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MAKING THE ONE PERCENT:
THE ROLE OF ELITE UNIVERSITIES AND ELITE PEERS

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ABSTRACT

This paper asks whether elite universities help students from modest socioeconomic backgrounds reach top positions in the economy. I combine administrative data on income and leadership teams at publicly traded firms with a regression discontinuity design based on admissions rules at elite business-focused degree programs in Chile. The 1.8% of college students admitted to these programs account for 41% of leadership positions and 38% of top 0.1% incomes. Admission raises the number of leadership positions students hold by 54% and their probability of attaining a top 0.1% income by 45%. However, these gains accrue only to applicants from high-tuition private high schools, not to students from other school types with similar admissions test scores. Admissions effects are equal to roughly half of the gap in rates of top attainment by high school background. A difference-in-differences analysis of the rates at which pairs of students lead the same firms indicates that peer ties formed between college classmates from similar backgrounds play an important role in driving the observed effects.

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A online appendix is available at http://www.nber.org/data-appendix/w22900/July2017OnlineAppendix.pdf

1 Introduction

This paper asks whether elite universities help students from modest socioeconomic backgrounds reach top positions in the economy. This question is an important one for at least two reasons. First, it speaks to a longstanding debate over the determinants of upward mobility into top jobs and top incomes. The stakes of this debate for the allocation of income overall have grown over time as top income shares have risen.[1] Elite universities are an interesting causal channel to explore because their graduates go on to hold a large share of top positions. For example, Capelli and Hamori (2004) report that 10% of Fortune 100 executives in 2001 attended an Ivy League college, while Cohen et al. (2008) find that 10% of all publicly traded firms in the US have at least one senior manager from Harvard. What happens at elite universities may affect not just access to top jobs for individual students, but the composition of top positions overall.

Second, the economic mechanisms through which elite schooling affects advancement to top jobs may differ from those that determine the returns to education elsewhere in the distribution. In contrast to the usual focus on skill development or signaling (see e.g. Card 1999), descriptive accounts of elite universities often emphasize the importance of ties formed between school peers, and particularly peers from similar social backgorunds (Mills 1956, Kantor 2013). These accounts are consistent with findings that peers affect compensation for executives who are graduates of elite schools (Shue 2013), and with promotional materials from schools that highlight networking opportunities.[2] Recent evidence suggests that, on average, attending a selective institution flattens the relationship between socioeconomic status and income (Zimmerman 2014, Dale and Krueger 2014, Chetty et al. 2017). But if students from wealthy backgrounds are able to form more valuable ties with school peers, this may not hold for the small set of programs that produce a large share of top outcomes.

In this paper, I combine administrative and archival data from Chile with a regression discontinuity design to identify the causal effects of admission to elite business-focused degree programs and explore the role of peer ties as a mechanism underlying these effects. I link data on applications to elite degree programs dating back more than 40 years to administrative tax records as well as to records of executive managers and board members at publicly traded corporations, and use variation in admissions outcomes generated by score-based cutoff rules to identify the causal effects of admission. In addition to facilitating measurement of top outcomes and credible causal inference, the Chilean setting is an informative one because intergenerational income elasticities, top income shares, and measures of business transparency suggest opportunities for

[1] See, e.g., Miller (1949), Miller (1950) Urahn et al. (2012), or Rivera (2016) for a discussion of mobility to top positions, and Atkinson et al. (2011) or Alvaredo et al. (2013) for trends in top income shares.

[2] See e.g. University of Chicago Booth (2014) and Harvard Business School (2013).

upward mobility similar to those in many Latin American and EU member states.[3]

I have three main findings. First, a small number of highly selective, business-focused degree programs account for large shares of leadership positions and top incomes. Second, gaining admission to one of these programs rather than a less selective program raises mean income and rates of top attainment for students from wealthy backgrounds, but not for other students. Admission to the programs that disproportionately produce top outcomes thus widens gaps in top attainment by baseline socioeconomic status. Third, college classmates from wealthy backgrounds are more likely to lead the same firms than students in different programs or the same program at different times. This suggests that peer ties formed between classmates are one mechanism through which elite universities raise top attainment.

The analysis proceeds in three steps. I first present new descriptive evidence on the distribution of firm leadership and top income attainment by educational background and student characteristics. I use data on the population of college-bound high-school graduates[4] from 1980 through the present, and focus on two measures of top attainment: holding an executive management position or board seat at a publicly traded firm, or having an income in the top 0.1% of the observed income distribution. I measure these outcomes for students who are between 12 and 39 years removed from the year of college application, or roughly ages 30 through 57. This allows sufficient time for students to complete schooling and, by the end of the sample period, to reach their career peaks.

I find that the 1.8% of students admitted to three business-focused majors at the two most selective universities in Chile (henceforth 'elite degree programs') make up 41% of all directors and top managers, 38% of the top 0.1% of the income distribution between 2005 and 2013, and 45.9% of the top 0.01%. The gap between rates of leadership attainment at these elite degree programs and the average program is roughly similar to the gap observed in the US between Ivy League graduates and the average college graduate. Conditional on selectivity, major matters: students in top business-focused programs are 5.8 times more likely to have an income within the top 0.1% of the distribution than students in equally selective medical programs, where average incomes are similar. For students admitted to elite degree programs, family background is a critical determinant of leadership and top attainment. Taking attendance at a high-tuition private high school as a proxy for parental SES, I find that students from wealthy backgrounds hold 2.6 times more leadership positions and are 3.1 times more likely to have top 0.1% incomes than students from other backgrounds in the same degree program with similar test scores.

The second stage of the analysis uses a regression discontinuity design based on admissions cut-

[3]Nor are they obviously worse than in the US. See Section 2 for details.

[4]Specifically, the population students who take a required college admissions exam.

offs to measure the *causal* effects of elite degree programs on the rate at which students reach top positions. The discontinuity analysis takes advantage of the fact that students crossing the threshold for admission to elite degree programs are most often drawn from less selective programs in similar fields. This means that effects can be interpreted in terms of changing selectivity within career path, rather than, for example, switches from medical to business careers within top universities.

Admission to an elite degree program raises the number of leadership positions students hold by 54% and the probability of attaining an income in the top 0.1% of the distribution by 45% relative to the below-threshold mean. However, gains accrue only to applicants from private high schools. Effects for students not from private high schools are small and statistically indistinguishable from zero. A matching analysis (Angrist and Rokkanen 2015) provides evidence that effects observed at the cutoff apply to most admitted students, and can account for large shares of the gap in rates of top attainment by high school type for students at elite programs. In addition to raising rates of top attainment, admission to elite programs raises mean income by 12%. As with top outcomes, admission raises mean income only for students from private high schools.

Additional analyses help confirm that the observed effects are driven by changes in success within business-focused careers, not by changes in career aims. Regardless of family background, applicants to elite degree programs most often go on to work in business-oriented sectors like finance and trade. Admission to an elite program has little effect on sector mix. Analyses that condition on students' listed next choices confirm that changes in measured selectivity play a key role in driving labor market outcomes holding other degree attributes fixed. Finally, tests of interactions between admission and other student covariates confirm that private high school background is the key observable driver of heterogeneous admissions effects, not possible correlates such as subject-specific admissions test scores or geography.

My findings confirm that the correlation between elite college background and the attainment of top outcomes or leadership positions documented in previous studies in part reflects an underlying causal relationship (Sorokin 1925, Taussig and Joslyn 1932, Miller 1949, Miller 1950, Warner and Abegglen 1955, Useem and Karabel 1986, Temin 1997, Temin 1999, Cappelli and Hamori 2004). However, they contrast with evidence that access to higher education in general and more selective higher education in particular flattens SES gradients in income (Zimmerman 2014, Dale and Krueger 2014, Chetty et al. 2017). They also contrast with additional results showing that admission to selective medical degree programs in Chile– where average earnings are high, but rates of top attainment are low– raises earnings more for students from low-SES backgrounds. Put another way, the small number of degree programs that produce a disproportionate share of top outcomes appear to differ from other degree programs in their effects on outcome gradients by SES.

A possible explanation for this difference is that the mechanisms underlying labor market gains at elite, business-focused programs differ from those at other programs. One mechanism that may be particularly important at elite degree programs is peer relationships. Descriptive accounts of top programs often discuss the importance of peer ties, and in particular peer ties formed between students from similar backgrounds, as drivers of business success (Mills 1956, Kantor 2013),[5] and institutions themselves highlight peer networking opportunities in promotional materials (University of Chicago Booth 2014, Harvard Business School 2013).

The third stage of analysis investigates the role of peer ties as determinants of leadership outcomes. I use a difference-in-differences approach that compares the rates at which pairs of college classmates who attend the same degree program at the same time serve on management teams at the same firm to rates for pairs of students who attend the same degree program at different times or different degree programs at the same time. The intuition is that within a degree program, classmates in the same cohort are similar to pairs of students a few years apart in terms of pre-college backgrounds and institutional inputs, but are more likely to know each other and to have mutual contacts. The use of data on pairs of individuals to identify peer ties resembles Bayer, Ross, and Topa (2008), who use pairs data to study neighborhood effects.

I find that admission to an elite degree program increases the rate at which students from private high schools lead the same firms as college classmates also from private high school backgrounds, but does not alter their chances of leading the same firms as non-classmates or classmates from different backgrounds. This suggests that peer effects play a role in determining leadership attainment, and is difficult to reconcile with explanations based on general or firm-specific skill accumulation. That students form ties with peers from similar backgrounds is consistent with descriptions of peer group formation among college students in Marmaros and Sacerdote (2006) and Mayer and Puller (2008), and can help explain the finding of large gains in top outcomes for students from wealthy backgrounds and zero gains for other students. More generally, these results show that the peer relationships shown in previous studies to affect managerial behavior and compensation conditional on attaining a top position (Shue 2013, Fracassi and Tate 2012, Fracassi 2012) also affect rates at which students *obtain* management positions.

[5] For example, Mills (1956) argues that

> Harvard or Yale or Princeton is not enough. It is the really exclusive prep school that counts, for that determines which of the 'two Harvards' one attends. The clubs and cliques of college are usually composed of carry-overs of association and name made in the lower levels at the proper schools.

More recently, Kantor (2013) describes a student at Harvard Business School who 'was told by her classmates that she needed to spend more money to fully participate, and that 'the difference between a good experience and a great experience is only $20,000."

My findings extend a line of research suggesting that college ties affect early-career outcomes (Marmaros and Sacerdote 2002, Arcidiacono and Nicholson 2005) to long-run outcomes and high levels of occupational attainment.[6]

My findings on the role of elite universities in providing a pathway to top positions and the importance of peer effects as an underlying mechanism build on an extensive literature studying the effects of college selectivity on mean earnings. Dale and Krueger (2002), Black and Smith (2006), Hoekstra (2009), Saavedra (2009), and Öckert (2010) each provide evidence on the mean returns to increasing college selectivity.[7] The most closely related paper within this literature is Hastings, Neilson and Zimmerman (2013; henceforth HNZ), which uses discontinuous admissions rules in the population of Chilean degree programs to study the effects of admission to different kinds of degrees. HNZ develops a model of earnings determination and selection into degree programs, and identifies the model using variation generated by admissions discontinuities. The authors find that increasing selectivity raises mean earnings within some fields, including business, and also show that transitions from low- to high-earning fields lead to earnings gains. The contribution of the present paper relative to previous work is to describe the special role of a small number of degree programs in producing a large share of corporate leaders and top incomes, and to present evidence that peer ties are a mechanism in producing these outcomes.

The paper proceeds as follows. Section 2 describes labor market and educational institutions in Chile. Section 3 presents descriptive results on the educational backgrounds of business leaders and individuals with very high incomes. Section 4 presents the regression discontinuity analysis of the effect of admission on the attainment of top positions. Sections 5 and 6 consider the mechanisms underlying the observed effects. Section 7 concludes.

2 Institutional background and data collection

2.1 Inequality and mobility in international context

Chile is a middle-income OECD member country, with per capita GDP equal to about $22,000 after adjusting for purchasing power parity (values reported in constant 2014 USD except as

[6]My findings do not imply inefficiency in corporate management. See section 7 for a discussion. Arcidiacono and Nicholson (2005), de Giorgi et al. (2010), and Sacerdote (2001) and Oyer and Schaefer (2012) also explore the role of peers in career choices.

[7]In addition, Kaufmann et al. (2013) show that increasing selectivity may also have returns through the marriage market. Oyer and Schaefer (2009) and Arcidiacono et al. (2008) study applicants to law and business graduate programs, respectively, and find evidence of large earnings returns. Reyes et al. (2013) model selection into college for Chilean students and find evidence of positive but heterogeneous returns.

noted). This is the highest in Latin America and roughly comparable to Eastern European states such as Poland (World Bank 2016). As is true elsewhere in Latin America, inequality in Chile is high. Fairfield and Jorratt De Luis (2015) report estimates of top 1% income shares obtained from administrative tax records in 2005 that range from 15% to 23%. For comparison, similarly measured top 1% shares were 18% in the US, 20% in Colombia, and 22% in Argentina.[8]

Despite high levels of income inequality, rates of intergenerational mobility for the primarily urban population that applies to and attends the elite programs that are the focus of this study are comparable to those observed in many developed countries. 41% of Chileans and 80% of elite applicants live in the greater Santiago area.[9] Both elite universities are located in Santiago. Measured rates of intergenerational income mobility in this region are similar to those observed in the US or UK in methodologically comparable studies. Núñez and Miranda (2011) use an instrumental variables approach to estimate the intergenerational income elasticity (IGE) in greater Santiago at 0.52 to 0.54. Solon (1992) and Björklund and Jäntti (1997) conduct comparable exercises in US data, and obtain IGE estimates of roughly 0.5, while Dearden et al. (1997) find a value of 0.57 in the UK.[10]

Chile also has rates of educational attainment that are fairly similar to the US, and scores well on measures of public corruption. 38% of adults between 25 and 34 years old in 2010 had obtained a tertiary degree, compared to 42% in the US, 22% in Mexico, and 12% in Brazil (OECD 2012). And only 0.7% of businesses report 'informal payments' to government officials, compared to 18.1% in Argentina, 11.7% in Mexico, or 14.7% in Poland. See Table A-1 for a comparison of economic and business indicators in Chile to those for several other countries.

To summarize, the subjects of this study work in an economy with levels of inequality and rates of mobility similar to those observed in the US. Comparisons between Chile and Latin American countries like Colombia or Argentina seem reasonable. Comparisons to larger, higher-income countries like the US are less straightforward, but it is not obvious that urban Chileans face a more challenging environment for upward mobility than prevails in the US.

[8]These shares exclude accrued profits and capital gains to facilitate cross-national comparisons. The Argentina share is from 2004 and adjusts for evasion, while the Colombia share is from 2003. See Fairfield and Jorratt De Luis (2015) for a detailed discussion.

[9]Population statistics from 2014. Elite applicant statistics from 1982.

[10]US IGE values in the neighborhood of 0.5 are larger than those reported in, e.g., Lee and Solon (2009), where estimates take an average value of 0.44 across all cohorts. However, as Chetty et al. (2014) point out, IGE estimates are sensitive to methodological choices. Núñez and Miranda (2011) use a two-sample two-stage least squares approach that instruments for father's income using schooling and occupation. Solon (1992) shows that this approach can upwardly bias IGE estimates relative to alternative approaches, so it is important to contrast Núñez and Miranda (2011) with IV estimates from other countries. The findings from Solon (1992), Björklund and Jäntti (1997), and Dearden et al. (1997) cited here all instrument for father's income as in Núñez and Miranda (2011).

2.2 Higher education institutions and applications

Until the late 1990s, almost all college students in Chile attended one of 25 'traditional' universities (Rolando et al. 2010). These are known as CRUCH universities (an acronym for 'Council of Rectors of Universities of Chile') and include a mix of public and private institutions. The two most selective are the Universidad de Chile (UC) and Pontificia Universidad Católica de Chile (PUC). Both are world class institutions, ranking 13th and 7th, respectively, in the 2016 U.S. News Latin American university rankings (US News and World Report 2016). Within these institutions, some fields are more selective and/or more business-oriented than others. I focus much of my analysis on three highly selective, business-oriented fields of study: law, business, and an applied math degree known as 'Civil Engineering, Common Plan.'[11] I show in Section 3 that students admitted to these programs account for large shares of leadership positions and top incomes. These findings are consistent with reports from other sources. For example, a 2003 study conducted by an executive search firm using a sample of business owners and business executives found that 58.1 percent had degrees from one of these six institution-field combinations (Seminarium Penrhyn International, 2003; henceforth SPI).

CRUCH applicants commit to specific fields of study prior to matriculation. The application process works as follows. Following their final year of high school, students take a standardized admissions exam.[12] After receiving the results from this test, students apply to up to eight degree programs by sending a ranked list to a centralized application authority. Degree programs consist of institution-degree pairs; e.g., law at UC or engineering at PUC. Degree programs then rank students using an index of admissions test outcomes and grades, and students are allocated to degrees based on a deferred acceptance algorithm. Students are admitted to only the most preferred degree program for which they have qualifying rank. For instance, a student who is rejected from his first choice but admitted to his second choice will not be considered for admission at his third choice. Students near the cutoff for admission are placed on a waitlist, and both admissions and waitlist outcomes are published in the newspaper. This process is similar to the medical residency match in the US (Roth and Peranson 1999), but with public disclosure of outcomes. The regression discontinuity analysis amounts to a comparison of the students near the bottom of the published admissions lists to students near the top of the waitlists.[13]

[11] Many Chilean degree programs that do not resemble engineering to a US observer have the word 'engineering' in the title. For example, business degrees are titled 'Commercial Engineering.' The engineering program I consider here is identified in my data as the source of many business leaders, and is best thought of as a business-oriented applied math degree.

[12] Prior to 2003, this test was known as the Prueba de Aptitud Académica, or PAA. The test was updated in 2003 and renamed the Prueba de Selección Universitaria.

[13] Weights may differ by degree program, so this mechanism is not a serial dictatorship. One implication is that the approach to RD estimation in Abdulkadiroglu et al. (2017) does not apply directly.

Four features of this process are worth highlighting. First, students do not have access to ex post choice between accepted outcomes. To change institution-degree enrollment, they must wait a year, retake the admissions test, and reapply. Second, the scores required for admission vary from year to year depending on demand for institutions and careers and the number of spots universities allocate to each career. Students can at best construct imprecise guesses about cutoffs based on data from previous years. This is consistent with the imprecise control condition required for unbiased regression discontinuity estimation (Lee and Lemieux 2010). Third, elite programs typically admit a few more students than their target class size. This allows some students to turn down spots (e.g., for medical reasons or because they wish to reapply next year) without waitlist movement, and means that admissions cutoffs observed in the newspaper will bind.[14] Fourth, students enrolled in different degree programs at the same institution generally do not take courses together. Each program studied here has its own physical plant, separated by between several blocks and several miles from the others. For this reason, I think of peer effects as operating within degree programs, not institutions.

2.3 Secondary education institutions

The effects of elite admission may depend on students' family background. In the absence of data on parental income or education, I use high school type to divide students into coarse socioeconomic strata. I interpret high school type as a proxy for home, school, and community inputs. I leave for future work questions related to the causal effect of high school background on labor market outcomes, holding other inputs fixed.

I focus primarily on two types of high schools: private high schools and non-private high schools. The private high school category consists of what Hsieh and Urquiola (2006) refer to as 'unsubsidized' private schools: private institutions that do not receive public funding. They generally charge high tuition and serve upper-income households. The non-private category includes both municipal schools, which are locally-run schools similar to public schools in the US, and voucher schools, which may be run by private groups but receive public funding and do not charge tuition (Neilson 2013). Unsubsidized private high schools accounted for less than ten percent of high school enrollment in the 1980s and early 1990s (Hsieh and Urquiola 2006). I consider finer high school classifications in Section 5.4. Online Appendix B describes the collection of high school data and the classification of high school types in detail.

[14] See Appendix B for more details. This feature of the process also mitigates concerns raised by de Chaisemartin and Behaghel (2015) about how the endogeneity of the count of offers to takeup rates can affect estimation in some school choice settings.

2.4 Data collection

2.4.1 Application records

I use three types of data on the college application process. The first is data on all admissions test takers between 1980 and 2001. The second is data on admissions outcomes at all CRUCH degree programs for the years 1982 through 2001. Applicants are a subset of test takers. The third is data on admissions outcomes at elite law, engineering and business programs for the years 1974 through 2001. This means that by 2013 (the last year for which I observe outcome data), the oldest students in the application dataset are 39 years removed from college application, or roughly 57 years old. Applications data are digitized from hard copies of published application and waitlist announcements stored in the Biblioteca Nacional de Chile. Records include all admitted students as well as a list of marginal rejected students that is typically equal in length to the list of admits. Online Appendix B discusses data collection in more detail. Data on student preference rankings becomes available beginning in 2000. I discuss this data in section 5.3.

Administrative application records include high school identifiers. However, these identifiers vary across years, and mappings between identifiers and school names are not available in all years. To identify high school types, I first use students who apply to college in multiple years to link codes across years. I then use data on school type from 2000 to classify schools from earlier cohorts. This procedure will work if a) there are at least some multi-year applicants in each high school in each year and b) the set of high schools is stable over time. Match rates are higher in more recent application cohorts. For example, I match 79 percent of elite admissions between 1980 and 2001 (the years used for descriptive analysis of income data) and 60 percent of elite applications between 1974 and 1991 (the years used in the discontinuity analysis of leadership outcomes). If the procedure falsely categorizes high school types, it will bias estimates of differences between private and public high schools towards zero, away from findings of cross-type heterogeneity. See Online Appendix B for details of the matching procedure. Section 4.3 discusses balance in the match to high school data across the admissions threshold.

The link between application records outcome records relies on government-issued personal identifiers (known as the Rol Único Tributarios; abbreviated as RUTs). Beginning in 1989, these records were published in the newspaper alongside admissions outcomes. Both before and after 1989, DEMRE maintained records of RUTs in their administrative application records. I match 94% of applications over the 1974-2001 period to RUTs. Non-matches are due to illegible records in newspaper or archival data. My discussion in the main text focuses on applications that are successfully matched to RUTs. See Online Appendix B for a discussion of the match process and evidence that match rates are balanced across the admissions threshold.

2.4.2 Firm leadership records

Publicly traded companies in Chile are required to disclose the identities of top executives and board members to the Superintendicia de Valores y Seguros (SVS), the Chilean analogue to the Securities and Exchange Commission in the US. I obtain leadership data using a web scrape of the SVS website (SVS 2013). I conducted this scrape in March of 2013. The SVS website allows users to search historical filing records by date for each firm. I searched for all executive managers and directors who served between January 1st, 1975 and January 1st 2013. Most firms do not provide leadership records for the earlier part of this period. The median leader was hired in 2009. 92 percent of leaders were hired in 1998 or later.

I observe a total of 10,220 leadership positions, of which 2,522 are held by applicants to elite degree programs in 1974 or later. Of the 2,522, 1,543 are directorships and the remainder are C-suite roles. Applicants hold these positions at a total of 619 firms; there are many firms in which more than one applicant holds a top job. Students go on to lead a variety of companies, including multinationals that are among the largest companies in Latin America, and Latin American subsidiaries of US companies. The firms in this data span a wide variety of sectors and corporate parents, and include some of the largest companies in the world. See Online Appendix B for more detail on the companies represented and positions held.

2.4.3 Tax records

I match admissions test takers in the years 1980 and later to individual tax records at the Chilean tax authority in compliance with Chilean privacy laws. Because test-taking is a requirement for application, the group of admissions test takers includes all applicants to elite programs. Tax records include all labor earnings (reported to the tax authority by employers) as well as income from pensions, rents, taxable capital gains, dividends, and distributed profits. Income data are not topcoded. These income records omit business profits that are reinvested in firms, which may lead to underestimates of top income shares (Fairfield and Jorratt de Luis 2015). For workers employed in long-term contracts, records also contain basic employer characteristics such as sector. Data are available on an annual basis for the years 2005 through 2013. See Online Appendix B for more details on the tax data.

Using these records I construct a dataset of labor force participants that excludes individuals who are fewer than 12 years removed from the year of college application (roughly age 30) or who have total annual income below 50% of what one would earn from a full year's work at the monthly minimum wage (about USD $2,300 per year in 2014 dollars). The purpose is to focus the analysis on individuals who have completed their schooling and are at least marginally connected to the labor force. The primary dependent variable of interest in the income data is an indicator for presence in the top 0.1% of the year-specific income distribution. In 2013, the threshold for a top 0.1% income was roughly $340,000, and average income in the top 0.1% was about $550,000. Average income in the top 0.01% was about $1.4 million. See Online Appendix B.4 for income levels in other years and at other percentiles.

3 Which degree programs lead to top outcomes?

The rate at which students attain top outcomes varies dramatically by degree program. Though degrees where rates of top attainment are high have high average incomes, not all degrees with high average incomes have high rates of top attainment. Panel A of Figure 1 plots mean income for students admitted to each degree program in the admitted student sample on the vertical axis against the mean of math and reading test scores on the horizontal axis. Degrees in the three business-focused fields are denoted by markers of different shapes. The UC and PUC programs in these fields have solid fill, while non-elite programs in the same fields have hollow fill. Medical degree programs are denoted by hollow circles.

Two features of this figure are worth highlighting. First, business-focused degrees have fairly high average earnings relative to other programs at similar selectivity levels. Second, the business, engineering, and law programs at UC and PUC are the most selective and have the highest average earnings within their respective fields. Mean income and selectivity at these programs are among the highest at any program and similar to levels observed at programs that train medical doctors. Table 1 reports descriptive statistics that accompany this and subsequent panels of Figure 1. Mean income for students admitted to elite degree programs in my data is just over $79,000. Average math and reading test scores for elite admits are more than 100 points higher than the average for all admitted students, and 200 points higher than the average for all test takers.[15]

Though mean incomes are similar for students in elite business-focused programs and medical programs, the distribution of income within these programs is very different. Panel B of Figure 1 plots the density of income by percentile of the test-taker income distribution for students admitted to the business-focused programs at PUC and UC and for students admitted to the medical programs at the same universities. Densities for both degree types are fairly low below the 95th

[15]The standardized admissions exam is normed to have a mean of 500 and a standard deviation of 100 across all test takers in each subject in each year. Some students take the exam in multiple years, and in this table I report scores for the first time students take the exam. First-time takers score somewhat lower than test takers as a whole.

percentile. Students admitted to medical degrees are more likely to have income between the 95th and 99.5th percentiles. Above the 99.5th percentile, the density of the income distribution for medical admits falls to zero, while the density for business-focused students rises. Students admitted to the elite business focused programs have incomes in the top 0.1% of the overall distribution in 1.6% of of earnings years, compared to 0.3% for students admitted to medical degrees. Students from top business-focused programs account for a large share of top incomes overall. Though they make up only 1.8% of admissions test takers, they account for 38.0% of the top 0.1% of the distribution between 2005 and 2013, and 45.9% of the top 0.01%.

Patterns in leadership attainment are similar to those for top income. Panel C of Figure 1 plots the mean number of leadership positions held for admitted students at each program against program selectivity. Six of the top nine programs in terms of leadership production are in the elite, business-focused set, including the top four. Students admitted to elite programs attain top positions at a rate of 4.1 per one hundred students, compared to 0.2 per one hundred students in the population of applicants. Of the 3,664 leadership positions matched to admissions test takers between 1980 and 2001, 1,488, or 41%, were held by the 1.8% of students admitted to elite degree programs. Rates are close to zero for medical programs despite high average incomes.

The gap between the rate of leadership attainment at elite programs and the average program is roughly similar to the gap between Ivy League institutions and other institutions observed in the US. Consider the following back-of-the-envelope calculation. Capelli and Hamori (2004) report that 10% (14%) of Fortune 100 executive managers in 2001 (1980) were Ivy League graduates. Ivy League graduates accounted for 1.1% of bachelor's degree recepients and 1.8% of degree recepients at the bachelor's level or above in 1980, the earliest year for which data are available.[16] If we assume Ivy League graduate shares are roughly constant over time, this suggests that Ivy graduates held between 6 and 10 times as many executive positions as the average graduate in 2001, and between 8 and 12 times as many positions in 1980, depending on whether one uses the bachelor's or all-degree share. These figures aggregate across all majors and would likely rise if one focused on business-oriented fields. In Chile, students admitted to one of the six elite degree programs hold 4.1 positions per 100 students, compared to 0.4 positions per 100 students in the population of students admitted to some degree program. That is, students at elite programs hold about 10 times more positions than the average student admitted to college.

Within the elite business-focused degree programs, students from private high schools are much more likely to obtain top positions. I observe high school type data for 79 percent of students admitted to elite degree programs, of whom 60% attended private high school. Panel D of Figure 1 shows the density of the income distribution for students from private and non-private high

[16]Source: Author's calculations from IPEDS.

schools who are admitted to the elite business focused programs. The densities rise in parallel through roughly the 99.5th percentile. Above the 99.5th percentile, the density for students from private high school backgrounds rises, reaching roughly double its level at the 99.5th percentile by the very top of the income distribution. There is no similar uptick in the density for students not from private high school. Similarly, as reported in Table 1, students from private high schools hold 5.3 leadership positions per hundred students, compared to 1.6 positions per hundred for students not from private schools.

Differences in attainment by high school type within elite programs are not driven by differences in observable pre-college ability. Conditional on admission, students who attended private high school score slightly higher on their admissions exams than students who did not, but differences in the probability of attaining top outcomes by high school background persist after conditioning on admissions test score. Panels E and F of Figure 1 show, respectively, average counts of leadership positions and the share of admitted students with top 0.1% incomes by position in the test score distribution for admitted students at elite programs. Conditional on program, cohort, and admissions score decile, students from private high schools hold 2.6 times as many leadership positions and are 3.1 times as likely to have a top 0.1% income as students not from private schools.

The descriptive analysis thus far includes all applicant-year observations where the applicant is at least 12 years removed from college application, or approximately age 30. However, students generally do not reach top positions in the income distribution until mid-career. Figure 2 shows age profiles for log income and top 0.1% share by high school type for students admitted to elite degree programs. Ages are calculated based on years since application. The gap in log income between students from private and non-private high schools rises over the career, from roughly 37 log points at age 30 to 45 log points by age 40. Incomes in the top 0.1% are rare before applicants reach their late 30s. Rates of top attainment then rise sharply for students from private high school backgrounds, reaching 4.2% by age 40 and 5.0% by age 50. Increases are much smaller for students without private high school backgrounds.

I draw three conclusions from the descriptive analysis. First, the business, law, and engineering programs at UC and PUC stand out relative to programs in other fields and other programs in the same field for the high rates at which their students attain top positions in management and in the income distribution, and for the large share of these positions they hold. The concentration of top attainment within a small number of programs motivates an investigation of their causal effects. Second, within these programs, there are big differences in rates of top attainment by student background. This motivates an analysis of heterogeneous effects. Third, it takes most students until mid-career to attain top positions. The regression discontinuity analysis will focus on applicants at ages 40 and older, to allow time for applicants to advance in their careers to the point where top outcomes become feasible.

4 Regression discontinuity analysis

4.1 Estimation

I use a regression discontinuity design generated by admissions cutoffs to provide evidence on the causal effects of access to elite business-focused programs. The goal of the discontinuity analysis is to understand how access to an elite program rather than a next-choice option helps students attain top positions. Effects of this type are of interest for applicants to elite programs waiting for admissions outcomes. Further, given observed substitution patterns (described in detail below), they reflect the effects of increases in selectivity within business-focused career paths. This is consistent with the broader goal of understanding the effects of elite access, as opposed to other changes in degree attributes.[17]

Several treatment concepts are potentially of interest. One is admission to any elite degree program. This is of interest in a model where the only distinction between programs in the production of top attainment is 'elite' status. Another is admission to the most selective program in a given field. This is of interest in a model where it is attending the top program that matters. A third is the effect of marginal increases in program selectivity at the top of the selectivity distribution. This effect is of interest in model in which both the transition into the elite category and the transition up the selectivity ladder within elite programs affect rates of top attainment.

Patterns of substitution in the data allow me to construct estimates of each these effects. Within each field, the PUC program is more selective than the UC program. Regression discontinuity estimates that compare students near the bottom of the published admissions lists to students near the top of the waitlists at UC programs therefore capture the 'any elite' treatment concept, while estimates using PUC applications capture the 'most selective' concept. Section 4.4 describes substitution patterns at the UC and PUC admissions margins. I focus my analysis on the marginal increase in selectivity concept, which pools over UC and PUC applications. However, I also present estimates of UC- and PUC-specific effects.

I estimate two types of regression discontinuity specifications, both of which compare students

[17]Effects at other margins may also be of interest. For example, one might like to know what would happen to a student at an elite medical program who was randomly assigned to a top business program. Treatment effects of this type are difficult to estimate with available data given low rates of cross-field substitution in top programs. They also reflect less directly on the role of elite institutions. See HNZ and Kirkeboen et al. (2016) for studies of major choice.

near the bottom of the published admissions lists to students near the top of the waitlists. I use the first type to study leadership outcomes. These specifications are of the form

$$Y_{ipc} = f(d_{ipc}) + \Delta A_{ipc} + e_{ipc} \tag{1}$$

where Y_{ipc} is a leadership outcome for student i applying to program p in application cohort c. d_{ipc} is i's score on application p in cohort c relative to the cutoff score, and $f()$ is some smooth function. $A_{ipc} = 1[d_{ipc} \geq 0]$ is a dummy equal to one if i is admitted to p in cohort c. The primary outcome variable of interest is the count of C-suite and directorship positions that applicants hold. I also consider specifications where the dependent variable is a dummy for holding any such position, and specifications that separate by position type.

The second type of specification has the form

$$Y_{ipct} = f(d_{ipc}) + \Delta A_{ipc} + e_{ipct} \tag{2}$$

The second type is identical to the first except that the outcome variable has a panel component. I use specifications of this form to study outcomes observed in tax data, which vary across the 2005 through 2013 outcome years. The outcome I focus on is a dummy variable equal to one if an applicant has an income in the top 0.1% of the year-specific income distribution. I supplement this with an analysis of log income.

In both equations 1 and 2, the parameter of interest is Δ, which captures the effect of admission to an elite program for marginal applicants relative to their next-choice option, averaged across degree programs and, in equation 2, outcome years (Cattaneo et al. 2016). The interpretation of Δ depends on the distribution of students' next choices, as described above.

I estimate these specifications using data on students near the admissions threshold at elite degree programs. There is one cutoff for each program in each year. Following Pop-Eleches and Urquiola (2013), I summarize information from these cutoffs by 'stacking' data across all cutoffs. The use of stacked data means that some students may show up in the data more than once. For example, there are many students who are rejected from degrees at PUC but admitted to programs in the same field and UC. To account for this in inference, standard errors are clustered at the student level throughout. The presence of multiple observations per student becomes rare as I restrict bandwidth to a narrow window around the cutoff value.

I focus on two versions of equations 1 and 2. The first version, which I refer to as the 'BW=10' specification, is a simple mean comparison of outcomes for students within a 10 point score window on either side of the admissions threshold. This specification does not include slope terms. The second version, which I refer to as the 'BW=20' specification, includes students within a 20 point window on either side of the admissions threshold. It allows for separate linear terms in scores above and below the cutoff. Point estimates for top outcomes are very similar across the two specifications. The narrow bandwidth specification increases statistical power in some instances because of the restriction on slope terms. In the text, I will generally refer to point estimates from this specification.

Online Appendix C discusses the selection of optimal bandwidths and polynomial degrees in more detail. Cross-validation procedures show that the polynomial degree that maximizes out-of-sample fit is zero, even at relatively wide bandwidths. This is consistent with my focus on the BW=10 specification, and with the observation that the relationship between the running variable and the rate at which students attain top positions is weak aside from the jump at the threshold point. Online Appendix C also discusses optimal bandwidth selection and standard error calculation using the selection procedures from Calonico et al. (2014) and Calonico et al. (2016) (henceforth CCT and CCFT, respectively). The CCT approach computes MSE-optimal bandwidths and incorporates bias-correction terms that adjust confidence intervals to account for bandwidth size. Inference is not materially affected. Online Appendix C also considers Lee-Card (2010) standard errors that account for clustering by value of the running variable, and specifications that add control variables. Neither change affects my findings.

I supplement the sharp RD specifications with instrumental variables specifications in which peer private high school share at the admitted degree program is the endogenous regressor. It is likely that no single mediating variable satisfies the exclusion restriction for an unbiased IV estimate. With that in mind, the goal of the IV specification is to scale changes to an intuitive measure of selectivity. I follow a literature on selective secondary exam schools that scales changes in student outcomes with measures of peer attributes (Abdulkadiroğlu et al. 2014). I focus on peer private high school share rather than other possible mediators, such as changes in peer test scores, because peer private high school share is a stronger predictor of changes in labor market outcomes. I present this evidence in Section 5.3.

Because I observe the population of admissions outcomes for only a subset of years, I obtain IV estimates using a two-sample procedure in which I compute first stage effects within the subset of years for which peer characteristics at non-elite degree programs are observable and reduced form effects in the full set of years for which elite admissions outcomes are available. I compute standard errors using a bootstrap clustered at the student level.

4.2 Regression discontinuity sample

Table 2 shows mean values of student-specific covariates for a) the full applicant sample, b) the sample of male applicants, c) the marginal sample of male applicants within 20 points on either side of the admissions threshold, and d) the sample of marginal applicants for whom data on high school type is available. The upper panel describes application-outcome year level data for applicants between 1980 and 1991, while the lower panel describes application-level data for students applying between 1974 and 1991. I use the former sample in my analysis of top incomes and the latter in my analysis of leadership positions.[18]

Applicants in the 1980-1991 sample have high scores on math and reading exams, with averages of 742 and 658 points, respectively, on tests that are normed to have a mean of 500 and a standard deviation of 100 in the population. I successfully assign a high school type to 72% of records. Of this 72%, 48.5% attended private high schools. I observe 80.6% of applicants in the labor force sample, with an average earnings of $87,500 per year. 1.8% of observations fall in the top 0.1% of the population income distribution. 76% of applicants are male. Male applicants have similar test score and high school backgrounds to the overall sample, with higher rates of match to income data and a roughly 25% higher probability of having a top 0.1% income. The regression discontinuity analysis focuses on male students because labor force participation rates for women in Chile were low during formative early-career years for applicants old enough to be leading firms today.[19] I present results for the full sample and for women only in section 5.4. Marginal applicants have mean attributes similar to those in the full applicant sample.

Applicants over the 1974-1991 period are very similar to those in the 1980-1991 subsample in terms of gender and private high school share. Rates of successful match to high school records are lower prior to 1980. See Online Appendix B.3 for more details. 2.9% hold either a C-suite or directorship position. Because some applicants go on to hold more than one such position, the average count of positions is 0.049, or 4.9 per hundred students. 0.2% of applicants hold more than four such positions, with the maximum number of positions held being 16. These are applicants who hold seats on the boards of many companies. To limit the effects of outliers while still allowing for intensive margin effects, my discontinuity analysis topcodes the count of positions held at four. I report results using the raw leadership counts and an indicator for holding any leadership position in supplemental tables. Effect sizes and inference are not affected.

[18] Recall that tax data is available only for 1980 and later cohorts. Data on students' standardized math and reading scores are also available only for 1980 and later application years, although the composite indices used to determine admissions outcomes are available in all years.

[19] For example, the female labor participation rate in Chile in 1990 was 32%, compared to 56% in the US at that time (World Bank 2016).

4.3 Validating the discontinuity design

Regression discontinuity estimates are unbiased only if determinants of leadership outcomes are balanced across the threshold. I consider two tests of cross-threshold balance. The first is to look for a discontinuity in the density of scores at the cutoff point (McCrary 2008). If more ambitious students are able to manipulate their test scores so as to fall just above the cutoff, one would expect a discontinuously higher density of scores at that point. Figure 3 shows a histogram of scores relative to admissions cutoff value. There is no evidence of clumping above the threshold. 31% of applications are within 10 points of the cutoff, 57% are within 20 points, and 84% are within 40 points. Figure A-1 shows separate density plots for students who attended private high schools and those who did not, as well for applicants to PUC and UC degree programs. Densities are smooth across the cutoff in each subsample.

The second test of RD validity is to check the balance of predetermined covariates across the threshold. Panels A, B, and C of Figure 4 display binned means of an indicator variable for match to high school data, an indicator for private high school background conditional on match, and a linear, earnings-weighted index of application cohort and institution-major specific dummy variables, respectively. There is no evidence that these variables change discontinuously across the admissions threshold. Regression results reported in Panel A of Table 3 confirm this impression. None of the ten statistical tests reject the null hypothesis of no discontinuity at the 10 percent level. These include tests that subset on the private and non-private high school samples. Panel D of Figure 4 and Panel B of Table 3 considers the effect of elite admission on selection into the labor force sample. Elite admission does not affect labor force participation rates. This mitigates concerns related to selective outmigration.

4.4 Changes in admissions outcomes across the threshold

The interpretation of threshold-crossing estimates depends on the mix of degree programs to which students would otherwise be admitted. Counterfactual admissions outcomes may differ from the target program in terms of both selectivity and field of study. Figure 5 shows the effects of admission to an elite degree program on two measures of selectivity: peer mean combined math and reading scores and the share of degree program peers from private high schools. The left, center, and right panels show results for the pooled applicant sample, applicants to UC programs, and applicants to PUC programs, respectively.[20]

[20] Below-threshold measures of peer characteristics are calculated using data for the 91% of marginally rejected students who are admitted to some degree program in the same year. Most of the remaining students are eventually admitted to some degree program. Figure A-3 in the Online Appendix shows results where peer attributes are determined by the highest-scoring (or highest private high school share) program to which a student is ever admitted.

In the pooled sample, admission to the target degree program is associated with a 9.8 percentage point increase in the share of peers from private high schools, and a 22.6 point gain in peer mean scores. Both peer scores and the fraction of peers from private high schools are lower for students admitted to the less-selective UC degrees than PUC degrees. Students admitted to the UC programs see the share of peers from private high schools rise by 5.9 percentage points from a base of 37.9%, and average peer math scores rise by 22.4 points. Students admitted to the more selective PUC programs see the fraction of peers from private high schools rise by 21.5 percentage points from a baseline of 44.6% and peer math scores rise by 21.8 points.

The left two columns of Table 4 report these results (in the rows marked 'pooled') as well as results allowing heterogeneous threshold-crossing effects by private high school background. In addition to interactions with the threshold-crossing dummy, the heterogeneous effect specifications include the full set of interactions between private high school background and intercept and slope terms. These results are reported in the rows marked 'main effect' and 'private HS interaction.' Students from private high schools who are admitted to elite programs experience slightly smaller gains in peer attributes from elite admission.

The right three columns of Table 4 explore transitions across fields. Cells in these three columns display below-threshold mean values of dummy variables equal to one if a marginally rejected student is admitted to a degree program of the type listed in the column. These are the intercept terms in the regression discontinuity equation. I again present results from specifications that pool across high school types and specifications that allow for interactions between threshold-crossing and own high school type. Most rejected students are admitted to degree programs in the same or similar fields to the program they are targeting. In the pooled sample, 12.4% of marginally rejected students are admitted to the other elite degree program in the same field as their target, and 51.0% are admitted to a non-elite program in the same field. 72.0% of marginally rejected students are admitted to a program in one of the business-oriented fields (business, law, or technology).

Students rejected from the less selective UC programs almost always leave the elite set, while students rejected from PUC programs typically end up at the UC program in the same field. 60.6% of students marginally rejected from UC degree programs are admitted to a non-elite program in the same broad field as their target program. 56.1% of marginal rejected students at PUC are admitted to the same program at UC while another 16.9% are admitted to a program in the same field at another institution. Students from private high schools are more likely to have another elite program as their next option.

Admissions effects matter insofar as they predict attendance. Data on matriculation are unavail-

This data is available for 96.5% of marginal rejected students. Results are similar to those presented here.

able for cohorts used in the analysis of top outcomes, but they are available for more recent cohorts. I consider the matriculation effects of admission for these more recent students in Appendix Table A-2 and Figure A-2. Admission to an elite degree program raises the probability students matriculate at that program by more than 90 percentage points, with slightly larger effects for students not from private high schools. Effects on graduation are also slightly larger for students not from private high schools.

To summarize, admission to an elite degree program raises peer quality as measured by test scores and high school type. These gains accrue to successful applicants to both UC and PUC programs. Most students have a same-field degree as their next option, with applicants who do switch fields often moving between the business-oriented fields that are the subject of this study. Sections 5.2 and 5.3 provide additional evidence showing that admission to an elite program has minimal effects on the sectors in which students work and that changes in selectivity drive labor market outcomes. In what follows, I focus on specifications that pool across fields and across applications to PUC and UC. Section 5.4 discusses specifications that disaggregate by institution and field.

4.5 Effect of elite admission on leadership and top income attainment

I now consider the effect of admission to elite programs on the rate at which students attain top positions. Panel A of Figure 6 shows how the count of leadership positions students hold changes across the threshold, while Panel B shows the same graphs for top 0.1% income. In both panels, the left graph shows results for the pooled sample and the right graph splits by high school type. In the pooled sample, discontinuities at the admissions cutoff are clear for both outcomes. Elite admission raises the mean leadership positions students hold by 0.019, a 54% gain relative to the below-threshold base of 0.036. It raises the probability a student will have a top 0.1% income by 0.008, a 45% gain relative to the base of 0.018.

Table 5 displays regression estimates of threshold crossing effects in the BW=10 and BW=20 specifications (in the columns labeled 'TC'), as well as IV estimates that scale the threshold-crossing effects by changes in private high school share at the degree programs to which students are admitted. Threshold-crossing estimates are nearly identical in the two specifications. This is because the relationship between the running variable and top-end labor market outcomes is relatively weak in the neighborhood of the cutoff. I cannot reject the hypotheses that slope terms are equal to zero in the BW=20 specifications at conventional levels, and, as discussed in Online Appendix C, the inclusion of slope terms leads to worse out of sample prediction even at wide bandwidths. In the BW=10 specification, leadership and top income effects are statistically significant at the one percent level. In the BW=20 specification, these effects are significant at the five and ten percent levels, respectively. Instrumental variables estimates indicate that the count of leadership positions students hold rises by about 0.019 for every ten percentage point increase in private school share at the program to which they are admitted, while the share of students with incomes in the top 0.1% of the distribution rises by about 0.008.[21]

The effects of admission on leadership and top income probability vary with high school background. Figure 6 and the 'Private HS' and 'Non-private HS' rows of Table 5 display estimates of leadership and top income effects for students from private and non-private high school backgrounds. Admission raises the count of leadership positions students from private high schools hold by 0.032, equal to 54% of the below-threshold mean of 0.059. Effects for students not from private high school backgrounds are approximately zero. Similarly, admission raises the probability students from private schools have incomes in the top 0.1% of the distribution by 1.8 percentage points, a 56% gain relative to a base probability of 3.2 percent. Effects for students from non-private high schools are again close to zero.

There is some visual evidence of a downward slope in leadership outcomes for private high school students below the admissions threshold (right panel of Figure 6.A). This slope is not statistically significant (p=0.49), and results from changes in the distribution of application cohorts and target degree programs as distance from the cutoff grows. Within applicants applying to a given program in a given year, there is no evidence of a negative below-threshold slope. Online Appendix Figure A-4 presents an alternate version of the graph that residualizes outcomes on target-program by application cohort dummies before plotting. This eliminates the slope but does not affect the discontinuity. The slight negative slope in top incomes for admitted private high school students (bottom right panel) is also statistically insignificant (p=0.54). I present evidence that top income effects are stable over the distribution of admitted students in the next subsection.

In the BW=10 specification, leadership and top income effects for private high school students are statistically significant at the five percent and one percent levels, respectively. p-values from tests of the hypothesis that effects are equal for students from private and non-private high schools are just below 0.05. Instrumental variables estimates show similar patterns of heterogeneity. This makes sense given that first stage results by high school type reported in Table 4 are similar for the two groups. As with the pooled estimates, the BW=20 specification yields nearly identical but more noisily estimated effects.

[21] For first stage estimation, I set private high school shares for the 9% of marginally rejected students whose admissions outcomes I do not observe to the mean value for marginally rejected students who are not admitted to any elite program. This causes IV estimates to differ slightly from results obtained by dividing the reduced form coefficients by the threshold-crossing estimates for peer private high school share reported in Table 4. See Section 4.2 for more discussion of missing data.

Cross-threshold changes in log income follow similar patterns to those observed for top outcomes. As shown in Figure 7, log income jumps discontinuously at the admissions threshold in the full sample and in the sample of private high school applicants. Regression estimates reported in the rightmost two columns of Table 5 show that admission raises income by sixteen to seventeen percent for private high school students depending on specification. There is no observable discontinuity for applicants not from private high schools. Because the running variable has a stronger relationship with log income than with top income or leadership attainment, I observe a statistically significant effect for non-private school students in the BW=10 specification. This should be treated skeptically given the lack of visual evidence and the near-zero effect in the BW=20 specification.

Findings for log income help rule out the hypothesis that top-end gains are small for applicants not from private schools because their incomes are too low for top outcomes to be feasible even with large overall gains. Online Appendix Figure A-5 and Table A-3 explore the distributional effects of admission in more detail by estimating versions of equation 2 on indicator variables for presence in different parts of the income distribution. For students from private high schools, the largest increase in the density of the income distribution in both level and percentage terms is in the top 0.1% of the distribution. For students not from private high schools, changes are smaller across the distribution. Effects at the very top of the income distribution are close to zero in levels, and small in percentage terms despite the small denominator.

4.6 Interpreting effect sizes

The economic magnitudes of the discontinuity estimates are large. One way to quantify them is as fractions of mean rates attainment. Male students in the 1974-1991 cohorts admitted to elite degree programs hold an average of 0.083 leadership positions. The pooled-sample admissions effect of 0.019 from Table 5 is equal to 22% of this value. The equivalent calculation for top 0.1% incomes shows that admissions effects are equal to 29% of the admitted-student mean. Another way is in terms of gaps in rates of top attainment by high school background. Students from private high schools admitted to elite degree programs hold an average of 0.079 more leadership positions than other students admitted to the same degree program. The average admissions effect for private high school students of 0.032 is equal to 41% of this value. For top income attainment, the equivalent share is 60%.

A third approach is to consider how causal effects scale relative to the population of corporate leaders and top incomes. A simple back-of-the-envelope calculation is helpful here. Say that all students in elite degree programs were shifted into programs where peer private high school shares were 9.8 percentage points lower. This is the average change in peer private high-school share from threshold-crossing. Assume that effects estimated at admissions margins apply to all admitted students affected by this shift. As discussed above, students at the six programs hold 41% of all leadership positions, and a 9.8 percentage point decline leads to a 22% drop in rates of leadership attainment. The total effect is to reduce the count of leadership positions these students hold by 9% (22% of 41%) of the population of leaders. A similar calculation for top incomes yields a reduction of 11% (29% of 38%) of the total number of income observations above the 0.1% cutoff rule.

In short, the causal effects of elite admission at the margin are large enough to have first order implications for the population of business leaders in the country if they apply to inframarginal students as well. Is this assumption reasonable? To evaluate it, I follow Angrist and Rokkanen (2015) and use the regression discontinuity framework to validate a matching design. The intuition is that treatment status is fully determined by the value of the running variable. Knowledge of the treatment assignment process permits tests of the conditional independence assumption required for unbiased matching estimation. Specifically, evidence that the running variable is uncorrelated with outcomes conditional on some set of covariates x_i suggests that those x_i form the basis for a valid matching design.

I implement the matching design in the sample of private high school students using subscores of applicants' admissions exams as matching variables. Key findings are as follows, with full results reported in Online Appendix D. First, for private high school students, tests of the relationship between the running variable and residual outcomes indicate that the matching design can provide unbiased effect estimates through the bottom 60% of the admissions score distribution for admitted students. Students in this range account for over half of all leadership and top income positions in the sample. Second, the effects of admission on top outcomes are stable over this range and similar to the effect observed at the discontinuity. Third, extending the matching design to incorporate all admitted students suggests that effects rise for students at higher score levels. The matching design becomes less compelling for students with very high scores so I interpret these findings cautiously.

The matching exercise suggests that the effects of access to elite degree programs persist for students within a fairly wide band above the admissions cutoff. Note that even if one accepts an assumption of constant effects across the score distribution for admitted students, the back-of-the-envelope calculation should not be taken as a prediction about what would happen under a counterfactual policy generating the described shift in peer attributes. How competition for top spots would play out under alternative institutional arrangements is beyond the scope of the paper. Rather, the implication is that the effects of elite programs are large enough and apply to enough students to affect the composition of economic leaders in the aggregate.

5 Interpreting the effects of elite admission

5.1 Effects by correlates of high school background

A concern about the interpretion of the results from Table 5 as heterogeneous effects by SES is that private high school status may be correlated with other student attributes besides SES that drive differential returns to admission. This would be a concern even if I observed parental income directly. Table 6 explores this possibility. Panel A describes how observable characteristics differ by high school type in the sample of marginal students. For each high school type, it presents means of math test scores, reading test scores, and an indicator variable equal to one if a student's high school is located in Santiago. I choose these covariates because they are available in a consistent form in each application cohort beginning in 1980. The test score variables capture multiple dimensions of student ability that may differ by high school type. The geographic indicator is informative because it may capture, e.g., preferences over region that affect earnings outcomes through geographic variation in price levels.

Differences by high school type are small. Students from private high schools have math and verbal test scores that are 8.8 and 5.4 points higher than students from non-private high schools, respectively. These gaps fall to 4.4 and 3.4 points after conditioning on target program. Recall that each test section has a standard deviation of 100. The small size of these gaps indicates that marginal students from different backgrounds have similar levels of academic preparation, and helps rule out scenarios in which students from low-SES backgrounds benefit from lax standards on the high school grades component of their index score and arrive to college unprepared. Private high school students are 1.4 percentage points more likely to come from Santiago, on a base of 84%.

Panel B directly tests the effects of allowing heterogeneity on these dimensions on estimated effects by high school background. I estimate regression discontinuity specifications that allow for a main admissions effect and an interaction between admission and each variable listed in the columns. The specification also includes a constant term and controls for main effects of each column variable. I find no evidence of effect heterogeneity on dimensions besides high school type. Further, allowing for additional heterogeneity does not affect estimates by high school type. This holds for each outcome: leadership, top income, and log income. These findings are consistent with the hypothesis that SES drives effect heterogeneity by high school type.

5.2 Changes in sector of employment

As reported in Table 4, admission to elite degree programs is associated with increasing selectivity, as measured by peer scores or high school backgrounds. For a minority of students it is also associated with a change in field of study. Changes in rates of top outcome attainment may stem from either or both of these transitions. I explore this issue first by considering the effects of elite admission on students' career paths using data on the broad sector of the firms where students work. If changes in top attainment are driven by students switching from, say, careers in journalism to careers in finance, we would expect to observe changes in the allocation of students to sectors across the admissions threshold. Data on sector of employment are available for just over 70% of application-outcome year observations.[22] I create dummy variables corresponding to employment in different sectors and use them as dependent variables in Equation 2.

Table 7 presents estimates of threshold-crossing effects as well as below-threshold means (i.e., intercept terms) for selected sectors. Marginal rejected students are most likely to work in business-oriented sectors. Three sectors (the real estate, rental, or business activities sector, the wholesale and retail trade sector, and the finance sector) account for 45.4% of observations. These three sectors make up large shares of employment for both students from private high school backgrounds (47.2%) and non-private backgrounds (41.2%). Students from non-private high schools are more likely to work in education or public administration (22.6% of observations vs. 13.8% for students from private schools). Threshold-crossing has limited effects on sector mix. The largest effect is to reduce the fraction of students going into public administration by 2.8 percentage points. This effect is similar for students from private and non-private high school backgrounds. There are no statistically significant changes in the rates at which students choose careers in the finance sector, the trade sector, or the real estate/rental sector. These findings suggest that students applying to business-focused programs from both private- and non-private high school backgrounds tend to pursue business-oriented careers, but that students from private high schools do so more successfully.

5.3 Earnings outcomes by below-threshold admission outcome

I next investigate the relative importance of changes in selectivity and changes in major using data on student choice lists. Beginning in 2000, application records include full student choice lists in addition to data on realized admissions and waitlist outcomes. The choice lists allow me to compare outcomes for students based on differences between their target program and their next option. For example, I can look at the effects of admission to elite degree programs only

[22] These are students who have labor income from long-term contracts.

for students who would be admitted to a non-elite degree program in the same field if rejected from the target. The downside of these data is that students applying to college in 2000 or later have not yet reached their peak labor market years, so I cannot look directly at leadership and top income outcomes. Nevertheless, the exercise provides a useful complement to the finding that elite admission results in limited changes in sector of work over the long run. The broad approach resembles Kirkeboen et al. (2016), but with an emphasis on selectivity as opposed to field of study

My analysis focuses on students applying to college in between 2000 and 2003. I consider only student-year observations where students are at least 10 years removed from the year of application, or age 28. The data cover applicants to the six elite, business focused degree programs. Based on student choice lists and test scores, I simulate counterfactual admissions outcome that would occur for each student if he were rejected from his targeted degree program. I label this program the next option. Appendix E discusses these data in more detail.

I estimate specifications of the form

$$y_{ipct} = \Delta^0 A_{ipc} + f^0(d_{ipc}) + \sum_n X^n_{ipc} A_{ipc} \Delta^n + \sum_n X^n_{ipc} f^n(d_{ipc}) + e_{ipct} \tag{3}$$

As in Equation 2, y_{ipct} is a labor market outcome for individual i applying to program-cohort pair pc in outcome year t. d_{ipc} is an individual's score relative to the cutoff and A_{ipc} is an admissions dummy. Equation 3 differs from Equation 2 in that in addition to the main effect of admission Δ^0 and smooth function $f^0(d_{ipc})$, both admissions effects and smooth functions are allowed to vary with covariates X^n_{ipc}. The X^n_{ipc} capture differences between attributes of the target degree and the next option degree for each student. To understand these specifications, it is helpful to consider a simple case in which there is only one binary X^n_{ipc}– say, an indicator equal to one if the alternative degree is in a business-focused field. In this case estimating Equation 3 is numerically equivalent to estimating separate RD specifications in the two groups defined by the binary dummy. Δ^n is equal to the difference between effect estimates between the two groups.

In practice, I estimate specifications that include multiple X^n_{ipc}, as well as non-binary X^n_{ipc} such as the difference in peer scores and peer private high school share between the target and next option degree. Relative to standard RD estimation, these specifications impose the restrictions that a) the X^n_{ipc} interact with A_{ipc} and f^n in an additively separable way, and b) that continuous variables interact linearly with the A_{ipc} and f^n. These restrictions allow for tractable estimation of heterogeneity along several characteristics simultaneously. The separability restriction is similar to Kirkeboen et al. (2016). I take log income as the outcome of interest, and focus on the BW=20 specification because, as discussed above, the running variable is predictive of log income. See Online Appendix E for results from the BW=10 specification, as well as balance tests, discontinuity graphs, and sample descriptions.

Table 8 presents results. Panel A presents estimates without interaction terms. Patterns are qualitatively similar to the long-run outcomes presented in section 4.5, with larger effects for students from private than non-private high schools. Sample sizes are smaller, and effects are more noisily estimated. Panel B presents estimates that include a main effect term as well as interactions between admission and a) an indicator equal to one if the next degree program is not in the business, engineering, or law fields, b) the gap in peer mean math scores at the target versus the next option field, c) the gap between share of private high school students at the target versus next option program, and d) a dummy variable equal to one if the next option is also an elite program. Peer score gaps and private high school share gaps are demeaned, so that the main effect captures the effect of admission to an elite program for a student whose next option is a non-elite program in a business area with mean test scores roughly 25 points below the target degree and peer private high school share 13 percentage points below the share at the target program.

Key findings are as follows. First, increases in the fraction of students from private high schools are strongly associated with earnings gains, holding other factors constant. The earnings gains associated with increases in peer private high school share are larger for students from private high school backgrounds. In contrast, there is little evidence that students who move to degree programs with higher peer scores experience earnings gains. Second, students who would otherwise be admitted to degrees in non-business areas may realize larger gains than those coming from business areas, but this difference is noisily estimated. At minimum, it is clear that gains are not limited to students transitioning from business to non-business fields. Finally, students admitted to their more preferred elite degree program who would otherwise attend another elite program realize small earnings losses in the short run.

Panel C of Table 8 displays earnings effects by terciles of the cross-threshold change in peer private high school share for private high school students. This confirms that students for whom peer private high school share rises (those in the upper two terciles) experience earnings gains, while students for whom peer private high school share stays constant or falls (those in the lower tercile) experience earnings losses in an RD setting that does not impose the separability or linearity assumptions in Equation 3. The takeaway from this analysis is that increased degree program selectivity within business-focused fields is a key driver of the observed labor market effects, with peer SES background a much stronger correlate of earnings gains than peer scores.

5.4 Heterogeneous effects by program characteristic, student background, and outcome type

Online Appendix F considers heterogeneous effects by institution, major, disaggregated student background, alternate measures of top outcomes. Key points are as follows. First, patterns of leadership and log income effects are similar for the PUC and UC programs, while admission to PUC programs has larger effects on top incomes. The effects observed for UC programs correspond to the 'any elite program' treatment concept described in Section 4.1, while the effects for PUC programs correspond to the 'top program in field' treatment concept. I cannot in general reject equality of institution-specific estimates. This is consistent with the observation that gains in peer attributes at UC and PUC programs are similar, and motivates a focus on pooled effect estimates.

Second, top income and leadership effects are larger for law and business degrees than for engineering degrees, with similar log income effects across the three fields. Third, I do not see evidence that admission raises rates of leadership attainment, top income attainment, or log income for women. Fourth, admission increases the rates at which students attain both C-suite and directorship positions, as well as their chances of holding any leadership position. Effects estimated using non-topcoded counts of leadership positions are nearly identical to those reported above. Fifth, I consider the possibility that admission to elite degree programs leads to top career attainment in less remunerative sectors. For example, admission to elite degree programs might help students from non-private high schools reach top jobs in government or education. I find no evidence of such effects.

Sixth, top income, leadership, and log income effects are larger for students from one of seven elite private high schools than from other private high schools, while effects for students from the most selective public exam high school in Chile are zero. The elite category consists of seven historically prestigious schools, distinguished by high prices, high test scores, and selective admissions. The selective public school has similar test scores to the elite private high schools, but is free to attend. These findings suggest that differences in academic rigor at the high school level do not drive my findings.[23]

Seventh, and finally, I show students from non-private high schools who are admitted to elite *medical* degree programs realize larger income gains than students from private high schools. This contrasts with the finding for business-focused programs. The takeaway point is that the degrees whose students regularly attain top positions in the economy offer low returns for poorer students, in contrast to other programs with similar average earnings. The finding that some pro-

[23]See Online Appendix B for a detailed description of high school type definitions.

grams reduce gaps by SES is consistent with descriptive results from Chetty et al. (2017).

6 Peer ties and leadership outcomes

The analysis above shows that admission to an elite degree program raises the probability of high income and occupational attainment, but only for students from private high schools, which I interpret as a proxy for high-SES background. Broadly speaking, there are two ways to explain these findings. The first is complementarity between a private high school background and non-peer institutional inputs such as coursework, faculty interaction, or signaling effects at top degree programs. For example, reaching a leadership position could require both skills learned at elite colleges and skills that parents of private high school students teach their children at a young age.

The second explanation is that gains in leadership positions for students from private high schools are driven by the people they meet in college. Students at elite universities may have ties to businesses through which they can refer college peers. Alternatively, school peers may be more productive if they work together, and working with peers may incentivize good performance at work. Students from private high school backgrounds could benefit disproportionately if they are better able to form valuable ties with their college peers.

This section separates the effects of peer ties from those of other institutional characteristics by looking at co-leadership rates, which I define as the probability that both members of a pair of students have leadership roles at the same firm. I compare co-leadership rates for students who were college peers (i.e., who attended the same degree program at the same time) to co-leadership rates for pairs who attended the same degree program at different times, or who attended a different degree program at the same time. The intuition is that within a degree program, same-cohort pairs are similar to pairs of students a few years apart in terms of pre-college backgrounds and institutional inputs, but that students in same-cohort pairs are more likely to know each other and to have mutual contacts. If students obtain jobs through contacts, or if peers are more productive when working together, college peers may be more likely to serve on leadership teams at the same firms than other pairs of similar students. If management hiring depends only on non-peer institutional inputs, there would be little reason to expect such a result. The the pairs-based empirical strategy is most closely related to Bayer, Ross, and Topa (2008), who explore how the probability two individuals work at the same address depends on how close together they live.

Panel A of Figure 8 presents co-leadership rates by cohort distance for students in the same degree program and students in the other elite degree program in the same field. The sample includes only pairs of students where both members of the pair are from private high schools (though not necessarily the same private high school). I present separate figures for all private high school students (left graph) and for the subset of private school students from elite private schools (right panel). Recall from Section 5.4 that leadership gains across the admissions threshold were largest for students from elite private high schools. Co-leadership rates are expressed on a per-100,000 pairs basis. One way to think about the group means is as the number of co-leaders who would emerge from a group of about 317 ($\approx \sqrt{100000}$) students. This is roughly the size of the admitted cohort in the engineering program at PUC during the period studied here. In both panels, co-leadership rates for students in the same degree program are relatively flat by cohort distance for students one or more year apart, but much higher for students who are peers in the same program in the same cohort. In contrast, there is no obvious pattern in co-leadership rates by cohort distance for students in different programs in the same field. This indicates that the elevated rates for peers are not driven by a propensity for non-peer students in the same cohort and same field to lead the same firms over the long run.

Panel B of Figure 8 displays co-leadership rates for pairs of students from private high schools by the position of the applicant relative to the admissions cutoff. I focus on the way admission changes applicants' co-leadership outcomes with three types of students: admitted students at the target degree program (e.g., the law program at PUC) in the same application cohort (i.e., students who will be applicants' college peers), admitted students at the target degree program separated by at least one year, and students from different degree programs in same field and same admission cohort. In contrast to the regression discontinuity graphs presented above, Panel B of Figure 8 displays binned means within a 40 point window on each side of the admissions threshold, and uses a wider bin width. Rates of co-leadership are relatively low compared to overall leadership rates,[24] reducing statistical power. I therefore focus this discussion on a) broad comparisons of means above and below the threshold, and b) global polynomial specifications that allow for separate slopes above and below the threshold rather than a local regression discontinuity analysis.

Students who are not admitted to their target program are similarly likely to co-lead firms with students accepted to the same cohort in their target program, with students accepted to other cohorts in that program, and with students from nearby cohorts in other same-field degree programs. Students admitted to their target program become roughly three times as likely to co-lead firms with their college peers, but are no more likely to co-lead firms with other types of admit-

[24] 428 male students from private high schools admitted to elite degree programs between 1974 and 1991 held at least one leadership position. Of these, 258 (60%) held a positition at the same firm as another private school student from the same field, and 195 (46%) with a private school student from the same program, and 78 (18%) with a private school student from the same program and at most one cohort apart.

ted students. For example, a student admitted to PUC Law class of 1983 is much more likely than a student rejected from that degree program to serve on the same corporate board as another PUC Law student from the class of 1983. However, he is no more likely than the rejected student to serve on the same corporate board as a student from the PUC Law class of 1980, or from the UC Law class of 1983. Statistical tests reported in Table A-4 reject the hypothesis that the observed gain in co-leadership for same degree, same cohort pairs is equal to zero, but fail to reject the hypothesis that co-leadership gains for other pair types are zero.

These results are consistent with a story in which ties between peers from private high schools play an important role in driving the increase in leadership hiring associated with admission to an elite degree program. It is natural to ask whether students from non-private high schools also benefit from peer ties at elite degree programs. The finding of no causal effect of admission in the regression discontinuity analysis suggests they may not. To explore this question I estimate single-difference specifications of the form

$$Y_{ij} = \alpha + \sum_g C_g(t_i, t_j)\pi_g + e_{ij} \tag{4}$$

within the sample of applicant pairs admitted to the same degree program, and difference-in-differences specifications of the form

$$Y_{ij} = \alpha + \beta S(p_i, p_j) + \sum_g C_g(t_i, t_j)\gamma_g + \sum_g S(p_i, p_j)C_g(t_i, t_j)\pi_g + e_{ij} \tag{5}$$

in the sample of applicant pairs admitted to same-field elite degree programs. Y_{ij} is a dummy equal to one if i and j hold leadership positions in the same firm. Denoting the application year for applicant i as t_i and the program where i is admitted as p_i, $C_g(t_i, t_j)$ is an indicator function equal to one if $|t_i - t_j| = g$, while $S(p_i, p_j)$ is an indicator equal to one if $p_i = p_j$. The coefficients of interest in both equations are the π_g. High values of π_0 relative to estimates for larger g indicate higher rates of co-leadership for college peers relative to non-peers. Equation 4 compares co-leadership rates for college peers relative to non-peers admitted to the same program, while equation 5 uses both same program and same field, different program students as controls. In both specifications, g takes values between zero and three, with values of g greater than or equal to four pooled into a single omitted category. For students from private high schools, estimates of Equation 4 are equivalent to comparing the same-program co-leadership rates in Panel A of Figure 8 to the value for $g = 4$, while estimates of Equation 5 can be obtained by differencing out the other-program effects and then making the same comparison.

As is standard in difference-in-difference analyses, the key assumption underlying the interpretation of π_0 as a causal effect of peer status on co-leadership outcomes is that firm-program effects and firm-cohort effects are additively separable; i.e., that there are not differential changes in the skill match between degree programs and firms over time. In practice, it is difficult to conclusively rule out violations of separability. That co-leadership rates are elevated only for students exposed to each other in the classroom helps alleviate this concern, as does the finding of a discontinuous break in co-leadership rates for admitted students.

Table 9 presents results. Panel A presents estimates of the π_g from Equation 4 and Panel B from Equation 5. The first two columns present estimates for pairs of students in which, respectively, both members of a pair are from private schools, and both members of a pair are from elite private schools. Effects are expressed relative to the omitted category of a four or more year cohort gap. As expected we see elevated rates of co-leadership only for students who are peers in the same cohort. Effect estimates are statistically significant at at least the 10% level in each specification. The single-difference specification indicates that students who are peers in the same cohort in the same program are 126% more likely to lead the same firm than students separated by four or more cohorts. For students from elite private high schools only, the effect estimate in percentage terms is 137%. Difference-in-difference estimates are slightly larger than single-difference estimates.

The third column in Table 9 shows effects for pairs of elite admits where one member is from a private high school and another member is not. Overall rates of co-leadership are lower for such pairs. Estimated peer effects are small and statistically insignificant. The fourth column of Table 9 shows estimates for pairs of students where neither member has a private high school background. Estimated peer effects are small here as well. The estimated effect at $g = 0$ in the difference-in-differences specification is significant at the 10 percent level. I interpret this effect cautiously because it is not replicated in the single difference specification. As shown in Online Appendix G, there is no visual evidence of a spike in co-leadership rates for peers in either the mixed-school type or non-private school samples.

The analysis of co-leadership rates shows that a) students from private high school backgrounds who are admitted to elite degree programs become more likely to lead firms with their same-background college peers, but not with same-background non-peers, and b) that students not from private high schools are no more likely to lead the same firms as their peers regardless of background. These findings parallel results from sections 4 and 5 showing that leadership gains from elite admission accrue only to students from private high school backgrounds. Both sets of results are consistent with the idea that ties to college peers play an important role in determining top outcomes. In contrast, it is difficult to reconcile the finding that there are no changes in co-leadership rates with non-peers with stories where peer ties play no role in hiring.

Online Appendix H formalizes this intuition using a simple model of leadership hiring in which hiring depends on student skills and referrals from school peers. The model maps gains in co-leadership rates with non-peers to skill effects, and differentially large gains in co-leadership rates with peers to peer effects. It provides the basis for a decomposition of the total effect of admission into a 'skill' component and a 'peers' component using the co-leadership estimates presented here. Results from this exercise suggest that peer ties can account for essentially all of the observed effect within at least one model of leadership hiring.

In supplementary analyses, I consider heterogeneity in co-leadership outcomes underlying the main estimates. Both baseline co-leadership rates and the effects of college peer status are large for students who attended the same private high school, though they are also present for students who attended different private high schools. This suggests that one factor underlying the increase in co-leadership rates associated with admission (visible in Panel B of Figure 8) is an increasing value of ties between students who may have already known each other in high school. Increases in rates of co-leadership are driven by pairs that include at least one directorship position. I also consider alternate clustering strategies for pairwise inference, and find results similar to those presented here. I present these findings in Online Appendix G.

7 Discussion

This paper asks whether elite colleges provide a pathway for talented students to positions at the top of the income distribution and leadership roles at major firms. I combine novel data on applicants to elite business, law, and engineering programs in Chile with a regression discontinuity design that exploits score-based admissions cutoffs. I link these records to data on top managers and directors at all publicly traded firms in Chile and to administrative tax records. Descriptive results show that students admitted to elite degree programs account for 41% of leadership positions and 38% of the top 0.1% of the income distribution for college admissions test takers aged 30 and over, despite making up just 1.8% of this population. Admission to an elite degree program raises the number of leadership positions students hold by 50%, and their probability of attaining income in the top 0.1% of the distribution by 45%. However, the gains accrue only to students who attended expensive private high schools. Students who attend other types of high schools, including elite public schools, do not realize any gains in top outcomes from elite college admission. Effects are the result of increased success in business-focused careers, not shifts to business from other sectors.

The composition of leadership teams at particular firms suggests that ties formed between college peers from private high schools are an important mechanism underlying the observed effects. Difference-in-difference estimates suggest that being peers in an elite degree program raises the rate at which pairs of private school students lead the same firms by 126% of the baseline co-leadership rate. Students who are not from private high schools are no more likely to co-lead firms with college peers who attended private high schools than they are with students from different degree programs or different cohorts. These findings are hard to reconcile with mechanisms based on general or firm-specific human capital accumulation.

As in the US, elite colleges in Chile view identifying and developing talented students from non-elite backgrounds as an important part of their mission. See Agosin (2012) and Lewis (1997) for examples of mission statements from UChile and Harvard. My findings suggest that these efforts have not been fully successful at the programs that produce a large share of top outcomes. This may be because students from poorer backgrounds have difficulty forming the kinds of social ties with richer classmates that facilitate access to top positions. Whether efforts to increase social integration at the secondary or postsecondary level can raise career attainment for lower-income students at elite degree programs is a topic for future work.

Implications for the efficiency of corporate management are ambiguous. In the context of informational frictions, signals conveyed through peer ties formed at elite colleges between pairs of students from wealthy backgrounds may help firms make better hires for top positions. Peer references would then increase efficiency, although possibly less so than if they were available for all students. Peer ties may also affect managerial productivity directly. Recent papers suggest that peer connections among managers could either reduce efficiency by encouraging lax oversight or inefficient compensation decisions (Fracassi and Tate 2012; Shue 2013), or increase efficiency by facilitating the flow of information within and across firms or taking advantage of peer complementarities in production (Cohen et al. 2008; Oyer and Schaefer 2012). Peer ties may also mitigate agency problems associated with the transfer of firm control, as described in Caselli and Gennaioli (2005, 2013). Network inputs into manager performance could offset any welfare losses created by informational frictions in hiring.

These findings also relate to the large body of research arguing that competition and turnover amongst elites is a key driver of economic growth over the long run (see e.g. Acemoglu and Robinson 2000, 2006, 2008, 2012). The failure of elite universities to facilitate upward mobility to top positions may be a cause or a consequence of elite entrenchment. The relationship between the determinants of mobility to the top and economic growth is a topic for future research.

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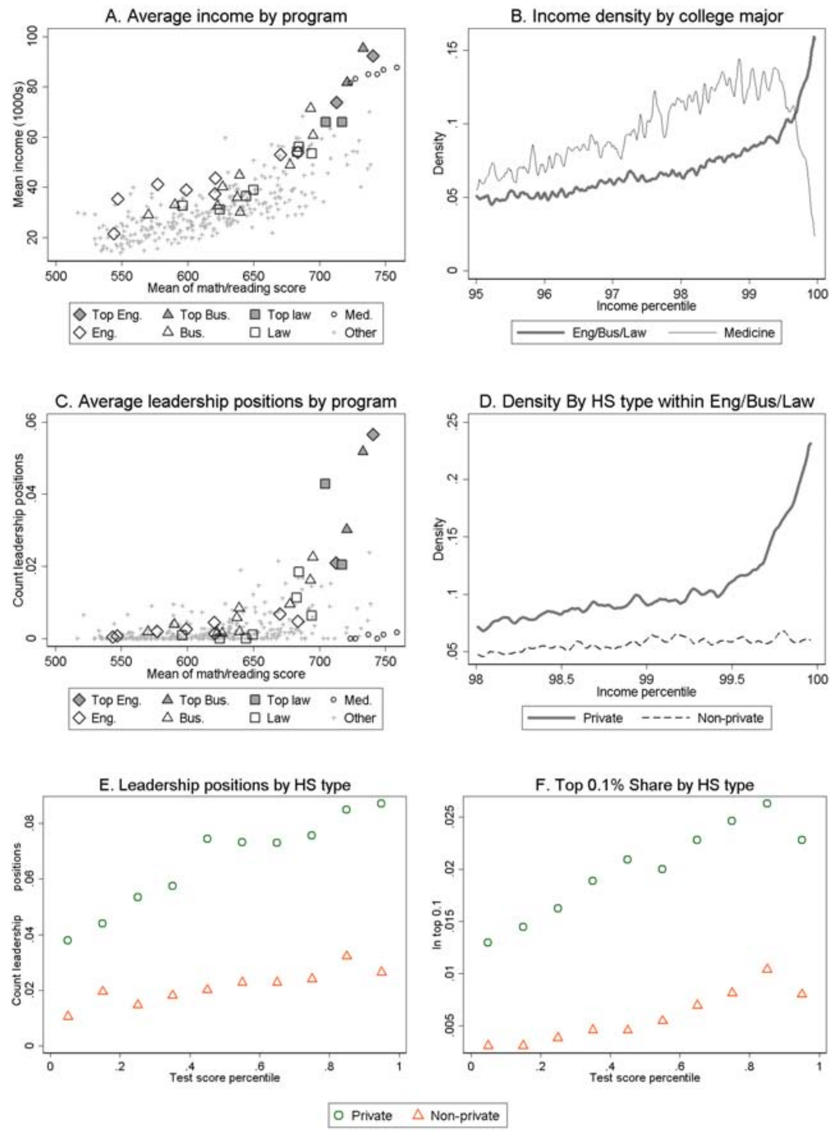
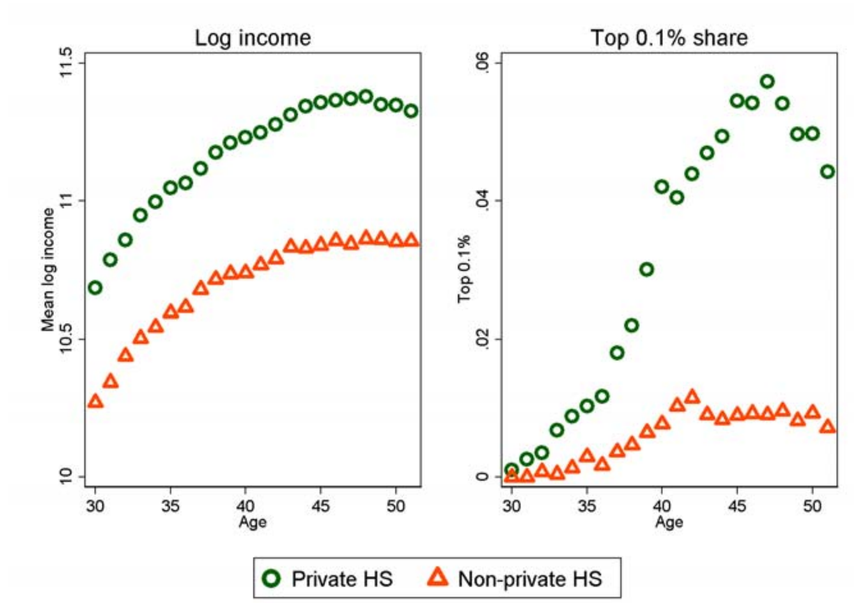
Tables and figures

Figure 1: High income shares by educational background



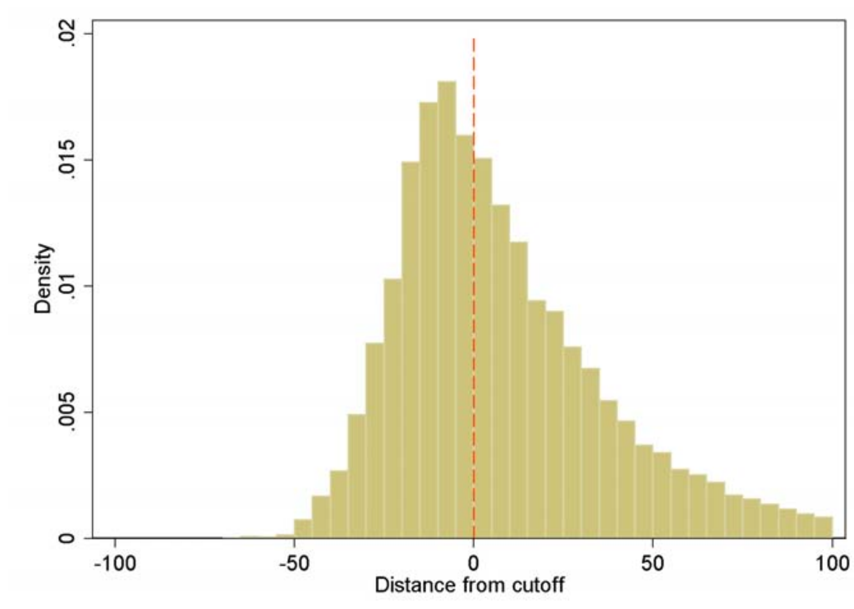
Panel A: mean income by degree program. Each point is a degree program, with marker shapes indicating degree field. Shaded degree programs are the six elite business-focused programs. Medical programs are identified by the 'Med' legend. Horizontal axis is mean of students' math and reading score. Panel B: Income densities. Horizontal axis is percentile of year-specific income distribution for population of admissions test takers age 30 and over. Vertical axis is density of income distribution for listed majors at PUC and UC. Density computed using Epanechnikov kernel with bandwidth 0.02 percentiles. Panel C: Same as panel A but for mean count of leadership positions. Panel D: Density of income by percentile of population income distribution for students admitted to engineering, business, or law programs at UC and PUC, split by high school type. Panels E/F: mean leadership positions (E) and share in top 0.1% of income distribution (F), split by high school type. Horizontal axis is percentile of admissions test distribution for admitted students. Points are means within centered ten percentile bins.

Figure 2: Income and top income shares by age and high school type



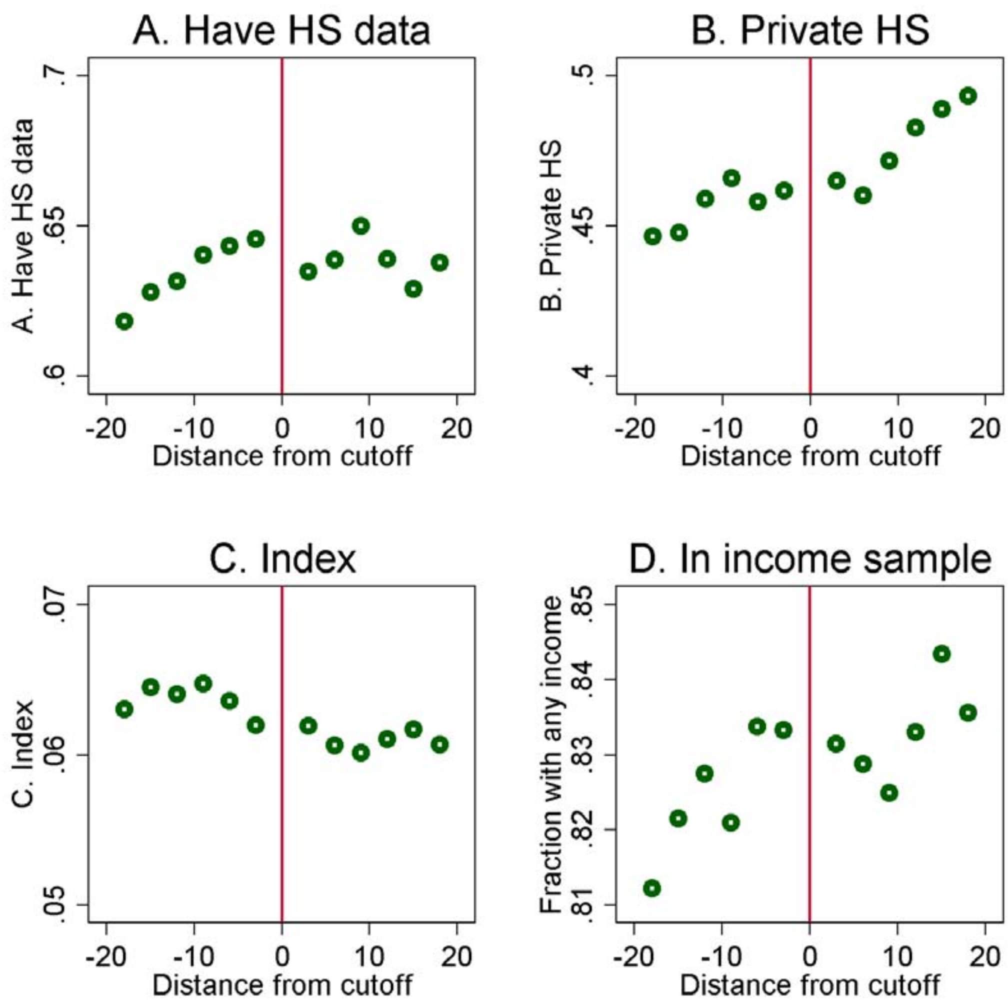
Mean log income and share in top 0.1% of population income distribution by high school type. Sample is students admitted to UC/PUC programs in Bus/Eng/Law. Earnings measured 2005-2013 for students applying for admission between 1980 and 2001. Age is calculated based on elapsed time since application, which is assumed to take place at 18.

Figure 3: Histogram of scores relative to cutoff for elite applications



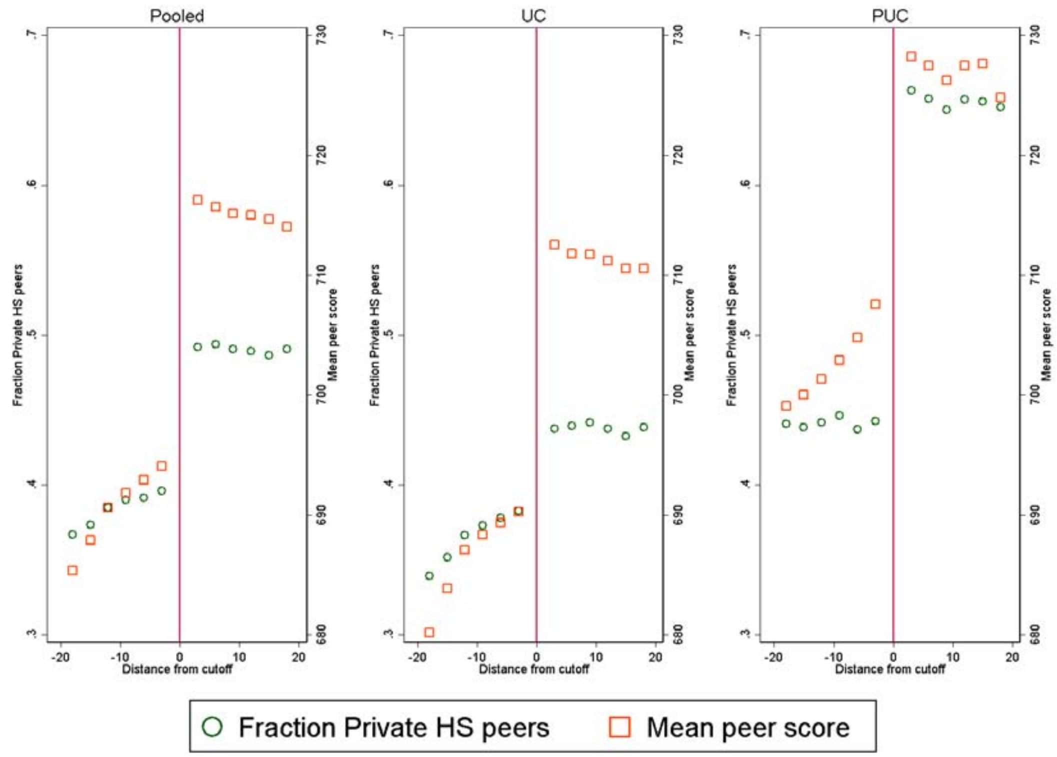
Density of scores for 1974-1991 applicants to elite degree programs. Densities reported within bins of width 5.

Figure 4: Predetermined covariates by position relative to threshold



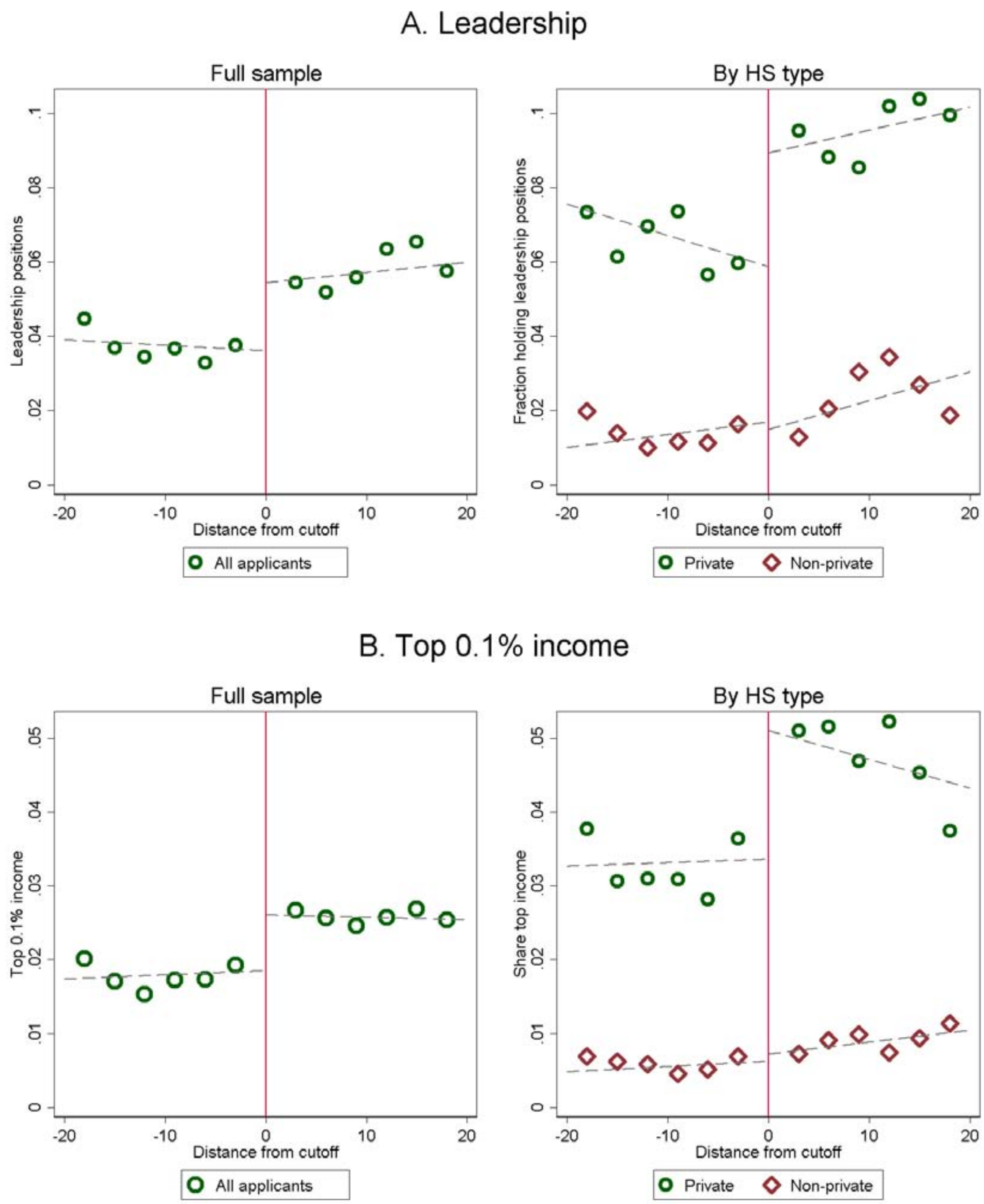
Binned means and fitted values of predetermined covariates by position relative to admissions threshold. Dependent variables given by panel title. Points reflect average outcomes for applicants within three points on either side of the horizontal axis value. Fitted lines obtained using BW=20 specification. Sample is male applicants to elite degree programs.

Figure 5: Changes in peer characteristics across the admissions threshold



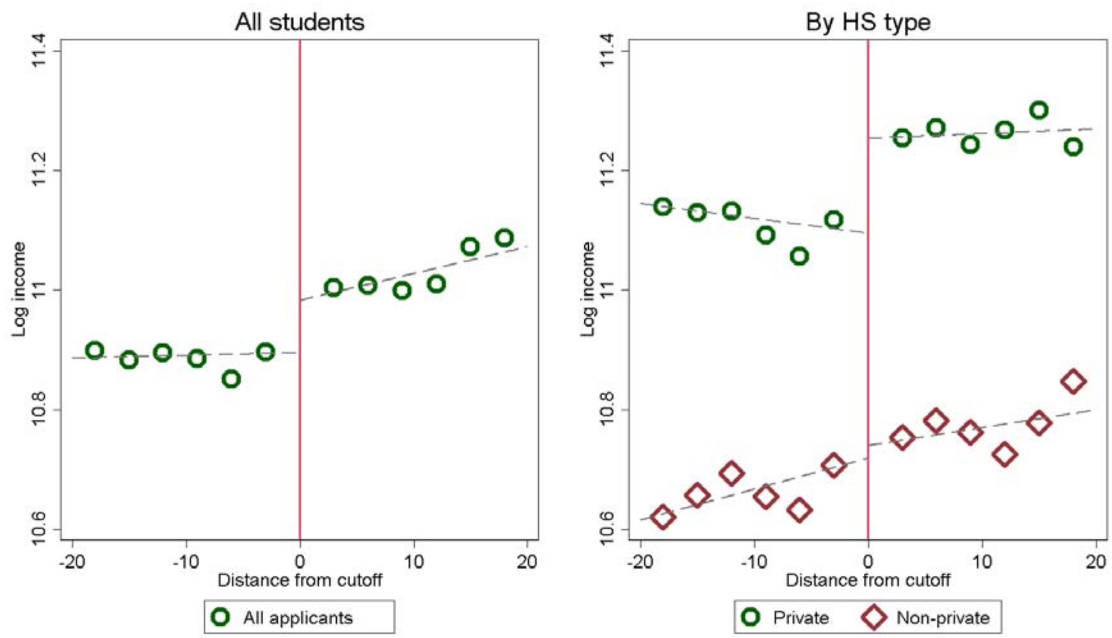
Changes in the fraction of students from private high schools and the mean peer math score at the degree programs to which students are admitted by position relative to threshold. Points reflect average values for applicants within three points on either side of the horizontal axis value. Left panel pools across UC and PUC programs. Center and right panel split applications to UC and PUC programs. Left axis is the fraction of private HS peers, right axis is mean peer scores.

Figure 6: Effect of admission on leadership and top income attainment



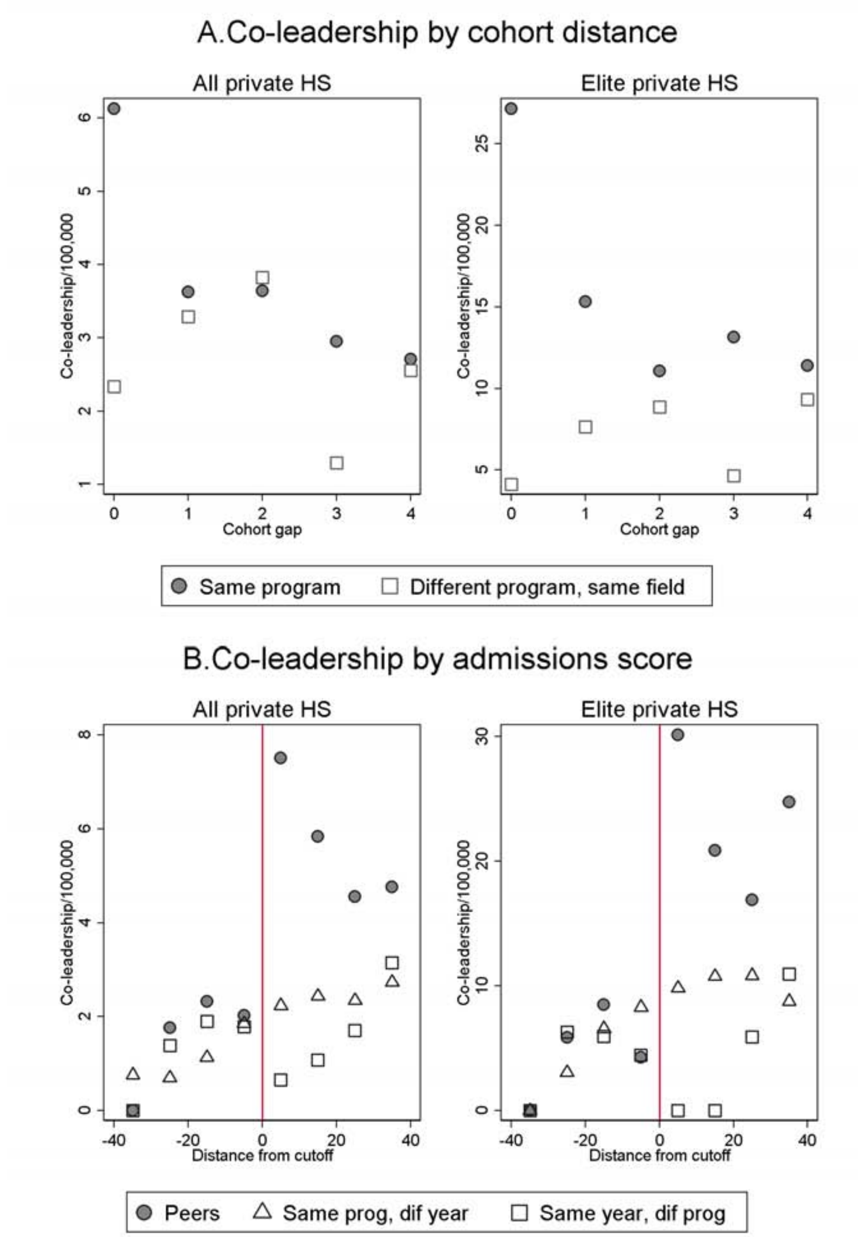
Count of leadership positions and fraction of students with incomes in the top 0.1% of the population distribution by position relative to the threshold. Graphs pool applications across elite degree programs. Points reflect average outcomes for applicants within three points on either side of the horizontal axis value. Fitted values from BW=20 specification. Negative trend to the left of the cutoff in the upper right panel is statistically insigificant (p=0.49). It results from changes in the composition of target programs away from the cutoff. There is no evidence of a negative below-threshold slope within program. See Section 4.5 for a discussion. See Figure A-4 for an alternate version of the figure that residualizes on target program and eliminates the slope.

Figure 7: Log income by position relative to admissions cutoff



Log income by position relative to the threshold. Graphs pool applications across elite degree programs. Points reflect average outcomes for applicants within three points on either side of the horizontal axis value. Fitted values from BW=20 specification.

Figure 8: Co-leadership rates for private high school students



Upper panel: rates of co-leadership per 100,000 pairs by absolute difference in application cohort for students in the same field at either the same institution or a different institution. Lower panel: Co-leadership rates for pairs of private high school students by position relative to cutoff and peer relationship. Points reflect rolling averages of means within 10 points on either side of the horizontal axis value. 'Peers' points are co-leadership rates with students admitted to the target institution-major in the same cohort as the applicant. 'Same prog, dif year' are co-leadership rates for applicants with students admitted to the same program in another cohort. 'Same year, different prog' presents co-leadership rates for applicants with students admitted to the other elite program in the field in the same cohort.

Table 1: Descriptive statistics for test taker and admitted students samples

	Test takers (80-01)	Elite admits	Private elite	Non-private elite	All admitted (82-01)
Reading	485	675	686	662	579
Math	481	738	747	724	601
Elite admit	0.018	1.00	1.00	1.00	0.049
Have HS		0.79	1.00	1.00	0.82
Private HS		0.6	1.00	0.00	0.27
Have leadership position	0.001	0.026	0.033	0.010	0.003
Count of positions	0.002	0.041	0.053	0.016	0.004
N individuals	1957450	36211	17106	11497	549170
In LF sample	0.683	0.855	0.859	0.839	0.844
Mean income (1000s)	25	79.4	88.4	63.0	35.9
90th-95th	0.05	0.189	0.188	0.177	0.086
95th-99th	0.04	0.242	0.257	0.193	0.073
99th-99.5th	0.005	0.044	0.049	0.030	0.009
99.5th-99.9th	0.004	0.045	0.056	0.024	0.008
99.9th and up	0.001	0.016	0.023	0.006	0.002
N person-years	9349480	224327	94594	70387	3103113

Descriptions of student characteristics, leadership outcomes, and income distributions. Upper panel reports observations at individual level. Lower panel reports observations at individual-outcome year level for outcome years at least twelve years after the application year, or roughly age 30. Outcome years range from 2005 to 2013. Columns: 'Test-takers' includes all admissions test takers over the 1980-2001 period. 'Elite admits' includes students admitted to one of six elite business-focused programs. 'Private elite' includes students identified as coming from private school backgrounds who are admitted to elite degree programs. 'Non-private elite' includes elite admits who do not attend private schools. 'All admitted' includes all students admitted to any program between 1982 and 2001. Rows: 'reading' and 'math' are students' admissions exam scores from their first test attempt. 'Elite admit' is a dummy equal to one if a student is admitted to one of the six elite degree programs. 'Have HS' identifies students matched to high school types; 'Private HS' identifies the set of matched students who attended private high schools. 'Have leadership position' is an indicator equal to one if a student has some directorship or C-suite position; 'count of positions' counts the total number of these positions. 'In LF sample' is a dummy equal to one for students who match to the labor force dataset. 'Mean income' is in 1000s of 2014 USD. Categorical percentile variables are means of dummies equal to one if a student's income an outcome year falls within the indicated percentile range.

Table 2: Regression discontinuity sample description

	All	Male	Marginal	HS data
A. 1980-1991				
Male	0.76	1.00	1.00	1.00
Age	43.9	43.9	44	44.1
Reading	658	657	653	654
Math	742	746	738	740
Have HS data	0.72	0.75	0.75	1.00
Private HS	0.485	0.484	0.482	0.482
In LF sample	0.806	0.833	0.828	0.861
Average earnings	87.5	94.3	92.8	93.8
Top 0.1% share	0.018	0.022	0.021	0.023
N application-years	236780	172590	98952	74171
N applicants	30066	21695	12790	8736
B. 1974-1991				
Male	0.77	1.00	1.00	1.00
Age	48.3	48.4	48.4	48.2
Have HS data	0.60	0.63	0.64	1.00
Private HS	0.466	0.468	0.465	0.465
Any leadership position	0.029	0.035	0.03	0.03
Count leadership positions	0.049	0.061	0.052	0.051
>4 positions	0.002	0.002	0.002	0.002
Count BOD	0.031	0.039	0.033	0.031
Count C-suite	0.018	0.022	0.018	0.020
N applications	57940	42392	24124	15371
N applicants	43845	31783	18501	11522

Description of sample means and counts. Upper panel describes sample used in estimation of earnings specifications. Observations are at application-outcome year level for outcome years 2005-2013 and application years 1980-1991. Only application-year outcome-year pairs in which students are age 40 or older are included in the sample. Lower panel describes the sample used in leadership specifications, and includes 1974-1991 applications to elite degree programs. Observations at application level. 'All' is full sample of applicants observed in data. 'Male' is sample of male applicants. 'Marginal' restricts sample to students within 20 points on either side of admissions threshold. 'HS data' subsets on marginal students for whom high school can be classified as private or non-private. 'Reading' and 'Math' are student scores on admissions tests. 'Private HS' is a dummy equal to one if students attended a private high school. 'In LF sample' is a dummy equal to one if an application-year record is matched to the labor force sample. Earnings measured in 2014 USD. 'Top 0.1% share' is a dummy equal to one if students have an income record in top 0.1% of the income distribution. 'Any leadership position' is a dummy equal to one if student holds at least one C-suite or directorship position. 'Count leadership positions,' 'count BOD', and 'count C-suite' count the total number of positions, directorships, and C-suite positions students hold.

Table 3: Balance on predetermined covariates

	BW=10			BW=20		
	All	Private	Non-private	All	Private	Non-private
A. Predetermined characteristics						
Have HS	-0.005			-0.017		
	(0.008)			(0.012)		
Private HS	0.007			-0.009		
	(0.011)			(0.016)		
Index	-0.001	0.000	0.000	0.002	0.002	0.003
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
N	12933	3853	4462	24009	7096	8190
B. Labor force participation						
In LF	0.002	0.000	0.006	-0.005	0.001	-0.003
	(0.008)	(0.011)	(0.012)	(0.012)	(0.015)	(0.016)
Intercept	0.826	0.873	0.843	0.833	0.876	0.852
N	54134	19507	21412	98935	35733	38434

Significance: *: 0.10 **: 0.05 ***: 0.01. Estimates of equations 1 and 2 where dependent variables are predetermined covariates (Panel A) and labor participation (Panel B). Left three columns are BW=10 specification, right three columns are BW=20 specification. See section 4.1 for a description of these specifications. 'All' column includes all applications. 'Private' and 'Non-private' columns report separate estimates by student high school type. Observations in Panel A are at the application level. 'Have HS' is an indicator equal to one if an applicant's high school can be classified. 'Index' is a linear, earnings-weighted index of application cohort and target degree program fixed effects. Observations in Panel B are at the application-outcome year level. 'In LF' is a dummy equal to one if a student is matched to the labor force sample in a given application-year. Sample sizes differ slightly from Table 2 due to missing data.

Table 4: Effect of elite admission on peer attributes and other acceptance outcomes

	Effects on peer attributes		Below-threshold acceptance outcomes		
	Peer elite HS	Peer math	Same field other elite	Same field not elite	Any bus field
A. All programs					
Pooled	0.0982***	22.57***	0.124***	0.510***	0.720***
	(0.003)	(0.505)	(0.006)	(0.009)	(0.008)
Main effect	0.106***	23.37***	0.0603***	0.542***	0.687***
	(0.005)	(0.878)	(0.007)	(0.015)	(0.014)
Private HS interaction	-0.0188**	-3.296***	0.0885***	-0.0686***	0.0279
	(0.008)	(1.205)	(0.013)	(0.021)	(0.020)
N	3925	3925	4143	4143	4143
B. UC programs					
Pooled	0.0591***	22.42***	0.001	0.606***	0.699***
	(0.003)	(0.508)	(0.001)	(0.010)	(0.009)
Main effect	0.0878***	23.38***	0.002	0.590***	0.682***
	(0.005)	(0.805)	(0.001)	(0.015)	(0.015)
Private HS interaction	-0.0402***	-2.892**	-0.002	-0.002	0.002
	(0.007)	(1.185)	(0.001)	(0.023)	(0.022)
N	3150	3150	3331	3331	3331
C. PUC programs					
Pooled	0.215***	21.81***	0.561***	0.169***	0.796***
	(0.007)	(1.191)	(0.018)	(0.014)	(0.015)
Main effect	0.201***	22.48***	0.413***	0.251***	0.713***
	(0.019)	(3.647)	(0.038)	(0.034)	(0.035)
Private HS interaction	0.00422	-3.862	0.198***	-0.135***	0.0968**
	(0.021)	(3.936)	(0.049)	(0.039)	(0.043)
N	775	775	812	812	812

Significance: *: 0.10 **: 0.05 ***: 0.01. Effects of admissions-threshold crossing on peer attributes (left two columns) and description of below-threshold admissions outcomes (right three columns). Panels A, B, and C present findings for all applications, applications to UC programs, and applications to PUC programs, respectively. Observations are at application level and include only the application years 1982 through 1991. These are the years for which applications to both elite degree programs and the broader set of CRUCH programs are available. See Online Appendix B.1 for a discussion. Left two columns: estimates obtained using BW=10 specification where dependent variable is the listed peer attribute. Within each panel the 'Pooled' effect is obtained using all applicants to listed program type. 'Main' and 'Private HS interaction' rows are estimates from specifications that allow for heterogeneous effects for students from private high school backgrounds. The main effect is for non-private students, the interaction term is for private HS students. Right three columns: 'pooled' rows are the fraction of marginally rejected students admitted to degrees of the type listed in the column. 'Main effect' row is the mean for students from non-private HS. 'Interaction' row is the difference between non-private mean and mean for private HS students.

Table 5: Effect of elite admission on income and leadership outcomes

		Leadership		Top income		Log income	
		TC	IV	TC	IV	TC	IV
A. BW=10	All	0.019***	0.189***	0.008***	0.084**	0.118***	1.213 ***
		(0.006)	(0.056)	(0.003)	(0.039)	(0.023)	(0.294)
		12933		54134		44756	
	Private HS	0.032**	0.333**	0.018***	0.204**	0.174***	1.95***
		(0.014)	(0.140)	(0.007)	(0.089)	(0.038)	(0.546)
		3853		19507		17032	
	Non-private	0.002	0.022	0.002	0.022	0.074 **	0.712 **
		(0.005)	(0.045)	(0.003)	(0.019)	(0.036)	(0.343)
		4462		21412		18105	
	Test	0.039**	0.034**	0.031**	0.038**	0.055*	0.099*
B. BW=20	All	0.018**	0.199**	0.008 *	0.087	0.087***	0.996**
		(0.008)	(0.090)	(0.004)	(0.055)	(0.034)	(0.472)
		24009		98935		81879	
	Private HS	0.031	0.348	0.017 *	0.219	0.159***	1.99 *
		(0.020)	(0.223)	(0.010)	(0.152)	(0.056)	(1.127)
		8190		35733		31298	
	Non-private HS	-0.002	-0.020	0.001	0.01	0.021	0.221
		(0.007)	(0.065)	(0.003)	(0.030)	(0.050)	(0.540)
		7096		38434		32547	
	Test	0.119	0.107	0.121	0.094*	0.068*	0.242

Significance: *: 0.10 **: 0.05 ***: 0.01. Estimates of effects of admission on leadership and top income attainment by high school type. Panel A reports estimates from BW=10 specification and Panel B from BW=20 specification. 'TC' columns are estimated threshold-crossing effects, 'IV' columns are IV estimates in which threshold-crossing instruments for peer private high school share at the admitted degree program. See section 4.1 for more details on estimation. Columns denote dependent variables. 'Leadership' is count of leadership positions. 'Top income' is a dummy equal to one if a student has income within the top 0.1% of the distribution. Observations in the top income and log income columns are at application-outcome year level, while observations in leadership column are at the application level. Top income dummy is zero for labor force non-participants. 'Test' row reports p-values from tests that the estimates for private and non-private HS students are equal. Standard errors clustered at person level.

Table 6: Heterogeneous effects by high school type and other observable characteristics

	Private HS	Math	Verbal	Santiago
A. Descriptive statistics				
Non private		734	655	0.844
Private		743	660	0.858
Gap		8.8	5.4	0.014
Adjusted gap		4.4	3.4	0.017
B. Admissions effect estimates				
Leadership	.0286*	0.002	0.001	0.000
	(0.016)	(0.002)	(0.001)	(0.013)
Top income	.019**	0.000	0.000	0.002
	(0.007)	(0.001)	(0.001)	(0.007)
Log Income	.116**	0.002	0.000	-0.085
	(0.053)	(0.006)	(0.004)	(0.062)

Significance: *: 0.10 **: 0.05 ***: 0.01. Panel A: Differences in variable listed in column by high school type within BW=10 sample of marginal applicants. 'Non-private' and 'private' rows are means by high school type. 'Gap' is the difference in means for each high school type. 'Adjusted gap' is mean difference within cells defined by target program and application year. Panel B: Estimates of interaction effects from BW=10 RD specification that allows for interactions between admission and each listed variable in columns. Dependent variables are listed in rows. Each specification includes a main effect of admission, an intercept term, and controls for main effects of the column variables. Sample: 1980-1991 marginal applications from male students with non-missing test score and geographic data. 1974-1979 application years omitted from leadership specifications due to unavailability of test score data.

Table 7: Effect of admission on sector of employment

	All		Private HS		Non-private HS	
name	BL	Effect	BL	Effect	BL	Effect
Real estate/rental/business activiites	0.171	0.004	0.165	0.019	0.163	0.008
Wholesale and retail trade	0.158	0.000	0.164	0.011	0.149	-0.007
Finance	0.125	0.009	0.143	0.017	0.100	0.005
Public administration	0.115	-0.028 ***	0.083	-0.027 **	0.148	-0.026 *
Construction	0.074	0.002	0.080	-0.010	0.073	0.004
Manufacturing (non-metallic)	0.069	0.009	0.088	0.005	0.059	0.006
Teaching	0.065	-0.003	0.055	-0.017 **	0.078	0.003
Transport/storage/communication	0.050	0.005	0.048	0.003	0.056	0.001
Manufacturing (Metallic	0.038	-0.002	0.038	0.004	0.036	-0.006
Other community service	0.037	0.001	0.032	-0.003	0.048	-0.001
Utilities	0.027	0.001	0.027	0.003	0.028	0.003
Mining	0.020	0.007 *	0.017	0.009	0.022	0.007
Agriculture	0.018	0.002	0.027	-0.005	0.009	0.006 *
Social services and health	0.018	-0.002	0.017	-0.005	0.020	-0.002
Hospitality	0.009	-0.003 *	0.008	-0.001	0.009	-0.002
Fishing	0.004	0.000	0.006	0.001	0.003	0.000
Foreign business	0.001	-0.001 *	0.002	-0.002	0.000	0.000
Building administration	0.000	0.000	0.000	0.000	0.000	0.000
Have sector	0.718	0.006	0.735	0.016	0.716	0.002
N	36995		13804		14942	

Significance: *: 0.10 **: 0.05 ***: 0.01. Below-threshold probability and effect of elite admission on the probability of having main job in selected sectors from equation 2 (BW=10 specification). 'All,' 'Private HS' and 'Non-private HS' headings denote sample populations. 'BL' column presents below-threshold baseline probability of working in listed sector (i.e., intercept in RD estimation equations). 'Effect' column presents point estimate of threshold-crossing effect. 'Have sector' is an indicator equal to one if sector data is available for a student. N refer to counts of application-years with available sector data. Standard errors clustered at student level. Sectors sorted by baseline share in full sample.

Table 8: Effect of elite admission by below-threshold outcome

	All	Priv	Non-priv
A. No interaction terms			
All	0.072	0.114	-0.027
	(0.059)	(0.070)	(0.104)
B. Including interaction terms			
Main effect	0.158	0.243*	0.102
	(0.100)	(0.124)	(0.167)
Non-business	0.137	0.12	-0.006
	(0.174)	(0.214)	(0.283)
Peer score gap	-0.001	-0.001	-0.001
	(0.004)	(0.005)	(0.006)
Private HS gap	1.05***	1.09***	0.664
	(0.321)	(0.398)	(0.561)
Elite program	-0.404**	-0.410**	-0.608**
	(0.166)	(0.200)	(0.297)
C. Split by tercile of private HS gap			
Large gap	0.223**	0.248**	0.143
	(0.092)	(0.121)	(0.142)
Medium gap	0.197**	0.267**	0.080
	(0.094)	(0.110)	(0.165)
Small gap	-0.221**	-0.160	-0.326
	(0.108)	(0.121)	(0.198)
N	12185	7849	4336

Significance: *: 0.10 **: 0.05 ***: 0.01. Estimates of Equation 3 by HS type using 2000-2003 application data and the BW=20 specification. Dependent variable is log income. Panel A reports estimates of earnings effects without interaction terms. Panel B reports estimated main admissions effect and estimates of interactions between admission and the listed variables. 'Non-business' is a dummy equal to one if a students' next-choice degree is not in business, law, or engineering. 'Peer score gap' is the difference between mean math scores at the target degree program and mean math scores at the next option. 'Private HS gap' is the difference between the fraction of students from private high schools at the target program and the fraction at the next option. Score gap and HS gap variables are demeaned (using means within the BW=20 sample). See Online Appendix E for descriptive statistics. 'Elite program' is a dummy variable equal to one if a students' next option is another elite degree program. Panel C reports estimates of equation 2 splitting by terciles of peer private HS gap. Sample pools over applications all elite degree programs, and excludes both admitted and rejected students who would not be admitted to any degree program if they were rejected from the target.

Table 9: Difference-in-difference estimates of peer effects on co-leadership

	Private/ Private/	Elite/ Elite	Private/ Non-private	Non-private/ Non-private
A. Single Difference				
Same cohort	3.42 **	15.72 *	0.37	0.29
	(1.37)	(9.17)	(0.31)	(0.30)
1 year gap	0.92	3.91	0.46 *	0.15
	(0.85)	(5.09)	(0.27)	(0.20)
2 year gap	0.94	-0.33	0.24	0.16
	(0.76)	(3.62)	(0.20)	(0.25)
3 year gap	0.24	1.74	0.14	-0.22 ***
	(0.77)	(5.58)	(0.23)	(0.07)
N	5761326	658422	13119911	8207263
B. Difference in differences				
Same cohort	3.64 **	20.93 **	0.58	0.56 *
	(1.67)	(10.19)	(0.56)	(0.32)
1 year gap	0.19	5.58	0.54	-0.02
	(1.20)	(6.14)	(0.39)	(0.42)
2 year gap	-0.33	0.14	0.04	-0.49
	(1.06)	(4.20)	(0.41)	(0.48)
3 year gap	1.51	6.43	0.44	-0.19
	(0.92)	(6.48)	(0.48)	(0.22)
N	10609222	1317245	22022462	11568265

Significance: *: 0.10 **: 0.05 ***: 0.01. Estimates of equations 4 and 5 by sample listed in column. 'Private/private' column consists of pairs of private high school students. 'Elite/elite' column uses pairs of students where both members are from an elite private high school. 'Private/non-private' considers pairs where one student is from a private HS and the other is not. 'Non-private/non-private' is pairs of students both from non-private schools. Standard errors clustered use two-way clustering at the person-person level.