STUDENT LOANS, COLLEGE CHOICE AND INFORMATION ON THE RETURNS TO HIGHER EDUCATION

by

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We present the results of a randomized intervention in which we provided college applicants in Chile with information about institution- and field of study-specific earnings and debt outcomes. We assemble this information by linking administrative records of high school, college, and standardized testing records for the population of high school graduates between 2000 and 2013 to administrative tax records. We accompany our information intervention with surveys measuring baseline earnings and cost expectations as well as preferences over degree programs. We find that students have unbiased but highly variable beliefs about tuition costs, and upward-biased beliefs about earnings outcomes for past graduates of their first-choice degree programs. Poorer students have less accurate information on earnings and costs, and choose degrees with lower predicted returns from the options available to them. The informational intervention does not affect whether students enroll in higher education, but does cause low-SES students to enroll in degrees with modestly higher predicted returns. Consistent with the predictions of a model of choice under imperfect information, these effects are driven by less-informed students and students with less intense degree-specific preferences. Effects of the intervention are close to zero for students receiving state-backed loans, raising concerns about the efficacy information-based policies as strategies for lowering student loan default rates and encouraging financially sound educational decisions.

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1 Introduction

Federal student loans and grants are a key component of the policy effort to expand access to higher education for students from low-income backgrounds. If these programs succeed in helping liquidity-constrained students make profitable investments in their future earnings, the social returns could be large. But, if students taking loans have limited information about their educational options, or face biases or high decision making costs, they may not make personally or socially sound investments in education. In the context of rising student loan default rates (DOE 2013), policy makers and economists hypothesize that such students may choose lower-return, higher-cost degrees based on poor information and marketing by higher education institutions (GAO 2010; Lewin 2011; Lederman 2009, 2011), potentially limiting the benefits of higher education to those loan subsidies are meant to help.

To reduce high default rates and limit the effects of misleading information from institutions competing for these students, policymakers have focused on two types of solutions. Demand-side interventions aim to improve education decisions by disseminating government-compiled information on academic, labor market, and financial outcomes for different degree programs. Supply-side regulations directly limit subsidies available to students enrolling in degree programs with a history of poor academic and/or labor market outcomes.[1] How effective these policies are at promoting higher-return educational investments depends on how much students already know about academic and financial outcomes at different degree programs, how much students care about these outcomes when making their choices, and how effectively the government designs and communicates new information for college applicants.

We present the results of a randomized intervention in which we provided college loan applicants in Chile with information about institution- and field of study-specific earnings and debt outcomes, directly testing a government-implemented demand-side intervention in Chile's higher education market. Chile is a middle-income, OECD member country with a higher education system that resembles the US in terms of completion rates, market structure, and public subsidy rates through federal student loans. We worked closely with a number of Chilean government agencies to develop and link student records of high school graduation, college enrollment, and standardized test scores for the population of Chilean high school graduates between 2000 and 2013. We then matched these records to administrative tax data.

Following the intervention, we tracked students in the treatment and control groups to see whether and where they chose to matriculate. To test predictions of the impact of treatment generated by a

[1] In the US, proposed gainful employment rules (Department of Education, 2014) encompassed both types of policies discussed here. See Shear (2014) for a description of ranking proposals. White House (2013) details of ranking and accompanying accountability proposals.

model of educational investment choice with limited information, we designed survey questions on enrollment plans and expectations to accompany the field experiment and supplement administrative data.

We administered the survey and field experiment in partnership with the Ministry of Education (MINEDUC) as part of the 2013 student loan application process. Directly following application submission, students applying for subsidized loans were sent an email from MINEDUC requesting that they log into a secure website to fill out an additional set of questions. Applicants logged in, were given an informed consent, and were asked six questions. These included questions about enrollment plans, questions about own earnings and tuition cost expectations at the degree in which they planned to enroll, and questions about expected earnings for typical students in that degree. 49,166 students completed the online questionnaire.

Upon completion, randomly-selected students continued to two additional web pages designed to provide them with information and prompt searching for higher net-value degrees. The web program was interactive, using prior survey responses to pipe in relevant, personalized information for each applicant. This information drew on our back-end-database linking educational and tax records for past graduates.

The first page displayed information on earnings gains (relative to no tertiary enrollment) for the participant's first-choice degree in monthly terms, tuition costs in monthly payments, and a "net value" which was the difference between monthly gains and payments in pesos. Costs and benefits were calculated over the 15 year student-loan repayment term, and converted into monthly gains and monthly debt payments. To encourage searching, the page also displayed how much more net value the applicant could receive by enrolling in an alternative institution offering the same major, or in a different major in the same broad field of study (e.g. nursing vs. nutritionist). The potential gain was drawn from degrees relevant to respondents based on the selectivity of their planned enrollment choices.

Finally, respondents were taken to a searchable database that allowed them to select a major and enter an entrance exam score. Based on that information, the page populated a table of degrees admitting students with similar scores, sorted in descending order by net value. The web program recorded all searches made. Slightly less than half of treated loan applicants conducted searches.

Our core results are as follows. First, many students have limited knowledge of the earnings and cost outcomes associated with different degree programs, and students from low socioeconomic status (SES) backgrounds tend to have less information on these degree characteristics than other students. Compared to students from higher-income backgrounds, students from low-income and non-college-educated backgrounds are 16 percentage points (on a base of 24%) more likely to say that they do not know tuition costs at their planned place of enrollment. They are similarly 15-22 percentage points more likely to say that they do not know what they or the typical graduate will earn upon completing their chosen degree, relative to a base for higher-income students of 30-37%. Students who do report expectations about degree-specific own and typical-student earnings systematically overestimate earnings for graduates. Furthermore, students are overconfident about how they will score on entrance exams, leaving them susceptible to last-minute changes in enrollment plans.

We then show that gaps in knowledge between higher- and lower-income students are consistent with differences in enrollment decisions between these two groups. Using our extensive data on historical enrollment records linked to tax earnings, we show that low-SES students systematically choose degrees that yield lower-expected-returns conditional on background and entrance exam scores than high-SES students with similar academic qualifications do (Beyer et al. 2014). This fits with the idea that low-income students are not fully informed and appear to make financially worse educational investments decisions, though it could also simply reflect differences in preferences across majors with systematically different returns in the marketplace.

We next analyze the results of our information experiment, motivating our empirical analysis with a simple model of college choice under limited information. We describe how our treatment can be viewed theoretically as informative and unbiased advertising. We predict strong impacts on enrollment decisions among students who have more limited information, who do not have strongly-formed educational preferences (erroneous or not), and who place more value on monetary gains and costs - all factors we can measure using our mixed-methods approach of supplementing administrative data with survey responses.

The impact of information treatment lines up with these predictions. However, the overall effect is small in magnitude. First, even with direct communication from the educational authority at the time of application, students from low-income backgrounds are the hardest to reach with information. This echoes difficulties in social service program take-up for those most in need found in other research across a range of social services (see, e.g., Currie 2006; Choi, Laibson, and Madrian 2011; Bettinger et al. 2012; Amior et al 2012).

Within the group of students who reach the randomization stage of the intervention, treatment has a significant and positive impact mainly on the enrollment choices of low-SES students. The intent-to-treat (ITT) effect is small overall, moving net value of the enrolled degree by approximately 7% of median potential gains from switching to a peer institution offering a similar degree. . Note that under the exclusion restriction that treatment effects accrue only to students who use the searchable database, the effects of search on net values, earnings gains could be 2.3 times as large, since 43% of the treatment group conducts searches. We find no impact of the informational intervention on students' extensive-margin choice to matriculate in any degree program; the point estimates are near zero and insignificant. The effects we observe are driven entirely by the intensive margin choice of where to enroll, and those effects are driven by impacts on students from low-SES backgrounds.

ITT effects are larger for students who have less information on earnings and costs at baseline, and who exhibit lower levels of pre-intervention preference for a given degree or program. Students who state in the survey that they do not know tuition or earnings expectations for their chosen degree have the largest treatment effects, with gains again concentrated among low-SES students. Similarly, students who express interest in multiple fields of study and those who express some stated uncertainty in their enrollment plans have the largest treatment effects. These effects are equal to 13% percent of the potential gain from switching degrees within field of interest. Again, treatment effects are concentrated among applicants from low-SES backgrounds.

An important question from a policy perspective is how students with access to subsidized loans respond to the informational treatment. We find that ITT effects are strongest among low-SES students who *do not* receive federal student loans (which are merit and need based and not caused by treatment). This is particularly true when the sample is further limited to students qualifying only for non-selective technical institutes, private universities and professional degree programs, treatment effects. Given that the returns to education are possibly negative (earnings gains do not justify costs) at these low selectivity degrees, our results suggest that information may not be helpful in nudging loan takers to make wiser educational investments (ones that will allow them to repay their student loans).

Finally, in all cuts of the data, we find that positive treatment effects on the net value of the chosen degree are generated by students choosing degrees with higher earnings gains rather than lower tuition costs. This suggests that demand response to information could chase returns estimates rather than put pressure on tuition, even if costs and earnings gains are presented separately. This result as well as the results by loan-receipt status may be due to lack of financial literacy and poor understanding of loan terms as measured in other surveys we conducted of student loan takers (Hastings et al. 2014).

We conclude by discussing the implications our findings have for the ability of information provision to nudge students towards more viable educational investments of student loan funds. We discuss the viability of alternative supply-side policies in light of our findings, as well as the potential for harm in a market with responsive firms providing higher-education services.

This paper makes several contributions to existing research. To the best of our knowledge, this is the first paper to evaluate the effects of an informational intervention in which prospective applicants are supplied within program-specific information on earnings and costs. Our experimental design closely aligns with current policy proposals. We build on smaller-scale interventions and surveys targeted at students already enrolled in elite schools (Arcidiacono, Hotz, and Kang 2012; Wiswall and Zafar 2013; Zafar 2013), interventions that provide information about average returns to college (Jensen 2010), and surveys and interventions aimed at making the application process more transparent for high-achieving students (Avery and Kane 2004; Avery and Hoxby 2012; Bettinger et al. 2012; Hoxby and Turner 2013).[2] Our intervention is the first to be carried out on the population of interest, and through the relevant government agency, thus directly testing an actual, implementable demand-side policy.

We contribute to a broader literature using application or enrollment records linked to administrative data on labor earnings (Hoekstra 2009; Saavedra 2009; Ockert 2010; Hastings et. al 2013; Zimmerman 2014). We contribute to research understanding how behavioral biases, limited information and decision making skills can influence the impact of social safety net programs (e.g., Thaler and Benartzi 2004; Hastings and Weinstein 2008; Duarte and Hastings 2011; Bhargava & Manoli 2011; Bettinger et al. 2012; see Madrian 2014 and Lavecchia, Lieu and Oreopoulos 2014 for reviews). Finally, our mixed-methods approach combines survey responses that measure knowledge and preferences with administrative data on actual decisions and field experimental variation in independent variables of interest to test predictions from models of limited information and decision making. Here we contribute to a developing literature that includes Ashraf, Karlan, and Yin (2006), Bettinger et al.(2012), and Hastings (2014).

2 Background: Chilean Higher Education System

2.1 Overview of the Chilean postsecondary education system

The Chilean higher education system resembles systems in the US and other upper-income OECD countries in several ways, including terms of rates of educational attainment, the role of student loans in higher education finance, and market structure. Chile, a middle-income OECD member country, introduced a major student loan expansion in 2006, called the Programa de Credito con Aval del Estado (CAE). CAE expanded access to federally subsidized student loans to students at all accredited degree programs including technical and professional programs, as well as degrees from non-selective private universities. Previously, federally subsidized loans had been available only to students in elite universities serving approximately the upper 50% of students score on national entrance exams.

Following the reform, the fraction of higher education revenues in Chile coming from loan dollars rose by 170%,[3] and college enrollment rates rose by more than 50% as a fraction of the college-

[2] See Scott-Clayton (2012) for a review of this literature.

aged population, from 48% in 2005 to 74% in 2012.[4] Today, rates of educational attainment and the share of higher education revenues coming from loans are similar to those in the US. 38% of adults between 25 and 34 years old in Chile in 2010 had tertiary degree, compared to 42% in the US (OECD 2013).

The loan expansion occurred in the context of a higher education marketplace that is in many ways similar to that in the US. Public, private non-profit, and private for-profit firms provide higher-education degrees in Chile. There are three main degree levels and three institution types: technical schools (CFTs) offer two- to three-year technical degrees, professional institutes (IPs) offer both technical and vocationally-oriented four-year degrees, while universities offer traditional undergraduate and graduate degrees.[5] In 2012, universities held 58.4% of all undergraduate matriculation while professional institutes and technical schools had 28.1% and 13.5% respectively.

Universities can be public or private-not-for-profit. IPs and CFTs are private and can be for-profit or not-for-profit. Even though for-profit universities have never been legal in Chile, in practice, portions of some universities are owned by for-profit parent companies.[6] Not-for-profit universities have outsourced enrollments to for-profit international education investment groups like Laureate International Universities, an international chain which owns five universities in the US (including Walden University and Kendall College) and thirty institutions in Latin America.[7] In Chile, Laureate institutions have a 12% market share of all higher education enrollment.[8] The Apollo Group, which owns the University of Phoenix in the U.S., also participates in this market having recently purchased the university UNIACC, a "leading arts and communications university in Chile." [9] In 2009, the Apollo Group settled in a False Claims law suit for its recruiting and advertising practices in the US.[10]

The oldest and most selective set of universities is collectively called the CRUCH (Council of Chilean University Rectors). These traditional universities have participated in a centralized, score-based application process since the late 1960s. The central piece of this process is a standardized test called the *Prueba de Selecion Universitaria* (or PSU, translated as Test for University Selection).[11] The entrance

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[3] Annuario Estadistico 2012 MINEDUC based on data from Servicio de Información de la Educación Superior (SIES), División de Educación Superior. Ministerio de Educación.

[4] Source: World Bank (2014). http://data.worldbank.org/indicator/SE.TER.ENRR/countries?page=1

[5] Some universities, particularly public universities, also offer two-year technical degrees.

[6] "Las 11 instituciones de Educación Superior cuestionadas por irregularidades en 2012." *La Tercera*. 27 November 2012. http://www.latercera.com/noticia/educacion/2012/11/657-495574-9-las-11-instituciones-de-educacion-superior-cuestionadas-por-irregularidades-en.shtml. Accessed 2 May 2013.

[7] http://www.laureate.net/, accessed 7 May 2013.

[8] Universidad Nacional Andres Bello has 6.4% of all undergraduate college enrollment and is the largest university in Chile.Universidad de las Americas has 5 % of all undergraduate enrollment and is the second largest.

[9] Apollo Global Fact Sheet, accessed 7 May 2013. http://www.apollo.edu/sites/default/files/files/Apollo-Group-Apollo-Global-Fact-Sheet.pdf

[10] Source: http://www.bloomberg.com/apps/news?pid=newsarchive&sid=a7cFhPKPB1mA. Accessed November 16, 2014. http://www.republicreport.org/2014/law-enforcement-for-profit-colleges/ provides a compilation of regulatory actions and inquiries against for-profit higher-education chains in the U.S. by both federal and state authorities. Accessed November 16, 2014.

[11] Prior to 2003, the entrance exam was called the PAA, *Prueba de Aptitude Academica*.

exam is constructed, administered and scored by a central testing agency under the authority of the CRUCH. Entrance exam takers complete exams in Mathematics and Language. Scores are scaled to a distribution with a mean and median of 500 and standard deviation of 100. Entrance exam scores are the key determinant of admissions, loan, and scholarship qualification.

Students in Chile apply to institution-major combinations (e.g., Sociology at the University of Chile), which we will term degrees. For CRUCH admissions, students submit one application with up to eight degree choices in order of preference. Degrees rank students based on their entrance exam scores, and the central admission algorithm assigns one admission slot to each student. Admission is given to each student's most preferred degree conditional on there being a seat open for that student (i.e. there are fewer, higher-scoring applicants than total seats available). Thus CRUCH degrees each have an annual admissions cutoff based on the entrance exam scores (cites – Hastings et al. 2013, other cites on efficient algorithms).[12] The least selective CRUCH degrees have admission score cutoffs near 475. Outside of CRUCH, private universities will still rely on the PSU and GPA for admissions decisions. Students admitted to these universities typically have entrance exam scores over 350. Most technical and vocational schools do not require an entrance exam score for admission, though many students who have entrance exam scores enroll in their degree programs.

2.2 Federally subsidized student loan and grant programs

There are two types of student loans in Chile. The smallest and oldest type of loan is the Fondo Solidario de Crédito Universitario (FSCU). It is both need- and merit-based. It has existed since 1981.[13] To qualify for a FSCU loan, students must be Chilean citizens, have "family income that makes payment of tuition difficult or impossible", and an average PSU score in Math and Language of at least 475 points. FSCU loans can only be used at CRUCH institutions. The interest rate is set at 2% and the loans are administered directly by the universities and funded by the government. The FSCU program is and was small and targeted the poorest students.

To increase higher-education opportunities for low-income students, the government introduced the the *Crédito con Garantía Estatal* (Loan with State Guarantee, most commonly known as CAE, for *Crédito Aval del Estado*) beginning with the 2006 school year.[14] CAE can be used to finance education

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[12] Hastings, Neilson and Zimmerman (2013) describes the process in detail and use the score-based cutoffs to measure long-run returns to CRUCH degrees.

[13] Originally called *Crédito Fiscal Universitario*, it was first introduced in 1981 by *D.F.L N°4* and modified in 1994 to its current state by *Articulo 70*.

[14] CAE was created by the passage of a new law in 2005, "*Crédito de la Ley 20.027 para Financiamento de Estudios de Educación Superior*."

at any accredited postsecondary institution: CRUCH universities, accredited private universities, professional institutes and technical schools are eligible. It is both need- and merit-based; for studying at a university, first-time applicants need to have scored an average of 475 on the PSU (the same as the Fondo Solidario loan program). To enroll in a technical or professional degree, students need either a high school GPA of 5.3 (approximately the median GPA, or a C average), or an average PSU score of 475. Recipients must be from the lowest four income quintiles.[15]

Students apply for the FSCU and CAE as well as several federal grant programs using Chile's *Formulario Unico de Acreditación Socioeconómica* (FUAS), a unified financial aid form which is similar to the FAFSA in the US. Applications for FCSU are completed online with an application deadline in the beginning of November. The PSU is given at the end of November or beginning of December. PSU scores are released at the end of December. CRUCH admission forms are submitted during the first two weeks in January. Admissions notifications are made one week later. The school year starts at the end of February or start of March. Appendix Figure A.1 shows a timeline of the loan application and college application process in Chile.

3 Data

3.1 Administrative data

In collaboration with MINEDUC and the other agencies within the Chilean government, we constructed a database that combines high school records, college records, loan records, and tax records for cohorts of Chilean students from 1982 through 2013. The purpose of the data collection effort was to inform upcoming policy decisions on whether and how to reform the student loan system. We refer to this database as the Proyecto 3E database, named for the project encompassing the research and data collection efforts under the partnership.[16] It includes the following types of records.

3.1.1 High school records

[15] "Quality Assurance in Higher Education in Chile." OECD. November 2012. http://www.oecd.org/chile/Quality%20Assurance%20in%20Higher%20Education%20in%20Chile%20-%20Reviews%20of%20National%20Policies%20for%20Education.pdf. Accessed 31 May 2013. Law 20,027. Article III, paragraph 2, section 9.3. NB. In the law itself, no mention is made of socioeconomic quintiles.
[16] Proyecto 3E: Expectativas. Estudiantes. Educatíon., translated "Project 3E: Expectations. Students. Education."

Student-level high school records include covariates such as gender, standardized test scores, high school identifiers, and high school characteristics. Test score measures are taken from an exam called the SIMCE (Sistema Nacional de Medición de la Calidad de la Educación). The SIMCE is administered to 10^{th} graders approximately every other year (2001, 2003, 2006, 2008, 2010), and includes separate math and language sections. Like many state-level accountability exams in the US, the SIMCE includes survey questions on topics such as parental education, parental income, and household characteristics such as access to internet, television, or computers.

Chile has a universal voucher system, and all schools (municipal/public, private-voucher-accepting and private-non-voucher-accepting) report enrollments, grades, standardized test scores, and graduation rates to MINEDUC. School-level characteristics include the type of school – municipal, voucher, or private (not accepting vouchers) – as well as a school-level poverty rating based on the poverty concentration of enrolled students. The poverty rating categorizes schools from A (highest poverty) through E (lowest poverty). High school records were available in electronic form from 2003 to 2013. We digitized records for graduation back through 1995.

We will often use the high school poverty rating as a categorization of low- versus high-socio-economic-status (SES). Students coming from A and B poverty schools are categorized as low-SES. Appendix Table A.1 shows how family background and academic performance as well as school characteristics vary with school poverty status. Low-SES schools are predominantly municipal schools while high-SES schools are private schools. The highest SES schools (E) do not accept vouchers for tuition. Students from low-SES schools perform substantially worse on college entrance exams. Their mean combined PSU score is 437, compared to 624 for the highest-SES group. Low-SES schools send substantially fewer graduates to college, and have much lower graduation rates. Parents of students at low-SES schools are unlikely to have completed college degrees. 3% of students from group A schools have at least one parent with a completed college degree, compared to 70% of students at E schools.

3.1.2 Entrance exam records and CRUCH applications

We constructed a database of entrance exam registration, scores, full CRUCH applications and admission decisions. Electronic records were available from MINEDUC from 2000- 2013. We digitized data from original hard-copy archives for test scores data from 1980-1999 and admissions data for years before 1982 through 1999. These data allow us to construct admissions cut-off scores by degree and year as described in Hastings, Neilson and Zimmerman (2013). Table A2 describes the distribution of combined math and reading exam scores by high school SES rating.

3.1.3 Federal loan and scholarship applications and awards

Individual records on FUAS (unified loan and scholarship) applications and awards were available from 2007 to 2013. Registering students provide limited demographic information about themselves and their family, as well as income information.

3.1.4 College matriculation and graduation

As part of the new loan system, all higher education institutions (HEI's) are required to report student-level enrollment and graduation data to MINEDUC each year. These data were available from 2007 through 2013 in a standardized format which included information such as tax ID number (called a RUT – Rol Único Tributario), unique school-degree code, birthday, gender, semester enrolled and graduation status. To complete the necessary data for the research projects, we assisted MINEDUC in designing an additional data requirement for HEI's to provide additional historic enrollment and graduation data back to 2000. These data show semester-by-semester enrollments by degree within an institution. They also provide graduation lists by year with degree conferred. These data cover almost all institutions, CRUCH and non-CRUCH, and all postsecondary programs including night degrees, technical degrees, professional degrees, bachelors, masters, and doctorate degrees.

We use these data to construct a degree enrollment history for each student. The data allow us to measure drop-out rates, changes in degrees within an institution, and changes in institutions. They also allow us to track if students are enrolled in a degree or a graduate degree, which is important when measuring earnings.

3.1.5 Earnings by institution and degree

Through an agreement with the tax authority, and for the purpose of informing specific higher education policy, we were permitted to link our database of student records to their tax returns from the 2005-2012 earnings years on a secure computer within the tax authority.[17] Over 99% of individuals in our data have matches in the tax records. Tax returns include earnings information from wages, contracts, partnerships, investments and retirement income. Hastings, Neilson and Zimmerman (2013) describe the tax data in detail, and provide an example of a tax form to illustrate the components used to calculate employment income.

Tax data were only accessible inside the Chilean tax authority on a secure, dedicated computer. In compliance with Chilean law, we were permitted to take out aggregate earnings information and regression output. In addition, we were able to merge parental identifiers to our student-level database

[17] This disclosure is required by the Chilean government. SOURCE: Information contained herein comes from taxpayers' records obtained by the Chilean Internal Revenue Service (*Servicio de Impuestos Internos*), which was collected for tax purposes. Let the record state that the Internal Revenue Service assumes no responsibility or guarantee of any kind from the use or application made of the aforementioned information, especially in regard to the accuracy, validity or integrity.

using family linkage data from the Civil Registry. This allowed us to calculate parental income for most students and use it to control for student characteristics when estimating earnings impact of degree enrollment. Using these data we construct measures of earnings gains by university and degree as described in Beyer et al. (2014). We estimate a value-added model of earnings by degree enrolled in through the first seven years of labor market experience. We estimate degree-level fixed effects, adjusting the fixed-effects estimates for sample size using a methodology similar to Chetty et al. (2014). The OLS value-added estimates conditional on enrollment vary flexibly with student SES, entrance exam score, gender, and field of study. We use these estimates to construct a best linear predictor of earnings for students enrolling in degrees in our experimental sample as a function of their baseline characteristics, the field of study and selectivity tier of the degree, and the value-added estimate of the degree they enrolled in. See Beyer et al. (2014) for further details.

3.1.6 Survey and experimental data

As part of our research partnership with MINEDUC, we designed and administered several surveys over the course of two years, collecting information on loan literacy, financial literacy, educational preparation and planning, and knowledge of expected labor market and graduation outcomes as well as tuition costs. Many of these survey results are summarized in Hastings et al. 2014.

To test whether information on financial outcomes for past students can push applicants towards higher-return educational choices, we worked with MINEDUC to implement a field experiment that provided randomly-selected loan applicants with information on earnings outcomes for recent graduates and current data on tuition costs. The purpose of the survey and field experiment was to measure how informed enrollment choices are, and to test whether information could improve loan efficacy by nudging loan takers towards positive-expected-value investments (and thus also exerting demand-side pressure on HEI's to offer higher return and lower-cost degrees in the long run).

Students in the 2012 graduating high school cohort and all other PSU registrants (including those from older high school cohorts) were pre-assigned to treatment and control groups. Treatment status was randomly assigned by high school for the current graduation cohort and by prior-PSU score bin for prior high school graduates. Upon completing a FUAS application, students were sent an email from MINEDUC requesting that they complete six additional survey questions related to the FUAS process. Students were assured that their responses would not impact their FUAS outcome, but that the questions were important and would be used by MINEDUC to help improve the education system in Chile.

The field experiment was constructed as follows. First, the universe of high school seniors and PSU registrants were merged together, and assigned to either the treatment or control group. We stratified treatment assignment by high school for current high school seniors,[18] and by prior PSU test score (50 point bins) for PSU registrants who had graduated in the two prior cohorts.[19] This list was merged to loan applications as the loans were completed. Loan applicants received an email from MINEDUC with a subject line "Código Confirmación FUAS" (FUAS Confirmation Code). The email asked applicants to participate in a brief survey that would be used by MINEDUC to make decisions about higher education. They were told that they would receive a confirmation code at the end of the survey, and that their survey responses would be kept anonymous, used only for research, and would not affect their FUAS applications in any way. Emails were managed using a service which allowed us to track bounce-backs, opens, and click-throughs for each email address.

Upon opening the email, applicants could click a link which would then take them to a customized website. They logged in with their identification number and email address and were given an informed consent to accept or reject. Conditional on acceptance, they began the survey. The survey asked them six questions, each of which appeared on its own page, with participants clicking a “next” button to proceed to the next question. Each question could only be completed once (if a respondent left the survey and started again it would start them where they left off).

The survey adapted questions based on responses. The first question asked students about their current education status. The second question asked them to list at least the top three, but up to the top five, institutions and majors (degrees) they planned to apply to (chosen from a nested set of drop down menus that filtered results to make lists of manageable size). The third question asked them how certain they were of these choices. The fourth question asked them what they thought the annual cost of studying (tuition plus registration fees) at each of their choices would be. Their choices were piped in from their prior responses. They could click a “No Sabé” (“I do not know”) button or move a slider to indicate the total annual cost. The fifth question asked them about expected earnings upon graduation (the survey and experiment focused on graduation rather than enrollment, consistent with MINEDUC’s objectives at that time). They were asked to estimate what their monthly salary would be once they started in a stable, full-time job after graduating from each of their choices. They were also asked to estimate what a typical graduate in each degree would earn. They were allowed to choose “I do not know” for each sub-question or fill in earnings amounts with a slider. The sixth question asked them what they expect to get on their

[18] Note, high school classes in Chile are small because most schools are private or private-voucher-accepting. Median graduating class size in 2012 was 59. Schools were broken into groups based on high school type (private not-accepting-vouchers, private voucher-accepting, and municipal), the fraction of students taking the PSU and the average PSU score from the prior two senior cohorts. Half of the schools within each randomization block were assigned to treatment.

[19] PSU registrants for the 2013 college entering class could use old PSU scores. This was a new policy. Hence the PSU registration list consisted of those who currently wanted to take the PSU, as well as those who had taken the PSU in prior years in case colleges requested their prior test score for admissions. Thus the PSU registrant list consisted of new test takers, test re-takers and prior test takers who were not retaking the test. This gave us a sample of older graduates who may apply for a loan.

PSU in language and math. We present the survey materials in Spanish (with English translations) in the online appendix.

Upon completing the final question, control subjects were shown a thank you page with their confirmation code. They also received a thank you email with the same message. Treated students continued to a new page which displayed five pieces of information. A table at the top presented monthly earnings outcomes for graduates from their top-choice degree relative to a high school graduate, projected and amortized over fifteen years (the length of a student loan).[20] We calculated these values using the Proyecto 3E database linked to tax returns. Each student was shown degree-specific earnings values for past graduates. The second column of this table displayed monthly payment costs for a loan covering tuition and registration through expected degree length. The third row displayed a net value – the difference between monthly earnings gains and costs. Below the table, in a highlighted box, students were told whether there were other institutions they could likely get into which offered the same degree with a higher net value, and they were told the additional net value they could gain by switching institutions (though they were not told which institution offered this better net value). The net value gain was calculated by the web program by referencing a back-end database and using the chosen major and the PSU score the respondent entered. Finally, treated participants were shown in a second highlighted box indicating whether or not there were other degrees within the same broad field of study as their first-choice degree that offered higher Net Value (again based on degrees they could get into with their expected PSU score), and they were shown the expected Net Value gain from switching degrees.[21]

After the information and suggestion page, treated subjects clicked through to a final page, which gave them the option to enter a PSU score, degree level (technical or university/professional), and major at the top of the page to populate a table of net values below. When populated, the table displayed institutions offering the specified major which a) frequently admitted students with the stated PSU score and b) were relatively high-earning outcomes within the PSU-major group. Specifically, the web program selected all degrees in the same major for which the stated PSU score fell within the 5th and 95th percentile for enrolling students. Students were informed at the top of the page that this new database was being produced by MINEDUC using tax records of past graduates, and that they could log back in at any time

[20] Note that we were constrained to display returns for graduates based on then current MINEDUC policy. However, returns conditional on enrollment is a more relevant metric for decisions at the time of enrollment, and the one that we eventually adopted as part of student loan policy resulting from the Proyecto 3E research as described in Beyer et al. 2014. We will show in all results tables both the impact of treatment on graduate Net Value as well as on best linear predicted earnings conditional on enrollment.

[21] The median gain in predicted earnings associated with the switch described in the first box was equal to 33% of predicted earnings in students' first listed choice. The median tuition change associated with the switch was 0. The median gain in predicted earnings associated with a switch to the degree program described in the second box was equal to 156% of predicted first choice earnings. The median tuition change was 34.2%. See Table A4 for more details.

and compile and view up to ten comparative tables to use in choosing their degree by entering their expected PSU score and selecting a major.

This final page also contained a thank you message and the confirmation code. Note that students were not required to search the database once there. Approximately 43% percent of students who were shown this page searched at least once. We will use treatment assignment to test for an impact of information on the characteristics of the degree in which students chose to enroll. We obtained enrollment data by matching student records from the survey and field experiment to MINEDUC's 2013 matriculation files. Online Appendix A provides questions, screen shots and further documentation of earnings and tuition statistics calculated for and displayed in the survey and field experiment.

4 Empirical Analysis

4.1 Descriptive results on college choice and information

4.1.1 Sample Characteristics

Table 1 summarizes the invited sample, comparing characteristics of the invited sample to those of eventual respondents. Overall, 69% of the emails we sent were opened. Of those, the respondent read and agreed to the informed consent disclaimer 73% of the time. 59% of students providing informed consent completed the survey through to receiving the confirmation code. Thus, 30% of the original email requests from MINEDUC to the email address given by the respondent for their college and loan applications resulted in a completed survey, with the largest attrition at the email opening and survey completion stages. We will refer to survey completers as respondents from this point forward.

Within our sample, students with lower baseline academic achievement, higher-poverty backgrounds, and lower-educated backgrounds were harder to reach. The average PSU score (entrance exam score) for respondents is 31 points higher than for invitees. The fraction of invited students from low-SES high schools was 43.70%; this falls to 35.70% in the sample of respondents. Respondents are more likely to have parents with some tertiary education, and score substantially higher on high school standardized tests (SIMCE) than invitees. Women are slightly more likely to respond than men (57.5% mean female among respondents versus 55.40% among invitees).

The second to last column shows characteristics of treated respondents. These are the students in the treated group who complete the survey. There are no substantial differences in baseline characteristics between treatment and control students. Appendix Table A.3 shows that baseline characteristics are balanced across treatment and control groups, including survey responses up until the end of the survey and routing into information treatment. A p-value test of joint significance of baseline characteristics in explaining treatment fails to reject the null with a p-value of 0.335. The final column shows characteristics of who searched the database conditional on being assigned to treatment. Overall, 43% of treated students searched. Their characteristics are very similar to those of the full sample of treated students, though they have slightly higher PSU and SIMCE scores, and are slightly more likely to be female and slightly less likely to be a newly graduating high school senior.

4.1.2 Survey Responses

Tables 2 through 5 show survey responses broken down by the poverty rating of students' high schools. The sample here includes all students who completed the survey (i.e., students in either the experimental treatment or control group). Table 2 shows the response of expected tuition costs versus actual tuition costs. Specifically, respondents were asked "¿Considerando los costos de matricula y arancel aproximadamente cuáles crees que serán los costos ANUALES de estudair la(s) carrera(s) en la(s) institución(es) elegida(s) anteriormente?" ("*Considering the costs of registration and tuition, approximately how much do you think the ANNUAL costs are for studying in the institution(s) previously selected?")* Overall, a third of students respond that they do not know the tuition costs at their enrollment choices. This percentage is 67% higher for those coming from the highest poverty schools compared to those coming from the lowest poverty schools. Stated ignorance of tuition costs is decreasing with socio-economic status, the opposite of what one might expect if budget constraints and necessity drove responses, but what one would expect if financial illiteracy and poor financial decision making ability was higher among high-poverty students. Conditional on claiming some knowledge of tuition costs, students are on average approximately correct. However, students from the poorest schools overestimate tuition, while students from higher-SES schools tend to slightly underestimate it. Further, although tuition estimates are generally centered around the correct values, many students' beliefs are inaccurate. For instance, a quarter of students underestimate tuition at their top choice degree program by at least 16.5%. For the students from the poorest schools, the mean absolute prediction error is 47.5%.

A similar pattern emerges in questions on earnings. Table 3 shows how students responded when asked a) what they would expect to earn if they completed their first choice degree, and b) what they expect average earnings are for students who complete the specified first choice degree. As with our cost results, we divide students' earnings estimates by the observed mean earnings outcome in the first choice degree, and describe the distribution of deviations from the observed mean in percentage terms.[22]

As with cost expectations, many students who reply to the survey claim not to know what to expect about earnings outcomes, either for themselves or for the average graduate. 35.8% of students select the "I don't know option" for own earnings, with the fraction rising from 29.0% in the highest-SES high schools to 44.2% in the lowest-SES high schools. 47.7% of students select the "I don't know" option when asked about average earnings, with the fraction rising from 37.3% in the highest-SES high schools to 59.5% in the lowest-SES high schools.

Conditional on providing an earnings value, expectations appear to be biased upward. On average, students expect their own earnings conditional on graduating from a given degree program to be 51.80% higher than observed values for past graduates. One explanation for this is that the students who select into our respondent pool and who respond to this question would in fact earn more than the average for past graduates. However, when asked directly about average earnings outcomes for past graduates, students overestimate these as well (second panel of results). Students view their earnings prospects as being similar to those for past graduates, but overestimate the earnings of past graduates on average. Average overestimates are particularly large for low-SES students, and across all SES groups they are driven by a skewed right tail of students with large, positive prediction errors.

Taken at face value, the earnings and tuition costs survey results suggest that one third to one half of students are uninformed about earnings and costs at the time of loan application and a short time before college application. Students confident enough to report earnings and cost expectations provide cost estimates that are unbiased on average but differ greatly from population values in many cases, and earnings estimates that are biased upwards. Despite this, the majority of students are very certain about their planned applications. Table 4 shows responses to the following question: "¿Qué tan seguro(a) estás que las opción(es) que mencionaste anteriormente serán a las que efectivamente postules el próximo año?" ("*How sure are you that the option(s) you listed will be the ones to which you apply next year?*") The response options were: "I am not sure at all," "I am a little sure," "I am fairly sure," "I am pretty sure," and "I am absolutely sure." (The Appendix also displays the options in Spanish). We break down results by high school poverty rating, and by current versus past high-school graduates. Overall, about two thirds of students are "absolutely sure" or "pretty sure" about their first choice. There is little

[22] Specifically, the question asks "¿Cuánto crees que será TU sueldo mensual al comenzar a trabajar, una vez titulado(a), con trabajo estable de tiempo completo? Responde a continuación en la columna izquierda. ("*What do you think YOUR monthly salary will be once you graduate and start to work in a stable, full-time job? Please respond below in the left-hand column.*") and "¿Cuánto crees que será el sueldo mensual de UN GRADUADO TÍPICO al comenzar de trabajar una vez titulado(a) con trabajo estable de tiempo completo? Responde a continuación en la columna derecha. ("*What you think the monthly salary with be FOR A TYPICAL GRADUATE once s/he graduates and starts to work in a stable, full-time job? Please respond below in the right-hand column.*") We compare this to experience year 1 and 2 mean earnings for graduates as the data analog individuals are being asked to predict.

variation with high school poverty rating even though information about earnings and cost outcomes varies significantly. Those who have taken time between high school and college applications are on average slightly more certain of their plans. This may be because they have work experience in particular fields.

Interestingly, while certain of their application plans, students are overconfident about their performance on entrance exams. Since entrance exams determine admissions to CRUCH degrees, overconfidence may force students to make last-minute changes to their enrollment plans. Table 5 shows the difference between predicted and realized PSU scores scores by poverty rating of the high school of origin, and by certainty of application choices. Overall, low income students perform about 50 points worse on the PSU than expected. 25% of low-income students overestimate their PSU score by at least 105.5 points. This is compared to a mean error of 29.1 points and an upper-quartile error of 55.0 points for higher-income students. Hastings et al. (2014) show that in other survey results, low-income students employ many fewer resources to prepare for entrance exams than high-income students do, for example relying on free courses or self-study rather than formal courses and private tutors. Interestingly, the mean overconfidence level is 47.1 among those who are absolutely certain about their degree choices, versus 40.8 among those who express some uncertainty.

4.1.3 Cross-SES differences in enrollment outcomes

Data on enrollment decisions show patterns consistent with the finding that low-SES students have less information on degree-specific earnings and costs. Figures 1 and 2 use value-added estimates conditional on enrollment from Beyer et al. (2014) to project expected earnings gains for students in the 2007 to 2011 entering freshman cohorts. We use the regression estimates from the Proyecto 3E data and the best linear predictors of degree-level value added to predict short run (out to age 30) earnings gains conditional on enrollment in the degree each student matriculated to and their demographic characteristics. These demographic characteristics include gender, test scores, high school socioeconomic status, and interactions between these variables. We also use the earnings growth rate estimates by selectivity and field of study to grow earnings estimates out to age 50 (see Beyer et al. 2014 for details). We emphasize that these estimates are descriptive and not causally estimated; students may select into degree programs in ways that are correlated with other earnings determinants conditional on observable covariates. As discussed in Beyer at al. (2014), they do produce earnings gains to admissions estimates comparable to those produced with regression discontinuity estimates from Hastings, Neilson and Zimmerman (2013).

Figures 1 and 2 show the distribution of predicted short- and long-run returns to enrollment across all degrees. Returns are calculated as a net present discounted value return over not going to college. They include opportunity cost of time and tuition cost at the expected degree length. The returns are therefore the net present value of predicted earnings for each student from enrolling in their observed degree, less expected tuition costs and the present discounted value of earnings had they entered the work force after high school graduation, divided by the present discounted value of earnings had they entered the work force after high school graduation. A value of 100 means that someone is expected to earn twice (in present discounted value) the amount they would earn if they did not enroll in college. A negative value indicates that tuition costs and opportunity cost of time exceed future expected earnings gains post-enrollment. To facilitate presentation, we present the distribution graphically, plotting means and percentiles of returns on the vertical axis versus PSU score on the horizontal axis. The figures show the enrollment-weighted distribution of degree-specific returns for students at a given PSU score level. Specifically, we determine which degrees students with a given PSU score enroll in, and we take an enrollment weighted average of predicted returns for students from the 2007-2011 freshman classes across those degrees. In Figures 1 and 2, a mean return of 200 at a PSU score of 600 implies that, on average, students with scores of 600 choose to go to degrees which offer on average a tripling of earnings net of tuition and opportunity costs relative to not enrolling in college at all.[23]

While the average long-run return for college enrollment is 331 and short-run return is 218, the average masks significant variation driven to a large extent by selectivity. Students in the roughly the lower 10% of the test score distribution choose from degrees with negative earnings returns on average. More than ten percent of students enroll in degree programs with negative net present values out through the median PSU score of roughly 500 points. Average returns to college can be misleading, particularly for low-income students who, on average, tend to have lower entrance exam scores and college preparation and qualify to get into the lower-return segment of the distribution. The upward, convex shape of mean returns is consistent with the regression discontinuity estimates found in Hastings et al. (2013), which examines long-run returns to admission using regression discontinuities in CRUCH admissions outcomes by degree for applicants from the 1980s through early 2000s.

Figure 3 displays differences in predicted returns by student socioeconomic status, and decomposes these returns into a component attributable to differences in within-degree effects by SES (holding enrollment decisions constant) and a component attributable to cross-SES differences in enrollment decisions. To do this, we use the tax data to calculate mean returns for each degree, separating students by SES category (high versus low). We display mean net values by SES category and PSU score using either i) overall enrollment weights (the average person's enrollment choice) or ii) SES-specific enrollment weights (adjusting for the fact that high- versus low-SES students may make different degree choices). Comparing the lines Low SES – Population weight with High SES – Population weight lines

[23] To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of 2004-2011 enrollment.

gives the mean difference within degree between predicted earnings gains for high and low SES students. Comparing the lines Low SES – SES weight and Low SES – Population weight shows how much of the mean difference in low- versus high-SES students can be explained by the differences in the returns at degrees they choose. Similarly, the difference in High SES – SES weight and High SES – Population weight shows the extent to which higher returns for High SES students can be attributed to which degrees they choose.

Overall, low-SES students enroll in degrees where the NPV is roughly 27% less than that for high-SES students. This gap is narrow at low entrance exam scores and widens as scores increase. Holding enrollment weights fixed at population averages, low-SES students earn roughly 13% less in expectation than high-SES students; differences in enrollment choices account for roughly half of the earnings gap. Conditional PSU score, low-income students select degrees with lower expected returns for low-income students – the green line is always below the blue line. They would do better in expectation choosing the typical degree. In contrast, high-SES students choose degrees with slightly higher-than-average returns for them.

Taken together, our survey and expected-earnings-at-enrollment results suggest that many students, particularly students from low-SES backgrounds, have poor information on relative costs and returns of higher education options they qualify to get into. Despite this, they have made loan and application decisions with a high degree of certainty in the choices they have made. These findings are consistent with a broader set of survey results reported in Hastings et al. (2014). That paper finds that the majority of survey respondents list prestige and accreditation as the primary reason for degree selection while only 11% list future earnings as one of their top three determinants of degree choice (only 2% list it as their top reason). Given a hypothetical question about willingness to switch careers in response to economy-wide changes in relative earnings, over 43% of applicants say they would never change their career in response to relative earnings changes.[24] Finally, financial literacy and loan literacy (knowledge of student loan terms) is very low, and lowest among those from low-SES backgrounds. The effect of a demand-side informational intervention on students' choices depends to a large extent on whether students would like to acquire information on earnings and cost outcomes but find it difficult to do so, or whether their preferences over degree programs are so strong that additional information will not affect the choice process.

[24] The question specifically asked "Suppose that INE [the National Labor Institute] just released a new report that proves that the salaries for graduates in [first choice field] have fallen by 10%. Now, instead of earning [respondent estimate of earnings in that field], you will earn [X% less than expected value]. Would you feel the need to change this career option for another?" X increased if the respondent answered "no", from 10% to 50%, at which point respondents could click "never" or fill in a value higher than 50% for the wage change it would take to induce them to switch careers.

4.2 Field Experiment: Framework and Results

4.2.1 Model and Empirical Predictions

We frame our analysis using a simple model of college choice with limited information. Suppose that students choose a degree to maximize utility, and that they have perfect information about expected labor market earnings, tuition costs, and the full set of options that they could be admitted to. Students choose the degree that maximizes utility subject to a budget constraint. Assuming student loans remove credit constraints for constrained students, adding only interest costs to present value of tuition costs, we can write utility as:

(1) $$u_{ij} = \alpha_i netgain_{ij}\left(t_j, l_i, \hat{y}_{ij}\right) + \delta_{ij} + \varepsilon_{ij}$$

Where $netgain_{ij}\left(t_j, l_i, \hat{y}_{ij}\right)$ is the net present value of expected earnings gains for *i* enrolling in degree *j* over not enrolling in tertiary education as a function of tuition at *j*, whether *i* receives a subsidized loan, and expected earnings returns for *i* conditional on enrolling in *j*; δ_{ij} is the value *i* places on non-pecuniary characteristics of *j* relative to not enrolling in tertiary education, and ε_{ij} is an *i.i.d.* extreme value error term. Maximizing utility implies the following logit probability that *i* chooses to enroll in degree *j*,

(2) $$L_{ij} = \frac{e^{\alpha_i netgain_{ij}\left(t_j, l_i, \hat{y}_{ij}\right) + \delta_{ij}}}{\sum_{k \in K_i} e^{\alpha_i netgain_{ik}\left(t_k, l_i, \hat{y}_{ik}\right) + \delta_{ik}}}$$

where K_i is the set of degrees to which *i* can gain admission. (We abstract here from probabilistic admissions outcomes in this illustrative model; in practice, many of the students in our data will apply to non-selective degrees.) An information treatment should have no impact on application choices and enrollment decisions in this case, as the individual is fully informed.

Suppose instead people make decisions with limited information, both on the options in their choice set and on the characteristics of those options (for example, tuition costs, expected labor market returns, or graduation probabilities). Our information treatment can be written as both providing information on which options are in the choice set and providing information on earnings and tuition costs. It is isomorphic to an advertising model where advertising is restricted to both increase salience (consideration) of relevant choices and impact perceptions of pecuniary option characteristics. Unlike advertising models where firms engage in advertising to obfuscate prices or net returns (Carlin 2009; Ellison and Ellison 2009; Hastings, Hortaçsu and Syverson 2013), for example by advertising employment outcomes for only top graduates, our treatment clarifies net returns, making them easier to compare across options.

Following DellaVigna (2009) and Hastings, Hortaçsu and Syverson (2013), we can write the limited-information utility model as:

(3) $$u_{ij} = \alpha_i \left(1 - \theta_i \left(T_i\right)\right) netgain_{ij} \left(t_j, l_i, \hat{y}_{ij}\right) + \bar{\delta}_{ij} + \tilde{\delta}_{ij} \left(T_i\right) + \varepsilon_{ij}$$

Where θ_i is a perception parameter that is zero when T_i=1 and between zero and one otherwise, $\bar{\delta}_{ij}$ is the preference of student *i* for career *j* given what they know about it without treatment, $\tilde{\delta}_{ij}\left(T_i\right)$ is an added preference for the degree as a result of receiving treatment. This can be interpreted either as added value to non-pecuniary characteristics, or simply as an added probability of choosing the degree now that it is more salient or prominent due to its appearance in the information treatment table searched by treated student *i*. To see this note that if all degrees receive the added treatment impact, they will have the same choice probabilities as without treatment. Similarly two degrees made salient in the comparative net value table will have the same relative likelihood of being chosen as if both were not in the treatment information, but both will be more likely to be chosen than a degree not included in the information treatment by a factor of $e^{\tilde{\delta}_{ij}(T_i)}$, all else equal. Within treatment status, the choice between two considered options depends on characteristics of the products and net returns and the relative importance to utility.

In this model we can write the probability that person *i* chooses career *j* as a function of information treatment

(4) $$L_{ij} = \frac{\omega_{ij} e^{\alpha_i \left(1-\theta_i \left(T_i\right)\right) netgain_{ij}\left(t_j, l_i, \hat{y}_{ij}\right) + \bar{\delta}_{ij}}}{\sum_{k \in K_i} \omega_{ik} e^{\alpha_i \left(1-\theta_i \left(T_i\right)\right) netgain_{ik}\left(t_k, l_i, \hat{y}_{ik}\right) + \bar{\delta}_{ik}}}$$

where

(5) $$\omega_{is} = \frac{e^{\tilde{\delta}_{is}(T_i)}}{\sum_{r \in K_i} e^{\tilde{\delta}_{ir}(T_i)}}$$

Thus, if a student receives treatment that makes them aware of a particular degree, their probability of choosing that degree increases by the exponential the treatment effect relative to the total treatment inclusion across all other degree programs.

Equations 4 and 5 generate several predictions for the reduced-form estimate of treatment on characteristics of the chosen degree given by

$$Y_i = \phi T_i + \beta' X_i + \upsilon_i \tag{6}$$

where Y_i is the outcome of interest such as net value of degree enrolled in, T_i is a dummy for treatment, X_i is a vector of baseline controls orthogonal to treatment that improve precision of estimates, and υ_i is a mean zero disturbance term.

First, treatment with information should increase the probability of choosing a higher net value degree among students who have low measures of information on choice characteristics prior to treatment. Lack of knowledge could signal low intrinsic preferences for net returns, α_i, or a high value of θ_i. Based on community experience with tertiary education, we expect lack of knowledge to signal true lack of knowledge among low-income students, but potentially lack of preference among less-budget-constrained high-income students. Second, treatment should increase the probability of choosing a better return degree if individuals do not have strong preferences for particular degrees ($\bar{\delta}_{ij}$ is low) before receiving information relative to their informed-preferences for present discounted value of investment.

4.3.1 Field Experiment Results

Pooled over all respondents

Table 6 shows the impact of information on educational investment decisions for the pooled sample, and for subsamples coming from low- and high-SES schools respectively. Specifications reported here and in following tables of experimental results include controls for randomization block and for the value corresponding to the dependent variable of students' first choice degree (e.g. net value of first-choice degree if dependent variable is Net value, monthly debt of first choice degree if dependent variable is Monthly debt). These controls reduce standard errors but do not substantially alter point estimates. Standard errors allow for clustering at the high school level for students applying to college directly out of high school.

The first panel shows impacts of treatment on the decision to matriculate to any tertiary degree program. The impact of treatment on the extensive margin is very close to zero and insignificant. The second panel shows the impact of treatment on the measures of information shown in the experiment: Monthly Debt, Earnings Gains (per month over 15 years vs. no college enrollment) and Net Value (the difference between the two). For 23% of the sample who did not matriculate to any degree, these three values are set to zero. The overall impact of treatment is therefore the change in the dependent variable given enrollment times the probability of enrollment (since the impact of treatment on enrollment is zero). Overall this is significant for Earnings Gains and Net Value only among those coming from low-SES households. The magnitude of the impact is also twice as large for low-SES students. As a percentage of mean Net Value, the impact is economically small at 2.7% among low-SES students. As a percentage of the median potential gain in Net Value within field but across institutions displayed to treated students, it is 5.5%.

Because the impact of treatment on matriculation is zero, we can estimate the inframarginal impact of information on the earnings and cost characteristics of the enrolled degree.[25] The third panel shows the impact of treatment conditional on matriculating to some tertiary degree. Here the impact is significant overall, and again is driven primarily by low-SES students. The size of the coefficient is now larger, and around 7% of median potential gains to net value among low-SES students. Interestingly, the impact of treatment comes from earnings gains and not from tuition savings. This is in part because there is smaller variation in tuition. However the sign of the treatment effect on monthly debt is positive. This is in some ways reminiscent of returns chasing in savings investments where individuals choose higher-cost funds (with certainty) with higher past returns (see for example Choi, Laibson and Madrian 2011).

Given that returns were constrained to be calculated using mean earnings projections for graduates, we estimate whether treatment also moved students towards degrees with long-run earnings gains conditional on enrollment (rather than graduation) and their personal demographic characteristics. Treatment moves students towards a higher predicted return degree by this measure as well, though the magnitude is smaller. This is because returns conditional on enrollment and demographics consider earnings outcomes for both dropouts and graduates from each degree, while the Net Value and Earnings Gains measures focus only on graduates.

By measures of informational background

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[25] Let R denote long-run annualized real return of the degree enrolled in, let M be an indicator if a student matriculates to any tertiary degree, and let T be an indicator if the is in the treatment group. Then

$$\frac{dE(\mathrm{R})}{d(T)} = \frac{d\,\Pr(\mathrm{M}=1)}{dT}\cdot E\left(R \mid M=1\right) + \frac{dE\left(R \mid M=1\right)}{dT}\cdot \Pr(M=1)$$. (McDonald and Moffit (1980)).

Table 7 shows treatment effects by whether the respondent claimed to know at least one of tuition, expected own earnings or expected earnings for the typical graduate from their first-choice degree, versus those who chose "I don't know" for all three. Again, matriculation impacts are very close to zero for all subsamples. The largest impacts on degree choice are among low-SES students with no stated knowledge of tuition or earnings for their first choice degrees. Among this subgroup the impact is around 13.6% of the potential Net Value Gains from choosing the highest return degree in their chosen field among programs they could get into. The treatment effect is still positive and significant among those with some information and low-SES, but less than half the magnitude. Among high-SES students, effects are generally not statistically significant. However, point estimates are often larger for students in the "high information" category. One possible explanation is that lack of information on earnings and costs for high-SES students reflects limited interest in information acquisition even given low acquisition costs, for example due to family wealth and income levels, while lack of information for low-SES students reflects high acquisition costs.

By measures of preference intensity

Table 8 shows treatment effects for students whose top two stated preferences spanned more than one broad field of study (e.g. nursing and psychology instead of both nursing). We take this as a measure of preference intensity: students choosing the same field for their top two choices may be less open to new information as their preference for a particular field or degree may be stronger. Consistent with this interpretation, treatment impacts for students indicating interest in more than one field are positive, significant, and much larger in magnitude than those for students expressing interest in only one field. The impacts again appear concentrated among low-SES students. This indicates that by the time of loan application, some students are set in their choices regardless of information. Note that low-SES students choose on average the same number of fields (2.12) as high-SES students (1.96), and those who choose more than one field are not substantially more likely to have responded "I Don't Know" to questions about tuition and earnings expectations.

We can also measure preference intensity more directly. Table 9 cuts the data by those who say they are absolutely certain of their application choices and those who say they are not absolutely certain. Recall that certainty is not highly correlated with SES nor with the number of fields listed (34.4% are absolutely certain for low-SES vs 32.7% for high-SES on average, absolutely sure list 2.64 fields on average while those with some uncertainty list 2.64). Table 9 shows that treatment effects are near zero among those who express certainty, but positive and significant among those who are not completely certain and are from low-SES backgrounds. Again, gains to net value are driven by gains in expected earnings rather than lower debt.

By loan access

We have established that low-income students are more impacted by information than higher-income students. Table 10 splits the sample by whether or not the student received a loan. Loan receipt does not cause treatment as it is determined by GPA and PSU scores and parental income (Appendix Table A.4 shows this statistically). However, loan receipt does impact treatment effects. Overall and particularly among low-SES students, those who do not receive a loan have positive, significant and substantial treatment effects. The treatment effect on Net Value of degree enrolled in is 11% of the median potential gain within field displayed to treated students, and 12% among the low-SES sub-population. Among high-SES students treatment point estimates are considerably larger for those who do not receive loans, though they are not significant.

One reason that the loan-receiving population may be less responsive to information in choice of degree to enroll in is that many qualify for CRUCH options where admission is uncertain and based on score cutoffs that are unknown at the time of application. We split the sample further to examine only students who scored below 475 on their PSU and do generally not qualify for CRUCH admissions. These students often matriculate to private universities, technical and professional degrees with little stringent selection. Among these students we still see that treatment effects are driven by the low-SES sub-population who do not receive loans.

As was the case in the full sample of respondents, the treatment effects on net earnings for students who are not loan takers do not come from tuition costs savings, but from higher earnings gains. This implies that without a loan, treated students pay more attention to potential earnings, but not to costs. This may be because they are more worried about paying off alternative funding sources (perhaps private loans at higher interest rates). It could also be because students, particularly from low-income backgrounds, treat loan dollars more like grants than out-of-pocket costs. There is growing evidence that individuals do not calculate loan costs correctly, and that they also treat dollars gained in particular domains differently (e.g. a dollar of funding for tertiary education is treated as a large wealth shock in that domain). It may be that both effects are at work among the low-SES population. Either way, information appears to have the largest impact among those who do not receive loans, which suggest that information may not be a successful way of nudging students to profitable educational investments when taking out a student loan.

By additional student characteristics

We also consider effect heterogeneity by student characteristics that are important predictors of enrollment outcomes but do not map as clearly to model inputs or policy questions. Table 11 shows the impact of treatment by whether at least one parent has some tertiary education coursework completed as reported in student surveys on the high school standardized tests. Though the results are noisy, students with a parent with at least some college have tend to have higher probabilities of significant treatment effects and/or larger effect sizes. One explanation is that although students with more educated parents may have access to more information about their college choices, these students are also more open to modifying their behavior in response to new information they receive.

In Table 12 we examine whether currently graduating students are more open to information and changing their choices relative to “older” loan applicants (1 to 2 years out of high school). It appears that this is the case; overall fresh high school grads have larger, more significant impacts. The impacts are slightly larger and more significant in the low-SES subpopulation. The low impacts for high-SES students are driven by near-zero impacts among the older cohorts. The fact that older students are less responsive to information is worrisome as recent research has shown that they are more likely to drop out of school and default on student debt, particularly coming from low-SES backgrounds (Deming, Goldin and Katz 2012). They may feel they have a stronger idea of what they want to do, though they may not in fact be making an informed decision.

5 Conclusion

We administered a survey and field experiment in partnership with the Chilean Ministry of Education (MINEDUC) as part of the 2013 student loan application process. The goal of this intervention was to test the impact of information about institution- and field of study-specific earnings and debt outcomes on higher education matriculation choices for students from low-income backgrounds. Our randomized controlled trial directly tests a government-implemented demand-side intervention to improve educational choices and outcomes for students in Chile’s higher-education market.

We find that many students have limited knowledge of the earnings and cost outcomes associated with different degree programs. Information on these topics is particularly lacking among students from low-SES backgrounds. These knowledge gaps are reflected in enrollment decisions: low-SES students systematically choose degrees that yield lower expected returns conditional on background and ability than high-SES students. These descriptive results are consistent with the idea that low-income students are not fully informed about their choice options and could benefit from an intervention conveying this information.

Upon completing the survey, randomly-selected students were given and information treatment to measure the potential impact on their choices. They were shown information on actual earnings gains (versus no tertiary enrollment) in monthly terms, tuition costs in monthly payments, and a "net value" which was the difference in monthly gains and payments in pesos. Costs and benefits were amortized over the fifteen year loan repayment term. Treated applicants were shown expected gains from searching, and allowed access to a searchable database which, upon selecting a major and an entrance exam score, populated a table of relevant enrollment options sorted in descending order by net value. Following the intervention, we tracked students in the treatment and control groups to see whether and where they chose to matriculate.

We find that treatment has a significant and positive impact mainly among low-SES students. However, the effect is small in magnitude, moving net value of the enrolled degree by approximately 7% of potential gains. In line with predictions, information has larger effects for students who have less information on earnings and cost and who exhibit lower levels of pre-intervention preference for a given degree program or program type. Among these subgroups of low-SES students, effect sizes are roughly twice as large.

Treatment effects are strongest among low-SES students who do not receive federal student loans (which are merit and need based and not caused by treatment). Among applicants qualifying only for non-selective technical institutes, private universities and professional degree programs, treatment effects are positive and significant and large only for those who do not receive a loan. Given that this area is one in which returns to education are possibly negative (earnings gains do not justify costs), this result suggests that information may not be helpful in nudging loan applicants to make wiser investment choices that will allow them to repay their student loans.

Finally, we find that in all cuts of the data, gains in the predicted net present value of the chosen degree are generated by higher returns rather than lower tuition costs, suggesting that demand response to information could chase returns estimates rather than put pressure on tuition, even if costs and earnings gains presented separately. Both results may be due to lack of financial literacy and poor understanding of loan terms measured in other surveys we conducted of student loan takers (Hastings et al. 2014).

These findings suggest that simply providing information on returns by institution and degree is unlikely to substantially help those most in need – those from low-income households and those taking out personal student loans to finance their higher education investments. While our main impacts are largest among loan applicants from low-SES backgrounds who do not receive loans, we also show that those from low-SES backgrounds are harder to reach with information, even if it is disseminated as part of the loan application process and by the government.

Finally, the small overall effect sizes and the treatment effect loading on higher earnings returns instead of lower tuition costs suggest that information is unlikely to discipline tuition just as it may not discipline management fees in markets for financial investments. Alternatively, if designed correctly, capping loans smoothly and in a way that directs students with limited information and decision-making ability towards higher value investments may be a more effective policy than providing information for nudging students toward financially viable choices. Beyer et al.(2014) discuss a loan cap policy we designed in collaboration with MINEDUC as a result of this research and integrated into the Chilean loan cap policy for the 2014 school year.

Finally we note that information could be more impactful if distributed regularly and earlier as part of secondary education as well as distributed at the time of loan and enrollment choice (Dinkelman and Martínez, 2014). However, we note that based on estimates of factors that impact past returns from our extensive data, the largest contributor to overall socio-economic status gaps in higher education is the gap in college preparation (as measured by entrance exam scores) between high and low-SES students. Selectivity is the primary driver of large returns to secondary education. Policies that increase the fraction of low-SES students qualifying for admissions to highly selective programs offer the largest potential for increasing upward mobility.

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Table 1. Comparison of Survey Sample Invitees, Opened Email, Consenting Sample & Respondents

	(1) Invited Sample	(2) Opened	(3) Consent	(4) Respondents	(5) Treated	(5) Treated & Searched
PSU Score	508.4	518.8	524.5	536.3	537.0	543.4
(Ave. Language & Math)	(155,167)	(101,736)	(72,474)	(47,568)	(23,402)	(10,339)
Municipal High School	37.80%	37.00%	36.50%	33.10%	32.50%	32.40%
	(164,798)	(114,398)	(83,346)	(49,166)	(24,162)	(10,448)
Private or Voucher High School	61.80%	60.90%	60.80%	66.60%	67.20%	67.30%
	(164,798)	(114,398)	(83,346)	(49,166)	(24,162)	(10,448)
Mother Some Tertiary Edu.	25.80%	26.80%	26.90%	29.80%	30.30%	29.50%
	(130,324)	(85,134)	(60,616)	(40,744)	(20,041)	(8,725)
Father Some Tertiary Edu.	27.20%	28.40%	28.60%	31.70%	32.00%	32.00%
	(126,082)	(82,449)	(58,722)	(39,511)	(19,439)	(8,452)
Low – SES school	43.70%	43.30%	43.20%	35.70%	34.50%	34.30%
	(153,706)	(105,441)	(76,476)	(46,444)	(22,680)	(9,891)
SIMCE Math (Z-score)	0.321	0.405	0.454	0.560	0.575	0.626
	(123,741)	(79,389)	(56,172)	(38,563)	(18,947)	(8,268)
SIMCE Language (Z-score)	0.332	0.423	0.477	0.577	0.587	0.644
	(123,740)	(79,373)	(56,159)	(38,554)	(18,942)	(8,267)
Female	55.40%	57.30%	58.20%	57.50%	56.50%	58.90%
	(164,786)	(114,265)	(83,215)	(49,166)	(24,162)	(10,448)
Delayed College Entrance	26.40%	36.40%	39.60%	24.50%	24.50%	26.50%
	(164,798)	(114,398)	(83,346)	(49,166)	(24,162)	(10,448)
Total Observations	164,798	114,398	83,346	49,166	24,162	10,448

Notes: Calculations are based on survey responses linked to administrative data from the Chilean Ministry of Education (Mineduc). The number of observations for each calculation are in parentheses. The "Invited Sample" is all November 2012 FUAS Applicants for the 2013 school year for whom we had a valid email address to send our survey invitation. The "Opened" sample is the subset of our Invited Sample who opened the survey invitation email. The "Consent" sample is the subset of those who opened the email and also consented to complete the survey. The "Respondents" are those who consented to complete the survey, completed all 6 questions in the survey, and graduated high school between 2009-2012. The "Treated" are those who were randomly assigned to be treated with degree information upon completion of the survey. The "Treated & Searched" are those who were treated with information who also searched for alternative degrees after being shown information about their first choice degree and a suggested institution and degree. PSU scores are the most recent PSU scores on record for the student. The type of high school (municipal, private, voucher) is from the 2012 high-school (RBD) graduation (source: Mineduc). Mother and Father having some tertiary education is defined if the mother/father have any higher education, as reported by the student in the national standardized test, SIMCE. Low-SES is defined as coming from a high school (RBD) in one of the two highest poverty categories as defined by Mineduc. SIMCE scores are results from standardized high school test scores that were nationally administered to all students enrolled in the 10th grade in 2001, 2003, 2006, 2008, and 2010, normalized within each testing year. Delayed college represents those that were not directly coming from high school; those who graduated high school prior to 2012.

Table 2. Error in Estimating Tuition Costs by RBD Poverty Rating

RBD Rating	"I Don't Know"	Obs. "I don't know"	Mean Error	Abs. Mean Error	P25	P50	P75	Obs. Tuition Error
A	39.60%	3,162	13.58%	47.52%	-19.21%	-5.18%	8.43%	1,374
B	36.30%	13,432	4.87%	35.47%	-18.87%	-5.44%	7.17%	6,051
C	33.90%	16,555	-1.86%	27.44%	-17.99%	-5.84%	4.32%	8,490
D	27.70%	10,177	-1.71%	21.98%	-13.95%	-5.13%	5.12%	6,233
E	23.70%	3,118	-4.10%	19.03%	-14.55%	-5.45%	2.94%	2,101
All	33.20%	49,166	0.94%	29.10%	-16.51%	-5.48%	5.48%	25,358

Notes: This table displays the results from Q4 in our survey (P3E 2012). The question and text response options are available in the Appendix. Respondents were asked to enter the annual tuition costs of their first choice career. The percentage difference between their response for tuition and actual tuition for their first choice career is calculated only for those that did not choose the option "I don't know". RBD Poverty Ratings are the poverty ratings for each school, produced by Mineduc. A is the highest poverty level, B the next highest, and E is the lowest poverty rating.

Table 3. Difference in Expected Earnings vs. Average Earnings

% Difference of Own Expected Earnings vs. Actual Average Earnings

RBD Rating	"I don't know" Own	N	Mean Error	Abs. Mean Error	P25	P50	P75	N Own
A	44.20%	3,162	71.30%	125.80%	-27.00%	7.80%	63.80%	865
B	40.30%	13,432	70.70%	109.40%	-24.20%	6.50%	55.50%	4,495
C	34.70%	16,555	48.40%	82.50%	-24.80%	0.90%	43.30%	6,624
D	30.20%	10,177	33.30%	67.40%	-26.50%	-1.40%	37.30%	4,795
E	29.00%	3,118	28.10%	57.90%	-26.20%	-0.60%	39.20%	1,605
ALL	35.80%	49,166	51.80%	87.90%	-25.00%	2.40%	45.60%	19,274

% Difference of Typical Expected Earnings vs. Actual Average Earnings

RBD Rating	"I don't know" Typical Earnings	N	Mean Error	Abs. Mean Error	P25	P50	P75	N Typical
A	59.50%	3,162	100.70%	101.70%	-20.90%	17.40%	90.00%	955
B	53.20%	13,427	85.00%	98.70%	-19.90%	15.40%	72.20%	4,911
C	46.70%	16,554	56.00%	77.00%	-23.20%	7.10%	52.60%	7,123
D	40.20%	10,174	37.60%	62.80%	-26.70%	-0.70%	41.00%	5,097
E	37.30%	3,118	25.20%	57.80%	-31.70%	-1.00%	39.50%	1,687
ALL	47.70%	49,155	60.90%	80.30%	-23.60%	7.80%	56.50%	20,738

% Difference of Own Expected Earnings vs. Typical Expected Earnings

RBD Rating	"I don't know" Both	N	Mean Error	Abs. Mean Error	P25	P50	P75	N Both
A	40.50%	3,162	8.00%	41.30%	-28.60%	-8.00%	13.10%	1,164
B	36.30%	13,427	9.90%	39.00%	-25.00%	-0.70%	16.70%	5,739
C	31.00%	16,554	17.40%	42.50%	-22.00%	0.00%	17.00%	8,197
D	26.40%	10,174	11.10%	32.70%	-18.80%	0.00%	20.00%	5,697
E	25.90%	3,118	13.00%	32.30%	-13.80%	0.00%	25.00%	1,859
ALL	32.00%	49,155	13.10%	38.50%	-22.20%	0.00%	18.90%	23,824

Note: This table presents the results from Q5 in P3E 2012. See the Appendix for question text and response options. Differences in own or typical expected earnings as compared to the average earnings for graduates in their first choice degrees are calculated only for those that did not choose the "I don't know" response option. Own earnings are what the respondent expects to earn after graduating and finding a stable job from their first choice degree. Average earnings were calculated using tax records of previous graduates in the second year after graduating from the respondent's first choice degree. Degrees for which earnings data for graduates was unavailable have corresponding actual average earnings set to missing. RBD Poverty Ratings are the poverty ratings for each high school as determined by Mineduc. A is the highest poverty level, B the next highest, and E is the lowest poverty rating.

Table 4. Degree Choice Certainty by RBD Poverty Rating and Older vs. Younger Graduates

	N	Absolutely Certain	Pretty Sure	Fairly Sure	Somewhat Sure	Not At All Sure
By RBD Poverty Rating:						
A	3,162	34.30%	31.30%	23.40%	8.60%	2.40%
B	13,432	34.40%	33.40%	22.80%	6.60%	2.70%
C	16,555	32.70%	35.20%	22.70%	6.80%	2.60%
D	10,177	32.20%	36.20%	22.80%	6.00%	2.90%
E	3,118	34.30%	38.30%	19.30%	6.10%	2.10%
All	49,166	33.80%	34.60%	22.30%	6.60%	2.60%
By HS Graduation Year:						
Current	37,111	33.10%	34.60%	22.80%	6.90%	2.70%
Older	12,055	36.10%	34.60%	21.10%	5.80%	2.40%
All	49,166	33.80%	34.60%	22.30%	6.60%	2.60%

Notes: This table presents the results from Q3 in the survey P3E 2012. Question text and response options are available in the Appendix. Respondents were asked how certain they were that the degrees they listed in their top three choices in Q2 would be the degrees that they would be applying to. RBD Poverty Ratings are the poverty ratings for each high school as determined by Mineduc. A is the highest poverty level, B the next highest, and E is the lowest poverty rating. Current high-school graduates are those who graduated high school in 2012. Older high-school graduates are those who graduated high-school between 2009-2011.

Table 5. PSU Overconfidence by RBD Rating

								Unsure		Absolutely Certain	
RBD Rating	N	Mean	P10	P25	P50	P75	P90	Mean	N	Mean	N
A	2,417	57.2	-34.0	8.5	55.0	105.5	156.0	54.4	1,643	63.3	774
B	10,980	49.0	-34.0	5.0	45.0	91.5	143.0	46.3	7,354	54.4	3,626
C	14,531	41.9	-23.0	5.0	37.5	75.0	114.5	40.1	9,894	45.7	4,637
D	9,286	33.6	-23.0	2.0	30.5	63.0	95.5	31.6	6,346	38.0	2,940
E	2,905	29.1	-20.5	1.0	25.5	55.0	85.5	28.9	1,929	29.4	976
All	42,125	42.9	-26.0	4.5	38.0	77.5	122.0	40.8	28,367	47.1	13,758

Notes: This table presents results from Q6 in P3E 2012. Question text is available in the Appendix. Respondents were asked to enter their expected PSU score. These responses were then linked with administrative data of actual PSU score results. The last four columns present statistics for whether the respondent answered Q3 that they were "absolutely certain" (vs. any other response – "Unsure") that the degrees they listed as choices in Q2 were the degrees that they would be applying to. RBD Poverty Ratings are the poverty ratings for each high school as determined by Mineduc. A is the highest poverty level, B the next highest, and E is the lowest poverty rating.

Table 6. Impact of Treatment on Outcome Variables

	Pooled	Low-SES	High-SES
Matriculate Anywhere	0.004	0.000	0.003
	(0.004)	(0.005)	(0.006)
	49,166	*16,594*	*29,850*
All Students			
Net Value	8,041	11,276*	5,204
	(6,276)	(5,403)	(10,114)
	34,848	*11,513*	*21,524*
Earnings Gains	8,639	11,845*	5,723
	(6,862)	(6,039)	(11,051)
	34,914	*11,544*	*21,552*
Monthly Debt	240.9	437.5	-6.871
	(634)	(663)	(977)
	47,007	*16,003*	*28,403*
Conditional on Matriculation			
Net Value	9,763**	15,782**	7,740
	(3,726)	(5,269)	(4,954)
	25,577	*8,168*	*16,154*
Earnings Gains	10,716**	16,630**	8,783
	(4,022)	(5,598)	(5,334)
	25,621	*8,187*	*16,174*
Monthly Debt	369.5	888.7	85.13
	(494)	(703)	(609)
	35,764	*11,781*	*22,085*
LR PV Earnings Gains (Enrollees)	2,893	5,765*	1,364
	(1,663)	(2,417)	(2,109)
	28,136	*9,094*	*19,042*
SR PV Earnings Gains (Enrollees)	653.2***	995.4***	477.4
	(198)	(254)	(260)
	28,136	*9,094*	*19,042*

Notes: Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7. Treatment Effects by Guessing Earnings & Tuition

	Pooled		Low-SES		High-SES	
	Some Info.	No Info.	Some Info.	No Info.	Some Info.	No Info.
Matriculate Anywhere	0.002	0.009	-0.003	0.010	0.004	-0.005
	(0.005)	(0.009)	(0.005)	(0.014)	(0.007)	(0.013)
	40,642	*8,513*	*13,184*	*3,405*	*25,317*	*4,529*
All Students						
Net Value	6,735	14,251	7,450	25,751*	6,498	-2,501
	(7,220)	(8,251)	(6,701)	(10,926)	(10,586)	(11,021)
	28,990	*5,852*	*9,216*	*2,293*	*18,339*	*3,183*
Earnings Gains	7,268	15,149	8,021	26,260*	7,074	-2,441
	(7,991)	(9,073)	(7,502)	(12,211)	(11,702)	(11,865)
	29,035	*5,873*	*9,238*	*2,302*	*18,357*	*3,193*
Monthly Debt	-30	1,473	102	1,526	-71	391
	(797)	(950)	(799)	(1,107)	(1,152)	(1,392)
	38,855	*8,143*	*12,725*	*3,273*	*24,081*	*4,319*
Conditional on Matriculation						
Net Value	9,421*	13,034	13,069*	30,493**	9,109	-443
	(3,968)	(9,393)	(6,203)	(10,551)	(5,009)	(11,451)
	21,597	*3,976*	*6,677*	*1,488*	*13,905*	*2,248*
Earnings Gains	10,325*	14,422	13,966*	31,388**	10,099	1,072
	(4,348)	(9,865)	(6,699)	(11,248)	(5,443)	(11,748)
	21,628	*3,989*	*6,691*	*1,493*	*13,919*	*2,254*
Monthly Debt	196	1,405	730	1,558	-4	901
	(570)	(956)	(809)	(1,202)	(681)	(1,236)
	29,934	*5,823*	*9,544*	*2,233*	*18,880*	*3,203*
LR PV Earnings Gains (Enrollees)	1,760	9,531**	3,848	15,036***	651	5,926
	(1,818)	(3,312)	(2,765)	(3,834)	(2,299)	(3,752)
	23,823	*4,307*	*7,432*	*1,658*	*16,391*	*2,649*
SR PV Earnings Gains (Enrollees)	504.0*	1,408***	878.1**	1,401*	323	1,406**
	(220)	(397)	(291)	(620)	(295)	(482)
	23,823	*4,307*	*7,432*	*1,658*	*16,391*	*2,649*

Notes: "At Least 1" is defined if the respondent guessed at least one of the following values: tuition, own expected earnings and typical expected earnings in the survey. "No Guesses" is defined if the respondent answered "I don't know" for all three value expectations. Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8. Chose More Than One Field

	Pooled		Low-SES		High-SES	
	1 Field	> 1 Field	1 Field	> 1 Field	1 Field	> 1 Field
Matriculate Anywhere	0.002	0.004	-0.009	0.001	0.002	0.004
	-0.004	-0.006	-0.011	-0.007	-0.005	-0.010
	19,555	*25,409*	*5,385*	*9,540*	*13,288*	*14,381*
All Students						
Net Value	-2,863	16,145	-12,240	17,122*	-1,316	15,411
	(5,562)	(9,380)	(9,610)	(6,767)	(7,502)	(15,972)
	14,194	*17,698*	*3,795*	*6,566*	*9,806*	*10,152*
Earnings Gains	-2,800	17,049	-13,442	17,864*	-828	16,442
	(6,222)	(10,159)	(10,481)	(7,529)	(8,405)	(17,261)
	14,217	*17,739*	*3,807*	*6,584*	*9,813*	*10,172*
Monthly Debt	-307	688	-422	709	-474	701
	(709)	(821)	(1,162)	(723)	(980)	(1,278)
	18,916	*24,093*	*5,225*	*9,176*	*12,845*	*13,500*
Conditional on Matriculation						
Net Value	373	15,709*	627	22,344**	-515	14,508
	(5,559)	(6,288)	(8,366)	(7,368)	(7,450)	(9,456)
	10,495	*12,861*	*2,686*	*4,615*	*7,398*	*7,577*
Earnings Gains	1,208	16,541*	532	23,404**	774	15,320
	(6,210)	(6,643)	(9,091)	(7,828)	(8,328)	(10,002)
	10,509	*12,890*	*2,693*	*4,626*	*7,403*	*7,592*
Monthly Debt	-164	643	489	1190	-505	497
	(723)	(577)	(1,146)	(799)	(893)	(759)
	14,585	*18,132*	*3,896*	*6,664*	*10,070*	*10,444*
LR PV Earnings Gains (Enrollees)	1,690	4,265	-2,739	11,212***	3,323	97
	(2,403)	(2,655)	(3,470)	(2,836)	(2,767)	(3,484)
	11,783	*14,084*	*3,031*	*5,146*	*8,752*	*8,938*
SR PV Earnings Gains (Enrollees)	348	1,023**	-32	1,782***	434	635
	(285)	(383)	(481)	(402)	(338)	(550)
	11,783	*14,084*	*3,031*	*5,146*	*8,752*	*8,938*

Notes: "1 Field" is defined if the respondent only listed one field of study in their three choices in Q2. ">1 Field" is defined if the respondent listed more than one field choice in their three degree choices in Q2. Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 9. Absolutely Certain About Career Choice

	Pooled		Low-SES		High-SES	
	Unsure	Certain	Unsure	Certain	Unsure	Certain
Matriculate Anywhere	0.007	-0.002	0.005	-0.007	0.005	-0.002
	(0.005)	(0.007)	(0.008)	(0.010)	(0.006)	(0.011)
	32,534	*16,632*	*10,881*	*5,713*	*20,088*	*9,762*
All Students						
Net Value	11,922	131	16,185*	3,371	9,227	-4,071
	(6,753)	(9,339)	(6,549)	(7,927)	(10,420)	(15,142)
	22,840	*12,008*	*7,424*	*4,089*	*14,386*	*7,138*
Earnings Gains	12,745	258	17,166*	3,254	9,945	-4,076
	(7,353)	(10,374)	(7,327)	(8,960)	(11,317)	(16,756)
	22,889	*12,025*	*7,447*	*4,097*	*14,407*	*7,145*
Monthly Debt	388	-21	910	-300	-26	-14
	(692)	(1,154)	(1,055)	(1,117)	(923)	(1,778)
	31,023	*15,984*	*10,469*	*5,534*	*19,070*	*9,333*
Conditional on Matriculation						
Net Value	12,921**	3,540	21,010**	7,422	9,737	3,225
	(4,908)	(5,903)	(6,588)	(6,988)	(6,591)	(8,666)
	16,285	*9,292*	*5,036*	*3,132*	*10,580*	*5,574*
Earnings Gains	13,777*	4,663	22,090**	7,952	10,448	4,899
	(5,378)	(6,464)	(7,196)	(7,555)	(7,222)	(9,318)
	16,319	*9,302*	*5,050*	*3,137*	*10,595*	*5,579*
Monthly Debt	84	939	1,203	438	-466	1,224
	(640)	(780)	(1,103)	(891)	(802)	(977)
	23,109	*12,655*	*7,443*	*4,338*	*14,627*	*7,458*
LR PV Earnings Gains (Enrollees)	3,947*	1,145	8,139**	1,698	1,761	941
	(1,988)	(3,013)	(2,544)	(3,873)	(2,638)	(3,990)
	18,238	*9,898*	*5,679*	*3,415*	*12,559*	*6,483*
SR PV Earnings Gains (Enrollees)	937.5***	171	1,333***	448	750.2*	-1.426
	(270)	(284)	(316)	(381)	(363)	(408)
	18,238	*9,898*	*5,679*	*3,415*	*12,559*	*6,483*

Notes: "Unsure" is defined if the respondent answered any of the options other than "I am absolutely certain" when asked in Q3 how certain they were that they would be applying to their listed degree choices. "Certain" is defined if the respondent answered "I am absolutely certain" in response to Q3. Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * p < 0.05, ** p < 0.01, *** p < 0.001.

Table 10. Approved for Loan

	Pooled		Low-SES		High-SES	
	No Loan	Loan	No Loan	Loan	No Loan	Loan
Matriculate Anywhere	0.007	0.003	-0.001	-0.003	0.004	0.005
	(0.009)	(0.004)	(0.012)	(0.006)	(0.016)	(0.006)
	12,251	*36,915*	*5,632*	*10,962*	*5,370*	*24,480*
All Students						
Net Value	19,726*	5,794	12,885	7,836	21,803	4,918
	(7,733)	(7,884)	(9,727)	(8,476)	(14,090)	(11,005)
	8,431	*26,417*	*3,859*	*7,654*	*3,742*	*17,782*
Earnings Gains	21,321*	6,381	12,201	8,688	24,959	5,399
	(8,422)	(8,712)	(10,644)	(9,468)	(15,303)	(12,191)
	8,462	*26,452*	*3,876*	*7,668*	*3,753*	*17,799*
Monthly Debt	875	338	-147	785	1,437	52
	(826)	(722)	(775)	(927)	(1,469)	(1,042)
	11,803	*35,204*	*5,457*	*10,546*	*5,149*	*23,254*
Conditional on Matriculation						
Net Value	22,692*	7,667	26,980**	10,295	20,327	6,967
	(9,212)	(4,153)	(10,025)	(6,679)	(17,637)	(5,294)
	4,344	*21,233*	*1,842*	*6,326*	*2,085*	*14,069*
Earnings Gains	24,644*	8,509	26,362*	11,280	22,856	7,907
	(9,775)	(4,575)	(10,602)	(7,355)	(18,789)	(5,774)
	4,360	*21,261*	*1,850*	*6,337*	*2,090*	*14,084*
Monthly Debt	998	329	-256	1,124	1,270	-12
	(797)	(528)	(818)	(862)	(1,347)	(651)
	6,568	*29,196*	*2,836*	*8,945*	*3,067*	*19,018*
LR PV Earnings Gains (Enrollees)	4,246	2,829	7,870**	4,983	-346	1,815
	(2,882)	(1,797)	(2,880)	(3,108)	(4,332)	(2,166)
	4,342	*23,794*	*1,939*	*7,155*	*2,403*	*16,639*
SR PV Earnings Gains (Enrollees)	815.6*	628.9**	1,307**	857.0**	102	539.6*
	(369)	(223)	(467)	(326)	(570)	(273)
	4,342	*23,794*	*1,939*	*7,155*	*2,403*	*16,639*

Notes: "No Loan" indicates that the respondent was not approved for a loan per our administrative files of FSCU and CAE loans. "Loan" indicates that the respondent was approved for a loan. Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 11. Treatment Effects by Parents' Education: Some College Education

	Pooled		Low-SES		High-SES	
	No College	At Least 1	No College	At Least 1	No College	At Least 1
Matriculate Anywhere	0.002	0.003	0.003	0.008	(0.000)	0.003
	(0.006)	(0.006)	(0.007)	(0.014)	(0.010)	(0.007)
	23,010	*16,355*	*10,558*	*2,227*	*11,507*	*13,524*
All Students						
Net Value	7,411	11,587	13,885	32,326	-170.4	9,493
	(6,632)	(10,738)	(7,862)	(23,664)	(13,017)	(12,582)
	16,119	*11,851*	*7,369*	*1,529*	*8,138*	*9,925*
Earnings Gains	7,914	12,329	14,686	35,489	-65.58	10,153
	(7,275)	(11,742)	(8,569)	(25,899)	(14,113)	(13,737)
	16,159	*11,863*	*7,393*	*1,532*	*8,152*	*9,933*
Monthly Debt	469.9	92.41	698.3	1,840	129.2	26.43
	(618)	(1,027)	(705)	(2,021)	(1,050)	(1,198)
	22,081	*15,549*	*10,196*	*2,143*	*10,978*	*12,830*
Conditional on Matriculation						
Net Value	10,084	14,436**	18,233*	37,898	4,976	11,992*
	(5,507)	(5,168)	(8,927)	(21,349)	(8,937)	(5,393)
	11,773	*8,851*	*5,244*	*1,121*	*6,095*	*7,447*
Earnings Gains	10,752	16,034**	18,412	42,537	6,368	13,166*
	(5,874)	(5,725)	(9,529)	(23,139)	(9,285)	(6,019)
	11,801	*8,858*	*5,258*	*1,122*	*6,107*	*7,452*
Monthly Debt	540.9	507.0	567.5	3,115	592.4	196.4
	(482)	(909)	(788)	(1,763)	(610)	(1,017)
	16,698	*12,079*	*7,502*	*1,647*	*8,522*	*9,986*
LR PV Earnings Gains (Enrollees)	4,176	2,914	5,650	17,330**	2,804	1,009
	(2,421)	(2,265)	(3,609)	(6,356)	(3,211)	(2,319)
	12,964	*10,102*	*5,792*	*1,300*	*7,172*	*8,802*
SR PV Earnings Gains (Enrollees)	735.6**	604.7*	949.4**	2051*	615	397
	(271)	(266)	(351)	(819)	(407)	(269)
	12,964	*10,102*	*5,792*	*1,300*	*7,172*	*8,802*

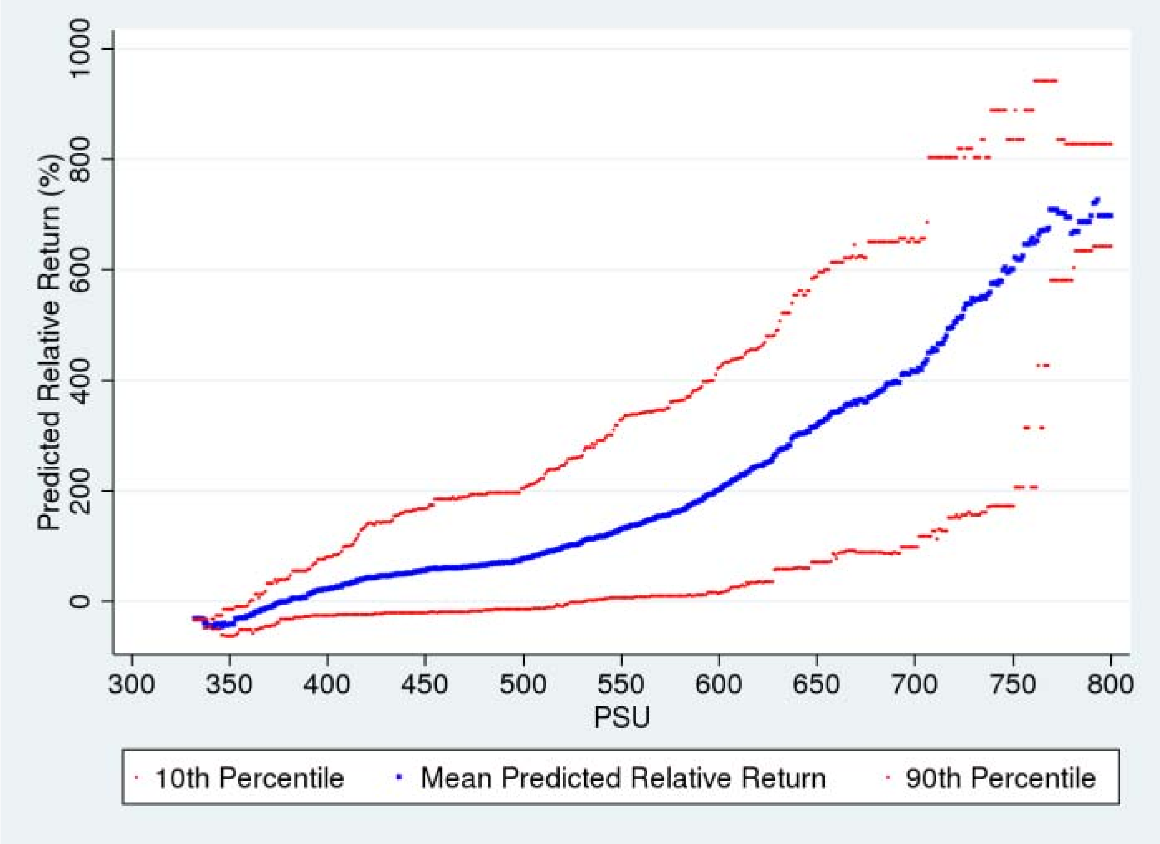
Notes: "At Least 1" is defined if at least one of the student's parents have some tertiary education, as reported by the students during SIMCE standardized tests. "No College" is defined if neither of the respondent's parents have any tertiary education. Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 12. Older vs. Younger HS Graduates

	Pooled		Low-SES		High-SES	
	Younger	Older	Younger	Older	Younger	Older
Matriculate Anywhere	0.007	-0.006	0.002	-0.005	0.005	-0.005
	(0.004)	(0.007)	(0.006)	(0.012)	(0.008)	(0.008)
	37,111	*12,055*	*12,613*	*3,981*	*22,375*	*7,475*
Conditional on Matriculation						
Net Value	11,250	-2,255	10,089	14,805	9,750	-8,693
	(7,784)	(7,936)	(6,118)	(12,673)	(12,633)	(10,341)
	26,466	*8,382*	*8,815*	*2,698*	*16,240*	*5,284*
Earnings Gains	12,077	-2,308	10,488	16,004	10,551	-8,959
	(8,493)	(8,721)	(6,893)	(13,850)	(13,788)	(11,377)
	26,517	*8,397*	*8,843*	*2,701*	*16,259*	*5,293*
Monthly Debt	473	-274	382	683	207	-546
	(790)	(863)	(792)	(1,257)	(1,232)	(1,161)
	35,463	*11,544*	*12,183*	*3,820*	*21,249*	*7,154*
All Students						
Net Value	12,124**	3,384	15,486*	16,618	11,103	-951
	(4,345)	(6,786)	(5,898)	(11,859)	(5,705)	(8,429)
	18,715	*6,862*	*6,083*	*2,085*	*11,680*	*4,474*
Earnings Gains	13,081**	4,363	16,360*	17,395	12,033	391
	(4,710)	(7,248)	(6,244)	(12,651)	(6,203)	(8,988)
	18,745	*6,876*	*6,100*	*2,087*	*11,691*	*4,483*
Monthly Debt	258	778	619	1,661	-53	495
	(644)	(722)	(870)	(1,094)	(801)	(943)
	26,030	*9,734*	*8,709*	*3,072*	*15,878*	*6,207*
LR PV Earnings Gains (Enrollees)	3,176	2,339	5,588	6,338	1,562	1,106
	(2,046)	(2,784)	(3,036)	(4,057)	(2,602)	(3,561)
	20,342	*7,794*	*6,645*	*2,449*	*13,697*	*5,345*
SR PV Earnings Gains (Enrollees)	705.7**	516	867.5**	1,360*	592	202
	(231)	(376)	(303)	(539)	(299)	(485)
	20,342	*7,794*	*6,645*	*2,449*	*13,697*	*5,345*

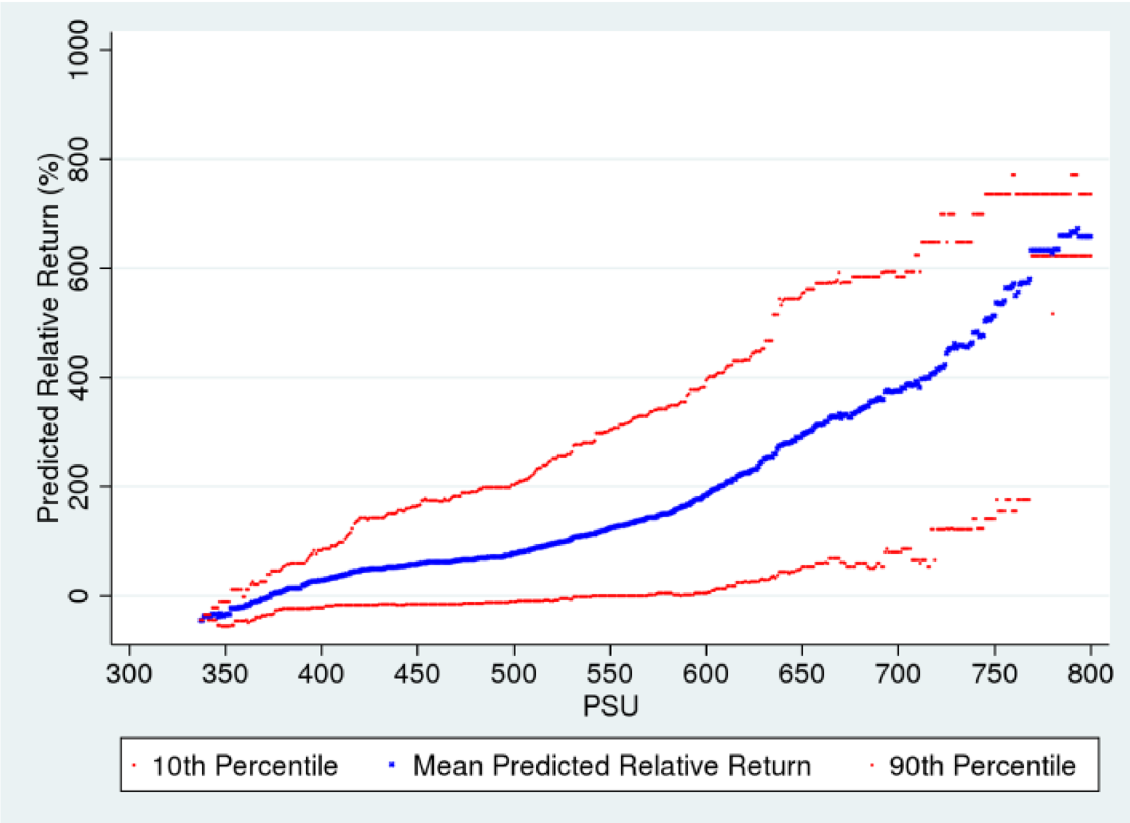
Notes: "Younger" is defined if the respondent graduates high school in 2012. "Older" is defined if the respondent graduated high school in 2009-2011. Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Sample sizes are reported in italics. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 10 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third panel reports intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR PV Earnings Gains are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to 25 years for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR PV Earnings Gains calculate predicted earnings for experience years 1-7 and deduct no-college earnings for 7 experience years. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 1. Long-Run Relative Returns to Degree



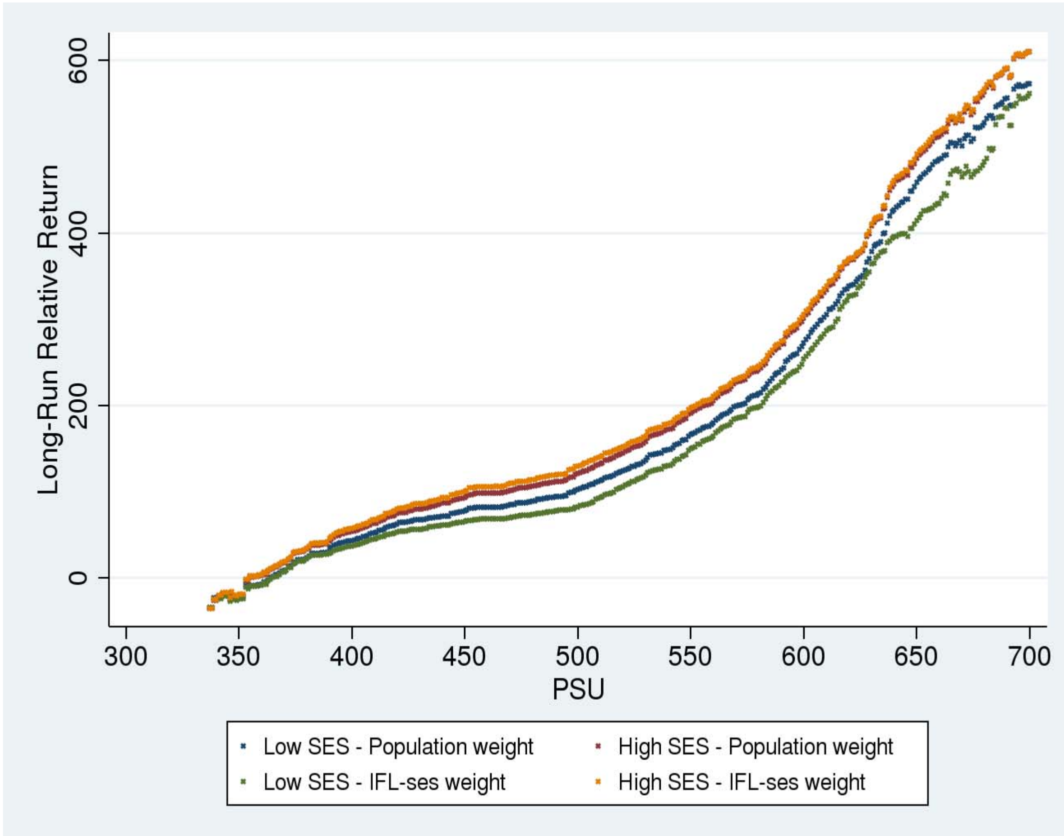
Notes: Returns are calculated as a net present discounted value return over not going to college. They include opportunity cost of time and tuition cost at the expected degree length. The returns are therefore the net present value of predicted earnings for each student from enrolling in their observed degree out to age 50 (assuming that the student enrolls at age 18), less expected tuition costs and the present discounted value of earnings had they entered the work force after high school graduation, divided by the present discounted value of earnings had they entered the work force after high school graduation. Thus a value of 100 means that someone is expected to earn twice (in present discounted value) the amount they would earn if they did not enroll in college. A negative value indicates that tuition costs and opportunity cost of time exceed future expected earnings gains post-enrollment. The figure shows the distribution of returns that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted returns for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of historic enrollment. We assume that a student scoring X's relevant degree choice set consists of the set of degrees for which his or her PSU score is in the 25th to 90th percentile of the historic range of admittees to that degree. The y axis value gives the enrollment-weighted mean expected earnings gains for students with a PSU of X over the degrees they could get into.

Figure 2. Short-Run Relative Returns to Degree



Notes: Returns are calculated as a net present discounted value return over not going to college. They include opportunity cost of time and tuition cost at the expected degree length. The returns are therefore the net present value of predicted earnings for each student from enrolling in their observed degree out to age 30 (assuming that the student enrolls at age 18), less expected tuition costs and the present discounted value of earnings had they entered the work force after high school graduation, divided by the present discounted value of earnings had they entered the work force after high school graduation. Thus a value of 100 means that someone is expected to earn twice (in present discounted value) the amount they would earn if they did not enroll in college. A negative value indicates that tuition costs and opportunity cost of time exceed future expected earnings gains post-enrollment. The figure shows the distribution of returns that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted returns for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of historic enrollment. We assume that a student scoring X's relevant degree choice set consists of the set of degrees for which his or her PSU score is in the 25^{th} to 90^{th} percentile of the historic range of admittees to that degree. The y axis value gives the enrollment-weighted mean expected earnings gains for students with a PSU of X over the degrees they could get into.

Figure 3. Long-Run Relative Returns to Degree by SES



Notes: Returns are calculated as a net present discounted value return over not going to college. They include opportunity cost of time and tuition cost at the expected degree length. The returns are therefore the net present value of predicted earnings for each student from enrolling in their observed degree out to age 30 (assuming that the student enrolls at age 18), less expected tuition costs and the present discounted value of earnings had they entered the work force after high school graduation, divided by the present discounted value of earnings had they entered the work force after high school graduation. Thus a value of 100 means that someone is expected to earn twice (in present discounted value) the amount they would earn if they did not enroll in college. A negative value indicates that tuition costs and opportunity cost of time exceed future expected earnings gains post-enrollment. The figure shows the distribution of returns that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted returns for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of historic enrollment. We assume that a student scoring X's relevant degree choice set consists of the set of degrees for which his or her PSU score is in the 25th to 90th percentile of the historic range of admittees to that degree.