by students, because they are more likely to have credit restrictions. This figure shows that African and Southeast Asian countries are in the worst position when it comes to high returns to education and high tuition costs, followed by Latin American countries and the United States. This indicates that the evidence presented for Chile could also be valid for other countries with conditions similar to those presented here.

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