THE EFFECTS OF EARNINGS DISCLOSURE ON COLLEGE ENROLLMENT DECISIONS

Justine Hastings
Brown University & NBER
justine_hastings@brown.edu

Christopher A. Neilson
Princeton University & NBER
cneilson@princeton.edu

Seth D. Zimmerman
University of Chicago & NBER
seth.zimmerman@chicagobooth.edu

First Draft: September 2014
This Draft: July 2017

Abstract

We use a large-scale survey and field experiment to evaluate a policy that provided information about earnings and costs to college applicants in Chile. The intervention was administered by the Chilean government and reached 30% of student loan applicants. Students overestimate post-graduation earnings outcomes at low-earning college degree programs but have reasonably accurate beliefs about tuition. Disclosure of earnings outcomes for past graduates reduces demand for degrees in the bottom tercile of the expected labor market return distribution by 3.3%. Effects are concentrated among low-SES and low-scoring students. Effect size is limited by strong preferences for non-earnings degree attributes.

This paper was previously titled "Student Loans, College Choice and Information on the Returns to Higher Education." We thank Noele Aabye, Phillip Ross, Unika Shrestha, Anthony Thomas, and Lindsey Wilson for outstanding research assistance. We thank Nadia Vasquez, Pablo Maino, Valeria Maino, and Jatin Patel for assistance with locating, collecting and digitizing data records. J-PAL Latin America and in particular Elizabeth Coble provided excellent field assistance. We thank Ivan Silva of DEMRE for help locating and accessing data records. We thank the excellent leadership and staff at the Chilean Ministry of Education for their invaluable support of these projects including Harald Beyer, Fernando Rojas, Loreto Cox Alcaíno, Andrés Barrios, Fernando Claro, Francisco Lagos, Lorena Silva, Anely Ramirez, and Rodrigo Rolando. We also thank the outstanding leadership and staff at Servicio de Impuestos Internos for their invaluable assistance. We thank Gene Amromin, John Friedman, Brad Larsen, Brigitte Madrian, Judith Scott-Clayton, Jesse Shapiro, Will Dobbie, and seminar participants at the NBER Household Finance Meetings and Public Economics Meetings, Harvard University, Princeton University, Columbia University, Stanford University, University of Chicago Harris School, Rutgers University, University of Toulouse, Paris School of Economics, Pontificia Universidad Catolica de Chile, Vanderbilt University, Uppsala University, University of Memphis, University of Illinois-Chicago, International Bank of Development, and the World Bank for helpful comments. We thank Raj Chetty and Larry Katz for helpful discussions during the field experiment design. This project was funded by Brown University, the Population Studies and Training Center at Brown, and NIA grant P30AG012810. Online Appendix available at http://faculty.chicagobooth.edu/seth.zimmerman/research/papers/HNZ_disclosure_appendix.pdf. Required disclosure: 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 for the use or application made of the aforementioned information, especially in regard to the accuracy, currency, or integrity..

1 Introduction

This paper uses a large-scale field experiment and survey to study how beliefs about earnings and costs affect students' choices about where to enroll in college and what to study. Our context is Chile, a middle-income OECD country with a higher education market in which private and public universities compete for students who often finance their education with federally-subsidized student loans. Facing protests over educational debt burdens and rising rates of student loan default, the Chilean government set out to understand how students make enrollment decisions and whether information about earnings and costs for different colleges and majors can help them make wiser educational investments. We assisted policymakers with the design, implementation, and evaluation of a web-based disclosure policy that reached 30% of student loan applicants in 2012.

Disclosure policies are important to study for two reasons. First, college enrollment choices have large effects on earnings but are often made with imperfect information.[1] In the US, for example, overall increases in loan default in the 2000s were driven by borrowers at non-selective, for-profit institutions, where default rates in some cases approached 50% (Looney and Yannelis 2015). Students applying to high-default programs often report difficulty accessing reliable information on program costs and benefits.[2] Challenges accessing reliable information may be acute for the low-income students who make up the majority of enrollees at the worst-performing programs. Second, in an effort to improve transparency and help students make better-informed college enrollment decisions, many countries have adopted disclosure policies that collect and disseminate information on earnings and costs, including the US, Australia, Colombia, Mexico, and Peru (Neilson et al. 2016).

Disclosure may be an effective policy solution if credible information on labor market returns helps students make choices but is difficult to obtain. It may be less intrusive and faster to implement than regulation (Loewenstein et al. 2014, Douglas-Gabriel 2017, State of California Department of Justice 2016, United States Department of Education 2014a), and its benefits may grow over time if it encourages suppliers to compete on earnings value added or price. However, disclosure may be ineffective if beliefs about earnings are fixed or if information is difficult to deliver.

Working in collaboration with Chilean policy makers, we were able to collect and analyze unique data on how students make enrollment choices and how choices change in response to disclosure. We assisted with three aspects of policy design. First, we computed earnings returns

[1] Altonji et al. (2016) describe cross-major earnings differences in the US. Chetty et al. (2017) describe cross-institution earnings differences. Saavedra (2008), Hastings et al. (2013b), and Kirkebøen et al. (2016) provide causal evidence on differences in earnings outcomes by major and/or selectivity in Colombia, Chile, and Norway.

[2] See United States Government Accountability Office (2010), Lewin (2011), United States Department of Education (2013), Lederman (2009), and Lederman (2011).

by college and major. To do this, we constructed a dataset linking administrative high school, college, student loan, and earnings records for all Chileans from 1980 through 2015. Second, we created a web survey that measured students' preferences and beliefs about earnings and costs. 49,166 student loan applicants completed the survey, which was administered by the Ministry of Education as part of the student loan application process. The survey asked students about application plans, own earnings and tuition cost expectations, and expectations for the typical student. Third, we integrated the survey with an intervention that provided randomly selected students with information from our database on average monthly earnings and loan repayment costs for graduating students. For each degree program, the intervention summarized earnings returns relative to non-attendance by projecting expected earnings and loan payments over the 15 year repayment period for federal student loans. We label this summary measure the Net Value of the degree.

In total, 30% of all student loan applicants in Chile participated in the intervention as part of the treatment or control group. The broad sampling frame allows us to study the lower-income, less-prepared students who are most likely to enroll in low-quality programs. In contrast, most previous studies of earnings beliefs in college choice focus on students already enrolled at selective universities or on students choosing whether or not to attend college at all.[3] We show that students planning to enroll in low-earning programs systematically overestimate earnings for typical students relative to those for past graduates. On average, students who plan to enroll in a degree program in the bottom decile of the mean earnings distribution believe that the typical graduate of that program earns 139% more than past graduates actually earned. In contrast, students listing a top-decile program as their first choice underestimate earnings by an average of 41%. Students follow through on stated enrollment plans, so belief errors conditional on enrollment are similar.

Students choosing low-earning degree programs have lower test scores and are more likely to come from low socioeconomic status (SES) backgrounds than students choosing higher-earning programs. 56% of students listing a bottom-decile degree program as their first choice come from a low-SES background, compared to 16% in the top decile. Accordingly, low-SES students are much more likely to have large belief over-estimates than other students. Beliefs about costs follow a similar pattern, but with smaller systematic errors. Students planning to enroll in the most costly degree programs underestimate costs by an average of 15%, while students planning to enroll in the least costly programs overestimate costs by 3%.

Beliefs about earnings and costs are linear in observed values with a slope of less than one.

[3]Papers studying the role of earnings beliefs in major choice for students already enrolled in selective colleges include Zafar (2011), Arcidiacono et al. (2012), Stinebrickner and Stinebrickner (2013), Wiswall and Zafar (2014), and Wiswall and Zafar (2015). Papers studying beliefs about college vs. non-college earnings include Jensen (2010), Nguyen (2010), Oreopoulos and Dunn (2013), Dinkelman and Martinez (2014), Bleemer and Zafar (2015), and McGuigan et al. (2016).

This linear form is consistent with the predictions of a standard Bayesian model where students with imprecise signals about degree-specific earnings shrink beliefs towards a mean belief about earnings for college graduates. In the absence of trustworthy degree-specific earnings information, programs with lowest earnings for graduates benefit the most. Even mean-zero uncertainty over degree-specific earnings leads to large positive errors in belief expectations at the lowest-performing degrees. These findings suggest that disclosure may push students away from the lowest-earning programs.

We study the effects of disclosure using the randomized trial. We find that disclosure does not affect *whether* students enroll in college, but does affect *which* degree programs they choose. Overall, treatment raises predicted earnings at the programs students choose by 1.4% of the control group mean. This is approximately 11% of the gap in predicted earnings between students with large and small belief errors. We show that this finding holds both for our Net Value measure of returns conditional on graduation and for "value-added" regression predictions of earnings conditional on enrollment that take into account student demographics and baseline academic achievement.

Disclosure shifts choice most for the low-SES students with low admissions test scores who are most likely to enroll in the lowest-earning degrees. For these students, treatment raises predicted earnings by 3.2% of the control group mean and 26% of the gap in predicted earnings between students with large and small belief errors. Treatment shifts enrollment away from degree programs at the bottom of the Net Value distribution and towards programs between roughly the 50th and 90th percentiles. Programs in the bottom tercile of Net Value see enrollment decrease by 3.3% in the full sample, by 4.6% for low-SES students, and by 3.9% for low test score students. Consistent with our finding that cost beliefs are more accurate than earnings beliefs, treatment does not push students towards lower-cost programs. This result parallels previous studies showing limited effects of interventions that provide information on costs, and highlights the importance of studying earnings disclosure independently.[4]

Our findings show that the effects of disclosure are concentrated at the bottom of the earnings distribution. To interpret these findings, we estimate a discrete choice model of college enrollment. The model helps explore why disclosure may have a limited impact on choice. One possibility is that disclosure helps students learn about earnings, but that factors such as non-earnings attributes or capacity constraints limit its impact. Alternatively, students may not learn much from the policy. In the model, disclosure affects choice by increasing the precision of students' beliefs about earnings and cost outcomes, leading them to weight observed earnings and costs more heavily when choosing a degree. This is consistent with the Bayesian model of belief formation from the descriptive analysis. We use the survey data to capture preference hetero-

[4]See Bettinger et al. (2012) and Hoxby and Turner (2013). Interventions that include help with application logistics show more positive effects. For more on application complexity, see Avery and Kane (2004); Hoxby and Avery (2013); Scott-Clayton (2012); and Dynarski and Scott-Clayton (2013).

geneity by major, field of study, and institution.

Estimates indicate that treatment raises the weight students place on earnings by 32%. This corresponds to a 43% reduction in the noise-to-signal ratio for earnings beliefs. The reductions in noise-to-signal for low-SES and high-SES students are 71% and 25%, respectively. Treatment does not meaningfully change the weight students place on costs. Disclosure succeeds in improving the information on past earnings outcomes, but not enough to cause a large shift in enrollment. The high values students place on institutions and majors limit the impact of changes in beliefs on enrollment decisions. For example, we show that the earnings elasticity of enrollment at students' stated first choice degrees would rise by 73% if institution-specific preferences were eliminated.

Capacity constraints did not mute treatment effects. We simulate the effects of a universal disclosure policy on demand at each degree program relative to a counterfactual in which no students are treated. Treatment reduces enrollment at degrees in the bottom 10% of the expected earnings distribution by 4.6%, and increases enrollment in degrees above the median by just under 2%. Consistent with our reduced-form estimates, students leave the lowest-return programs but choose a wide range of higher-earning degrees. These higher-earning degrees are non-selective and report slack admissions capacity that exceeds the additional demand induced by disclosure. The degrees that experience the largest declines in demand are typically run by for-profit providers and are often in low-earning but potentially easy-to-market fields such as cooking, sound engineering, and tourism. These are similar to programs identified in other contexts as disproportionate producers of poor student outcomes (Looney and Yannelis 2015).

The finding that capacity constraints do not bind suggests that experimental estimates of treatment effects are reasonable predictors of the equilibrium effects of a fully scaled policy in the short run. Concerns about how the effects of the policy may change with full scale-up and the transition from researcher control to policymaker control (Muralidharan and Sundararaman 2015) are limited here because the government administered the evaluation itself. However, the extent to which demand-side impacts on low-performing programs could incentivize increased value added and efficiency in the non-selective higher education market remains to be seen (Beyer et al. 2015).

The implications of disclosure policies for earnings over the long run depend on how predicted earnings map to earnings outcomes. We assess the predictive power of our observational earnings measures using quasi-random variation in degree assignment generated by discontinuous admissions rules (Hastings, Neilson, and Zimmerman 2013; henceforth HNZ). We show that changes in observed outcomes across an admissions threshold rise one-for-one with changes in predicted outcomes based on the degrees in which above- and below-threshold students enroll. This exercise extends a literature that uses quasi-experimental designs to validate estimates of teacher effects (Chetty et al. 2014a, Chetty et al. 2014b) and school effects (Deming 2014) to

a higher education setting. Our findings contrast with recent evidence suggesting that some students select into degree programs on the basis of individual-specific comparative advantage (Kirkebøen et al. 2016), but may be unsurprising in a setting where students appear to have little information about differences in earnings across degree programs. They are also consistent with findings from Goodman et al. (2015) that differences in observed graduation rates for students randomized between low- and moderately-selective US institutions track mean institution-level graduation rates. We add to a literature that uses both discontinuous admissions rules (Hoekstra 2009, Saavedra 2008, Ockert 2010, Hastings et al. 2013b, Zimmerman 2014, Kirkebøen et al. 2016) and observational estimation approaches (Brunner 2004, Brunner 2009, Brunner and Meller 2009, Altonji et al. 2012, Reyes et al. 2013) to study how intensive-margin choices about where and what to study impact earnings.[5]

This paper contributes to a developing literature on disclosure policies in higher education. Hurwitz and Smith (2016) and Huntington-Klein (2017) study the College Scorecard in the US, using SAT score-sending and web search data, respectively, to measure student responsiveness to policy implementation. Our combination of data on beliefs and choices with a randomized trial lets us examine enrollment outcomes directly and study students' choice processes in detail. Students choosing low-selectivity programs may not show up in measures of score-sending or web searches. Kerr et al. (2015) conduct a randomized trial in Finland and find little evidence that information affects enrollment choices. In the Finnish higher education market, college is effectively free to students, and institutions negotiate with regulators to determine the number of seats available in each program. The fringe of non-selective degree programs where we find disclosure to be most valuable does not exist. The combination of public subsidies with lightly-regulated private provision we observe in the Chilean market closely resembles other settings in which disclosure policies have been implemented (Neilson et al. 2016).

Looking beyond college choice, we contribute to a broader literature on how imperfect information and policies that seek to address it affect choices in settings where decisions are complicated and information scarce. These settings include primary and secondary education markets (Hastings and Weinstein 2008, Kapor et al. 2017), markets for healthcare and health insurance (Abaluck and Gruber 2011, Kling et al. 2012, Handel and Kolstad 2015), and markets for consumer financial products (Thaler and Benartzi 2004, Agarwal et al. 2014). Our finding of mean regression in earnings beliefs illustrates how mean-zero uncertainty can advantage low-quality products even if consumers behave optimally in other ways. That uncertainty rotates demand curves to advantage low-quality goods follows from a theoretical literature on advertising (Johnson and Myatt 2006), and is likely relevant in other markets as well. Our collaboration with the government to design and implement a nationwide policy is also an innovation here. Because policy effects depend on the details of design, the outcome of actual policies may

[5]We note that our findings contrast with Dale and Krueger (2002) and Dale and Krueger (2014).

differ from predictions based on studies that use researcher-defined samples and interventions (Duflo 2017).

The paper proceeds as follows. Section 2 describes educational institutions in Chile. Section 3 describes the administrative datasets we use to develop and evaluate the informational intervention. Section 4 describes the survey and intervention procedures. Section 5 presents our descriptive and experimental findings. Sections 6 and 7 present further discussion and robustness checks. Section 8 concludes.

2 Higher education in Chile

As in Latin America as a whole, college enrollment in Chile expanded in the 2000s, with much growth taking place at private higher education institutions (Ferreyra et al. 2017). By the 2010s, the Chilean higher education system resembled the US 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 (Organization for Economic Co-operation and Development 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.[6] Public, private non-profit, and private for-profit firms provide tertiary degrees in Chile. Vocational degree programs are run by private companies and can be for-profit or not-for-profit. These programs tend to be less selective and shorter in duration. Universities offering Bachelor's-equivalent degrees or higher may be public or private not-for-profit. In practice, however, portions 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.[7] The combination of public funding and private provision parallels the US market and suggests that firms may face similar incentives when making decisions about educational and marketing inputs.

Students in Chile apply to, take courses within, and graduate from institution-major combinations, which we refer to as degree programs. Entrance exam scores are the key determinant of admissions. In the early 2010s, roughly 200,000 students took the Prueba de Selecion Universitaria, or PSU, each year. 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. Older, more selective

[6]Source: Chilean statistics from Mineduc (2012), Tables Programas, Becas y Ayudas Estudiantiles A.4.4 and Matricula D.2.45. US statistics from United States Department of Education (2014b) Table 3.2-A. US statistics refer to the 2011-2012 school year.

[7]See La Tercera (2012) and Laureate International (2017). See Apollo Group (2013) for a discussion of Apollo's purchase of the Chilean university UNIACC. In 2009, the Apollo Group, which owns the university of Phoenix, settled in a False Claims lawsuit for its recruiting and advertising practices in the US. See Lederman (2009).

universities typically have a minimum cutoff score of 475 for admission, with higher cutoffs for more selective programs. 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 such programs. Even at the most selective degree programs, admissions outcomes typically do not depend on qualitative inputs like extracurricular activities or essays.[8] In what follows, we will measure selectivity using the average of Math and Language scores for enrolling students.

The two main student loan programs in Chile are the Crédito Aval del Estado (CAE) and the Fondo Solidario de Crédito Universitario (FSCU). 35.6% of low-SES postsecondary students used CAE loans in 2012. Students are eligible for CAE loans if they meet a set of PSU and GPA cutoffs, and if they have family income in the lowest four quintiles of the distribution. CAE loans may be used at any accredited institution. 7.1% of low-SES students received FSCU loans in 2012. FSCU loans may be used only at a set of older, selective institutions. Eligibility depends on test score and family income. Interest rates for both types of loans were 2% for students originating loans during the period we study over a standard repayment period of fifteen years.[9] We use these loan terms to compute measures of costs such as, e.g., the monthly payment associated with paying off debt from a given degree program.

In early- to mid-November, students apply for FSCU, CAE, and several federal grant programs using the Formulario Único de Acreditación Socioeconómica (FUAS), a unified financial aid form 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. 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. We nested the survey and the intervention in the FUAS application, so students described their preferences and received information at a time when they were thinking about college choice but had roughly two months remaining to adjust their enrollment plans before application deadlines.

3 Data

We use educational data from administrative records and labor market outcomes from tax returns to evaluate students' beliefs against observed benchmarks, to track enrollment choices in

[8]The most selective institutions admit students only on the basis of an index of grades and test scores. See HNZ for details.

[9]Ley 20027 - CAE; "Quality Assurance in Higher Education in Chile." OECD. November 2012. See Solis (2017) for further details on eligibility rules for CAE.

the years following the intervention, and to construct measures of earnings returns for all degree programs in Chile.

3.1 High school records

We use high school records to define our sampling frame and to describe students' socioeconomic backgrounds. Our high school graduation records cover the 1995-2012 graduating cohorts. These data include information on gender, parental education, scores on standardized tests administered to 10th graders (known as the SIMCE), high school identifiers, and high school characteristics such as school-level SES ratings. The SES rating categorizes schools from A (lowest) through E (highest) and is based on parental income. Since we do not observe parental income, we use high school SES as a proxy for student SES. The 41% of students in our sample universe of Fall 2012 applicants coming from the 45% of schools with A and B ratings are categorized as low-SES. The small size of Chilean high schools (the median graduating senior class size is 57) makes this classification more informative. 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. 70% of students enrolled in schools within the low-SES category come from the bottom income quintile of the population distribution, compared to 9% from the top two quintiles. 4% of the low-SES students in our treatment or control groups have a parent with a college degree.

3.2 College and college application records

We use college enrollment and graduation records to document educational backgrounds for past students and to track enrollment outcomes for applicants. Enrollment data are available for the years 2000 through 2014. These data follow students semester by semester, recording major- and institution-specific enrollment and graduation. We focus our experimental analysis on initial enrollment choices (made in 2013), and present additional findings on enrollment persistence using 2014 data in Section 7. Application records include entrance exam scores for all students by subject for the years 1980-2013. We use these as covariates when making earnings predictions, as described below.

3.3 Labor market data

We link the database of student records to tax returns from the 2005-2012 earnings years. Over 99% of individuals in our data have matches in the tax records. Tax returns include wage, contract, partnership, investment and retirement income. HNZ describe the tax data in detail, and provide an example of a tax form to illustrate the components used to calculate income.

3.4 Earnings predictions

We construct two measures of earnings outcomes by degree program. We developed the first, which we term Net Value, in collaboration with Mineduc for presentation to students as part of the informational intervention. At the time of the intervention, Mineduc preferred to focus on outcomes for graduates rather than enrollees, and on binned means rather than regression-adjusted predictions.[10] Mineduc also preferred to present earnings and cost outcomes discounted back to the year of labor market entry, which may depend on educational choice, as opposed to the first year following high school completion.

With these constraints in mind, we compute Net Value for degree j, NV_j, as

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

where $\hat{\mu}_{jt}$ 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 compute these values using graduation data for the years 2001 and 2005 through 2009, and data on non-enrollees from the 2006 and 2007 cohorts of high school completers. Data from other years were not available at the time of the intervention in Fall 2012. We compute mean earnings values directly for experience years one through five. For experience years six through fifteen, we compute predicted earnings by combining outcomes from earlier experience years with field-specific linear slope terms. Costs are based on current tuition levels and suggested degree lengths. We set the discount rate to 2%, the rate of interest on subsidized loans. We present information to students in terms of the monthly payment that would yield a present value equal to NV_j over a fifteen year period. We also present monthly equivalents of the earnings and cost components of NV_j. See Online Appendix Section B for more details.

These earnings measures condition on graduation and do not adjust for student characteristics. This means that selection into enrollment or graduation may drive cross-degree differences in earnings outcomes. To address this issue, we construct an alternate earnings measure that conditions on enrollment as opposed to graduation and uses observable student characteristics to correct for selection. We use this measure in conjunction with the Net Value measure to describe the relationship between beliefs and enrollment choices and measure the effects of the

[10] Mineduc eventually came to favor enrollment-based earnings measures, with the goal of incentivizing institutions to increase earnings for all enrolled students rather than through selective graduation (Beyer et al. 2015).

intervention.

To obtain earnings predictions, we estimate specifications of the form

$$\begin{aligned} y_{ijct} &= X_{ict}\beta_{s(j)} + W_{ijct}\delta_{m(j)s(j)} + v_{ijct} \\ v_{ijct} &= \mu_{jc} + \epsilon_{ijct} \end{aligned} \tag{2}$$

We use data on the set of student-year observations in which students who enrolled in college between 2000 and 2011 are between two and seven years removed from schooling completion through either graduation or dropout. y_{ijct} is labor market earnings for student i enrolling in institution-major combination (degree) j in entering cohort c at labor market experience year t. X_{ict} includes dummies for student socioeconomic status, gender, and whether a student took the entrance exam. It also includes linear controls for entrance exam score, years of labor market experience, interactions between labor market experience and student covariates, and tax year dummies. Coefficients on the X_{ict} are permitted to vary over broad area of study[11] and five selectivity tiers. $s(j)$ denotes the interaction between broad area and selectivity tier. We allow the effects of gender, SES, test taking and test scores, and labor market experience, denoted W_{ijct}, to vary with major $m(j)$ as well. We use a Mineduc classification system that divides programs into 178 possible majors. We decompose the residual v_{ijct} into a degree-cohort specific mean residual component μ_{jc} and an idiosyncratic error ϵ_{ijct}. The μ_{jc} captures degree-specific intercept shifts in earnings outcomes, which may vary by cohort. We estimate equation 2 using a two-step process. In the first step, we control for degree-specific fixed effects when estimating β_s and δ_{ms} to avoid confounding the effects of student characteristics with selection into degree programs. In the second step we use estimates of β_s and δ_{ms} to obtain mean residuals.

We use earnings predictions based on these regressions to describe choices for fall 2012 applicants. Our analysis focuses on predictions for the outcome year 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.[12] In Section 5, 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. We base our predictions at each degree j on the average of mean residuals μ_{jc} across entering cohorts $c = 2000$ through $c = 2005$. Our predictions thus correspond to the average over the age 26 cohorts in the five years leading up the

[11] We use CINE-UNESCO field classifications, which break programs into Business, Agriculture, Art and Architecture, Basic Sciences, Social Sciences, Law, Education, Humanities, Health, and Technology.

[12] 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.

intervention year of 2013. Mean residuals do not vary much across years, so choice of cohort does not affect our findings. See Online Appendix Section C for more details on estimation.

These earnings predictions improve on the Net Value measure by accounting for dropout and selection on observable characteristics. However, they will provide unreliable guides to future outcomes for current applicants if earnings differences across degrees are driven by selection on unobservable characteristics, or if degree effects are not stable over time. In Section 6, we benchmark our earnings predictions using quasi-random variation in degree assignment generated by admissions cutoff rules. We fail to reject the null hypothesis that earnings outcomes rise one-for-one with predicted values. Moreover, effects appear to be stable across cohorts.

4 Survey and experimental intervention

We worked with Mineduc to implement the survey and disclosure intervention. We began by pre-assigning students who graduated from high school in 2012 or who registered for the PSU admissions exam to treatment and control groups. We assigned treatment status at the high school level for current high school seniors, and at the individual level for past graduates. An advantage of school level randomization for current students is that estimated effects will better predict what would be observed in a universal policy rollout in the presence of school-level peer spillovers. We stratified assignment by high school type and quality for current graduates and by coarse PSU bins for past graduates to ensure that we would observe both treatment and control students across the distribution of student backgrounds.[13]

Upon submitting their FUAS applications, applicants received an email from Mineduc with the subject line "FUAS Confirmation Code." The email asked applicants to participate in a brief survey to help Mineduc make education policy decisions, and informed them that they would receive a confirmation code at the end of the survey. The email also told students that their survey responses would be anonymous, used only for research, and would not affect their FUAS applications. We track bounce-backs, opens, and click-throughs for each email address.

The survey link in the email directed applicants to a login page, where they were prompted to enter their identification number and email address. After logging in, applicants viewed an informed consent, which they could accept or reject. Conditional on acceptance, they began the survey. The survey asked six questions, each appearing on its own page. 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 to incorporate students' prior re-

[13]We stratify schools 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. We assign half of schools within randomization block to the treatment group. PSU registrants for the 2013 college entering class could use old PSU scores. This was a new policy in 2012. The PSU registration list thus consisted of those signed up to take the PSU, as well as those who had taken the PSU in prior years.

sponses. See Section B of the Online Appendix for survey materials with accompanying English translations. This section also provides screen shots of the survey and information treatment.

The first question asked students about their current educational status. Students indicated 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 three degree programs (institution-major combinations) to which they planned to apply, in preference order. Students made these choices using 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 previously entered choices would be. 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. The first part of the question asked students to estimate what their monthly salary would be once they started in a stable, full-time job after graduating from each of their elicited choices. The second part asked students to estimate what a typical graduate in each degree would earn. Students could 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 proceeded to a thank you page that displayed their confirmation code. They also received a thank you email. Treated students continued to a new page which displayed information on students' listed degree programs and two search prompts. A table at the top of the page presented the earnings and cost components of the Net Value measure in the first two columns, and the Net Value measure itself in the third column. Each row of the table was one of the degrees a student listed in question 2. Accompanying text explained to students that these values were derived from data on costs and earnings for past students.

A highlighted box below this table told students whether there were other institutions to which they would likely be admitted that offered the same major with a higher Net Value. The box also displayed the additional Net Value associated with a switch to the highest Net Value degree program fitting this description. To determine the contents of this box, 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. A second highlighted box indicated whether or not there were other degrees within the same broad field of study as their first-choice degree that offered higher Net Value, and displayed the expected Net Value gain from the within-field switch. To populate this box, the web application searched across degrees with similar entrance exam score distributions to the listed first choice.[14]

[14]The median gain in predicted earnings associated with the switch described in the first box was equal to 33% of

A link below the suggestion boxes invited treated students to view a searchable career database. Text at the top of this page explained the underlying data and gave subjects a place to enter a PSU score, degree level, and major. Students could fill in these fields and view tables displaying institutions offering the specified major and serving students with scores similar to the listed PSU value.[15] The table displayed the institution name, the major, and the same earnings, costs, and Net Value measures as the previous page. Results were sorted in descending order by Net Value. The search results also displayed a suggested alternative major to search for with higher Net Value in the same broad field of interest. For example, both nursing and nutrition are in the broad field of health but earnings for nursing are generally higher. Students could log back in at any time to compile and view up to ten comparative tables. This page also contained a thank you message and the confirmation code. Students were not required to search.

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 students opened the initial email. Of those, 73% agreed to the informed consent. 59% of students providing informed consent completed the survey. 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, and focus our analysis on this group. Recall that the intervention is identical for treatment and control groups through survey completion.

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 28 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 majors. 77.0% of respondents matriculate in some degree program. The mean value of predicted earnings at age 26 for respondents who enroll in college is $464,307 CLP each month, or USD $893.

Column 5 shows characteristics of treated respondents. There are no statistically or economically significant 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

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.

[15] 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.

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 searchers' stated first-choice enrollment plans is 4% higher than for non-searchers. See Tables A.4 and A.5 for further comparison of treatment and control groups.

5 Empirical Analysis

5.1 Earnings beliefs

Figures 1 and 2 and Table 2 describe students' responses to survey questions about expected earnings and compare them to values observed in tax data. We compare students' stated beliefs about earnings for a typical graduate once they have completed school and started work to earnings we observe in the tax data for students in the first two years following graduation. The horizontal axis in each graph is demeaned observed (log) earnings for past graduates, which we refer to as observed earnings. Figure 1 shows the fraction of students choosing the "I don't know" option at their most-preferred program. Each point is a binned mean within a ventile of the observed earnings distribution. Students who list the lowest-paying programs as their top choice are more likely to state that they do not know earnings outcomes for typical students; rates decline from 46% in the lowest decile to 28% in the highest. Overall, students indicate lack of knowledge for 50% of typical earnings beliefs and 39% of own earnings beliefs. We discuss the possible effects of non-reporting on our findings for students who do report beliefs below.

Panel A of Figure 2 shows how beliefs about typical earnings vary with observed earnings for past students. To facilitate comparison with demeaned true values, we subtract the observed earnings mean from beliefs. If beliefs tracked observed values on average, the belief mean would fall along a 45-degree line through the origin, which we plot for reference. We find that mean beliefs are linear across the distribution of observed earnings values, but do not track the 45-degree line. The slope is flatter, and the intercept shifted upwards. A linear regression of beliefs on true values yields a slope of 0.427 (0.007) and an intercept of 0.173 (0.004). The implication is that students with stated interest in low-earning degree programs believe earnings for a typical graduate are far higher than those observed for past graduates, while students with stated interest in high-earning programs think earnings for a typical graduate are somewhat lower than observed past values. For example, students expressing interest in bottom-decile degree programs believe the typical graduate will earn 87 log points (139%) more than the observed value for past graduates, while students expressing interest in top-decile programs have beliefs 34 log points (41%) below past values on average. Beliefs are on average correct for programs near the 75th percentile of the distribution.

These findings are consistent with a parsimonious model of belief formation in which applicants respond to uncertainty by shrinking degree-specific beliefs back towards a belief about mean earnings for college graduates in general (regardless of degree). Consider the following information structure. Applicants believe earnings values for typical graduates at degree j, $\tilde{Y}_j$, are drawn from a normal distribution $N(\bar{Y} + b, \sigma_Y^2)$. $\bar{Y}$ is the true mean across programs, b is a bias term reflecting applicants' optimism about degree programs on average, and σ_Y^2 is the variance of earnings outcomes. Each individual then receives a noisy signal $\tilde{Y}_{ij} = \tilde{Y}_j + b + e_{ij}$ about typical earnings outcomes at degree program j, with $e_{ij} \sim N(0, \sigma_e^2)$. Beliefs for overall and degree-specific mean earnings outcomes are biased up by b. Students' posterior expectation of earnings $\tilde{Y}_{ij}^e$ is given by

$$\tilde{Y}_{ij}^e = \alpha_Y \tilde{Y}_{ij} + (1 - \alpha_Y)(\bar{Y} + b) = b + \alpha_Y \tilde{Y}_j + (1 - \alpha_Y)(\bar{Y}) + r_{ij} \tag{3}$$

where $\alpha_Y = \frac{\sigma_e^{-2}}{\sigma_e^{-2} + \sigma_Y^{-2}}$ is a precision weighting and $r_{ij} = \alpha_Y e_{ij}$.

A qualitative implication of the model is that even mean-zero uncertainty leads to systematic overestimates for low-earning programs and underestimates for high-earning programs. Low-quality programs may benefit from students' belief errors even without marketing. These patterns are not consistent with predictions from alternate models of belief formation in which students have uniform biases across programs, or have upward-biased beliefs about heavily marketed low-earning programs but accurate beliefs about others.

This model also yields a useful quantitative interpretation of both the slope and intercept terms in the belief graphs. The slope is equal to α_Y, and our estimated value of 0.427 implies a noise-to-signal ratio of $\frac{\sigma_e^2}{\sigma_Y^2} = \frac{1-\alpha_Y}{\alpha_Y} = 1.34$; i.e., the variance of beliefs errors is 34% larger than the variance of the underlying distribution of observed values. This is similar to an independent estimate of the noise-to-signal ratio we obtain by computing the variance of belief errors and the variance of observed earnings values in the microdata. The variance of belief errors (defined as the difference between reported typical student beliefs and the observed value) is 0.565 and variance of observed outcomes is 0.353, for a ratio of 1.60. The belief bias term corresponds to the intercept on the graph. Our estimates indicate that students overestimate earnings by an average of 17 log points. This average across programs is second-order compared to the large overestimates of earnings outcomes for the lowest-earning programs.

We see similar patterns in reported beliefs about own earnings outcomes. Panel B of Figure 2 shows that own earnings beliefs closely track typical earnings beliefs across the observed earnings distribution. On average, students believe that their own earnings conditional on graduation will fall slightly below those for a typical graduate. The correlation between own- and typical-earnings beliefs is 0.78. The slope of own earnings beliefs in observed earnings values is 0.474, implying a noise-to-signal ratio of 1.112, and the intercept is 0.119.

One concern is that the linear relationship between beliefs and observed values might not persist or might have a different slope if non-reporters were forced to report beliefs. Several pieces of evidence suggest that this is unlikely. First, conditional on observed earnings at their listed preferences, students who do and do not report earnings beliefs are similar in terms of both predetermined characteristics and subsequent enrollment patterns. Second, to the extent differences in demographics and enrollment outcomes do exist, they suggest a linear relationship with an even flatter slope for non-reporters. See Appendix D for details. Third, we observe a linear relationship between beliefs and observed values for both own and typical graduate beliefs, even though rates of non-report are much lower for own earnings. A straightforward interpretation of our findings is that non-reporters tend to be more uncertain than reporters, and would drive the slope of beliefs in observed earnings downwards if included. Another concern is that measurement error in benchmark values could produce a similar pattern of results. We show in Appendix D that measurement error is limited in this setting and does not affect our findings.

Students who express preferences for the lowest-earning programs come from low-SES backgrounds and have low admissions test scores. Panel A of Figure 3 plots low-SES share and mean PSU scores by demeaned observed earnings. 56% of students planning to enroll in programs in the bottom earnings decile come from low-SES backgrounds, compared to 16% in the top decile. The average bottom-decile PSU score is 463, compared to 623 in the top decile. The median belief errors about earnings for past graduates are higher for low-SES students (15 log points) than for high-SES students (3 log points), and for students with below-median PSU scores (20 log points) than for students with above-median PSU scores (0 log points). As reported in Table 2, the relationship between beliefs and observed earnings is fairly similar for low- and high-SES students. The slope of beliefs in observed values is 0.42 for low-SES students and 0.43 for high-SES students. However, low-SES students are much more likely to prefer low-earning programs where this relationship leads to an overestimate.

Students' response to disclosure depends on their choice set, which is determined largely by admissions test score. Panel B of Figure 3 plots non-response rates and typical earnings beliefs by ventile of PSU score. The rate at which students claim not to know their typical earnings values declines with test score. Belief errors are largest at the bottom of the belief distribution. For example, students scoring at the fourth ventile of the PSU distribution overestimate their earnings by 35 log points (41%) over actual earnings. Degrees in this selectivity range serve mostly low-SES students. As shown in Panels C and D of Figure 3, rates of belief reporting and mean errors are fairly similar for low- and high-SES students after conditioning on test score.

5.2 Cost beliefs

Figure 4 displays the distribution of cost beliefs. 33% of students do not report cost beliefs. As shown in Panel A of Figure 4, the share of non-reports falls somewhat as costs rise, from 35% in the bottom ventile of the cost distribution to 25% in the top decile. As was the case with earnings beliefs, cost beliefs are linear in observed costs. However, as shown in Panel B of Figure 4, cost beliefs track true values much more closely. The estimated slope of cost beliefs in observed costs is 0.879 (0.009), and the intercept is -0.083 (0.004). Students choosing the bottom-decile degree programs overestimate costs by an average of 3% while students choosing the top-decile programs underestimate costs by 15%. While cost underestimates at high-cost programs are arguably substantial, they are an order of magnitude smaller than earnings overestimates at the lowest-earning programs. The noise-to-signal ratio corresponding to the slope term in costs is 0.138, one tenth the value for earnings. As with earnings, the relationship between cost beliefs and observed costs is similar for low- and high-SES students.

A concern in interpreting subjective beliefs data is that errors may result from inattention to the survey, not lack of knowledge about actual outcomes. That students report accurate beliefs about costs helps mitigate concerns that belief errors in earnings are the result of inattention. Other pieces of evidence support this as well. For example, ordered logit estimates show that, within students' preference lists, earnings beliefs predict relative rank, but, conditional on earnings beliefs, true earnings outcomes for past students do not predict relative rank. We would expect the opposite if students knew true outcomes but provided random responses to questions about own beliefs. We present these results in Appendix Table A.6. Students' elicited preferences are also strong predictors of higher-stakes enrollment choices, as we discuss below.

5.3 Beliefs and enrollment outcomes

Students' elicited preferences are strong predictors of enrollment choices. 45% of matriculating students enroll in one of their listed preferences. 27% enroll in their first choice. This is true even though we elicit preferences before application decisions are made, and suggests that we are accurately recovering preference information. For students who enroll in degree programs for which we elicited earnings beliefs, the gap between beliefs and values for past students follows a similar pattern to what we observe in the analysis of preferences. Panel A of Figure 5 shows that the slope of beliefs in observed earnings conditional on enrollment is 0.41, and the intercept is 0.15. These are very similar to the values of 0.43 and 0.17 that we observe in analysis of beliefs overall. Students who enroll in bottom ventile programs believe the typical graduate earns 169% more than observed values for past graduates. Panel B shows that costs also follow a similar pattern, with a slope of 0.86 and an intercept of -0.076.

Panel C of Figure 5 shows that students with larger positive belief errors enroll in programs

with lower earnings and rates of on-time loan repayment for past students across the PSU score distribution. The graph splits students into two groups. Students with above the median positive belief error of 38 log points are in the large error group, while students with an error below the median positive belief error are in the small error group. Students in the large error group enroll in programs where their predicted earnings (as measured using predictions from equation 2) are 22 log points lower than students in the small error group.

We take the gap in observed outcomes between students with large and small overestimates as a comparison point for our information intervention treatment effect estimates. The intuition is that this represents a reasonable upper bound on what an informational intervention could accomplish. It is inclusive of the causal effect of belief errors as well as any correlation between belief errors and other factors that push students towards low-earning programs. For example, applicants who place low value on earnings may have large belief errors because it is not worth it for them to seek out accurate earnings information (Hastings et al. 2016).

5.4 Experimental estimates of information treatment

Table 3 shows the impact of treatment on Net Value, predicted earnings, and cost outcomes at the degree programs where students choose to enroll. We show results separately for the full sample of students, and for subsamples by SES and PSU score. We estimate specifications of the form

$$Y_i^{outcome} = \beta_0 + \beta_1 T_i + \beta_2 Y_i^0 + \gamma_{g(i)} + e_i \tag{4}$$

where $Y_i^{outcome}$ is a characteristic of a student's chosen degree program, such as Net Value, costs, or predicted earnings. T_i is an indicator for treatment, and $\gamma_{g(i)}$ is a randomization block fixed effect. Y_i^0 is the characteristic of the outcome variable of interest at student i's stated first choice degree program. These controls reduce standard errors but do not substantively alter point estimates. Tables A.7 presents 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 coefficient of interest is β_1, the effect of treatment on the characteristic of the chosen program.

The first panel shows impacts of treatment on the decision to matriculate to any degree program. The impact is close to zero and statistically insignificant across all subsamples. The second panel shows the impact of treatment on Net Value and the costs and earnings components of the Net Value measure in the applicant population. For the 23% of the sample who did not matriculate to any degree, these three values are by definition zero. The estimated effects, which average the zero effect for non-matriculating students with intensive margin effects for matriculating students, do not differ statistically from zero for Net Value or its earnings or cost components.

Because the impact of treatment on matriculation is zero, we can estimate the inframarginal impact of treatment on the earnings and cost characteristics conditional on enrollment. 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. Treatment raises Net Value by $10,029 CLP, or 1.7% of the control group mean. This effect is driven by larger gains for low-SES students, and particularly low-SES students with low PSU scores. 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 control 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. For low-SES students with low PSU scores, treatment raises Net Value by $18,430 CLP, equal to 5.0% of the control mean for this group, 6.3% of the average gain, and 42% of the average monthly debt payment. Across all samples, effects are driven by increases in earnings, not decreases in costs.

The fourth panel of Table 3 shows how treatment affects earnings at age 26 conditional on enrollment as opposed to graduation. This earnings measure, estimated using equation 2, reflects changes in degree "value added" conditional on gender, student SES, and test score. We find positive and statistically significant treatment effects, again driven by gains for low-SES students and students with low-PSU scores. The gain for low-SES students of $11,759 CLP is equal to 21.7% of the gap in predicted earnings between students with large and small belief errors for students in this group. The gain of $11,337 for low-PSU, low-SES students is equal to 25.6% of the gap for these students.

The effect of treatment is to push applicants away from very low-earning degree programs and towards a range of higher-earning ones. Figure 6 plots the distribution of Net Value at the matriculating degree by treatment status. The upper panel splits students by SES background, and the lower panel splits students by above- and below-median admissions test scores. We observe a shift in mass from below the median of the control group distribution to roughly the 50th through 95th percentiles, primarily among low-SES and low-PSU students. Treatment reduces the share of treated students choosing degrees with Net Values within the bottom tercile of the distribution among control students by 3.3% overall, by 4.6% for low-SES students, and by 3.9% for low-PSU students.

The bottom panel of Table 3 measures the impact of treatment on measures of long-run earnings gains. 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 C for a full description of the calculation.) We find that treatment raises the PDV of post-schooling earnings net of direct costs through age 30 by just under one million Chilean 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.[16] 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%. Reweighting the respondent sample to match the universe of applicants on observable characteristics (SES background, gender, age, and test score) indicates that applying the treatment to this larger group would yield gains of US $217 million through age 30. This second calculation relies on the assumption that, conditional on observables, survey takers and non-takers would respond similarly to treatment, which we cannot verify. However, because the disclosure intervention is scalable and inexpensive to implement, the rate of return on treatment is likely high.

5.5 Information treatment and program-specific demand

Our experimental and descriptive findings raise two sets of questions. The first is why the effects of treatment account for only a minority of the gap in outcomes between students with large and small belief errors. The second is how disclosure alters demand for different kinds of degree programs, and therefore supplier incentives. This is critical for understanding how the effects of disclosure evolve over the long run (Hastings et al. 2015). To address these questions, we use a discrete choice framework to explore student preferences and study how treatment shifts demand across degree programs.

We estimate logit specifications based on the student choice problem

$$\max_{j \in J_i} u_{ij} = X_j\beta_1 + X_{ij}\beta_2 + \pi_0^Y \tilde{Y}_j + \pi_1^Y \tilde{Y}_j T_i + \pi_0^C \tilde{C}_j + \pi_1^C \tilde{C}_j T_i + \epsilon_{ij} \tag{5}$$

Each student chooses the utility-maximizing degree program j within their choice set J_i, with J_i determined by students' scores on the admissions exam. Utility u_{ij} for student i at degree j depends on degree characteristics X_j, the interaction between i's tastes and j's attributes X_{ij}, and iid logit shock ϵ_{ij}. Treatment T_i affects choice by changing the weights students place on observed (log) earnings and cost values $\tilde{Y}_j$ and $\tilde{C}_j$.

Treatment operates by causing students to upweight the information displayed in the intervention. The beliefs model from Section 5.1 provides a plausible microfoundation. Assume that students make degree choices based on expectations about their own earnings and costs, using utility weights τ_Y and τ_C respectively. Setting $\tau_C = 0$ for purposes of exposition, we may then write the choice problem as

$$\max_{j \in J_i} u_{ij} = X_j\beta_1 + X_{ij}\beta_2 + \tau_Y Y_{ij}^e + \tilde{\epsilon}_{ij}$$

Treatment changes students' earnings expectations Y_{ij}^e by raising the precision of earnings and

[16] See 7.1 for a discussion of treatment and degree length.

cost signals α_Y, with $\alpha_y(t)$ being precision of earnings beliefs for $T_i = t$. Treatment may also alter intercept terms b, but these do not affect intensive-margin degree choice. We can project own earnings beliefs Y^e_{ij} onto beliefs about earnings for past graduates, $\tilde{Y}^e_{ij}$, so that $Y^e_{ij} = \rho_Y \tilde{Y}^e_{ij} + \eta_{ij} = \rho_Y \alpha_Y(t) \tilde{Y}_j + a_0 + a_{ij}$, where $a_0 = \rho_Y(b + (1 - \alpha_y(t))\bar{Y})$ is constant across degrees and $a_{ij} = \rho_Y r_{ij} + \eta_{ij}$ is an idiosyncratic degree-specific shock. ρ_Y captures the predictive power of information about past earnings for formulation of own earnings expectations. If we impose the distributional assumption that $\tilde{\epsilon}_{ij} + a_{ij} = \epsilon_{ij}$, we can write the utility weights in equation 5 as $\pi^Y_0 = \rho_Y \tau_Y \alpha_Y(0)$ and $\pi^Y_1 = \rho_Y \tau_Y (\alpha_Y(1) - \alpha_Y(0))$. The coefficients reflect belief precision and the effect of treatment on belief precision, scaled by the weight of earnings beliefs in utility and the relevance of past outcomes for own beliefs. Further, the fraction change in coefficients $(\pi^Y_1 - \pi^Y_0)/\pi^Y_0 = (\alpha_Y(1) - \alpha_Y(0))/\alpha_Y(0)$ in the treated relative to untreated sample gives the fraction increase in belief precision relative to baseline.

Alternate microfoundations for this empirical specification are possible. For example, treatment may cause students to place more utility weight on earnings and cost outcomes even if they do not update beliefs about these outcomes. It is also possible that this empirical specification misses some channels through which treatment systematically affects choice. We consider one additional channel below, using a specification that allows treatment to effect choice by increasing the salience of the specific degree programs recommended by the web application. Close parallels between our experimental results and our model estimates suggest that our formulation provides a reasonable accounting of treatment effects.

When estimating the model, we exploit our detailed survey data to capture variation in students' preferences across programs. The X_j consist of dummies for narrowly defined field of study (178 categories) and institution (141 categories), as well as mean test scores and high-SES share for admitted students. To allow the utility of degree characteristics to vary across students, the X_{ij} interact degree characteristics with student preferences as measured in our survey response data.[17] The X_{ij} consist of indicator variables for whether j's attributes match those of degrees that i ranked Nth on the pre-survey, for N=1, 2, or 3. The attributes we consider are narrow major, broad area (10 categories), and institution. To measure the intensity of listed preferences, we interact these indicators with a dummy equal to one if i reported certainty in their preference listing. We capture preferences over geography by including indicators equal to one if an institution is located in i's home region or in the same region as one of i's listed preferences. To capture substitution patterns across selectivity levels, we include controls for the absolute difference between the high-SES share and mean admissions score for matriculating students at i's listed first choice versus those at degree j.

We report estimated coefficients from this exercise in Appendix Table A.8, and summarize our

[17] Mixed logit demand models typically draw from distributions of unobserved preferences for characteristics when measures of preferences are not available. In contrast, we measure preferences for particular degree characteristics directly and allow stated preferences to shift utilities.

estimates in Table 4. Panel A of Table 4 compares the utility weights that treated and untreated students, respectively, place on earnings and costs. To obtain these values we compute predicted utilities $\hat{u}_{ij}$ and regress the values on log earnings and log costs. We estimate separate regressions in the treatment and control groups. This allows our estimated earnings and cost utility weights to incorporate the role that earnings and costs play in the formation of stated student preferences, rather than conditional on stated preferences. The estimate for the untreated group reflects the correlation between utility and earnings at a degree program, including any correlation between earnings and other degree attributes that affect utility. The difference between estimates for treated and untreated students shows how disclosure raises the correlation between earnings for past students and utility at a degree program.

Overall, treatment raises the utility weight students place on earnings by 32%, while raising (in absolute value) the weight students place on costs by 4%. This is consistent with our experimental finding that disclosure raises earnings but does not reduce cost at the programs where students enroll. Treatment raises the utility weight placed on earnings by 72% for low-SES students compared to 12% for high-SES students. The utility weights rise by 199% for low-scoring students, but from a baseline close to zero. These findings are consistent with the heterogeneous effects observed in the experimental setting. It is worth noting that, for high-PSU students, costs are positively correlated with utility on average. This is likely due to correlations between desirable degree attributes and some combination of high production costs and markups over production costs in this subsample.

In the full sample, our baseline estimate of $\alpha_Y(0)$ from the pre-treatment beliefs data is 0.427, for a noise-to-signal ratio of 1.34. A 32% increase from $\alpha_Y(0)$ to $\alpha_Y(1)$ corresponds to a 43% decrease in the noise-to-signal ratio from 1.34 to 0.78. The reduction in noise-to-signal ratio for low-SES students is 1.40 to .40 (71%), and for high-SES students is 1.35 to 1.10 (25%). Our findings are what would be expected from substantial increases in belief precision, but suggest that uncertainty remains even following treatment.

Even though treatment increases precision in students' beliefs we see modest enrollment effects because students place high utility weights on degree attributes other than earnings. To illustrate this, we calculate the elasticity of enrollment at students' stated first choice degree program with respect to earnings under counterfactual assumptions on student preferences. We report results in Panel B of Table 4.

The "Baseline" row reports observed choice elasticities. In the pooled sample, treatment raises the earnings elasticity from 0.143 to 0.182, with larger effects for low-SES students. The subsequent rows eliminate, one at a time, student preferences over geography, institution, and narrow major. We obtain these preferences by zeroing out the portion of utility attributable to, respectively, an institution's location, a student's stated preference over institutions, and a student's stated preference over narrow majors. (When eliminating preferences over narrow majors,

we allow students to maintain stated preferences over broad fields of study). These variables are demeaned so that average utility over institutions and majors does not change in the counterfactual setting. Eliminating geographic preferences raises earnings elasticity at the first choice degree for treated students by 16%, to 0.210. Eliminating preferences over institutions (majors) raises the same elasticity by 62% (77%), to 0.294 (0.321). These findings suggest that strong preferences for particular majors and institutions reduce students' willingness to substitute towards higher-earning programs. They also suggest that survey data successfully capture preference heterogeneity.

Demand shifts under hypothetical scaled treatment

With model estimates in hand, we simulate the effects of a population-scaled informational intervention. We use the SES-specific logit estimates to simulate enrollment probabilities for each survey respondent under the assumptions that either a) all applicants received treatment, or b) no applicants receive treatment. We then assign each applicant a sampling weight based on their high school type, gender, age, and admissions test score so that the weighted sample matches the population of loan applicants on these characteristics. Using the weighted demand estimates, we compute the expected change in enrollment in each degree program between the all-treated and none-treated scenarios.

The upper panel of Figure 7 shows mean percent change in enrollment by deciles of degree-level mean earnings. Treatment shifts students away from the lowest-earning degree programs and towards programs with earnings in the top 60% of the distribution. Enrollment declines by 4.6% in the lowest-earning tenth of programs, and grows by just under 2% in the top three deciles. Shifts away from low-earning programs are larger for students from low-SES backgrounds, reaching 6.4% in the bottom decile. The lower panel of Figure 7 plots the same outcome by deciles of the cost distribution. There are no systematic changes in enrollment across the distribution of program costs. Modest shifts towards low-cost programs for low-SES students are balanced by small shifts away from those programs for high-SES students. These simulation results echo the reduced-form evidence from Figure 6. Treatment reduces demand for the degree programs with the worst labor market outcomes, particularly among low-SES students.

Degree programs with biggest declines are low-selectivity programs run by for-profit providers. The majors where enrollment declines are the largest include sound engineering, cooking, and tourism, as well as special education. These are the types of degree programs that drive high loan default rates in our setting and also in the US (see e.g. Looney and Yannelis 2015). Appendix Table A.9 reports enrollment changes by major and institution-major combination for a selection of degree programs where demand declines.

Capacity constraints

The analysis thus far assumes that universal treatment does not shift the set of degree programs to which students have access. If treatment caused students to chase a small number of spots in capacity-constrained programs, this would affect our analysis of demand effects in the short run. To understand whether capacity constraints bind, we compare simulated changes in enrollment to slack capacity using data on degree-specific vacancies. These data are reported each year to a centralized accreditation authority.[18] Figure 8 reports results. The left panel presents the mean of scores for admitted students by decile of percentage change in demand for the program. The distribution of changes in enrollment is relatively tight. The mean enrollment change in the bottom (top) decile of the change distribution is -5% (5%). The upside-down V shape in the left panel indicates that the largest changes in enrollment (both positive and negative) take place at less-selective degrees, while changes at more-selective programs tend to be smaller. Treatment shifts students between non-selective programs. These findings make sense because treatment effects are largest among low-SES and low-PSU students.

The right panel of Figure 8 presents mean admissions slack (in percentage terms) by decile of enrollment change. The rough V shape of this graph indicates that the degrees where enrollment changes are the largest also have the most unfilled capacity. Degrees in the top decile of the change distribution have an average enrollment increase of 5%, but an average of 14% excess enrollment capacity. Informational treatment does not result in students chasing a small number of spots at capacity-constrained programs.

6 Validating earnings predictions

The analysis so far examines how disclosure affects students' enrollment choices. This is appropriate for understanding effects on the higher education market. However, to preview the long-run effects on student outcomes, it is also helpful to ask whether the earnings predictions we use to evaluate choices capture the causal effects of attending different degree programs on earnings. This is challenging because meaningful measures of earnings outcomes are not available until students reach their mid- to late-20s. Most students starting college in 2013 will turn 26 in 2020.

What we can do is test whether earnings outcomes for past students assigned quasi-randomly to different degree programs rise one-to-one with earnings predictions. To do this, we exploit the fact that admissions outcomes at most selective degree programs in Chile are determined only by indices of grades and test scores. As described in HNZ, this policy generates sharp cutoffs

[18] One third of degree programs report enrolling more students than their stated number of open spots. We compute percentage excess capacity for each program as the maximum of 0 and the difference between the logs of available spots and observed enrollment. This underestimates true slack because a) there are implicitly more spots available at some undersubscribed programs than there are listed vacancies, and b) it is possible that the true capacity at (nominally) oversubscribed programs is above observed enrollment.

in admissions outcomes around score thresholds that are unknown at the time of application. Students above the threshold attend their chosen program, while students below the threshold attend a mix of next-best programs. Earnings changes across the admissions threshold reflect the causal effects of admission to the target program relative to the below-threshold mix.

We compute regression discontinuity estimates at each selective degree program using observed earnings outcomes, and compare the results to alternate regression discontinuity estimates obtained using predicted earnings values from equation 2. If observed earnings changes rise one-for-one with predicted changes, we interpret this evidence that our predicted values capture causal effects. We formalize this intuition in Section E of the Online Appendix.

We estimate regressions of the form

$$\hat{\delta}_j^{obs} = \lambda_0 + \lambda_1 \hat{\delta}_j^{pred} + e_j \tag{6}$$

where $\hat{\delta}_j^{obs}$ is the regression discontinuity estimate for degree j obtained using earnings outcomes and $\hat{\delta}_j^{pred}$ is the estimate obtained using predicted outcomes. To account for measurement error in the independent variable, we shrink estimates of the $\hat{\delta}_j^{pred}$ towards the sample mean based on their sampling variance. We account for measurement error in the dependent variable by weighting using the inverse of the sampling variance for $\hat{\delta}_j^{obs}$. If $\hat{\delta}_j^{pred}$ is an unbiased predictor of $\hat{\delta}_j^{obs}$, we expect λ_1 to equal one and λ_0 to equal zero. When estimating this regression, we by necessity focus on selective programs (i.e., programs that reject some students), and programs for which we can make earnings predictions. This eliminates discontinued older programs which lack the recent enrollment data necessary to predict outcomes and non-selective programs. Because we drop the non-selective programs in which the least-informed students typically enroll from our sample, we expect to overstate the role of selection into degree programs on the basis of program-specific comparative advantage, which depends on knowledge of fit.

We estimate these specifications using two sets of application cohorts. The first spans 2000 through 2005. These are the application cohorts used to make earnings predictions. Results from this cohort test whether regression predictions provide reasonable approximations of causal effects. We use estimated pre-2000 mean residuals μ_{jc} when computing predicted values. The second is 1982 through 1993. 1982 is the earliest application cohort we observe. We choose the 1993 end date because students applying in 1993 have in almost all cases completed schooling by 2000 and are not included in the enrollment data. Findings for the earlier cohort speak to the stability of degree effects over time and experience.

Table 5 and Figure 9 present results. Each point on the graphs represents the mean of the observed earnings gain within bins defined by decile of the distribution of $\hat{\delta}_j^{pred}$. The binned means track the linear fit across the distribution of predicted values. In both graphs, observed values are linear in predicted values, have slope close to one, and pass through the origin. In

the 2000-2005 application cohort, our estimate of λ_1 is 0.716 with a 95% CI of (0.33, 1.10). Our estimate of λ_0 is 28.9, with a CI of (-130, 187). In the 1982-1993 cohorts, our estimate of λ_1 is 0.778, with a 95% CI of (0.42, 1.14) and our estimate of λ_0 is 150, with a CI of (-96, 396). We cannot reject the hypotheses that $\lambda_1 = 1$ or that $\lambda_0 = 0$ at conventional levels in either specification.

We cannot reject the hypothesis that our measures of predicted earnings succeed in capturing the causal effects of enrollment in different degree programs, and that effects are relatively stable across cohorts. This is consistent with recent research showing that value added measures of teacher and school effects provide reasonable estimates of causal effects (Kane and Staiger 2008, Chetty et al. 2014a, Chetty et al. 2014b, Deming 2014). These papers argue that selection of teachers based on student unobservables may be negligible in practice.

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. This is consistent with our survey evidence that students do not differentiate themselves from typical graduates and with our discrete choice analysis suggesting that students weight other preferences more heavily than earnings when making enrollment choices. Our findings also suggest that the regression-adjusted earnings predictions used in university accountability systems in Chile and elsewhere (Beyer et al. 2015, Scott-Clayton and Minaya 2016) may be reasonably accurate on average.

That predictions are unbiased for past students does not mean they will be unbiased for current students. Though degree effects seem fairly stable over time, skill prices may change, either exogenously or in response to the disclosure policy. It is also possible that earnings effects are different for students whose choices are affected by disclosure. We discuss these issues in the next section.

7 Discussion and robustness

7.1 Predictions over the medium- and long-run

Mismatch and academic outcomes

Outcomes for past students may not reflect outcomes for students whose choices are shifted by the disclosure policy. One way to get a sense of skill match without waiting for treated students to complete their education and enter the labor market is by looking at the effects of treatment on academic outcomes. We use data on college enrollment in 2014 (the second academic year following the informational treatment) to see whether treatment affects the rate at which students drop out or switch degree programs. Our findings, reported in Appendix Table A.10, show that treated and untreated students choose degree programs with near-identical graduation rates and mean enrollment durations for past cohorts. This means that if treatment

does not affect academic fit at the chosen program, it should not affect graduation or persistence outcomes. Consistent with this observation, we find that treatment has effects on dropout and degree switching that are close to zero and statistically insignificant. Treated students are 0.2 percentage points less likely to matriculate in 2014 than untreated students, relative to a mean rate of 87.3%. Conditional on matriculating in 2013, they are 0.3 percentage points more likely to drop out by 2014 (mean rate of 8.4%), and 0.2 percentage points less likely to switch degree programs (mean rate of 15.5%). Effects are similar for students from low- and high-SES backgrounds. Changes in degree choice induced by informational treatment do not appear to come at the cost of skill match.

Skill prices

Informational interventions could affect future skill prices by raising or lowering the supply of skills of different types. To understand how supply changes across broad skill types, we aggregate enrollment changes under the full treatment counterfactual and no treatment counterfactuals across broad field of study. Consistent with the strong field- and major-specific preferences we observe in our discrete choice model, we find that treatment does not substantially shift the distribution of enrollment by field. The field with the largest enrollment increase is Technology, where enrollment rises by 1.2%. The fields with the largest declines are Education and Humanities, where enrollment falls by 1.4%. and 1.2%, respectively. 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. Mean monthly earnings for students choosing technology degrees in the no treatment counterfactual are 120% higher than earnings for students choosing humanities degree programs despite the fact students enrolling in these fields have, on average, similar test scores. We report these findings in Appendix Table A.9.

7.2 Treatment design

Psychological underpinnings of treatment effects

Our treatment had two parts: it highlighted specific degree programs that offered high returns on average for past students, and it provided information about earnings and cost outcomes for programs of the students choice. We test whether effects derive from the recommendation or from the more generalized information treatment. To do so, we augment our baseline choice model with indicator variable equal to one for degree programs in the career that we recommended to the student (or, for control-group students, would have recommended had they been treated) and an interaction between this indicator and treatment. If treatment effects shift away from the interaction with degree-specific earnings and towards the interaction with the recommendation the student received, this would suggest that our recommendation was a driver of

student choice, and that, for example, policy makers could simply advertise the names of higher return degrees rather than work to update students earnings information and beliefs through disclosure.

Our findings, reported in Appendix Table A.8, suggest that adding the recommendation effects does not reduce effects of treatment on students preference for degree mean earnings. The treatment appears to operate by raising the utility weights students place on earnings outcomes more broadly. The coefficient on the interaction between treatment and the recommendation indicator is positive but not statistically significant. The advertising component of the intervention is not the main driver of the effects we see.

8 Conclusion

This paper uses a survey and a randomized evaluation of a scaled disclosure policy to explore why some college applicants enroll in degree programs where past students have had very low earnings. The scale of our survey and intervention and a link between our survey records and population tax data allow us to describe beliefs and belief errors across the distribution of degree programs and student characteristics, and show how enrollment decisions change when students have access to accurate information about earnings and costs.

Our findings paint a mixed picture of the efficacy of disclosure interventions in the higher education market. On one hand, large belief errors about past labor market outcomes indicate scope for such interventions. We show that students who plan to enroll in the lowest-earning degree programs think the typical graduate will earn 139% more than the average value for past graduates. Overall, the distribution of earnings beliefs is consistent with a model in which students shrink noisy signals about degree-specific earnings at their preferred programs towards a mean belief about outcomes for graduates. In contrast, mean beliefs about costs rise one-for-one with observed values. Belief errors are largest for low-achieving students and students from low-SES backgrounds.

On the other hand, an informational intervention implemented by the Chilean Ministry of Education as part of the financial aid application process has relatively modest effects. Treatment reduces the fraction of low-SES students choosing degree programs in the bottom tercile of the earnings distribution by 4.6%, and raises their mean predicted earnings by 21% of the gap the between students with large and smaller earnings overestimates. Drops in student demand are largest for a set of low-earning, non-selective programs that are often run by for-profit providers. This suggests that short-run effects of the disclosure intervention are likely a lower bound on long-run effects, which may grow over time as institutions respond to changes in demand. However, the effects of the intervention are mitigated by students unwillingness to substitute across narrowly-defined majors and institutions.

We conclude by noting that, as is typical in policy design, details matter (Duflo 2017). Our findings have several implications for the fine-grained design of disclosure policies. The first is that reaching out to students is important. Our intervention included direct, personalized outreach from the central education authority. Even with this recruitment strategy, we observe negative selection into treatment: the low-scoring, low-SES students who benefit most from disclosure are less likely than other students to complete our survey and reach the disclosure stage of randomization. A pilot survey we conducted in the previous year that included outreach but not from the central authority yielded much lower takeup than the 30% we observe here (Hastings et al. 2016). Our results echo findings across a range of social services on the difficulty of program takeup for those most in need (Currie 2006, Choi et al. 2011, Amior et al. 2012). Higher education disclosure policies in most countries do not include an outreach component: policymakers simply post a search tool online (Neilson et al. 2016). This may limit their effects.

The second and third implications concern the type of information and the timing of the interventions. Recent consumer finance research suggests that access to information has its greatest effect at or near the time of choice and when the information reflects directly on the choice at hand (Hastings et al. 2013a). Students in Chile choose institution and major jointly at the time of application, so our search tool disaggregated data at this level. In contrast, disclosure policies in many countries disaggregate only by institution, or by institution and occupation (Neilson et al. 2016). This may make mapping from the disclosed values to potential outcomes more challenging for students. The optimal timing of a single intervention may be close to the time of choice. However, our findings indicate that students have strong preferences over institutions by this time. If students form preferences over college options in the months and years leading up to application, informational interventions during this period may complement the time-of-choice intervention.

References

Abaluck, Jason and Jonathan Gruber, "Choice Inconsistencies among the Elderly: Evidence from Plan Choice in the Medicare Part D Program," *American Economic Review*, June 2011, *101* (4), 1180–1210.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, and Douglas D. Evanoff, "The Effectiveness of Mandatory Mortgage Counseling: Can One Dissuade Borrowers from Choosing Risky Mortgages?," *NBER Working Paper 19920*, 2014.

Altonji, Joseph G., Erica Blom, and Costas Meghir, "Heterogeneity in Human Capital Investments: High School Curriculum, College Major, and Careers," *Annual Review of Economics*, 2012, *4* (1), 185–223.

____, Peter Arcidiacono, and Arnaud Maurel, "The Analysis of Field Choice in College and Graduate School: Determinants and Wage Effects," *Handbook of the Economics of Education*, 2016, *5*, 305 – 396.

Amior, Michael, Pedro Carnerio, Emanuela Galasso, and Rita Ginja, "Overcoming Barriers to the Take-Up of Social Subsidies," *World Bank Conference Paper*, 2012.

Apollo Group, "Global Fact Sheet," 2013.

Arcidiacono, Peter V, Joseph Hotz, and Songman Kang, "Modeling college major choices using elicited measures of expectations and counterfactuals," *Journal of Econometrics*, 2012, *166* (1), 3–16.

Avery, Christopher and Thomas J. Kane, "Student Perceptions of College Opportunities. The Boston COACH Program," in "College Choices: The Economics of Where to Go, When to Go, and How to Pay For It" NBER Chapters, National Bureau of Economic Research, Inc, October 2004, pp. 355–394.

Bettinger, Eric P, Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu, "The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment," *The Quarterly Journal of Economics*, 2012, *127* (3), 1205–1242.

Beyer, Harald, Justine Hastings, Christopher Neilson, and Seth Zimmerman, "Connecting Student Loans to Labor Market Outcomes: Policy Lessons from Chile," *American Economic Review: Papers & Proceedings*, 2015, *105(5)*, 508–513.

Bleemer, Zachary and Basit Zafar, "Intended College Attendance: Evidence from an Experiment on College Returns and Cost," IZA Discussion Papers 9445, Institute for the Study of Labor (IZA) Oct 2015.

Brunner, José Joaquín, *Oferta y Demanda de Profesionales y Técnicos en Chile. El Rol de le Información Pública*, Universidad de Chile, Facultad de Ciencias Físicas y Matemáticas, Departamento de Ingeniería Industrial, 2004.

____ , *Educación superior en Chile: Instituciones, Mercados y Polticas Gubernamentales (1967-2007)*, Universidad Diego Portales, 2009.

____ **and Patricio Meller**, *Educación Técnico-Profesional y Mercado Laboral en Chile: Un Reader*, Gobierno de Chile, Minesterio de Educacion y Universidad Diego Portales y Ingeneria Industrial Universidad de Chile, 2009.

Chetty, Raj, John Friedman, and Jonah Rockoff, "Measuring the Impact of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood," *American Economic Review*, 2014, *104(9)*, 2633–2679.

____ **,** ____ **, and** ____ , "Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates," *American Economic Review*, 2014, *104(9)*, 2593–2632.

Chetty, Raj, John N. Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan, "Mobility Report Cards: The Role of Colleges in Intergenerational Mobility," *Working Paper*, January 2017.

Choi, James, David Laibson, and Brigitte Madrian, "$100 Bills on the Sidewalk: Suboptimal Investment in 401(k) Plans," *The Review of Economics and Statistics*, August 2011, *93* (3), 748–763.

Currie, Janet, "The Take-up of Social Benefits," in "Public Policy and the Income Distribution.," Russell Sage Foundation, 2006, chapter 3, pp. 80–148.

Dale, Stacy Berg and Alan B. Krueger, "Estimating the Payoff to Attending a More Selective College: An Application of Selection on Observables and Unobservables," *Quarterly Journal of Economics*, 2002, *117(4)*, 1491–1527.

____ **and** ____ , "Estimating the Effects of College Characteristics over the Career Using Administrative Earnings Data," *Journal of Human Resources*, 2014, *49(2)*, 323–358.

Deming, David J, "Using School Choice Lotteries to Test Measures of School Effectiveness," *The American Economic Review*, 2014, *104* (5), 406–411.

Dinkelman, Taryn and Claudia Martinez, "Investing in Schooling in Chile: The Role of Information about Financial Aid for Higher Education," *The Review of Economics and Statistics*, 2014, *96* (2), 244–257.

Douglas-Gabriel, Danielle, "SEC settles fraud charges against defunct for-profit college company ITT," 2017. Accessed 3 July 2017.

Duflo, Esther, "The Economist as Plumber," Working Paper 23213, National Bureau of Economic Research March 2017.

Dynarski, Susan and Judith Scott-Clayton, "Financial Aid Policy: Lessons from Research," *Future of Children*, 2013, *23* (1), 67–91.

Ferreyra, María Marta, Ciro Avitabile, Francisco Haimovich Paz et al., *At a Crossroads: Higher Education in Latin America and the Caribbean*, World Bank Publications, 2017.

Goodman, Joshua, Michael Hurwitz, and Jonathan Smith, "College Access, Initial College Choice, and Degree Completion," NBER Working Paper 20996 2015.

Handel, Benjamin R. and Jonathan T. Kolstad, "Health Insurance for 'Humans': Information Frictions, Plan Choice, and Consumer Welfare," *American Economic Review*, Aug 2015, *105* (8), 2449–2500.

Hastings, Justine S and Jeffrey M Weinstein, "Information, School Choice, and Academic Achievement: Evidence from Two Experiments," *The Quarterly Journal of Economics*, November 2008, *123* (4), 1373–1414.

Hastings, Justine S., Brigitte Madrian, and William Skimmyhorn, "Financial Literacy, Financial Education and Economic Outcomes," *Annual Review of Economics*, 2013, *5*, 347–373.

____ **, Christopher A. Neilson, and Seth D. Zimmerman**, "Are Some Degrees Worth More than Others? Evidence from College Admission Cutoffs in Chile," *NBER Working Paper 19241*, 2013.

____ **,** ____ **, Anely Ramirez, and Seth D. Zimmerman**, "(Un)informed college and major choice: Evidence from linked survey and administrative data," *Economics of Education Review*, 2016, *51* (C), 136–151.

Hoekstra, Mark, "The Effect of Attending the Flagship State University on Earnings: A Discontinuity-Based Approach," *The Review of Economics and Statistics*, November 2009, *91* (4), 717–724.

Hoxby, Caroline M. and Christopher Avery, "The Missing One-Offs: The Hidden Supply of High-Achieving, Low-Income Students," *Brookings Papers on Economic Activity*, 2013, *Spring*, 1–65.

Hoxby, Caroline M and Sarah Turner, "Expanding College Opportunities for High-Achieving, Low Income Students," *Stanford Institute for Economic Policy Research*, Mar 2013. Working Paper.

Huntington-Klein, Nick, "The Search: The Effect of the College Scorecard on Interest in Colleges," *Working Paper*, 2017.

Hurwitz, Michael and Jonathan Smith, "Student responsiveness to earnings data in the College Scorecard," *Available at SSRN: https://ssrn.com/abstract=2768157*, 2016.

Jensen, Robert, "The (Perceived) Returns to Education and the Demand for Schooling," *The Quarterly Journal of Economics*, 2010, *125* (2), 515–548.

Johnson, Justin P. and David P. Myatt, "On the Simple Economics of Advertising, Marketing, and Product Design," *American Economic Review*, June 2006, *96* (3), 756–784.

Kane, Thomas J. and Douglas O. Staiger, "Estimating Teacher Impacts on Student Achievement: An Experimental Evaluation," *NBER Working Paper 14607*, 2008.

Kapor, Adam, Christopher A. Neilson, and Seth D. Zimmerman, "Heterogeneous Beliefs and School Choice Mechanisms," Technical Report 2017.

Kerr, Sari Pekkala, Tuomas Pekkarinen, Matti Sarvimäki, and Roope Uusitalo, "Post-Secondary Education and Information on Labor Market Prospects: A Randomized Field Experiment," 2015.

Kirkebøen, Lars J, Edwin Leuven, and Magne Mogstad, "Field of study, earnings, and self-selection," *The Quarterly Journal of Economics*, 2016, *131* (3), 1057–1111.

Kling, Jeffrey R., Sendhil Mullainathan, Eldar Shafir, Lee C. Vermeulen, and Marian V. Wrobel, "Comparison Friction: Experimental Evidence From Medicare Drug Plans," *The Quarterly Journal of Economics*, 2012, *127*, 199–235.

La Tercera, "'Las 11 instituciones de Educación Superior cuestionadas por irregularidades en 2012'," 2012.

Laureate International, "Our Network," 2017.

Lederman, Doug, "$78.5M Settles U. of Phoenix Case," *Inside Higher Ed.*, 12 2009. Accessed 14 November 2014.

____, "For-Profits and the False Claims Act," *Inside Higher Ed.*, 8 2011. Accessed 13 November 2014.

Lewin, Tamar, "For-Profit College Sued as U.S. Lays Out Wide Fraud," *The New York Times*, 8 2011. Accessed 14 November 2014.

Loewenstein, George, Cass Sunstein, and Russell Golman, "Disclosure: Psychology Changes Everything," *Annual Review of Economics*, 2014, *6*, 391–419.

Looney, Adam and Constantine Yannelis, "A Crisis in Student Loans? How Changes in the Characteristics of Borrowers and in the Institutions They Attended Contributed to Rising Loan Defaults," *Brookings Papers on Economic Activity*, Sept 2015.

McGuigan, Martin, Sandra McNally, and Gill Wyness, "Student Awareness of Costs and Benefits of Educational Decisions: Effects of an Information Campaign," *Journal of Human Capital*, 2016, *10* (4), 482–519.

Muralidharan, Karthik and Venkatesh Sundararaman, "The Aggregate Effect of School Choice: Evidence from a two-stage experiment in India," *Quarterly Journal of Economics*, Aug 2015, *130*, 1011–1066.

Neilson, Christopher, Jose Luis Flor, and David Barraza, "Rol de la informacin para la toma de decisiones de estudios postsecundarios," Informe IPA 2, Innovations for Poverty Action Nov 2016.

Nguyen, Trang, "Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar," MIT Working Paper 2010.

Ockert, Bjorn, "What's the Value of an Acceptance Letter? Using Admissions Data to Estimate the Return to College," *Economics of Education Review*, 2010, *29(4)*, 504–516.

Oreopoulos, Philip and Ryan Dunn, "Information and College Access: Evidence from a Randomized Field Experiment," *Scandinavian Journal of Economics*, 2013, *115(1)*, 3–26.

Organization for Economic Co-operation and Development, "Education at a Glance: OECD Indicators," 2013. Accessed 4 March 2015.

Reyes, Loreto, Jorge Rodrguez, and Sergio S. Urza, "Heterogeneous Economic Returns to Postsecondary Degrees: Evidence from Chile," *NBER Working Papers 18817*, 2013.

Saavedra, Juan E., "The Returns to College Quality: A Regression Discontinuity Analysis," Unpublished Manuscript, Harvard University 2008.

Scott-Clayton, Judith, "Information Constraints and Financial Aid Policy," *NBER Working Paper 17811*, 2012.

Scott-Clayton, Judith and Veronica Minaya, "Should student employment be subsidized? Conditional counterfactuals and the outcomes of work-study participation," *Economics of Education Review*, 2016, *52* (C), 1–18.

Solis, Alex, "Credit Access and College Enrollment," *Journal of Political Economy*, 2017, *125* (2), 562–622.

State of California Department of Justice, "Attorney General Kamala D. Harris Obtains $1.1 Billion Judgment Against Predatory For-Profit School Operator," Mar 2016.

Stinebrickner, Ralph and Todd Stinebrickner, "A Major in Science? Initial Beliefs and Final Outcomes for College Major and Dropout," *Review of Economic Studies*, 2013, *81(1)*, 426–472.

Thaler, Richard and Shlomo Benartzi, "Save More Tomorrow (TM): Using Behavioral Economics to Increase Employee Saving," *Journal of Political Economy*, 2004, *112(S1)*, S164–S187.

United States Department of Education, 2013. Accessed 14 November 2014.

____ , "Obama Administration Announces Final Rules to Protect Students from Poor-Performing Career College Programs," 2014. Accessed 14 November 2014.

____ , "Web Tables: Student Financing of Undergraduate Education 2011-12," 2014. NCES 2015-173.

United States Government Accountability Office, "For-Profit Colleges: Undercover Testing Finds Colleges Encouraged Fraud and Engaged in Deceptive and Questionable Marketing Practices," 2010. Hearing Before the Committee on Health, Education, Labor and Pensions, U.S. Senate. Statement of Gregory Kutz, Managing Director Forensics Audits and Special Investigations.

Wiswall, Matthew and Basit Zafar, "Determinants of College Major Choices: Identification from an Information Experiment," *Review of Economic Studies*, 2014, *82(2)*, 791–824.

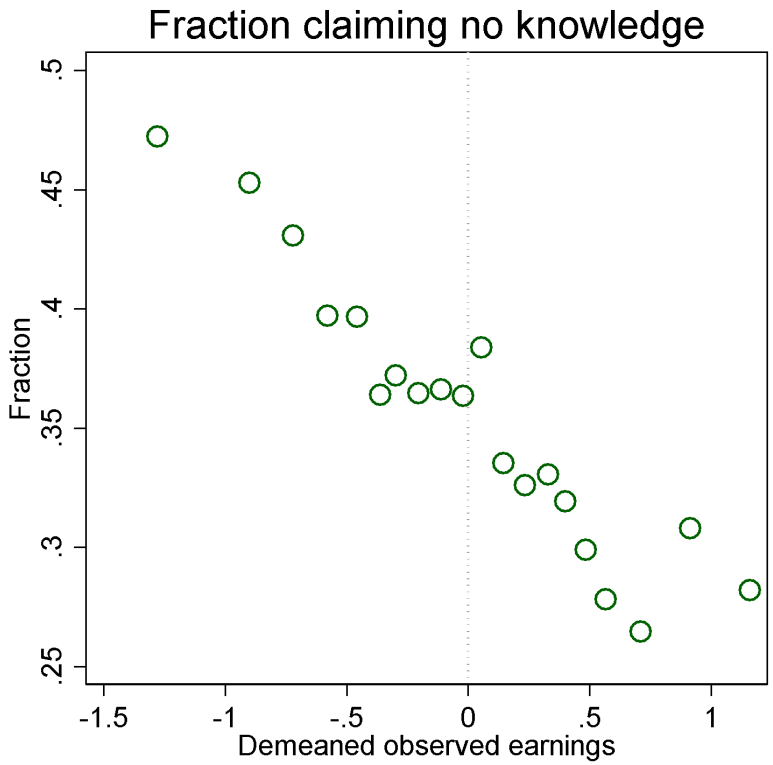
Wiswall, Matthew and Basit Zafar, "How Do College Students Respond to Public Information about Earnings?," *Journal of Human Capital*, 2015, *9* (2), 117–169.

Zafar, Basit, "How do College Students form Expectations?," *Journal of Labor Economics*, 2011, *29.2*, 311–348.

Zimmerman, Seth, "The Returns to College Admission for Academically Marginal Students," *Journal of Labor Economics*, 2014, *32(4)*, 711–754.

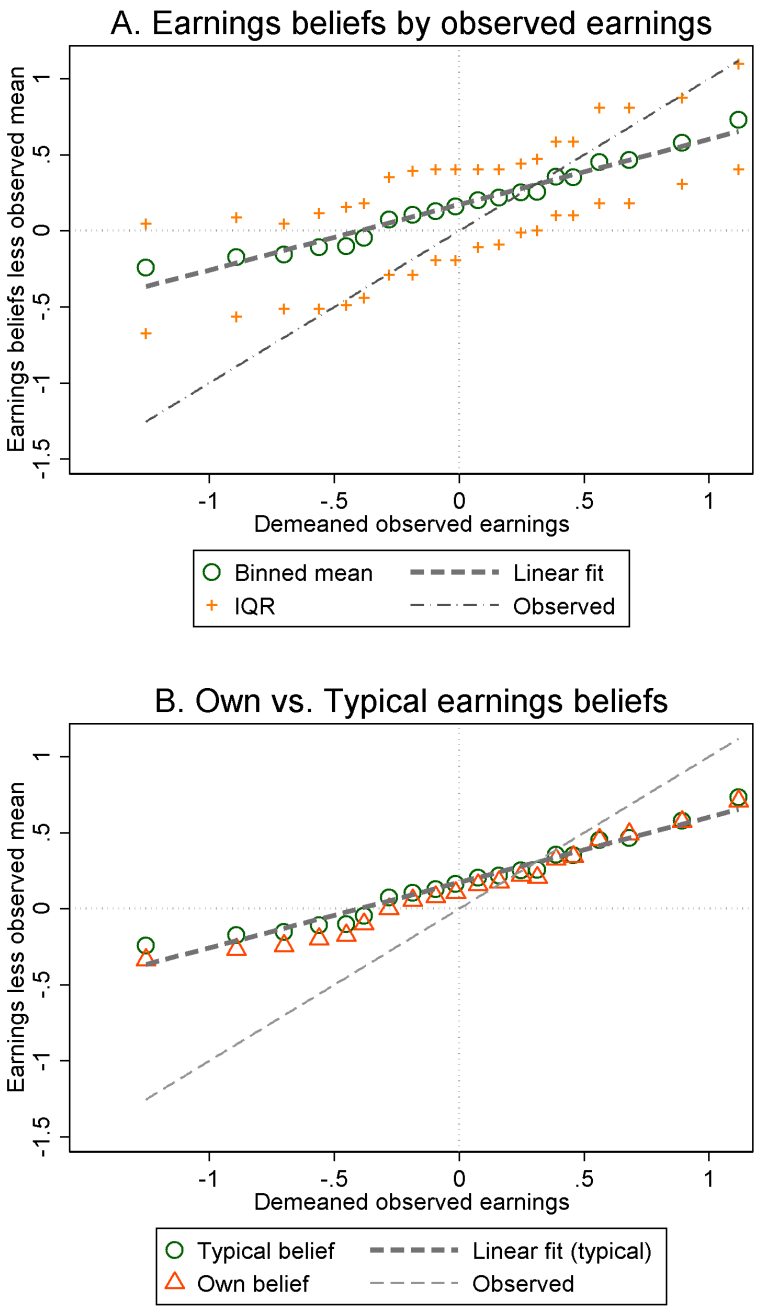
Figures and tables

Figure 1: Belief nonreports by observed earnings ventile



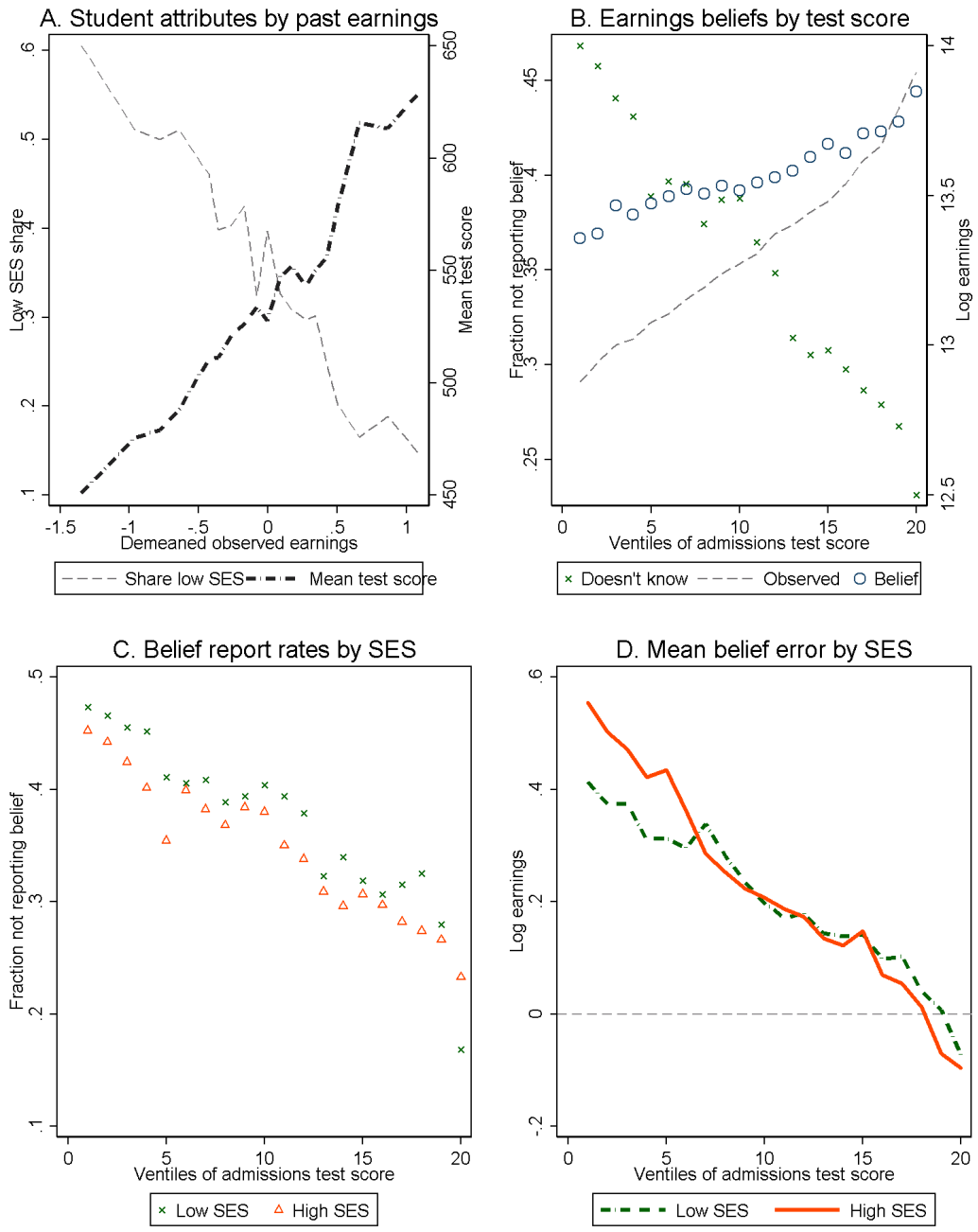
Horizontal axis is demeaned observed (log) earnings for past graduates at students' elicited first choice programs. Each point is the fraction of students who do not report a belief within a ventile of the observed earnings distribution.

Figure 2: Earnings beliefs relative to observed earnings



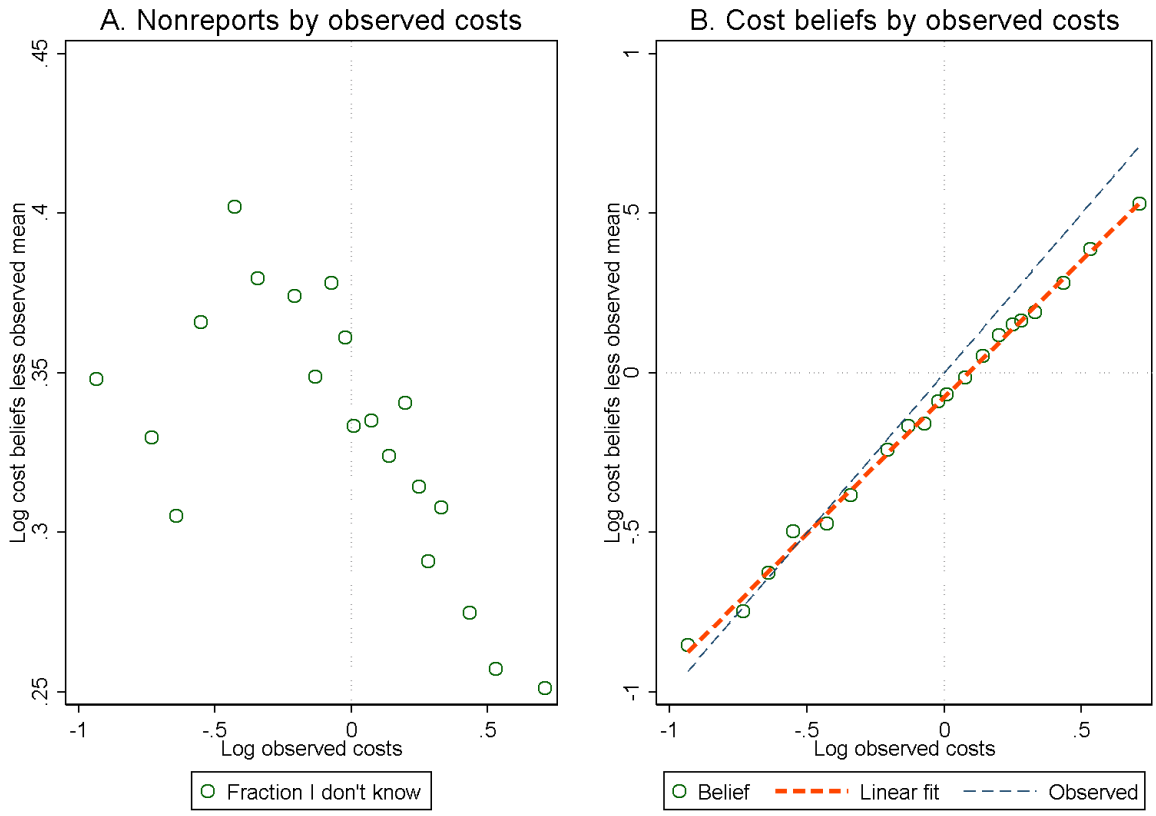
Horizontal axis in both panels is demeaned observed (log) earnings for past graduates at students' stated degree preferences. Points are binned means by ventiles of this variable. Panel A: Typical earnings beliefs by observed earnings outcomes. Circles are mean beliefs about typical earnings, + symbols show the IQR within each bin. Thick dashed line is a linear fit of observed belief means. "Observed" is the 45-degree line, plotted for reference. Panel B: Means of beliefs about own earnings and typical earnings conditional on graduation and observed earnings for a typical student. Thick dashed line is linear fit of typical earnings beliefs. "Observed" is the 45-degree line, plotted for reference.

Figure 3: Earnings beliefs and student demographics



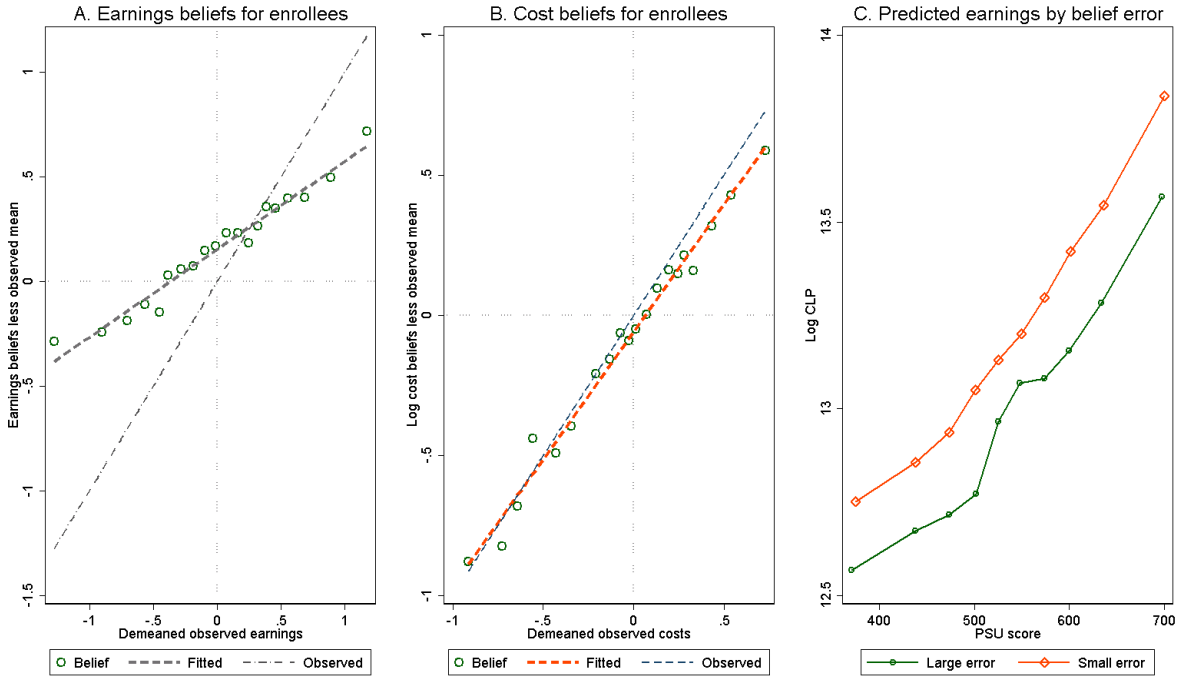
Panel A: Fraction low-SES students (left axis) and mean test scores (right axis) by demeaned observed earnings at preferred degree (horizontal axis). Panel B: Horizontal axis: admissions test scores. Points reflect binned means within ventiles of score distribution. Left axis: Fraction belief non-reports at first-choice program. Right axis: Binned means of beliefs about typical graduate earnings at listed programs (circles) and observed values for past graduates (line). Panel C: Belief report rates by ventile of admissions test score and SES. Panel D: mean belief error by ventile of admissions test score and SES.

Figure 4: Cost beliefs and observed costs



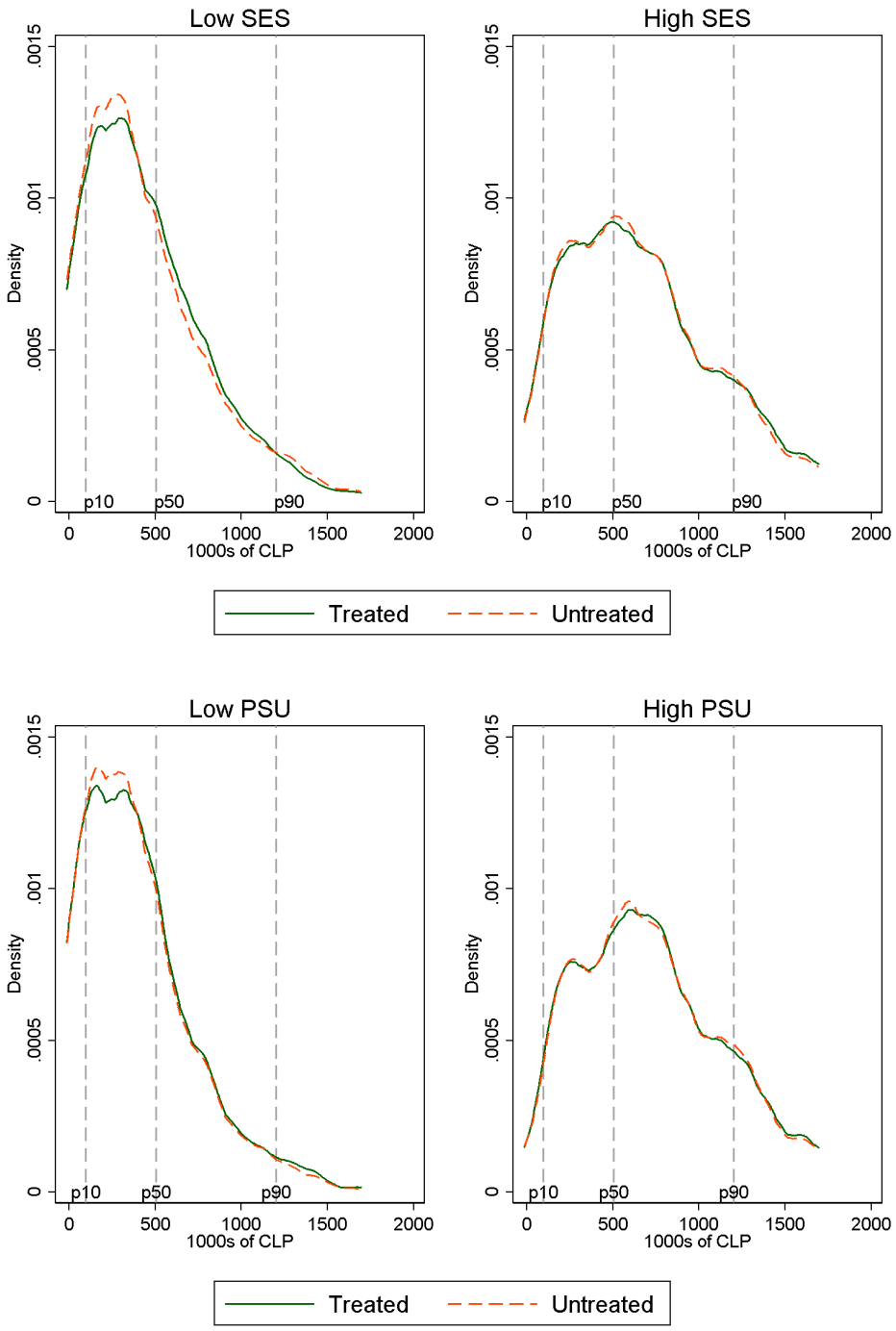
Panel A: fraction of cost non-reports by ventile of demeaned observed cost distribution. Panel B: binned means of cost beliefs (vertical axis) by ventile of observed cost distribution (horizontal axis). Thick dashed line is linear fit. "Observed" is 45 degree line plotted for reference.

Figure 5: Earnings and cost beliefs conditional on enrollment



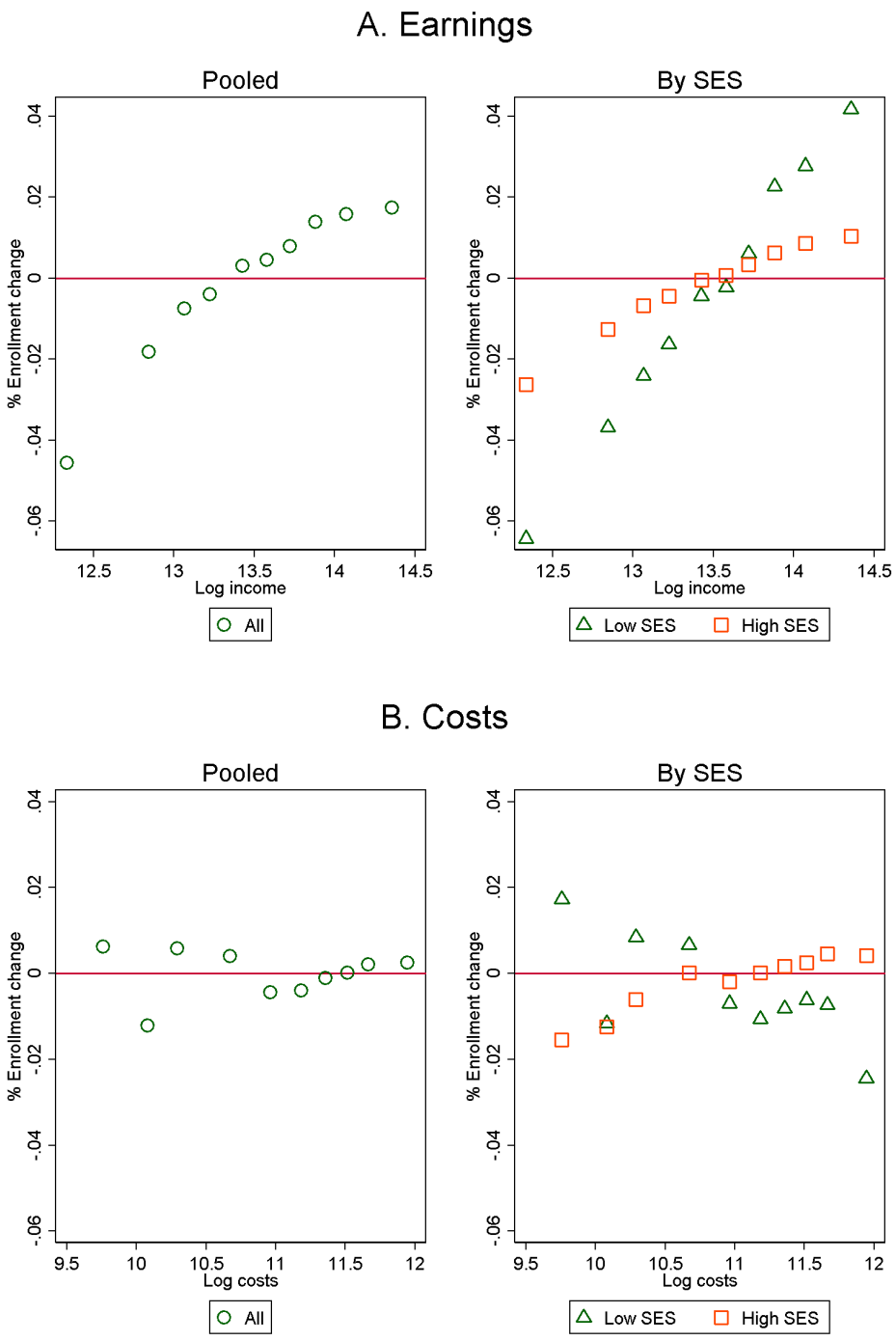
Panel A: (Log) Beliefs about earnings for typical graduates (vertical axis) by ventile of observed log earnings distribution (horizontal axis) for enrolling students. Thick dashed line is linear fit of observed values, with 45-degree line plotted for reference. Panel B: (Log) Beliefs about costs (vertical axis) by ventile of observed log cost distribution (horizontal axis). Thick dashed line is linear fit of observed values, with 45-degree line plotted for reference. Panel C: Predicted earnings at the enrolled degree (vertical axis) by admissions test score (horizontal axis) and belief error type. "Large error" is sample of students with larger than the median positive belief error on their stated most-preferred program. "Small error" is other students.

Figure 6: Distribution of Net Value by treatment status and SES



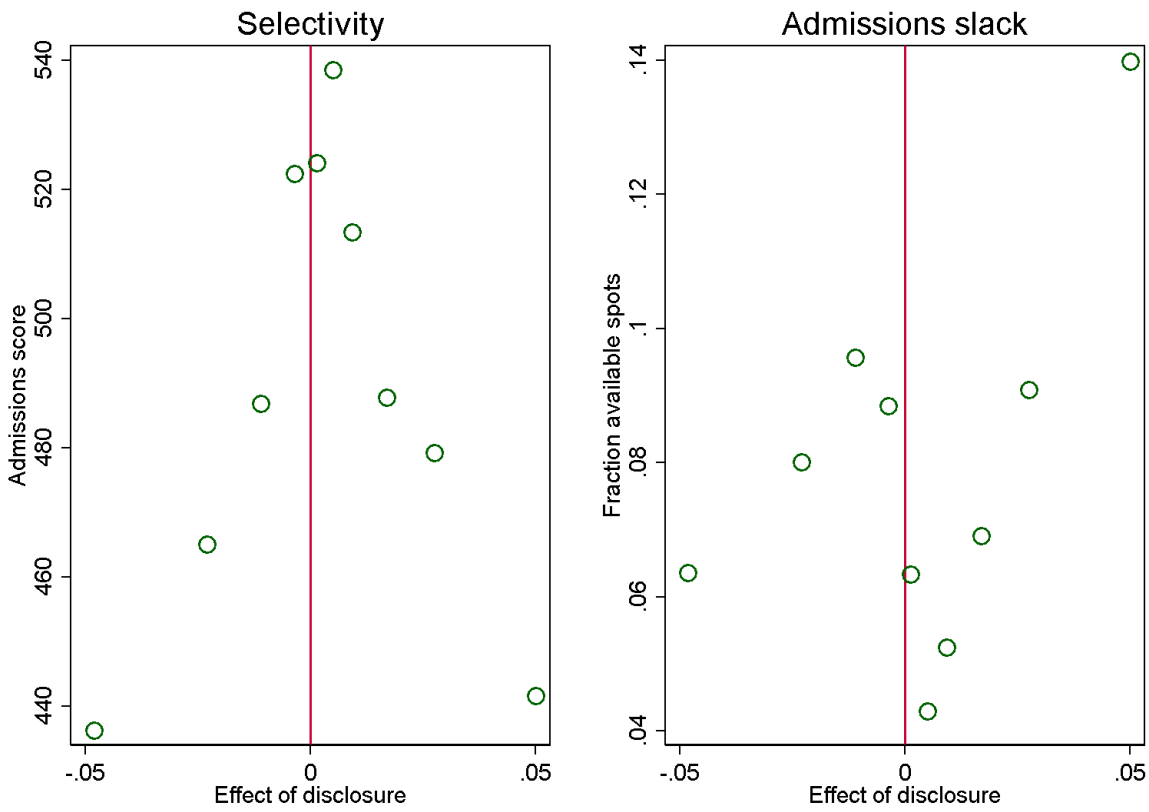
Density of log predicted earnings distribution for matriculating students by treatment status and SES background (upper panel) or PSU score (lower panel). Vertical lines are quantiles of control group distribution in pooled sample. Both panels use an Epanechnikov kernel with bandwidth 75.

Figure 7: Enrollment effects by deciles of log monthly income and costs



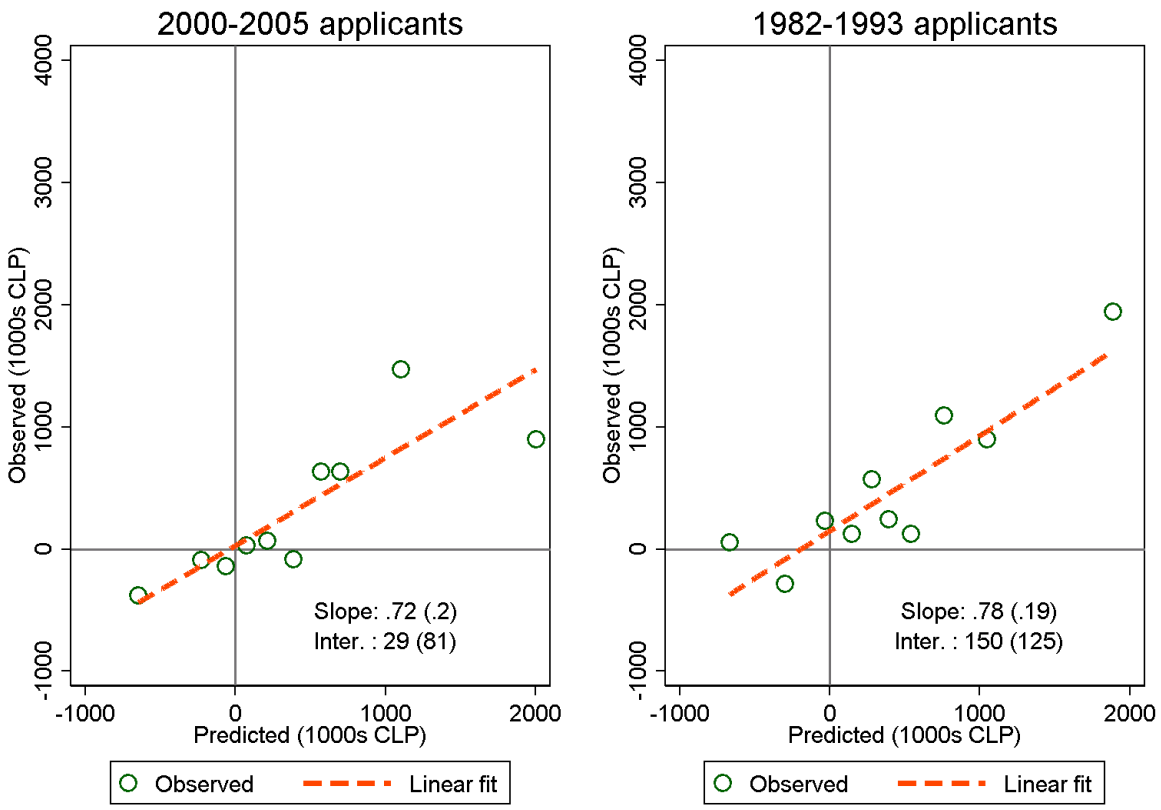
Mean percentage enrollment changes in full-treatment relative to no-treatment counterfactuals by deciles of degree-level mean earnings distribution (upper panel) and costs distribution (lower panel). Left graphs show results for pooled sample and right graphs split by SES. See section 5.5 for description of counterfactual simulations.

Figure 8: Degree selectivity and admissions slack by enrollment effect size



Left panel: mean PSU admissions score (vertical axis) by decile of disclosure effect distribution (horizontal axis). Right panel: mean admissions slack as fraction of total enrollment (vertical axis) by decile of disclosure effect distribution (horizontal axis). Admissions slack is a measure of available spots in a degree program relative to enrolling students. Disclosure effects reflect comparisons of the full-treatment and no-treatment counterfactuals. See section 5.5 for details.

Figure 9: Observed and predicted cross-threshold earnings changes



Regression discontinuity estimates of observed changes in earnings outcomes across admissions thresholds (vertical axis) by predicted changes (horizontal axis). Points are binned means with deciles of the predicted value distribution, and dashed lines are linear fits from estimates of equation (6). Left panel: outcome variable is observed earnings effects for entering 2000-2005 entering cohorts. Right panel: outcome is observed earnings effects for 1982-1993 entering cohorts. See section 6 for details of the benchmarking exercise.

Table 1: Comparison of survey sample invitees, opened email, consenting sample, & respondents

	Invited Sample (1)	Opened (2)	Consent (3)	Respondents (4)	Treated (5)	Treated & Searched (6)
PSU Score (Ave. Language & 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)
Average of Math + Lang 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)
Predicted 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

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 graduation records. Mother and Father having some tertiary education is as reported by the student in the survey component of the national standardized test, SIMCE. Low-SES is defined as coming from a high school in one of the two highest poverty categories. SIMCE scores are standardized high school test scores 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. 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: Elicited earnings and cost beliefs vs. observed outcomes

A. Typical student earnings

	All	Low SES	High SES	Low PSU	High PSU
Slope	0.427	0.416	0.425	0.366	0.463
	(0.007)	(0.014)	(0.009)	(0.012)	(0.009)
Intercept	0.173	0.265	0.123	0.328	0.078
	(0.004)	(0.009)	(0.005)	(0.008)	(0.005)
Share non-reports	0.500	0.566	0.461	0.579	0.435
Noise-to-Signal	1.344	1.402	1.352	1.731	1.158
N	51415	14503	34610	19565	31850

B. Own earnings

	All	Low SES	High SES	Low PSU	High PSU
Slope	0.474	0.450	0.464	0.392	0.498
	(0.006)	(0.012)	(0.008)	(0.011)	(0.008)
Intercept	0.119	0.163	0.088	0.224	0.048
	(0.004)	(0.008)	(0.005)	(0.007)	(0.005)
Share non-reports	0.389	0.444	0.356	0.451	0.337
Noise-to-Signal	1.112	1.223	1.154	1.554	1.008
N	62893	18593	41324	25508	37385

C. Costs

	All	Low SES	High SES	Low PSU	High PSU
Slope	0.879	0.842	0.864	0.797	0.866
	(0.009)	(0.017)	(0.012)	(0.016)	(0.013)
Intercept	-0.083	-0.081	-0.083	-0.106	-0.069
	(0.004)	(0.008)	(0.004)	(0.007)	(0.004)
Share non-reports	0.397	0.453	0.363	0.471	0.336
Noise-to-Signal	0.138	0.188	0.157	0.255	0.155
N	59820	17474	39640	23552	36268

Table compares student beliefs about earnings and costs to observed values. Observations are at the student-degree level. "Share non-reports" is the share of possible belief elicitations to which a student responds "I don't know." "Slope" and "Intercept" are estimated slope and intercept terms from a univariate regression of log beliefs on log observed values. Standard errors (in parentheses) are clustered at the student level. "Noise to Signal" reports the noise-to-signal ratio corresponding to the estimated slope. See Section 5.1 for details. Columns denote student subsamples. Panel A presents results for beliefs about earnings for a typical student, Panel B for beliefs about own earnings, and Panel C for beliefs about costs. See section 5.1 for a description of belief elicitation and benchmarking procedures. Standard errors clustered at student level are in parentheses.

Table 3: Impact of treatment on outcome variables

	Pooled	Low-SES	High-SES	Low-PSU	High-PSU	Low-SES & Low-PSU
Matriculation	0.004	0.000	0.003	-0.005	0.008	-0.012
	(0.004)	(0.008)	(0.006)	(0.006)	(0.005)	(0.010)
All Students						
Net Value	8,270	10,749	5,427	5,071	11,059	5,790
	(5,217)	(7,296)	(7,370)	(5,352)	(8,094)	(6,874)
Earnings Gains	8,856	11,252	5,932	5,534	12,031	5,817
	(5,740)	(7,973)	(8,139)	(5,812)	(8,886)	(7,445)
Monthly Debt	267	319	34.8	385	361	-157
	(536)	(722)	(775)	(503)	(816)	(621)
Conditional on Matriculation						
Net Value	10,029*	15,274*	8,040	12,008*	7,545	18,430*
	(4,230)	(7,149)	(5,435)	(5,547)	(5,455)	(7,366)
Earnings Gains	10,971*	16,083*	9,066	13,091*	8,438	19,288*
	(4,532)	(7,671)	(5,819)	(5,887)	(5,784)	(7,795)
Monthly Debt	376	763	125	1,036*	-166	750
	(435)	(680)	(580)	(491)	(552)	(610)
Degree Average Earnings at Age 26	6,324*	11,759**	3,789	2,682	7,936*	11,337**
	(2,814)	(4,425)	(3,771)	(3,421)	(3,949)	(4,396)
Monthly Payment (conditional on enrollment)	498	824	344	1,197*	164	933
	(459)	(758)	(568)	(578)	(546)	(713)
PDV of long- and short-run returns						
Returns to Degree at Age 50	$2,459,579^{+}$	4,190,955*	1,458,519	1,422,974	2,735,342	3,995,739*
	(1,480,585)	(2,107,587)	(1,947,204)	(1,430,280)	(2,038,518)	(1,704,860)
Returns to Degree at Age 30	999,737*	1,369,854*	778,104	434,677	1,252,546*	1,364,551**
	(399,217)	(568,630)	(526,576)	406,904	(556,829)	(490,399)

Table reports coefficients on treatment dummy from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment and randomization block. Clustered standard errors are in parentheses. 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. Third and fourth panels report intensive margin effects in sample of matriculants. Net Value, Earnings Gains, and Monthly Debt are the values for degrees as exhibited in our experiment. Degree Average Earnings at Age 26 is a regression-adjusted estimate of earnings based that conditions on student SES, test scores, and gender. LR and SR Returns are predicted earnings gains conditional on enrollment projected to ages 30 and 50, respectively. See section 3.4 for details. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. Low PSU is defined as below the median PSU in the experiment sample (median = 537), High PSU is defined as above median PSU. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4: Utility weights and enrollment elasticities for earnings and costs

	All	Low SES	High SES	Low PSU	High PSU	Low PSU, Low SES
A. Utility weights						
Earnings						
Treated	0.3666***	0.4844***	0.3513***	0.1419***	0.2154***	0.2192***
Untreated	0.2879***	0.2977***	0.3159***	0.0699***	0.1027***	0.0691***
Costs						
Treated	-0.6197***	-1.2305***	-0.2334***	-0.9056***	0.7124***	-1.5899***
Untreated	-0.6042***	-1.1262***	-0.2416***	-0.8617***	0.6714***	-1.4982***
B. Earnings elasticities at first choice program						
Baseline						
Treated	0.1816	0.2378	0.1748	0.0737	0.1021	0.1125
Untreated	0.1428	0.1462	0.1574	0.0363	0.0488	0.0354
No geography preference.						
Treated	0.2104	0.2842	0.1994	0.0859	0.1171	0.1325
Untreated	0.1655	0.1748	0.1795	0.0423	0.0560	0.0418
No institutional preference.						
Treated	0.2943	0.3909	0.2809	0.1166	0.1702	0.1792
Untreated	0.2312	0.2403	0.2527	0.0574	0.0812	0.0564
No major preference.						
Treated	0.3206	0.4216	0.3081	0.1238	0.1883	0.1897
Untreated	0.2519	0.2593	0.2772	0.0609	0.0899	0.0598

Panel A: estimated utility weights that students place on earnings and cost outcomes. Results based on logit estimates using survey data. See Section 5.5 for details and Table A.8 for additional results from logit estimation. "Treated" row shows results for treated students, and "Untreated" row results for untreated students. Inference based on standard errors that cluster at student level. + p <0.10, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001. Panel B: Estimated elasticities of enrollment probability with respect to changes in earnings at students stated first choice programs. 'Baseline' is elasticities estimated in data. Subsequent subpanels eliminate preferences over listed degree attributes while keeping mean utility at each degree program fixed. See section 5.5 for details. Columns identify student subsamples.

Table 5: Regressions of observed earnings effects on predicted earnings effects

	2000-2005	1982-1993
Corrected		
Slope	0.716	0.778
	[0.332,1.100]	[0.415,1.142]
Intercept	28.90	149.9
	[-129.6,187.4]	[-95.82,395.6]
Uncorrected		
Slope	0.466	0.180
	[0.324,0.608]	[0.0895,0.271]
Intercept	134.7	341.9
	[-26.77,296.2]	[116.6,567.2]
N cutoffs	500	501

Point estimates and 95% CIs from regressions of RD estimates of observed earnings changes on RD estimates of predicted earnings changes as described in equation 6. Null hypothesis is a slope of one and an intercept of zero. Observations are at degree program level with results weighted by inverse sampling variance of dependent variable. "Corrected" panel reports results that account for sampling error in the independent variable by shrinking values towards the sample mean. "Uncorrected" panel reports results that do not account for sampling error. "2000-2005" column predicts outcomes for 2000 through 2005 entering cohorts. "1982-1993" column predicts outcomes for 1982-1993 entering cohorts. See Section 6 for details.