Labor Market Returns to Student Loans for University: Evidence from Chile*

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June, 2019

Abstract

We study the labor market returns to a State Guaranteed Loan used to finance university degrees in Chile. Using a regression discontinuity design, we show that marginally eligible students forgo vocational education in favor of university education but reduce their probability of graduation. Despite the fact that university loan takers accumulate more student debt, their labor market outcomes are not different from those of ineligible students. We find suggestive evidence that the lower quality of the receiving institutions accounts for these results. Finally, we extrapolate the effects away from the eligibility cutoff and show that supramarginal students benefit from this policy.

*Corresponding author: Pablo Muñoz, University of California at Berkeley, Department of Economics. Email: pablomh@berkeley.edu. Our thanks to Heidi Berner, Amanda Dawes, Cristian Labra, Isidora Palma, Javiera Troncoso, and the Ministerio de Desarrollo Social of Chile for their help accessing the data. The views expressed here are those of the authors and do not reflect the views of the Ministerio de Desarrollo Social. We also thank the editor and three anonymous referees for useful comments and suggestions. Nikhil Agarwal, Daron Acemoglu, Joshua Angrist, David Card, Loreto Cox, Francisco Gallego, Jeanne Lafortune, Julien Lafortune, Parag Pathak, Tomas Rau, Jesse Rothstein, Seth Zimmerman, and seminar participants at the MIT labor field lunch, the PUC-Chile applied microeconomics seminar, Universidad de Chile economics seminar, and Universidad del Rosario economics seminar also provided useful comments and suggestions. Alonso Bucarey thanks the financial support of the National Academy of Education/Spencer Dissertation Fellowship and the George P. and Obie B. Shultz Fund. Dante Contreras acknowledges the financial support provided by the Centre for Social Conflict and Cohesion Studies (CONICYT/FONDAP/15130009). Pablo Muñoz thanks the financial support of the Institute for Research on Labor and Employment. Carlos Guastavino provided excellent research assistance. Alonso Bucarey, email: alonsobucareyc@gmail.com. Dante Contreras, Universidad de Chile, Faculty of Economics and Business, Department of Economics, email: dcontrer@fen.uchile.cl.

1 Introduction

State Guaranteed Loans (SGL) are an integral part of many higher education systems and they are shown to be key in making college education more attainable.[1] Government intervention is based on the idea that the banking system is not structured for students to borrow against their future income, but because higher education offers a promising return on investment, a student's university education is worth funding. However, multiple countries have seen a rise in the default rates of student loans, paired with protests over the high cost of higher education and policy proposals to overwrite or forgive student loans. According to Hasting et al. (2013), there was an increase in default rates on federal student loans in the United States throughout the 2000s, and a large protest in early 2010 over high student debt emerged. Similar movements have recently been observed in other countries, such as England and Chile. This suggests that, at least to some extent, not all students benefit from taking out loans. Indeed, it is often argued that student indebtedness may decrease home ownership, delay saving for retirement, and affect family planning decisions (e.g. Mezza et al., 2016; Cheatham et al., 2013). Political analysts have also pointed out that the current level of student debt may become problematic for the economy as a whole if students are not able to repay their loans (Foroohar, 2017; Shell, 2018).

Why do students have trouble repaying their student loans? There are many possible explanations: students face income shocks that make them miss some payments, students are mismatched to their chosen degrees, students use loans to finance low return degrees, or students acquire loans for high return degrees, but with low graduation rates. All these alternative explanations have quite different policy implications. For example, negative income shocks call for the use of income contingent loans, while students who are mismatched could benefit from being more informed. Further, students using the loans to enroll at low return degrees, or at programs with high dropout rates, may suggest regulating "where" loans can be used. In spite of the fact that student loans are a controversial and policy relevant topic, causal evidence on its effects is still sparse.

In this paper, we evaluate educational paths and labor market outcomes for periods of

[1] In the U.S., attendance increased by 52% between 1990 and 2010 (NCES Digest, 2011). During the same time period, student loans rose by 250%. In Chile and in other Latin American countries there has been a similar increase (Ferreyra et al., 2017).

up to nine years after high school graduation. We use rich administrative data and a score-based cutoff for student loan eligibility to contribute to the literature, with causal estimates of the educational and earnings gains associated to taking out a student loan. In doing so, we explore the mechanisms behind our results and offer new evidence to better inform the debate surrounding student loans. We study the case of Chile, a middle-income OECD member country in which a SGL with sharp eligibility criteria was introduced in 2006. Like the United States, Chile has experienced a large expansion of higher education attainment based on means-tested financial aid policies. Unlike the United States, it is possible to systematically link records of all financial aid applicants to higher education enrollment, along with graduation and labor market outcomes in Chile. We believe that the Chilean experience is appealing for several reasons. First, 41% of students use loans to finance their higher education, and 69% of those loans correspond to the referenced SGL. Second, students gain access to the SGL to finance university degrees by applying for financial aid and scoring above a fixed minimum cutoff in a centralized college entrance exam, an institutional feature that allows us to leverage quasi-random variation in loan eligibility in order to study its effects and to instrument take-up of the SGL. Finally, like many countries, Chile has experienced intense policy debate around high indebtedness and poor labor market outcomes of loan takers.

Our analysis focuses on the causal effect of ever using a SGL on educational and labor market outcomes when the SGL is used to finance university attendance. Our main empirical strategy is a fuzzy regression discontinuity design that uses initial loan qualification as an instrument for ever taking out a university SGL. First stage estimates show that being eligible increases take-up by 35%, with an F-statistic above 100. Manipulation and balance checks support the internal validity of our design. Regarding its external validity, we follow Abadie (2002) to show that SGL compliers constitute a relevant population to evaluate the effects of financial aid policy. Compliers in our setting disproportionately come from the poorest 20% of the population, have less educated parents, and rely more heavily on public health insurance.[2] Furthermore, most of them are eligible for a SGL at a vocational institution, where the requirement is to have a minimum GPA, making this type of higher education a natural alternative for them. Therefore, while this population is not necessarily constrained to finance a vocational degree, they are less likely to have the opportunity to finance a university degree.

[2]Similar strategies have been used before in the context of student loans, e.g. Solis (2017); Gurgand, Lorenceau, and Mélonio (2011); Melguiso, Sanchez and Velasco (2016). Score-based cutoffs have been used in multiple contexts in higher education studies, e.g. Hoekstra (2009), Hastings et al. (2013), and Saavedra (2009).

In line with findings in financial aid literature (e.g. Angrist et al. 2014), our main results show that students who take out university loans due to their initial eligibility substitute alternative vocational options for university degrees. Thus, relative to their counterfactual, they increase their total years of higher education only by two years. Eight years after high school, only 40% of university loan takers will have graduated, compared to 65% of the students who did not take out a university loan because of not being eligible initially. Moreover, by year nine out of high school, university loan takers will have increased their student debt by 14 thousand dollars, and they will have lost 1.2 years of labor market experience without obtaining any significant gain on wages, employment, type of contract, or type of firm. Together, these results suggest that subsidized loans can be an effective way to get students into college but ineffective in helping them graduate and improve their labor market prospects.[3]

We interpret these null labor market effects for marginal students in light of recent evidence of the heterogeneous labor market returns to higher education alternatives (e.g. Hoekstra, 2009; Saavedra, 2009; Hastings et al., 2013; Zimmerman, 2014; Kierkeboen et al. 2016; Rodriguez et al., 2016). We find that marginally eligible loan takers opt out of high selectivity vocational institutions in favor of mid selectivity universities, which are of lower quality as evidenced by both the number of years of institutional accreditation and an observational wage value-added measure. Moreover, leveraging geographical variation of the predetermined supply of higher education institutions found around each student's high school, we show that students who enroll at universities, which are in the predicted top quartile of years of accreditation, will secure work at higher paying firms and are more likely to work in the public sector; graduation and wages also seem to improve for them, relative to other loan takers who enroll at institutions of lower predicted quality. Mismatch or a parental response could also account for the null labor market effects. However, we find no evidence in favor of these alternative hypotheses. We show that the inframarginal loan taker who enrolls at a university has a similar GPA to that of the average student enrolled at the receiving program. Moreover, we do not find differential labor market responses by the parents of the students at the margin of loan eligibility.

Finally, we study the effects of the SGL away from the cutoff using the extrapolation methodology proposed by Angrist and Rokkanen (2015). We find that, similar to the results of students around the cutoff, inframarginal students do not benefit from the use of a uni-

[3]This problem has also been documented in the U.S., where only about 30 percent of Pell Grant recipients had completed college by year 6 (Baum and Scott-Clayton, 2013).

versity loan. However, university loan takers who score 0.5 standard deviations above the loan eligibility cutoff, and who are able to use loans to attend better institutions, graduate from university at similar rates as they would have had they graduated from vocational institutions, and they also experience a wage increase of 426 dollars per month. Together, these findings suggest that the null returns of the SGL may come from lower graduation rates among marginal students, a result related to that of Cohodes and Goodman (2014) who show that financial aid could incentivize students to attend poorer quality institutions.

1.1 Related Literature

This paper is related to a large body of literature that estimates the causal effect of schooling and the role of credit constraints (Card 1999, 2001; and Carneiro and Heckman 2002). Specifically, it contributes new evidence to a growing literature on the effects of financial aid on educational outcomes. While most of the literature has focused on the effects of scholarships or grants for higher education (Abraham and Clark, 2006; Angrist et al., 2014; Avery et al., 2006; Bound and Turner, 2002; Cornwell et al., 2006; Dynarski, 2000; Goodman, 2008; Kane, 2007), our focus is on the effects of student loans on educational outcomes, e.g. Marx and Turner (2018). Indirectly, our paper also relates to previous evidence on the heterogeneous returns to degrees, e.g. Dale and Krueger (2002, 2014), Hoekstra (2009), Armona et al. (2018), among others in the U.S.

Our work directly contributes to an incipient literature on the effects of student loans on early labor market outcomes, e.g. Rothstein and Rouse (2011), Weidner (2016), and Ji (2018). The scarcity of evidence for the medium- or long-term impact of financial aid on labor market outcomes reflects the challenge of following students from high school graduation through to their adulthood in the labor market. Works that do provide causal estimates, at the state-specific level, in the United States are Scott-Clayton and Zafar (2016), Bettinger et al. (2019), and Denning, Marx, and Turner (2018); whose results suggest a positive effect of merit-based scholarships on labor market outcomes. Respect to them, our work informs the debate and contributes evidence on the effect of a different, nationwide, financial aid policy: State Guaranteed Loans.

In the Chilean context, a prominent work is Solis (2017) that reports large effects of SGL on college enrollment. While we replicate some of the short-run results documented in this paper, our analysis suggests that accounting for the counterfactual alternative of marginal students is important. We show, in online appendix 1, that in contrast to Solis (2017), the

university enrollment gap between rich and poor students with access to the loan is very similar to the rich vs. poor enrollment rate of students who do not have access to the loan (the difference is only 5 percentage points). Moreover, we show that accounting for enrollment at vocational institutions is important, as students who are not eligible for the university SGL eventually enroll in some type of higher education without it.

Furthermore, in the Chilean context, Rau et al. (2013), RRU hereafter, first studied the effect of loan take-up on labor market outcomes five years after high school graduation using a structural model. While they also report a null effect of the SGL on earnings, our paper exhibits several differences with their work. First, RRU suggest that institutions enrolling SGL takers decrease their quality in order to keep enrollment up and retain funding. In contrast, our results indicate that SGL takers have low graduation rates and end-up attending multiple institutions. Second, RRU analyzes earnings five years after high school graduation, which does not allow enough time for university students to graduate. Indeed, we show that, by year five after high school, there is still a significant share of students enrolled in higher education, limiting the interpretation of RRU's findings in the labor market. Finally, as in Solis (2017), the regression discontinuity analysis by RRU does not account for the significant trade-off between enrollment at universities and vocational institutions. A contemporaneous study, developed in parallel to ours, is Montoya et al. (2018), which supports our finding of a zero effect on earnings, using the same time horizon. Our work differs, however, in the way in which theirs assesses the presence of debt aversion as a potential mechanism behind the null labor market effect, while we show the importance of institutional quality in explaining this result.

The rest of this paper is organized as follows. Section 2 discusses institutional features of the SGL program. Section 3 presents the data and section 4 explains the empirical strategy used in this paper. Section 5 shows the effects of university loan take-up, and section 6 presents suggestive evidence on the role of higher education institutions. Finally, section 7 presents the extrapolation methodology and its results. Section 8 concludes.

2 Background

High school graduates applying for admission at 4-year institutions take a centralized college admission test (Prueba de Seleccion Universitaria, PSU), which includes sections on math, language, science, and history. Scores on each section are normalized within a range of 150 to 850, with a mean of 500 and a standard deviation of 110. In 2006, the Chilean government

introduced a State Guaranteed Loan program (SGL). This policy provides access to loans at any accredited higher education institution to students who complete a socio-economic information form (Formulario Único de Acreditación Socioeconómica, FUAS hereafter)[4] and score above 475 points on the average between the math and language sections of the college admission exam. Students who do not meet the cutoff minimum, but who have a high school GPA above 5.27 (GPA range: 1 to 7), are also eligible for a SGL at an accredited vocational institution but are not eligible for a university SGL. Figure 1 shows that the data conforms to these sharp eligibility criteria. Panels A and B show the total first-year student debt at any institution and the university debt, respectively.

Loan applicants know their eligibility status before enrolling in higher education, and the general terms of the loan are publicly available. Students considering higher education alternatives can easily determine their eligibility status for a SGL at different types of institutions after their PSU tests are graded because the same SGL eligibility cutoffs have been in place since 2006. Moreover, both the government and higher education institutions inform the students of their eligibility to receive the state guaranteed loan and provide information on other forms of financial aid.

Some institutions and degree programs are allowed to impose requirements for loan eligibility above those established by legislation. These institution-specific requirements are also available to students at the time of enrollment. During the years of this study, the terms of the loan were as follows: a 6% real interest rate; a fixed payment over a period of 5, 10, or 15 years, depending on the total debt; and a grace period of 18 months before students have to make the first payment after their graduation, or one month after a student drops out. Additionally, conditional on being in good academic standing, students could finance their degree for up to three years in excess of the official duration if it is for a university degree, and for two additional years if it is for a vocational degree.

Students can use the SGL at any accredited institution. Accreditation is the responsibility of the Comisión Nacional de Acreditación (CNA), an independent agency that decides whether an institution receives accreditation and determines the number of years of accreditation based on different records from the university and external auditors. After the

[4]This form helps the government determine family income quintiles. Although the SGL program was initially meant to benefit students in the first four income quintiles, we have learned through conversations with policy makers involved in the implementation of the loan program and through our own analysis of the data that individuals in the fifth income quintile also became eligible in years when there was enough funding.

introduction of the SGL, the total number of accredited institutions rapidly increased, from 14 in 2004 to 45 in 2016 (see Table 1 in the Appendix for more details). As of 2018, 64.3% of all higher education institutions are accredited, but there is significant heterogeneity in the number of years for which they receive the accreditation (from 1 to 7). After enrolling at an accredited institution of their choice, students can borrow up to a degree-institution specific maximum. The degree-institution cap for borrowers is, on average, 90% of the tuition, so the difference would have to be covered by students, their families, or scholarships. Alternative financing options include another university loan, available only for students enrolled at universities belonging to the "Council of rectors" (Consejo de Rectores de Universidades Chilenas, CRUCH), and government-provided scholarships.

For the cohorts in our analysis, students who are among the poorest 40% of the population, and who scored above an average of 550 points on the Mathematics and Language exams of the PSU have access to scholarships that partially cover tuition for four-year degrees, while students with a GPA above 5.0 are eligible for scholarships in two-year degree programs. Therefore, students who qualified by a small margin for a SGL at a university do not have access to government-provided scholarships. Indeed, most students who did not meet the minimum eligibility requirements for a loan would probably have had trouble securing a private loan to fund their education. According to the nationally representative household survey CASEN, only 7.5% of the loans held by all students in 2015 came from private banks (without State guarantee).

Since the implementation of the SGL, the total debt held by students has increased at a rate of 70% per year, and the total number of students holding a student loan increased from 15.8 thousand students, in the first year of operation, to 652 thousand students by 2016. Figure 2 presents the evolution of both the number of students using the SGL and the total debt they incurred. As these figures began to increase, commentators started arguing about the burden that student debt can impose on borrowers and whether the rising outstanding debt should be of public concern. In fact, in April 2018, congress created a commission to reformulate the SGL and investigate whether debt negatively affected students. The Chilean government is currently considering a complete reform of the SGL.

3 Data and Descriptive Statistics

Our dataset includes demographic information, test scores, enrollment, graduation, financial aid, and labor market outcomes for the totality of high school graduates from 2007 to 2017.

In our analysis, we focus on a sample of students who graduated from high school in 2007 or 2008, took the college admission test immediately after, and provided their socio-economic information to apply for financial aid. For these students, we observed enrollment in higher education, their loan take-up, whether or not they graduated and when, the characteristics of their degrees, as well as their labor market participation, wages, and employer characteristics for up to nine years after high school graduation.

Labor market outcomes come from the Chilean Unemployment Insurance (UI) dataset, which covers all dependent labor in the private sector between January 2007 and October 2017, and the Pension System (PS) dataset that includes dependent and independent labor in both the public and private sector, but which we can access only from 2013 to 2015. Thus, while we conduct most of our analysis using UI data, we also use the PS data to study public sector work and to check the robustness of our results to the inclusion of independent workers. The online appendix 9 provides further details on data processing and sample construction.

Financial aid applicants and students in our analysis sample are lower income than the general population of test takers. Table 1 reports descriptive statistics for test takers, financial aid applicants, students eligible for a university loan, and students within a bandwidth of 40 points around the cutoff (RD sample).[5] Column (2) shows that financial aid applicants are more likely to have attended a public high school, to have public health insurance, and to have less-educated parents. In contrast, column (3) shows that students who qualify for a university loan by scoring above 475 points come from a higher socio-economic background. Our RD sample, in column (4), has similar characteristics to the rest of financial aid applicants. No student in the RD sample is eligible for a State-provided scholarship, a byproduct of the test score bandwidth that excludes scholarship-eligible students. Finally, the labor market outcomes of students in these four samples are similar.

The characteristics of higher education institutions where the loan is used are important. Table 2 presents a comparison of universities and vocational institutions at different selectivity tiers. University tiers were constructed following Beyer et al. (2015) so that lower-tier institutions would have higher average math and language admission scores and a higher share of students taking the admission test. Top vocational institutions have a greater share of students who took the college admission test and are above the median within these types of institutions. We see that middle-tier universities have the highest concentration of stu-

[5]These 40 points correspond to 0.36 standard deviations of the running variable.

dents using the SGL. On the one hand, the more selective second-tier universities are better than, or similar to, top vocational institutions in terms of years of accreditation, graduation rates, and the share of students with scholarships. On the other hand, less selective third-tier universities are worse than top vocational institutions considering all of these dimensions. Moreover, vocational institutions are significantly cheaper than these middle-tier universities. Finally, the two types of institutions that concentrate the smallest share of students with the SGL (first- and fourth-tier universities) are at opposite extremes in terms of their characteristics, and they enroll fewer students using the SGL for distinct reasons. Students enrolled at first-tier universities have more access to merit-based scholarships, decreasing their need for loans. Meanwhile, 18% of universities in the fourth-tier universities are not accredited, which makes their students ineligible to use the SGL. We validated this measure of quality with an observational value-added model (VAM) at the institution level. We come back to this in section 5.

4 Empirical Framework

The first causal relationship of interest is the effect of being eligible for the university SGL (right after graduating from high school) on labor market outcomes. This is identified directly by the sharp regression discontinuity design of SGL eligibility. We estimate the following specification:

$$Y_i = \gamma Z_i + h(r_i) + e_i \tag{1}$$

where γ identifies the causal effect of interest, $Z_i = 1(r_i > 0)$ is an initial eligibility dummy that equals 1 if the student scored above 475 in her first attempt at the college admission exam, and $h(r_i)$ is a function of the running variable r_i = (Average math and language - 475) where math and language correspond to the score in the first attempt at the college admission exam. Specifically, we use a linear polynomial of r_i on both sides of the threshold.

A second relationship of interest is the effect of taking up a SGL at a university in educational and labor market outcomes. We model this relationship by

$$Y_i = \beta L_i + f(r_i) + e_i \tag{2}$$

where Y_i is an outcome for student i, L_i is an indicator of treatment equal to one if the student ever used a SGL to enroll at a university, and $f(r_i)$ is a function of the running variable modeled similarly to $h(r_i)$.

A potential threat to identification in this setting is that the decision of taking up a SGL to attend university may be related to students' comparative advantage. If that is the case, ordinary least squares (OLS) estimates of equation (2) will not recover the causal effect of loan take-up. To address this concern, we estimate equation (2) by two-stage least squares (2SLS), using the following first-stage equation:

$$L_i = \pi Z_i + g(r_i) + v_i \tag{3}$$

where $g(r_i)$ is a linear function of the running variable r_i with a different slope at each side of the cutoff and Z_i is the initial eligibility indicator defined before. In our setting, where students cannot manipulate the score they get, the first-stage exploits the quasi-random nature of initial eligibility around the eligibility cutoff. Figure 3 plots university loan take-up among students who initially applied for financial aid and those who did not. Plotted points are conditional means for all students in our analysis sample within a two-point binwidth of the average math and language scores obtained by the students on their first PSU attempt. Panel A shows the increase in university loan take-up among students who cross the first-year eligibility cutoff. As previously reported by Solis (2017), immediately after high school graduation, university loan take-up jumps from 0 to 15% for eligible students. On the other hand, students who did not apply for financial aid do not have access to the SGL. This validates the sharp nature of initial eligibility exploited in equations (1) and (3). The figure also shows that loan take-up starts decreasing above the cutoff of 550 points needed to access government scholarships. Panel B of this figure graphically shows the first stage equation (3), where crossing the initial eligibility cutoff discontinuously increases the probability of ever taking up a university loan. This also highlights the fuzzy nature of this quasi experiment. Even without being initially eligible, prospective students are able to retake the college admission test or get a SGL as second-year university students.

Estimates of equation (3) confirm the graphical representation of the first stage. Within a bandwidth of 40-points, initial eligibility increases the probability of ever taking up a university loan by 8 percentage points, a 35% increase relative to the 23% take-up among initially ineligible students.[6] This effect is precisely estimated with a standard error of 0.008, which implies a strong first stage with an F-test above 100. Moreover, our design passes the standard tests of non-manipulation of the running variable, and covariates are balanced between treated and untreated students. Visual inspection of the density of the running variable in

[6]We use a 40-point bandwidth for our analysis. We report results using outcome-specific bandwidths using the method in Calonico et al. (2014).

Figure 4 and the overlapping confidence intervals of density estimates on both sides of the cutoff indicate no manipulation on the centrally administered tests used to construct the running variable. Corroborating our conclusions, the tests proposed by Cattaneo et al. (2016, 2019) and McCrary (2008) fail to reject the null hypothesis of equal densities around the cutoff. Additionally, students who scored above the cutoff look much like students who scored below it. Table 3 presents a simple comparison of baseline characteristics for students on both sides of the cutoff. The coefficients come from regressions of each baseline characteristic on the initial eligibility indicator Z_i , substituting L_i by the corresponding characteristic in equation (3). We consider different samples: column (1) uses all test takers and column (2) only uses test takers who applied for financial aid. Finally, column (3) includes test-takers who completed the financial aid application and scored within a 40-point bandwidth of the eligibility cutoff. i.e. our RD sample. As reflected by the coefficients in column (3), mean differences are small and statistically insignificant in the RD sample, indicating that random assignment of initial eligibility for a university loan is a reasonable assumption around the cutoff.

Under standard assumptions, the 2SLS estimate of β in equation (2) may be interpreted as a local average treatment effect (LATE).[7] This is an average causal effect of SGL use for compliers, i.e. those students who use the loan at some point to enroll at a university should they be initially eligible, and thus would never use the loan to enroll at a university otherwise (Imbens and Angrist, 1994; Angrist et al., 1996). While the nature of our approach restricts the group for which we can identify the causal effect of the university SGL, this strategy deals with a potential threat arising from the test retaking behavior of students.[8] In our context, compliers are likely to be in need of financial aid to access a university education. Table 4 presents average demographic characteristics for different groups: all students in the analysis sample, university loan eligible students and university loan eligible compliers. Average characteristics for the latter group are estimated following Abadie (2002). Panel A shows that eligible compliers are more likely to have parents who did not pursue higher education, are more likely to have public health insurance, and to have attended public high schools. Additionally, Panel B shows that 51% of eligible compliers are part of the poorest 20% of the population, while 36% of students who are eligible for the university loan come

[7]Standard LATE assumptions in this case would imply that initial university loan eligibility only influences educational and labor market outcomes through the use of the loan to enroll at a university, and that initial loan eligibility weakly increases the take-up for all students.

[8]In online appendix 4.2 we present results from our analysis of retaking behavior. Nevertheless, an alternative strategy where the endogenous variable is defined as loan take-up on the first attempt and that leads to compliers who might retake the admissions exam, thus later qualify and take the SGL leads to similar results.

from this group. Finally, Panel C shows that only 36% of the eligible population take-up the university loan, and nobody in the complier population gained access to a university scholarship, but 74% of compliers are eligible for a SGL at a vocational institution.

5 Results

This section presents the effects of the university loan on education and labor market outcomes. We comment both the reduced form effects of loan eligibility, and the 2SLS estimates of university SGL take-up. The first estimates are the key input for ex-ante analysis of policies that give broader access to student loans, while the second are the key input from students' perspective or an ex-post evaluation. Reduced form effects are more precisely estimated and are emphasized throughout.

5.1 Effects of the University Loan on Education

Initial eligibility for the university loan did not have an effect on the decision of ever enrolling in some form of higher education; it did, however, encourage students to substitute vocational education in favor of university degrees and increased their total years of schooling. Figure 5 provides a graphical representation of the reduced form in equation (1) showing conditional means of an indicator for ever enrolling in higher education against the running variable. Panel A shows that students above and below the cutoff enrolled in some form of higher education at least once throughout the nine years after high school graduation, with students above the cutoff substituting vocational education for university degrees, as shown by panels B and C. Despite the null extensive margin effect, Figure 6 shows that initial eligibility increased the overall years of higher education. Panel A shows reduced form evidence that students who are initially eligible for the loan increase their overall education, with students increasing university attainment and reducing attainment at vocational degrees as shown in panel B and C.

Table 5 summarizes the previous reduced form effects and presents 2SLS estimates. The first row in this table shows the first stage, with initial university loan eligibility boosting the probability of ever taking up a university student loan by 8 percentage points over a mean take-up below the cutoff of 23 percentage points (a 35% effect with a F-statistic above 100). Column (1) shows the reduced form differences in enrollment between initially eligible students and students who, by a small margin, did not cross the cutoff. These estimates summarize the magnitudes displayed in Figures 5 and 6. Initial SGL eligibility at a university

increased the years of education by 0.18 years with an increase in university attainment of 0.43 years and decrease in vocational degrees of 0.25 years. Column (3) in Table 5 presents the 2SLS coefficients, which in our just-identified IV model correspond to the reduced form effects scaled by the first stage coefficient. Panel A shows that taking up a university loan increases the probability of ever enrolling at a university by 83 percentage points, while decreasing the probability of ever enrolling at a vocational institution by 71 percentage points. In Panel B we also find that taking up the university loan increases the total years of higher education by 2.1, similar to the difference in nominal duration between a university degree and a vocational degree. In fact, those who are induced by initial eligibility to take-up the university SGL gain 5.1 years in university while giving up 3 years in vocational institutions. An important caveat when interpreting this last result is that the take-up of the university SGL increases the number of institutions in which compliers pursue a degree by 40%.[9] Moreover, Panel C shows that taking the loan at a university decreases overall graduation by 25 percentage points, which results from a reduction in vocational graduation of 65 percentage points and an increase in university graduation of only 40 percentage points. Thus, while the loan helped students to move from a vocational degree into a university, by year eight out of high school, it did not help them finish their degrees.

Column (2) presents the complier mean among untreated students, $E[Y_{i0}|L_{i1} > L_{i0}]$ in the potential outcomes notation.[10] These estimates show that almost all ineligible compliers attended vocational education, and 17 percent of them also attended a university. Moreover, from the fact that $E[Y_{i1}|L_{i1} > L_{i0}] = E[Y_{i1} - Y_{i0}|L_{i1} > L_{i0}] + E[Y_{i0}|L_{i1} > L_{i0}]$, we conclude that 30 percent of all eligible compliers also attended vocational institutions.

Previous results are not significantly affected by students being enrolled in higher education at the time of our measurement. Figures 7 to 9 plot mean educational outcomes for every year after high school. By year nine, both the years of schooling and the enrollment rates have converged between eligible and ineligible compliers. Panel A of Figure 7 presents the fraction of students enrolled at a university between 1 and 9 years after high school, with panel B displaying analog results for vocational degree enrollment. Enrollment rates at both types of institutions decline significantly over the years, and there is convergence between eligible and ineligible compliers by year 9 out of high school. Moreover, by the

[9]The increase in the number of institutions in which a student enroll may arise from students switching either within 2- or 4-year institutions, or between 2- and 4-year institutions. We have excluded advanced degrees from our analysis.

[10]These are computed following Abadie (2002). Basically, we regress $(1 - L_i)Y_i = \rho(1 - L_i) + f(r_i, Z_i) + v_i$ using initial eligibility as an instrument. In this context, ρ is an estimate of $E[Y_{0i}|L_{i1} > L_{i0}]$.

end of our sample window, enrollment is less than 10% in vocational and university degrees. Similarly, Figure 8 shows that 7 years after high school graduation, the years of schooling at each type of institution have converged. Finally, Figure 9 shows that the proportion of students holding a degree increases over the years to reach 40% among eligible compliers and 65% among ineligible compliers. Although the graduation rate from university does not show convergence, the fact that the average number of years of schooling is stable while the enrollment rate declines to zero, indicates that a significant graduation increase is unlikely.

5.2 Effects on Debt and Labor Market Outcomes

Students who initially scored above the eligibility cutoff have higher debt and similar earnings compared to students who were ineligible by a small margin. Figure 10 shows graphical evidence of the reduced form effects of the increase in total accumulated SGL debt at any type of institution nine years after high school graduation. Students just above the cutoff accumulate more debt, an unsurprising result given that they also enroll longer and at institutions that are more expensive. More surprising is that students end up with a similar level of earnings regardless of their initial university loan eligibility status. Figure 11 shows the average monthly wage of students nine years after high school where we see that students above and below the cutoff have almost indistinguishable average income. We find similar reduced form patterns for the probability of being employed, the probability of having a fixed term contract, the probability of having a part-time job, and the average wage paid at the firm.

Table 6 presents again the first stage results at the top, the reduced form effects in column (1), ineligible complier mean in column (2), the 2SLS estimates in column (3), and OLS estimates in column (4). The reduced form in Panel A shows that students initially eligible for a university SGL increase their overall debt in 1.2 thousand dollars of total debt substituting debt at vocational institutions for university debt, while Panels B and C show an insignificant effect on labor market outcomes with eligible students earning between -4.5 to -10.4 dollars per month less than ineligible students, being able to reject effects larger than 22 dollars. Turning to the causal estimates of taking up a university SGL we find that students who are induced to take up a university student loan because of their initial eligibility, i.e. the compliers, accumulate 14.3 thousand more dollars in student debt. In column (2), we see that the mean debt for students who are not initially eligible is 3 thousand dollars, which is explained by their eligibility for a loan at a vocational institution, their posterior eligibility to a loan at a university, and by the ability of students to borrow once

they enroll as second-year students in good academic standing. Panel B shows statistically insignificant effects of loan take-up on wages (ranging from -381 to 273 USD) and probability of employment (ranging from -0.27 to 0.07 percentual points). As shown by Panel C, these estimates are robust to the inclusion of independent and public sector workers. Students around the cutoff also work at firms that pay similar average wages, leaving little room for differential career paths that may arise as a consequence of the initial employer-employee match. Moreover, and unlike in previous studies (e.g. Rothstein and Rouse, 2011), we do not find any effect of the treatment on the probability of working in the public sector.[11] While the 2SLS estimates are noisy, the direct effect of eligibility tightly estimates a zero effect. In addition, we also examined differential effects by gender. The evidence indicates that women participate less in the labor market, but the effects of loan take-up on educational and labor market outcomes are similar for men and women.[12]

Finally, students who take out the university loan lost 1.2 years of labor market experience. From this last result, one might conjecture that the null effect on wages is partly because of the lost return to experience. In online appendix 2, we discuss this hypothesis using correlational evidence from a Mincer-type model that we estimate using the same time horizon and analysis sample used here. There we show that the zero effect on wages comes from both the cost of losing experience and the small return to university, relative to vocational, degrees. We conclude that the cost of lost experience is not large and it can only account for the null effect of the SGL on wages since the returns to the university loan take-up were extremely low compared to the returns to attending vocational degrees. However, the evolution of the return to experience vis a vis the relative return to university degrees is uncertain to us as researchers. Short-run and long-run effects might differ, for instance, if experience profiles were significantly steeper for university loan takers.

The negative effect on graduation and the null labor market gains associated with the take-up of university loans might be surprising. In the next section, we explore how the quality of destination institutions relative to fallback institutions could account for these results. In the spirit of Abdulkadiroglu et al. (2014) and Abdulkadiroglu et al. (2018), we start by characterizing the educational fallbacks for ineligible compliers and the destination institutions of eligible compliers. Then, we show how the results from the 2SLS approach used here differs for students facing a supply of high or low quality universities in the geographic

[11] This setting is different from Rothstein and Rouse (2011) in the sense that we study the effect of taking up a loan for university in a context where the counterfactual for ineligible compliers includes the possibility of using loans for vocational studies.

[12] Online appendix 5 provides details of this analysis.

location of their high school. We also discuss the plausibility of other hypotheses that could account by our main results.

6 Fallbacks, Destinations and the Role of Institutional Quality

The destination of eligible compliers and the fallback of ineligible compliers are important to understand the previous results. For instance, if students without initial access to the loan attend schools with similar or better performance than the institutions in which loan takers enroll, then the zero labor market effect might emerge naturally as a consequence of the high returns in fallback schools rather than as a consequence of the low performance of the universities that eligible compliers attend. To explore this hypothesis, we characterize the mix of schools that define the loan complier destinations and fallbacks. Following Abadie (2002), we estimate the ineligible compliers' fallback options with the equation:

$$C_{s(i)}(1 - L_i) = (1 - L_i)\gamma + f(r_i) + e_i \tag{4}$$

where $s(i)$ indicates either the first university where student i uses the SGL or the first institution she attended (when the student did not take-up a university loan), and $C_{s(i)}$ is the characteristic of that institution. Instrumenting $(1 - L_i)$ with the initial eligibility indicator Z_i, the 2SLS coefficient γ captures the average of the institution characteristic $C_{s(i)}$ for ineligible compliers. Similarly, we can replace $(1 - L_i)$ by L_i on both sides of the equation to estimate the mean characteristics of destination institutions for eligible compliers.

Columns (1) and (2) of Table 7 show the mean characteristics for all students, eligible and ineligible to take-up a university loan. Columns (3) and (4) report the same means only for students within the 40-points bandwidth. Finally, columns (5) and (6) show these means for eligible and ineligible compliers, and column (7) reports the difference among compliers, i.e. the local average treatment effects. On one hand, Panel A shows that loan take-up increases the years of enrollment at second- and third-tier universities, but has no meaningful impact on attendance at first-tier universities. On the other hand, the main fallback for the eligible compliers are top vocational institutions. Indeed, years of enrollment at top vocational institutions decreases by almost 3 years as a consequence of the university loan take-up, an economically significant effect closely associated with the average duration on vocational programs. Thus, a direct effect of the university loan was to divert marginal students from selective vocational programs into less selective, medium-tier universities. Consistent with this, Panel B reports the years of accreditation of the institutions at which eligible and ineli-

gible compliers enroll. Since the accreditation process for vocational and university programs may differ in criteria, we normalize the years of accreditation within the corresponding type of program at which students enroll for the first time. Columns (3)-(5) show that while all compliers attend institutions with accreditation below the sample mean, eligible compliers attend institutions with 0.6 standard deviations fewer years of accreditation than the average institution attended by ineligible compliers. We interpret this as a drop in the quality of the institution attended by students with a university SGL. This is corroborated by a positive relationship between institutions' accreditation and an observational measure of institutions' value-added. Reassuringly, we also find a drop in wage value-added among compliers, in line with our interpretation of the drop in accreditation as a drop in quality.[13]

We interpret the previous findings as evidence that the null effect of university loan take-up on wages could be related to the lower quality of destination universities attended by marginally eligible students (relative to their fallback alternatives). To further investigate this hypothesis, we estimate differential effects of using the university SGL at institutions of different quality. Specifically, we estimate:

$$Y_i = \alpha + \beta_0 L_i + \beta_1 \hat{Q}_i + \beta_2 \hat{Q}_i \times L_i + f(r_i, \hat{Q}_i \times r_i) + \nu_i$$

where $\hat{Q}_i$ is a dummy variable that equals one if the institution in which student i enrolls belongs to the top quartile of predicted accreditation, and L_i is an indicator for university loan take-up. Since enrollment at institutions of higher quality is a decision made by the student, and consequently an endogenous variable, we leverage variation in the predetermined accreditation of institutions around each student's high school. Specifically, we consider all students within a 40-point bandwidth around the eligibility threshold, and we use the average accreditation of the universities around a student's high school (within a 15 kilometer radius) as an instrument that predicts the accreditation of the institution in which the student uses the university SGL. Then, we classify the institutions into quartiles of predicted "quality" considering only the institutions attended by students around the eligibility cutoff. It is worth noting that, since the eligibility threshold is rather low, these students do not access very selective institutions. In this regard, our estimates speak to the role of relative institutional quality among students who can only access medium to low quality universities.[14]

[13]Estimating institutions' value-added is outside the scope of this paper, but we provide details and report results in the online appendix 7.

[14]Figure 1 in the Appendix shows the relationship between the average accreditation of universities in a student's geographical zone and the actual accreditation of the receiving institutions in which they use the university loan. Both variables are strongly associated with a t-stat of 25.

To estimate this model, we instrument $\hat{Q}_i \times L_i$ and L_i with the initial eligibility of the student for a university loan and its interaction with $\hat{Q}_i$. The coefficient of interest is β_2, the differential effect of taking out the loan to attend a university with higher accreditation. In addition to the assumptions used in our fuzzy RD, here we also assume that the supply of higher education only affects student outcomes through their choice of institution. As before, we run this regression within a 40-point bandwidth around the eligibility threshold, and we include a linear polynomial of the running variable for each group on both sides of the threshold.

Table 8 presents the 2SLS effects of university loan take-up on students who enroll at highly accredited universities vis a vis the effects on other university loan-takers. Columns (1) and (2) show the results obtained from estimating a fully saturated model, which allows us to compute the effects of university loan take-up for each group. In general, these results replicate the main findings presented in section 5. The differential effects of interest are presented in Column (3). Panel A shows the effects on educational outcomes. Students at top quartile universities seem to pay more tuition, and they also consistently accumulate more debt. Importantly, they also have a better probability of graduating from university and attend fewer institutions. Panel B and C show the differential effects on labor market outcomes. We see that students enrolling at top quartile universities are matched with better paying firms, suggesting better career prospects; and they have a higher probability of working in the public sector. In summary, we believe that these results are suggestive evidence in favor of our hypothesis about the role of institutional quality on labor market outcomes.[15]

We conclude this section by briefly discussing two alternative hypotheses that could explain the null effect of the university SGL on wages: 1) mismatch of students to institutions, and 2) a family response to loan access. Under the first alternative, loan takers are mismatched in their ability at universities and consequently fail to graduate. The second hypothesis suggests that parents compensate for loan ineligibility by changing their behavior (e.g. Pop-Eleches and Urquiola, 2013) and this in turn affects the outcomes of students. As shown in online appendix 8, we find no empirical evidence in support of these hypotheses. First, we show that the inframarginal loan taker has a GPA similar to that of the median student enrolled in the receiving program. If the student loan produces academic mismatch,

[15]These results are robust whether using the accreditation of the first institution in which a student enrolls or using the accreditation of the first institution in which a student uses the university loan. Moreover, while our prediction of actual accreditation using local supply of universities is noisy, most of our results are also robust to including the local supply of universities directly in a pseudo intention-to-treatment approach.

then it could indicate an increase in the distance between student ability and the ability of the median student enrolled in the receiving program. To study this, we compute the standardized GPA for the median first year student for each college-major-year combination. We then compute how far each student is from the median GPA of the first college major where they enroll. Then, we estimate whether loan take-up pushes students below the median of their college major. We find that this is not the case (see Table 13 in the online appendix). Second, while we have no information on parent investment, we have access to the labor market outcomes of parents' at the same time that students in our sample are graduating from high school. We do not observe differential labor market behavior of the parents for the marginal loan taker (see table 14 in the online appendix). While these tests cannot completely eliminate the scope for these alternative hypotheses, we believe that they reassure the role of institutional quality in explaining our main findings.

7 Effects Away from the Cutoff

We showed that, on average, the marginal students do not benefit from university SGLs in the labor market. However, we also showed that students who attend better institutions seem to perform better. Likewise, it is possible that students who qualify more easily for a loan will benefit more. To study this hypothesis, we extrapolate the effect of the university SGL for students away from the eligibility cutoff following the method introduced by Angrist and Rokkanen (2015). Their method is built on the following conditional independence assumption (CIA), similar to the unconfoundedness assumption used for matching. The intuition is that it is conditional on a set of observable characteristics, we break the link between running variable and treatment, and therefore, we can explore the effect of the treatment for different values of the running variable. Formally, the assumption is:

$$E[Y_{ij}|r_i, x_i] = E[Y_{ij}|x_i]; \quad j = 0, 1.$$

where j indexes the treatment status and outcomes are assumed to be mean-independent of the running variable conditional on x_i. In a RDD this has a testable implication:

$$E[Y_{i1}|r_i, x_i, r_i \geq 0] = E[Y_{i1}|x_i] = E[Y_{i1}|x_i, r_i \geq 0],$$

so we should expect that covariates that satisfy the CIA obey:

$$E[Y_i|r_i, x_i, Z_i = 1] = E[Y_i|x_i, Z_i = 1], \tag{5}$$

with a similar expression for $Z_i = 0$. In our case, we exploit the fact that the average of math

and language scores determines the initial eligibility for a university loan, $Z_i = 1(r_i \geq 0)$, but this is not the only measure that predicts achievement. We also observe a student's high school GPA and test scores in science, history, and math, all of which are used in the college admission process and that we can include in x_i. To validate our exercise, we first test the conditional independence assumption graphically. As noted by Angrist and Rokkanen (2015), the CIA implies the graphical pattern mentioned by Lee and Lemieux (2010): "in a randomized trial using a uniformly distributed random number to determine treatment assignment, the randomizer becomes the running variable for the RD design. The relationship between outcomes and this running variable should be flat, except possibly for a jump at the quantile cutoff that determines treatment assignment." Figure 12 plots wage residuals from a regression of wages on x_i against running variable. Panel A shows unconditional wages in three-point binwidth against the running variable, along with a fitted linear function at each side of the cutoff. As expected, we observe a positive relationship between the running variable and wages. From Panel B, we see that once we control for x_i (e.g. other available scores), the relationship between outcome residuals and the running variable becomes essentially flat, a fact that supports the conditional independence assumption needed for extrapolation. For completeness, we also follow Angrist and Rokkanen (2015) in using a regression of outcomes on x_i and r_i on either side of the cutoff as a simple test of the CIA assumption in equation (5) and its analog for $Z_i = 0$. Table 4 in the Appendix shows the extent to which conditioning on covariates can eliminate the relationship between the running variable and several outcomes, at specific intervals of the running variable. Given the evidence in favor of the conditional independence assumption for wages, and the existence of a common support (see Figure 2 in the Appendix), we move to estimate the causal effects of the loan away from the cutoff. Additionally, because the running variable has a negligible effect over graduation after conditioning by x_i, we also extrapolate this outcome in order to provide a more detailed picture of the mechanisms that underlie our findings.

At specific intervals of the running variable, the CIA and a common support assumption lead to the following matching-style estimand for the reduced form (with an analog expression for the first stage):

$$E[Y_{i1} - Y_{i0}|c_0 < r_i < c_1] = E[E[y_i|x_i, Z_i = 1] - E[y_i|x_i, Z_i = 0]|c_0 < r_i < c_1].$$

Letting $\lambda(x_i) \equiv E[Z_i|x_i]$ denote the propensity score. We use the propensity score

weighting estimator, which begins with the observation that the CIA implies:

$$E\left[\frac{y_i(1-Z_i)}{1-\lambda(x_i)}\middle| x_i\right] = E[Y_{i0}|x_i]$$
$$E\left[\frac{y_i Z_i}{\lambda(x_i)}\middle| x_i\right] = E[Y_{i1}|x_i]$$

Bringing these expressions together, the reduced form effect on students in any interval $r_i \in (c_0, c_1)$ is given by:

$$E[Y_{i1} - Y_{i0}|c_0 < r_i < c_1] = E\left[\frac{y_i[Z_i - \lambda(x_i)]}{\lambda(x_i)[1-\lambda(x_i)]} \times \frac{P[c_0 < r_i < c_1|x_i]}{P[c_0 < r_i < c_1]}\right] \tag{6}$$

where a propensity score weighting estimator for the reduced form is given by the sample analog of equation (6). This estimator requires a model for the probability $P[c_0 < r_i < c_1|x_i]$ as well as for $\lambda(x_i)$. For simplicity, we parameterize both in the same way with a logit model.

Figure 13 reports the reduced form estimates of university loan take-up on wages for different values of the running variable, calculated following expression (6). Consistent with our RD-based results, we see that students at the margin do not benefit from the use of a university loan, and neither do inframarginal students. Moreover, the point estimate using our RDD bandwidth is close to the estimates presented in the previous section, another reassuring fact supporting the extrapolation exercise for wages. However, we see that stronger applicants who scored above the loan eligibility cutoff experience gains in terms of wages; and these gains increase for higher values of the running variable.

Finally, Angrist and Rokkanen (2015) derive a Wald-type IV estimand with a generalized version of the CIA that we present in online appendix 3. The LATE estimand is given by:

$$E[Y_{i1} - Y_{i0}|L_{i1} > L_{i0}, c_0 < r_i < c_1] = \frac{E[E[y_i|x_i, Z_i = 1] - E[y_i|x_i, Z_i = 0]|c_0 < r_i < c_1]}{E[E[L_i|x_i, Z_i = 1] - E[L_i|x_i, Z_i = 0]|c_0 < r_i < c_1]}$$

Wald estimates for wages and graduation at different intervals of the running variable are presented in Table 9. Consistently with the reduced form results, the estimated coefficients for wages in column (2) show small and insignificant effects below the eligibility cutoff and evidence of large and increasing effects above it. In particular, we find that the effect of the

SGL for those who scored 0.5 standard deviations above the eligibility cutoff is around 430 dollars per month, an economically significant effect considering that the standard deviation of wages in our analysis sample is 633 USD. Columns (3) and (4) confirm the substitution pattern triggered by the university SGL. Interestingly, for students below the cutoff the increase in graduation from university less than compensates the drop in graduation from vocational. However, students above the cutoff seem to graduate from university at similar or higher rates than they graduate from vocational institutions. This result offers an explanation for the positive wage effect for students with higher scores, and it also suggests that marginal students have a hard time translating a university SGL into labor market returns because of the low graduation rate.

8 Summary and Conclusions

Nine years after high school graduation, marginal students who took out the university loans hold an extra 14 thousand dollars in student debt, have lost 1.2 years of labor market experience, have a lower graduation rate, and are more likely to have attended two or more institutions. Furthermore, their wages, employability, job security, and employers' characteristics are no different from those of students who did not enroll using the university SGL (but who would have if they had been initially eligible). Overall, these findings depict a concerning picture for the average students at the margin of eligibility, whom, despite a low socioeconomic background, decided to take the university selection exams and apply for financial aid, signaling their willingness to pursue higher education.

Nevertheless, our analysis also offers suggestive evidence that university loans could help students to finance their education and to benefit from it in the labor market. We show that marginal students who substitute vocational education for better quality university programs seem to perform better in terms of educational and labor market outcomes. Furthermore, extrapolation away from the cutoff shows that students with higher test scores can also profit from taking up this loan, a result related to having a better opportunity to complete university.

On a final note, we have shown that most students are out of higher education by the time we measure their labor market outcomes. However, our paper is silent about longer-run effects of this policy. Short-run and long-run effects might differ, for instance, if experience profiles were significantly steeper for university loan takers. Nonetheless, our results speak to the current debate about the labor market performance of the first generations of students

whom the SGL intended to help. How these students fare in the long run is an important task for future work.

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Tables

Table 1: Descriptive statistics for students

	Test takers	Analysis sample	University loan eligible	RD sample
	(1)	(2)	(3)	(4)
Panel A. Demographics at the time of high school graduation				
Female	0.54	0.57	0.53	0.59
Public high school	0.36	0.40	0.34	0.42
Voucher high school	0.52	0.56	0.60	0.57
Private high school	0.11	0.04	0.06	0.01
Average Math and Language college admission score	496.6	509.6	566.3	477.0
High school GPA (from 1 to 7)	5.60	5.70	5.86	5.57
Public health insurance	0.67	0.72	0.67	0.77
Mother with more than high school	0.28	0.25	0.33	0.18
Father with more than high school	0.33	0.30	0.38	0.22
Father monthly wage (dollars)	518.9	474.2	515.9	429.3
Have information on father wage	0.39	0.41	0.42	0.41
Panel B. Financial aid				
Ever taking up a university loan	0.21	0.28	0.36	0.31
University scholarship eligibility	0.09	0.16	0.25	0.00
Observations	298,859	177,470	113,059	53,416

Notes: This table reports descriptive statistics for different samples of high school students. Column (1) considers students graduating from high school in 2007 and 2008 who took the college admission test (PSU) right after high school graduation. This corresponds to 72% of all high school graduates. Column (2) further restricts the sample to students who filled out a financial aid application form, this is our analysis sample. Column (3) further restricts the sample to students who scored above 475 points on average on the Mathematics and Language sections of the PSU. Finally, column (4) includes students in the analysis sample with an average Mathematics and Language score around the eligibility cutoff, specifically in a 40-point bandwidth. Admission scores presented in panel A have a mean of 500 and a standard deviation of 110 points, and GPA ranges between 1 and 7 with a mean of 5.56 and standard deviation of 0.55.

Table 2: Descriptive statistics for higher education institutions

	University				Vocational	
	Tier 1	Tier 2	Tier 3	Tier 4	Top	Bottom
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. First year student characteristics in 2008						
Took admission test	0.65	0.52	0.38	0.24	0.31	0.17
Average Math and Language score	640.5	557.6	493.0	446.7	445.3	418.6
Students with a state guaranteed loan	0.07	0.18	0.16	0.00	0.21	0.05
Students with a scholarship	0.28	0.19	0.04	0.00	0.20	0.14
Panel B. Institutional characteristics weighted by first year enrollment in 2008						
Number of institutions	10	26	16	7	55	60
Average number of degrees	299.8	127.5	103.3	38.4	76.2	33.2
Total enrollment	29,629	50,279	23,713	4,207	63,290	13,663
Accredited degree	0.49	0.26	0.08	0.06	0.03	0.00
Accredited institution	1.00	1.00	0.97	0.80	0.96	0.62
Years of accreditation	5.98	4.44	2.82	1.60	4.99	1.86
Graduation rate	0.48	0.39	0.33	0.23	0.37	0.31
Tuition (thousand dollars)	5.10	4.25	3.29	2.95	2.11	1.29

Notes: This table reports characteristics of all higher education institutions. Universities are categorized in selectivity tiers following Beyer et al. (2015). Tiers are defined using the average Math and Language score of enrolled students. First-tier are in the range 600-850; second-tier in the range 525-600; third-tier in the range 450-525; and fourth-tier includes institutions with an average below 450 and with more than half of students without a score. Vocational institutions are classified using the fraction of students who took the college admission exam. Top vocational institutions have a fraction above the median (23%). All characteristics are weighted by the total level of first-year enrollment in 2008. Graduation rate is constructed at the institution level and it corresponds to the share of students who are enrolled in their first year in 2008 and ever graduate (between 2008-2015) from the institution. Tuition corresponds to the annual tuition.

Table 3: Covariate balance

	Test takers (1)	Analysis sample (2)	RD sample (3)
Female	0.023***	0.017***	-0.002
	(0.003)	(0.004)	(0.008)
Father monthly wage in t=0	-11.092	-11.509	-7.577
	(6.781)	(7.571)	(13.271)
Have information on father wage	0.016***	0.013***	0.013
	(0.003)	(0.004)	(0.008)
Mother has more than high school	0.009***	0.003	0.004
	(0.003)	(0.004)	(0.007)
Father has more than high school	0.014***	-0.001	0.000
	(0.003)	(0.004)	(0.007)
Public health insurance	-0.002	0.005	0.005
	(0.003)	(0.004)	(0.007)
Public high school	-0.031***	-0.040***	0.006
	(0.003)	(0.004)	(0.008)
Voucher high school	0.085***	0.070***	-0.006
	(0.003)	(0.004)	(0.009)
Private high school	-0.054***	-0.030***	0.000
	(0.002)	(0.002)	(0.002)
High School GPA	0.004	0.002	0.009
	(0.004)	(0.004)	(0.010)
Observations	298,859	177,470	53,416

Notes: This table compares characteristics of eligible and ineligible students to the university State Guaranteed Loan in three different samples. Column (1) reports differences among all test takers, column (2) presents a comparison between test takers who also applied for financial aid, and column (3) restricts the comparison to a sample of students within 40 points around the eligibility cutoff. Coefficients come from regressions of each baseline characteristic on the initial eligibility indicator Z_i, controlling by the test score with a different slope at each side of the cutoff. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 4: Complier characteristics

	Analysis sample (1)	University loan eligible (2)	Eligible compliers (3)
Panel A. Demographics			
Female	0.57	0.53	0.64
Mother has more than high school	0.25	0.33	0.20
Father has more than high school	0.30	0.38	0.23
Public health insurance	0.72	0.67	0.81
Public high school	0.40	0.34	0.41
Voucher high school	0.56	0.60	0.59
Private high school	0.04	0.06	0.00
Panel B. Family income quintile			
1st	0.44	0.36	0.51
2nd	0.19	0.19	0.17
3rd	0.14	0.16	0.20
4th	0.12	0.16	0.10
5th	0.11	0.14	0.02
Panel C. Financial aid			
University loan eligible	0.63	1.00	1.00
University loan ever take up	0.28	0.36	1.00
University scholarship eligibility	0.16	0.25	0.00
Vocational loan eligible	0.81	0.91	0.74

Notes: This table presents the average characteristics for our analysis sample in column (1). These are high school graduates who took the college admission test and applied for financial aid in their last year of high school. Column (2) shows average characteristics of students in the analysis sample who qualified for a university loan, and column (3) shows the estimated average characteristics of the eligible complier population computed following Abadie (2002). GPA>5.27 implies that the student is eligible for a SGL at vocational institutions.

Table 5: Effect of loan take-up on educational outcomes

	Reduced form (1)	Ineligible complier mean (2)	2SLS (3)	OLS (4)
First stage			0.083***	
			(0.008)	
Panel A. Ever enrollment				
Any institution	0.004	0.953	0.047	0.046***
	(0.00)		(0.04)	(0.00)
University	0.068***	0.175	0.823***	0.347***
	(0.01)		(0.09)	(0.00)
Vocational	-0.058***	0.999	-0.705***	-0.222***
	(0.01)		(0.10)	(0.00)
Panel B. Years of enrollment				
Any institution	0.177***	3.484	2.141***	1.125***
	(0.04)		(0.41)	(0.01)
University	0.426***	0.000	5.140***	2.151***
	(0.05)		(0.52)	(0.01)
Vocational	-0.249***	3.650	-3.000***	-1.026***
	(0.04)		(0.43)	(0.01)
Panel C. Graduation and number of institutions				
Overall graduation	-0.021**	0.646	-0.248**	-0.043***
	(0.01)		(0.11)	(0.00)
Graduation university	0.033***	0.004	0.396***	0.118***
	(0.01)		(0.08)	(0.00)
Graduation vocational	-0.053***	0.642	-0.645***	-0.161***
	(0.01)		(0.10)	(0.00)
Number of institutions	0.038***	1.158	0.460***	0.110***
attended	(0.01)		(0.12)	(0.00)
Observations		53,416		177,470

Notes: This table presents university loan take-up effects on ever enrollment, years of education, and graduation from different types of institutions. The first row reports first-stage effects of initial university loan eligibility on university loan take-up (F-test of 107.64). Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed in our RD sample, restricting to observations in a 40-point bandwidth of the eligibility cutoff. Column (4) uses the whole analysis sample. Out of range complier means are replaced by bound values (0 or 1). Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 6: Effect of loan take-up on total debt and labor market outcomes

	Reduced form (1)	Ineligible complier mean (2)	2SLS (3)	OLS (4)
First stage		0.083***		
		(0.008)		
Panel A. Total debt and cost of degree (thousand dollars)				
Total debt all institutions	1.2***	3.00	14.3***	13.8***
	(0.13)		(1.16)	(0.04)
Debt university loan	1.4***	0.00	17.0***	14.7***
	(0.13)		(0.94)	(0.04)
Debt at vocational loan	-0.2***	3.00	-2.7***	-1.0***
	(0.07)		(0.82)	(0.01)
Tuition	0.1**	2.0	1.4**	0.8**
	(0.02)		(0.20)	(0.01)
Panel B. Labor market outcomes from UI data				
Monthly wage (dollars)	-4.5	924.50	-54.8	-35.1***
	(13.72)		(167.08)	(5.36)
Probability of employment	0.0	0.56	-0.1	-0.1***
	(0.01)		(0.09)	(0.00)
Probability of fixed-term job	0.0	0.41	-0.1	0.1***
	(0.01)		(0.12)	(0.00)
Probability of part-time job	0.0	0.10	0.0	0.0***
	(0.00)		(0.05)	(0.00)
Years of experience	-0.1**	3.91	-1.2**	-0.6***
	(0.04)		(0.49)	(0.01)
Average firm wage	10.3	1012.77	125.7	-67.9***
	(15.47)		(189.88)	(5.31)
Panel C. Labor market outcomes from pension system data				
Monthly wage (dollars)	-10.4	840.71	-121.8	-54.2***
	(11.52)		(134.78)	(4.74)
Probability of employment	0.0	0.51	-0.1	-0.1***
	(0.01)		(0.10)	(0.00)
Public sector worker	0.0	0.34	0.0	0.1***
	(0.01)		(0.10)	(0.00)
Observations		53,416		177,470

Notes: This table presents university loan take-up effects on debt and labor market outcomes. The first row reports first-stage effects of initial university loan eligibility on university loan take-up (F-test of 107.64). Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed in our RD sample, restricting to observations in a 40-point (0.36 standard deviations) bandwidth of the eligibility cutoff. Column (4) uses the whole analysis sample. The total number of observations for monthly wage (excluding zeros) in panel B are 33,484 in columns (1)-(3) and 104,279 in column (4). Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 7: Fallback and Destination Institutions

	All applicants		Applicants in bandwidth		Compliers		
	Eligible (1)	Ineligible (2)	Eligible (3)	Ineligible (4)	Eligible (5)	Ineligible (6)	Difference (7)
Panel A. Years of education							
University							
Tier 1	1.6	0.1	0.3	0.1	-0.2	-0.1	-0.02
							(0.21)
Tier 2	2.7	0.6	2.0	0.9	2.2	-0.7	2.87***
							(0.51)
Tier 3	0.8	0.8	1.3	1.1	3.0	0.3	2.64***
							(0.43)
Tier 4	0.1	0.2	0.1	0.2	0.0	0.4	-0.42***
							(0.15)
Vocational							
Top	0.8	2.3	1.5	2.1	0.5	3.3	-2.79***
							(0.42)
Bottom	0.1	0.5	0.2	0.3	0.2	0.5	-0.36*
							(0.19)
Panel B. Institution characteristic							
Years accredited	0.2	-0.3	-0.2	-0.2	-0.8	-0.2	-0.61***
							(0.20)

Notes: This table presents average years of schooling by university tier and vocational institution and shows average characteristics of the first institution attended by students. These are shown by eligibility status in the full sample in columns (1) and (2) and among compliers in columns (3) and (4). Complier characteristics are computed following Abadie (2002). Column (5) reports the difference in means between columns (3) and (4), which corresponds to 2SLS estimates. Accreditation in panel B corresponds to the first institution where the student enrolled and it is normalized with respect to the mean accreditation of the corresponding program, i.e. university or vocational. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 8: Heterogenous effects of loan take-up

	Predicted Acreeditation at Top Quartile		Diff-in-Diff Coefficient
	Yes	No	
	(1)	(2)	(3)
Panel A. Debt, tuition, graduation and institutions attended			
Total debt all institutions	16.5***	14.2***	2.4
	(3.16)	(1.31)	(3.42)
Debt university loan	18.6***	15.9***	2.7
	(2.62)	(1.02)	(2.81)
Debt vocational loan	-2.1	-1.7*	-0.4
	(2.06)	(0.94)	(2.27)
Tuition	1.7***	1.3***	0.4
	(0.48)	(0.22)	(0.53)
Overall graduation	-0.2	-0.3**	0.0
	(0.26)	(0.12)	(0.29)
Graduation university	0.5**	0.3***	0.2
	(0.22)	(0.09)	(0.24)
Graduation vocational	-0.8***	-0.6***	-0.2
	(0.25)	(0.11)	(0.27)
Number of institutions	-0.2	0.5***	-0.7**
attended	(0.26)	(0.13)	(0.29)
Panel B. Labor market outcomes from UI data			
Monthly wage (dollars)	243.5	-142.0	385.6
	(354.07)	(190.31)	(401.97)
Probability of employment	-0.2	0.0	-0.2
	(0.23)	(0.11)	(0.25)
Probability of fixed-term job	0.1	0.0	0.1
	(0.23)	(0.13)	(0.26)
Probability of part-time job	-0.2	0.0	-0.2
	(0.14)	(0.06)	(0.15)
Years of experience	-1.2	-1.1**	-0.1
	(1.22)	(0.56)	(1.34)
Average firm wage	984.9**	-132.9	1,117.8**
	(453.55)	(216.81)	(502.71)
Panel C. Labor market outcomes from pension system data			
Monthly wage (dollars)	892.2	-282.4**	1,174.7
	(972.13)	(133.44)	(981.25)
Probability of employment	0.0	-0.1	0.1
	(0.24)	(0.11)	(0.26)
Public sector worker	0.5*	-0.1	0.5*
	(0.26)	(0.12)	(0.28)

Notes: This table presents university loan take-up effects for students taking-up the loan to enroll at top quartile universities versus the rest of university loan-takers. Column (1) presents quasi-experimental estimates for students in top quartile universities in terms of accreditation. Column (2) presents the results for students in enrolling at universities with accreditation below the 75 percentile. Column (3) shows the differential effect between these groups. For the quartile classification, we consider the predicted ex-ante accreditation of the receiving institution, using the predetermined average accreditation of universities around a student high school (15 kilometer radius) as a predictor. We restrict the sample to students for whom there is at least one university around the 15 kilometer radius around their high school. Given this sample restriction, the regression includes 42,961 observations within the 40-points bandwidth. Finally, a 35% of the 10,261 students in the top quartile actually take-up the university loan. Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 9: Extrapolation: Reduced form, first-stage, and Wald-type IV

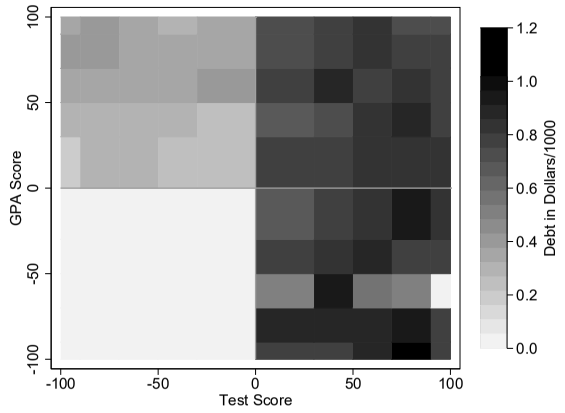
Distance from cutoff	First stage	Wald-type IV estimates		
		Wages	Vocational graduation	University graduation
	(1)	(2)	(3)	(4)
Below:415-425	0.199***	-1.212	-0.687**	0.333***
	(0.05)	(363.13)	(0.33)	(0.12)
Below:425-435	0.196***	11.453	-0.654***	0.381***
	(0.04)	(254.43)	(0.20)	(0.10)
Below:435-445	0.191***	21.146	-0.624***	0.424***
	(0.03)	(183.76)	(0.15)	(0.08)
Below:445-455	0.187***	29.257	-0.613***	0.470***
	(0.02)	(138.49)	(0.10)	(0.06)
Below:455-465	0.182***	16.871	-0.612***	0.510***
	(0.02)	(109.90)	(0.07)	(0.05)
Above:485-495	0.171***	151.020**	-0.694***	0.725***
	(0.01)	(74.70)	(0.05)	(0.05)
Above:495-505	0.164***	180.551**	-0.741***	0.813***
	(0.01)	(91.14)	(0.06)	(0.06)
Above:505-515	0.166***	255.243**	-0.802***	0.870***
	(0.01)	(100.17)	(0.07)	(0.07)
Above:515-525	0.161***	304.375**	-0.874***	0.951***
	(0.01)	(135.96)	(0.09)	(0.10)
Above:525-535	0.152***	426.495**	-0.962***	1.064***
	(0.01)	(173.58)	(0.12)	(0.12)

Notes: This table presents first-stage, and Wald-type IV estimates following Angrist and Rokkanen (2015). Each row shows estimates for the 10-unit binwidth indicated in the first column. Wages corresponds to the monthly wage. Standard errors (shown in parentheses) were computed using a nonparametric bootstrap with 500 replications. *significant at 10%; **significant at 5%; ***significant at 1%.

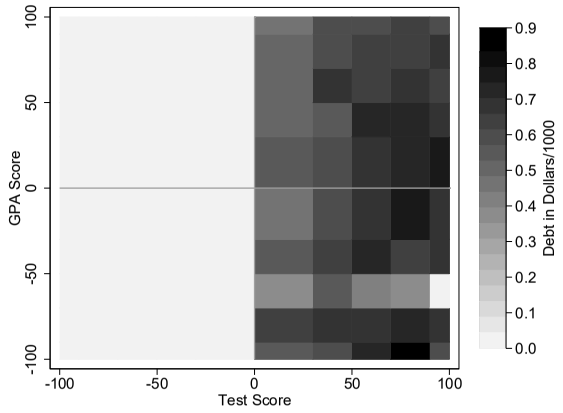
Figures

Figure 1: First-year amount borrowed using the SGL by type of institution

Panel A: Amount borrowed at any institution



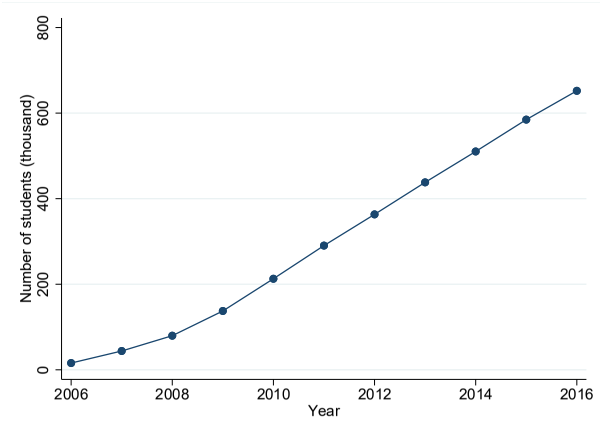
Panel B: Amount borrowed at university



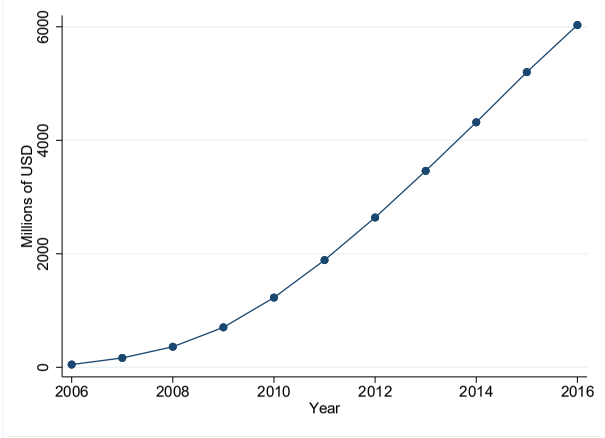
Notes: These figures present the first-year total amount borrowed by students with different average Math and Language test scores centered on the eligibility cutoff (475) on the x-axis, and with different GPA scores also centered on the eligibility cutoff. Panel A shows the total first-year debt at any institution and Panel B restricts this debt to universities.

Figure 2: Total number of students with a student loan and total accumulated debt

Panel A: Number of students who ever held a SGL



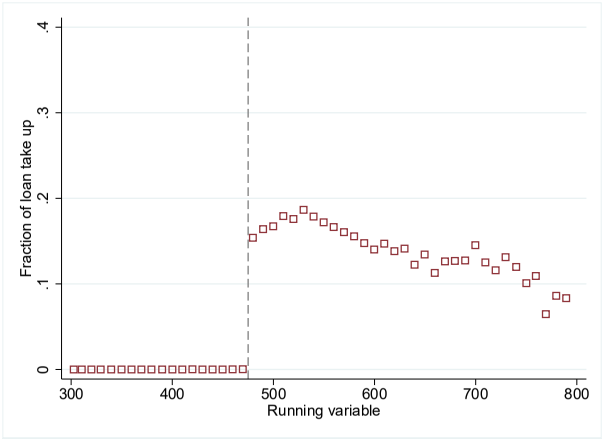
Panel B: Accumulated debt in the SGL



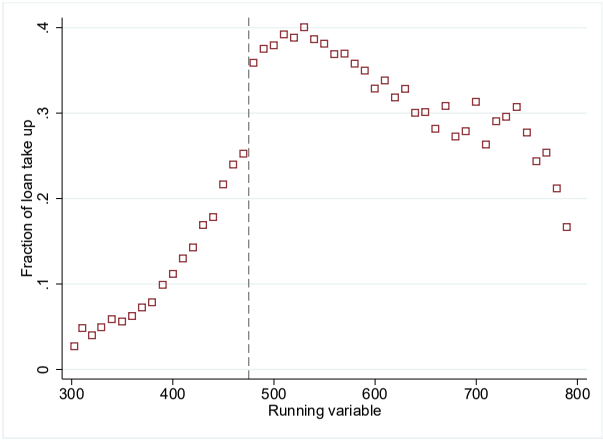
Notes: These figures show the total number of students who ever took-up a State Guaranteed Loan (SGL) in Panel A and the accumulated student debt in the SGL in Panel B, by year.

Figure 3: University loan take-up

Panel A: Take-up of university loans in the first year after high school graduation

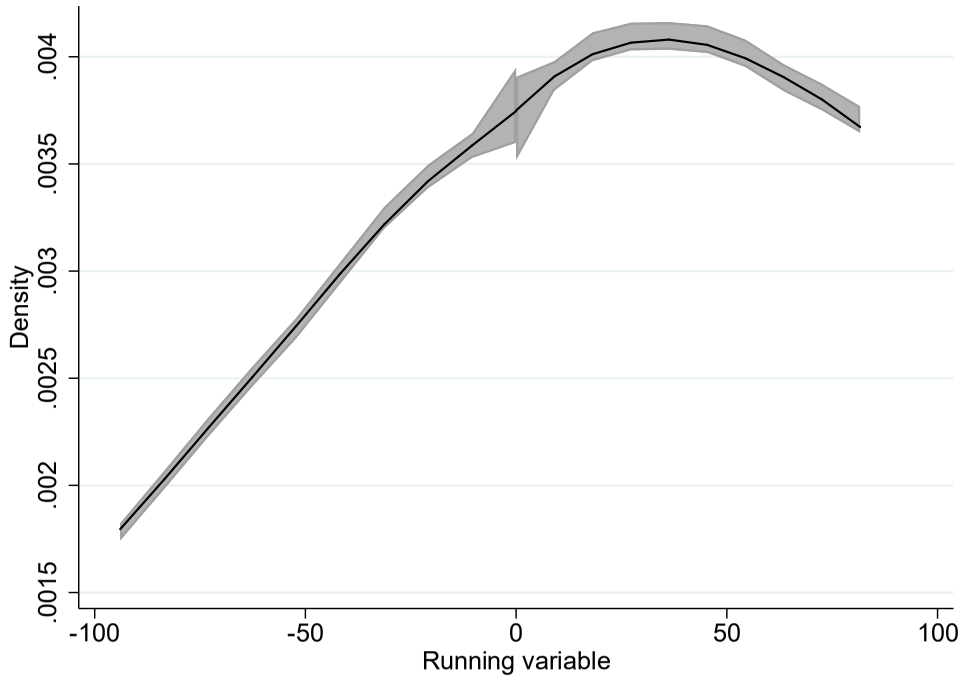


Panel B: Ever taking up university loan up to 9 years after high school graduation



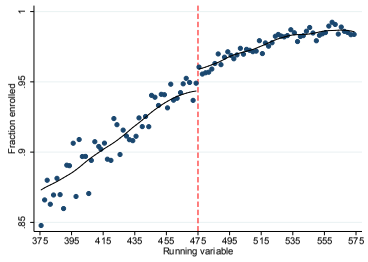
Notes: These figures plot the fraction of students who take-up a university loan as a function of the running variable by initial financial aid application status. Each dot shows a conditional mean in a 10-points binwidth of the running variable. Panel A shows take-up in the year just after high school graduation and panel B shows take-up at any point up to nine years after high school graduation.

Figure 4: Density of running variable

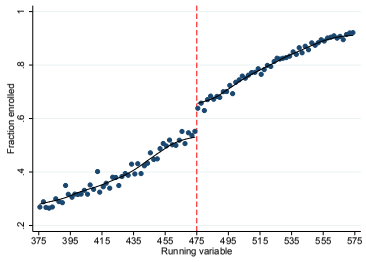


Notes: This figure presents estimates of the density of the running variable on both sides of the eligibility cutoff with their confidence interval in grey. This plot was produced using the rddensity command of Cattaneo et al. (2016).

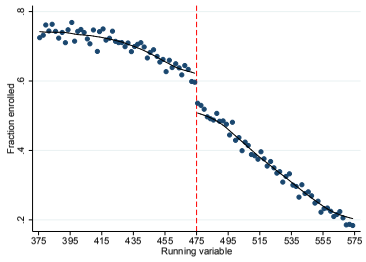
Figure 5: Ever enrollment rates, by type of institution



Panel A: Any type of institution



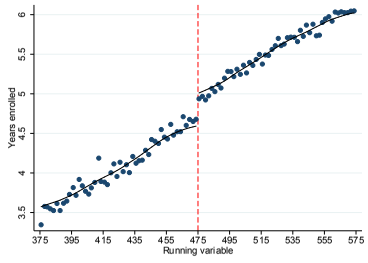
Panel B: University



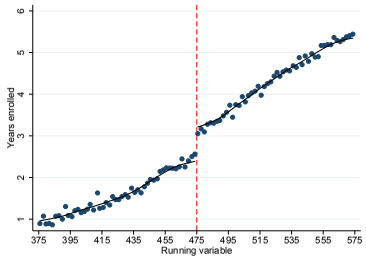
Panel C: Vocational

Notes: These figures show the average fraction of students enrolled in a 2-points bandwidth of the running variable at different types of institutions. The dotted vertical line shows the eligibility cutoff for a first-year university loan, and the solid lines are local linear fits using a rule-of-thumb bandwidth and an Epanechnikov kernel. The bandwidth is the plugin estimator of the asymptotically optimal constant bandwidth (Fan and Gijbels, 1996).

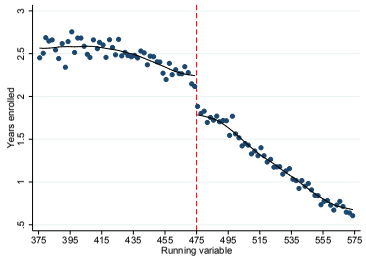
Figure 6: Years of enrollment, by type of institution



Panel A: Any type of institution



Panel B: University

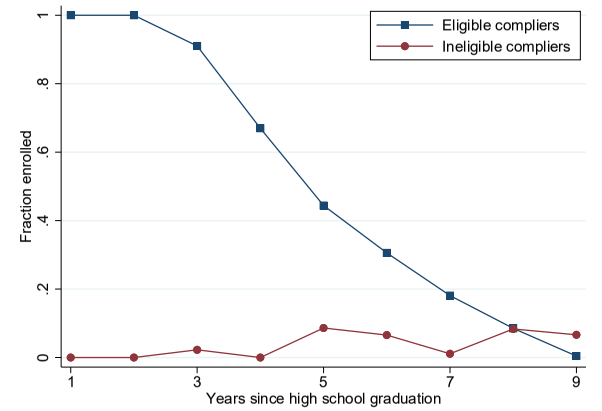


Panel C: Vocational

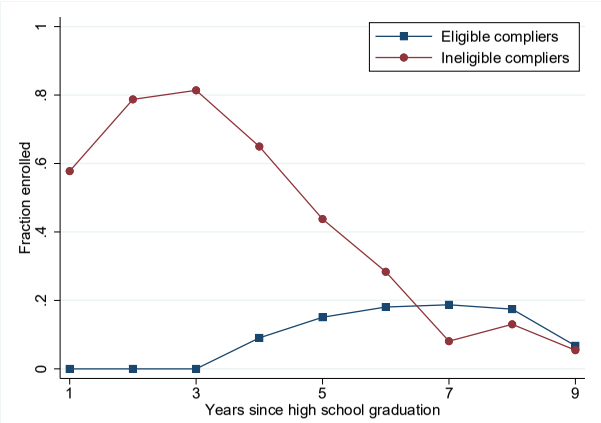
Notes: These figures show the average years of education in a 2-points bandwidth of the running variable at different types of institutions. The dotted vertical line shows the eligibility cutoff for a first-year university loan, and the solid lines are local linear fits using a rule-of-thumb bandwidth and an Epanechnikov kernel. The bandwidth is the plugin estimator of the asymptotically optimal constant bandwidth (Fan and Gijbels, 1996).

Figure 7: Fraction of compliers enrolled, by year and initial eligibility status

Panel A: University



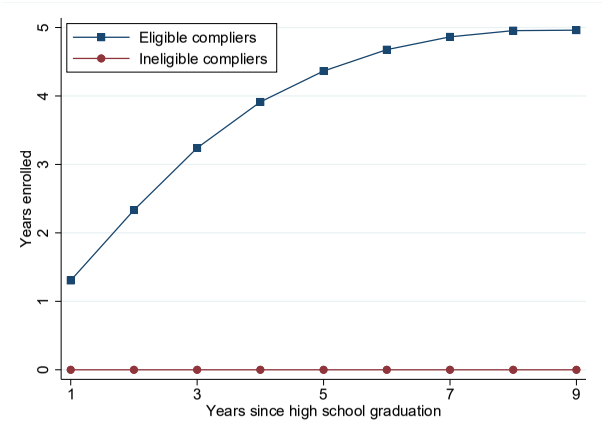
Panel B: Vocational institutions



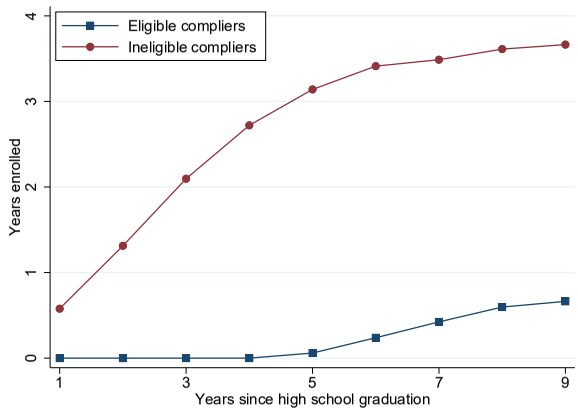
Notes: These figures present the fraction of complier students enrolled by year (after high school graduation) and by initial university loan eligibility. Panel A shows the enrollment at universities and Panel B shows enrollment at vocational institutions. Each dot is the complier mean obtained using the method in Abadie (2002). We replace the values by 0 or 1 when they are out of bound.

Figure 8: Years of enrollment among complier students, by year and initial eligibility

Panel A: University

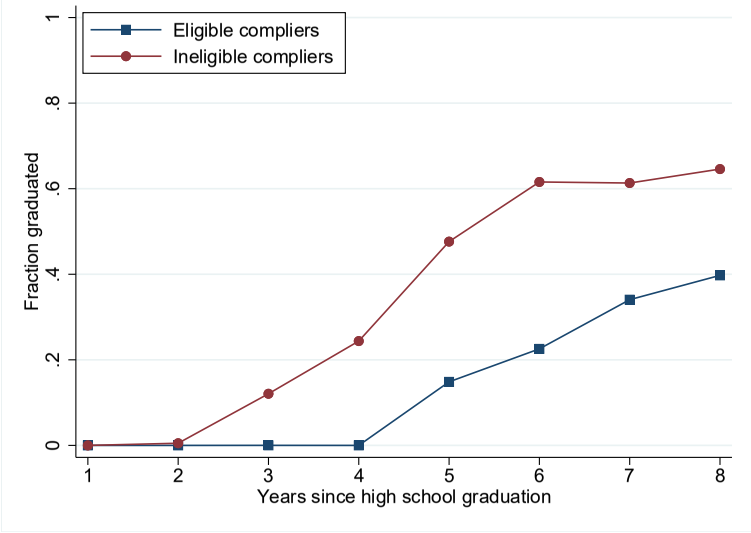


Panel B: Vocational institutions



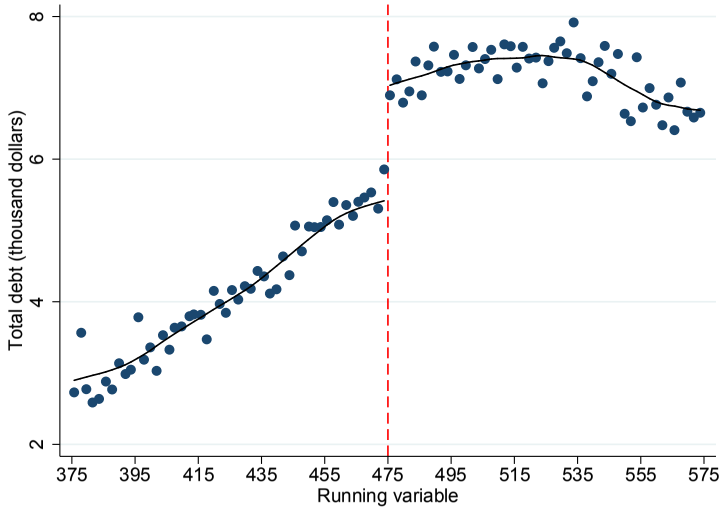
Notes: These figures present the total number of years of enrollment among complier students by year (after high school graduation) and by initial university loan eligibility. Panel A shows results for enrollment at universities and Panel B at vocational institutions. Each dot is the complier mean obtained using the method in Abadie (2002). We replace the values by 0 or 1 when they are out of bound.

Figure 9: Fraction of students graduated with a degree at any type of institution, by year and initial eligibility



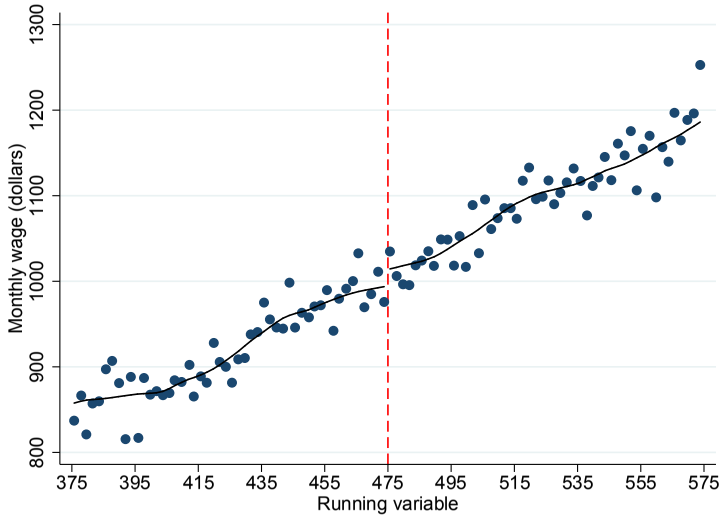
Notes: This figure presents the average fraction graduated from any type of institution among complier students by year (after high school graduation) and by initial university loan eligibility. Each dot is the complier mean obtained using the method in Abadie (2002). We replace the values by 0 or 1 when they are out of bound.

Figure 10: Total SGL debt at any institution



Notes: This figure presents the total average student debt in State Guaranteed Loans accumulated over 9 years. Each dot is the average debt in a 2-point bandwidth of the running variable. The dotted vertical line shows the eligibility cutoff for a first-year university loan, and the solid lines are local linear fits using a rule-of-thumb bandwidth and an Epanechnikov kernel. The bandwidth is the plugin estimator of the asymptotically optimal constant bandwidth (Fan and Gijbels, 1996).

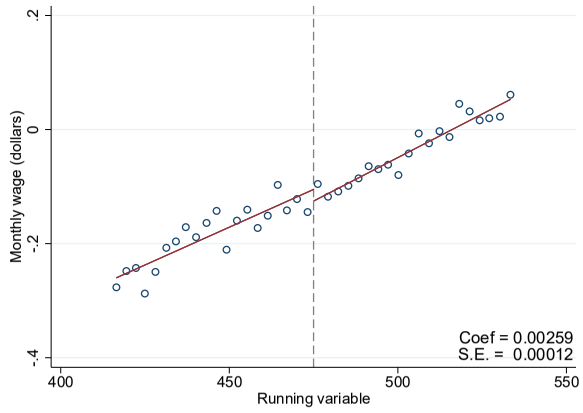
Figure 11: Average wage (excluding 0s) by year 9 out of high school



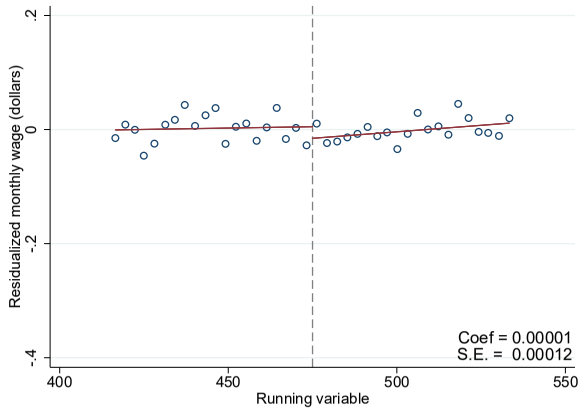
Notes: This figure presents the average monthly wage in dollars in 2-points binwidth of the running variable. The dotted red line shows the eligibility cutoff for a first-year university loan, and the solid black lines are local linear fits using a rule-of-thumb bandwidth and an Epanechnikov kernel. The bandwidth is the plugin estimator of the asymptotically optimal constant bandwidth (Fan and Gijbels, 1996).

Figure 12: Visual test of the conditional independence assumption (CIA)

Panel A: Monthly wages (dollars)

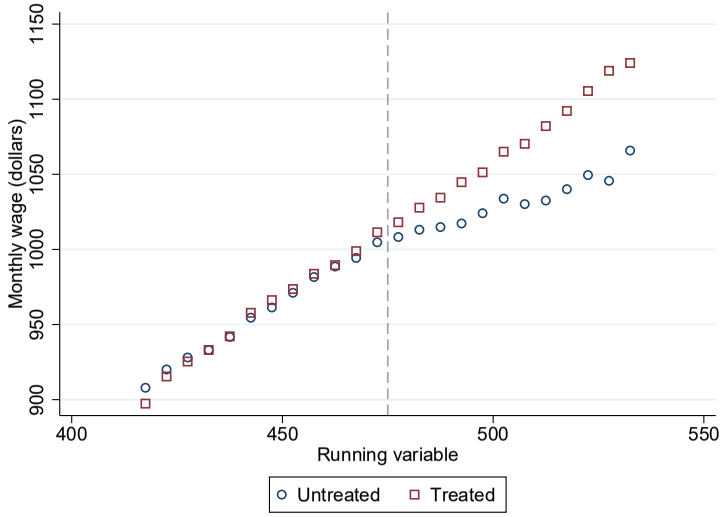


Panel B: Residualized monthly wages (dollars)



Notes: These figures present mean wages in a three-unit binwidth of the running variable. Panel A shows mean wages against the running variable. Panel B shows means of wage residuals (obtained from a regression of wages on other test scores, GPA, and controls) against the running variable.

Figure 13: CIA-based estimates of $E[Y_{1i}|r_i = c]$ and $E[Y_{0i}|r_i = c]$ for$c$'s in the window $[-60, 60]$



Notes: This figure presents CIA-based estimates of the potential outcomes at different 5-unit binwidths of the running variable, for a window of 60 points around the eligibility cutoff. Each point is computed using the propensity score weighted estimator presented in section 7.

Appendix: Additional Tables and Figures

Table 1: Accredited Institutions over time

Year	University	Vocational institutions
2004	14	2
2005	30	11
2006	38	16
2007	43	21
2008	45	21
2009	45	21
2010	47	25
2011	51	27
2012	50	28
2013	48	36
2014	45	37
2015	44	38
2016	45	36

Notes: The total numbers between 2004 and 2006 come from World Bank (2011). Numbers between 2007 and 2016 are constructed using data from SIES MINEDUC.

Table 2: Effect of loan take up on educational outcomes. Robust bandwidth

		Ineligible			Bandwidth	
	Reduced form	complier mean	2SLS	OLS	Reduced form	Fuzzy RD
	(1)	(2)	(3)	(4)	(5)	(6)
First stage		0.083***				
		(0.008)				
Panel A. Ever enrollment						
Any institution	0.005	0.963	0.075	0.046***	71.0	46.7
	(0.00)		(0.05)	(0.00)		
University	0.071***	0.159	0.829***	0.347***	39.7	52.3
	(0.01)		(0.10)	(0.00)		
Vocational	-0.063***	1.016	-0.751***	-0.222***	42.1	44.0
	(0.01)		(0.12)	(0.00)		
Panel B. Years of enrollment						
Any institution	0.173***	3.582	2.124***	1.125***	48.9	47.9
	(0.04)		(0.46)	(0.01)		
University	0.463***	0.000	5.345***	2.151***	41.6	57.2
	(0.06)		(0.54)	(0.01)		
Vocational	-0.279***	3.705	-3.394***	-1.026***	46.0	44.5
	(0.04)		(0.53)	(0.01)		
Panel C. Graduation and number of institutions						
Overall graduation	-0.016*	0.642	-0.171	-0.043***	69.6	46.7
	(0.01)		(0.12)	(0.00)		
Graduation university	0.036***	0.009	0.444***	0.118***	49.9	50.4
	(0.01)		(0.09)	(0.00)		
Graduation vocational	-0.052***	0.651	-0.615***	-0.161***	48.9	44.6
	(0.01)		(0.11)	(0.00)		
Number of institutions	0.037***	1.179	0.476***	0.110***	53.2	45.4
attended	(0.01)		(0.13)	(0.00)		
Observations		53,416		177,470		

Notes: This table presents university loan take up effects on ever enrollment and years of education in different types of institutions. Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by fuzzy regression discontinuity, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed using the optimal bandwidth in Calonico et al. (2014) presented in columns (5) and (6). Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 3: Effect of loan take up on debt and labor market outcomes. Robust bandwidth

		Untreated			Bandwidth	
	Reduced form	complier mean	2SLS	OLS	Reduced form	Fuzzy RD
	(1)	(2)	(3)	(4)	(5)	(6)
First stage		0.1***				
		(0.01)				
Panel A. Total debt (thousand dollars)						
Total debt all institutions	1.2***	2.9	14.0***	13.8***	42.4	45.9
	(0.16)		(1.33)	(0.04)		
Debt university loan	1.5***	0.0	17.3***	14.7***	40.5	41.9
	(0.16)		(1.10)	(0.04)		
Debt at vocational loan	-0.3***	3.0	-3.2***	-1.0***	56.4	48.3
	(0.07)		(0.90)	(0.01)		
Tuition	0.1***	2.0	1.5***	0.8***	44.5	53.1
	(0.02)		(0.21)	(0.01)		
Panel B. Labor market outcomes from UI data						
Monthly wage (dollars)	0.5	1,056.9	3.4	-35.1***	54.9	52.7
	(15.26)		(173.98)	(5.36)		
Probability of employment	0.0*	0.6	-0.2	-0.1***	62.6	47.1
	(0.01)		(0.11)	(0.00)		
Probability of fixed-term job	0.0	0.3	-0.1	0.1***	37.2	45.6
	(0.01)		(0.13)	(0.00)		
Probability of part-time job	0.0	0.1	0.0	0.0***	66.2	46.8
	(0.00)		(0.06)	(0.00)		
Years of experience	-0.1***	4.0	-1.6***	-0.6***	53.9	47.2
	(0.05)		(0.57)	(0.01)		
Average firm wage	31.9*	1,168.1	301.1	-67.9***	43.3	51.0
	(18.76)		(203.65)	(5.31)		
Panel C. Labor market outcomes from pension system data						
Monthly wage (dollars)	-4.8	856.3	-56.7	-54.2***	58.4	52.4
	(12.56)		(145.64)	(4.74)		
Probability of employment	0.0	0.6	0.0	-0.1***	64.1	45.6
	(0.01)		(0.11)	(0.00)		
Public sector worker	0.0	0.3	0.1	0.1***	56.4	47.5
	(0.01)		(0.11)	(0.00)		
Observations		53,416				

Notes: This table presents university loan take up effects on debt and labor market outcomes. Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are estimated by fuzzy regression discontinuity, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed using the optimal bandwidth in Calonico et al. (2014) presented in columns (5) and (6). Column (4) is using the whole analysis sample. Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

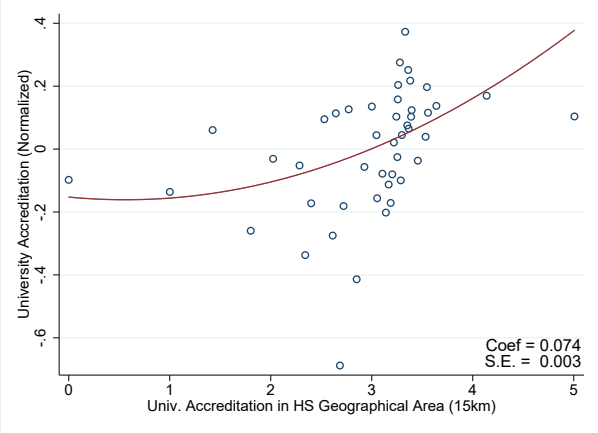
Table 4: Testing the conditional independence assumption (CIA)

	+- 10 points		+- 20 points		+- 30 points		+- 40 points		+- 50 points		+- 60 points		+- 70 points		+- 80 points		+- 90 points	
	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
Years enrolled	0.002	0.005	0.007**	0.011***	0.006***	0.009***	0.010***	0.007***	0.011***	0.006***	0.010***	0.006***	0.010***	0.006***	0.010***	0.005***	0.010***	0.005***
	(0.011)	(0.009)	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Graduation university	0.000	0.000	0.001	0.001*	0.001***	0.002***	0.001***	0.002***	0.001***	0.002***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Graduation vocational	0.000	-0.003*	-0.001	-0.001	-0.001***	-0.002***	-0.001***	-0.002***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Monthly wage (dollars)	-2.571	-2.778	-0.600	0.382	-0.309	-0.134	-0.517	0.296	-0.066	0.379	0.280	0.053	0.497**	-0.246	0.487**	-0.337	0.256	-0.489**
	(3.728)	(3.509)	(1.278)	(1.179)	(0.696)	(0.678)	(0.461)	(0.473)	(0.348)	(0.359)	(0.276)	(0.287)	(0.228)	(0.245)	(0.198)	(0.217)	(0.178)	(0.196)
Probability of employment	-0.001	-0.002	-0.001	0.001	0.000	0.000	-0.001***	-0.001**	0.000*	-0.001***	0.000***	-0.001***	0.000***	-0.001***	0.000***	-0.001***	-0.001***	-0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

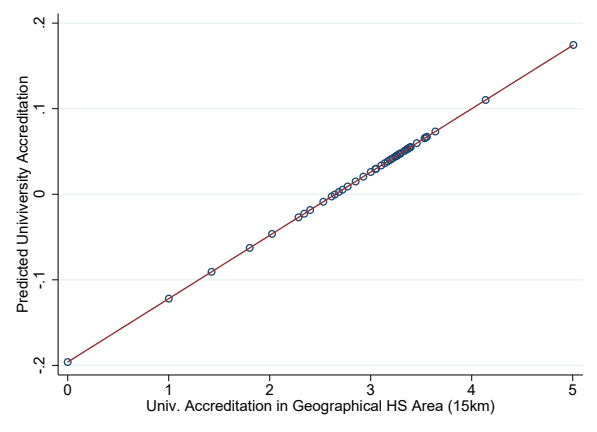
Notes: This table presents the regression-based CIA test proposed by Angrist and Rokkanen (2015). Each row presents a test for a different dependent variable and the columns indicate the window below and above the university loan eligibility cutoff used for the test. Each cell entry corresponds to the coefficient on the running variable in a regression of the respective dependent variable against Math, the maximum between History and Science, GPA, and the running variable. Each regression uses data below or above the cutoff, according to the description of the column, up to the number of points reported in the column title. Robust standard errors are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Figure 1: Predicted ex-ante accreditation of the receiving institution

Panel A: Pseudo first-stage

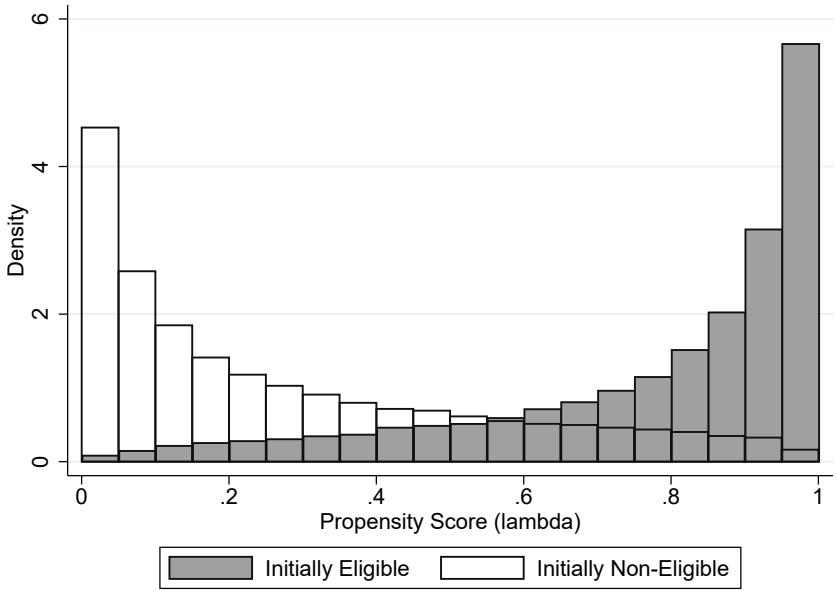


Panel B: Prediction used for quartile classification



Notes: Panel A presents the relationship between the average accreditation of universities around a student's high school and the accreditation of the actual receiving univerisity. Panel B shows the prediction fit used to classify an institution into the top quartile.

Figure 2: Common Support



Notes: This figure presents the overlap between the propensity score distribution for initially eligible and initially non-eligible students.

Online Appendix
Labor Market Returns to Student Loans for University: Evidence from Chile

Alonso Bucarey Dante Contreras Pablo Muñoz

June, 2019

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1 Replication and extension of Solis (2017)

In this appendix we present an analysis similar to that in Solis (2017). That paper concluded that the gap of enrollment between top and bottom income quintiles closed thanks to the SGL. The paper had access to enrollment up to two years after high school graduation. Our goal in this Appendix is to determine whether increasing the number of years considered in Solis (2017) from 2 years after high school graduation to 8 years changes the main conclusion. The objects of interest are two: enrollment at any type of higher education and enrollment at universities. We estimate the effect on both outcomes following the strategy presented in Solis (2017). Specifically, we consider the following specification:

$$\begin{aligned} \textit{Ever Loan Eligibility}_{it} &= \alpha + \beta 1(r_i > 0) + f(r_i;\theta) + \epsilon_{it} \\ \textit{Ever Enrolled}_{it} &= \gamma + \rho \textit{Ever Loan Eligibility}_{it} + g(r_i;\delta) + \nu_{it} \ , \end{aligned}$$

where $\textit{Ever Loan Eligibility}_{it}$ and $\textit{Ever Enrolled}_{it}$ are measured at time $t = 1, \ldots, 8$ years post high school graduation, r_i is the running variable with $1(r_i > 0)$ determining initial loan eligibility, and $f(\cdot)$ and $g(\cdot)$ are linear functions of r_i that we fit on both sides of the eligibility cutoff. We estimate this model by 2SLS for students in the lowest and highest income quintiles, and we compute the mean enrollment among ineligible compliers following Abadie (2002). Tables 5 and 6 present the results of this analysis. Column (1) shows the enrollment rate of students who belong to the first income quintile and are ineligible for the loan. Column (2) shows the same number for students in the fifth income quintile. Columns (1) and (2) allow us to construct the enrollment gap for marginally ineligible students for each time window. This gap is presented in column (3). Column (4) shows the effect of the loan in enrollment for students in the first quintile, which added to the ineligible mean corresponds to the enrollment rate above the eligibility cutoff. Column (5) does that for the fifth income quintile, and together these estimates allow us to compute the gap for treated students, which is presented in column (6). Each row indicates the year after high school graduation that we use in estimation.

Regarding enrollment at any type of institution, our main finding is that access to the loan closes the gap only in the first year after graduating from high school, but by the second year the enrollment gap returns to its original level. A driving force in closing this gap is the enrollment of students without initial access to the loan, which over time erodes the large enrollment effect that the loan had during the first year.

In terms of the university enrollment gap, we find that the loan reduces the gap by 0.12, from 0.197 to 0.08, in the first year. However, this effect also dissipates over time. By year two after high school, the difference in the gap between those without access to the loan and those with access to it, is only 0.05. This effect seems to be driven by the decrease in enrollment among students from the first income quintile.

In summary, our analysis suggests that accounting for vocational alternatives and extending the time horizon qualifies the main conclusions in Solis (2017) regarding the effects of the loan on equalizing access to tertiary education for marginal students.

2 The role of labor market experience

2.1 Mincer estimation

We have shown that loan take-up increases years of schooling and reduces labor market experience.[1] A natural question that arises from this finding is whether the cost of losing experience is large enough to offset the benefits of increasing schooling. In an effort to address this issue, we estimate the following variant of the Mincer model using our analysis sample:

$$W_i = \beta_0 + \rho_{uni}\text{Years university}_i + \rho_{voc}\text{Years vocational}_i + \beta_1 X_i + \beta_2 X_i^2 + u_i\,, \tag{1}$$

where W_i is the annual average of monthly wages, Years university$_i$ and Years vocational$_i$ are the number of years of enrollment at a university and vocational institutions respectively, and X_i stands for the labor market experience observed in the UI administrative data; all variables measured by year nine out of high school.

Mincer estimates, in Table 7, show statistically significant returns on experience and education. The linear term on experience in column (1) shows a return of 94 dollars to an extra year of experience. This implies a wage increase of 9% per year of experience (the average monthly wage in our sample is 1,011 USD). Regarding the wage return to higher education, we observe it varies by type of institution with a return of 48 dollars per year of enrollment in university (a 4.1% rate of return) and a return of 30 dollars per year of enrollment at a vocational institution (a 2.5% rate of return per year).

Consistent with our main findings, a back-of-the-envelope calculation based on previous estimates suggests that nine years after high school graduation, there is a zero net return to taking up a university loan. To arrive to this conclusion, we plugged in our quasi-experimental estimates from the previous section[2] to estimate: $\frac{dW_i}{d\text{Years university}_i} - \frac{dW_i}{d\text{Years vocational}_i}$, the differential effect of university education on wages relative to the effect of vocational education on wages. We find that experience lost completely offsets the extra gains from attending a university (instead of a vocational institution), leading to a null effect on wages. Our point estimate for the differential effect is -6.8 dollars.[3]

In summary, this exercise suggests that the zero effect on wages comes from both the cost of losing experience and the small return to university, relative to vocational, degrees.

2.2 Additional details

For simplicity, in this section we re-write the variables Years university$_i$ and Years vocational$_i$ presented in equation 1 as Uni$_i$ and Voc$_i$. As previously discussed, we are interested in the differential effect of years of university education (relative to years of vocational education) on wages. Thus, we need to evaluate the following object that accounts for both the cost of experience loss and the returns to years of enrollment at each type of institution:

[1] A result that is consistent with classical models for the effects of education on earnings. See Card (1999) for a review of these models.

[2] University loan take-up induced compliers to attend 5.1 years of university schooling while giving up 3 years of vocational education. It also decreased labor market experience by 1.2 years.

[3] We use delta method to compute the standard error (6.7 dollars), see the next section for more details.

$$\frac{\Delta W_i}{\Delta Uni_i} - \frac{\Delta W_i}{\Delta Voc_i} = \rho_{uni} \times \Delta Uni_i - \rho_{voc} \times \Delta Voc_i + (\beta_1 + 2\beta_2 X_i) \times \left(\frac{\Delta X_i}{\Delta Uni_i} - \frac{\Delta X_i}{\Delta Voc_i} \right)$$

We also define experience as: $X_i = X_{0i} - \delta \times I(Uni_i > 0)$, such that:

$$\frac{\Delta W_i}{\Delta Uni_i} - \frac{\Delta W_i}{\Delta Voc_i} = \rho_{uni} \times \Delta Uni_i - \rho_{voc} \times \Delta Voc_i - (\beta_1 + 2\beta_2 X_i) \times \delta$$

Plugging-in our causal estimates of ΔUni_i and ΔVoc_i presented in the main text, and using the average years of labor market experience in our sample (2.8 years), we obtain:

$$\frac{\Delta W_i}{\Delta Uni_i} - \frac{\Delta W_i}{\Delta Voc_i} = g(\rho_{uni}, \rho_{voc}, \beta_1, \beta_2) = \rho_{uni} \times 5.1 - \rho_{voc} \times 3 - (\beta_1 + 5.6\beta_2) \times 1.2$$

Finally, using estimates from the mincer equation and the fact that $g'() = (5.1, -3, -1.2, -6.7)$, we can compute both the $E\left[g(\cdot)\right]$ and $V\left[g(\cdot)\right]$:

$$E\left[\frac{\Delta W_i}{\Delta Uni_i} - \frac{\Delta W_i}{\Delta Voc_i} \right] = -6.8$$

$$V\left[\frac{\Delta W_i}{\Delta Uni_i} - \frac{\Delta W_i}{\Delta Voc_i} \right] = 45.11$$

where the variance is obtained using delta method, with $V\left[g()\right] = g()'^T V g'()$ equal to:

$$V = \begin{pmatrix} 3.75 & & & \\ 3.02 & 4.85 & & \\ 1.96 & 0.70 & 33.61 & \\ -0.06 & 0.03 & -3.57 & 0.42 \end{pmatrix}$$

3 Assumptions for extrapolation

Let X_i be a set of covariates such that the following assumptions are satisfied:

1. Generalized conditional independence assumption (GCIA): $(Y_{0i}, Y_{1i}, W_{0i}, Y_{1i}) \perp r_i | X_i$
2. Common support: $0 < P[D_i|X_i] < 1$ $a.s.$
3. Conditional first stage: $P[W_{1i}|X_i] > P[W_{0i}|X_i]$ $a.s.$

4. Monotonicity: $W_{1i} \geq W_{0i}$ for all i.

Angrist and Rokkanen (2015) prove that the LATE estimand in this case corresponds to:

$$E[Y_{1i} - Y_{0i} | W_{1i} > W_{0i}, c_0 < r_i < c_1] = \frac{E[E[y_i|X_i, D_i = 1] - E[y_i|X_i, D_i = 0]|c_0 < r_i < c_1]}{E[E[W_i|X_i, D_i = 1] - E[W_i|X_i, D_i = 0]|c_0 < r_i < c_1]}$$

4 Potential violations of the exclusion restriction

This Appendix discusses potential violations of the exclusion restriction in our main empirical strategy. First, we check the robustness of our results to a different definition of treatment: *immediate take-up of a university SGL* (instead of *ever take-up of a university SGL*), for which our instrument produces a larger first stage. Second, we study how crossing the SGL eligibility threshold affects the exam re-taking behavior and how this may induce a violation in the exclusion restriction of our instrument. Finally, we follow Conley et al. (2012) to benchmark the plausible exogeneity of threshold crossing as an instrument.

4.1 Effects of immediate loan take-up

Tables 8 and 9 show the robustness of our main results to the treatment *immediate take-up of a university SGL*. It is well known that the sensitivity of the 2SLS estimator to violations of the exclusion restriction depends on the strength of the instruments (See, for example, Angrist and Krueger (1994) and Bound, Jaeger, and Baker (1995)). Thus, the robustness of our results to this related definition of treatment, which delivers a stronger first stage, is reassuring.

While using *immediate take-up of a university SGL* as a treatment improves the first stage and the precision of the 2SLS estimates, we believe it is not the policy relevant treatment since it does not take into account that some of the students not taking up the loan *immediately* eventually do, therefore this treatment would not account for the overall effect of the loan. Indeed, the lower magnitude of the first stage when using *ever take-up of the SGL* reflects that some students that were initially ineligible for a university loan eventually took up the loan, most likely at vocational institutions.

Furthermore, as we show in the next subsection, students who do not initially qualify for the student loan have a larger probability of retaking the admission exam. To the extent that through this channel they eventually qualify and take up the student loan, using *ever take-up of the SGL* guards against this direct source of violation of the exclusion restriction. The next subsection discusses to what extent retaking could introduce a violation of the exclusion restriction through indirect channels that are not related to eligibility for the SGL.

4.2 Retaking behavior

Column (1) of Table 10 shows that around 40% of students who in the margin do not qualify for a SGL end up retaking the admissions exam afterwards, while crossing the threshold reduces this probability by 6.5 percentage points. This could be a threat to the exclusion restriction of our instrument if, for instance, waiting one more year before enrolling in higher education allows students to acquire new information about their alternatives and enrolled in a program that is a better match for their skills. However, we believe that this is not a big concern for various

reasons. First, students that crossed the threshold immediately and enrolled at a university (eligible compliers) could still enroll in another eligible program without losing their student loan, as long as they enroll in the new program within one year after leaving their first program. Second, column (2) in Table 10 shows that students below the cutoff improve their scores in 11.86 points (around 0.1 standard deviations) while students above the threshold improve it in 9.45 points (11.86-2.41). It would be hard to argue that 2.4 points (around 0.02 standard deviations) are enough to change the prospects of initially ineligible students. Indeed, we are not aware of evidence suggesting that waiting before enrolling in higher education helps students. Third, interventions in Chile have shown limited effects of information on students' choices, e.g. Hastings et al. (2015) show an increase of 1.4% in the expected earnings after an information-based intervention. Therefore, the threat that by waiting an extra year students could become better informed and through that channel increase their earnings has, from our point of view, limited power in explaining our findings.

Finally, it is worth highlighting that initially ineligible students who become eligible for a university SGL afterwards by crossing the threshold in a later attempt, are not part of the complier population of our study. Given our definition of treatment as *ever take-up of the university SGL,* the complier population only includes people who take-up (or not) the loan as a consequence of initial eligibility.

4.3 Plausible Exogeneity

In an attempt to benchmark the plausible exogeneity of our instrument, we use the method proposed by Conley et al. (2012). This method accounts for violations of the exclusion restriction by allowing the instrument *loan initial eligibility* to directly affect the outcome of interest, e.g. wages. Figure 1 shows the relationship between the estimated effect of the treatment on wages and different (theoretical) direct effects of initial eligibility on mean wages.

We use different priors on the effect of initial eligibility on the outcome in order to have sufficient structure to set bounds on the effect of interest. As shown by this figure the direct effect of *loan initial eligibility* on wages would have to be implausibly large (around 5 times the reduced form effect) to make the estimated effect of *ever take-up of the SGL* on wages statistically different from zero.

5 Differentials effects by gender

How different are our results for men and women? In this section we study the heterogeneity of treatment effects by gender. Tables 11 and 12 present separate results for men and women using our preferred specification.

Inspection of the mean for ineligible compliers suggests that ineligible complier women enroll less at any institution, specially 4-year college and they also have a lower labor market participation. Although some interesting patterns can be seen,[4] we do not find statistically differential effects of the loan for men and women.

6 Conditional effects of treatment on a discrete outcome

In our setting, one may be interested in the effects of the student loan on a conditional outcome, e.g. what is the probability that a student graduate from college, conditional on college enrollment, as a consequence of the student loan? In this section we show how to compute the treatment effect on a discrete outcome y_i^B, conditional on another

[4] For instance, women completely switch from graduating from 2-year to 4-year programs and at a smaller loss in terms of graduation compared to men.

discrete outcome y_i^A. Lets start by considering the following identity:

$$\begin{aligned} \mathrm{E}[y_i^A y_i^B \mid c] &= \mathrm{Prob}(y_i^A y_i^B = 1 \mid c) \\ &= \mathrm{Prob}(y_i^A = 1 \mid c) \times \mathrm{Prob}(y_i^B = 1 \mid y_i^A = 1, c), \\ &= \mathrm{E}[y_i^A | c] \times \mathrm{Prob}(y_i^B = 1 \mid y_i^A = 1, c) \end{aligned}$$

where c stands for treated compliers. Then, we can solve for the $\mathrm{Prob}(y_i^B = 1 \mid y_i^A = 1, c)$ in terms of potential outcomes

$$\mathrm{Prob}(y_i^B = 1 \mid y_i^A = 1, c) = \frac{\mathrm{E}[y_i^A y_i^B \mid c]}{\mathrm{E}[y_i^A \mid c]},$$

Finally, using Abadie (2002) method to estimate moments in the complier population, we can estimate $\mathrm{E}[y_i^A y_i^B \mid c]$, $\mathrm{E}[y_i^A | c]$ and combine them to compute the object of interest for the compliers. It is worth to mention that these point estimates should be taken with caution because this method to characterize complier populations does not provide standard errors and estimates are sometimes out of bound.[5]

7 Value-added of the higher education institutions

We calculate an institution-specific value-added measure in order to check the robustness of our claim that students move to lower quality institutions as a consequence of taking up the university SGL.[6] We estimate a value-added model (VAM) for vocational and university institutions using the labor market outcomes of a different cohort of students who were in their second year of higher education in 2007. Specifically, we estimate the following specification:

$$W_{ij} = \delta_j + X_i\beta + \epsilon_{ij},$$

where W_{ij} are the earnings in 2017 for student i who is enrolled in second year at institution j (with $W_{ij} = 0$ in case the student is not working) and X_i is a set of controls including students' centralized admission average score (math and language), GPA, students' high school fixed effects, fixed effects for the municipality where student i's high school was located, mother's and father's education level dummies, and type of health insurance (private or public). Under a standard conditional independence assumption, we interpret the estimated $\hat{\delta}_j$ fixed effects as the value added of institution j, i.e. $\hat{\delta}_j$ is the value added of the institution to monthly earnings, relative to the earnings of students with only higher education. We estimate this model using 355,541 students using their wages in 2017, and recover 174 fixed effects of institutions. It is worth noting that the measure of value added used here considers the possibility of drop out after enrolling in the institution, as we use second year students in our VAM.

Table 13 presents the RD estimates using the institution-specific value-added as an outcome. We find that taking up a student loan reduces the value added of the chosen institution by 93 dollars per month, a result that goes in line with our analysis using years of accreditation. Reassuringly, our measure of value-added is positively correlated with years of accreditation.[7]

[5] It is simple to extend this result to the case with three or more discrete variables, which may be useful, for instance, to study the probability that an eligible complier ever enroll at a vocational institution, conditional on having enrolled at a university without graduating from it.

[6] We have shown that loan take-up leads to a decrease in the years of accreditation of the receiving institutions. However, the use of years of accreditation may be a imperfect proxy for quality since it reflects inputs more than the output of higher education institutions.

[7] A one standard deviation increase in the standardized measure of accreditation is associated with an increase in VAM for the

8 Mismatch and other hypotheses

In this section we briefly discuss alternative hypotheses that could rationalize our finding of null labor market effects of student loans for marginally eligible students. In our analysis, we have suggested that these results may relate to low graduation, a consequence of low institutional quality of the receiving institutions. An alternative hypothesis is that while receiving institutions are of similar or higher quality, students are mismatched in terms of their own ability and consequently fail to graduate. We assess this hypothesis by showing that eligible and ineligible compliers are equally distant in terms of ability to the median of their classmates.

We use high school GPA as our measure of ability since it is available for the whole population of students, while test scores are not required in many vocational institutions and consequently not every enrolling student takes the exam. If the student loan produces academic mismatch then one sign of this could be an increase in the distance between marginal students' GPA and the GPA of the median student enrolled in the receiving program. In other words, under this hypothesis, the loan pushes students to move to programs where they have lower skills relative to their classmates. To study this, we compute for each college-major-year combination the standardized GPA for the median first year student. We then compute how far is each student from the median GPA of the first college-major where they enrolled. Then, we estimate whether loan take-up pushes students below the median of their college-major. We find that this is not the case. Table 13 shows that the distance in GPA relative to the median changes in only 0.016 standard deviations of GPA as a result of the loan take up.

Another alternative hypothesis that could explain our results is that parents reduce their investment when their children enroll at a university with a SGL relative to the situation where they enroll at a vocational institutions. This is a mechanism related to the one shown in Pop-Eleches and Urquiola (2013). While we have no information on parents' investment, we have access to parents' labor market outcomes right at the time students in our sample are graduating from high school. Table 14 shows that parents around the eligibility threshold have very similar labor market participation and wages, suggesting there is no behavioral response in line with this alternative hypothesis.

9 Data

This project combines college application test scores data and higher education enrollment and graduation records provided by the Ministry of Education, with labor market outcomes coming from the Unemployment Insurance and Pension System datasets provided by the Ministry of Social Development. The authors did not have access to personal identifiers because the files were anonymized using a unique number by the Ministry of Social Development. This appendix describes each data file used in the analysis and details the procedures used to clean and match them. Table 15 presents the descriptive statistics.

1. College application test scores data

The Ministry of Education provided data for all test takers in the process of 2008 and 2009 who graduated from high school after 2002. The raw data included 412,051 unique students. This files also included information on students' GPA, History and Science test scores, high school, self-reported parental education, and sex, among others.

From the raw data, we extract first-time test takers that just graduated from high school and who actually took the Math and Language tests, a necessary requirement in order to be in the potential pool of university loan eligible students. This leaves 298,859 students.

institution of 40 dollars.

2. Financial aid benefits application

The Ministry of Education provided data on all students filling out the socio-economic form in 2008-2009 and who graduated from high school after 2002. Students need to fill this form (FUAS in Spanish) to be eligible for government-provided financial aid. This dataset contains 315,666 students. We match 177,470 students in the restricted test score data presented above.

3. Higher education enrollment

The Ministry of Education provided data on all students enrolled in higher education between 2008-2016 who graduated from high school after 2002. Out of the 177,470 students in our base dataset, we match 168,504 of them to some enrollment over the years. This file uniquely identifies the college-major (including university degrees and shorter vocational degrees) where the student was enrolled in a given year.

4. College characteristics

The Ministry of Education also provided a supplementary dataset containing all college-major identifiers with college characteristics such as sticker tuition, municipality where it is located, institutional accreditation, college identifiers, and type of higher education, among others. We match all enrollment records to the characteristics of the college-major.

5. Graduation from higher education

The Ministry of Education provided access to the graduation records between 2007-2015. This file restricts information to students who graduated from high school after 2002. The file contains 622,461 unique students. We match 84,804 students out of the 177,470 students who took the college admission test and applied for financial aid.

6. Student loans

The "Comision Ingresa", the institution in charge of the subsidized guaranteed loan, provided data on the take-up of the loan and the amount borrowed between 2008-2016 for all students graduating high school after 2002. We match 80,712 students out of the 177,470 students in our main dataset.

7. Wages and employment

The Ministry of Social Development provided access to monthly wage information coming from the Unemployment Insurance (UI) system. The original dataset contains wages of all dependent workers formally hired by private companies in Chile. We had access to data for our main sample between 2007m1-2017m9. Out of the 177,470 students in our analysis sample, 154,706 students appear at least once in the UI wage data.
We complemented this dataset using monthly wage information coming from the Pension system. The original dataset contains wages of all dependent workers formally hired either by private companies or the public sector in Chile. Moreover, this dataset includes independent workers who voluntarily decided to save for retirement, a group that represents approximately 14% of all independent workers. We had access to data for our main sample between 2013m4-2015m3. Out of the 177,470 students in our analysis sample, we matched 119,861 to the wage data.

8. Supply side instrument

We supplemented the administrative information with geographic information on the quality of the college alternatives. Based on students' high school location, and based on a geocode of the college campus locations, we

constructed the geodesic distance between high school and college campus. Based on the accreditation information, we computed the average accreditation of the institutions located within 15, 25, and 35 kilometers of each student's high school.

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Tables and Figures

Table 1: Accredited institutions over time

Year	University	Vocational institutions
2004	14	2
2005	30	11
2006	38	16
2007	43	21
2008	45	21
2009	45	21
2010	47	25
2011	51	27
2012	50	28
2013	48	36
2014	45	37
2015	44	38
2016	45	36

Notes: The total numbers between 2004 and 2006 come from World Bank (2011). Numbers between 2007 and 2016 are constructed using data from SIES MINEDUC.

Table 2: Effect of loan take up on educational outcomes. Robust bandwidth

		Ineligible			Bandwidth	
	Reduced form	complier mean	2SLS	OLS	Reduced form	Fuzzy RD
	(1)	(2)	(3)	(4)	(5)	(6)
First stage		0.083***				
		(0.008)				
Panel A. Ever enrollment						
Any institution	0.005	0.963	0.075	0.046***	71.0	46.7
	(0.00)		(0.05)	(0.00)		
University	0.071***	0.159	0.829***	0.347***	39.7	52.3
	(0.01)		(0.10)	(0.00)		
Vocational	-0.063***	1.016	-0.751***	-0.222***	42.1	44.0
	(0.01)		(0.12)	(0.00)		
Panel B. Years of enrollment						
Any institution	0.173***	3.582	2.124***	1.125***	48.9	47.9
	(0.04)		(0.46)	(0.01)		
University	0.463***	0.000	5.345***	2.151***	41.6	57.2
	(0.06)		(0.54)	(0.01)		
Vocational	-0.279***	3.705	-3.394***	-1.026***	46.0	44.5
	(0.04)		(0.53)	(0.01)		
Panel C. Graduation and number of institutions						
Overall graduation	-0.016*	0.642	-0.171	-0.043***	69.6	46.7
	(0.01)		(0.12)	(0.00)		
Graduation university	0.036***	0.009	0.444***	0.118***	49.9	50.4
	(0.01)		(0.09)	(0.00)		
Graduation vocational	-0.052***	0.651	-0.615***	-0.161***	48.9	44.6
	(0.01)		(0.11)	(0.00)		
Number of institutions	0.037***	1.179	0.476***	0.110***	53.2	45.4
attended	(0.01)		(0.13)	(0.00)		
Observations		53,416		177,470		

Notes: This table presents university loan take up effects on ever enrollment and years of education in different types of institutions. Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by fuzzy regression discontinuity, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed using the optimal bandwidth in Calonico et al. (2014) presented in columns (5) and (6). Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 3: Effect of loan take up on debt and labor market outcomes. Robust bandwidth

		Untreated			Bandwidth	
	Reduced form	complier mean	2SLS	OLS	Reduced form	Fuzzy RD
	(1)	(2)	(3)	(4)	(5)	(6)
First stage		0.1***				
		(0.01)				
Panel A. Total debt (thousand dollars)						
Total debt all institutions	1.2***	2.9	14.0***	13.8***	42.4	45.9
	(0.16)		(1.33)	(0.04)		
Debt university loan	1.5***	0.0	17.3***	14.7***	40.5	41.9
	(0.16)		(1.10)	(0.04)		
Debt at vocational loan	-0.3***	3.0	-3.2***	-1.0***	56.4	48.3
	(0.07)		(0.90)	(0.01)		
Tuition	0.1***	2.0	1.5***	0.8***	44.5	53.1
	(0.02)		(0.21)	(0.01)		
Panel B. Labor market outcomes from UI data						
Monthly wage (dollars)	0.5	1,056.9	3.4	-35.1***	54.9	52.7
	(15.26)		(173.98)	(5.36)		
Probability of employment	0.0*	0.6	-0.2	-0.1***	62.6	47.1
	(0.01)		(0.11)	(0.00)		
Probability of fixed-term job	0.0	0.3	-0.1	0.1***	37.2	45.6
	(0.01)		(0.13)	(0.00)		
Probability of part-time job	0.0	0.1	0.0	0.0***	66.2	46.8
	(0.00)		(0.06)	(0.00)		
Years of experience	-0.1***	4.0	-1.6***	-0.6***	53.9	47.2
	(0.05)		(0.57)	(0.01)		
Average firm wage	31.9*	1,168.1	301.1	-67.9***	43.3	51.0
	(18.76)		(203.65)	(5.31)		
Panel C. Labor market outcomes from pension system data						
Monthly wage (dollars)	-4.8	856.3	-56.7	-54.2***	58.4	52.4
	(12.56)		(145.64)	(4.74)		
Probability of employment	0.0	0.6	0.0	-0.1***	64.1	45.6
	(0.01)		(0.11)	(0.00)		
Public sector worker	0.0	0.3	0.1	0.1***	56.4	47.5
	(0.01)		(0.11)	(0.00)		
Observations		53,416				

Notes: This table presents university loan take up effects on debt and labor market outcomes. Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are estimated by fuzzy regression discontinuity, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed using the optimal bandwidth in Calonico et al. (2014) presented in columns (5) and (6). Column (4) is using the whole analysis sample. Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 4: Testing the conditional independence assumption (CIA)

	+- 10 points		+- 20 points		+- 30 points		+- 40 points		+- 50 points		+- 60 points		+- 70 points		+- 80 points		+- 90 points	
	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
Years enrolled	0.002	0.005	0.007**	0.011***	0.006***	0.009***	0.010***	0.007***	0.011***	0.006***	0.010***	0.006***	0.010***	0.006***	0.010***	0.005***	0.010***	0.005***
	(0.011)	(0.009)	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Graduation university	0.000	0.000	0.001	0.001*	0.001***	0.002***	0.001***	0.002***	0.001***	0.002***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Graduation vocational	0.000	-0.003*	-0.001	-0.001	-0.001***	-0.002***	-0.001***	-0.002***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Monthly wage (dollars)	-2.571	-2.778	-0.600	0.382	-0.309	-0.134	-0.517	0.296	-0.066	0.379	0.280	0.053	0.497**	-0.246	0.487**	-0.337	0.256	-0.489**
	(3.728)	(3.509)	(1.278)	(1.179)	(0.696)	(0.678)	(0.461)	(0.473)	(0.348)	(0.359)	(0.276)	(0.287)	(0.228)	(0.245)	(0.198)	(0.217)	(0.178)	(0.196)
Probability of employment	-0.001	-0.002	-0.001	0.001	0.000	0.000	-0.001***	-0.001**	0.000*	-0.001***	0.000***	-0.001***	0.000***	-0.001***	0.000***	-0.001***	-0.001***	-0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Notes: This table presents the regression-based CIA test proposed by Angrist and Rokkanen (2015). Each row presents a test for a different dependent variable and the columns indicate the window below and above the university loan eligibility cutoff used for the test. Each cell entry corresponds to the coefficient on the running variable in a regression of the respective dependent variable against Math, the maximum between History and Science, GPA, and the running variable. Each regression uses data below or above the cutoff, according to the description of the column, up to the number of points reported in the column title. Robust standard errors are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 5: Enrollment (any) effects of access to loan over time

Years after high school graduation	Complier mean below threshold		Gap with no access to loan (Q5-Q1)	Effect of loan eligibility		Gap with access to loan (Q5-Q1)
	Quintile 1	Quintile 5		Quintile 1	Quintile 5	
	(1)	(2)	(3)	(4)	(5)	(6)
1	0.556	0.618	0.062	0.130***	0.050	-0.018
				(0.01)	(0.03)	
2	0.806	0.879	0.073	0.032***	0.035*	0.076
				(0.01)	(0.02)	
3	0.872	0.925	0.053	0.020**	0.025	0.058
				(0.01)	(0.02)	
4	0.896	0.939	0.042	0.012	0.025*	0.055
				(0.01)	(0.01)	
5	0.911	0.947	0.036	0.008	0.025*	0.053
				(0.01)	(0.01)	
6	0.919	0.951	0.032	0.008	0.023*	0.047
				(0.01)	(0.01)	
7	0.928	0.958	0.030	0.005	0.020	0.045
				(0.01)	(0.01)	
8	0.935	0.961	0.026	0.002	0.018	0.042
				(0.01)	(0.01)	
9	0.937	0.961	0.024	0.002	0.017	0.040
				(0.01)	(0.01)	

Note: This table presents the proportion of students enrolled at any type of higher education. Columns 1 and 2 show the share of students, from the first and fifth income quantile respectively, who are below the university loan eligibility threshold but enroll in some type of higher education. Column 3 shows the enrollment gap between these two groups of students. Similarly, columns 4 and 5 show the effect of loan eligibility on students from the first and fifth income quantile, and column 6 shows the difference between both groups. Income quintiles are defined by the government in the process of application to financial aid. The calculations are based on the same data described in the article.

Table 6: Enrollment (university) effects of access to loan over time

Years after high school graduation	Complier mean below threshold		Gap with no access to loan (Q5-Q1)	Effect of loan eligibility		Gap with access to loan (Q5-Q1)
	Quintile 1	Quintile 5		Quintile 1	Quintile 5	
	(1)	(2)	(3)	(4)	(5)	(6)
1	0.165	0.362	0.197	0.200***	0.084**	0.080
				(0.01)	(0.03)	
2	0.327	0.543	0.216	0.138***	0.089***	0.166
				(0.01)	(0.03)	
3	0.372	0.580	0.209	0.135***	0.085***	0.159
				(0.01)	(0.03)	
4	0.405	0.603	0.198	0.126***	0.082**	0.153
				(0.01)	(0.03)	
5	0.436	0.616	0.180	0.110***	0.088***	0.158
				(0.01)	(0.03)	
6	0.456	0.638	0.182	0.102***	0.078**	0.157
				(0.01)	(0.03)	
7	0.470	0.656	0.186	0.103***	0.064**	0.147
				(0.01)	(0.03)	
8	0.487	0.665	0.179	0.094***	0.058*	0.142
				(0.01)	(0.03)	
9	0.492	0.670	0.178	0.091***	0.053*	0.140
				(0.01)	(0.03)	

Note: This table shows the proportion of students enrolled at university.Columns 1 and 2 show the share of students, from the first and fifth income quantile respectively, who are below the university loan eligibility threshold but enroll in university. Column 3 shows the enrollment gap between these two groups of students. Similarly, columns 4 and 5 show the effect of loan eligibility on students from the first and fifth income quantile, and column 6 shows the difference between both groups. Income quintiles are defined by the government in the process of application to financial aid. The calculations are based on the same data described in the article.

Table 7: Schooling and experience effects on monthly wages

	(1)	(2)	(3)
Years of university	41.7***	43.4***	45.7***
	(1.9)	(1.9)	(2.7)
Years of vocational	25.5***	24.8***	25.3***
	(2.2)	(2.2)	(3.1)
Experience	94.4***	191.4***	182.9***
	(1.7)	5.8	8.3
Experience^2		-11.3***	-10.4***
		(0.6)	(0.9)
Observations	33,484	33,484	16,698
Mean of monthly wage	1,012	1,012	1,007
Std. dev. of monthly wage	(633)	(633)	(631)
Sample	RD sample	RD sample	Analysis sample

Notes: This table reports estimates of equation 1. We measure wages as the annual average monthly wage (if employed) in the 9th year out of high school. Years of schooling correspond to years of enrollment at a given institution, and experience is constructed as the number of years each person is observed in the UI administrative data. We consider all students in the analysis sample within a 40-point bandwidth around the eligibility cutoff. All regressions control by PSU test-scores and high school GPA, as well as sex, family income quintiles, and type of high school dummies (public, voucher, private). Standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 8: Effect of *immediate* loan take-up on educational outcomes

	Reduced form (1)	Ineligible complier mean (2)	2SLS (3)	OLS (4)
First stage		0.141***		
		(0.005)		
Panel A. Ever enrollment				
Any institution	0.004	0.973	0.027	0.023***
	(0.00)		(0.03)	(0.00)
University	0.068***	0.517	0.483***	0.204***
	(0.01)		(0.06)	(0.00)
Vocational	-0.058***	0.678	-0.414***	-0.189***
	(0.01)		(0.06)	(0.00)
Panel B. Years of enrollment				
Any institution	0.177***	4.558	1.255***	0.552***
	(0.04)		(0.26)	(0.01)
University	0.426***	2.077	3.014***	1.306***
	(0.05)		(0.34)	(0.02)
Vocational	-0.249***	2.482	-1.759***	-0.754***
	(0.04)		(0.26)	(0.01)
Panel C. Graduation and number of institutions				
Overall graduation	-0.021**	0.645	-0.146**	0.021***
	(0.01)		(0.06)	(0.00)
Graduation university	0.033***	0.196	0.232***	0.143***
	(0.01)		(0.05)	(0.00)
Graduation vocational	-0.053***	0.450	-0.378***	-0.122***
	(0.01)		(0.05)	(0.00)
Number of institutions	0.038***	1.144	0.270***	0.112***
attended	(0.01)		(0.07)	(0.00)

Notes: This table presents immediate university loan take-up effects on ever enrollment, years of education, and graduation from different types of institutions. The first row reports first-stage effects of initial university loan eligibility on immediate university loan take-up (F-test of 795). Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed in our RD sample, restricting to observations in a 40-point bandwidth of the eligibility cutoff. Column (4) uses the whole analysis sample. Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 9: Effect of *immediate* take-up on total debt and labor market outcomes

	Reduced form (1)	Ineligible complier mean (2)	2SLS (3)	OLS (4)
First stage		0.141***		
		(0.005)		
Panel A. Total debt and cost of degree (thousand dollars)				
Total debt all institutions	1.2***	6.81	8.4***	14.0***
	(0.13)		(0.87)	(0.07)
Debt university loan	1.4***	4.47	10.0***	14.8***
	(0.13)		(0.82)	(0.07)
Debt at vocational loan	-0.2***	2.33	-1.6***	-0.9***
	(0.07)		(0.48)	(0.02)
Tuition	0.1***	2.48	0.8***	0.8***
	(0.02)		(0.12)	(0.01)
Panel B. Labor market outcomes from UI data				
Monthly wage (dollars)	-4.5	1015.69	-33.0	25.7***
	(13.72)		(100.48)	(8.47)
Probability of employment	0.0	0.52	0.0	0.0***
	(0.01)		(0.05)	(0.00)
Probability of fixed-term job	0.0	0.45	0.0	0.0
	(0.01)		(0.07)	(0.00)
Probability of part-time job	0.0	0.08	0.0	0.0
	(0.00)		(0.03)	(0.00)
Years of experience	-0.1**	3.09	-0.7**	-0.3***
	(0.04)		(0.29)	(0.02)
Average firm wage	10.3	1074.21	75.6	-44.8***
	(15.47)		(113.46)	(7.92)
Panel C. Labor market outcomes from pension system data				
Monthly wage (dollars)	-10.4	884.37	-75.2	24.6***
	(11.52)		(83.31)	(7.61)
Probability of employment	0.0	0.49	0.0	0.0***
	(0.01)		(0.06)	(0.00)
Public sector worker	0.0	0.37	0.0	0.0***
	(0.01)		(0.06)	(0.00)
Observations		53,416		177,470

Notes: This table presents immediate university loan take-up effects on debt and labor market outcomes.. The first row reports first-stage effects of initial university loan eligibility on immediate university loan take-up (F-test of 795). Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed in our RD sample, restricting to observations in a 40-point bandwidth of the eligibility cutoff. Column (4) uses the whole analysis sample. Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 10: Reduced form effect of initial university SGL eligibility on retake and change in score

	Retake	Change in score
	(1)	(2)
Reduced Form	-0.065***	-2.413***
	(0.01)	(0.47)
Average Below Cutoff	0.39	11.86
Observations	53,416	

Notes: This table presents reduced form effects of threshold crossing on retake of the admissions exam (column (1)) and change in score in the exam (column (2)) defined to be 0 for students who do not retake. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 11: Effect of loan take-up on educational outcomes by gender

	Female			Male		
	Reduced form	Ineligible complier mean	2SLS	Reduced form	Ineligible complier mean	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
First stage		0.090***			0.073***	
		(0.011)			(0.012)	
Panel A. Ever enrollment						
Any institution	0.006	0.933	0.066	0.001	0.988	0.012
	(0.00)		(0.05)	(0.01)		(0.09)
University	0.077***	0.138	0.856***	0.056***	0.239	0.766***
	(0.01)		(0.11)	(0.01)		(0.16)
Vocational	-0.064***	0.961	-0.710***	-0.051***	1.069	-0.695***
	(0.01)		(0.12)	(0.01)		(0.18)
Panel B. Years of enrollment						
Any institution	0.218***	3.162	2.415***	0.120**	4.073	1.645**
	(0.05)		(0.48)	(0.06)		(0.77)
University	0.472***	0.000	5.227***	0.363***	0.000	4.994***
	(0.06)		(0.64)	(0.08)		(0.91)
Vocational	-0.254***	3.273	-2.812***	-0.243***	4.348	-3.349***
	(0.05)		(0.49)	(0.06)		(0.82)
Panel C. Graduation and number of institutions						
Overall graduation	-0.013	0.597	-0.147	-0.031**	0.726	-0.421**
	(0.01)		(0.12)	(0.01)		(0.20)
Graduation university	0.043***	0.000	0.473***	0.019*	0.064	0.255**
	(0.01)		(0.11)	(0.01)		(0.13)
Graduation vocational	-0.056***	0.625	-0.621***	-0.049***	0.662	-0.676***
	(0.01)		(0.11)	(0.01)		(0.17)
Number of institutions	0.036***	1.162	0.404***	0.041***	1.148	0.561**
attended	(0.01)		(0.13)	(0.02)		(0.22)

Notes: This table presents university loan take-up effects on ever enrollment, years of education, and graduation from different types of institutions. The first row reports first-stage effects of initial university loan eligibility on university loan take-up. Columns (1) and (4) show the reduced form effect, columns (2) and (5) show the complier mean for ineligible students computed following Abadie (2002), and columns (3) and (6) present the treatment effect estimated by 2SLS. Estimates are computed in our RD sample, restricting to observations in a 40-point bandwidth of the eligibility cutoff. Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 12: Effect of loan take-up on total debt and labor market outcomes by gender

	Female			Male		
	Reduced form	Ineligible complier mean	2SLS	Reduced form	Ineligible complier mean	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
First stage		0.090***			0.073***	
		(0.011)			(0.012)	
Panel A. Total debt and cost of degree (thousand dollars)						
Total debt all institutions	1.3***	3.10	14.5***	1.0***	2.83	13.8***
	(0.18)		(1.37)	(0.20)		(2.07)
Debt university loan	1.6***	0.00	17.8***	1.1***	0.00	15.5***
	(0.18)		(1.22)	(0.18)		(1.44)
Debt at vocational loan	-0.3***	3.10	-3.3***	-0.1	2.83	-1.7
	(0.08)		(0.92)	(0.12)		(1.61)
Tuition	0.1***	1.94	1.5***	0.1***	2.11	1.4***
	(0.02)		(0.24)	(0.03)		(0.33)
Panel B. Labor market outcomes from UI data						
Monthly wage (dollars)	-5.4	975.68	-57.0	-3.3	831.42	-50.0
	(17.15)		(181.43)	(22.06)		(333.50)
Probability of employment	0.0	0.49	0.0	0.0	0.69	-0.2
	(0.01)		(0.11)	(0.01)		(0.16)
Probability of fixed-term job	0.0	0.27	0.1	0.0	0.65	-0.3
	(0.01)		(0.13)	(0.01)		(0.23)
Probability of part-time job	0.0	0.11	0.0	0.0	0.07	0.0
	(0.01)		(0.06)	(0.01)		(0.09)
Years of experience	-0.1*	3.57	-0.9*	-0.1*	4.58	-1.7*
	(0.05)		(0.55)	(0.07)		(0.92)
Average firm wage	25.6	979.98	269.8	-8.2	1066.40	-124.3
	(18.65)		(201.50)	(25.50)		(385.20)
Panel C. Labor market outcomes from pension system data						
Monthly wage (dollars)	-6.6	878.68	-70.8	-16.8	790.75	-223.2
	(13.94)		(150.22)	(19.33)		(255.02)
Probability of employment	0.0	0.46	0.0	0.0	0.61	-0.1
	(0.01)		(0.11)	(0.01)		(0.17)
Public sector worker	0.0	0.39	0.0	0.0	0.24	0.1
	(0.01)		(0.12)	(0.01)		(0.17)
Observations		31,698			21,718	

Notes: This table presents university loan take-up effects on debt and labor market outcomes. The first row reports first-stage effects of initial university loan eligibility on university loan take-up. Columns (1) and (4) show the reduced form effect, columns (2) and (5) show the complier mean for ineligible students computed following Abadie (2002), and columns (3) and (6) present the treatment effect estimated by 2SLS. Estimates are computed in our RD sample, restricting to observations in a 40-point bandwidth of the eligibility cutoff. Out of range complier means are replaced by bound values (0 or 1). Tuition corresponds to the annual tuition. Average firm wages corresponds to the average of the monthly wages paid by the firm. Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1% .

Table 13: Effect of loan take-up on relative GPA and value added

	Reduced form	Ineligible complier mean	2SLS	OLS
	(1)	(2)	(3)	(4)
First stage		0.083***		
		(0.008)		
GPA - median GPA	0.0	0.00	0.016	-0.1***
	(0.01)		(0.17)	(0.00)
VAM	-7.8***	20.22	-92.6***	-75.3***
	(3.01)		(34.93)	(0.95)
Observations		53,416		

Notes: This table presents university loan take-up effects on ever enrollment, years of education, and graduation from different types of institutions. The first row reports first-stage effects of initial university loan eligibility on university loan take-up (F-test of 107.64). Column (1) shows the reduced form effect, column (2) shows the complier mean for ineligible students computed following Abadie (2002), column (3) presents the treatment effect estimated by 2SLS, and column (4) shows OLS estimates. Estimates in columns (1) to (3) are computed in our RD sample, restricting to observations in a 40-point bandwidth of the eligibility cutoff. Column (4) uses the whole analysis sample. Out of range complier means are replaced by bound values (0 or 1). Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 14: Effect of loan take-up on parents' labor market outcomes

	Father			Mother		
	Reduced form	Ineligible complier mean	2SLS	Reduced form	Ineligible complier mean	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Employment						
t=0	-0.006	0.372	-0.064	0.008	0.117	0.091
	(0.01)		(0.11)	(0.01)		(0.08)
t=1	0.000	0.328	-0.002	0.006	0.165	0.074
	(0.01)		(0.11)	(0.01)		(0.08)
Panel B. Wages						
t=0	-7.577	498.4	-93.290	10.682	85.2	122.271
	(12.99)		(160.76)	(7.25)		(84.27)
t=1	-7.721	705.3	-91.283	-6.190	472.4	-67.699
	(16.84)		(199.83)	(11.70)		(129.14)
Observations		36767			42845	

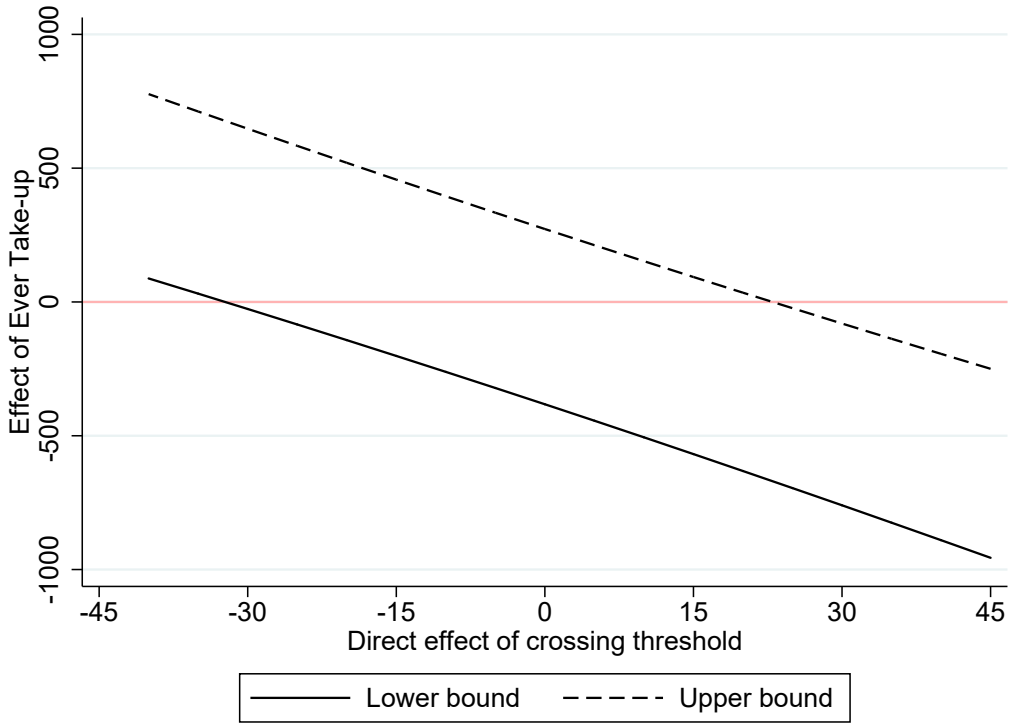
Notes: This table presents university loan take-up effects on employment and wages for parents of students around the university SGL eligibility threshold. Columns (1) through (3) report effects for fathers and columns (4) to (6) for mothers. Columns (1) and (4) show the reduced form effects. Columns (2) and (5) show the complier mean for ineligible students computed following Abadie (2002), while columns (3) and (6) present the treatment effect estimated by 2SLS. Out of range complier means are replaced by bound values (0 or 1). Robust standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 15: Descriptive statistics

	Observations	Mean	Std. Dev.	1st Percentile	99th Percentile
	(1)	(2)	(3)	(4)	(5)
Average Math and Language	177,470	509.64	95.59	297	733.5
Language score	177,470	508.50	101.92	271	747
Math score	177,470	510.78	102.32	264	748
History score	110,878	505.42	102.43	271	754
Science score	109,466	502.84	103.27	268	749
max(History, Science)	176,044	513.21	102.65	271	757
GPA	177,470	5.70	0.61	4.70	6.80
Quintile 1	177,470	0.44	0.50	0	1
Quintile 2	177,470	0.19	0.39	0	1
Quintile 3	177,470	0.14	0.34	0	1
Quintile 4	177,470	0.13	0.33	0	1
Quintile 5	177,470	0.11	0.31	0	1
Ever enrolled	177,470	0.95	0.22	0	1
Ever enrolled at a university	177,470	0.68	0.46	0	1
Ever enrolled in vocational	177,470	0.41	0.49	0	1
Number of institutions	168,504	1.21	0.46	1	3
Year of enrollment by year 8	177,470	5.18	2.23	0	9
Years of enrollment at a university by year 8	177,470	3.75	3.09	0	9
Years of enrollment at a vocational by year 8	177,470	1.43	2.01	0	7
Graduation by year 7	177,470	0.48	0.50	0	1
Total debt (thousands of dollars)	177,470	5.57	8.29	0	31.85
Total debt university (thousands of dollars)	177,470	4.18	8.14	0	31.78
Total debt vocational (thousands of dollars)	177,470	1.39	3.37	0	15.54
Sticker tuition cost	160,368	3.11	1.39	0.93	7.34
Wage by year 9 (excluding zeros)	104,279	1086.48	723.21	71.52	3525.98
Employment in year 9	177,470	0.46	0.45	0	1
Temporary work in year 9	104,279	0.37	0.44	0	1
Part-time work in year 9	177,470	0.06	0.25	0	1
Number of months of employment in year 9	177,470	2.49	2.32	0	8.42
Average wage at firm in year 9	104,279	1295.72	750.60	199.75	3691.24
Wage (pension system data)	90,668	848.66	568.21	59.60	2770.13
Employment (pension system data)	177,470	0.44	0.46	0	1
Public employment (pension system data)	177,470	0.43	0.49	0	1

Notes: This table presents number of observations, mean, standard deviation, 1st and 99th percentile of each variable.

Figure 1: Plausible exogenous



Note: This figure presents results from the bounding exercise, described in the appendix, in which we allow initial eligibility to affect the wage directly. The x-axis measures (theoretical) direct effects of initial eligibility. The y-axis measures the corresponding effect of ever loan take-up. See Conley et al. (2012) for details.