THE EFFECTS OF EARNINGS DISCLOSURE ON COLLEGE ENROLLMENT DECISIONS*

by

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We study how information about labor market outcomes and costs affects college enrollment choices at scale using a survey and randomized controlled trial administered as a part of the Chilean federal student loan application process. After taking a survey about their preferences over degree programs and their beliefs about earnings and costs at these programs, randomly selected applicants received information on earnings and costs for past students at their stated enrollment choices, as well as access to a searchable database covering all degree programs. The mostly low-SES students planning to enroll in the lowest-earning programs overestimate earnings for past graduates of these programs by more than 100%. Information disclosure shifts students away from low-earning, low-selectivity programs and towards low-selectivity programs with higher earnings for past students, with demand for spots in the lowest-earning programs falling by 5% overall. Disclosure effects are more consistent with updated beliefs and greater weight placed on earnings than they are with increased salience of particular degree options. The disclosure impact is mitigated by students' preferences for non-pecuniary degree attributes like geographic proximity and field of study, suggesting earlier interventions could be more effective.

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1 Introduction

In many countries, federal student loans and grants play a key role in efforts to expand higher education access. The goal is to reduce credit constraints and enable all students to make high-return investments in education (Solis 2015; Autor et al. 2016). However, not all of the investments students make with public funds yield high returns. Rates of student loan default at the worst-performing US institutions exceed 50% (Looney and Yannelis 2015), and students have expressed ex post regret of educational investments in the form of both protests and lawsuits against education providers (Douglas-Gabriel 2016; Schecter 2016).

Why do students enroll in low quality degree programs? One hypothesis is that some students lack accurate information about degree quality and costs, and are vulnerable to targeted marketing directing them to lower-return and higher-cost degrees. Students from low-income and college-inexperienced backgrounds may have particular difficulty accessing reliable information on program costs and benefits (GAO 2010; Lewin 2011; Department of Education 2013; Lederman 2009, 2011; Hoxby & Turner 2015). Policymakers have considered several responses to this challenge, including regulations on which institutions can receive federal student loan dollars (USDOE 2017), and ex post legal prosecution for false advertising about degree attributes, financing, and job placement (Douglas-Gabriel 2015; SOCDOJ 2016).

A third common strategy is disclosure. Disclosure policies provide students with credible information on academic, labor market, and cost outcomes for different degree programs. For example, the College Scorecard produced by the US Department of Education provides students with an online tool to search through institution-specific graduation, earnings, and debt outcomes for past enrollees (USDOE 2015). The goal of disclosure policies is to improve financial outcomes by helping students make better choices about where to enroll. Disclosure may be less intrusive than direct regulation (Lowenstein, Sunstein, and Golman 2014),[1] and its benefits may grow over time if it increases institutions' incentive to provide high quality programs. However, the effectiveness of disclosure depends on what students already know about the disclosed outcomes, on whether students care about these outcomes when making enrollment choices, and on how effectively the government designs and communicates new information.

This paper uses a government-implemented survey and randomized trial in the Chilean higher education system to test the effects of disclosure policies at scale. All college applicants applying for

[1] In the US, proposed gainful employment rules (Department of Education, 2014a) encompass both types of policies discussed here. See Shear (2014) for a description of ranking proposals. White House (2013) details of ranking and accompanying accountability proposals. An analogy in personal finance markets would be providing information on payday lending interest rates versus capping payday loan interest rates or prohibiting payday loans all together.

federal student loans in 2013 were asked to complete a survey about their preferences over different degree programs and their beliefs about earnings and cost outcomes at these programs. Randomly-selected applicants were then given information about cost and earnings outcomes for past students at their preferred programs, and access to a search tool similar to the College Scorecard in the US that allowed them to look up this information for other degree programs. We measure the impact of treatment on enrollment decisions, and then integrate survey, treatment, and enrollment data into a model of enrollment choice. Model estimates provide insight into the mechanisms underlying disclosure effects and the demand effects of a disclosure policy applied to all students.

We show that the predominantly low-SES students with weak academic qualifications who choose the lowest-paying institution-major combinations overestimate earnings for past graduates of these programs by more than 100%. The disclosure treatment shifts students away from the lowest-paying programs and towards other non-selective programs with higher earnings for past students, but its effects are mitigated by students' strong preferences for non-pecuniary degree characteristics such as field of study and geographic proximity.

The survey and field experiment worked as follows. Directly after the submission of student loan applications, students were sent an email from the Ministry of Education (MINEDUC) asking them to log into a secure website to fill out an additional set of questions. Applicants logged in, accepted an informed consent statement, and were asked six questions. These included questions about the institution-major combinations (henceforth "degree programs") to which the student planned to apply,[2] own earnings expectations and expectations for typical-student earnings after degree completion, and expectations for tuition costs for each desired degree. 49,166 students completed the online survey.

After the last survey question, randomly selected students continued to two additional web pages. The first page displayed personalized information about the actual earnings returns to the student's stated first choice degree based on past cohorts. Returns were framed as monthly expected earnings gains from enrollment relative to no tertiary enrollment over 15 years. Next to them appeared tuition costs in monthly payments assuming current federal loan borrowing costs, and a "Net Value" which was the difference between monthly earnings gains and payments in pesos. The earnings gains were estimated using a database of linked student-level high school, entrance exam, college enrollment, college matriculation and administrative tax records that we constructed and which covers all Chilean high school graduates from 1984 to 2014.[3] To encourage search, the page also displayed information on the Net Value from pursuing

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

[3] We constructed this database with the Chilean Government for the purposes of this and related studies.

the same major at similar institutions, or pursuing a different but related major in the same broad field of study (e.g. nursing vs. nutrition) at similarly selective institutions.[4]

Treated respondents then continued to a second page which displayed a searchable database and prompted students to select a major and enter an entrance exam score to initiate search. Based on their entered information, the page populated a table of degrees admitting students with similar scores and sorted in descending order by Net Value. Students were told they could save up to ten search tables and could log back in to view them any time. 43% of students searched. Following treatment, we use administrative data to track students in the treatment and control groups and identify whether and where they chose to enroll in the subsequent school year.

We first use survey data to describe the beliefs of students who choose degree programs with poor labor market outcomes for past graduates. We find that students have fairly accurate beliefs about costs, but that students who list low-earning degrees as their top choices widely overestimate earnings for graduates. The median student who lists a degree program in the bottom ventile of the earnings distribution as his first choice believes that the typical graduate of that program earns 170% more than past graduates actually earned. In contrast, students have approximately correct beliefs about programs near the middle of the income distribution and underestimate earnings outcomes at the highest-return programs.

The students choosing low-earning degree programs have lower test scores and are more likely to come from low-SES backgrounds than students choosing higher-earning programs. 61% of students listing a bottom-ventile degree program as their first choice come from a low-SES background, compared to 32% in the tenth ventile and 15% in the top ventile. Accordingly, low-SES and low-scoring students are much more likely to have large belief over-estimates than other students. Conditional on test scores, median belief errors are fairly similar for high- and low-SES students, but low-SES students are more likely to claim they do not know anything about earnings outcomes at their preferred program.

We next consider how beliefs translate to choices. Using baseline administrative data we show that low-income students choose degrees with lower returns to enrollment than high-income students with similar baseline entrance exam scores. Conditional on admissions test score, low-SES students earn about 13.5% less than high-SES students. Almost half of this gap (47%) is due to differences in degree choice between low- and high-SES students, as opposed to differential earnings outcomes within degrees. This "choice gap" suggests room for disclosure to improve information on earnings returns and potentially impact enrollment decisions.

[4] Potential gains from switching institutions or degrees were drawn from degrees the student could get into based on their entrance exam scores.

We find no significant impact of disclosure on the extensive-margin choice to matriculate in any degree program. Point estimates of extensive-margin effects are economically and statistically insignificant. However, we find positive and significant intensive-margin effects on the Net Value of the chosen degree conditional on enrollment. In the full sample, treatment raises Net Value of the chosen program by 1.7%, or 3.7% of mean potential gains from switching to a peer institution offering a similar degree. Treatment effects are highest for low-SES students with low entrance exam scores. Among low-SES students treatment increases the Net Value of the chosen degree by 3.4% of the mean, or 5.3% of mean potential gains from switching to a peer institution. This represents 38.4% of the "choice gap," defined above. These impacts increase for low-SES, low-scoring students to 5.0% of mean Net Value, or 6.3% of mean potential gains. Increases in net value reflect a shift from programs below the median of the Net Value distribution to programs between the 50th and 90th percentile of that distribution.

Choices for low-income students improve on several measures of degree performance. Treated, low-income students enroll in degrees with significantly lower student-loan default rates and higher on-time repayment rates (as calculated from administrative loan repayment data for past cohorts). The treatment effect on loan default rates represents about 70% of the "choice gap" in default rate of degrees enrolled in between low- and high-income students with similar baseline test scores. We also find significant impacts on estimates of long-run earnings returns. Finally, we show that treated students are no more likely than control students to drop out or switch degree programs by the beginning of the second academic year following application. These results support the hypothesis that earnings predictions based on outcomes for demographically similar past enrollees offer a reasonable guide for current applicants, and that students were not "nudged" into alternatives at which they were immediately worse off.

Motivated by our experimental findings, we use a discrete choice model of college enrollment to explore the mechanisms driving the effects of disclosure and to quantify the effects of disclosure policy on demand for different degree programs. In the model, students choose degrees based on preferences for institutions, fields of study, selectivity as measured by entrance exam scores, and costs and earnings expectations. Students form expectations about their own earnings and costs based in part on noisy estimates of values for past graduates. The disclosure treatment affects students' choices process by a) increasing the precision of students' beliefs about earnings for past graduates, or b) making the specific degree program shown on the recommendation page more salient in the choice process. We use elicited preference data from our survey to capture heterogeneity in preferences and preference intensity by field of study and institution.

Model estimates show that treatment raises the weight students place on earnings outcomes for past graduates when making enrollment choices. It does not change the weight students place on costs. Impacts on earnings preferences are largest for low-SES students. Thus treatment shifts preferences most on the margin where prior beliefs differ significantly from actuals (earnings instead of costs) and for students most likely to have large belief errors (low-SES students). In contrast, the effects of suggesting a field of study on preferences for that field are positive but not statistically significant. Our findings suggest that treatment operates primarily through an increase in the weight that low-information students place on actual past earnings outcomes, rather than through the salience of particular suggested degrees. This is consistent with a mechanism based on belief updating. Results from this exercise also show that students have very strong preferences over geography and narrowly-defined field of study relative to the change in utility weights induced by treatment. Students' unwillingness to substitute away from stated top-choice fields or from their home region helps explain why the treatment effects we observe are small relative to potential gains.

We use our model estimates to simulate the effects of a universal disclosure policy on enrollment at each degree program relative to an alternate counterfactual in which no students are treated. We find that treatment reduces enrollment at degrees with the lowest expected earnings by nearly 5%, and increases enrollment at higher earning degrees by about 2%. Consistent with our reduced-form estimates, students leave the lowest-return programs but choose a wide range of higher-earning degrees. A comparison with data on degree-specific capacity constraints shows that in the short run, the market could absorb the effects of disclosure without causing capacity constraints at high-earning programs to bind. In the long-run, the market may adjust to demand-side pressure for higher earnings returns (Hastings et al. 2015).

In summary, we find that disclosure significantly changed choices for low-SES students and those with low entrance exam scores. The impact on enrollment choices is consistent with updated beliefs about earnings, moving students from degrees where past enrollees have lower earnings and higher loan default rates to degrees with better financial outcomes. While the impact on enrollment choices is small relative to overall enrollment, the web-based intervention was very inexpensive. Using earnings projections, we show that treatment raises the present discounted value of predicted earnings net of costs for respondents by roughly USD 72m, substantially exceeding administration costs of a web-based disclosure policy.

We conclude with a discussion of potential concerns about earnings disclosure policies. One criticism of disclosure policies is that reported earnings means do not reflect causal differences for any particular student and may mislead students who would otherwise make choices based on private knowledge of comparative advantage. This concern may be second order in a setting where the students choosing the lowest-earning programs overestimate earnings for past graduates by a factor of more than two and do not differentiate between own earnings and past averages. We show that earnings predictions do correspond closely to quasi-experimental estimates of degree-level impacts on earnings identified using historic, discontinuous admissions rules (HNZ 2013).

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

Our analysis adds to a growing literature on college "undermatching" for students from low-SES backgrounds. Bowen, Chingos, and McPherson (2009), Dillon and Smith (2013), and Hoxby and Avery (2013) show that many students enroll in colleges less selective than those to which they could be admitted, and that this is particularly common for lower-income students. Goodman, Hurwitz, and Smith (2015) show that students granted access to institutions with higher graduation rates become more likely to complete college. Our findings of cross-institution and cross-major variation in earnings outcomes are broadly consistent with recent evidence from discontinuous admissions rules (Hoekstra 2009; Saavedra 2008; Ockert 2010; Hastings, Neilson, and Zimmerman 2013 [henceforth HNZ 2013]; Zimmerman 2014; Kirkebøen, Leuven, and Mogstad 2014) as well as from non-discontinuity approaches (Altonji, Blom, and Meghir 2012; Reyes, Rodriguez, and Urzua 2013).[5] Like Hastings and Weinstein (2008), Jensen (2010), and Wiswall and Zafar (2014), we find that treatment shifts enrollment towards degrees with a track record of better academic and financial outcomes.

Finally, we contribute to the literature on the impact of behavioral biases, limited information, and decision making skills on the effectiveness of public programs (e.g., Thaler and Benartzi, 2004; Bhargava and Manoli, 2011; Duarte and Hastings, 2012; Bettinger et al., 2012)[6] and regulation and disclosure policy in consumer financial product markets (Agarwal et al. 2010; Collins and O'Rourke 2010; Woodward and Hall 2012; Agarwal et al. 2014; Lowenstein, Sunstein, and Golman 2014). Our approach combines survey responses that measure knowledge and preferences with administrative data on actual decisions and field experimental variation in independent variables of interest to test predictions from models of choice incorporating psychology and limited information. We build on Karlan (2005), Ashraf, Karlan, and Yin (2006), Fehr and Goette (2007), Ashraf, Berry, and Shapiro (2010), Jensen

[5] We note that these findings contrast with Dale and Krueger (2002; 2014).
[6] See Madrian (2014) and Lavecchia, Lieu, and Oreopoulos (2014) for reviews of this literature.

(2010), and Hastings (2015). Our approach and results contribute to the growing body of literature incorporating behavioral economics into public policy design (Chetty 2015).

This paper is part of a set of projects investigating the returns to education and college choice in Chile. Hastings, Neilson, and Zimmerman (2013) use discontinuous admissions rules at hundreds of degree programs to explore how the earnings effects of college admission on long-run earnings vary by selectivity and field of study. Beyer et al. (2015) describes a set of policies regulating the availability of student loans at specific degree programs. These policies were adopted in part in response to the research described here. Hastings et al. (2015) uses a broader set of surveys of Chilean college applicants to describe the challenges students face in their attempts to acquire deploy information and make informed enrollment decisions. Our work draws on a Chile-focused literature describing heterogeneity in returns to higher education and the role of higher education markets education attainment and labor market outcomes (see e.g., Brunner 2004; Brunner 2009a; Brunner 2009b; Reyes, Rodriguez, and Urzua 2013).[7]

2 Higher education and student loans in Chile

Chile is a middle-income OECD member country with a higher education system similar to those in the US and other upper-income OECD countries in terms of educational attainment rates, the role of student loans in financing higher education, and higher education market structure. In 2010, 38% of adults between 25 and 34 years old in Chile had a tertiary degree, compared to 42% in the US (OECD 2013). 35.8% of students enrolled in Chilean higher education institutions used state-backed student loans in 2011, compared to 40.2% in the US.[8]

Public, private non-profit, and private for-profit firms provide tertiary degrees in Chile. There are three main degree levels and three institution types: technical schools (CFTs) offer two- to three-year technical degrees, professional institutes (IPs) offer both technical and vocationally-oriented four-year degrees, and universities offer traditional undergraduate and graduate degrees.[9] In 2012, universities accounted for 58.4% of all undergraduate enrollment while professional institutes and technical schools accounted for 28.1% and 13.5% respectively. IPs and CFTs are run by private companies and can be for-profit or not-for-profit. Universities may be public or private not-for-profit. In practice, however, portions

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

of some universities are owned by for-profit parent companies, including companies like Laureate International and the Apollo Group, which also own for-profit universities in the US.[10]

Students in Chile apply to, take courses within, and graduate from institution-major combinations (e.g., Sociology at the University of Chile). We will refer to an institution-major combination as a "degree." Entrance exam scores are the key determinant of admissions as well as loan and scholarship awards. The standardized test is called the *Prueba de Selecion Universitaria*, or PSU.[11] Entrance exam takers complete exams in Mathematics and Language, and may take additional exams in subjects such as science or history. Scores are scaled to a distribution with a mean and median of 500 and standard deviation of 110.

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

Students admitted to less prestigious universities typically have entrance exam scores over 350. Most technical and vocational schools do not require an entrance exam score for admission, though many students who have entrance exam scores enroll in their degree programs. In what follows we will measure selectivity using the average of Math and Language scores for enrolling students.

Chilean students rely primarily on two subsidized student loan programs. The older type of loan is the Fondo Solidario de Crédito Universitario (FSCU). FSCU loans are both need- and merit-based, and have existed since 1981.[12] To qualify for a FSCU loan, students must be Chilean citizens, have "family income that makes payment of tuition difficult or impossible,"[13] and have an average PSU score in Math and Language of at least 475 points. FSCU loans can only be used at CRUCH institutions. The interest rate is set at 2% and the loans are administered directly by the universities and funded by the government.

[10] See "Las 11 instituciones de Educación Superior cuestionadas por irregularidades en 2012." *La Tercera*. 27 November 2012. http://www.latercera.com/noticia/educacion/2012/11/657-495574-9-las-11-instituciones-de-educacion-superior-cuestionadas-por-irregularidades-en.shtml. Accessed 2 May 2013. Laureate International owns five universities in the US, including Walden University and Kendall college. See http://www.laureate.net/, accessed 7 May 2013. The Apollo Group owns the University of Phoenix in the US. See Apollo Global Fact Sheet, accessed 7 May 2013. See http://www.apollo.edu/sites/default/files/files/Apollo-Group-Apollo-Global-Fact-Sheet.pdf for a discussion of Apollo's purchase of the Chilean university UNIACC. In 2009, the Apollo Group settled in a False Claims law suit for its recruiting and advertising practices in the US. See http://www.bloomberg.com/apps/news?pid=newsarchive&sid=a7cFhPKPB1mA. Accessed November 16, 2014. http://www.republicreport.org/2014/law-enforcement-for-profit-colleges/ provides a compilation of regulatory actions and inquiries against for-profit higher-education chains in the U.S. by both federal and state authorities. Accessed November 16, 2014.

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

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

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

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

To increase higher education opportunities for low-income students, the government introduced the Crédito con Garantía Estatal (Loan with State Guarantee, most commonly known as CAE, for Crédito Aval del Estado) beginning with the 2006 school year. [14] CAE can be used to finance education at any accredited postsecondary institution: CRUCH universities, accredited private universities, professional institutes, and technical schools are eligible. CAE eligibility is both need- and merit-based. To study at a university, first-time applicants need to have scored an average of 475 on the PSU (the same as the Fondo Solidario loan program). To enroll in a technical or professional degree, students need either a high school GPA of 5.3 (approximately the median GPA, or a C average), or an average PSU score of 475. Recipients must be from the lowest four income quintiles.[15] 35.6% of low-SES postsecondary students used CAE loans in 2012.

CAE loans reshaped the higher education landscape in Chile. Following the introduction of CAE loans, the fraction of higher education revenues in Chile coming from loan dollars rose by 170%,[16] and college enrollment rates rose by more than 50% as a fraction of the college-aged population, from 48% in 2005 to 74% in 2012.[17] Many of the new enrollees came from low-SES or low-performing backgrounds. The fraction of first-year college enrollees coming from low-SES backgrounds rose by 27% from 2005 to 2012, and the fraction scoring below the median on their high school standardized tests (the SIMCE) increased by 107% from 2006 to 2012. Online Appendix Section 6 provides further detail on changes in baseline ability of freshman enrollees, total enrollment and fraction of tuition revenues coming from federal student loans by degree selectivity.

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

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

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

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

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

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

3 Data

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

3.1 High school records

We use student-level high school records for the years 1995 through 2012. The high school data include basic student covariates such as gender and parental education, scores on standardized tests administered to 10th graders (known as the SIMCE, or Sistema Nacional de Medición de la Calidad de la Educación), high school identifiers, and high school characteristics.

Also included is data on school-level ratings of socioeconomic status (SES) computed by Mineduc. The SES rating categorizes schools from A (lowest SES) through E (highest SES) and is based on parental income. Since we do not observe parental income directly, we use high school SES as a proxy for student SES. High schools in Chile are small: the median graduating senior class size is 57.[18] Students coming from A and B schools are categorized as low-SES. Table A.2 shows how family background, academic performance, and school characteristics vary with school poverty status, and describes cross-validation of poverty rankings using available tax records for parents of children attending each high school. Schools categorized as low-SES are much more likely to be municipal public schools (as opposed to private or voucher schools), and graduates from low-SES schools are much less likely to attend college or have parents who completed college degrees.

3.2 College and college application records

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

Data on college application, enrollment, graduation, and student aid come from several sources. Application records include entrance exam scores for all students by subject for the years 1980-2013. We observe loan applications and awards for the years 2007 through 2013 as well as which students take out loans and when, when students enter repayment, and payment status for loans in the repayment stage. We use these data to construct cohort-degree specific summary statistics on loan repayment and default rates.

We track students post treatment using enrollment and graduation data from MINEDUC, available for the years 2000 to 2014. These data follow students semester by semester, recording major- and institution-specific enrollment and graduation outcomes. We do not observe semester-by-semester grade outcomes, but do observe listed semester-level tuition and suggested degree length. We focus our experimental analysis on initial enrollment choices (made in 2013), and present additional findings on schooling persistence using 2014 data in section 6.2.

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

3.3 Labor market outcomes

Through an agreement with the tax authority granted for the specific purpose of informing higher education policies, we were permitted to link the database of student records to tax returns from the 2005-2013 earnings years on a secure computer within the tax authority.[19] Over 99% of individuals in our data have matches in the tax records. Tax returns include wage, contract, partnership, investment and retirement income. HNZ (2013) describe the tax data in detail, and provide an example of a tax form to illustrate the components used to calculate income. We were able to access tax data only inside the Chilean tax authority on a secure, dedicated computer. In compliance with Chilean law, we were permitted to take out aggregate data and regression output.

We use tax records to construct several measures of earnings outcomes by degree program (institution-major) and student characteristics. The first, which we term "Net Value," was provided to students treated with our informational intervention. We worked with MINEDUC to develop a measure the agency deemed appropriate for providing to students. MINEDUC's preferences at the time of the intervention included a focus on outcomes for graduates of degree programs rather than enrollees, and on

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

binned means rather than regression-adjusted predictions. [20] MINEDUC preferred to focus on earnings and cost outcomes discounted back to the year of labor market entry, which may differ depending on educational choice, as opposed to, say, the first year following high school completion.

With these constraints in mind, we compute Net Value as

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

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

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

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

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

Here, y_{ijct} is labor market earnings for student *i* enrolling in degree program *j* in cohort *c* at labor market experience year *t*. X_{ict} includes dummies for student socioeconomic status, gender, and whether a student took the entrance exam, linear controls for entrance exam score, years of labor market experience, interactions between labor market experience and student covariates, and tax year dummies. Z_j are major

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

specific dummy variables, and W_{ijt} are interactions between major and gender, SES, test taking and test scores, and labor market experience. μ_{jc} are degree-cohort specific mean residual components and ϵ_{ijct} is a mean-zero idiosyncratic error. We estimate these equations separately within five selectivity tiers $s(j)$ and by broadly defined CINE-UNESCO areas of specialization.[21] See Online Appendix Section 3 for more details on estimation.

The earnings measure we consider from this regression is predicted earnings averaged across cohorts, $\hat{y}_{ijt}$. This measure captures degree-specific earnings outcomes conditional on enrollment, including cross-degree differences driven by student sorting. Cross-degree differences may vary with labor market experience. In our main analysis, we focus on earnings eight years after college application, or approximately age 26. We choose age 26 because it allows students enough time to complete schooling. Earnings outcomes at later ages are also of interest, but measurement is more difficult because we observe fewer cohorts and the population of degree programs changes over time.[22] In Section 6, we discuss results in which we compute the present discounted value of earnings through ages 30 and 50 for each degree program using more aggregated data on selectivity- and field-specific earnings profiles. Our focus on early-career earnings outcomes reduces observed effects in percentage terms compared to estimates that include data for older earners. See Online Appendix Section 3 for additional details.

3.4 Non-earnings degree characteristics

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

3.5 Descriptive Statistics on Choices, Earnings and Loan Outcomes

Figure 1 plots financial outcomes and demographic characteristics for past enrollees as a function of the entrance exam scores of students they accept. Each point on the graph is a mean, 10th percentile or 90th

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

[22] 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.

percentile of outcomes across all degrees who would likely admit a student with a particular PSU score.[23] Thus it plots characteristics of the potential enrollment choice set for students by student PSU score. Panel A shows predicted earnings at age 26 for first-year college students between 2007 and 2011 calculated from estimates of equation (2), as well as a horizontal line showing average earnings for high school graduates who do not enroll in college. Panel B shows loan repayment rates for past cohorts, and Panel C shows fraction past enrollees coming from low-SES high schools.

Predicted earnings rise steadily by PSU score, with mean earnings for students at the 75th percentile of the score distribution (581) 52% higher than those for students at the 25th percentile (431). However, within test score, students choose degrees characterized by very different earnings outcomes. For students with PSU scores equal to 505 – the median for college enrollees—degrees at the 90th percentile of the predicted earnings distribution have mean earnings twice as high as those at the 10th percentile. Mean earnings for the average high school graduate remain close to those for the 10^{th} percentile college degree past the 75^{th} percentile of the score distribution. Many students across a choose degree programs where the labor market returns may be negative. Panels B and C illustrate degrees with lower earnings, and potentially negative returns, are also serving students who are much more likely to come from low-SES backgrounds. Their students are also much more likely to default on their student loans.

Figure 2 plots four lines showing how choices and earnings returns vary within SES. The outer two lines are the mean expected earnings at degrees admitting students with a particular PSU score by High- versus Low-SES status. The difference between these two lines incorporates both within-degree differences earnings by socio-economic status – the SES returns gap – as well as differences in earnings across degrees chosen by High- versus Low-SES students with similar entrance exam scores – the SES choice gap. The inner two lines show mean earnings for High- versus Low-SES students, weighting degrees at population enrollment weights. The difference between Low-SES and Low-SES-common-weights shows the choice gap. Low SES students choose degrees with lower earnings among the degrees they qualify to get into; the mean Low SES line is lower than the Low-SES-common-weights line. In contrast, the mean High-SES-common weights line is above the mean High-SES-common-weights line, implying High-SES students choose degrees with higher earnings than the average student does, conditional on ability.

Conditional on ability, low-SES students enroll in degrees where their earnings are on average 13.5% lower than for high-SES students. Holding enrollment weights fixed at population averages within

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

score bins, low-SES students earn 7.2% less in expectation than high-SES students. Differences in enrollment choices thus account for the remaining 6.3%, or just under half of the earnings gap. Incorporating tuition and loan costs of 2%, low-SES students with enrollment scores at the median for enrolling students changed their degree choices to be the same as those in the population as a whole, their predicted monthly earnings would rise by $51,089 CLP. Their costs would rise by only $6,748 CLP. Low-SES students would do better in expectation if they chose the same distribution of degrees as the broader population.[24]

Choice-driven gaps in earnings outcomes for low- and high-SES students persist even net of costs and suggest that many students, and particularly those from low-income backgrounds, could choose and get into degrees with higher returns. We use our survey responses to investigate whether differences in information may explain differences in choices, and then use our RCT to test whether information can affect choices and close the cross-SES gap in predicted financial outcomes.

4 Survey and experimental intervention

The survey and field experiment were constructed as follows. Students in the 2012 graduating high school cohort and all other PSU registrants (including those from older high school cohorts) were pre-assigned to treatment and control groups. Treatment status was stratified by high school for current high school seniors,[25] and by prior PSU test score (in 50-point bins) for PSU registrants who had graduated in the two prior cohorts.[26] This list was merged to loan applications as the applications were completed. Upon submitting their applications, loan applicants received an email from MINEDUC with the subject line "Código Confirmación FUAS" (FUAS Confirmation Code). The email asked applicants to participate in a brief survey that would be used by MINEDUC to make decisions about higher education, and that they would receive a confirmation code at the end of the survey. They were informed that their survey

[24] Finally, if we decompose the difference in choices (top line to bottom line) into differences in choice of institution versus choice of major, conditional on ability, we find that a little less than half (5.5% out of 13.5%) of the SES gap in earnings is attributable to within-major differences in institution effects. To calculate this we set all degree-specific effects, μ_{jc}, to the mean of μ_{jc} so that differences in predicted earnings across enrollees come only from SES effects and difference in choice of major.

[25] Note that high school classes in Chile are small. Median graduating class size in 2012 was 57. Schools were broken into groups based on high school type (private not-accepting-vouchers, private voucher-accepting, and municipal), the fraction of students taking the PSU and the average PSU score from the prior two senior cohorts. Half of the schools within each randomization block were assigned to treatment. An advantage of school level randomization is that estimated effects will better predict what would be observed in a universal policy rollout in the presence of school-level peer spillovers.

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

responses would be kept anonymous, used only for research, and would not affect their FUAS applications. Emails were managed using a service which allowed us to track bounce-backs, opens, and click-throughs for each email address.

Upon opening the email, applicants were invited to click a link taking them to the survey website. They logged in with their identification number and email address and were given an informed consent to accept or reject. Conditional on acceptance, they began the survey. The survey asked six questions, each appearing on its own page, with participants clicking a “next” button to proceed to the next question. Each question could only be completed once: if a respondent left the survey and started again they would restart where they left off. The survey program adapted questions based on prior responses. Survey materials in Spanish (with English translations) are in the Online Appendix Section 2, along with screen shots of the survey and information treatment pages.

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

Upon completing the final question, control subjects were shown a thank you page with their confirmation code. They also received a thank you email with the same message. Treated students continued to a new page which displayed five pieces of information. A table at the top presented the monthly earnings gain component of the Net Value measure, described in Section 3.3 in the left column. The second column of this table displayed the monthly cost component. The third column displayed the Net Value measure itself – the difference between monthly earnings gains and costs. Accompanying text explained to students that these values were derived from past data on earnings and costs.

In a highlighted box below this table, students were told whether there were other institutions they could likely get into which offered the same major with a higher Net Value, and were shown the additional Net Value associated with a switch to the highest Net Value degree (though they were not told which institution offered this value). The net value gain was calculated by the web application, referencing a back-end database on earnings outcomes at different degree programs. The web application searched across institutions offering the same major as the first-choice degree, looking for degree programs with similar entrance exam distributions and higher Net Values. Finally, treated participants were shown a second highlighted box indicating whether or not there were other degrees within the same broad field of study as their first-choice degree that offered higher Net Value, as well as the expected Net Value gain from the within-field switch. Again, the web application searched across degrees with similar entrance exam score distributions to the listed first choice.[27]

After the information and suggestion page, treated subjects clicked through to a final page, the "Buscador de Carreras" (Career Searcher). It explained what the searchable database was, and gave subjects a place to enter a PSU score, degree level (technical or university/professional), and major at the top of the page to populate a table of Net Values below. When populated, the table displayed institutions offering the specified major and serving students with similar PSU scores.[28] Institutions were sorted in descending order by Net Value. The table displayed the institution name, the major, the earnings gains for graduates in monthly terms, the monthly loan costs, and the Net Value. It also displayed a suggested alternative major to search for with higher Net Value but in the same field of interest (e.g. suggesting nursing to someone interested in nutrition). Students were informed at the top of the page that this new database was being produced by Proyecto 3E – a consortium of international researchers collaborating with MINEDUC - using tax records of past graduates, and with the purpose of helping students make informed decisions for their future. Students could log back in at any time and compile and view up to ten comparative tables to use in choosing their degree. This final page also contained a thank you message and the confirmation code; students were not required to search the database.

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

[27] The median gain in predicted earnings associated with the switch described in the first box was equal to 33% of predicted earnings in students' first listed choice. The median tuition change associated with the switch was 0. The median gain in predicted earnings associated with a switch to the degree program described in the second box was equal to 156% of predicted first choice earnings. The median tuition change was 34.2%. See Table A.3 for more details.

[28] 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.

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

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

5 Empirical Analysis

5.1 Survey Responses on Expectations

Figure 3 describes students' responses to survey questions about expected earnings and compares them to true values observed in tax data. Panel A describes students' beliefs about earnings for past graduates by ventile of the distribution of earnings values observed in the tax data. Students who list the lowest-paying programs among their top choices are much likely to state that they do not know earnings outcomes for past students, with rates declining from 47% in the lowest ventile to 28% in the highest. Conditional on reporting a belief, the median student reporting a preference for bottom-ventile programs overestimates earnings by 99 log points (169%). Beliefs rise less than one-for-one with true values, so that students in the 14^{th} ventile have approximately accurate beliefs while students underestimate earnings at the most selective programs.

Students' beliefs about own earnings parallel their beliefs about typical earnings. As shown in Panel B of Figure 3, students generally believe they will earn slightly less than the typical past graduate of a program. The median bottom-ventile student has own earnings beliefs that are 82 log points (127%) higher than the observed value for past students, while students in the top ventiles believe they will earn less than past graduates.

Students who express preferences for the lowest-earning programs come from low SES backgrounds and have low admissions test scores. Panel C of Figure 3 plots mean low-SES share and mean PSU scores by ventile of the true earnings distribution. 60% of students in the bottom ventile come from low-SES backgrounds, compared to 15% in the top ventile. The average bottom-ventile PSU score is 450, compared to 630 in the top ventile. Accordingly, 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).

Students' response to disclosure depends on their choice set, which is determined largely by admissions test score. Panel D of Figure 3 plots non-response rates 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. Students scoring at the fourth ventile of the PSU distribution overestimate their earnings by 35 log points (41%) over actual earnings, while those in the 16^{th} quantile accurately guess the average earnings. Degrees in this selectivity range serve mostly low-SES students. They also have the highest default rates on loans. Conditional on PSU score and reporting any belief, the median overestimate is slightly smaller for low-SES students than for high-SES students in the lower half of the score distribution.

Figure 4 displays the distribution of cost beliefs by ventiles of the true value of costs (Panel A) and by ventiles of test score (Panel B). Cost beliefs closely track true cost values throughout the distribution.

5.2 Experimental estimates of information treatment

5.2.1 Experimental results

Table 2 shows the impact of information on Net Value, earnings, and cost outcomes in students' chosen degrees. We show results separately for the full sample of students, and for subsamples by SES and PSU score. Specifications reported here and in following tables of experimental results include controls for randomization block and for the value corresponding to the dependent variable of students' stated first-choice degree (i.e., Net Value of first-choice degree if the dependent variable is Net Value, monthly debt payment of first choice degree if dependent variable is monthly debt payment). These controls reduce standard errors but do not substantively alter point estimates. Tables A.6, A.9, and A.10 present alternate estimates that drop all controls and examine the effect of treatment on changes between outcomes at students' stated first choices and the degrees in which they ultimately enroll. Standard errors allow for clustering at the high school level for students applying to college directly out of high school.

The first panel shows impacts of treatment on the extensive margin decision to matriculate to any degree. The impact is very close to zero and statistically insignificant across all subsamples. The second panel shows the impact of treatment on monthly debt, earnings gains (per month over 15 years vs. no college enrollment) and Net Value (the difference between the two). For the 23% of the sample who did not matriculate to any degree, these three values are by definition zero. The overall impact of treatment is therefore the change in the dependent variable given enrollment times the probability of enrollment (since the impact of treatment on enrollment is zero). The effects of treatment on Net Value and earnings outcomes at the chosen degree are not statistically significant.

Because the impact of treatment on matriculation is zero, we can estimate the inframarginal impact of information on the earnings and cost characteristics of the enrolled degree.[29] The third panel shows the impact of treatment conditional on matriculating to some tertiary degree. Treatment effects on Net Value and earnings outcomes are statistically significant in the full sample and are driven by large gains for students from low-SES backgrounds and particularly by 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 mean Net Value for low-SES students matriculating in college, 5.3% of the average gain associated with a switch to a peer institution, and 28.4% of the average monthly debt payment. The impact of treatment comes from gains in earnings, not savings on tuition. The impact is similar among students with low SES scores and increases by about 20% among low-SES students with low PSU scores.

The fourth panel of Table 2 presents estimates of the effect of treatment on earnings at age 26 estimated from equation (2), conditional on enrollment as opposed to graduation (the measure used in Figures 1 and 2). This measure reflects changes in degree "value added" conditional on gender, student SES, and test score. We find positive and statistically significant treatment effects for this outcome as

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[29] Let R denote long-run annualized real return of the degree enrolled in, let M be an indicator if a student matriculates to any tertiary degree, and let T be an indicator of whether the student is in the treatment group. Then $\frac{dE(\mathrm{R})}{d(T)} = \frac{d\,\Pr(\mathrm{M}=1)}{dT} \cdot E\left(R \mid M=1\right) + \frac{dE\left(R \mid M=1\right)}{dT} \cdot \Pr(M=1)$. (McDonald and Moffit, 1980). Note also here that treatment is independent of student observables conditional on matriculation. A joint test of the effect of student observable characteristics on treatment within the sample of matriculating students fails to reject the null, returning a p-value of 0.194.

well, again driven by gains for low-SES students and students with low-PSU scores. One way to frame these results is in the context of the cross-SES earnings gaps displayed in Figure 2. The treatment effect on predicted earnings for low-SES students of $11,759 CLP is equal to 18.4% of the gap between earnings outcomes for high- and low-SES students conditional on ability (i.e., the average gap between the upper and lower lines in Figure 2, weighted by the low-SES score distribution). The treatment effect is equal to 38.4% of the component of that gap driven by differential degree choice (i.e., the sum of the gap between the lower two lines and the gap between the upper two lines).

Figure 5 shows where choices change the most in response to information by plotting the distribution of Net Value at the matriculating degree by treatment status. Treatment effects stem from a shift of mass from between the 10^{th} and 50^{th} percentiles of the control group distribution to roughly the 50^{th} through 95^{th} percentiles. As reported in the top row of Table A.7, low-SES students in the treatment group are 2.3 percentage points less likely to matriculate in degrees with Net Values below the control group median. Treatment appears to push applicants away from very low-earning degree programs and towards a range of higher-earning degrees.

Table 3 shows treatment effects on additional measures of degree performance. Using administrative data on loan taking and repayment, we compute degree-specific default and repayment rates as of 2013. Because the CAE loan program is relatively new, students using CAE loans at many degree programs had not yet entered repayment at the time of treatment. We focus our attention on degree programs in which we observe at least ten students who have entered repayment and for whom the time elapsed since matriculation is at least the predicted degree length plus 1.5 years.[30] We are able to compute repayment rates for 58.0% of the degrees chosen by students in our experimental sample. These degrees tend to be shorter in duration and less selective than the enrollment-weighted population of degrees. Treatment does not make students more likely to choose a degree in the repayment sample.[31]

The second panel of Table 3 displays unconditional mean on-time repayment and default rates for degrees chosen by high- and low-SES students, mean repayment and default rates conditional on entrance exam score (weighted by the population score distribution), and the effects of the information treatment on default and repayment rates at chosen degrees. On-time repayment rates are 8.5 percentage points higher at the degrees chosen by high-SES students. This gap falls to 1.4 percentage points conditional on entrance exam score. Treatment pushes low-SES students toward degrees with on-time payment rates that are 1.0 percentage points higher. This is equal to roughly 12% of the unconditional gap between choice outcomes for high- and low-SES students and roughly 70% of the gap conditional on exam score.

[30] We employ this last restriction so as to avoid considering only the dropouts from longer degree programs.
[31] Table A.14 describes degrees in and out of the repayment sample.

Findings for default rates are similar. The effect of treatment on default rates in the full population is small and does not differ significantly from zero.

To evaluate the return on investment, we compute the present discounted value (PDV) of post-college earnings net of direct costs through ages 30 and 50. We extrapolate from our regression-based earnings measure using data on field- and selectivity-specific earnings trends. See Online Appendix Section 3 for a full description of the procedure. We estimate that the informational treatment raises the PDV of post-schooling earnings net of direct costs through age 30 by just under one million Chilean pesos, or USD $1,923. Because treatment does not affect expected degree length, the cost of earnings forgone while in school seems likely to be negligible. Multiplying by the 37,747 students in the matriculating respondent sample suggests an increase in the PDV of aggregate earnings net of the direct costs education of roughly USD $72 million at the loan interest rate of 2%. Because the disclosure intervention is scalable and inexpensive to implement, the return on investment could be quite large.

5.3 Demand Effects and Capacity Constraints

The potential for high returns to disclosure depends on how treatment shifts demand. If treatment raises demand for spots in programs where capacity constraints bind, the positive effects observed could be offset by losses for the students pushed out of these programs. Moreover, in the long run, a key goal of disclosure is to reward the supply of high-quality degree programs through demand-side pressure. To understand where this pressure falls requires a demand analysis.

Suppose 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.

(2) $$max_{j \in J_i} u_{ij} = X_j \beta_1 + X_{ij} \beta_2 + T_i E_j \pi_1 + T_i C_j \pi_2 + \epsilon_{ij}$$

In the absence of treatment T_i, utility u_{ij} for student i at degree j is 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 affects choice by changing the utility weights individuals place on degree-average log earnings E_j (the log of the monthly earnings component of the net value measure provided to students) or log annual tuition costs C_j. In the Online Appendix, we show how utility functions of this form arise in a setting where treatment affects choice by a) increasing the precision of students' beliefs about differences in earnings and cost outcomes across programs, and/or b) altering the utility weights placed on earnings or costs conditional on beliefs. The coefficients π_1 and π_2 reflect a combination of these channels.

The X_j consist of dummies for narrowly defined field of study (178 categories) and institution (141 categories), as well as institution-major specific log earnings, log annual tuition, mean test score for admitted students, and high-SES share for admitted students. To allow the utility of degree characteristics to vary across students, we include X_{ij}, which interact degree characteristics with student preferences as measured in our survey response data.[32] X_{ij} includes indicator variables for whether j's narrow field of study was i's Nth-ranked preference on the pre-survey, for N=1, 2, or 3. We include parallel indicators for each i's listed preferences over broadly defined areas (10 categories) and institutions. 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 top preferences. We include dummies interacting the broad area of i's first-choice degree program and the broad area of j, as well as 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. These capture substitution patterns across majors and selectivity levels.

Table 4 presents multinomial logit estimates of equation 3 in the pooled sample of students, as well as separate estimates for low- and high-SES students. The dependent variable is an indicator if individual *i* enrolled in degree *j*. The upper panel reports estimates of π_1 and π_2. Treatment raises the utility weights placed on earnings, but does not affect weights placed on costs. Effects on earnings are larger for low-SES and near-zero for high-SES students. Each of these findings is consistent with our analysis of experimental effects in the previous section.

The lower panel presents estimated coefficients for a selection of the X_{ij}. These estimates help scale treatment effects to preferences over other degree attributes. Students have a strong preference for their home region and for programs similar to their first-listed program. If one degree has earnings for past graduates that are 100 log points higher than another (roughly the average belief overestimate for students whose first choice programs are in the bottom ventile of the earnings distribution) treatment raises the relative preference for that program by an amount equal to less than 10% of the utility weight placed on a same region degree and less than 20% of the weight placed on a degree in the stated first-

[32] Mixed logit demand models typically draw from distributions of unobserved preferences for characteristics when measures of preferences are not available. We directly allow utility to shift for students with stated survey preferences for particular degree characteristics.

choice area. These results help explain why the effects of treatment are not larger: it is very hard for revelation of earnings and cost data to shift applicants across geographic regions or fields of study.

Survey data appear capture important preference heterogeneity. The estimated utility placed on degree attributes decreases with the rank of the listed choice. We report only coefficients by rank for broad area here, but similar patterns hold for other degree attributes such as narrowly-defined majors and institutions of their listed top choices. These results suggest simulations will capture realistic substitution patterns across programs in response to a scaled informational treatment.

We simulate the effects of a population-scaled informational intervention as follows. We use the by-SES 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 these weighted demand estimates we the change in enrollment in each degree program between the all-treated and none-treated scenarios.

Capacity constraints under hypothetical scaled treatment

Figure 6, Panel A shows the mean percentage change in enrollment by decile of the degree mean log earnings distribution in the full-treatment counterfactual. Earnings here are defined as in the informational treatment provided to students. Consistent with the reduced-form evidence in Figure 6, 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 just under 5% in the lowest-earning tenth of programs, while gains are distributed almost uniformly over the top 60% of the earnings distribution. These changes are driven mostly by low-SES students. Low-SES enrollment in bottom-decile degree programs falls by more than 6% in the treatment counterfactual, while top-decile enrollment rises by over 4%.

The lower panel of Figure 6 shows percentage change in enrollment by decile of the cost distribution. We do not observe a systematic pattern here. Taken together, simulations from structural estimates in Figure 6 echo the reduced-form evidence from Figure 6: treatment reduces demand for the degree programs with the worst labor market outcomes and particularly those that serve low-SES students, but increases demand slightly across a range of higher-performing degrees.

To measure capacity constraints from the treatment-driven demand shift, we compare our simulated changes in enrollment to slack capacity using data on degree-specific vacancies relative to total enrollment (reported each year to a centralized accreditation authority).[33] Figure 7 reports results. The left

[33] 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

panel presents the mean degree program admissions score 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 take place at less-selective degrees, while changes at more-selective programs tend to be smaller. That is, treatment shifts students between non-selective programs. These findings make sense because treatment effects are largest among low-SES and low-PSU students: students most likely to qualify for admission to non-selective programs only.

The right panel of Figure 7 presents mean admission slack (in percentage terms) by decile of enrollment change. The 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 4%, 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.

Psychological underpinnings of treatment effects

Estimating a model of demand allows us to examine the 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 student's choice. We test whether effects derive from the recommendation or from the more generalized information treatment. 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 advertise simply the names of higher return degrees rather than work to update student's earnings information and beliefs through disclosure.

We present our findings in Panel B of Table 4. 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. We cannot rule out that our recommendations play no role in student choice. We note that the general increase in the utility value of earnings induced by treatment may arise either because treatment

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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.

increases the precision of students' beliefs about earnings differences across degree programs (updating), or because it raises the utility weight students place on earnings conditional on some set of beliefs. The Online Appendix presents a utility model that makes this statement more precise. These two channels have similar implications for demand effects of treatment.[34]

6 Discussion and robustness

Using past earnings as disclosure

Earnings disclosure policies necessarily extrapolate future outcomes from past outcomes, and often in contexts where random assignment is not possible. This raises several issues.

First, estimated degree effects may not accurately capture causal effects for past students. In Section 5 of the Online Appendix, we compare our OLS measures of predicted earnings to regression discontinuity estimates similar to those in HNZ (2013). We find that, after adjusting for measurement error, observed differences in earnings outcomes across admissions thresholds are similar to those predicted by the OLS estimates for students' above- and below-threshold enrollment choices. Observed earnings discontinuities rise one-for-one with predicted discontinuities, and are close to zero at thresholds where OLS estimates predict a zero effect. These findings suggest that our measures of predicted earnings may succeed in capturing the causal effects of enrollment in different degree programs.

This is consistent with recent research in the context of teacher and school effects. Several papers in this literature find that value added estimates in many cases accurately capture differences in causal effects (Kane and Staiger 2009; Chetty, Friedman, and Rockoff 2014a, 2014b). The authors of these papers argue that selection of teachers based on student unobservables may be negligible given observed assignment policies. In the context of higher education, selection on unobservables may be small if students are uninformed about their own degree-specific pecuniary deviation from mean returns conditional on observables, or if they weight non-pecuniary factors heavily when choosing schools. Table 2 suggests that the former is true here, and additional evidence presented in Hastings et al. (2015) and discussed in Section 5.2.2 indicates the latter may be true as well.

Realized earnings outcomes may differ from predicted outcomes because students who respond to the informational treatment have skills that are a better or worse match for their chosen degree programs

[34] Because the implications for choice are similar, distinguishing between these two channels is difficult without access to data on posterior (post-treatment) beliefs. In particular, pre-treatment belief errors may be correlated with students' preferences for earnings and costs. This renders heterogeneous effects analysis by baseline belief error challenging to interpret.

than past students who chose those programs. 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 Table A.16, show 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. These results are consistent with our finding in Table 3 that treated and untreated students choose degree programs with near-identical graduation rates. They suggest that the switches in degree choice induced by informational treatment may not come at the cost of skill match.

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

Skill prices

It is also possible that treatment itself could induce skill price changes. This seems unlikely given the relatively small shifts in degree choices we observe. As shown in Table A.15, the distributions of broad field and of degree type are similar in the treatment and the control group. The two highest-earning fields in our data are science/technology and health. Treatment group students are 2.11% more likely to enroll in science/technology degrees and 1.98% less likely to enroll in health fields than control students. Treated students are 0.92% more likely to enroll in professional degrees (as opposed to technical degrees) than control students. Treatment does not cause students to flood the market in certain fields or degree levels. If skill prices are determined by supply of and demand for graduates at the field-degree type level, it would require very large elasticities of demand for the changes we observe to substantially reduce cross-field wage gaps, which are quite large. The gap in average predicted earnings between science/technology and the next highest non-health field, social science, is 35%, while the gap between earnings for students in professional as opposed to technical degrees is 79.1%.

Treatment Size and Timing

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

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

7 Conclusion

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

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

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

Conditional on entrance exam scores, treatment effects reduce the gap in default rates at degrees chosen by low- and high-SES students by roughly 70%. However, this gap is small, so effects on the overall average default rates at the degrees students choose are limited. The informational treatment appears to offer a high return on investment overall. Treatment raises the present discounted value of earnings net of direct costs for matriculating students through age 30 by a little under USD $2,000. Though this is only 3% of the mean present value of net earnings in the experiment sample, the treatment is very inexpensive and easy to reproduce and scale each year. If earnings value added estimates for past enrollees are a guide to those for current applicants, our treatment would raise aggregate earnings by USD

$72 million if applied to the full sample of respondents. This value far exceeds the costs of administering the treatment, even including one-time fixed costs.

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

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

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

	(1) Invited Sample	(2) Opened	(3) Consent	(4) Respondents	(5) Treated	(6) Treated & Searched
PSU Score (Ave. Lang & Math)	508.4	518.8	524.5	536.3	537.0	543.4
	(155,167)	(101,736)	(72,474)	(47,568)	(23,402)	(10,339)
Municipal High School	37.80%	37.00%	36.50%	33.10%	32.50%	32.40%
	(164,798)	(114,398)	(83,346)	(49,166)	(24,162)	(10,448)
Mother Some Tertiary Edu.	25.80%	26.80%	26.90%	29.80%	30.30%	29.50%
	(130,324)	(85,134)	(60,616)	(40,744)	(20,041)	(8,725)
Father Some Tertiary Edu.	27.20%	28.40%	28.60%	31.70%	32.00%	32.00%
	(126,082)	(82,449)	(58,722)	(39,511)	(19,439)	(8,452)
Low-SES School	43.70%	43.30%	43.20%	35.70%	34.50%	34.30%
	(153,706)	(105,441)	(76,476)	(46,444)	(22,680)	(9,891)
Ave. of Lang + Math SIMCE (Z-score)	0.326	0.414	0.465	0.568	0.581	0.635
	(123,937)	(79,504)	(56,255)	(38,625)	(18,981)	(8,282)
Female	55.40%	57.30%	58.20%	57.50%	56.50%	58.90%
	(164,786)	(114,265)	(83,215)	(49,166)	(24,162)	(10,448)
Delayed College Entrance	26.40%	36.40%	39.60%	24.50%	24.50%	26.50%
	(164,798)	(114,398)	(83,346)	(49,166)	(24,162)	(10,448)
Net Value 1st Choice Degree	--	--	--	734,948	736,867	769,452
	--	--	--	(40,806)	(20,048)	(8,683)
Potential Institution Gains	--	--	--	267,566	266,131	262,685
	--	--	--	(48,672)	(23,922)	(10,350)
Observed Earnings at Age 26	--	--	--	464,307	466,988	482,513
	--	--	--	(31,549)	(15,532)	(6,713)
Matriculation Rate	--	--	--	77.0%	77.2%	77.6%
	--	--	--	(49,166)	(24,162)	(10,448)
Total Observations	164,798	114,398	83,346	49,166	24,162	10,448

Notes: Calculations are based on survey responses linked to administrative data from the Chilean Ministry of Education (Mineduc). The number of observations for each calculation is in parentheses. The "Invited Sample" is all November 2012 FUAS Applicants for the 2013 school year for whom we had a valid email address to send our survey invitation. The "Opened" sample is the subset of our Invited Sample who opened the survey invitation email. The "Consent" sample is the subset of those who opened the email and also consented to complete the survey. The "Respondents" are those who consented to complete the survey, completed all 6 questions in the survey, and graduated high school between 2009-2012. The "Treated" are those who were randomly assigned to be treated with degree information upon completion of the survey. The "Treated & Searched" are those who were treated with information who also searched for alternative degrees after being shown information about their first choice degree and a suggested institution and degree. PSU scores are the most recent PSU scores on record for the student. The type of high school (municipal, private, voucher) is from the 2012 high-school (RBD) graduation (source: Mineduc). Mother and Father having some tertiary education is defined if the mother/father have any higher education, as reported by the student in the national standardized test, SIMCE. Low-SES is defined as coming from a high school (RBD) in one of the two highest poverty categories as defined by Mineduc. SIMCE scores are results from standardized high school test scores that were nationally administered to all students enrolled in the 10th grade in 2001, 2003, 2006, 2008, and 2010, normalized within each testing year. Delayed college represents those that were not directly coming from high school; those who graduated high school prior to 2012. Net-Value 1st Choice Degree is the Net-Value displayed in the experiment for the student's stated first-choice degree. Potential Gains from Switching Institution is the maximum gains in net-value that was displayed to treatment group if they chose a different institution in the same major as their stated first-choice degree.

Table 2. 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	498	824	344	1,197*	164	933
	(459)	(758)	(568)	(578)	(546)	(713)

Notes: Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 50 point bins of prior PSU scores. Regression results in the second panel combine extensive and intensive margins; values of the outcome variables are set to zero if the respondent didn't matriculate anywhere in 2013. The third and fourth panels report intensive margin effects, set to missing the outcome variable of interest if the respondent didn't matriculate to a higher education degree in 2013. Net Value, Earnings Gains, and Monthly Debt are the values for degrees as exhibited in our experiment. We have five years of experience earnings of graduates averaged on the degree level from the tax authority in Chile (SII). We then project earnings for years 6-15 using linear estimated growth rates. To calculate earnings gains we subtract off the earnings in the corresponding experience year for those that did not attend a higher education institution. We take the present-value of these earnings gains and convert it to a monthly amount. Total tuition was calculated using the 2012 tuition values for the reported length of the degree plus any associated matriculation fees. The total tuition for the degree was amortized over 15 years (180 months) to get the monthly debt. Net Value is the difference between the monthly earnings gains and monthly debt. The LR and SR Relative Returns are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to age 50 for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR Relative Returns calculate predicted earnings using the same methodology, out to age 30. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. 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 3. Impact of Treatment on Returns to Degree & Repayment Rates

	Pooled	Low-SES	High-SES	Low-PSU	High-PSU	Low-SES & Low-PSU
Students who matriculate to a degree in repayment sample						
Percent	0.580	0.606	0.561	0.636	0.527	0.643
Treatment Effect	-0.002	-0.004	-0.004	0.0107	-0.0121+	0.00689
	(0.005)	(0.009)	(0.007)	(0.008)	(0.007)	(0.011)
Degree on-time repayment rate						
Average	0.592	0.543	0.628	0.533	0.664	0.520
Average conditional on exam score	0.602	0.594	0.608	0.558	0.615	0.559
Treatment effect	0.002	0.010**	-0.003	0.00386	0.00166	0.0112**
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Degree default rate						
Average	0.305	0.350	0.272	0.361	0.237	0.372
Average conditional on exam score	0.292	0.299	0.288	0.333	0.277	0.335
Treatment Effect	-0.001	-0.008*	0.004	-0.00272	-1.83E-05	-0.0114**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
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)

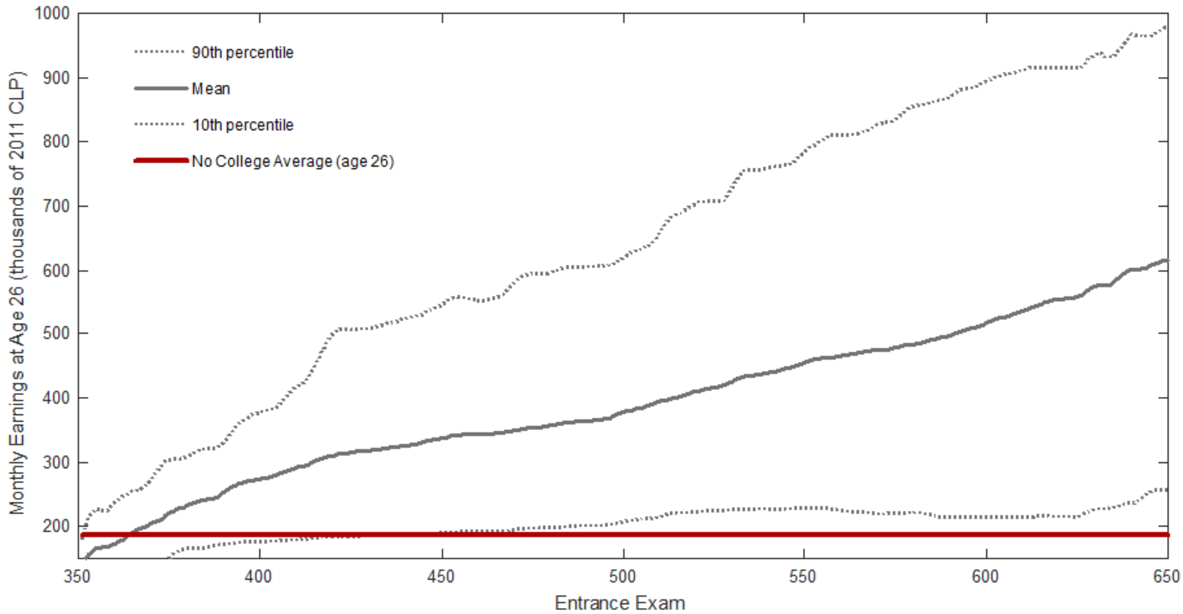
Notes: Table reports coefficients on Treatment from a regression of the dependent variable (row) on treatment, the dependent variable value for the survey response first choice for enrollment in Q2, and randomization blocks used to assign treatment. Clustered standard errors are in parentheses. For 2012 high school graduates, randomization blocks were assigned based on four characteristics: (1) school type (2) categories for distribution of 2010, 2011 senior PSU scores (3) 2012 school size (4) 2012 PSU registration rate. For 2009-2011 high school graduates, randomization was assigned based on 50 point bins of prior PSU scores. The LR and SR Relative Returns are predicted earnings gains conditional on enrollment (rather than only for graduates) estimates on the 2000-2005 freshmen cohorts. We estimate a flexible value-added model of earnings by degree enrollment as a function of field of study, selectivity tier of the degree, SES, PSU score, and gender along with a full set of interactions. We estimate fixed effects by degree (including adjustments for small samples). We use these regression estimates to predict expected earnings over 7 years of experience for each individual in our sample given their characteristics and the degree characteristics. We allow earnings to grow out to age 50 for long run estimates using estimated growth rates by field of study and selectivity tier of the degree. The SR Relative Returns calculate predicted earnings using the same methodology, out to age 30. All present-value calculations (PV) are calculated assuming 2% APR. Low-SES is defined as the lowest two income quintiles as defined by Mineduc; High-SES is the highest 3 income quintiles. Low-PSU is defined as being below the median PSU score in the experiment sample, High-PSU is above median. Degree on-time repayment rates and default rates are conditional on the degree having at least 10 students in repayment as of April 2013. + p <0.10, * p < 0.05, ** p < 0.01, *** p < 0.001.

Table 4. Impact of Treatment Using a Multinomial Logit

	Pooled	Low-SES	High-SES
Panel A. Main specification treatment effects			
Treat X Log(Earnings)	0.0897*	0.187**	0.035
	(0.054)	(0.088)	(0.068)
Treat X Log (Tuition)	-0.0315	-0.104	0.008
	(0.049)	(0.078)	(0.064)
Selected Coefficients			
In home region	2.126***	2.292***	2.047***
	(0.033)	(0.058)	(0.041)
First choice area	1.272***	1.244***	1.283***
	(0.033)	(0.053)	(0.042)
Second choice area	0.758***	0.745***	0.769***
	(0.039)	(0.064)	(0.049)
Third choice area	0.603***	0.651***	0.579***
	(0.042)	(0.066)	(0.054)
First choice narrow area	2.306***	2.354***	2.285***
	(0.032)	(0.057)	(0.038)
First choice institution	0.441***	0.488***	0.432***
	(0.048)	(0.079)	(0.060)
	13,112,133	4,518,344	8,593,789
Panel B. Adding salience effects			
Treat X Log(Earnings)	0.088	0.185**	0.033
	(0.054)	(0.088)	(0.068)
Treat X Log (Tuition)	-0.032	-0.107	0.008
	(0.049)	(0.079)	(0.064)
Treat X recommended career	0.124	0.191	0.091
	(0.155)	(0.264)	(0.192)

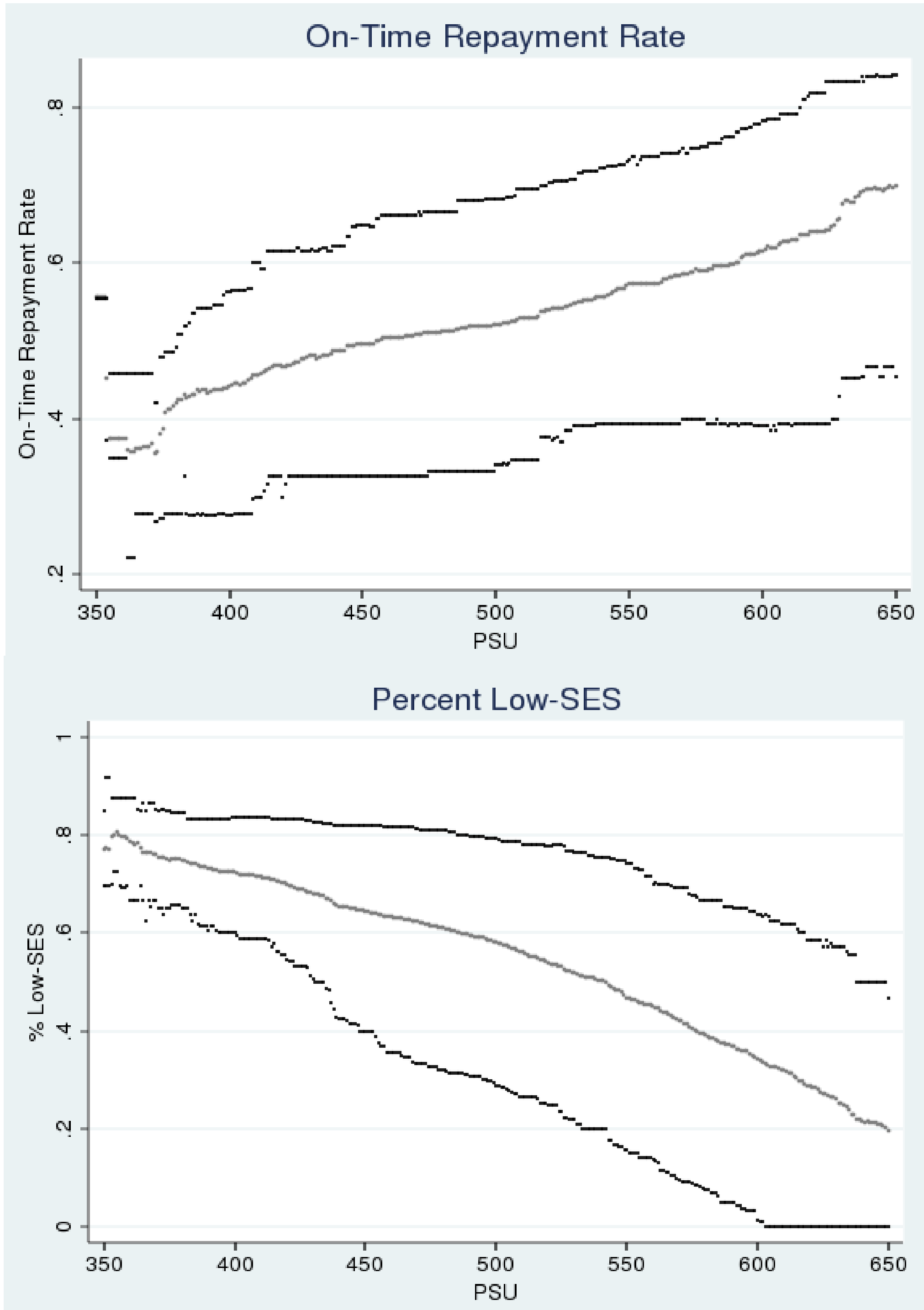
Notes: Multinomial logit estimates of equation 3. Clustered standard errors in parentheses. Dependent variable is an indicator for enrolling in a degree program. Samples given by column headers. Panel A: Treatment enters through interactions with log earnings and log tuition. "Treatment effects" subpanel shows these interactions. "Selected coefficients" panel shows estimates for a subset of control variables. Controls include narrow major and institution fixed effects as well as elicited preferences and preference intensity. See section 5.3 for a full list. Panel B adds an indicator for whether a degree was (would have been) recommended to students as part of treatment and the interaction between this variable and treatment.

Figure 1(a). Predicted Monthly Earnings (Age 26)



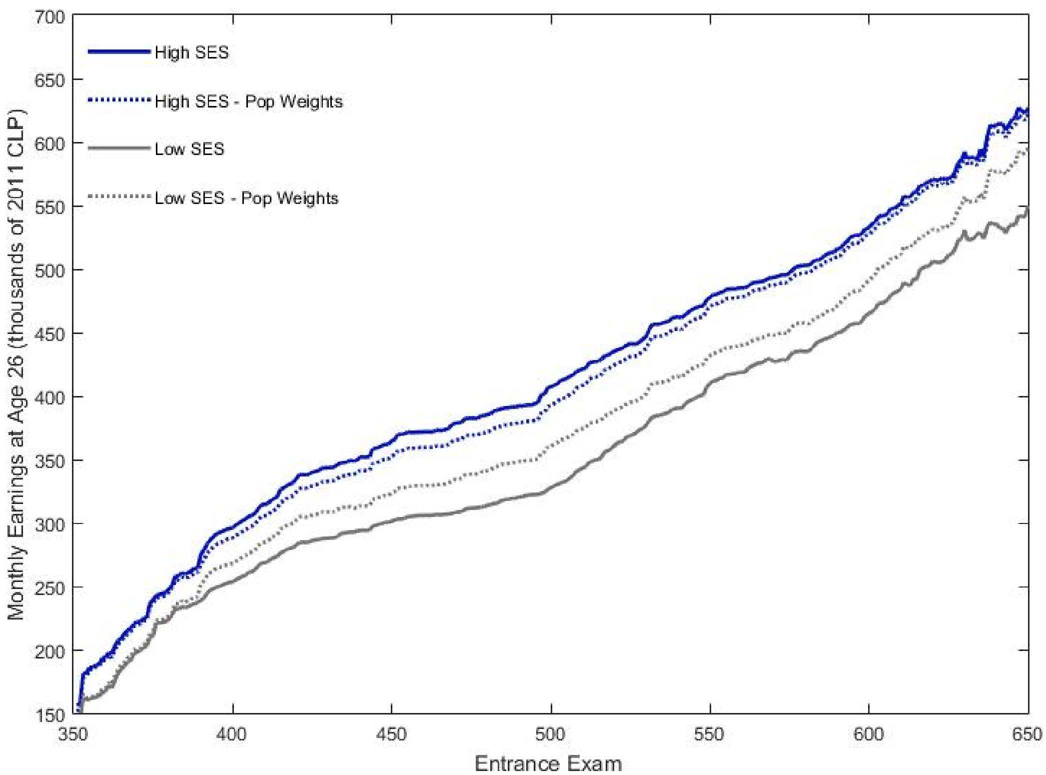
Notes: The figure shows the distribution of earnings that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted earnings for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of historic enrollment. We assume that a student scoring X's relevant degree choice set consists of the set of degrees for which his or her PSU score is in the 25th to 90th percentile of the historic range of admittees to that degree. The y axis value gives the enrollment-weighted mean expected earnings for students with a PSU of X over the degrees they could get into. The red line represents the average earnings at 26 years of age for those who graduated high-school, but did not enroll in a HEI.

Figure 1(b). Loan Repayment Rate & Socioeconomic Status



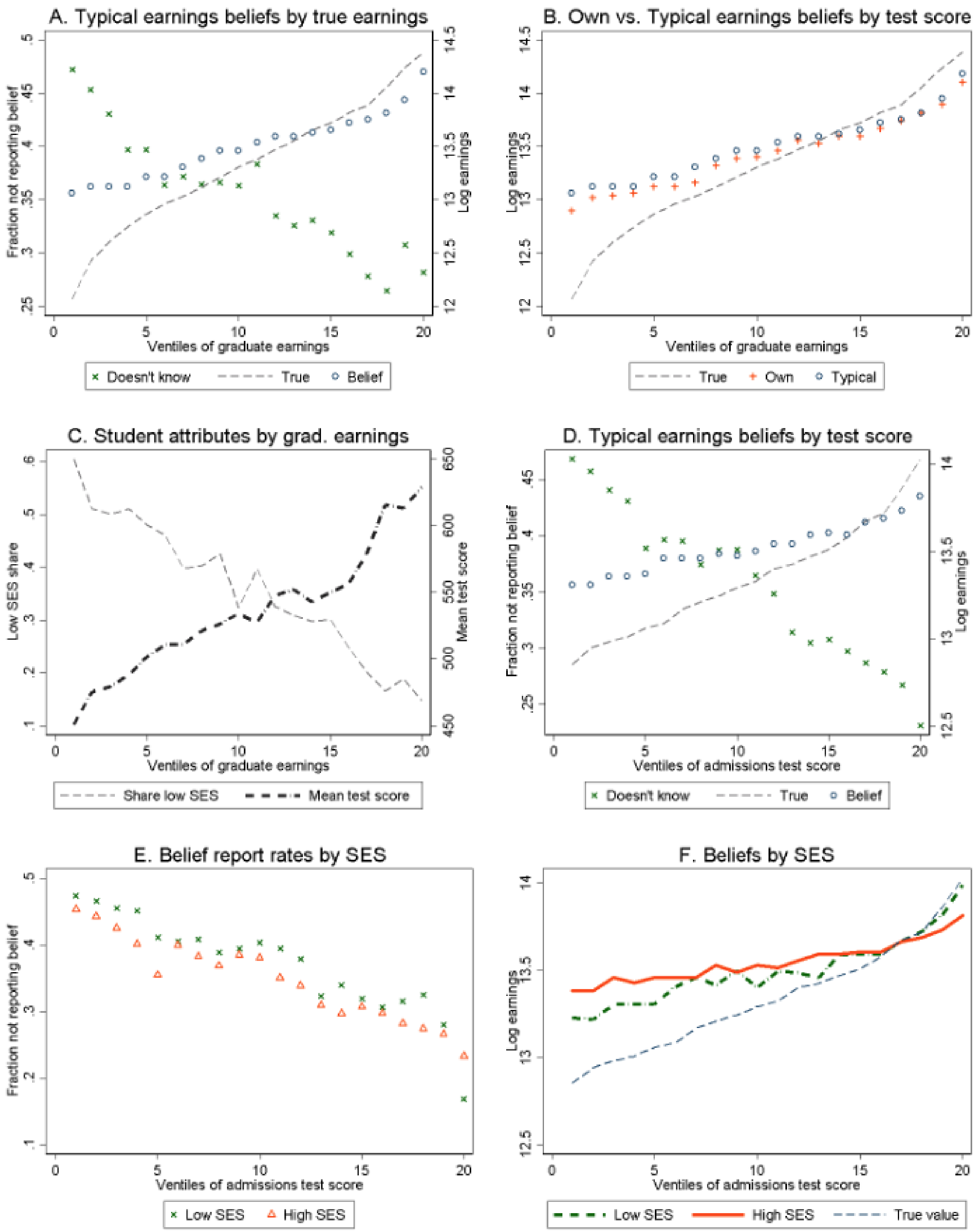
Notes: The figure shows the distribution of measures that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted earnings for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of historic enrollment. We assume that a student scoring X's relevant degree choice set consists of the set of degrees for which his or her PSU score is in the 25^{th} to 90^{th} percentile of the historic range of admittees to that degree. The y axis value gives the enrollment-weighted mean for students with a PSU of X over the degrees they could get into.

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



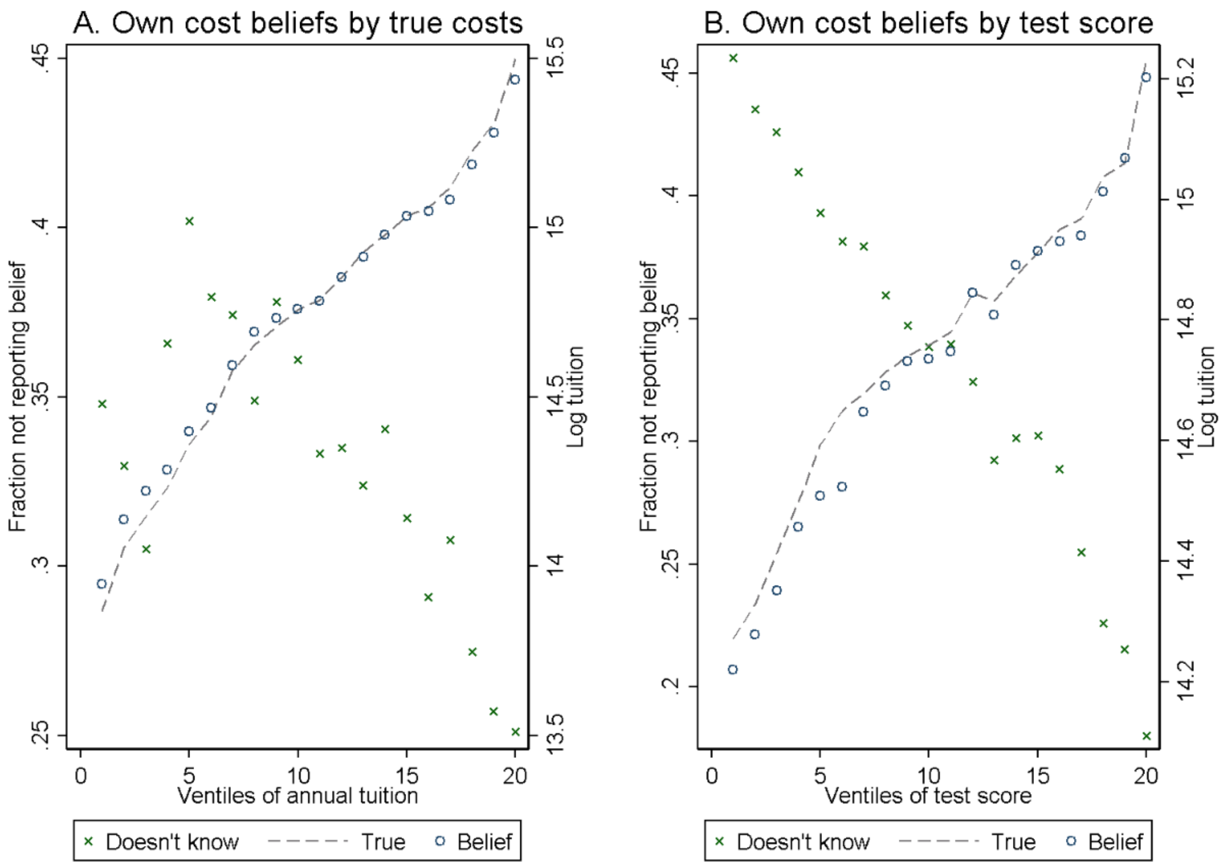
Notes: The figure shows the distribution of earnings that a student scoring X on their entrance exam realizes in expectation. We average within each score bin over predicted returns for observed 2007-2011 enrollment outcomes. To facilitate presentation, if a degree does not have sufficient student observations with PSU scores, we use the student's high school test scores to predict their PSU, and categorize the degree accordingly on the PSU admissions scale. This happens for 4.6% of degrees in low-selectivity regions representing 3.8% of historic enrollment. We assume that a student scoring X's relevant degree choice set consists of the set of degrees for which his or her PSU score is in the 25^{th} to 90^{th} percentile of the historic range of admittees to that degree.

Figure 3. Earnings Beliefs and Expectations by PSU score



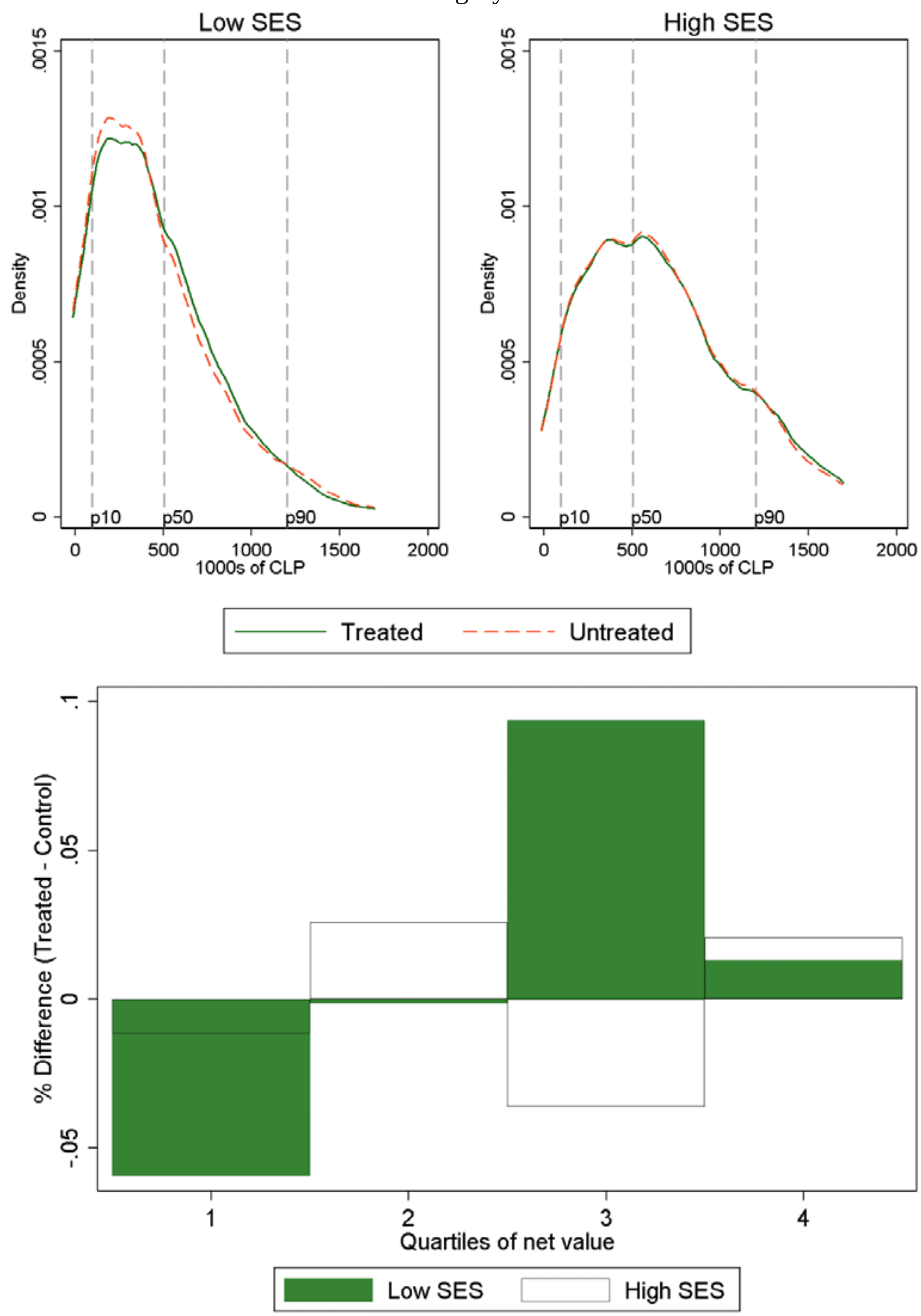
Notes: Panel A: Elicited beliefs about earnings for typical graduate by ventile of empirical graduate earnings distribution. Left axis: fraction claiming not to know typical earnings at first choice degree. Right axis: log of median belief and observed value within each ventile. Panel B: Median beliefs about own earnings and earnings for a typical graduate by ventile of empirical earnings distribution. "True" line displays median in each ventile. Panel C: Share of low-SES students (left axis) and mean admissions test score (right axis) by ventile of empirical graduate earnings distribution. Panel D: Share reporting belief about typical earnings at first choice program (left axis) and median belief about typical-student earnings and empirical median value (right axis) by ventile of test score distribution. Panel E: Rates of reporting own-earnings belief at first choice degree by ventile of admissions test score and student SES. Panel F: Median typical student earnings belief error by SES and ventile of admissions score distribution.

Figure 4. Costs Expectations



Notes: Panel A: Fraction not reporting cost belief (left axis) and median belief about annual tuition and true tuition value by ventile of empirical annual tuition distribution. Panel B: Fraction not reporting cost belief (left axis) and median belief about annual tuition and true tuition value by ventile of admissions test score distribution.

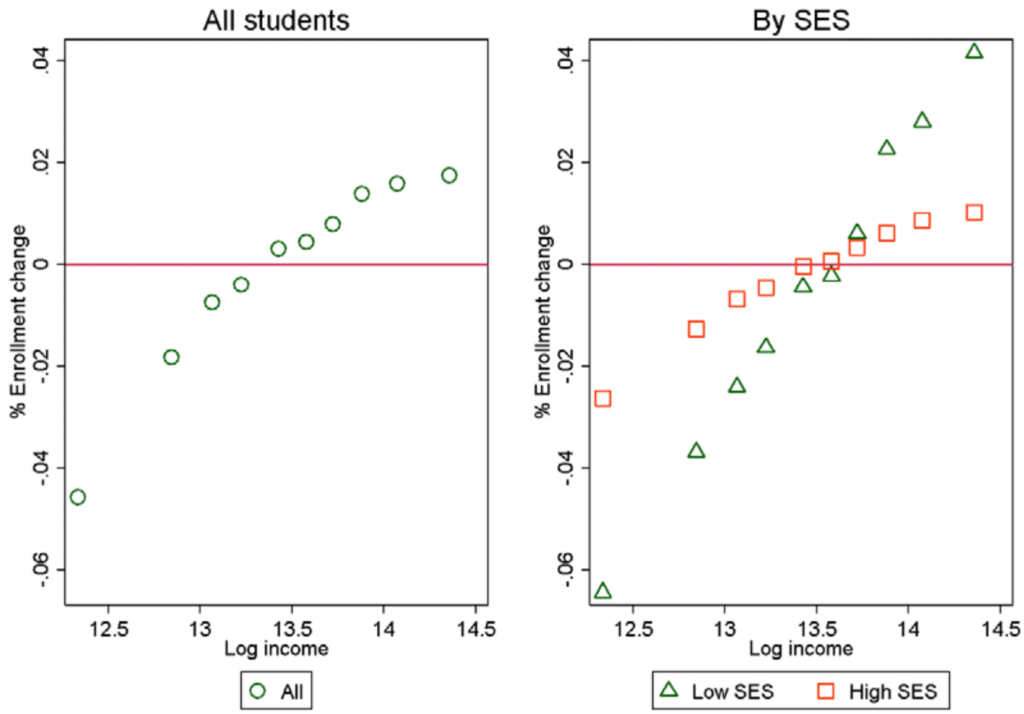
Figure 5. Density of Net Value in treatment and control groups by SES category



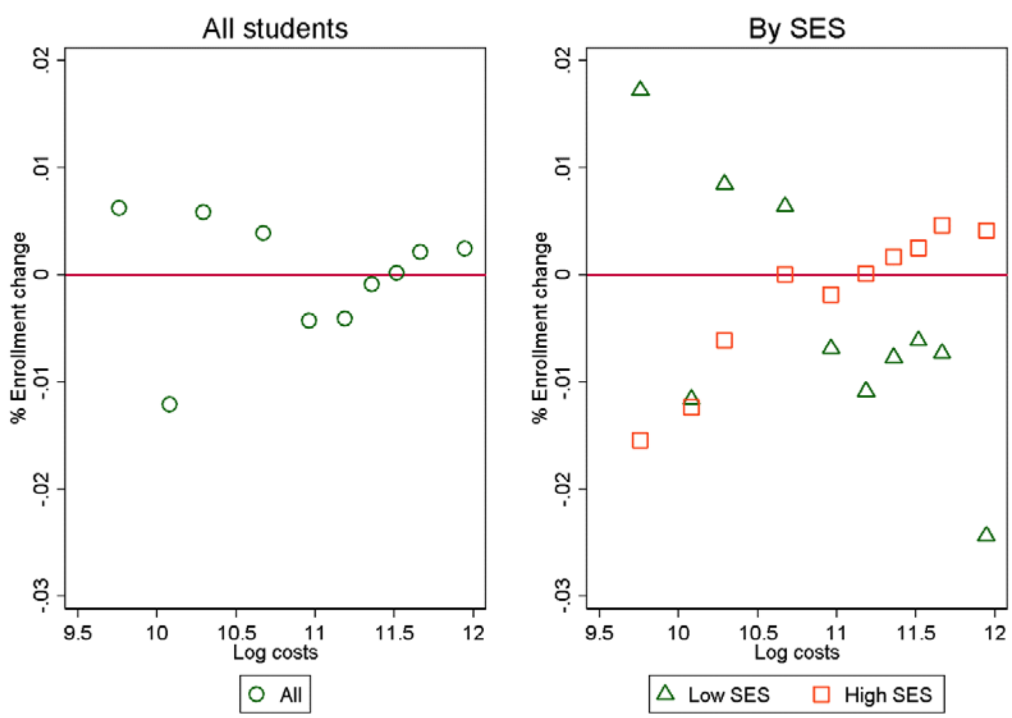
Notes: Epanechnikov kernel with bandwidth 100,000. Vertical lines denote percentiles of empirical distribution pooled over treatment status and SES background.

Figure 6. Changes in Degree Enrollment

B. Enrollment changes by degree mean income decile

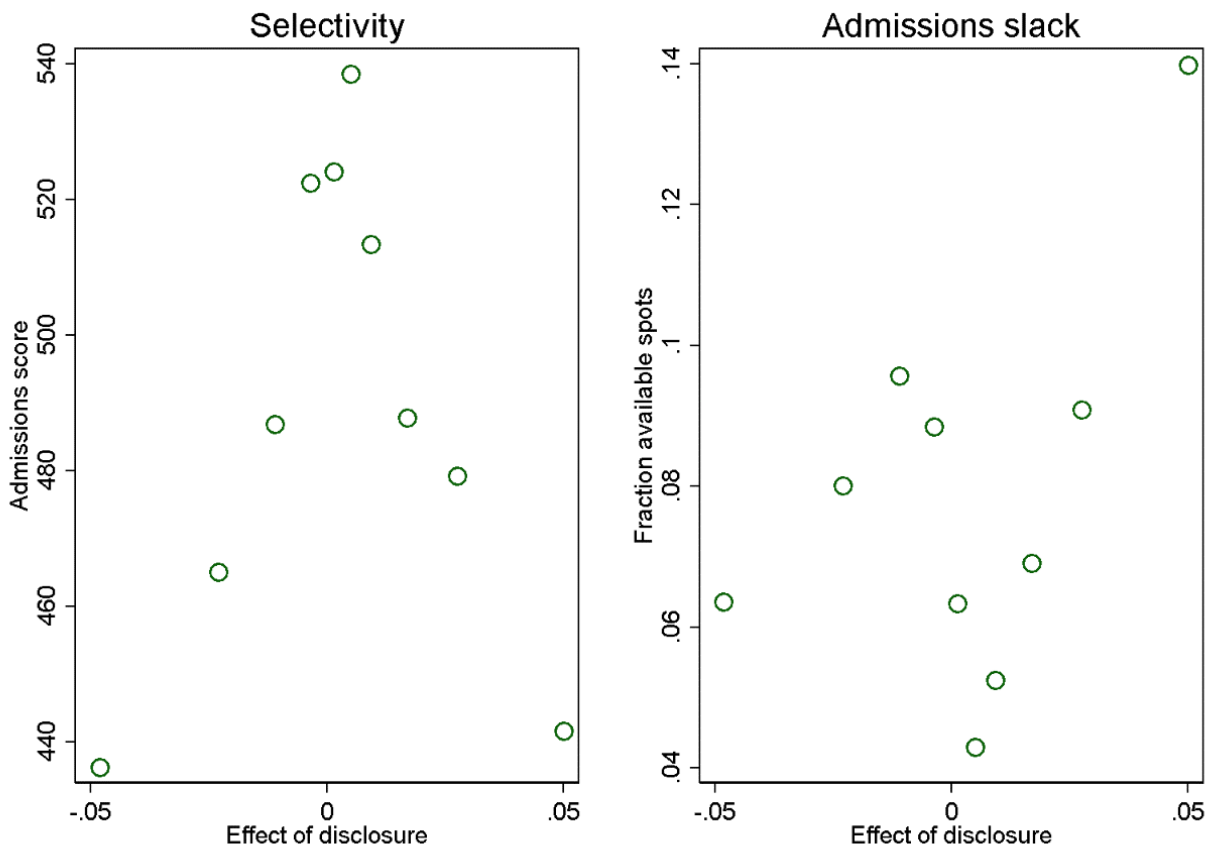


A. Enrollment changes by degree cost decile



Notes: Percent changes in enrollment for all-treated relative to none-treated counterfactuals. Degree-specific enrollment simulations based on estimates of equation 3. Estimates are reweighted to match the overall applicant population in terms of SES, gender, age, and admissions scores. See text for details. Upper panel: mean percentage change in enrollment by decile of the log of the monthly income component of Net Value. Left graph is for the population of applicants, right graph splits by SES. Lower panel: mean percentage change in enrollment by decile of log monthly cost component of net value. Left graph is for population of applicants, right graph splits by SES.

Figure 7. Characteristics of Degree Programs



Notes: Characteristics of degree programs (vertical axis) by decile of percent change in enrollment (horizontal axis). Effects based on degree-specific enrollment simulations using estimates of equation 3. Estimates are reweighted to match the overall applicant population in terms of SES, gender, age, and admissions scores. See text for details. Left panel shows mean admissions exam score for enrolling students on the vertical axis. Right panel shows the mean percentage of slack enrollment capacity on the vertical axis. Slack capacity computed using reported data on admissions vacancies. See text for details.