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THE EFFECTS OF EARNINGS DISCLOSURE ON COLLEGE ENROLLMENT DECISIONS

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ABSTRACT

We test the impact of information about institution- and major-specific labor market outcomes on college enrollment decisions using a randomized controlled trial administered within the online Chilean federal student loan application process. Using linked secondary and post-secondary education records and tax returns for fourteen cohorts of Chilean high school graduates, we created measures of past-cohort earnings for nearly all institution and major combinations in the Chilean higher education system. Applicants were asked a series of survey questions about their enrollment plans and their beliefs about earnings and cost outcomes. Following the survey questions, randomly selected applicants were given information on earnings and costs for past students at their planned enrollment choices, as well as access to a searchable database that allowed them to compare earnings and costs across degrees. Students have unbiased but highly variable beliefs about costs, and upward-biased beliefs about earnings outcomes. Poorer students have less accurate information and choose lower-earning degrees conditional on baseline ability and demographics. While treatment has no effect on whether students enroll in postsecondary education, it does cause low-SES students to enroll in degrees where earnings net of costs were higher for past enrollees. Though effect sizes are small, they substantially exceed the cost of implementing the disclosure policy.

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1 Introduction

Federal student loans and grants are a key policy tool in the effort to expand access to higher education. These programs have the potential to produce large social returns if they facilitate valuable educational investments. However, in the face of rising student loan debt and default rates (Department of Education, 2013), policymakers and researchers hypothesize that students from low-income, college-inexperienced backgrounds may choose lower-return, higher-cost degree programs based on poor information or targeted marketing (GAO 2010; Lewin 2011; Lederman 2009, 2011), reducing the benefits of loan subsidies. Moreover, uninformed decision makers may incentivize providers to raise tuition, increase advertising expenditures, or lower admissions and academic standards rather than invest in raising the quality of degree programs (Beyer et al. 2015).

Policymakers have focused on two types of solutions. Disclosure policies aim to help students make financially sound decisions by compiling and distributing information on academic, labor market, and cost outcomes for different degree programs. This could improve students' financial outcomes by reducing uncertainty and supplanting persuasive marketing. Alternatively, regulation policies seek to directly limit the subsidies available to education providers whose students exhibit a pattern of poor academic and/or labor market outcomes.[1] Disclosure policies may be less intrusive (Lowenstein, Sunstein, and Golman 2014), but their effectiveness depends on what students already know about academic and financial outcomes at different degree programs, how much students value these outcomes, how effectively the government designs and communicates new information, and how the demand response to information provision changes incentives for providers.[2]

We present the results of a randomized intervention in which we test a large-scale, government-implemented disclosure policy in the Chilean higher education market. Federal student loan applicants were asked to complete survey questions about their application plans, their beliefs about future earnings outcomes for themselves and for others, and their beliefs about tuition costs. Randomly selected students were then provided with information on earnings and costs at their planned application choices and given access to a database that allowed them to build comparative tables of earnings and costs at institution-

[1] In the US, proposed gainful employment rules (Department of Education, 2014a) encompass 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. An analogy in personal finance markets would be providing information on payday lending interest rates versus capping payday loan interest rates or prohibiting payday loans all together.
[2] Duarte and Hastings (2012) show how disclosure requirements can be undone by strategic firms if not carefully designed to incorporate firm response in the context of private pension markets. Beyer et al. (2015) discuss similar issues in the context of higher-education disclosure and student loan regulation.

major combinations (we refer to an institution and major combination as a "degree" going forward)[3] serving students with similar baseline academic abilities. We measure the impact of information provision on students' enrollment decisions. Following predictions from a model of choice under limited information, we also investigate how effects vary as a function of student demographics and survey responses.

We worked closely with a number of Chilean government agencies to design the intervention and necessary supporting data. The supporting data match student records of high school graduation, college enrollment, and standardized test scores for the population of Chilean high school graduates between 2000 and 2013 to administrative tax records. The survey and field experiment were designed and implemented in partnership with the Chilean Ministry of Education (MINEDUC). Directly following the submission of student loan applications, students 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, accepted an informed consent statement, and were asked six questions. These included questions about application plans, questions about own earnings and tuition cost expectations at the degree programs to which the student planned to apply, and questions about expected earnings for typical students at those degree programs. 49,166 students completed the online survey.

Upon survey completion, randomly-selected students continued to two additional web pages designed to provide information about and prompt search for degrees with higher earnings net of costs for past graduates. Our web application used prior survey responses to display personalized information for each applicant based on a back-end database linking educational and tax records for past graduates. The first page displayed information on earnings gains (relative to no tertiary enrollment) in monthly terms for the participant's first-choice degree, 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. To encourage search, the page also displayed information on the Net Value associated with enrollment in an alternative institution offering the same major, or in a different major in the same broad field of study (e.g. nursing vs. nutrition). Potential gains were drawn from degrees relevant to respondents based on the selectivity of their listed application choices.

The second page consisted of a searchable database that allowed students 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. Students were told they could save up to ten search tables and could re-login to view them any time. We use administrative data to track

[3] For example, a degree in Civil Engineering at UC Berkeley is a major in Civil Engineering at UC Berkeley. We refer to institution-major combinations as degrees since Chilean students apply to and are admitted at majors within institutions when applying to college.

students in the treatment and control groups and identify whether and where they chose to enroll in the subsequent school year.

Our findings are as follows. First, using survey responses and enrollment data from prior cohorts, we examine whether there are differences in knowledge of degree-specific cost and earnings outcomes between students from low- and high-socioeconomic status (SES) backgrounds. We find that many students have limited knowledge of the earnings and cost outcomes associated with different degree programs, and that students from low-SES backgrounds tend to have less information on these degree characteristics than others. Compared to students from high-income backgrounds, students from low-income backgrounds are 6.3 percentage points (on a base of 30.7%) more likely to say that they do not know tuition costs at their planned place of enrollment. They are 8.5 percentage points more likely to say that they do not know what they will earn upon completing their chosen degree (on a base of 32.6%), and are similarly more likely to say that they do not know what a typical student will earn. Students who do report expectations about degree-specific own and typical-student earnings systematically overestimate earnings for graduates. Low-SES students overestimate short-run earnings outcomes for graduates at their chosen degree by an average of 70.8%, with an interquartile range in expectations of 80.5%.

Earnings outcomes for past cohorts are consistent with the survey results demonstrating gaps between low- and high-SES students in information available at the time of choice. We estimate OLS models of returns to education by institution and major using a flexible function of student baseline ability, gender, SES, and other demographics. We find that mean post-college earnings at the degrees students choose rise steeply with admissions test scores, but that many students throughout the ability distribution choose low-earning degree programs. Statistics on average earnings outcomes for college enrollees mask lower earnings at degrees serving lower-scoring, lower-SES students. Conditional on admissions test score, low-SES students earn about 13.5% less than high-SES students. Almost half of this gap (47%) is due to differences in degree choice between low- and high-SES students, as opposed to differential earnings outcomes within degrees. Coupled with the survey results, these descriptive findings suggest scope for disclosure policies to push students from low-SES backgrounds towards degrees with better financial outcomes.

Our intervention tests the effects of such a policy. To guide our analysis, we develop a model of degree choice under limited information in which treatment provides accurate information about financial outcomes for past students and may affect the salience of different degrees in the choice set. The model predicts strong impacts on enrollment decisions among students who are less certain about earnings and cost outcomes, who place more value on pecuniary degree characteristics, and who are considering degrees spanning a broad range of financial outcomes. Our survey responses provide measures of these mediating factors.

We first show that students from low-income backgrounds are harder to reach with information than other students, even when using direct communication from the educational authority near the time of application. However, once reached, positive ITT effects of information on the Net Value of the chosen degree are concentrated among low-income students. This echoes findings across a range of social services on the difficulty of program take-up for those most in need (see, e.g., Currie 2006; Choi, Laibson, and Madrian 2011; Bettinger et al. 2012; Amior et al 2012). We find no impact of disclosure on students' extensive-margin choice to matriculate in any degree program; point estimates are small and statistically insignificant. Positive treatment effects are driven by the intensive margin choice of where to enroll, and those effects are largest for students from low-SES backgrounds. For these students, treatment increases the Net Value of the chosen degree by 3.4% of mean Net Value. This is equal to 5.3% of mean potential gains from switching to a peer institution offering a similar degree, and 38.4% of the "choice gap," defined here as the component of the gap between predicted earnings outcomes for high- and low-SES students at the same score level attributable to differential degree choice.

Subgroup treatment effects line up with model predictions. Effects are larger for low-SES students who have less baseline information on earnings and costs, who exhibit lower levels of pre-intervention preference for a given degree or program, and whose stated pre-intervention plans include degrees at a range of earnings levels. We find similar results when we focus on measures of earnings value added that hold observable student characteristics fixed.

Our findings suggest that the returns on investments in informational interventions are potentially high. Treatment raises the present discounted value of predicted earnings net of costs for respondents by roughly USD $72m, substantially exceeding administration costs. At the same time, informational treatments seem unlikely to substantially alter student loan default rates. Though treatment closes the gap in student loan default rates at degrees chosen by high- and low-SES students at similar ability levels by over 70%, this gap is not that large, so effects on the overall default rate are small. Nor do disclosure policies seem likely to substantially raise incentives for higher education institutions to offer degrees where enrollees have lower default rates. The demand shift we observe towards higher earning degrees is small relative to potential gains, and treatment does not shift students towards lower-cost degrees.

We make several contributions to existing research. To the best of our knowledge, this is the first paper to evaluate the effects of a large scale, institution- and major-specific earnings disclosure policy. We build on smaller-scale major-specific information interventions targeted at students already enrolled in selective schools (Wiswall and Zafar 2014; see also Zafar 2011; Arcidiacono, Hotz, and Kang 2012; Zafar 2013; Stinebrickner and Stinebrickner 2013), interventions that provide information about average returns to educational attainment (Jensen 2010; Nguyen 2010; Oreopoulos and Dunn 2013; Dinkelman and Martinez 2014), and interventions aimed at making the financial aid and college application processes more transparent (Bettinger et al. 2012; Hoxby and Turner 2013; see also Avery and Kane 2004; Avery and Hoxby 2012; Scott-Clayton 2012; and Dynarski and Scott-Clayton 2013). [4] Our design allows us to study populations (lower-income, loan eligible students) and choice margins (where to study and what) that are critical to debates over higher education subsidies and market structure.

Like Hastings and Weinstein (2008), Jensen (2010), and Wiswall and Zafar (2014), we find that information provision appears to push students toward choices associated with higher earnings or better academic outcomes. However, we also find that treatment effects are limited by negative selection into information receipt, the relatively low value many applicants place on financial outcomes in college choice, and the fact that many applicants consider degrees within a relatively small earnings range. This suggests the efficacy of the intervention could increase if interacted with policies that help or incentivize students to use the new information more effectively (Bettinger et al. 2012; Hastings, Madrian, and Skimmyhorn 2013; Hastings 2015; Beyer et al. 2015).

We also contribute to the broader literature examining how behavioral biases, limited information, and decision making skills can influence the efficacy of subsidy and safety net programs (e.g., Thaler and Benartzi, 2004; Bhargava and Manoli, 2011; Duarte and Hastings, 2012; Bettinger et al., 2012).[5] In particular, there are close parallels between the higher education market and markets for other financial investments, such as mortgages, where government policies and loan guarantees affect market outcomes, and where both disclosure and regulatory policies are topics of current policy debate (Agarwal et al. 2010; Collins and O'Rourke 2010; Woodward and Hall 2012; Agarwal et al. 2014; Lowenstein, Sunstein, and Golman 2014). Disclosure policies also play an important role in health investment, healthcare markets, and health policy (Mathios 2000; Jin and Leslie 2003; Jin 2005). Our findings are consistent with modest gains from information provision in these settings as well.

This paper is part of a set of projects investigating the returns to education and college choice in Chile. Hastings, Neilson, and Zimmerman (2013) use discontinuous admissions rules at hundreds of degree programs to explore how the earnings effects of college admission on long-run earnings vary by selectivity and field of study. In Section 6 we use these discontinuous admissions rules to benchmark the more broadly available OLS earnings estimates that we use in the present analysis. Beyer et al. (2015) describes a set of policies regulating the availability of student loans at specific degree programs. These policies were adopted in part in response to the research described here. Hastings et al. (2015) uses a broader set of surveys of Chilean college applicants to describe the challenges students face in their

[4] While not the primary focus of this paper, to construct earnings and costs used in the information disclosure, we contribute to a broader literature using application or enrollment records linked to administrative data on labor earnings to construct estimates of the labor market effects of admission to different degree programs (Hoekstra 2009; Saavedra 2008; Ockert 2010; Hastings, Neilson and Zimmerman, 2013; Reyes, Rodriguez, and Urzua (2013); Zimmerman 2014; Kirkebøen, Leuven, and Mogstad 2014).

[5] See Madrian (2014) and Lavecchia, Lieu, and Oreopoulos (2014) for reviews of this literature.

attempts to acquire and deploy information on the degree-specific financial and academic characteristics. Our work draws on a Chile-focused literature describing heterogeneity in returns to higher education and the role of higher education markets in determining education attainment and labor market outcomes (see e.g., Brunner 2004; Brunner 2009a; Brunner 2009b; Reyes, Rodriguez, and Urzua 2013).[6]

Finally, our 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 choice incorporating psychology and limited information. We build here on Karlan (2005), Ashraf, Karlan, and Yin (2006), Fehr and Goette (2007), Ashraf, Berry, and Shapiro (2010), Jensen (2010), and Hastings (2015). Our approach and results contribute to the growing body of literature incorporating behavioral economics into public policy design (Chetty 2015).

2 Higher education and student loans in Chile

Chile is a middle-income OECD member country with a higher education system similar to those in the US and other upper-income OECD countries in terms of educational attainment rates, the role of student loans in financing higher education, and higher education market structure. In 2010, 38% of adults between 25 and 34 years old in Chile had a tertiary degree, compared to 42% in the US (OECD 2013). 35.8% of students enrolled in Chilean higher education institutions used state-backed student loans in 2011, compared to 40.2% in the US.[7]

Public, private non-profit, and private for-profit firms provide tertiary 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, and universities offer traditional undergraduate and graduate degrees.[8] In 2012, universities accounted for 58.4% of all undergraduate enrollment while professional institutes and technical schools accounted for 28.1% and 13.5% respectively. IPs and CFTs are run by private companies and can be for-profit or not-for-profit. Universities may be public or private not-for-profit. In practice, however, portions

[6] In particular, the Futuro Laboral program described in Brunner (2009b) compiled major- (but not institution-) specific labor market outcomes for a subset of majors and made this data publicly available. The closest parallel to Futuro Laboral in the US is likely the government-compiled occupation-specific wage statistics compiled as in BLS (2015).
[7] Source: Chilean statistics from Mineduc (2012), Tables Programas, Becas y Ayudas Estudiantiles A.4.4 and Matricula D.2.45. US statistics from Department of Education (2014b) Table 3.2-A. US statistics refer to the 2011-2012 school year.
[8] Some universities, particularly public universities, also offer two-year technical degrees.

of some universities are owned by for-profit parent companies, including companies like Laureate International and the Apollo Group, which also own for-profit universities in the US.[9]

Students in Chile apply to, take courses within, and graduate from institution-major combinations (e.g., Sociology at the University of Chile). We will refer to an institution-major combination as a "degree." Entrance exam scores are the key determinant of admissions as well as loan and scholarship awards. The standardized test is called the *Prueba de Selecion Universitaria*, or PSU.[10] Entrance exam takers complete exams in Mathematics and Language, and may take additional exams in subjects such as science or history. Scores are scaled to a distribution with a mean and median of 500 and standard deviation of 110.

Students hoping to be admitted to older, more prestigious universities typically need to score at least 475 points on their Math and Language exams. Students in these universities, known collectively as the CRUCH (Council of Rectors of the Universities of Chile), made up 26% of total higher education enrollees in 2013. CRUCH universities run a joint admissions process. In this process, each degree scores students based on entrance exam scores and GPAs, and students rank up to eight degrees in order of preference. Students are allocated to the most-preferred choice still available after higher-ranked applicants are admitted. See HNZ (2013) for admissions algorithm details.

Students admitted to less prestigious 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. In what follows we will measure selectivity using the average of Math and Language scores for enrolling students.

Chilean students rely primarily on two subsidized student loan programs. The older type of loan is the Fondo Solidario de Crédito Universitario (FSCU). FSCU loans are both need- and merit-based, and have existed since 1981.[11] To qualify for a FSCU loan, students must be Chilean citizens, have "family income that makes payment of tuition difficult or impossible,"[12] and have 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.

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[9] See "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. Laureate International owns five universities in the US, including Walden University and Kendall college. See http://www.laureate.net/, accessed 7 May 2013. The Apollo Group owns the University of Phoenix in the US. See Apollo Global Fact Sheet, accessed 7 May 2013. See http://www.apollo.edu/sites/default/files/files/Apollo-Group-Apollo-Global-Fact-Sheet.pdf for a discussion of Apollo's purchase of the Chilean university UNIACC. In 2009, the Apollo Group settled in a False Claims law suit for its recruiting and advertising practices in the US. See 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.

[10] Prior to 2004, the entrance exam was called the PAA, *Prueba de Aptitude Academica*.

[11] 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*.

[12] Law 20,027. Article III, paragraph 2, section 9.3. NB.

FSCU loans target the poorest students admitted to selective degree programs and are available to relatively few students in part because most low-income students have lower academic performance when applying to colleges. In 2012, 7.1% of low-SES postsecondary students received FSCU loans.

To increase higher education opportunities for low-income students, the government introduced 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. [13] CAE can be used to finance education at any accredited postsecondary institution: CRUCH universities, accredited private universities, professional institutes, and technical schools are eligible. CAE eligibility is both need- and merit-based. To study 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.[14] 35.6% of low-SES postsecondary students used CAE loans in 2012.

CAE loans reshaped the higher education landscape in Chile. Following the introduction of CAE loans, the fraction of higher education revenues in Chile coming from loan dollars rose by 170%,[15] and college enrollment rates rose by more than 50% as a fraction of the college-aged population, from 48% in 2005 to 74% in 2012.[16] Many of the new enrollees came from low-SES or low-performing backgrounds. The fraction of first-year college enrollees coming from low-SES backgrounds rose by 27% from 2005 to 2012, and the fraction scoring below the median on their high school standardized tests (the SIMCE) increased by 107% from 2006 to 2012. Online Appendix Section 6 provides further detail on changes in baseline ability of freshman enrollees, total enrollment and fraction of tuition revenues coming from federal student loans by degree selectivity.

In early- to mid-November, students apply for FSCU, CAE and several other federal grant programs using the Formulario Unico de Acreditación Socioeconómica (FUAS), a unified financial aid form which is similar to the FAFSA in the US. After completing the FUAS, students face a short timeline for college choice. They take the PSU in late November or early December, learn their PSU scores in late December or early January, and begin to send in applications during the first two weeks in January. Note

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

[14] "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.

[15] Change from 2007 to 2012. Source: Annuario Estadistico 2012 MINEDUC based on data from Servicio de Información de la Educación Superior (SIES), División de Educación Superior. Ministerio de Educación.See Solis (2013) for a discussion of the causal effects of loan access on college attendance.

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

that Chilean college applications typically do not include components such as essays or discussion of extracurricular activities (see HNZ 2013). Students begin to learn of admissions outcomes as early as mid-January, and the school year begins in late February or early March depending on the year and degree program. Table A.1 provides a timeline of the loan and college application processes in Chile during the 2012-2013 application cycle.

3 Data

In collaboration with MINEDUC and other agencies within the Chilean government, we constructed a database combining high school records, college records, loan records, and tax records for cohorts of Chilean college applicants from 1980 through 2013. The purpose of the data collection effort was to conduct research to inform upcoming higher education policy decisions.

3.1 High school records

We use student-level high school records for the years 1995 through 2012. These data were available in electronic form for cohorts starting in 2003 and digitized from hard copies in earlier years. The high school data includes basic student covariates such as gender and parental education, scores on standardized tests administered to 10th graders (known as the SIMCE, or Sistema Nacional de Medición de la Calidad de la Educación), high school identifiers, and high school characteristics.

Of particular importance are school-level ratings of socioeconomic status (SES) computed by Mineduc. The SES rating categorizes schools from A (lowest SES) through E (highest SES). These ratings are based on parental income. Since we do not observe parental income directly, we use high school SES as a proxy for student SES. High schools in Chile are fairly small: the median graduating senior class size is 57.[17] Students coming from A and B schools are categorized as low-SES. Table A.2 shows how family background, academic performance, and school characteristics vary with school poverty status, and describes cross-validation of poverty rankings using available tax records for parents of children attending each high school. Schools categorized as low-SES are much more likely to be municipal public schools (as opposed to private or voucher schools), and graduates from low-SES schools are much less likely to attend college or have parents who completed college degrees.

[17] This is due to the universal voucher system – most schools are private schools accepting voucher.

3.2 College and college application records

Data on college application, enrollment, graduation, and student aid come from several sources. Application records include entrance exam scores for all students by subject for the years 1980-2013, drawn from electronic storage at MINEDUC beginning with the year 2000 and digitized from archival records in earlier years. We observe loan applications and awards for the years 2007 through 2013. These data include information on which students take out loans and when, when students enter repayment, and payment status for loans in the repayment stage. We use these data to construct cohort-degree specific summary statistics on loan repayment and default rates.

We track applicants forward to college using enrollment and graduation data from MINEDUC. These data were available at MINEDUC in electronic format for the years 2007 to 2013. To facilitate the study of labor market outcomes over the longer run, we assisted MINEDUC in designing a rule requiring institutions to provide additional historic enrollment and graduation data back to 2000. These data follow students semester by semester, recording major- and institution-specific enrollment and graduation outcomes. We do not observe semester-by-semester grade outcomes, but do observe listed semester-level tuition and suggested degree length.

Taken together, high school, college application, and college enrollment records allow us to construct enrollment histories at the student level, and describe degree programs in terms of the types of students that enroll, their graduation rates, tuition costs, loan-financing, repayment and default rates.

3.3 Labor market outcomes

Through an agreement with the tax authority granted for the specific purpose of informing higher education policies, we were permitted to link the database of student records to tax returns from the 2005-2013 earnings years on a secure computer within the tax authority.[18] Over 99% of individuals in our data have matches in the tax records. Tax returns include wage, contract, partnership, investment and retirement income. HNZ (2013) describe the tax data in detail, and provide an example of a tax form to illustrate the components used to calculate student income. We were able to access tax data only inside the Chilean tax authority on a secure, dedicated computer. In compliance with Chilean law, we were permitted to take out aggregate data and regression output.

We use tax records to construct several measures of earnings outcomes by degree program (institution-major) and student characteristics. The first, which we term "Net Value," was provided to

[18] 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.

students treated with our informational intervention. We worked with MINEDUC to develop a measure the agency deemed appropriate for providing to students. MINEDUC's preferences at the time of the intervention included a focus on outcomes for graduates of degree programs rather than enrollees, and on binned means rather than regression-adjusted predictions. [19] MINEDUC preferred to focus on earnings and cost outcomes discounted back to the year of labor market entry, which may differ depending on educational choice, as opposed to, say, the first year following high school completion.

With these constraints in mind, we compute Net Value as

$$NV_j = \sum_{t=1}^{t=15} \beta^t (\hat{\mu}_{jt} - \hat{\mu}_{0t}) - C_j \tag{1}$$

Here, $\hat{\mu}_{jt}$ are mean earnings for graduates of degree *j* at experience year *t*, $\hat{\mu}_{0t}$ are mean earnings for students who do not enroll in any degree program at experience year *t*, and C_j is the present value of tuition costs for degree *j*, discounted to experience year 1. $\beta = 1/(1+r)$, where *r* is the discount rate. Net Value is the present value of earnings over the fifteen-year time horizon for loan repayment for graduates, less the present value of fifteen years of earnings for students who do not attend college and the present value of direct costs.

We observe mean earnings values directly for experience years one through five. For years 6 through 15, we use predicted earnings based on field-specific linear slope terms. Costs are based on current tuition levels and suggested degree lengths. The discount rate is set to 2%, the rate of interest on subsidized loans. We convert Net Values to a monthly equivalent before presenting the information to students. See Online Appendix Section 2 for more details. We also present monthly equivalents of the present discounted values of earnings gains and tuition costs.

In addition to Net Value, we consider a measure based on regression predictions of earnings conditional on enrollment. We focus on flexible specifications of the form

$$y_{ijct} = X_{ict}'\beta_{s(j)} + Z_j \gamma_{s(j)} + W_{ijt}\delta_{s(j)} + \mu_{jc} + \epsilon_{ijct} \tag{2}$$

Here, y_{ijct} is labor market earnings for student *i* enrolling in degree program *j* in cohort *c* at labor market experience year *t*. X_{ict} includes dummies for student socioeconomic status, gender, and whether a student

[19] This is similar to calculations proposed in the US Gainful Employment Act. Eventually, after discussion and data analysis, MINEDUC came to favor the provision information conditional on enrollment, with the goal of incentivizing institutions to increase earnings for all enrolled students rather than through selective graduation (Beyer et al. 2015).

took the entrance exam, linear controls for entrance exam score, years of labor market experience, interactions between labor market experience and student covariates, and tax year dummies. Z_j are major specific dummy variables, and W_{ijt} are interactions between major and gender, SES, test taking and test scores, and labor market experience. μ_{jc} are degree-cohort specific mean residual components and ϵ_{ijct} is a mean-zero idiosyncratic error. We estimate these equations separately within five selectivity tiers $s(j)$ and by broadly defined CINE-UNESCO areas of specialization.[20] See Online Appendix Section 3 for more details on estimation.

The earnings measure we consider from this regression is predicted earnings averaged across cohorts, $\hat{y}_{ijt}$. This measure captures degree-specific earnings outcomes conditional on enrollment, including cross-degree differences driven by student sorting. Cross-degree differences may vary with labor market experience. In our main analysis, we focus on earnings eight years after college application, or approximately age 26. We choose age 26 because it allows students enough time to complete schooling. Earnings outcomes at later ages are also of interest, but measurement is more difficult because we observe fewer cohorts and the population of degree programs changes over time.[21] In Section 6, we discuss results in which we compute the present discounted value of earnings through ages 30 and 50 for each degree program using more aggregated data on selectivity- and field-specific earnings profiles. Our focus on early-career earnings outcomes reduces observed effects in percentage terms compared to estimates that include data for older earners. See Online Appendix Section 3 for additional details.

We evaluate the effects of the disclosure treatment by estimating the impact of treatment on Net Value and predicted earnings at the degrees in which students choose to enroll. The predicted earnings measure addresses some of the limitations of Net Value. Earnings predictions conditional on enrollment include earnings outcomes for dropouts, not just graduates, and compare earnings a fixed length of time from the application year. Importantly, information treatment effects computed using predicted earnings capture differences in earnings outcomes holding fixed student test score, gender, and SES. Treatment effect estimates based on predicted earnings reflect changes in degree "value added" conditional on student observables, and are not driven by, e.g., low-SES students who switch into degrees with more high-SES students.

3.4 Non-earnings degree characteristics

[20] CINE-UNESCO areas are Business, Agriculture, Art and Architecture, Basic Sciences, Social Sciences, Law, Education, Humanities, Health and Technology.

[21] We are able to compute predicted age 26 earnings for 83% of students enrolling in college in 2013. As shown in Table A.5, experimental treatment does not predict enrollment in a degree for which earnings estimates are unavailable.

We also consider the effects of treatment on graduation rate, loan repayment rate, and average enrollment length at the chosen degree. We compute these values using the matriculation, graduation, and loan data. We consider two types of loan repayment outcomes. The first is the fraction of students in repayment whose payments are current. The second is the fraction who have defaulted (defined as three or more payments behind schedule).

4 Survey and experimental intervention

The survey and field experiment were constructed as follows. 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 stratified by high school for current high school seniors,[22] and by prior PSU test score (in 50 point bins) for PSU registrants who had graduated in the two prior cohorts.[23] This list was merged to loan applications as the applications were completed. Upon submitting their applications, loan applicants received an email from MINEDUC with the 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. Students 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. 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 were invited to click a link taking them to the survey 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 six questions, each appearing 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 they would restart where they left off. The survey program adapted questions based on prior responses. We present

[22] Note that high school classes in Chile are small. Median graduating class size in 2012 was 57. 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. An advantage of school level randomization is that estimated effects will better predict what would be observed in a universal policy rollout in the presence of school-level peer spillovers.
[23] 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 likely to apply for loans.

survey materials in Spanish (with English translations) in the Online Appendix Section 2, along with screen shots of the survey and information treatment pages.

The first question asked students about their current educational status (e.g., whether they were applying to college for the first time or whether they were already enrolled and considering re-applying). The second question asked students to list up to the top three degrees (institution-major combinations) to which they planned to apply. These were chosen from a nested set of drop down menus that filtered results to make list sizes manageable. Students were required to list at least one entry to proceed to the next question. The third question asked students how certain they were of their application plans. The fourth question asked students what they thought the annual cost of studying (tuition plus registration fees) at each of their choices would be. Choices were piped in from prior responses. Students could click an "I do not know" button or move a slider to indicate the total annual cost. The fifth question asked about expected earnings upon graduation. Students 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 students about their expected PSU scores in Language and Math.

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 the monthly earnings gain component of the Net Value measure, described in Section 3.3 in the left column. The second column of this table displayed the monthly cost component. The third column displayed the Net Value measure itself – the difference between monthly earnings gains and costs. Accompanying text explained to students that these values were derived from past data on earnings and costs.

In a highlighted box below this table, students were told whether there were other institutions they could likely get into which offered the same major with a higher Net Value, and were shown the additional Net Value associated with a switch to the highest Net Value degree (though they were not told which institution offered this value). The net value gain was calculated by the web application, referencing a back-end database on earnings outcomes at different degree programs. The web application searched across institutions offering the same major as the first-choice degree, looking for degree programs with similar entrance exam distributions and higher Net Values. Finally, treated participants were shown 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, as well as the expected Net

Value gain from the within-field switch. Again, the web application searched across degrees with similar entrance exam score distributions to the listed first choice.[24]

After the information and suggestion page, treated subjects clicked through to a final page, the "Buscador de Carreras" (Career Searcher). It explained what the searchable database was, and gave subjects a place 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 and serving students with similar PSU scores.[25] Institutions were sorted in descending order by Net Value. The table displayed the institution name, the major, the earnings gains for graduates in monthly terms, the monthly loan costs, and the Net Value. It also displayed a suggested alternative major to search for with higher Net Value but in the same field of interest (e.g. suggesting nursing to someone interested in nutrition). Students were informed at the top of the page that this new database was being produced by Proyecto 3E – a consortium of international researchers collaborating with MINEDUC - using tax records of past graduates, and with the purpose of helping students make informed decisions for their future. Students could log back in at any time and compile and view up to ten comparative tables to use in choosing their degree. This final page also contained a thank you message and the confirmation code; students were not required to search the database.

We evaluate the effects of the informational intervention by linking data on treatment and control students to administrative enrollment records. We are able to observe whether students enrolled in any degree program, and describe the degree programs students chose in terms of academic and financial outcomes for past students.

5 Empirical Analysis

5.1 Baseline differences in enrollment choices by socio-economic status

Students from low-SES backgrounds make different enrollment choices than other students in terms of expected costs and earnings outcomes. To illustrate this, we use the enrollment data and regression output from equation (2), and calculate demographic- and degree-specific earnings predictions

[24] 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 A.3 for more details.
[25] 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.

at age 26 for first-year college students between 2007 and 2011.[26] Figure 1 shows the mean, 10th, and 90th percentiles of the predicted earnings distribution by entrance exam (PSU) score. The horizontal line shows average earnings for high school graduates who do not enroll in college, aggregated across all score groups. Predicted earnings rise steadily by PSU score, with mean earnings for students at the 75th percentile of the score distribution (581) 52% higher than those for students at the 25th percentile (431). However, within test score, students choose degrees characterized by very different earnings outcomes. For students with PSU scores equal to 505 – the median for college enrollees—degrees at the 90th percentile of the predicted earnings distribution have mean earnings twice as high as those at the 10th percentile. Mean earnings for the average high school graduate remain close to those for the 10th percentile college degree past the 75th percentile of the score distribution. This descriptive finding is consistent with the idea that many students across a broad range of the ability distribution choose degree programs where the labor market returns are negative.

Figure 2 displays differences in predicted earnings by student socioeconomic status, and decomposes predicted earnings 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. We display mean earnings at chosen degrees by SES category and PSU score using either i) SES-specific enrollment weights (allowing for the fact that high- and low-SES students make different degree choices at the same score level) or ii) overall enrollment weights (the average student's enrollment choice at a score level, adjusting low- and high-SES weights so the two groups share the same degree mix). Comparing the Low SES – SES weight and Low SES – Population weight lines shows how much low-SES students could raise their predicted earnings by selecting the population average degree mix. Similarly, the difference in High SES – SES weight and High SES – Population weight shows how much high-SES students would reduce their predicted earnings by selecting the population average degree mix.

Conditional on ability, low-SES students enroll in degrees where their earnings are on average 13.5% lower than for high-SES students. Holding enrollment weights fixed at population averages within score bins, low-SES students earn 7.2% less in expectation than high-SES students. Differences in enrollment choices thus account for the remaining 6.3%, or just under half of the earnings gap. Low-SES students would do better in expectation if they chose the same distribution of degrees as the broader population. In contrast, high-SES students choose degrees with higher average earnings than the distribution in the population. The within-score variation in enrollment choices described here

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

exacerbates inequality in enrollment outcomes driven by cross-group differences in scores. As shown in Table A.2, students from low-SES backgrounds are disproportionately likely to fall in the lower part of the PSU distribution. Finally, if we decompose the difference in choices (top line to bottom line) into differences in choice of institution versus choice of major, conditional on ability, we find that a little less than half (5.5% out of 13.5%) of the SES gap in earnings is attributable to differences in choice of institution rather than choice of major.[27]

Differences in costs across degree choices are relatively small compared to earnings differences. Figure 3 displays mean predicted monthly payments by entrance exam score and SES background. Predicted monthly payments are computed based on 2013 tuition values and observed average enrollment durations for past students. We use the subsidized student loan interest rate of 2% and repayment period of 15 years. Between the 25th and 75th percentiles of the score distribution for enrolling students, average monthly payments for low-SES students rise from $20,917 to $67,326 Chilean pesos (CLP). Earnings rise from $309,075 to $456,025 CLP, leading to an increase in take-home pay of $100,541 CLP. Similarly, within-score differences in costs across SES groups are much smaller than differences in earnings. If low-SES students with enrollment scores at the median for enrolling students changed their degree choices to be the same as those in the population as a whole, their predicted monthly earnings would rise by $51,089 CLP. Their costs would rise by only $6,748 CLP.

Choice-driven gaps in earnings outcomes for low- and high-SES students persist even net of costs and suggest that many students, and particularly those from low-income backgrounds, could choose and get into degrees with higher returns. We use our survey and experimental data to explore whether gaps in information exist that could explain the differences in choices, and if so, whether access to information can affect choices and close the cross-SES gap in predicted financial outcomes, or if choices are fully-informed and reflect persistent differences in preferences.

5.2 Baseline differences in information and expectations by socio-economic status

5.2.1 Sample description

Table 1 describes the sample of students invited to participate in our intervention, comparing characteristics of the invited sample to those of eventual respondents. 69% of the emails we sent were opened. Of those, the respondent read and agreed to the informed consent disclaimer 73% of the time.

[27] To calculate this we set all degree-specific effects, μ_{jc}, to the mean of μ_{jc} so that differences in predicted earnings across enrollees come only from SES effects and difference in choice of major.

59% of students providing informed consent completed the survey through to receiving the confirmation code. In total, 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 survey completion stage. We refer to survey completers as respondents from this point forward.

Within our sample, students with lower baseline academic achievement, low-SES backgrounds, and lower-educated backgrounds were harder to reach. The average PSU entrance exam score for respondents is 31 points higher than for invitees. The fraction of invited students from low-SES high schools was 43.7%; this falls to 35.7% for respondents. Respondents are more likely to have parents with some tertiary education, and score substantially higher on high school standardized tests (SIMCE) than invitees. On average, degree programs respondents list as their first choices offer Net Values of $734,948 CLP per month (just over $1,400 USD using November 2013 exchange rates) relative to not attending college. Students could raise this value by an average of 36% ($267,566 CLP) by switching to peer institutions offering similar degrees. 77.0% of respondents matriculate in some degree program. At age 26, our regressions predict respondents earn an average of $464,307 CLP each month, or USD $893.

Column 5 shows characteristics of treated respondents. There are no substantial differences in baseline characteristics between treatment and control students. A test of joint significance of baseline characteristics in explaining treatment fails to reject the null of no effect with a p-value of 0.191. The final column shows characteristics of treated students who searched the database. 43% of treated students searched, and searchers are similar to non-searchers in terms of observable characteristics and survey responses. Students who search have slightly higher SIMCE scores, and the Net Value of their stated first-choice enrollment plans is 4% higher than for non-searchers. See Tables A.4 and A.5 for a comparison of treatment and control groups.

5.2.2 Survey Responses

Table 2 shows survey responses broken down by the SES rating of students' high schools. The sample is all survey completers. The top panel of Table 2 compares expected tuition costs to actual tuition costs. Respondents were asked, "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?" (See Online Appendix 2 for the Spanish-language questionnaire.) The web program displayed for them the institution and major they had previously listed as their first, second, and third planned enrollment choices.

Just under one third of students responded that they do not know the tuition costs at their stated top choice for enrollment. Ignorance of tuition costs is decreasing with socio-economic status. Among those that registered a peso-value response, we compare their responses with actual tuition and matriculation fees from the administrative data. Conditional on claiming some knowledge of tuition costs, students are on average close to correct. We present results as percentage deviations from observed tuition values at students' first choice schools. Students from the poorest schools overestimate tuition, while students from higher-SES schools tend to slightly underestimate it. 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%.

The second and third panels of Table 2 show how students responded when asked a) what they expect earnings are for "typical" students who complete the specified first choice degree, and b) what they would expect to earn if they completed their first choice degree. [28] We divide students' earnings estimates by the observed mean earnings outcomes for past cohorts at their first choice degrees (using the linked higher education and tax data). We describe the distribution of deviations from the observed mean in percentage terms. Many respondents claim not to know what to expect about earnings outcomes, either for themselves or for the typical graduate. 47.7% of students select the "I don't know" option when asked about earnings for a typical graduate, with the fraction rising from 43.5% in high-SES high schools to 54.4% in low-SES high schools. 35.8% of students select the "I don't know option" for own earnings, with the fraction rising from 32.6% in high-SES high schools to 41.1% in the low-SES high schools.

Conditional on providing an earnings value, expectations are highly variable and appear to be biased upward. On average, students think that the typical graduate of their first choice degree program will earn 60.9% more than past graduates of that program have actually earned. Overestimates are particularly large for low-SES students, and across all SES groups they are driven by a right tail of students with large, positive prediction errors. Expectations about own earnings are slightly below those for the typical graduates. On average, students expect their own earnings conditional on graduating from a given degree program to be 51.8% higher than observed values for past graduates while they expect the average graduate to earn 60.9% more than past graduates. Though it is difficult ex ante to rule out a scenario in which students' optimistic beliefs about their own earnings prove to be accurate despite their inaccurate beliefs about outcomes for the typical student, the more straightforward explanation for our findings is that students view themselves as relatively similar to typical graduates and overestimate earnings both for the typical graduate and for themselves.

The majority of students are very certain about their application plans despite lacking accurate information on earnings and costs. Panel 4 of Table 2 shows responses to the question "How certain 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 certain at all," "I am a little certain," "I am fairly certain," "I am quite certain," and "I am

[28] Specifically, the question asks "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 "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 value to earnings for graduates in the first two years after they complete their degrees.

absolutely certain." About two thirds of students are "absolutely certain" or "quite certain" about their first choice. There is little variation by high school SES rating even though information about earnings and cost outcomes varies substantially.

Survey findings suggest differential access to information on earnings and costs may play a role in driving the gap in earnings outcomes between high- and low-SES students at the same ability levels. However, the survey results are also consistent with a story in which college applicants choose not to acquire information because they do not find it helpful in their decision process. Hastings et al. (2015) consider this possibility in more depth and present results from other surveys of Chilean college applicants showing that many more students list prestige and accreditation as the primary reasons for degree selection than list future earnings. 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 not consider changing their chosen career even in response to a 50% decline in relative earnings.[29] Rates of financial literacy and loan literacy (i.e., knowledge of loan terms) are also quite low. Randomly providing access to information on degree-specific earnings and costs and measuring the impact on enrollment choices provides a test between these two alternative explanations for the observed lack of information and gap in earnings outcomes.

5.3 Experimental estimates of information treatment

5.3.1 Modeling the impact of treatment on degree choice

To motivate our analysis of the impact of information on enrollment choices, we outline a simple model of choice under limited information and derive predictions for where we expect to find larger or smaller treatment effects. See Online Appendix Section 4 for derivations of results shown here.

Student preferences are given by:

$$u_{ij} = \beta_i E[y_{ij}] + \delta_{ij} + \epsilon_{ij} \quad (3)$$

[29] 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 X%. 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%.

where $E[y_{ij}]$ is student i 's expected earnings in degree j, δ_{ij} are student i 's non-pecuniary preferences for degree j, ϵ_{ij} is a type I *i.i.d.* extreme value error term, and β_i represents student i 's preference for pecuniary relative to non-pecuniary degree characteristics.

Students form beliefs about earnings using information on labor market outcomes for past graduates, as well their best guesses about their own skills and future labor market conditions. Because we are interested in how disclosure of degree-specific average earnings affects choice, we write earnings expectations as a linear projection onto beliefs about average earnings outcomes for past students:

$$E[y_{ij}] = \rho_i Y_{ij}^e + \phi_{ij}$$

where Y_{ij}^e is student *i*'s expectation of average earnings for past graduates of degree *j*. ρ_i captures the extent to which students update beliefs about own earnings as beliefs about outcomes for past graduates change. If students find outcomes for past students uninformative about future outcomes, ρ_i will be close to 0. In our discussion of the model, we assume that ρ_i is positive, so that students who believe that past students earned more will all else equal have higher expectations about own earnings. This is consistent with the observation that students tend to think own earnings conditional on graduation will be similar to those for the typical graduate of the same degree. ϕ_{ij} captures components of earnings expectations that are orthogonal to expectations about outcomes for past students. This may include knowledge of own relative skills or beliefs about future changes in degree quality and labor market conditions.

Students have imperfect knowledge of earnings outcomes for past students. Suppose that average past-graduate earnings for degree j, Y_j, are drawn from some distribution with mean $\bar{Y}$ and variance σ_Y^2. Students receive a noisy signal $Y_{ij} = Y_j + e_{ij}$ about each degree program j, where e_{ij} have mean zero, variance σ_i^2, and are independent across j for each i. Assuming linear updating of expectations, earnings expectations for untreated students are given by $Y_{ij}^0 = \alpha_i \tilde{Y}_{ij} + (1-\alpha_i)\bar{Y}$, where $\alpha_i = \sigma_i^{-2} / (\sigma_i^{-2} + \sigma_Y^{-2})$ is a precision weighting. Students with more precise signals on degree-specific earnings outcomes place more weight on these signals.

Let T_{ij} be an indicator variable equal to one if our treatment provides student i with information about degree j. Treatment provides students with the true values of Y_j, so that

(4) $$\begin{aligned} Y_{ij}^{e} &= T_{ij}(Y_j - Y_{ij}^{0}) + Y_{ij}^{0} \\ &= (1-\alpha_i)Y_j T_{ij} + \alpha_i Y_j + (1-T_{ij})(\alpha_i e_{ij} + (1-\alpha_i)\bar{Y}) \end{aligned}$$

This means that $Y_{ij}^{e} = Y_j$ if $T_{ij} = 1$ but that expectations remain at $Y_{ij}^{0} = \alpha_i \tilde{Y}_{ij} + (1-\alpha_i)\bar{Y}$ if $T_{ij} = 0$ (i.e., if a student does not receive the degree *j* treatment).

In addition, treatment may also affect students' preferences for degree programs through channels other than updates to beliefs. We allow for degree-specific "salience" treatment effects so that $\delta_{ij} = \delta_{ij}^{0} + T_{ij}\delta_{ij}^{1}$. This captures the fact that students may not be aware of degree options they search for and see in the searchable database, and treatment makes these options salient and therefore more likely to be chosen than unknown or less-salient options.[30]

Define P_{ij} as the probability student i chooses career j. Then,

(5) $$\frac{dP_{ij}}{dY_j} = P_{ij}(1-P_{ij})\rho_i \beta_i (\alpha_i + (1-\alpha_i)T_{ij}).$$

Earnings outcomes for degree j have larger effects on choice probabilities for students who value pecuniary characteristics more, who find earnings outcomes for past students more informative when forming expectations about own earnings, and who are somewhat likely but not completely certain to choose degree *j* (probability of choice is close to 0.5). Treatment scales up the effects of earnings outcomes on choice probabilities, with larger effects for students who have less precise prior beliefs.

In practice, our treatment provides students with information about a number of degree programs, and may also affect non-pecuniary preferences for degrees. Define D_{ij} as an indicator equal to one if i chooses j, and define $Y_i = \sum_j D_{ij} Y_j$ as the value of expected earnings at the degree i chooses. Y_i is our outcome variable of interest: net earnings gains for past students at the chosen degree program. Define T_i as an indicator variable equal to one if student i receives treatment for some set of degrees and let S_{ij} be an indicator variable equal to one if j is in the treated set. Then for each individual i

[30] Hastings, Hortaçsu and Syverson (2013) show that this representation of the impact of information is equivalent to a consideration set model, where information or advertising increases the probability that a particular product is considered out of a set of available but potentially unknown products.

$$(6) \qquad \frac{dE[Y_i \mid T_i]}{dT_i} = \rho_i \beta_i (1-\alpha_i)\left(P_s \sigma^2_{Y|S} + P_s(1-P_s)(\bar{Y}_{j|S} - \bar{Y})(\bar{Y}_{j|S} - \bar{Y}_{j|1-S})\right) + \sigma_{Y\delta}$$

$P_s = \sum_j S_{ij} P_{ij}$ is the probability i chooses a treated degree program, $\sigma^2_{Y|S} = \mathrm{Var}(Y_j \mid S_{ij} = 1)$ is the choice-probability weighted variance of degree Net Value for the treated degrees, $\bar{Y}_{j|S} = P_s^{-1} \sum_j S_{ij} P_{ij} Y_j$ is the mean degree effect for treated degrees, $\bar{Y}_{j|1-S} = (1-P_s)^{-1} \sum_j (1-S_{ij}) P_{ij} Y_j$ is the mean degree effect for untreated degrees, and $\sigma_{Y\delta} = \mathrm{Cov}(Y_j, S_{ij}\delta^1_{ij})$.

Treatment affects expected average earnings at the chosen degree in three ways. First, it allows students to make more informed choices within the set of treated degrees. This is captured in the $P_s \sigma^2_{Y|S}$ term. Effects will be larger, all else equal, when more degrees are treated and when there is more variation in earnings outcomes within the treated degrees. Second, updating can make students more or less likely to choose degrees in the treated set. This is captured in the $P_s(1-P_s)(\bar{Y}_{j|S} - \bar{Y})(\bar{Y}_{j|S} - \bar{Y}_{j|1-S})$ term. Students become more likely to shift into treated degrees if the mean effect for treated degrees $\bar{Y}_{j|S}$ exceeds the prior mean $\bar{Y}$. If they do shift, they realize gains proportional to the difference between mean earnings for treated and untreated degrees, $\bar{Y}_{j|S} - \bar{Y}_{j|1-S}$. Both of these effects are larger when students place high value on pecuniary characteristics (β_i is large), when students find average outcomes for past graduates more informative about own earnings (ρ_i is large) and when students have little information on degree-specific outcomes prior to treatment (α_i is close to zero). Third, treatment raises expected earnings at the chosen degree if salience effects of treatment are strong for high-earning degrees.

Given survey evidence suggesting that many students do not place a high weight on pecuniary characteristics in college choice, and that many students claim a high degree of certainty about choice despite weak knowledge of earnings and cost outcomes, the model suggests that some students will not respond to treatment.

5.3.2 Experimental results

Table 3 shows the impact of information on Net Value, earnings, and cost outcomes in students' chosen degrees. We show results separately for the full sample of students, and for subsamples coming from low- and high-SES schools. 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' stated first-choice degree (i.e., Net Value of first-choice degree if the dependent variable is Net Value, monthly debt payment of first choice degree if dependent variable is monthly debt payment). These controls reduce standard errors but do not substantively alter point estimates. Tables A.6, A.9, and A.10 present alternate estimates that drop all controls and examine the effect of treatment on changes between outcomes at students' stated first choices and the degrees in which they ultimately enroll. 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 extensive margin decision to matriculate to any tertiary degree program. The impact of treatment on the extensive margin is very close to zero and statistically insignificant across all subsamples. The second panel shows the impact of treatment on monthly debt, earnings gains (per month over 15 years vs. no college enrollment) and Net Value (the difference between the two). For the 23% of the sample who did not matriculate to any degree, these three values are by definition 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). The effects of treatment on Net Value and earnings outcomes at the chosen degree are not statistically significant at conventional levels. Point estimates suggest limited effects of treatment on cost outcomes, with larger effects for low- than high-SES students on earnings and Net Value.

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.[31] The third panel shows the impact of treatment conditional on matriculating to some tertiary degree. Treatment effects on Net Value and earnings outcomes are statistically significant in the full sample and are driven by large gains for students from low-SES backgrounds. For low-SES students, the intensive-margin effect of treatment is to raise Net Value at the chosen degree by \$15,274 CLP. This is equal to 3.4% of mean Net Value for low-SES students matriculating in college, 5.3% of the average gain associated with a switch to a peer institution, and 28.4% of the average monthly debt payment. The impact of treatment comes from gains in earnings, not savings on tuition. This is in part because there is less variation in tuition than earnings across degrees. However the sign of the treatment effect on monthly debt is positive. This is reminiscent of "returns chasing" in savings investments: individuals choose funds that have higher costs (with certainty) if the funds have higher past returns (see for example Choi, Laibson and Madrian 2011).

[31] 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)). Note also here that treatment is independent of student observables conditional on matriculation. A joint test of the effect of student observable characteristics on treatment within the sample of matriculating students fails to reject the null, returning a p-value of 0.194.

Recall that earnings gains and Net Value were calculated using earnings and costs projections for graduates. The fourth panel of Table 3 presents estimates of the effect of treatment on earnings at age 26 conditional on enrollment as opposed to graduation (the measure used in Figures 1 and 2). As discussed in Section 3.3, treatment effects measured using the age 26 earnings measure reflect changes in degree "value added" conditional on gender, student SES, and test score. We find positive and statistically significant treatment effects for this outcome as well, again driven by gains for low-SES students. One way to frame these results is in the context of the cross-SES earnings gaps displayed in Figure 2. The treatment effect on predicted earnings for low-SES students of $11,759 CLP is equal to 18.4% of the gap between earnings outcomes for high- and low-SES students conditional on ability (i.e., the average gap between the upper and lower lines in Figure 2, weighted by the low-SES score distribution). The treatment effect is equal to 38.4% of the component of that gap driven by differential degree choice (i.e., the sum of the gap between the lower two lines and the gap between the upper two lines).

The fifth panel of Table 3 presents estimates of the effect of treatment on graduation rates and average duration of attendance at students' chosen degree programs. Treatment does not move students towards longer degree programs or degree programs with higher graduation rates.

In the Online Appendix, we present additional analyses that explore how changes along different enrollment margins lead to the changes in Net Value we observe. Figure A.1 shows how the distribution of Net Value at the matriculating institution differs for the treatment and the control group. The treatment effects we observe come from a shift of mass from between the 10th and 50th percentiles of the control group distribution to roughly the 50th through 95th percentiles. As reported in the top row of Table A.7, low-SES students in the treatment group are 4.7% less likely to matriculate in degrees with Net Values below the control group median. Treatment appears to push applicants away from low-earning degree programs.

Students whose beliefs about own future earnings were above those for past graduates are the most likely to switch from their pre-treatment top choice. Table A.8 presents results from logit specifications in which outcome variables are dummies for an applicant matriculating in a degree different from the stated first choice, a degree in an institution different than the stated first choice, a degree in a different narrow major classification than the stated first choice, and a degree in a different broad field than the stated first choice. We interact treatment with indicator variables for each tercile of the expectation error distribution (the difference between own expected Net Value at the first choice and the Net Value information we present to students). We control for these indicator variables as well as for randomization blocks. Estimates of average marginal effects indicate that treated students in the lowest tercile of expectation errors (who on average slightly underestimate Net Value at their top choice) do not change their behavior in response to treatment. However, low-SES students in the top tercile of Net Value overestimation are 6.6 percentage points more likely to switch narrow majors (relative to mean rate of 57%) and 4.8 percentage points more likely to switch broad fields (relative to a mean rate of 18%) than students in the lowest tercile. Effects on institutional switching are slightly smaller and not statistically significant even for students who overestimate earnings.

These findings are consistent with a decomposition exercise, presented in the lower two rows of Table A.7. Using enrollment weights, we regress our degree-specific Net Value estimates on institution and major dummies. These dummies capture 94% of the variation in Net Value across degrees. We then estimate specifications identical to those in Table 3 but with estimated institution and major effects on the left hand side. Treatment raises the institutional component of Net Value for low-SES students by a statistically insignificant 2,225 CLP. In contrast, treatment raises the major-specific component by 13,826 CLP, or 91% of the overall effect reported in Table 3. To summarize, students who overestimate earnings are most likely to change their behavior following treatment, and the bulk of the observed change in Net Value is attributable to major switching as opposed to institution switching.

5.3.3 Heterogeneous treatment effects using survey data and choice model predictions

Table 4 presents the effects of the informational intervention on Net Value conditional on matriculation for subgroups of the population predicted to have large or small effects based on equation 6. Tables A.11 and A.12 report results for the matriculation and predicted earnings outcomes. Note that the subgroup definitions displayed here are only weakly correlated, so the subgroups represent distinct cuts of the data. We document the relationship between the subgroup variables in Table A.13.

The first panel in Table 4 presents results for students who claimed to know at least one of tuition, expected own earnings, and expected average graduate earnings for their first choice degrees and for students who claimed not to know any of the three. We use this as a coarse measure of the precision of prior beliefs. In the pooled sample, treatment effects are larger in magnitude for students with less information on earnings and cost outcomes, though less precisely estimated. Focusing on low-SES students, treatment effects are quite large for students with low information at baseline. Gains in Net Value for this group reach $28,701 CLP, or 7.0% of average Net Value in the low-information sample. This is more than twice the size of the effect for the high-information group, though relatively large standard errors do not allow us to differentiate statistically between the two estimates. These findings are consistent with the prediction from equation 6 that low-information students should respond more strongly to treatment. For high-SES students, this pattern is reversed, with smaller (though again imprecisely estimated) effects for low-information students. One possible explanation is that lack of information on earnings and costs for high-SES students reflects limited interest in earnings outcomes even with low acquisition costs, while lack of information on these outcomes for low-SES students reflects high acquisition costs.

The second panel of Table 4 shows estimated treatment effects for students who report that they are absolutely certain of their application choices and for students who say they are not. We interpret this measure as capturing non-pecuniary preferences for chosen degrees at the time of information revelation, though it may capture pecuniary preferences as well. (We note that stated certainty about degree choice has a weak *negative* correlation with knowledge of earnings outcomes, suggesting the former rather than the latter.) Effects in the full sample and for both high- and low-SES students are small and statistically insignificant for students who are certain of their preferences. They are large and statistically significant for students who are not certain. By the time of loan application, many students appear to have formed strong opinions about different degree programs even in the absence of accurate information on cost and labor market outcomes. Earnings disclosure has little effect on these students.

Equation 6 also suggests that students may not respond strongly to treatment if the variance of earnings outcomes within their choice set is relatively low. The third and fourth panels of Table 4 address this prediction. Because we do not observe students' full choice sets or choice probabilities, we consider proxies for variation in earnings outcomes within the choice set. The third panel compares students who list careers in more than one field among their top three choices to students who do not. These students are likely considering degrees with a broader range of financial outcomes. They may also have weaker field-specific preferences. Treatment effects are large for students considering careers in multiple areas, and close to zero and statistically significant for students considering careers in a single area. The fourth panel compares students with above-median variance in Net Value of listed careers to students with below-median variance. For low-SES students, treatment effects are positive and statistically significant for students with high-variance listed choices, and small and statistically insignificant for students with low-variance choice sets.

The bottom panel of Table 4 shows treatment effects for students who have at least one parent with tertiary education, and for students who do not. We include this analysis because it was planned at the time of study design, though it is unclear how parental education would influence the treatment effect. For example, children with college-educated parents may have stronger or weaker preferences for earnings or tuition costs. Regardless of mechanism, we find that treatment effects are on average somewhat larger for children of more educated parents.

6 Discussion

6.1 Policy implications and cost-benefit analysis

We assess the efficacy of the informational treatment in two ways. First, we consider the intervention as a solution or partial solution to problems of student default. Second, we evaluate the return on investment associated with the intervention. Even if the intervention does not change default rates very much, it may be a cheap way to nudge a fairly large number of students towards higher Net Value degrees.

Using our administrative data on loan taking and repayment, we compute degree-specific rates of repayment and default as of 2013, and examine whether treatment pushes students to choose degree programs with lower default and/or higher repayment rates for past students. Because the CAE loan program is relatively new, students using CAE loans at many degree programs have not yet entered repayment. We focus our attention on degree programs in which we observe at least ten students who have entered repayment and for whom the time elapsed since matriculation is at least the predicted degree length plus 1.5 years.[32] As reported in the upper panel of Table 5, we are able to compute repayment rates for 58.0% of the degrees chosen by students in our experimental sample. These degrees tend to be shorter in duration and less selective than the enrollment-weighted population of degrees. Treatment does not make students more likely to choose a degree in the repayment sample.[33]

The second panel of Table 5 displays unconditional mean on-time repayment and default rates for degrees chosen by high- and low-SES students, mean repayment and default rates conditional on entrance exam score (weighted by the population score distribution), and the effects of the information treatment on default and repayment rates at chosen degrees. On-time repayment rates are 8.5 percentage points higher at the degrees chosen by high-SES students. This gap falls to 1.4 percentage points conditional on entrance exam score. Treatment pushes low-SES students toward degrees with on-time payment rates that are 1.0 percentage points higher. This is equal to roughly 12% of the unconditional gap between choice outcomes for high- and low-SES students and roughly 70% of the gap conditional on exam score. Findings for default rates are similar. The effect of treatment on default rates in the full population is small and does not differ significantly from zero.

To evaluate the return on investment, we compute the present discounted value (PDV) of post-college earnings net of direct costs through ages 30 and 50. We extrapolate from our regression-based earnings measure using data on field- and selectivity-specific earnings trends. See Online Appendix Section 3 for a full description of the procedure. We estimate that the informational treatment raises the PDV of post-schooling earnings net of direct costs through age 30 by just under one million Chilean

[32] We employ this last restriction so as to avoid considering only the dropouts from longer degree programs.
[33] Table A.12 describes degrees in and out of the repayment sample.

pesos, or USD $1,923. Because treatment does not affect expected degree length, the cost of earnings forgone while in school seems likely to be negligible. Multiplying by the 37,747 students in the matriculating respondent sample suggests an increase in the PDV of aggregate earnings net of the direct costs education of roughly USD $72 million at the loan interest rate of 2%. This far exceeds the costs of the intervention. The survey and intervention took students an average of 15 minutes to complete. Valuing students' time at the Chilean minimum wage of USD $2.30,[34] this suggests a total participation cost of USD $28,720 across all respondents. We conservatively estimate (over-estimate) the total cost of data construction efforts for researchers and government officials at USD $1m. This includes fixed startup costs that would not be incurred in subsequent years. The informational intervention does not seem likely to have large effect on rates of student default, but because it is scalable and cheap to implement, the return on investment in producing and administering the treatment remains quite large.

6.2 Validating earnings projections

Earnings disclosure policies necessarily rely on extrapolating future outcomes from past outcomes, most often in contexts where random assignment and compliance enforcement are not possible. This raises several issues. First, estimated degree effects may not accurately capture causal effects for past students. Second, even if one obtains unbiased estimates of causal effects for some groups of past students, these may not provide an accurate guide to policy effects. This is because a) labor market conditions may change over time, perhaps because b) large scale disclosure policies may induce general equilibrium changes in skill prices, and because c) degree effects for students who respond to disclosure may differ from those for students who choose degree programs for other reasons.

Benchmarking OLS estimates to RD estimates

In Section 5 of the Online Appendix, we compare our OLS measures of predicted earnings to regression discontinuity estimates similar to those in HNZ (2013). We find that, after adjusting for measurement error, observed differences in earnings outcomes across admissions thresholds are similar to those predicted by the OLS estimates for students' above- and below-threshold enrollment choices. Observed earnings discontinuities rise one-for-one with predicted discontinuities, and are close to zero at thresholds where OLS estimates predict a zero effect. These findings suggest that our measures of predicted earnings may succeed in capturing the causal effects of enrollment in different degree programs.

This is consistent with recent research in the context of teacher and school effects. Several papers in this literature find that value added estimates in many cases accurately capture differences in causal

[34] OECD 2014. http://stats.oecd.org/Index.aspx?DataSetCode=RMW.

effects (Kane and Staiger 2009; Chetty, Friedman, and Rockoff 2014a, 2014b). The authors of these papers argue that selection of teachers based on student unobservables may be negligible given observed assignment policies. In the context of higher education, selection on unobservables may be small if students are uninformed about their own degree-specific pecuniary deviation from mean returns conditional on observables, or if they weight non-pecuniary factors heavily when choosing schools. Table 2 suggests that the former is true here, and additional evidence presented in Hastings et al. (2015) and discussed in Section 5.2.2 indicates the latter may be true as well. Selection could also take place on the institution side through admissions and recruitment policies. However, if less selective institutions face little incentive to selectively admit students based on expected match quality (e.g. digital technology institutes do not screen students for relative ability in that degree), but instead respond to public subsidies by lowering admissions standards and expanding enrollment (i.e., maximizing enrollment rather than value-added in the labor market), supply-side-driven selection may also be limited in equilibrium.[35]

Time trends and general equilibrium effects

Predicting future skill prices is difficult, but we can use older data from HNZ (2013) to compare our OLS earnings estimates to outcomes for cohorts of applicants from the 1980s and early 1990s. As discussed in Section 5 of the Online Appendix, we find that observed admissions threshold-crossing effects for pre-1994 college applicants track predicted values based on our OLS estimates. As with the more recent data, the slope of the relationship between observed and predicted threshold-crossing effects is close to one and the intercept is close to zero. Because the analysis is limited to the subset of CRUCH degree programs that persist in the data between the 1980s and early 1990s and the 2000s, we interpret this finding cautiously. However, it does support the hypothesis that at least some degree effects are stable over time.

It is also possible that treatment itself could induce skill price changes. This seems unlikely given the relatively small shifts in degree choices we observe. As shown in Table A.15, the distributions of broad field and of degree type are similar in the treatment and the control group. The two highest-earning fields in our data are science/technology and health. Treatment group students are 2.11% more likely to enroll in science/technology degrees and 1.98% less likely to enroll in health fields than control students. Treated students are 0.92% more likely to enroll in professional degrees (as opposed to technical degrees) than control students. Treatment does not cause students to flood the market in certain fields or degree

[35] A more general point here is that if disclosure policy generates sufficient demand response, and/or regulation is shaped to effectively incentivize institutions to screen students for match quality, then, in equilibrium, OLS value-added estimates of past returns may be of limited relevance for the average high school graduate, but still very relevant for students who can actually gain admission. This depends on institutions having both the incentive and ability to effectively predict enrollee success. In many markets, private firms provide screening and information to tailor experience and products to customer types. Firms with access to data aggregated over many customer decisions and outcomes may face lower screening costs than either regulators or individuals (Thaler and Tucker 2013).

levels. If skill prices are determined by supply of and demand for graduates at the field-degree type level, it would require very large elasticities of demand for the changes we observe to substantially reduce cross-field wage gaps, which are quite large. The gap in average predicted earnings between science/technology and the next highest non-health field, social science, is 35%, while the gap between earnings for students in professional as opposed to technical degrees is 79.1%.

6.3 Treatment effect size and treatment timing

While treatment increased net earnings and decreased default rates at the degrees students chose to enroll in, and did so particularly for low-SES students, the impacts are relatively small when compared to potential gains across degrees available to students conditional on academic ability. Our intervention reached students at a salient point in time - near the time of choice and as part of the loan application process. But it could be that reaching students with degree-specific earnings information earlier in their decision process would have a larger impact. We do observe, for example, that students who are already set on enrollment plans are non-responsive to new information, regardless of how uninformed their decision process was to date. However, recent consumer finance research suggests that the effects of informational interventions are the largest when they occur at or near the time of choice (Hastings, Madrian, and Skimmyhorn 2013). In addition, without entrance exams scores in hand to determine which degrees are in their choice set, students may have difficulty putting information on earnings outcomes to use.

In addition, information given early in high school necessarily will be more out of date. Older information may be less useful to students, and the relative insensitivity of long-run averages to short-run changes in effort may dampen demand side incentives to raise earnings outcomes by increasing quality. Policies that improve general knowledge of returns and costs early on and policies that provide detailed, updated information at the time of choice may be most effective in combination. Beyer et al. (2015) discuss the trade-off between short and long-run earnings measures in the context of a policy that combines earnings disclosure with earnings-dependent, degree-specific caps on the availability of loan funding.

7 Conclusion

We administered a survey and field experiment in partnership with the Chilean Ministry of Education as part of the 2012-2013 student loan application process. We document the beliefs and preferences of college applicants, and estimate the effects of disclosing information about institution-and-major-specific earnings and cost outcomes on matriculation choices as a function of prior plans for and beliefs about higher education outcomes. We focus on the higher education choice process for loan applicants from low-SES backgrounds. Our randomized controlled trial directly tested a government-implemented information disclosure policy aimed at improving the expected educational and financial outcomes for students coming from backgrounds with limited information about and experience with higher education.

Using a unique database of linked high school, higher education, tax return, and student loan data, we show that average earnings outcomes for past enrollees rise with entrance exam scores and that many students choose degrees that appear to add little value to average earnings relative to not going to college. We find that earnings for high-SES students are 13.5% higher than those for low-SES students at the same score level, with approximately half of this gap attributable to cross-SES differences in degree choice within ability level (as opposed to within-degree earnings differences). Responses to survey questions administered as part of the federal student loan application process show that many students have limited knowledge of the earnings and cost outcomes associated with different degree programs, and that students from low-SES backgrounds make enrollment decisions with less information about costs and labor market outcomes than students from higher-SES backgrounds. These findings suggest scope for public policies that compile and disclose earnings and cost information on higher-education degree options.

Our randomized controlled trial directly tests the effects of such a policy. We provided a randomly selected subset of financial aid applicants with information on earnings and cost outcomes at the degrees to which they plan to apply, as well as access to a searchable database of outcomes for other degrees. Treatment causes low-SES students to enroll in degree programs with higher earnings and value added outcomes. The informational intervention raises predicted earnings at age 26 for low-SES students by an amount equal to 18.4% of the cross-SES earnings gap, and 38.4% of the component of that gap attributable to enrollment choices for high- and low-SES students at the same score level. Consistent with the predictions from a model of degree choice with limited information, effects are largest among low-SES students who had less information on earnings and costs and who exhibited lower levels of pre-intervention preference for a particular degree. Among these subgroups of low-SES students, effect sizes are roughly twice as large.

Conditional on entrance exam scores, treatment effects reduce the gap in default rates at degrees chosen by low- and high-SES students by roughly 70%. However, this gap is small, so effects on the overall average default rates at the degrees students choose are limited. The informational treatment appears to offer a high return on investment overall. Treatment raises the presented discounted value of earnings net of direct costs for matriculating students through age 30 by a little under USD $2,000.

Though this is only 3% of the mean present value of net earnings in the experiment sample, the treatment is very inexpensive and easy to reproduce and scale each year. If earnings value added estimates for past enrollees are a guide to those for current applicants, our treatment would raise aggregate earnings by USD $72 million if applied to the full sample of respondents. This value far exceeds the costs of administering the treatment, even including one-time fixed costs.

Gains in the predicted net present value of the chosen degree are generated by higher returns rather than lower tuition costs. Paralleling findings from research on markets for financial investments, this suggests that demand response to information disclosure could chase returns estimates rather than put pressure on tuition and fees, even if costs and earnings gains are presented separately. Our results may be related to limited financial literacy and poor understanding of loan terms we observe in other surveys of student loan takers (Hastings et al. 2015). The effects of limited financial literacy may be exacerbated if students interpret the public provision of loans as an endorsement of loan-eligible degree programs.

Our findings suggest that although providing information on earnings and cost outcomes for different degree programs offers a high return on investment for policymakers, it is unlikely to substantially reduce rates of default. It is possible that information could have a larger effect on behavior if it were distributed earlier in secondary school as well as at the time of loan and enrollment choice (Dinkelman and Martínez, 2014). Though it may serve a motivational purpose, information provided early in secondary school is likely a weaker guide to choice given changing macroeconomic and labor markets. In addition, dated information may provide less incentive for institutions to improve value, as gains from investments in quality may not impact demand for many years. Regulation of higher education institutions may provide an effective alternative or supplement to disclosure (Beyer et al. 2015). In particular, providing incentives for non-selective, enrollment-maximizing degree programs to raise quality and screen for good matches may help students benefit from information that is at present available only to higher education providers.

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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	(6) Treated & Searched
PSU Score (Ave. Lang & Math)	508.4	518.8	524.5	536.3	537.0	543.4
	(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)
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)
Ave. of Lang + Math SIMCE (Z-score)	0.326	0.414	0.465	0.568	0.581	0.635
	(123,937)	(79,504)	(56,255)	(38,625)	(18,981)	(8,282)
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)
Net Value 1st Choice Degree	--	--	--	734,948	736,867	769,452
	--	--	--	(40,806)	(20,048)	(8,683)
Potential Institution Gains	--	--	--	267,566	266,131	262,685
	--	--	--	(48,672)	(23,922)	(10,350)
Observed Earnings at Age 26	--	--	--	464,307	466,988	482,513
	--	--	--	(31,549)	(15,532)	(6,713)
Matriculation Rate	--	--	--	77.0%	77.2%	77.6%
	--	--	--	(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. Net-Value 1st Choice Degree is the Net-Value displayed in the experiment for the student's stated first-choice degree. Potential Gains from Switching Institution is the maximum gains in net-value that was displayed to treatment group if they chose a different institution in the same major as their stated first-choice degree.

Table 2. Survey Expectations

	% "I Don't Know"	Mean Error	P25	Median Error	P75	N
Tuition Expectation Errors						
All Students	33.2	0.9	-16.5	-5.5	5.5	49,166
High-SES	30.7	-2.1	-15.6	-5.6	4.4	29,850
Low-SES	37.0	6.5	-18.9	-5.4	7.3	16,594
Typical Earnings Expectations Errors						
All Students	47.7	60.9	-23.6	7.8	56.5	49,166
High-SES	43.5	45.5	-25.5	3.2	46.8	29,850
Low-SES	54.4	87.6	-19.9	15.6	74.1	16,594
Own Earnings Expectation Errors						
All Students	35.8	51.8	-25.0	2.4	45.6	49,166
High-SES	32.6	40.4	-25.7	-0.2	40.5	29,850
Low-SES	41.1	70.8	-24.4	6.7	56.1	16,594
Certainty of Degree Choices						
	Absolutely Certain	Quite Certain	Fairly Certain	Somewhat Certain	Not at all Certain	N
All Students	33.8	34.6	22.3	6.6	2.6	49,166
High-SES	32.7	35.9	22.4	6.5	2.6	29,850
Low-SES	34.4	33.0	22.9	7.0	2.6	16,594

Notes: All values are stated as percentages. The first panel 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. This second and third panels present 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. The last panel 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 3. Impact of Treatment on Outcome Variables

	Pooled	Low-SES	High-SES
Matriculation	0.004	0.000	0.003
	(0.004)	(0.008)	(0.006)
All Students			
Net Value	8,270	10,749	5,427
	(5,217)	(7,296)	(7,370)
Earnings Gains	8,856	11,252	5,932
	(5,740)	(7,973)	(8,139)
Monthly Debt	267	319	34.8
	(536)	(722)	(775)
Conditional on Matriculation			
Net Value	10,029*	15,274*	8,040
	(4,230)	(7,149)	(5,435)
Earnings Gains	10,971*	16,083*	9,066
	(4,532)	(7,671)	(5,819)
Monthly Debt	376	763	125
	(435)	(680)	(580)
Degree Average Earnings at Age 26	6,324*	11,759**	3,789
	(2,814)	(4,425)	(3,771)
Monthly Payment (conditional on enrollment)	498	824	344
	(459)	(758)	(568)
Degree Graduation rate ('00-'05 cohorts)	0.002	0.002	0.002
	(0.003)	(0.005)	(0.004)
Expected Length of Matric. Degree ('00-'05 cohorts)	0.014	0.019	0.010
	(0.018)	(0.032)	(0.023)

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. 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 50 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 and fourth panels report 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 are 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 Relative Returns 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 age 50 for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR Relative Returns calculate predicted earnings using the same methodology, out to age 30. 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 <0.10, * p < 0.05, ** p < 0.01, *** p < 0.001.

Table 4. Impact of Treatment on Net Value

	Pooled	Low-SES	High-SES
Information on Career Earnings & Costs			
Some Information	9,729*	12,643+	9,484+
	(4,393)	(7,470)	(5,628)
No Information	13,002	28,701+	-151
	(9,925)	(15,026)	(13,919)
Certainty of Enrollment Plans			
Absolutely Certain	3,387	7,634	2,616
	(5,696)	(9,236)	(7,656)
Uncertain	13,341*	19,774*	10,405
	(5,393)	(9,420)	(6,776)
Number of Fields Listed in Top Choices			
One Field	826	-197	-204
	(5,468)	(11,404)	(6,405)
More Than One Field	15,343*	21,722*	14,146
	(6,227)	(9,155)	(8,621)
Variation in Net-Value of Listed Choices			
Low Variation	4,319	-4,904	8,723+
	(4,267)	(7,904)	(5,186)
High Variation	8,898	25,037*	2,870
	(7,319)	(12,453)	(9,349)
Parent's Tertiary Education			
At Least One Parent Has Some Tertiary Education	15,376*	39,261*	12,615+
	(6,639)	(18,359)	(7,317)
Neither Parent Has Any Tertiary Education	9,964+	17,308*	4,881
	(5,778)	(8,665)	(7,840)

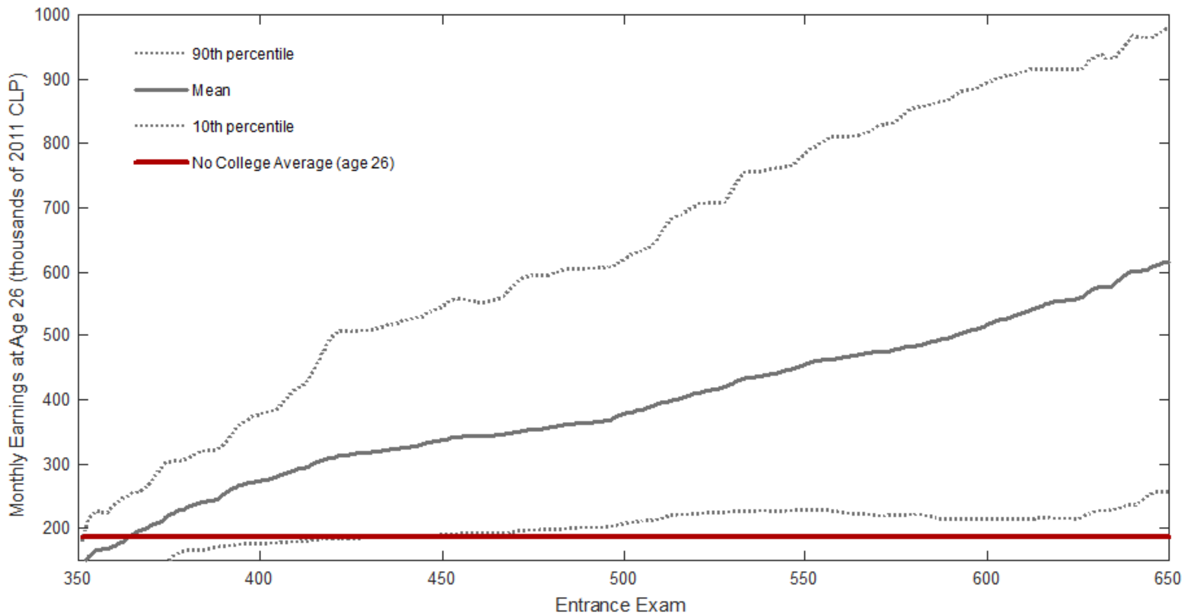
Notes: Net Value is conditional on matriculation to a higher education degree in 2013. "Some Information" is defined if the respondent guessed at least one of the following values: tuition, own expected earnings and typical expected earnings in the survey. "No Information" is defined if the respondent answered "I don't know" for all three value expectations. "Absolutely Certain" is defined if the respondent answered "I am absolutely certain" in response to survey question Q3. "Uncertain" is defined if the respondent answered any one 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. "One Field" is defined if the respondent only listed one field of study in their three choices in Q2. "More Than One Field" is defined if the respondent listed more than one field choice in their three degree choices in Q2. "Low Variation" is defined if the student's standard deviation in net-value among their top three choices was less than the median standard deviation in net-value of choices for all respondents; "High Variation" is larger than the median. "At Least One Parent Has Some Tertiary Education" is defined if at least one of the student's parents have some tertiary education, as reported by the students during SIMCE standardized tests. "Neither Parent Has Any Tertiary Education" is defined if neither of the respondent's parents have any tertiary education. Table reports coefficients on treatment from a regression of the net-value on treatment, the net-value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. 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 50 point bins of prior PSU scores. Net Value are 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 Relative Returns 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 age 50 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 using the same methodology, out to age 35. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined if the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5. Impact of Treatment on Returns to Degree & Repayment Rates

	Pooled	Low-SES	High-SES
Students who matriculate to a degree in repayment sample			
Percent	0.580	0.606	0.561
Treatment effect	-0.002	-0.004	-0.004
	(0.005)	(0.009)	(0.007)
Degree on-time repayment rate			
Average	0.592	0.543	0.628
Average conditional on exam score	0.602	0.594	0.608
Treatment effect	0.002	0.010**	-0.003
	(0.002)	(0.004)	(0.003)
Degree default rate			
Average	0.305	0.350	0.272
Average conditional on exam score	0.292	0.299	0.288
Treatment effect	-0.001	-0.008*	0.004
	(0.002)	(0.003)	(0.003)
PDV of long- and short-run returns			
Returns to degree at age 50	2,459,579+	4,190,955*	1,458,519
	(1,480,585)	(2,107,587)	(1,947,204)
Returns to degree at age 30	999,737*	1,369,854*	778,104
	(399,217)	(568,630)	(526,576)

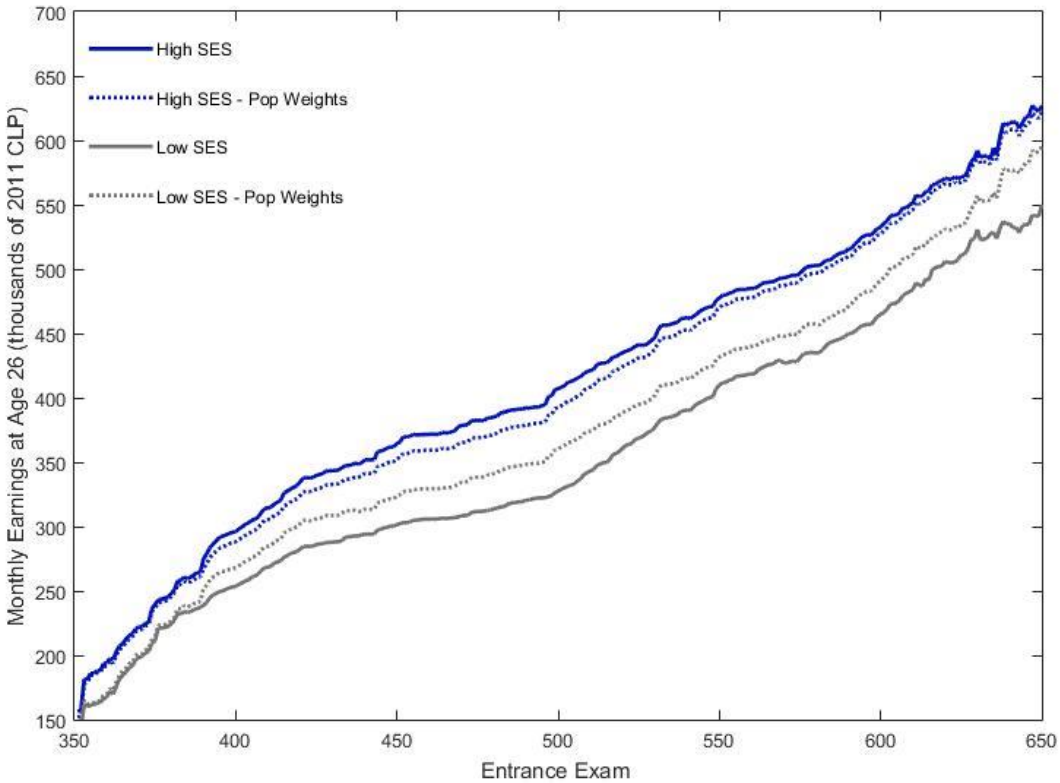
Notes: Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Clustered standard errors are in parentheses. For 2012 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 50 point bins of prior PSU scores. The LR and SR Relative Returns 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 age 50 for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR Relative Returns calculate predicted earnings using the same methodology, out to age 30. 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. Degree on-time repayment rates and default rates are conditional on the degree having at least 10 students in repayment as of April 2013. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 1. Predicted Monthly Earnings (Age 26)



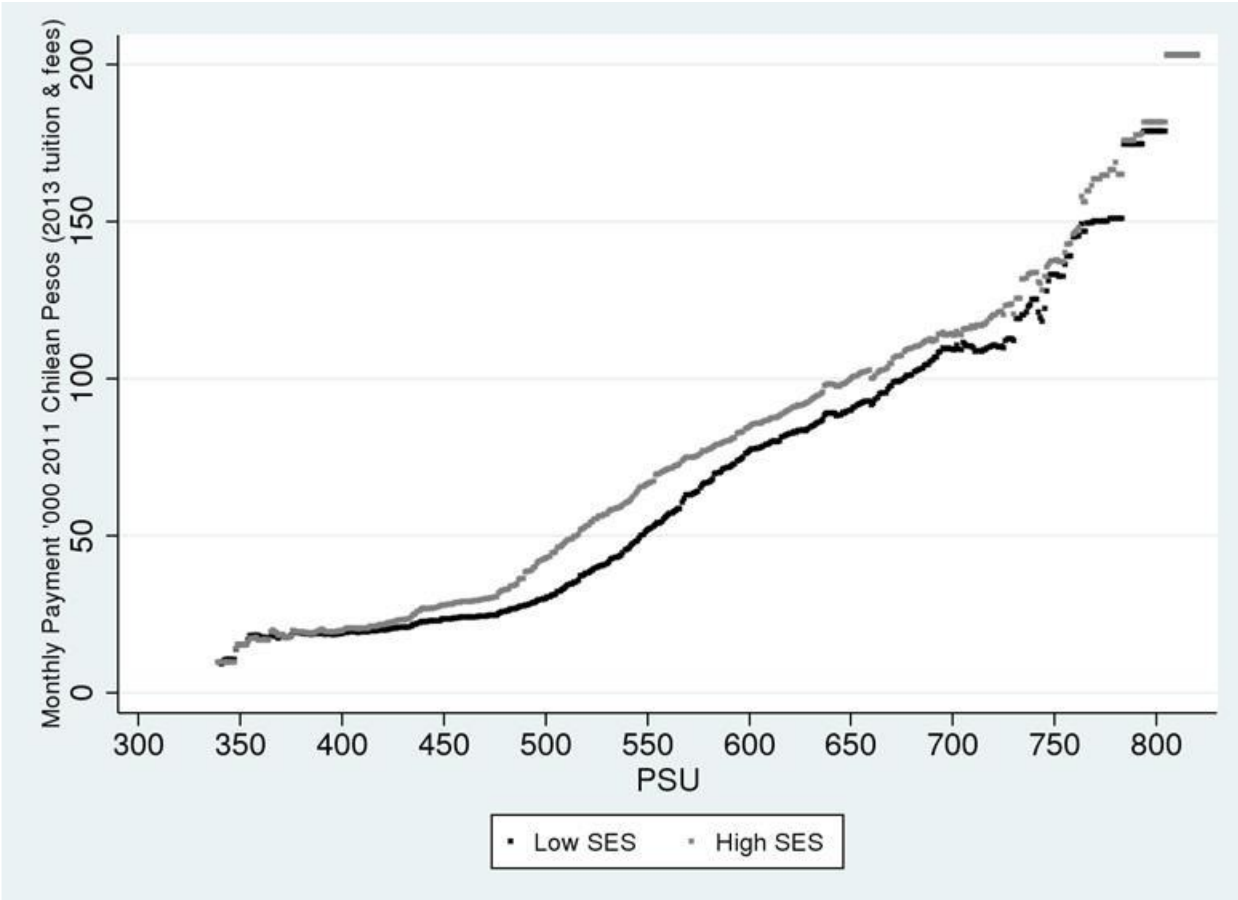
Notes: The figure shows the distribution of earnings that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted earnings for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations 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 for students with a PSU of X over the degrees they could get into. The red line represents the average earnings at 26 years of age for those who graduated high-school, but did not enroll in a HEI.

Figure 2. Predicted Monthly Earnings (Age 26) by Socioeconomic Status



Notes: The figure shows the distribution of earnings 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.

Figure 3. Degree Costs by Socioeconomic Status



Notes: The figure shows the distribution of monthly debt that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over monthly debt for observed experiment sample 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.