The returns to STEM programs for less-prepared students

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February 2023

Abstract. The returns to selective STEM programs depend on: (a) whether students complete these programs, (b) earnings conditional on completion, and (c) counterfactual schooling choices. Much existing research has focused on only one of these margins—potentially leading to an incomplete picture of the returns to STEM education for students with less academic preparation. Using data from a Colombian university and two empirical strategies, we find that less-prepared students have higher earnings returns to selective STEM programs than more-prepared students, even though they are less likely to complete these programs. A key mechanism is that less-prepared students have lower-paying counterfactual schooling options.

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Many countries have policies to increase the number of college students who earn degrees in science, technology, engineering, and mathematics (STEM). These policies often seek to boost STEM enrollment by drawing in students from less advantaged backgrounds, who may also be less prepared for STEM coursework on average. Research on field of study often finds that there is a high earnings premium to a completed STEM degree (Altonji et al., 2012; Kirkebøen et al., 2016), and policymakers hope that new STEM enrollees will have similarly high returns. But other work shows that STEM programs have high dropout rates, especially among students with relatively less academic preparation (Stinebrickner and Stinebrickner, 2014; Arcidiacono et al., 2016), so these expansions may not necessarily help the new marginal students. In this paper, we highlight that looking only at average returns to completed degrees (as in, e.g., Altonji et al., 2012) or STEM completion rates (as in, e.g., Arcidiacono et al., 2016) does not speak to the returns to such policies, which depend on both the likelihood of graduating and the returns conditional on graduating. We use data from selective STEM programs at a flagship university in Colombia to *jointly* examine the relationship between students' completion rates and earnings returns—leading to a more complete analysis of the costs and benefits of expanding STEM education to less-prepared student populations. Our empirical strategy combines a regression discontinuity (RD) design based on admission cutoffs with two sources of variation in the academic preparation of marginal admits. This allows us to ask two questions: 1) How do more- and less-prepared students vary in their completion rates and earnings to returns to enrolling in selective STEM programs? and 2) What are the mechanisms for heterogeneity in these returns?

Our paper is novel in identifying how the returns to STEM enrollment vary explicitly along the dimension of academic preparation for STEM coursework. There is growing evidence that the earnings returns to attending a selective college are larger for students from disadvantaged backgrounds than for advantaged students (Dale and Krueger, 2002; Chetty et al., 2020; Bleemer, 2022; Black et al., 2023), including from papers that use RD designs similar to our own (Saavedra, 2009; Zimmerman, 2014; Smith et al., 2020; Bleemer, 2021).[1] But these papers cannot directly look at heterogeneity in the returns to STEM enrollment because

[1] There is a large literature on the earnings returns to college selectivity, and not all papers find that these returns are higher for disadvantaged students. Zimmerman (2019) and Michelman et al. (2022) find that the returns to attending elite colleges are larger for high-SES students, while Hastings et al. (2013) and Mountjoy et al. (2020) do not find significant heterogeneity in the returns to college selectivity by race and/or socioeconomic status. Canaan and Mouganie (2018) find that low-skilled students experience earnings gains from attending more selective colleges in France, but these returns are larger for the high-SES students within their sample. Andrews et al. (2016) find that the earnings gains to attending Texas A&M are largest in low quantiles—consistent with larger benefits for disadvantaged students—but the opposite is true for the returns to attending University of Texas Austin. Machado et al. (2022) find that attending an elite Brazilian university leads to early-career earnings gains (only) for students admitted through affirmative action, but these gains fade as individuals' careers progress. Other papers find earnings gains to college selectivity but do not examine heterogeneity (Hoekstra, 2009; Anelli, 2020; Sekhri, 2020).

admissions are at the university level rather than the major level. There are only a few papers that examine heterogeneity in the returns to STEM enrollment or STEM degrees, and these papers tend to find, if anything, that students with less preparation have *lower* returns than other students. Bertrand et al. (2010) find that the returns to attending selective engineering programs in India are lower for affirmative action students than for general track students, although they acknowledge that their results are underpowered. Arcidiacono (2004) finds that the returns to a completed STEM degree are increasing in SAT math scores, but this result relies on a selection-on-observables assumption.[2] Our contribution is to use two new identification strategies that provide well-identified evidence on how the returns to enrolling in selective STEM programs vary with academic preparation. Our first strategy takes advantage of data on a large number of pre-college test scores, which allows us to estimate heterogeneity in our RD coefficients based on an applicant's predicted likelihood of completing a STEM degree.[3] Our second strategy exploits changes in admission policies that caused the university's STEM quotas to double in some majors and cohorts, which meant that the marginal admits came from lower in the application pool than usual. Thus we provide direct evidence on the efficacy of policies that induce less-prepared students to enroll in STEM programs or that reduce the weight on academic preparation in STEM admissions.

Our paper is also the first to shed light on the role of counterfactual schooling choices as a mechanism for heterogeneity in the returns to STEM enrollment. STEM programs are at the center of a debate over "mismatch" in admission to selective universities because academic preparation is particularly important for completing a STEM degree. Arcidiacono et al. (2016) present evidence that minority students whose academic preparation is relatively less than that of their classmates would be more likely to graduate with a STEM degree if they attended less selective schools. Conversely, Bagde et al. (2016), Mountjoy et al. (2020), and Bleemer (2022) do not find that increases in college selectivity reduce disadvantaged students' STEM completion rates. On both sides of this debate, it is often implicitly assumed that disadvantaged students who pursue STEM degrees at selective universities would have also pursued STEM degrees at other schools if they had been rejected. It is unclear whether this

[2] Altonji (1993), Webber (2014), and Kinsler and Pavan (2015) also present results on ability variation in the returns to STEM degrees using selection-on-observables strategies, but the evidence is less clear. Kinsler and Pavan (2015) find that the returns to science degrees are increasing in SAT math scores, but only for individuals who work in jobs related to their major. Altonji (1993) finds that the returns to a college degree in a technical major relative to other educational outcomes are larger for high-ability women than for low-ability women, but the opposite is true for men. Webber (2014)'s estimates of the lifetime earnings premiums of a STEM degree relative to other educational outcomes are broadly similar across ability quintiles.

[3] Our RD heterogeneity strategy is similar to Abdulkadiroğlu et al. (2014)'s approach of using additional test scores to examine heterogeneity in the impacts of attending elite high schools.

assumption holds in practice.[4] Research shows that disadvantaged students tend to apply to fewer and less-selective colleges than other applicants (Hoxby and Avery, 2013; Pallais, 2015; Angrist et al., 2022), but these papers do not observe individuals' interest in STEM programs at different schools. We shed light on this by using Colombia's national higher education census, which allows us to observe the schools and majors that students enrolled in when they were rejected from a selective STEM program. Thus our paper presents novel evidence on how STEM applicants of differing preparedness vary in their counterfactual schooling options and the role this plays in their earnings outcomes.

Our analysis focuses on selective STEM programs at a public flagship university in Cali, Colombia called "Univalle" (*Universidad del Valle*). Univalle offers roughly 60 degree programs each year including STEM programs in engineering and natural sciences. Admission to these STEM programs is highly competitive; the median STEM enrollee scored at the 96$^{\text{th}}$ percentile of the ICFES national college entrance exam, and over 90 percent of admitted students chose to enroll. We obtained data on applicants to all Univalle programs in 1999–2004, and linked this to administrative records that provide individuals' scores on each ICFES exam subject. We also match our data to a national census of college enrollment and graduation, and to administrative earnings records for the year 2017. This allows us to observe applicants' enrollment choices even if they were not admitted to Univalle, as well as their graduation outcomes and formal sector earnings measured roughly 15 years later.

We begin by estimating mean graduation rates and earnings returns for students on the margin of admission to Univalle's STEM programs. Students apply to specific programs at Univalle, and admission is determined solely by scores on the ICFES exam. We use a RD design that estimates the causal effect of enrolling in Univalle for students near the admission thresholds. Across all engineering and natural science programs, the mean graduation rate for marginal enrollees was only 34 percent, but enrolling in these programs increased average monthly earnings in 2017 by 14 percent. Univalle's STEM programs are unique in having low graduation rates and high returns. In non-STEM programs at Univalle, we find that 50 percent of marginal enrollees graduated, but the mean earnings return was close to zero.

We then use two empirical strategies to examine how the returns to STEM enrollment vary for less- and more-prepared applicants. First, we estimate heterogeneity in the RD

[4] The most compelling evidence on the relationship between selective university admissions and STEM degree attainment comes from Bleemer (2022), who shows that a ban on affirmative action in the University of California system reduced underrepresent minority (URM) students' STEM degree completion rates. But Bleemer (2022) only has data on enrollment in STEM courses for five UC campus, so it is hard to know whether the aggregate decline in STEM completion occurred because URM students were less likely to persist in STEM programs after affirmative action was banned, or because they were less likely to pursue a STEM degree to begin with. Mismatch research typically focuses on the student/college fit as it relates to persistence in STEM courses, whereas we provide evidence on relationship between selective university admissions and enrollment in STEM programs.

coefficients by exploiting the fact that our data contain multiple measures of pre-college academic preparation. Univalle admissions are based on a weighted average of up to nine subject scores on the ICFES exam, and the weights chosen by the admission committees differ from those that best predict which students would graduate. This allows us to define a measure of an applicant's propensity to complete a STEM degree, and to estimate RD regressions separately for less- and more-prepared applicants as defined by graduation propensity.

Our second strategy exploits variation in the size of Univalle's admission quotas across cohorts. Univalle usually admitted cohorts of about 60 applicants to each STEM program, but changes in admission policies increased these quotas to over 120 applicants in certain programs and years. In the cohorts with large quotas, students on the margin of admission were lower-ranked in their application pool than usual, and were thus less prepared academically for the program. We use an RD difference-in-differences design to examine how the graduation and earnings returns for marginal admits changed in the cohorts with large quotas. An advantage of this strategy is that the estimates reflect the effects of an actual expansion of STEM quotas that could be implemented at other universities.

Our main finding is that less-prepared students had significantly *higher* earnings returns to enrolling in Univalle's STEM programs than more-prepared students. These higher returns occurred in spite of the fact that less-prepared students were less likely to graduate from Univalle's STEM programs than more-prepared students. Across our two empirical strategies, we find that less-prepared enrollees were 9–18 percentage points less likely to complete the Univalle STEM program than more-prepared enrollees. Yet less-prepared enrollees experienced average large earnings gains from enrolling, with magnitudes ranging from 30–40 percent across our two approaches. For more-prepared students, we find small positive returns to STEM enrollment that are not statistically different from zero.

An important mechanism for the heterogeneity in earnings returns is where applicants enrolled when they were not admitted to Univalle's STEM programs. Using our national higher education data, we find that, relative to more-prepared applicants, less-prepared applicants were less likely to enroll in STEM programs at other universities when they were rejected. As a result of these different counterfactual enrollment choices, the causal effect of enrolling in Univalle on the likelihood of earning *any* STEM degree was similar for less- and more-prepared applicants. Less-prepared applicants' fallback programs tended to be in lower-paying majors or at technical schools, suggesting that alternative schooling choices partly explain the heterogeneity in returns. Using transcript data, we also find that the gap in college GPA between less- and more-prepared graduates narrowed over the course of the program. This suggests that the less-prepared students who managed to earn a STEM degree may have had higher skill accumulation than more-prepared graduates, although the evidence on this mechanism relies on stronger assumptions.

Our findings show that policies that encourage students to enroll in STEM programs can yield large earnings returns, even if the affected students are less prepared for STEM coursework on average. These policies are motivated by a perceived shortage of STEM workers (Carnevale et al., 2011; Deming and Noray, 2020) and by evidence that STEM skills are important for growth and innovation (Peri et al., 2015; Bianchi and Giorcelli, 2020). Our results suggest that students affected by these policies may have even larger mean returns than existing enrollees, and that there may be welfare gains to admitting more disadvantaged students to STEM programs (Bleemer and Mehta, 2021). An important caveat is that these gains may be unevenly distributed given low degree attainment rates. Our estimates also do not capture general equilibrium effects that might arise from very large increases in STEM enrollment (Bianchi, 2020).

Further, our paper shows that STEM programs can play an important role in reducing earnings inequality among students who arrive at college with different levels of academic preparation. Our results are consistent with many other papers that find that academic preparation is a strong predictor of whether students can complete a STEM program (e.g., Sabot and Wakeman-Linn, 1991; Ost, 2010; Arcidiacono et al., 2012). But our findings show that focusing *only* on graduation rates can lead to an overly-pessimistic view about the benefits of admitting less-prepared students to selective STEM programs. Similarly, our paper highlights that the debate over mismatch in STEM persistence rates is only part of the story. In choosing which students to admit to selective STEM programs, it is important to consider that students with lower levels of academic preparation may be less likely to pursue STEM degrees elsewhere if they are rejected.

The paper proceeds as follows. Section 1 motivates our analysis by presenting descriptive patterns on the relationship between academic preparation, STEM degree attainment, and earnings returns using nationwide data from Colombia. Sections 2 describes our Univalle data and main analysis sample. Section 3 presents mean returns to Univalle STEM programs using an RD design. Sections 4–5 present our two empirical approaches that examine heterogeneity in these returns by academic preparation and underlying mechanisms. Section 6 concludes.

1. Motivation

1.1. **Colombian administrative data and institutional background.** Our analysis is made possible by three administrative education and labor market datasets from the country of Colombia. Our first dataset is from a national standardized exam called the ICFES, which all Colombian students are required to take to apply to college (ICFES, 2013a).[5] The ICFES is similar to the SAT exam in the United States, but it is taken by nearly all high school graduates in the country. It also contains more detailed subjects; over the past two decades

[5] The ICFES is now named Saber 11, but we use the name that matches the period of our data.

the ICFES has included 7–9 different subject tests. Our dataset includes subject scores and demographic characteristics for all students who took the exam in 1998–2003.

Second, we use Ministry of Education data on enrollment and graduation at nearly all colleges in the country (SPADIES, 2013). Colombia has a wide range of public and private colleges with varying selectivity and degree offerings. Most of its 33 regions have a public flagship university, which is typically the most selective college in the region and is much less expensive to attend than comparable private universities. Colombia, like the United States, has a decentralized system of college admissions; colleges set their own admission criteria, and students apply separately to each institution and choose among their offers. A difference is that Colombian students apply to institution/major pairs that we call "programs." Our data include all students who enrolled in college programs tracked by the Ministry from 1998–2012. We observe each student's institution, field of study, dates of entry and exit, and graduation outcome.

Finally, we use earnings data from the Ministry of Social Protection's tax records (PILA, 2019). These data provide monthly earnings in 2017 for any individual who worked in the formal sector. Our data do not include earnings from informal firms that are not registered with the Ministry. The informal sector is a substantial portion of the Colombian labor market, although it is less important for college-educated workers. Below we examine effects on formal employment—defined as appearing in the Ministry's data—and we discuss the sensitivity of our results to missing data on informal earnings.

We link the administrative datasets using individuals' names, birthdates, and ID numbers. Appendix C.1 provides details on the data coverage and merge process.

1.2. **Descriptive patterns.** Before turning to our main analysis, Table 1 presents descriptive statistics on STEM enrollment, graduation, and earnings outcomes using our national administrative data. We categorize all bachelor's degree programs in the Ministry of Education data into STEM (engineering and natural sciences) and non-STEM (all other).[6] We also group programs based on whether they are offered by a public or private university. Our main interest is in STEM programs at public universities because these are the programs for which there is significant excess demand and thus for which quotas and admission policies are most important. Many private colleges in Colombia are essentially open enrollment, and even at elite private schools, admissions are much less competitive due to high tuition.

In the columns of Table 1, we group high school graduates based on a measure of academic preparation for STEM programs. The sample for Table 1 includes all students who took the ICFES exam in 1998–2003. To define academic preparation, we take the subsample of

[6] Bachelor's degree programs in Colombia typically have on-time durations of 4–5 years. Table 1 also presents statistics for technical training programs, which are typically 2–3 years in duration.

students who enrolled in a public STEM program and regress an indicator for completing the program on the vector of ICFES subject scores. Our measure of STEM preparation is the predicted values from this regression in the full sample. This measure averages the ICFES subject scores based on their predictive power for STEM graduation. We group the population of exam takers in each year into deciles based on academic preparation and show outcomes for the top five deciles.[7]

We highlight three patterns in Table 1. First, STEM enrollment is heavily concentrated among students with the highest levels of academic preparation. Panel A shows the proportion of students who enrolled in each college program group. In the top decile of academic preparation, 19 percent of students enrolled in a STEM program at a public university (column A). In the sixth decile, only two percent of students enrolled in a public STEM program (column E). More-prepared students were also significantly more likely to choose private STEM programs if they did not enroll in a public STEM program. In the lower deciles of academic preparation, the majority of students did not enroll in any college program, and STEM enrollment was less common conditional on attending college.

Second, public STEM programs have the lowest graduation rates of all bachelor's programs, and STEM degree completion is especially rare for less-prepared students. Panel B shows the fraction of enrollees who earned a degree by 2012 in each program group. In the top decile of academic preparation, 49 percent of public STEM enrollees completed the program. In the sixth decile, only 22 percent of public STEM enrollees graduated by 2012. Students who enrolled in other college programs were more likely to graduate within each decile, but this gap in graduation rates is larger for less-prepared students.[8]

Third, the earnings premium of a public STEM degree relative to other degrees is larger for less-prepared students. Panel C shows log monthly earnings in 2017 for students who completed a degree in each program group. In the most-prepared decile, mean earnings for public STEM graduates were 0.05 log points higher than those for graduates from all other programs. The earnings gap between public STEM graduates and all other degree holders grows wider as academic preparation decreases, reaching 0.14 log points by the sixth decile. This pattern is driven by two factors: 1) more-prepared students have relatively better earnings outcomes in other degree programs; and 2) more-prepared graduates are more concentrated in the higher-paying programs within the "all other" group.

In Panel D of Table 1, we summarize all of these patterns by estimating a return to public STEM *enrollment* conditional on individual characteristics. For this panel, we regress log

[7] We focus on the top five deciles because few students in lower deciles attend public STEM programs.

[8] The STEM graduation rates in Table 1 are comparable to those in the University of California system. Arcidiacono et al. (2016) show that science degree completion rates at UC schools range from 40–60 percent for students in the top quartile of academic preparation, and from 10–20 percent for bottom-quartile students.

monthly earnings on a dummy for enrolling in a public STEM program and a large vector of individual covariates, which includes demographic variables, ICFES subject scores, and high school dummies. This regression measures the earnings return to enrolling in a public STEM program relative to a weighted average of all other college options, including not attending college at all. Thus, these estimates incorporate both the variation in program choices (Panel A) and the variation in degree attainment rates (Panel B).

The results in Panel D suggest that less-prepared students may have larger returns to *enrolling* in public STEM programs, despite their lower graduation rates. For the most-prepared students, the earnings premium to public STEM enrollment is modest at 1.3 percent. This premium is substantially larger in the other deciles, and it is 7–8 percent for the least-prepared students. Of course, this finding should be interpreted with caution because our covariates, while comprehensive, may not fully account for selection into STEM enrollment. This caveat motivates the rest of our paper, which focuses on one public university for which we can more credibly identify heterogeneity in the returns to STEM enrollment.

2. Univalle data and sample

2.1. **Univalle.** Our main analysis focuses on a public flagship university in Colombia called *Universidad del Valle*, or "Univalle" for short. Univalle is located in Cali—the country's third largest city and the capital of the Valle del Cauca region. Like other Colombian flagship schools, Univalle is the largest and most selective university in its region. In national university rankings, Univalle frequently places in the top 10.

Univalle offers roughly 60 undergraduate majors each year in STEM and other fields. Students apply to specific programs and admission is based solely on the ICFES exam. Univalle's admission scores are weighted averages of applicants' scores on the ICFES subject tests, with weights that vary across programs. Applicants with the highest scores are admitted up to a cutoff that is determined by the quota for each program. Below we exploit variation in both quotas and admission score weights to explore heterogeneity in the returns to Univalle's STEM programs.

2.2. **Sample.** We collected data on admission scores and admission decisions for all applicants to Univalle's undergraduate programs from Fall 1999 to Spring 2004 (Univalle, 2017). We linked the Univalle data to the national administrative datasets described in Section 1.1 (see Appendix C.1). This allows us to observe applicants' college enrollment and graduation outcomes even if they did not attend Univalle, as well as their formal earnings in the year 2017.

We focus primarily on 18 of Univalle's bachelor's degree programs in STEM subjects, which we define as those in its engineering and natural science faculty areas. This includes Biology, Chemistry, Mathematics, Physics, Statistics, and a dozen different engineering programs. For comparison, we also present results for 30 non-STEM programs, which include Economics and some health programs where the classification is less clear. Our definition follows the standard classification of STEM majors in Canada, which has pre-professional undergraduate health programs that are similar to those in Colombia (Statistics Canada, 2017).[9]

Table 2 presents summary statistics for our analysis sample, which includes all Univalle applicants who faced the standard admission criterion. We exclude applicants in special admission groups who were not subject to the primary admission thresholds (e.g., disabled or indigenous applicants).[10] We also exclude applicants with missing ICFES scores and those who applied to program/cohort pairs for which all applicants were accepted. Our full sample includes 16,022 STEM applicants (column A) and 23,439 non-STEM applicants (column D). Roughly one-third of applicants were admitted, and nearly 90 percent of admitted students chose to enroll (columns B and E). Columns (C) and (F) show our benchmark RD sample, which includes applicants within 30 positions of the admission cutoff.

Univalle STEM applicants were very high achieving relative to the population of high school graduates. The mean STEM applicant scored at the 86$^{\text{th}}$ percentile of the ICFES exam (averaged across all subjects), and admitted STEM students scored at the 93$^{\text{rd}}$ percentile on average. STEM applicants were also disproportionately male and from high-income families.

3. Mean returns to Univalle STEM programs

3.1. **Regression discontinuity (RD) model.** We begin by estimating the average return to enrolling in Univalle's STEM programs for applicants on the margin of admission. For this we use a two-stage least squares (2SLS) RD model:

$$E_{ip} = \theta D_{ip} + \alpha x_{ip} + \psi D_{ip} x_{ip} + \gamma_p + \epsilon_{ip} \quad \text{if } |x_{ip}| \leq h \tag{1}$$

$$Y_{ip} = \beta E_{ip} + \tilde{\alpha} x_{ip} + \tilde{\psi} D_{ip} x_{ip} + \tilde{\gamma}_p + \tilde{\epsilon}_{ip} \quad \text{if } |x_{ip}| \leq h. \tag{2}$$

Y_{ip} is an outcome for individual i who applied to Univalle in application pool p. Application pools are defined by a program and semester of application and, in some years, by the applicant's version of the ICFES exam.[11] The endogenous treatment variable, E_{ip}, is an indicator equal to one if the applicant enrolled in the Univalle program that they applied to. We instrument for Univalle enrollment with D_{ip}, which is an indicator for having an

[9] We exclude 2–3 year programs that terminate in technical degrees and programs that did not use ICFES scores for admissions (e.g., Music). See Appendix C.2 for details on our sample and the included programs.
[10] This follows Abdulkadiroğlu et al. (2014)'s approach in defining "sharp" RD samples, in which admission is equivalent to having a score above the threshold.
[11] The ICFES exam underwent a major reform in 2000 (Riehl, 2023), so from 2000–2002 some programs allowed students to apply with either old or new ICFES scores. Our regressions include dummies for program/cohort/ICFES-version triples because these variables define the relevant admission threshold.

admission score above the threshold for that application pool. We estimate equations (1)–(2) separately for applicants to Univalle's STEM and non-STEM programs.

We use a local linear specification to estimate returns for applicants on the margin of admission. Equations (1)–(2) include the running variable, x_{ip}, which is an individual's rank in their application pool normalized to equal zero for the last student above the threshold. Following Pop-Eleches and Urquiola (2013) and Abdulkadiroğlu et al. (2014), we include an interaction between x_{ip} and D_{ip}, as well as fixed effects for each application pool, γ_p. Our regression samples focus on the subset of applicants whose admission ranks are within h positions of the admission thresholds, i.e., $|x_{ip}| \leq h$. Our benchmark model uses $h = 30$, which is roughly the mean of the Calonico et al. (2014) bandwidths across all dependent variables. Appendix Tables A1–A2 show that our main results are similar using bandwidths of 15 or 45 positions, and also if we use the Calonico et al. (2014) bandwidth for each outcome. Some students appear in our sample multiple times because they reapplied to Univalle, so we cluster standard errors at the individual level.

3.2. **Identification assumptions and balance tests.** Our identification relies on the standard RD, instrumental variable, and local average treatment effect (LATE) assumptions.

The main RD assumption is that individuals' admission ranks are effectively randomly assigned near the cutoffs (Lee and Lemieux, 2010). Although students likely have an idea about the program's quota and standards, their exact rank and admission outcome are uncertain because of the presence of other applicants. Appendix Table A3 provides support for this assumption from balance tests that use applicants' observable characteristics as outcome variables in our RD specification. We find no evidence that ICFES scores, age, socioeconomic background, or gender change discontinuously at the admission thresholds, and we cannot reject the hypothesis that the RD coefficients for all characteristics are jointly zero. This table also shows balance on predicted outcomes based on these characteristics; the point estimates for predicted outcomes are close to zero and are statistically insignificant. Lastly, we find no evidence that the density of admission scores changes discontinuously at the admission thresholds using the McCrary (2008) test (Appendix Figure A1).

We also make the standard instrumental variable and LATE assumptions (Angrist et al., 1996). Instrument relevance is satisfied because the probability of Univalle enrollment increases sharply at the admission thresholds, as we show below. The exclusion restriction states that crossing the admission threshold affects individuals' outcomes only through the channel of Univalle enrollment. This could be violated if, for example, admission to Univalle caused individuals to apply to other schools. We cannot rule out this possibility, but we believe our results are primarily attributable to Univalle enrollment because the first-stage coefficient is large.[12] The monotonicity assumption requires that there are no applicants who would enroll in a Univalle program if and only if they were just *below* the admission threshold, which is plausible in our setting.

Under these assumptions, the β coefficient from equation (2) can be interpreted as the average causal effect of enrolling in Univalle for a population of marginally-admitted "compliers," which are students who would have enrolled if and only if they scored above the cutoff. In regressions with log earnings as the dependent variable, β identifies the complier average return to enrolling in a Univalle program.

3.3. **Mean returns.** Table 3 presents our RD estimates of the mean returns to Univalle enrollment. Panel A displays the first stage coefficients, θ, from equation (1). Panel B presents 2SLS coefficients, β, from equation (2) for three dependent variables: 1) an indicator for graduating from the Univalle program by 2012; 2) an indicator for formal employment in 2017, defined as appearing in our administrative earnings records; and 3) log monthly earnings in 2017 (conditional on formal employment). Columns (A)–(B) show results for STEM applicants, and columns (C)–(D) show results for applicants to other programs. Columns (A) and (C) display means of each dependent variable for marginally-rejected compliers, which we estimate following Katz et al. (2001).[13] Columns (B) and (D) display the RD coefficients. We present corresponding RD graphs in Figure 1. The x-axis in each panel is our running variable, x_{ip}, and markers represent means of the dependent variables in eight unit bins of x_{ip}. We display separate means for STEM programs (red circles) and other programs (hollow triangles), as well as predicted values from non-parametric regressions.

Panel A of Table 3 shows that the large majority of marginally-admitted applicants chose to enroll in Univalle. Crossing the admission threshold increased the probability of Univalle enrollment by 75 percentage points for STEM applicants (column B) and 78 percentage points for other applicants (column D). These large first-stage estimates reflect the fact that Univalle is the top choice for many students from the Valle del Cauca region.

The first row of Panel B shows that the graduation rate for marginal compliers was significantly lower in Univalle's STEM programs than in its other programs. Only 34 percent of marginally-admitted compliers completed the STEM program by 2012. In Univalle's other programs, the graduation rate for marginal compliers was more than 15 percentage points higher, at roughly 50 percent. The low STEM graduation rate is striking because STEM

[12] Our exclusion restriction is weaker than that in related work that use peer characteristics (Abdulkadiroğlu et al., 2014) or degree completion (Kirkebøen et al., 2016) as the endogenous regressor.

[13] Specifically, the "control complier means" in columns (A) and (C) are the means for treated (i.e., admitted) compliers minus the 2SLS RD coefficients in columns (B) and (D). In Panel A, columns (A) and (C) show the mean enrollment rate for all applicants who were 1–5 positions below the admission thresholds.

admits tend to have higher pre-college test scores than other Univalle admits (Table 2), but it is consistent with the national patterns in Table 1.

Yet despite lower rates of degree completion, STEM applicants had *higher* mean earnings returns to attending Univalle than applicants to other programs. In the last row of Panel B, we find that enrolling in a Univalle STEM program raised individuals' 2017 monthly earnings by 14 percent on average (column B). By contrast, the RD estimate of the earnings return in other programs is slightly negative (but insignificant), and we reject equality of the STEM and non-STEM coefficients ($p = 0.02$). Panel D of Figure 1 shows RD graphs for the reduced-form earnings effects; for STEM applicants (red circles), there is a discontinuous increase in log mean earnings at the admission thresholds.

Appendix Figure A2 provides evidence that our estimated earnings return for STEM applicants is unlikely to have arisen by chance. In this figure, we follow Beuermann and Jackson (2022) in comparing the actual reduced-form RD earnings estimate for STEM applicants to a distribution of placebo RD coefficients based on randomly-chosen cutoffs in each application pool. The actual RD coefficient is at the 97.5^{th} percentile of the placebo distribution.

Our STEM graduation and earnings estimates are similar in magnitude to those in related literature. The 34 percent graduation rate for marginal STEM compliers is comparable to persistence rates for less-prepared students at U.S. flagship schools. At UC Berkeley, for example, Arcidiacono et al. (2016) find that only 28 percent of minority students with an initial major in science earned a degree within five years. Using Norwegian data, Kirkebøen et al. (2016) find that the mean earnings premium of science and engineering degrees relative to individuals' next-choice fields is roughly 40 percent.[14] This is consistent with our mean earnings return of 14 percent given that our estimates reflect returns to STEM enrollment rather than degree attainment, and that only one-third of enrollees complete a degree.

3.4. **Potential impact of informal earnings.** An important caveat for our earnings results is that our data do not include informal sector jobs. In Panel B of Table 3, we do not find significant effects of Univalle enrollment on the likelihood of formal employment (defined as appearing in our earnings data), but the point estimates suggest a sizable positive effect in both samples. Appendix Table A4 provides summary statistics on informal employment and earnings using data from a Colombian household survey called the *Gran Encuesta Integrada de Hogares*, or GEIH (GEIH, 2019). 20 percent of workers with a bachelor's degree were employed in the informal sector in 2017, and this proportion rises to 50 percent for workers with only a high school degree. For workers with similar ages and educational attainment as those in our sample, mean earnings in the formal sector are roughly double those in the

[14] In Kirkebøen et al. (2016), the average payoffs to science and engineering degrees are roughly 23,000 U.S. dollars (Figure IX) and mean earnings in their sample is 56,000 U.S. dollars (Table III).

informal sector. Thus, our earnings estimates in Panel B may understate the return to Univalle enrollment because they exclude earnings in informal jobs, which are likely to be lower on average.

To explore the sensitivity of our results to the informal sector, Panel C of Table 3 displays RD estimates in which we impute informal earnings under different assumptions. We first use the 2017 GEIH surveys to compute mean informal monthly earnings for workers with a given birth year, gender, and highest degree (high school, technical, or bachelor's). Next, we assume that all individuals in our sample with missing formal earnings are employed in the informal sector, and we impute their informal earnings using the GEIH means. Lastly, we make three different assumptions on the causal impact of Univalle enrollment on informal earnings: 1) no impact on informal earnings ($\beta^{\text{Informal}} = 0$); 2) the same impact on informal earnings as on STEM formal earnings ($\beta^{\text{Informal}} = 0.133$ in both samples); and 3) twice the STEM formal sector impact ($\beta^{\text{Informal}} = 0.266$). This last assumption is likely to overstate the impact of informal earnings on our results because some individuals are out of the labor force, and because the standard deviation of earnings is much lower in the informal sector (Appendix Table A4).

The results in Panel C show that informal earnings are unlikely to change our finding that Univalle's STEM programs had higher mean returns than its other programs. Under the assumption that STEM enrollment has no causal effect on informal earnings, our RD estimate that includes imputed earnings (0.136) is nearly identical to the estimate on formal sector earnings (0.133). In other programs, the estimate with imputed earnings is close to zero (column D), as the increased access to higher-paying formal jobs offsets the negative point estimate for formal sector earnings in Panel B. Even in our most extreme scenario—which assumes that Univalle enrollment raises informal earnings by 0.266 log points—the return to non-STEM programs is lower than the smallest of our STEM point estimates.

In sum, this section showed that enrollees in Univalle's STEM programs had high mean earnings returns despite low graduation rates. We next ask if a similar pattern holds *within* STEM programs for students with different levels of academic preparation.

4. Heterogeneity in STEM returns among marginal admits

4.1. **Variation in academic preparation among marginal admits.** Our goal in this section is to examine how the earnings returns to Univalle's STEM programs vary with students' academic preparation. We define academic preparation as the likelihood that a student is able to complete the program requirements and earn a degree. This focus is motivated by the debates over STEM admission policies, which center on the question of whether students with relatively less academic preparation benefit from admission to selective STEM programs (e.g., Bleemer and Mehta, 2021).

Our heterogeneity analysis exploits the fact that our data contains additional information on an individual's likelihood of graduating beyond their admission score. Univalle's admission scores are program-specific weighted averages of individuals' scores on the ICFES exam, which included up to nine subjects during the period of our data. As we show below, the weights chosen by the admission committees differ from those that best predict which students would graduate. This allows us to divide our sample into two groups with similar admission scores, but different likelihoods of earning a degree. Our approach follows Cunha et al. (2010) in anchoring test scores to an outcome of interest, and it mirrors Abdulkadiroğlu et al. (2014) in using additional test scores to examine heterogeneity in RD estimates.

Table 4 illustrates this variation by showing how the ICFES scores relate to four outcomes: 1) the Univalle admission score; 2) an indicator for graduating from the Univalle program; 3) scores on a field-specific college *exit* exam;[15] and 4) log monthly earnings in 2017. We regress each outcome variable on the nine ICFES subject scores using a sample of Univalle enrollees.[16] We run these regressions separately for each of Univalle's 48 programs and normalize the estimated coefficients to sum to one. The admission score regressions recover the program-specific weights set by the admission committees. The other regression coefficients give the best linear predictors of graduation, exit scores, and earnings. Columns (A)–(D) show the subject weights for each outcome averaged across Univalle's 18 STEM programs. For comparison, columns (E)–(H) show the mean weights in other programs.

Table 4 shows that admission to Univalle's STEM programs was based primarily on quantitative subject scores, but an admission score that maximized graduation rates would have placed even *more* weight on these quantitative subjects. Column (A) shows that the typical STEM program placed 65 percent of the weight in admissions on the four quantitative ICFES subjects (biology, chemistry, math, and physics). However, the best linear predictor of STEM graduation places 81 percent of the weight on quantitative subjects (column B). This is largely driven by the ICFES chemistry exam, which was highly predictive of STEM degree completion.[17] Other Univalle programs placed more weight on qualitative subjects in admissions (column E), and yet quantitative subject scores were again relatively more informative for degree completion (column F).

[15] We match our sample of Univalle applicants to administrative data on a national field-specific college exit exam called *Saber Pro* (ICFES, 2013b). The exit exam was voluntary during the period of our data, but most Univalle graduates took it. See MacLeod et al. (2017) for details on the Colombian exit exam.

[16] Table 4 shows results for the subjects offered on the post-2000 ICFES exam. Appendix Table A5 shows similar patterns for subjects on the pre-2000 ICFES exam, but the weights are more noisily estimated since these applicants are a small portion of our sample.

[17] The weights in Table 4 reflect the impact of a higher subject score *conditional* on the other subject scores, which may partly explain why some weights are negative (Kinsler and Pavan, 2015). These weights are also noisily estimated due to small sample sizes in each program.

Thus conditional on the admission score, Univalle enrollees were more likely to graduate if they scored relatively higher on quantitative subjects than on qualitative subjects. This suggests that Univalle may have valued other outcomes beyond graduation in choosing which students to admit.[18] The admission weights are most similar to those that predict graduates' performance on the field-specific exit exam (columns C and F in Table 4), suggesting that Univalle prioritized students who could master the course material. While this is one important aspect of earning a degree, many other factors influence graduation rates—including motivation, perseverance, and financial resources—and these factors may not be well measured by a test of course content (Jackson, 2018; Beuermann et al., 2023). Notably, the weights that predict STEM graduation (column B) are most similar to those that predict earnings (column D), which shows that the skills that affect STEM degree completion are rewarded in the labor market.

4.2. **Heterogeneity in returns.** We estimate heterogeneity in returns by splitting our sample into applicants with higher and lower graduation propensities conditional on admission scores. Specifically, we use the subject score weights from column (B) of Table 4 to define a program-specific measure of an applicant's propensity to graduate from Univalle. We regress this measure of graduation propensity on individuals' admission ranks with application pool dummies and take the residuals from this regression. Lastly, we split our sample into two groups based on the median value of these residuals in each application pool.[19]

Table 5 shows RD coefficients from equations (1)–(2) estimated separately for less- and more-prepared STEM applicants as defined by graduation propensity. Column (A) shows the mean of each outcome for marginally-rejected STEM compliers with below-median graduation propensity, and column (B) shows the RD coefficient in this sample. Columns (C)–(D) are similar, but the sample includes more-prepared STEM applicants, defined as those with above-median graduation propensities. Column (E) reports the p value from an F test that the RD coefficients for less- and more-prepared applicants are equal. Figure 2 shows RD graphs that correspond to these regressions; this figure is similar to Figure 1, except we include only STEM applicants and plot outcomes separately for our more-prepared (black triangles) and less-prepared (red circles) samples.

We find that, relative to more-prepared students, less-prepared students were less likely to complete a Univalle STEM degree if they enrolled. Our two samples had similar rates of Univalle enrollment when they gained admission (Panel A of Table 5), but more-prepared

[18] Another possibility is that the admission committees were uncertain about the true relationship between the subject scores and potential outcomes.

[19] We use leave-cohort-out weights to define our less- and more-prepared samples to avoid a mechanical correlation with graduation rates. Specifically, to define the two samples for program m and cohort t, we estimate the ICFES score weights using all enrollees in program m in cohorts *other than* t.

applicants were much more likely to complete the Univalle STEM program (first row of Panel B). Among marginally-admitted compliers, 38 percent of students with above-median graduation propensities completed the Univalle STEM program, as compared with 29 percent of below-median students. (See also Panel B of Figure 2.) This shows that our measure of graduation propensity contains information on students' likelihood of earning a STEM degree, as intended.

Our main finding is that, despite lower completion rates, less-prepared applicants had significantly *higher* earnings returns to enrolling than more-prepared applicants. The last row of Panel B presents 2SLS estimates of the impact of Univalle enrollment on log monthly formal earnings. The earnings gain was 0.24 log points (28 percent) for marginal STEM compliers with low graduation propensities (column B), while it was only 0.03 log points (3 percent) for those with higher graduation propensities (column D). This shows that the positive mean return to STEM enrollment in Table 3 was driven primarily by students with relatively less academic preparation. We can reject that the less- and more-prepared returns are equal at the 10 percent level (column E).[20] Panel D of Figure 2 shows the graphical version of this result; there is a large discontinuity in earnings at the admission thresholds for less-prepared applicants, and no evidence of a discontinuity for more-prepared applicants. Panel B of Appendix Figure A2 shows that the actual reduced-form RD earnings estimate for less-prepared applicants is larger than 98.9 percent of placebo RD coefficients based on randomly-chosen cutoffs.

Panel C of Table 5 examines the potential impact of informal earnings on these results using the same imputation methods as in Table 3. For less-prepared applicants, the estimated earnings returns are not very sensitive to assumptions on informal earnings because the RD estimate for formal employment is close to zero (second row of Panel B). For more-prepared applicants, the impact of Univalle enrollment on the likelihood of formal employment is larger (though not significant) at 4.4 percentage points, and so the imputation assumptions are more consequential. The results in Panel C suggest that our estimate for formal sector earnings may understate the true earnings impact for more-prepared applicants once we account for informal earnings. Yet even when we assume a very large informal sector return ($\beta^{\text{Informal}} = 0.266$), the point estimate for more-prepared applicants (0.129) is half of the magnitude of our main formal sector estimate for less-prepared applicants (0.244). This suggests that informal earnings are unlikely to alter our main finding.

[20] If the earnings effects in Table 5 were driven solely by degree attainment, they would imply a return to a STEM degree of 0.85 log points for less-prepared applicants. Although this is a large return, it is within the range of many of the estimated payoffs to field of study in Kirkebøen et al. (2016) (see their Figure VIII). Appendix Figure A4 shows the full distribution of treatment effects on earnings following Abadie (2002)'s method. Appendix Figure A5 displays RD graduation and earnings effects by academic preparation estimated separately for each of the 18 STEM programs in our sample.

The results in Table 5 are robust to different measures of academic preparation. Appendix Table A7 shows that our findings are similar when we define academic preparation using the relationship between ICFES subject scores and graduation in our national higher education data, rather than in our Univalle sample. This table also shows results from a specification in which we use predicted earnings to divide our sample rather than graduation propensity; we find that students with lower predicted earnings also have higher returns to STEM enrollment, although the heterogeneity is not statistically significant. Appendix Table A8 shows that graduation rates for were lower for applicants who applied with post-2000 ICFES scores than those who applied with pre-2000 ICFES scores, consistent with Riehl (2023)'s findings that the 2000 ICFES reform reduced the informativeness of the scores. However, we continue to find lower graduation rates and higher earnings returns for less-prepared STEM applicants within the samples that took each version of the exam. This pattern is also unique to Univalle's STEM programs; in non-STEM programs, we find smaller differences in graduation rates between less- and more-prepared applicants, and we do not find significant earnings returns in either sample (see Appendix Table A6).

The remainder of this section considers mechanisms for why less-prepared applicants had higher earnings returns to Univalle's STEM programs. All else equal, students who are more likely to complete a Univalle STEM degree would have larger returns to enrollment if that degree improves an individual's labor market prospects. But all else may not be equal for less- and more-prepared applicants. Appendix B presents a simple framework that illustrates different channels through which the returns to attending a selective STEM program may vary with academic preparation. Below we present evidence on two of these channels: counterfactual schooling options and heterogeneity in skill accumulation.

4.3. **Counterfactual schooling options.** One potential explanation for our results is that less- and more-prepared applicants may have differed in their next-choice college programs. Our RD estimates in Table 5 depend not only on the realized outcomes of marginal Univalle enrollees, but also on their counterfactual outcomes if they had been rejected. Related research argues that students whose academic preparation is relatively less than that of their classmates would be more likely to graduate with a STEM degree if they attended less selective schools (Arcidiacono et al., 2016). But less- and more-prepared applicants may differ in their likelihood of enrolling in other STEM programs when they are rejected because of differences in their preferences, financial resources, or ability to gain admission elsewhere. Even if less-prepared students choose programs in the same field of study, they may tend to enroll in colleges with low average earnings outcomes. For example, some less-prepared students may have been unable to afford tuition at selective private colleges since they often came from lower-income families.[21]

Our administrative data allows us to examine this mechanism because it includes enrollment and graduation at nearly all Colombian colleges through 2012. We define outcome variables that reflect characteristics of the college programs that Univalle applicants attended and the degrees they attained. Table 6 shows RD results estimated separately for less- and more-prepared STEM applicants, defined in the same way as for Table 5. Column (A) shows the mean of each outcome for marginally-rejected compliers in our less-prepared sample, and column (B) shows the 2SLS RD coefficient. Columns (C)–(D) are analogous but use our more-prepared sample. Column (E) displays the p value from an F test of equality of the RD coefficients in columns (B) and (D).

We find that, relative to more-prepared applicants, less-prepared applicants were less likely to enroll in other STEM bachelor's (BA) programs if they were rejected from Univalle. The first row of Panel A shows that 43 percent of marginally-rejected compliers in our less-prepared sample enrolled in another STEM bachelor's degree program (column A), as compared with 51 percent of more-prepared applicants (column C). As a result, the causal impact of enrolling in Univalle on the likelihood of attending *any* STEM bachelor's program was significantly larger for less-prepared applicants than for more-prepared applicants. Univalle enrollment raised the probability of enrolling in a STEM bachelor's program by 57 percentage points for less-prepared compliers (column B), as compared with 49 percentage points for more-prepared compliers (column D).

The other rows in Panel A show that, relative to more-prepared applicants, less-prepared applicants were more likely to pursue technical degrees—or forgo college altogether—if they were rejected from Univalle. Roughly one in six marginally-rejected compliers never enrolled in college, and this proportion was slightly higher in the less-prepared sample (19 percent) than in the more-prepared sample (14 percent). More significantly, admission to Univalle shifted less-prepared students from technical to bachelor's degree programs. Univalle enrollment reduced the probability of pursuing a technical degree by 13.5 percentage points among less-prepared students (column B), and by only five percentage points for more-prepared students (column D).

These different enrollment choices meant that the causal impact of attending Univalle on the likelihood of completing *any* STEM bachelor's degree was the same for less- and more-prepared students. The first row of Panel B shows that enrolling in a Univalle STEM program raised the likelihood of completing any STEM bachelor's degree by 24.5 percentage points for less-prepared compliers, and by 25.2 percentage points for more-prepared compliers. Thus

[21] Univalle, like most public universities in Colombia, offers tuition discounts to low SES students. Financial aid for private colleges typically did not exist during the period of our data.

while less-prepared students graduated from Univalle's STEM programs at lower rates than more-prepared students, they also earned STEM degrees from other colleges at lower rates, and these two effects offset. Univalle enrollment also had similar impacts on less- and more-prepared applicants' likelihood of completing any college degree.

To examine how these enrollment choices relate to our earnings estimates, Panel C of Table 6 uses dependent variables that measure the average earnings of other students in an individual's college program. We compute the (leave out) log mean earnings in 2017 for all students in our administrative data who enrolled in an applicant's college, major, and college/major pair between 1998 and 2003.[22] These outcomes are computed using both drop-outs and graduates, and thus provide a measure of the expected earnings outcome for a typical enrollee in each program.

In each case, we find that, relative to more-prepared applicants, less-prepared applicants attended programs with lower mean earnings outcomes when they were rejected from Univalle. The means for marginally-rejected students are lower in column (A) than in column (C), and, as a result, Univalle enrollment had a larger impact on program mean earnings for less-prepared applicants than for more-prepared applicants (columns B vs. D). For example, Univalle enrollment caused less-prepared applicants to attend college/major pairs in which expected earnings for a typically enrollee was 0.19 log points higher, while this earnings premium was only 0.13 log points more-prepared applicants.

As further evidence that counterfactual program choices contribute to our results, Appendix Tables A9–A10 examine heterogeneity in returns by gender and type of STEM program. The earnings returns to STEM enrollment are larger for men than for women, and they are larger for Univalle's engineering programs than for its natural science programs. In both cases, the causal impact of Univalle enrollment on program mean earnings is larger for the group with the larger earnings returns (men and engineering applicants). Further, the RD estimates for individual and program-mean earnings are larger for less-prepared applicants within each of these subsamples.

The results in Table 6 can partly explain the heterogeneity in our earnings results from Table 5. All of the outcomes in Table 6 suggest that, relative to more-prepared applicants, less-prepared applicants tended to have lower-paying degree programs as their next-choice option. If the estimates in Panel C reflect the causal effects of program enrollment, then counterfactual choices alone would lead the earnings return to Univalle enrollment to be five percent higher for less-prepared students than for more-prepared students. This is a strong assumption, although it is broadly consistent with the key identification assumption in Chetty et al. (2020)'s analysis of returns to U.S. colleges. Our results run counter to the

[22] For applicants who did not attend college, we use the leave-out mean earnings of all non-college enrollees.

hypothesis that less-prepared students would be more likely to earn STEM degrees if they were rejected from selective STEM programs (Arcidiacono et al., 2016).

At the same time, alternative schooling options may not fully explain the heterogeneity in returns. Our estimates in Table 5 suggest that the earnings return to Univalle enrollment was 0.21 log points higher for less-prepared applicants than for more-prepared applicants. Although these coefficients have large standard errors, the gap in earnings returns is significantly larger than the gaps in the effects on program mean earnings in Panel C of Table 6. Thus, other mechanisms may play a role in explaining the heterogeneity in returns.

4.4. **Skill accumulation from a STEM degree.** A second potential explanation for our results is that skill accumulation from completing a STEM degree is higher for less-prepared students than for more-prepared students. This hypothesis states that—within the sample of students who graduated with a Univalle STEM degree—those with relatively less academic preparation accumulated more skill over the course of the program. This could arise, for example, if less-prepared applicants had to study more than their peers in order to earn a degree.

To examine this possibility, we begin by plotting the earnings of Univalle STEM graduates and drop-outs in Panel A of Figure 3. The x-axis in this panel is an individual's graduation propensity, defined as in column (B) of Table 4. The y-axis is the individual's log monthly earnings in 2017. We plot outcomes in two samples: individuals who completed a Univalle STEM degree and individuals who enrolled in, but dropped out of these programs. Markers depict means in ventiles of graduation propensity, and dashed lines are predicted values from local linear regressions.

The results in Panel A are consistent with the hypothesis that the skill accumulation from a STEM degree is larger for students who are less likely to graduate. Among drop-outs, mean earnings in 2017 were more than 20 percent higher for the most-prepared students than for the least-prepared students (14.2 vs. 14.0 log points). Yet the relationship between earnings and graduation propensity is surprisingly flat in the sample of Univalle STEM graduates. Thus, the earnings gap between graduates and drop-outs is larger for less-prepared students.

Why do less- and more-prepared graduates have similar earnings? To shed light on this, we take advantage of Univalle transcript data for five engineering programs in our sample.[23] These data contain students' grades in every course at Univalle. Panel B of Figure 3 plots the mean grade point average (GPA) that graduates of these programs received in each year in the program. To compute GPA, we include only courses that were required for the major, and we group courses based on the year in which students typically take them. This

[23] Our transcript data cover 2000–2001 enrollees in Univalle's Chemical, Electrical, Electronic, Materials, and Mechanical Engineering programs. See de Roux and Riehl (2022) for details on this transcript data.

includes roughly 40 required courses in each program, most of which teach topics in math or engineering. Lines depict the non-parametric relationship between GPA in each year (y-axis) and graduation propensity (x-axis). Importantly, Panel B shows how the grade distribution changed over time in a fixed sample since we include only students who earned a degree.

The patterns in Panel B suggest that less-prepared graduates "caught up" academically with their more-prepared peers over the course of the program. In first-year courses (solid line), there is a strong relationship between GPA and academic preparation, with GPA increasing by 0.6 points across the distribution of graduation propensity.[24] This relationship is less pronounced for second-year GPA and is almost flat for GPA in years 3–5. It is possible that this flattening reflects changes in the nature of courses or grading across years, but Appendix Table A11 shows that the relationship between GPA and earnings is similar in each of the five years. Thus, Panel B suggests a narrowing of the gap between less- and more-prepared graduates in skills that are valued by the labor market.

A caveat is that the findings in Panel A of Figure 3 may be driven by differences in unobserved ability. Students who graduate are likely be positively selected on unobservables, and the degree of selection may be more pronounced for less-prepared students. We find a similar pattern of results after controlling for demographic and high school characteristics (Appendix Figure A6), but this does not rule out the possibility of unobserved selection. This caveat also applies to other research that asks how returns to completed STEM degrees vary with ability (Altonji, 1993; Arcidiacono, 2004; Webber, 2014; Kinsler and Pavan, 2015), which also relies on strong identification assumptions. But a standard discrete choice model of selection into graduation would *not* predict a flat relationship between graduation propensity and earnings, as we observe in Panel A. Indeed, the strong relationship between first-year GPA and graduation propensity in Panel B suggests that selection cannot fully account for the flat gradient in earnings. Thus, we believe this pattern is partly driven by less-prepared graduates learning more along the way to completing their degrees.

4.5. **Other potential mechanisms.** In addition to the two channels discussed above, there are other potential explanations for the heterogeneity in STEM returns. One possibility is that less-prepared students benefit from an informational channel if they are pooled with more-prepared students in the labor market. Although we do not observe early-career earnings, we do not think this signaling channel fully explains our results because the gap in earnings returns between less- and more-prepared applicants *increases* with potential experience in our sample (Appendix Figure A7).[25]

[24] In Colombia, grades are reported on a 0–5 scale with 3 representing a passing grade. 0.6 GPA points is approximately 60 percent of the standard deviation of STEM grades in our transcript data.

[25] We also do not think that graduate school plays a significant role in our findings. Our Ministry of Education data includes information on enrollment in graduate degree problems for a subset of the years

Another possibility is that students may benefit from taking STEM courses or from access to internships even if they do not graduate. Appendix Figure A8 shows that STEM applicants who did *not* earn a college degree had similar earnings in 2017 regardless of whether or not they were admitted to Univalle. This descriptive result is hard to square with the hypothesis that Univalle drop-outs had a significant earnings return. Consistent with these findings, Appendix Figure A9 shows that the treatment effects for less-prepared STEM applicants are much larger at higher earnings quantiles, which suggests that the positive returns are driven primarily by graduates. But we cannot rule out the possibility that our findings are partly driven by benefits of STEM enrollment for non-graduates.

Lastly, restrictions in the supply of both STEM programs and STEM jobs may cause wages to diverge from marginal product, which may disproportionally benefit less-prepared students. Although the Colombian higher education system features many private colleges that offer STEM programs, these programs are often perceived to be low quality (Carranza and Ferreyra, 2019). Our results on counterfactual program choices (Table 6) suggest that there is an undersupply of high-quality STEM programs relative to applicant demand. Further, many STEM graduates work in Colombia's mining and natural resource extract industries, which are heavily regulated and feature large state-owned firms (e.g., Ecopetrol). In Colombia, as in many Latin American countries, public firms typically pay high wages, and these wages may be less related to worker productivity. This may partly explain the flat relationship between earnings and academic preparation for STEM graduates in Figure 3.[26]

5. Heterogeneity in STEM returns from quota expansions

5.1. **Variation in admission quotas.** Our second approach to estimating heterogeneity in STEM returns exploits variation in Univalle's admission quotas across cohorts. Figure 4 shows the number of students who were admitted to each Univalle STEM program by semester of application. Univalle, like most Colombian colleges, offers cohorts that begin in either August or January, and the number of admits per semester and program typically ranged from 45–65 students. In six STEM programs, however, changes in admission policies caused these quotas to roughly double in certain cohorts. Our analysis in this section examines how the returns for marginally-admitted students changed when these six programs expanded their quotas. Thus our estimates from this strategy are directly informative for policies that seek to expand university STEM quotas (e.g., Holdren, 2013).

in our full dataset (2007–2011). Using our RD design, we find that enrolling in an undergraduate Univalle STEM problem has a small positive effect on the probability of pursuing a graduate degree in these years, but this effect does not vary significantly with academic preparation.

[26] A caveat is that if access to public firms is an important mechanism, our results may not generalize to developed countries that do not have large public/private sector wage gaps.

There were two reasons why these Univalle STEM programs expanded their quotas. First, the Biology and Systems Engineering programs changed their desired cohort size in some years.[27] Figure 4 shows that the Biology program (black circles) admitted cohorts of 80–100 students each fall from 1999–2001, but in January 2002 it began admitting cohorts of roughly 50 students each semester (except for another large cohort in Fall 2002). Similarly, Systems Engineering (gold squares) typically had cohorts of about 60 students, but in August 2001 it admitted more than 120 students.

The second source of quota variation came from changes in how Univalle conducted admissions. Programs typically would use separate admission pools when they offered both fall and spring cohorts. In certain years, however, four engineering programs "tracked" applicants into fall and spring cohorts all at once. Under tracking admissions, roughly the top 60 students in the application pool were admitted to an August cohort, while the next 60 students were admitted to a January cohort. Figure 4 shows that the Chemical, Electrical, and Electronic Engineering programs used tracking admissions in Fall 2000, while Mechanical Engineering did so in Fall 2001.[28] In each case, more than 120 students were admitted in the tracking year, with the lower half starting at Univalle in the following January. Thus, tracking caused the number of admits to roughly double.

For each of these six programs, students who would not have gained admission to Univalle in a typical year were offered admission to the cohorts with large quotas. The other 12 STEM programs in our sample had a relatively stable number of admits per cohort during this time, as illustrated by the dashed grey line in Figure 4.

5.2. **RD difference-in-differences specification.** We use an RD difference-in-differences (RDDD) specification that estimates how quota expansions impacted the returns to STEM enrollment for marginal admits. This specification comes from a two-step estimation procedure. The first step is similar to our 2SLS RD specification as defined by equations (1)–(2), except we interact *all* covariates with dummies for application pools, p (i.e., program/cohort pairs). Thus, this first step yields a 2SLS RD coefficient β_p for each application pool p. These β_p coefficients estimate the impacts of enrolling in a Univalle STEM program for compliers who are on the margin of admission in pool p.

The second step relates these RD coefficients to variation in admission quotas across programs and cohorts. In this step, we notate application pools, p, by the combination of program, m, and application cohort, t. The second-step regression uses the RD coefficients

[27] Univalle's Systems Engineering program is similar to what is often called a Computer Science major.

[28] de Roux and Riehl (2022) analyze the impacts of this tracking for marginal admits to the high- and lower-ability cohorts.

$\beta_p = \beta_{mt}$ as the dependent variable in a standard difference-in-differences model:

$$\beta_{mt} = \gamma_m + \gamma_t + \pi L_{mt} + \upsilon_{mt}. \quad (3)$$

This regression includes fixed effects for programs, γ_m, and semester of application, γ_t. The variable of interest, L_{mt}, is an indicator for admission pools with large quotas, which we define in two ways. First, we define L_{mt} to be a binary variable for program/cohorts with unusually large quotas, as indicated by the solid markers in Figure 4. Specifically, $L_{mt} = 1$ for the large-quota cohorts of the six programs listed in Figure 4 and $L_{mt} = 0$ for all other cohorts of these programs and for all other STEM programs. Second, we define L_{mt} as the number of admitted students in each program/cohort, as indicated by the y-axis values in Figure 4. In this case, L_{mt} is a positive number for all program/cohort pairs, but most of the variation is driven by the unusually large quotas. We divide L_{mt} by 60 in this second definition so that the magnitudes of our estimates reflect a typical quota expansion.[29]

The coefficient of interest, π, shows how the returns to STEM enrollment for marginal compliers, β_{mt}, changed when the quota increased. We follow Card and Krueger (1992) in weighting observations in equation (3) by the inverse squared standard errors of the RD coefficients.[30] We cluster standard errors at the program/cohort level, which is the level of variation in our treatment variable, L_{mt}. Our main outcomes of interest, Y_{ip}, are Univalle graduation and log monthly formal earnings in 2017.

5.3. **Effects of large quotas on the characteristics of marginal compliers.** In the programs and cohorts with quota expansions, marginal admits came from lower in the application pool than usual, and thus they tended to be less-prepared as defined by admission scores. Formally, the 2SLS RD coefficients that we use as dependent variables in equation (3), β_{mt}, represent the average return to enrolling in Univalle for compliers on the margin of admission, i.e., for students who would have enrolled if and only if they scored above the cutoff. Our RDDD strategy is explicitly designed to measure how these returns change when the characteristics of marginal compliers change. In the programs and cohorts with large quotas, we expect marginal compliers to be less-prepared as defined by their propensity to complete a STEM degree, and they may also be more likely to come from demographic groups that tend to have lower admission scores.[31]

To illustrate this compositional effect, Panel A of Table 7 shows how the characteristics of compliers near the admission thresholds for Univalle's STEM programs changed in the

[29] See Appendix Table A12 for details on our two definitions of L_{mt}.

[30] Appendix Table A13 shows that our results are similar if we estimate our RDDD specification in a single-step regression using individual-level observations. We derive this single-step regression by plugging equation (3) into our first-step 2SLS specification (equations 1–2).

[31] Quota expansions may impact the returns of marginal admits through channels other than individual heterogeneity, such as class size or peer effects. We consider these potential mechanisms in Section 5.5.

cohorts with large quotas. For this panel, we compute mean complier characteristics in each program/cohort (mt) using different individual traits, including our measure of graduation propensity. For reference, column (A) shows the mean characteristic of marginally-rejected compliers averaged over all programs and cohorts. In columns (B)–(D), we estimate the DD regression (3), but the dependent variables are the program/cohort-specific mean complier characteristics. Column (B) defines the variable of interest, L_{mt}, as a binary indicator for large cohorts. Column (C) defines L_{mt} as the number of admitted students divided by 60. Column (D) is similar to column (B), but we "stack" our dataset so that the π coefficients are identified *only* by comparing programs with quota expansions to those without expansions.[32] This follows De Chaisemartin and d'Haultfoeuille (2020)'s approach to addressing potential concerns with staggered DD designs.

The results in Panel A of Table 7 show that marginal compliers had significantly lower graduation propensity in cohorts with large quotas, as intended. The dependent variable in the first row is our measure of an applicant's propensity to graduate from their Univalle program based on their ICFES subject scores, as in Section 4.1. The graduation propensity of marginally-admitted compliers declined by six percentage points in the STEM programs and cohorts with large quotas, and this estimate is similar across our three specifications (columns B–D). The other rows of Panel A show that compliers near the threshold were also more likely to be female, less likely have a college educated mother, and less likely to be from a high-income family, although these demographic changes are mostly insignificant.

5.4. **Effects of large quotas on graduation rates and earnings returns.** Panel B of Table 7 presents our main RDDD results on how the returns for marginal STEM admits changed in the large-quota cohorts. We consider the same four outcomes as in our RD analysis: Univalle enrollment, Univalle graduation, formal employment in 2017, and log monthly earnings in 2017. Column (A) shows means of each outcome for marginally-rejected compliers, and columns (B)–(D) show the π coefficients from our RDDD specification (3). As in Panel A, we present results using both our binary and integers measures of L_{mt} (columns B and C), as well as our stacked specification (column D).

Our main finding is that when the STEM quotas expanded, the marginal enrollees were less likely to complete the STEM program, but they had *larger* earnings returns to enrollment. The second row of Panel B shows that the RD estimates for Univalle graduation declined

[32] We combine the six "treated" STEM programs into three groups based on the cohort(s) in which their quotas expanded: 1) Biology (Fall 1999–2002); 2) Chemical, Electrical, and Electronic Engineering (Fall 2000 only); and 3) Mechanical and Systems Engineering (Fall 2001 only). We then create three datasets that include all 12 "control" STEM programs plus the treated programs in each group. Lastly, we stack these three datasets and estimate the DD or RDDD specification with all covariates (except L_{mt}) interacted with dummies for these three groups. The coefficients in column (D) are a regression-weighted average of the coefficients that one would get from estimating equation (3) separately for each of the three groups.

by 15–19 percentage points in the programs and cohorts with large quotas (columns B–D). This is consistent with the lower graduation propensity of marginally-rejected students in these cohorts (Panel A) and it shows that our RDDD specification captures variation in students' academic preparation, as intended. Yet despite the decrease in completion rates, the RD estimates for log monthly earnings are much larger in the programs and cohorts with large quotas. The RDDD coefficients range from 0.18 to 0.39 log points (20 to 48 percent) depending on the definition of L_{mt}. The magnitude of these estimates should be interpreted with caution since the coefficients have large standard errors. But there is no evidence that the earnings returns to STEM enrollment declined in the large cohorts, and our earnings estimate is significant at $p < 0.10$ using our binary treatment variable (column B).[33]

Figure 5 shows a graphical version of the graduation and earnings results from Table 7. Each circle represents a program/cohort pair for the six Univalle STEM programs that had a large quota during our sample period (see Figure 4). The y-axis value is the 2SLS RD coefficient for graduation rates (Panel A) and log earnings (Panel B) estimated separately for each program/cohort. The x-axis in both panels shows the quota size for the program/cohort. We demean both variables at the program level, and show the linear relationship between them with a dashed line. Although the program/cohort-specific RD coefficients are noisy, there is a negative relationship between quota size and the graduation rates of marginal compliers (Panel A), and a *positive* relationship between quota size and the earnings returns of marginal compliers (Panel B).

To examine the key identification assumption of parallel trends, Figure 6 displays an event study version of our graduation and earnings results.[34] These event studies are not standard because the large quotas both "switch on" and "switch off" for the STEM programs in our sample (see Figure 4). Thus for this figure, we restrict the sample to the five Univalle engineering programs that had *exactly one* cohort with a large quota during our sample period, and we use the engineering programs without significant quota variation as the control group.[35] We estimate a modified version of the DD regression (3) in which the variables of interest are dummies for years $k \in \{-2, 0, 1, 2, 3\}$ relative to the large-quota cohort in the five treated programs. Thus the $k = 0$ coefficient corresponds to the cohort

[33] The first row of Panel B shows that quota expansions increased the likelihood that marginal admits chose to enroll in the Univalle STEM programs, suggesting that these applicants had less desirable next-choice options. (These estimates come from a reduced-form version of our RDDD specification.) We find no significant effects on formal sector employment rates.

[34] Our RDDD strategy relies on the RD and 2SLS assumptions discussed in Section 3.2, and also on the assumption that graduation rates and earnings would have followed parallel trends across Univalle programs in the absence of quota expansions. A violation of parallel trends would arise if, for example, there were industry-specific macroeconomic shocks that are correlated with L_{mt}. This is less likely given the haphazard timing of large quotas and the fact that our "control group" includes similar STEM programs.

[35] We exclude the Biology program from our event study analysis because it had four large cohorts and four normal-sized cohorts with overlapping timing (Figure 4).

with the large quota, while the coefficients for other values of k correspond to normal-sized cohorts before and after the large cohort.

Figure 6 shows that the large-quota cohort ($k = 0$) is an outlier in that is has both the lowest graduation rates and the highest earnings returns, but the test of parallel trends is inconclusive because the event study estimates are noisy. In Panel A, the graduation rate for marginal compliers in the cohort with the large quota ($k = 0$) is roughly 12 percentage points lower than that in both the preceding and the following cohort. Panel B shows that the large-quota cohort also had an earnings return for marginal compliers (0.31 log points) that was larger than that for any of the prior or following cohorts. Yet the event study coefficients are imprecise, especially at the tails of the graph where they are identified from only a few treated programs. Thus an important caveat for our RDDD results is that we are underpowered to present strong evidence for or against the parallel trends assumption.

Our finding that quota expansions increased the earnings returns of marginal enrollees is unique to Univalle's STEM programs. Appendix Table A14 replicates the RDDD analysis in Table 7 for non-STEM programs, which is possible because several Univalle business and architecture programs also expanded their quotas during our data period (see Appendix Table A12). We find that graduation rates and earnings returns do not change significantly with non-STEM quota expansions, which is consistent with our RD heterogeneity results.

5.5. **Mechanisms.** The results in Table 7 show that marginal enrollees in the larger cohorts of Univalle's STEM programs were less likely to earn a degree than those in smaller cohorts, but they had higher earnings returns to enrollment. This is consistent with our main result from Section 4 on the heterogeneity in STEM returns (Table 5), which suggests that the underlying mechanisms for this finding may be similar. In particular, since marginal admits to the larger cohorts were less prepared on average, they may have chosen lower-paying programs when they were rejected from Univalle, and they may have had a higher skill accumulation from a completed STEM degree.

Appendix Table A15 provides evidence that heterogeneity in counterfactual schooling options partly explains why earnings returns were higher for marginal students in large cohorts. This table uses our administrative higher education data to define outcome variables that reflect enrollment in other college programs, as in Table 6. We find that marginally-rejected students in cohorts with large quotas were less likely to enroll in other STEM bachelor's programs than those in normal-sized cohorts, and they also chose college programs with lower mean earnings (columns B–C). As a result, enrolling in a Univalle STEM program had a larger causal effect on overall STEM enrollment and on program mean earnings for students in larger cohorts (columns D–E). These results are similar to the findings in Table

6, suggesting that fallback programs are also an important mechanism for the impacts of quota expansions.[36]

Quota expansions also raise the possibility of peer and class size mechanisms. In our RDDD analysis, identification comes from programs that roughly doubled their admission quotas. This reduced the academic preparation of the *average* enrollee in large cohorts and, in some cases, led to larger class sizes.[37] This may have affected the graduation and earnings outcomes of Univalle enrollees through peer interactions (Sacerdote, 2001), professor responses to classroom composition (Duflo et al., 2011), or class size effects (Angrist and Lavy, 1999). Importantly, these mechanisms may impact the returns of both marginal and average enrollees.

Appendix Table A16 provides some evidence that peer and class size effects are not a significant driver of our results. For this, we define a sample of "top enrollees" in each Univalle STEM program; this sample contains students whose admission ranks were high enough such that they could have enrolled in *any* cohort of their program, regardless of the quota size. We then estimate simple DD regressions in this top enrollee sample using equation (3). We do not find significant effects on graduation rates or earnings outcomes in this specification, suggesting that the outcomes of top enrollees did not change differentially in the programs and cohorts with large quotas.

In sum, the similarity our results in Sections 4–5 suggests that heterogeneity in the academic preparation of marginal enrollees is the primary driver of our results in Table 7. While we cannot conclusively rule out other mechanisms related to quota expansions, such mechanisms are also relevant for policies that seek to increase the size of STEM programs at selective universities.

6. Conclusion

On many college campuses, STEM programs have a reputation for "weeding out" underperforming students through low grades. This reputation is consistent with a large literature that finds that academic preparation is especially important for completing a STEM degree (e.g., Stinebrickner and Stinebrickner, 2014). The evidence in our paper is consistent with this prior research. Using data from a flagship university in Colombia and two different empirical designs, we found that less-prepared students were significantly less likely to complete

[36] Since we only have Univalle transcript data for one cohort of each STEM program, we cannot use this data to examine heterogeneity in skill accumulation (as in Section 4.4).

[37] Our RDDD analysis also relies partially on variation in the time at which students enrolled. In the four programs with tracking admissions, marginal admits in the large cohorts had to wait approximately five months before enrolling (see Section 5.1). Thus, variation in returns between large- and small-quota cohorts could be partly affected by timing mechanisms such as learning decay (Cooper et al., 1996) or age-at-enrollment effects (Bedard and Dhuey, 2006).

the university's selective STEM programs than more-prepared students. Among admitted students with the lowest levels of academic preparation, more than 70 percent dropped out of the program.

On the other hand, our results show that raising admission standards may not necessarily increase the number of students who obtain *any* STEM degree. Using data from a national higher education census, we found that, relative to more-prepared applicants, less-prepared applicants were less likely to enroll in another STEM program when they were rejected. These counterfactual enrollment choices fully offset the impact of less-prepared students' lower graduation rates from the standpoint of STEM degree attainment. In other words, the causal impacts of admission to a selective STEM program on the likelihood of earning any STEM degree were similar for less- and more-prepared applicants.

Our paper also shows why less-prepared students might choose to enroll in selective STEM programs despite lower completion rates. We found that the earnings returns to STEM enrollment measured roughly 15 years later were *higher* for less-prepared students than for their more-prepared peers. We found similar patterns in our descriptive analysis of STEM earnings premiums by academic preparation, suggesting that our causal results may generalize to other selective STEM programs in Colombia.

Our results suggest that policies that expand selective STEM programs can lead to large earnings gains for the students they induce to enroll. Similarly, changes in STEM admission standards that reduce the emphasis on pre-college academic preparation may allow students with larger potential earnings returns to enroll.

An important caveat to these conclusions is that these benefits may be concentrated in the population of students who manage to complete a STEM degree. Thus, the earnings gains from policies that promote STEM enrollment may be unequally distributed as long as graduation rates remain low. This highlights the importance of other initiatives that help students to develop STEM skills at younger ages. For example, Goodman (2019) shows that compulsory math coursework in high school can have large and persistent earnings benefits for disadvantaged students. Such initiatives can increase the stock of STEM skills that students possess prior to attending college, and therefore help to raise STEM degree completion rates.

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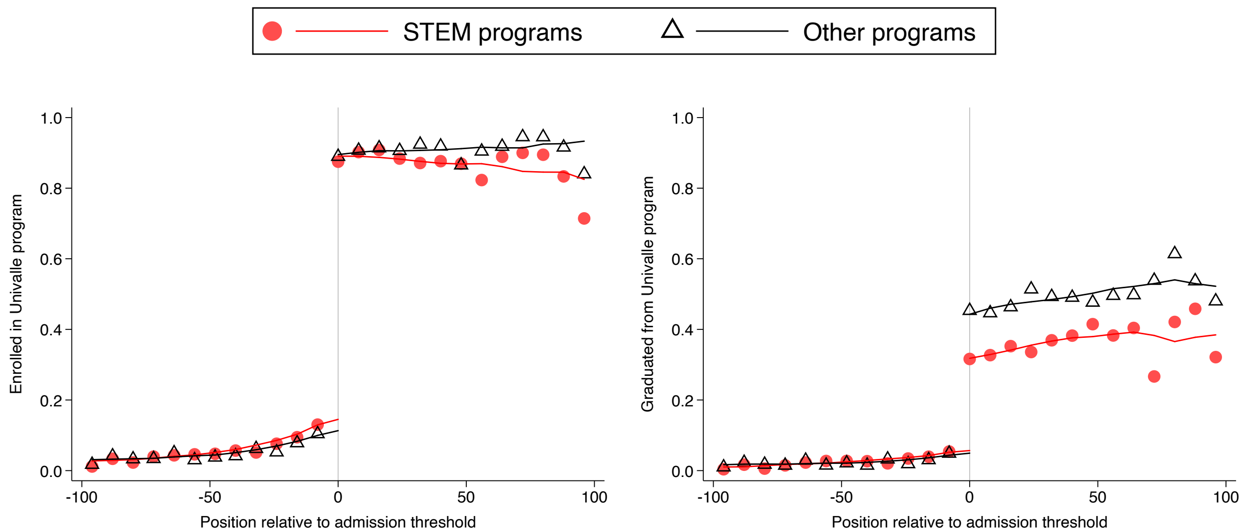
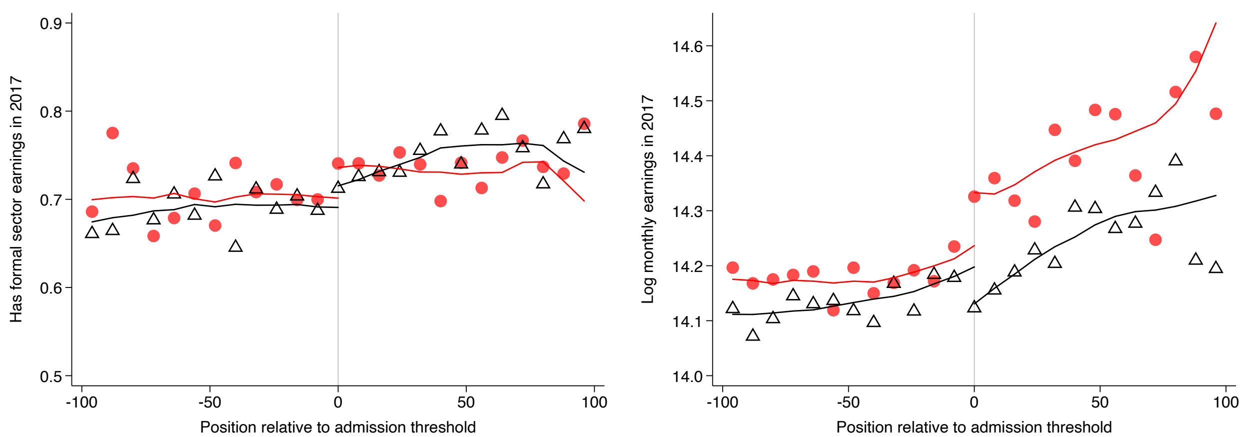


Panel A. Enrolled in Univalle program

Panel B. Graduated from Univalle program

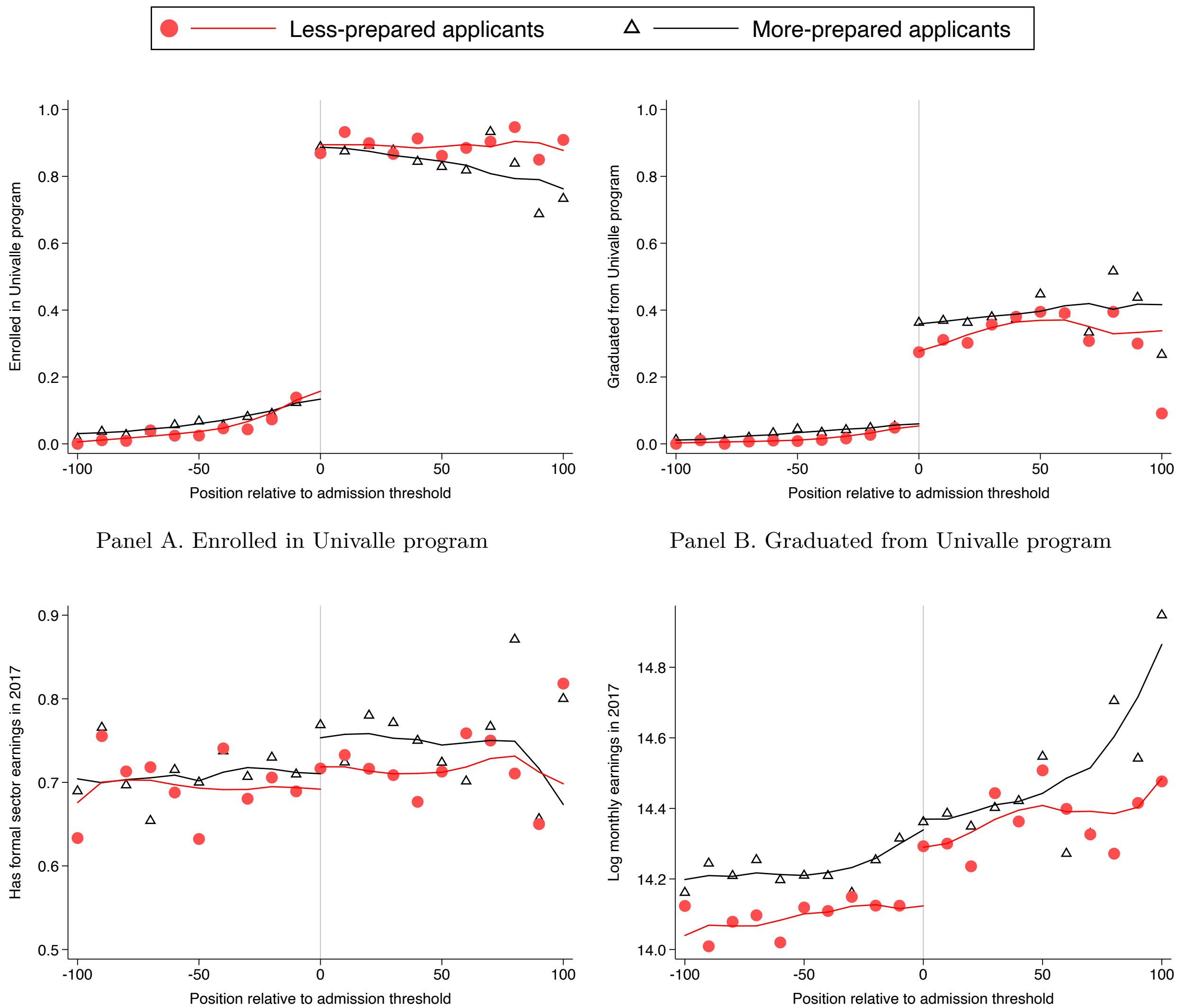


Panel C. Employed in formal sector in 2017

Panel D. Log monthly earnings in 2017

Figure 1. RD effects of admission to Univalle STEM and other programs

Notes: This figure presents RD graphs of the effects of admission to Univalle programs. The x-axis in each panel is an applicant's position in their application pool normalized to zero at the threshold. The variable on the y-axis is listed in the panel title. Markers depict means of the dependent variable in bins of eight positions. Lines are predicted values from local linear regressions estimated separately above and below the threshold with a uniform kernel and a 30-rank bandwidth. Red circles and lines show estimates for STEM applicants. Hollow triangles and lines show estimates for applicants to other programs.



Panel A. Enrolled in Univalle program

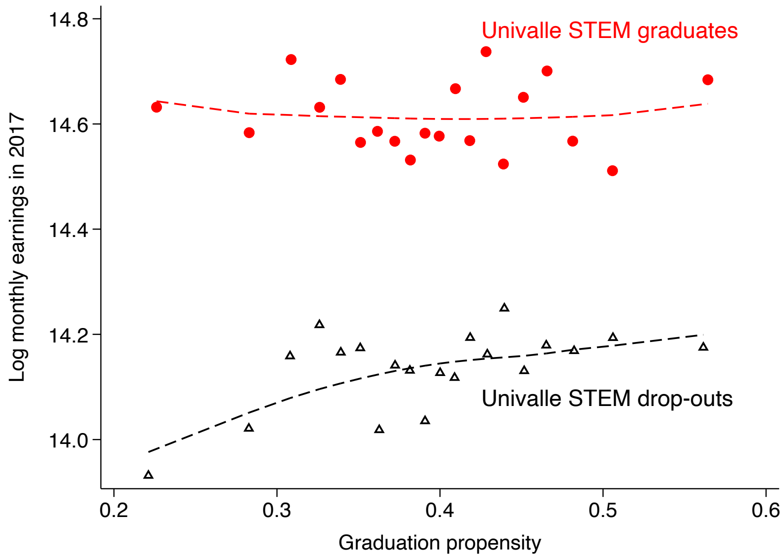
Panel B. Graduated from Univalle program

Panel C. Employed in formal sector in 2017

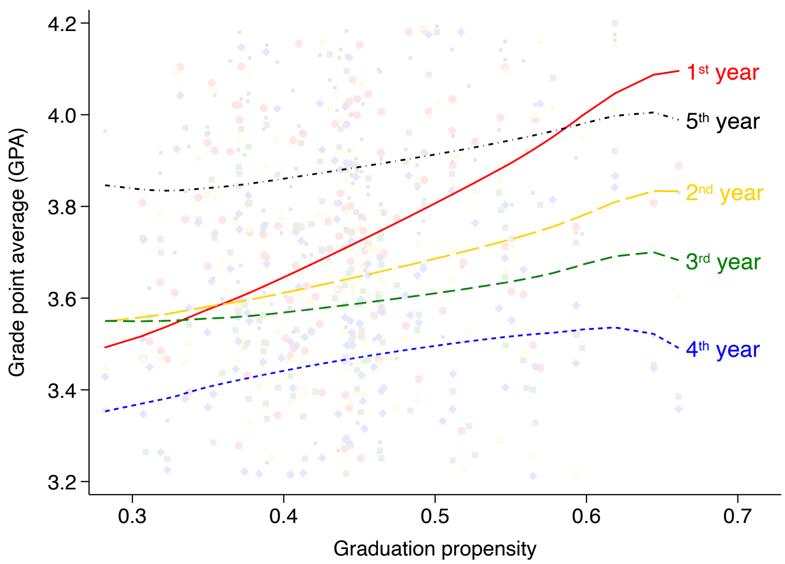
Panel D. Log monthly earnings in 2017

FIGURE 2. Heterogeneity in returns to Univalle STEM programs by academic preparation

Notes: This figure presents RD graphs of the effects of admission to Univalle's STEM programs. The x-axis in each panel is an applicant's position in their application pool normalized to zero at the threshold. The variable on the y-axis is listed in the panel title. Markers depict means of the dependent variable in bins of ten positions. Lines are predicted values from local linear regressions estimated separately above and below the threshold with a uniform kernel and a 30-rank bandwidth. Red circles and lines show estimates for less-prepared applicants. Hollow triangles and lines show estimates for more-prepared applicants. We define our less- and more-prepared samples as described in the notes to Table 5.



Panel A. Log earnings for Univalle graduates and dropouts



Panel B. GPA for Univalle STEM graduates by year

FIGURE 3. Academic preparation and skill accumulation for Univalle STEM graduates

Notes: Panel A plots log monthly earnings in 2017 (y-axis) by graduation propensity (x-axis) for students who enrolled in Univalle's STEM programs. Markers depict means in ventiles of graduation propensity, with red circles representing students who completed the Univalle STEM program and hollow triangles representing students who dropped out. Dashed lines are predicted values from local linear regressions.

Panel B plots grade point average (GPA) in each year of the program (y-axis) by graduation propensity (x-axis) for students who completed a Univalle STEM degree. The sample includes graduates from the 2000 and 2001 cohorts of five Univalle engineering programs for which we have transcript data: Chemical, Electrical, Electronic, Materials, and Mechanical Engineering. To compute GPA, we include only courses that were required for the major, and we group courses based on the modal year in the program in which students take them. Lines depicted the non-parametric relationship between students' GPA in the required courses for each year and their graduation propensity. Markers depict GPA in each year for each graduate in our sample; we do not plot markers below 3.2 or above 4.2 to make the graph more readable. See the text in Section 4.4 for details on the transcript data and grades at Univalle.

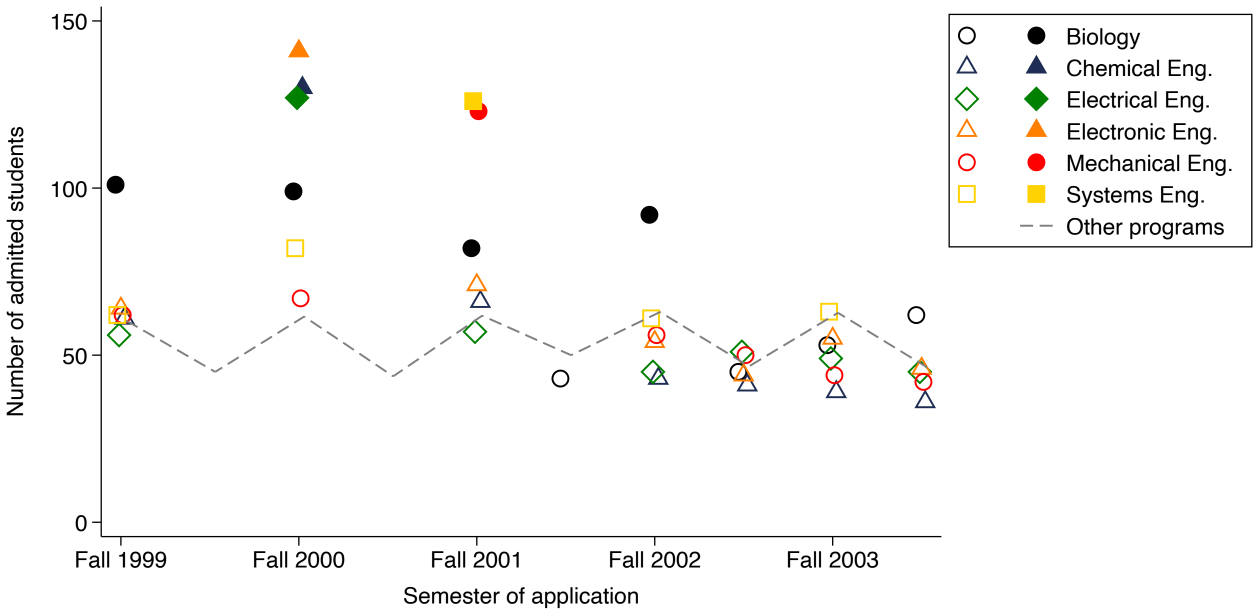
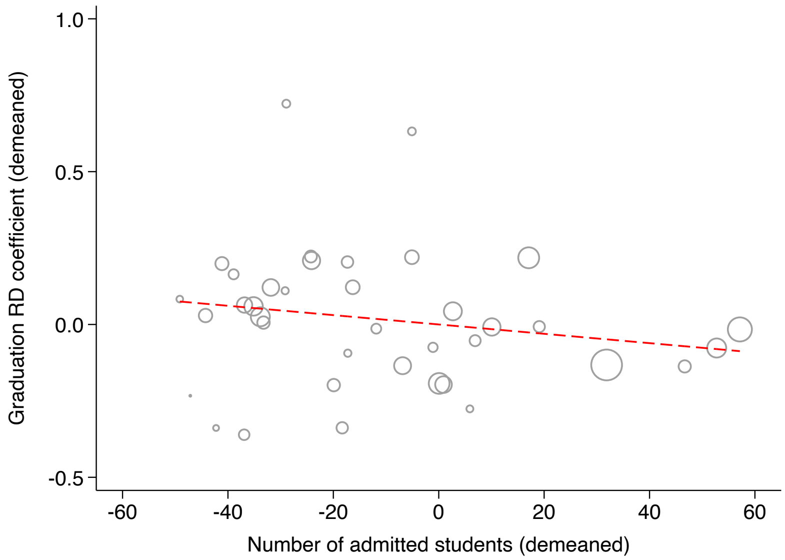
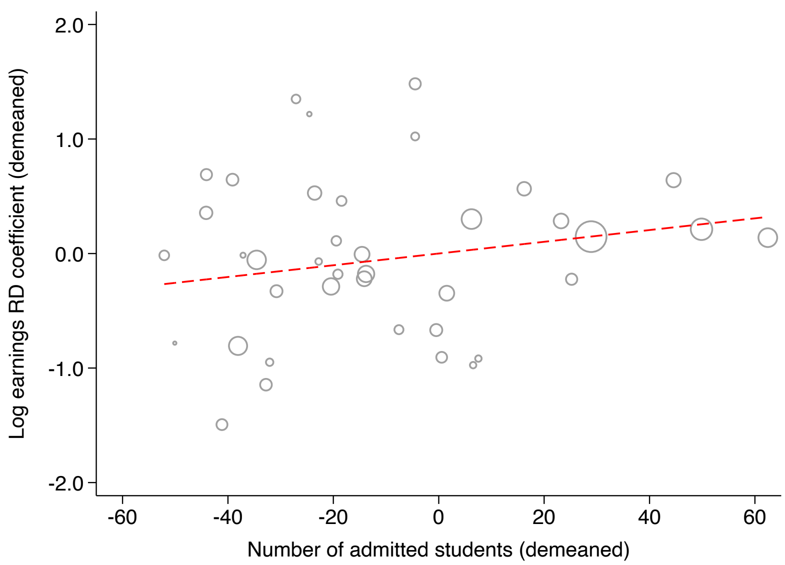


FIGURE 4. Quota expansions in Univalle's STEM programs

Notes: This figure shows the number of students who were admitted to Univalle's STEM programs in each application cohort. The x-axis denotes the semester of application, which we observe from Fall 1999 through Spring 2004. The y-axis shows the number of admitted students in each program and semester. Markers depict the six STEM programs in which the admission quotas changed significantly during this time period: Biology, Chemical Engineering, Electrical Engineering, Electronic Engineering, Mechanical Engineering, and Systems Engineering. Solid markers depict cohorts that we define as having large quotas for our binary measure of L_{mt} (see Section 5.2) and hollow markers depict small-quota cohorts. The dashed grey line plots the mean number of admits in the other 12 STEM programs in our sample. See Appendix Table A12 for details on the number of admitted students in each Univalle program.



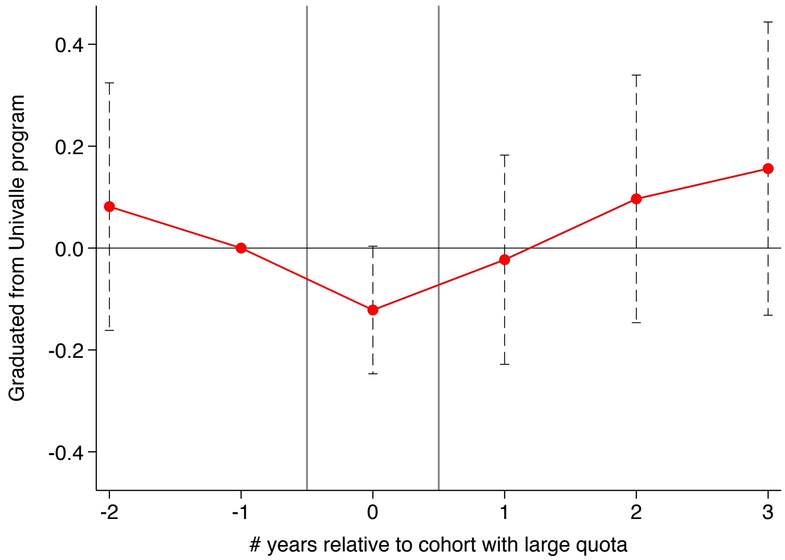
Panel A. Graduated from Univalle program



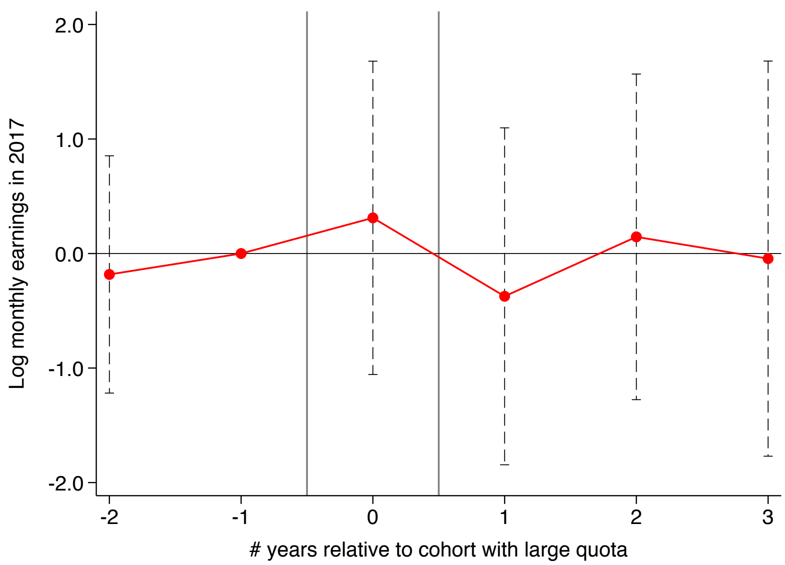
Panel B. Log monthly earnings in 2017

FIGURE 5. RD estimates for STEM graduation rates and earnings returns by quota size

Notes: This figure plots RD estimates for STEM graduation rates and earnings returns against the size of the admission quota. Each circle represents a program/cohort pair for the six Univalle STEM programs that had a large quota during our sample period (see Figure 4). In both panels, the x-axis value is the quota size for the program/cohort (the y-axis value in Figure 4). The y-axis value is the 2SLS RD coefficient, β, from equation (2) estimated separately for each program/cohort. The dependent variables for these RD regression are Univalle graduation (Panel A) and log monthly earnings in 2017 (Panel B). Circle sizes are proportional to the inverse squared standard errors of the RD coefficients. The dashed lines show the OLS relationship between the y- and x-axis variables. To make the graphs more readable, we do not display a few RD estimates that are very large and imprecise.



Panel A. Graduated from Univalle program



Panel B. Log monthly earnings in 2017

FIGURE 6. Event studies for graduation rates and earnings returns in the large-quota cohort (Engineering programs only)

Notes: This figure displays event study estimates for STEM graduation rates and earnings returns in the cohorts with large admission quotas. The sample includes the five Univalle engineering programs that had exactly one cohort with a large quota during our sample period (see Figure 4), plus Univalle's other engineering programs as the control group. We estimate a modified version of the DD regression (3) in which the variables of interest are dummies for years $k \in \{-2, 0, 1, 2, 3\}$ relative to the large-quota cohort in the five treated programs. Thus $k = 0$ corresponds to the cohort with the large quota, $k < 0$ are normal-sized cohorts prior to the large cohort, and $k > 0$ are normal-sized cohorts after the large cohort. The dependent variables for these DD regressions are the program/cohort-specific 2SLS RD estimates, β_{mt}, for Univalle graduation (Panel A) and log monthly earnings in 2017 (Panel B). The graphs plot the π_k coefficients from these DD regressions (y-axis) against the years k relative to the large-quota cohort (x-axis). Dashed vertical lines are 95 percent confidence intervals using standard errors clustered at the program/cohort level.

Table 1. Enrollment, graduation, and earnings by college program and academic preparation

	(A)	(B)	(C)	(D)	(E)
	Deciles of academic preparation for STEM				
Group	Top decile	9th decile	8th decile	7th decile	6th decile
Panel A. Proportion enrolled in each program type					
# ICFES exam takers	290,117	290,114	290,112	290,113	290,113
Public STEM	0.19	0.08	0.05	0.03	0.02
All other (total)	0.81	0.92	0.95	0.97	0.98
Private STEM	0.16	0.12	0.09	0.07	0.06
Public non-STEM	0.29	0.25	0.21	0.18	0.15
Private non-STEM	0.05	0.06	0.06	0.06	0.05
Technical training	0.05	0.09	0.10	0.10	0.09
No college	0.27	0.40	0.49	0.57	0.62
Panel B. Graduated from program (if enrolled)					
Public STEM	0.49	0.37	0.30	0.26	0.22
All other (mean)	0.57	0.45	0.40	0.35	0.33
Private STEM	0.55	0.40	0.35	0.29	0.27
Public non-STEM	0.61	0.50	0.45	0.41	0.38
Private non-STEM	0.56	0.46	0.39	0.36	0.33
Technical training	0.40	0.36	0.32	0.30	0.28
Panel C. Log monthly earnings in 2017 (if graduated)					
Public STEM	14.73	14.54	14.47	14.41	14.36
All other (mean)	14.68	14.45	14.35	14.28	14.22
Private STEM	14.82	14.59	14.48	14.41	14.34
Public non-STEM	14.64	14.43	14.35	14.28	14.22
Private non-STEM	14.67	14.44	14.32	14.24	14.21
Technical training	14.44	14.33	14.27	14.22	14.17
Panel D. Return to public STEM enrollment					
Public STEM	0.013	0.054	0.055	0.080	0.070
	(0.005)	(0.007)	(0.009)	(0.010)	(0.012)

Notes: This table presents descriptive statistics on STEM enrollment, graduation, and earnings outcomes using our national Colombian administrative data. The sample includes all students who took the ICFES exam in 1998–2003. The columns group these exam takers based on a measure of academic preparation for STEM programs. For this, we take the subsample of students who enrolled in a public STEM program, and regress an indicator for completing the program on the vector of ICFES subject scores. Our measure of STEM preparation is the predicted values from this regression in the full sample; we show outcomes for the top five deciles of STEM preparation in columns (A)–(E). The rows of each panel categorize all bachelor's degree programs in our Ministry of Education data based on field of study area—STEM (engineering and natural sciences) and non-STEM—and university ownership—public or private. "Technical training" includes all non-bachelor's programs. "All other" includes also programs except public STEM.

Panel A shows the proportion of ICFES takers who enrolled in each program type, including those who do not appear in the Ministry of Education data ("no college"). Panel B displays the proportion enrollees in each program who graduated by 2012. Panel C displays mean log monthly earnings in 2017 for graduates from each program. For Panel D, we regress log monthly earnings on an indicator for enrolling in a public STEM program and dummies for gender, birth year, mother's education, family income, ICFES cohort, and high school. We display the coefficient on the public STEM indicator and standard errors clustered at the individual level (in parentheses).

Table 2. Sample summary statistics

	(A)	(B)	(C)	(D)	(E)	(F)
	STEM programs			Other programs		
Variable	All applicants	Admits	RD sample	All applicants	Admits	RD sample
# applicants	16,022	5,660	6,699	23,439	6,767	7,664
# enrollees	4,992	4,992	3,091	6,155	6,155	3,587
Female	0.36	0.30	0.32	0.64	0.57	0.59
Age at application	18.69	18.75	18.95	18.88	19.25	19.35
College educated mother	0.36	0.44	0.37	0.34	0.37	0.35
Family income > 2x min wage	0.58	0.64	0.60	0.56	0.59	0.59
ICFES percentile	0.86	0.93	0.90	0.80	0.86	0.84

Notes: This table displays summary statistics on our sample of Univalle applicants. Column (A) includes applicants to the 18 STEM programs in our sample (listed below). Column (B) includes the subset of applicants who were admitted. Column (C) shows our benchmark RD sample, which includes applicants with admission scores that are within 30 positions of the threshold. Columns (D)–(F) include analogous samples for the 30 non-STEM programs in our sample (listed below). See Appendix C.2 for details on the programs and applicants that we include in our sample.

STEM programs (18 in total):

- Engineering: Agricultural, Chemical, Civil, Electrical, Electronic, Industrial, Materials, Mechanical, Sanitary, Statistics, Systems, Topographical
- Natural sciences: Biology, Chemical Technology (day & night), Chemistry, Math, Physics

Other programs (30 in total):

- Administration: Accounting (day & night), Business (day & night), Foreign Trade
- Health: Audiology, Bacteriology, Dentistry, Medicine, Nursing, Occupational Therapy, Physical Therapy
- Humanities: History, Recreation, Social Work, Teaching (Elementary), Teaching (Foreign Lang., day & night), Teaching (History), Teaching (Literature), Teaching (Philosophy), Teaching (Social Science)
- Integrated arts: Architecture, Communication, Dramatic Arts, Teaching (Music), Visual Arts
- Social sciences: Economics, Psychology, Sociology

Table 3. Mean returns to enrollment in Univalle STEM and other programs

	(A)	(B)	(C)	(D)
	STEM programs		Other programs	
Dependent variable	Mean below threshold	RD coef	Mean below threshold	RD coef
Panel A. First stage				
Enrolled in Univalle program	0.149	0.746	0.106	0.784
		(0.015)		(0.013)
N		6,699		7,664
Panel B. 2SLS regressions				
Graduated from Univalle program	0.000	0.344	0.000	0.498
		(0.020)		(0.018)
Employed in formal sector in 2017	0.704	0.040	0.690	0.035
		(0.027)		(0.025)
Log monthly earnings in 2017	14.168	0.133	14.158	−0.047
		(0.061)		(0.051)
N (with earnings defined)		4,845		5,441
Panel C. Log monthly earnings returns with imputed informal earnings				
$\beta^{\text{Informal}} = 0$	14.016	0.136	13.970	−0.001
$\beta^{\text{Informal}} = 0.133$	14.016	0.171	13.970	0.036
$\beta^{\text{Informal}} = 0.266$	14.016	0.205	13.970	0.072
N		6,699		7,664

Notes: This table displays RD coefficients from separate regressions for Univalle applicants to STEM (columns A–B) and other (columns C–D) programs.

Columns (A) and (C) present means of each dependent variable for applicants who were just below the admission thresholds. In Panel A, these columns show means over all applicants who were 1–5 positions below the thresholds. In Panels B–C, these columns show control complier means estimated following Katz et al. (2001).

Columns (B) and (D) present RD coefficients using samples of applicants within 30 positions of the admission thresholds. Panel A displays reduced-form RD coefficients, θ, from equation (1). Panel B displays 2SLS RD coefficients, β, from equation (2) using the dependent variable listed in the row header. Panel C displays 2SLS RD coefficients for log monthly earnings in which we impute values for individuals with missing earnings. For this, we use the 2017 waves of the Colombian GEIH household survey (*Gran Encuesta Integrada de Hogares*) to compute mean log informal monthly earnings for workers with a given birth year, gender, and highest degree (high school, technical, or bachelor's). We assume that all individuals in our sample with missing formal earnings have the GEIH informal mean earnings based on their values of these three covariates. For applicants with missing earnings who enrolled in Univalle, we increase their earnings from the GEIH informal mean by β^{Informal}, where this term takes three different values: 0, 0.133, and 0.266. We estimate the 2SLS RD specification (1)–(2) using log monthly earnings including these imputed values as the outcome and display the θ coefficients in Panel C.

Parentheses contain standard errors clustered at the individual level.

Table 4. ICFES subject score weights in admission scores and outcomes

	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
	STEM programs				Other programs			
ICFES subject (Post-2000 exam)	Admit score	Grad- uated	Exit score	Log earnings	Admit score	Grad- uated	Exit score	Log earnings
Biology	0.11	0.03	0.13	0.01	0.09	0.09	0.13	−0.10
Chemistry	0.14	0.69	0.33	0.76	0.05	0.49	0.21	0.35
Math	0.27	−0.19	0.10	−0.16	0.21	−0.28	0.06	0.12
Physics	0.12	0.28	0.12	0.43	0.04	0.27	0.06	0.09
Geography	0.05	−0.10	0.09	−0.33	0.12	−0.18	0.11	−0.20
History	0.07	−0.21	0.03	0.05	0.13	−0.03	0.17	0.15
Interdisciplinary	0.03	0.08	−0.02	−0.06	0.03	0.09	0.04	0.08
Language arts	0.14	0.18	0.13	0.18	0.23	0.40	0.14	0.11
Philosophy	0.06	0.24	0.08	0.13	0.12	0.15	0.09	0.40
Quantitative subjects	0.65	0.81	0.68	1.04	0.39	0.57	0.46	0.46
Qualitative subjects	0.35	0.19	0.32	−0.04	0.61	0.43	0.54	0.54
Mean absolute deviation from admit score		0.21	0.06	0.26		0.20	0.06	0.14
N (enrollees w/ outcome)	4,491	4,491	1,460	3,311	5,677	5,677	2,499	4,278

Notes: This table shows how subject scores on the ICFES exam relate to four outcomes: 1) the Univalle admission score; 2) an indicator for graduating from the Univalle program; 3) scores on a field-specific college *exit* exam called *Saber Pro* (formerly *ECAES*); and 4) log monthly earnings in 2017. We regress each outcome variable on the nine ICFES subject scores using all Univalle enrollees in our sample who took the post-2000 version of the ICFES. (See Appendix Table A5 for analogous results using pre-2000 ICFES exam takers.) We run these regressions separately for each of Univalle's 48 programs and normalize the estimated coefficients to sum to one. Columns (A)–(D) show the subject weights for each outcome averaged across Univalle's 18 STEM programs. Columns (E)–(H) show the subject weights for each outcome averaged across Univalle's 30 non-STEM programs. We report the sum of the weights for quantitative subjects (biology, chemistry, math, and physics) and qualitative subjects (geography, history, interdisciplinary, language arts, and philosophy). We also report the mean absolute deviation between the average admission score weights (columns A and D) and the average weights for each other outcome (columns B–D and F–H). The last row shows the number of Univalle enrollees for which each outcome is defined.

Table 5. Heterogeneity in returns to Univalle STEM enrollment by academic preparation

	(A)	(B)	(C)	(D)	(E)
	Less-prepared applicants		More-prepared applicants		
Dependent variable	Mean below threshold	RD coef	Mean below threshold	RD coef	p value diff
Panel A. First stage					
Enrolled in Univalle program	0.155	0.726	0.144	0.761	0.253
		(0.022)		(0.021)	
N		3,306		3,390	
Panel B. 2SLS regressions					
Graduated from Univalle program	0.000	0.288	0.000	0.375	0.031
		(0.029)		(0.028)	
Employed in formal sector in 2017	0.710	0.019	0.703	0.044	0.664
		(0.042)		(0.037)	
Log monthly earnings in 2017	13.992	0.244	14.307	0.032	0.091
		(0.094)		(0.083)	
N (with earnings defined)		2,338		2,500	
Panel C. Log monthly earnings returns with imputed informal earnings					
$\beta^{\text{Informal}} = 0$	13.899	0.213	14.121	0.062	
$\beta^{\text{Informal}} = 0.133$	13.899	0.249	14.121	0.095	
$\beta^{\text{Informal}} = 0.266$	13.899	0.286	14.121	0.129	
N		3,306		3,390	

Notes: This table displays RD coefficients from separate regressions for less-prepared (columns A–B) and more-prepared (columns C–D) applicants to Univalle's STEM programs. We define the less- and more-prepared samples using a leave-cohort-out version of the graduation ICFES score weights from column (B) of Table 4. Specifically, for each program m and cohort t, we regress an indicator for Univalle graduation on the ICFES subject scores in a sample that includes all enrollees in program m in cohorts *other than* t. We take the predicted values from this regression as a measure of the graduation propensity of applicants to program m and t. Lastly, we regress graduation propensity on individuals' admission ranks with application pool dummies and take the residuals from this regression. Less-prepared applicants are those with below median residuals of graduation propensity in their application pool. More-prepared applicants are those with above median residuals.

Columns (A) and (C) present means of each dependent variable for applicants who were just below the admission thresholds. In Panel A, these columns show means over all applicants who were 1–5 positions below the thresholds. In Panels B–C, these columns show control complier means estimated following Katz et al. (2001).

Columns (B) and (D) present RD coefficients using samples of applicants within 30 positions of the admission thresholds. Panel A displays reduced-form RD coefficients, θ, from equation (1). Panel B displays 2SLS RD coefficients, β, from equation (2) using the dependent variable listed in the row header. Panel C displays 2SLS RD coefficients for log monthly earnings in which we impute values for individuals with missing earnings; see the notes to Table 3 for details on this imputation method. We estimate the 2SLS RD specification (1)–(2) using log monthly earnings including these imputed values as the outcome and display the θ coefficients in Panel C.

Column (E) displays the p value from an F test for equality of the RD coefficients in columns (B) and (D).

Parentheses contain standard errors clustered at the individual level.

Table 6. Effects of Univalle STEM enrollment on college program and degree characteristics

	(A)	(B)	(C)	(D)	(E)
	Less-prepared applicants		More-prepared applicants		
Dependent variable	Mean below threshold	2SLS coef	Mean below threshold	2SLS coef	p value diff
Panel A. Enrollment in college programs					
Enrolled in any STEM BA program	0.428	0.572	0.507	0.493	0.082
		(0.034)		(0.031)	
Enrolled in any BA program	0.692	0.308	0.782	0.218	0.032
		(0.032)		(0.028)	
Enrolled in any technical program	0.275	−0.135	0.173	−0.052	0.077
		(0.037)		(0.030)	
Enrolled in any college program	0.813	0.187	0.861	0.139	0.208
		(0.029)		(0.025)	
Panel B. Graduation from college programs					
Completed any STEM BA program	0.109	0.245	0.211	0.252	0.896
		(0.038)		(0.038)	
Completed any BA program	0.236	0.191	0.402	0.135	0.344
		(0.042)		(0.041)	
Completed any technical program	0.105	−0.059	0.076	−0.030	0.351
		(0.023)		(0.020)	
Completed any college program	0.338	0.126	0.476	0.093	0.595
		(0.045)		(0.041)	
Panel C. Log mean earnings in college program					
Mean earnings in college	14.063	0.045	14.100	0.013	0.070
		(0.013)		(0.012)	
Mean earnings in major	14.092	0.111	14.119	0.062	0.031
		(0.017)		(0.015)	
Mean earnings in college/major	14.078	0.187	14.136	0.133	0.028
		(0.018)		(0.017)	
N		3,306		3,390	

Notes: This table displays RD coefficients from separate regressions for less-prepared (columns A–B) and more-prepared (columns C–D) applicants to Univalle's STEM programs. We define less- and more-prepared applicants as described in the notes to Table 5. In Panels A–B, the outcome variables are indicators for enrolling in and graduating from programs in our Ministry of Education data between 1998 and 2012. We define STEM programs, bachelor's (BA) programs and technical programs in the same way as in Table 1. In Panel C, the dependent variables are the mean log earnings in the college, major, or college/major pair that an applicant enrolled in. We calculate this as the leave-individual-out mean for all enrollees in the Ministry's data. For Univalle applicants who did not enroll in college, we use the leave-out mean log earnings for all ICFES exam takers who do not appear in the Ministry's data.

Columns (A) and (C) show control complier means for each dependent variable estimated following Katz et al. (2001). Columns (B) and (D) present RD coefficients using samples of applicants within 30 positions of the admission thresholds. All coefficients are 2SLS RD coefficients, β, from equation (2) using the dependent variable listed in the row header. Column (E) displays the p value from an F test for equality of the RD coefficients in columns (B) and (D). Parentheses contain standard errors clustered at the individual level.

Table 7. Effects of quota expansions on returns to Univalle STEM enrollment

	(A)	(B)	(C)	(D)
		Effect of quota expansion		
Dependent variable	Control complier mean	Large quota (binary)	60 extra admits (integer)	Stacked DD (binary)
Panel A. Characteristics of marginally-admitted compliers (DD coefficients)				
Graduation propensity	0.371	−0.062	−0.056	−0.060
		(0.017)	(0.014)	(0.017)
Female	0.354	0.150	0.156	0.134
		(0.105)	(0.094)	(0.096)
College educated mother	0.324	−0.173	−0.101	−0.179
		(0.097)	(0.088)	(0.105)
Family income > 2x min wage	0.579	−0.086	−0.065	−0.091
		(0.119)	(0.104)	(0.123)
N (# program/cohorts)		104	104	232
Panel B. Returns to Univalle enrollment (RDDD coefficients)				
Enrolled in Univalle program	0.149	0.122	0.113	0.109
		(0.066)	(0.065)	(0.067)
Graduated from Univalle program	0.000	−0.179	−0.148	−0.191
		(0.061)	(0.051)	(0.069)
Employed in formal sector in 2017	0.704	0.040	0.014	0.043
		(0.117)	(0.093)	(0.125)
Log monthly earnings in 2017	14.168	0.393	0.183	0.357
		(0.217)	(0.201)	(0.231)
N (# program/cohorts)		104	104	232

Notes: This table shows how the characteristics (Panel A) and outcomes (Panel B) of applicants near the admissions threshold for Univalle's STEM programs changed when the quotas increased.

In both panels, column (A) shows control complier means for each dependent variable estimated following Katz et al. (2001). In Panel A, columns (B)–(D) show π coefficients from equation (3) in which the dependent variables are program/cohort-specific mean complier characteristics. In Panel B, columns (B)–(D) show π coefficients from equation (3) in which the dependent variables are program/cohort-specific RD coefficients, β_{mt}, from our 2SLS specification (1)–(2).

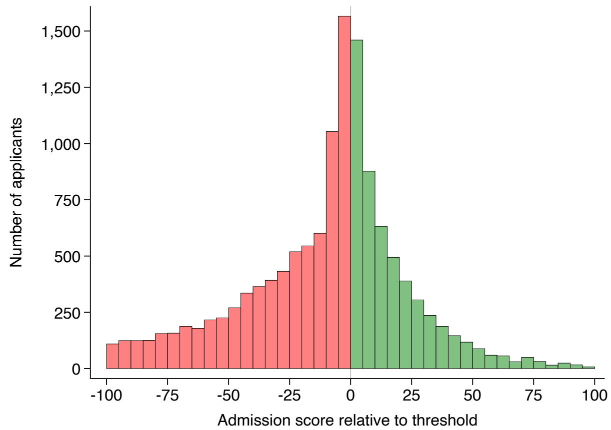
Column (B) reports estimates of π in which the variable of interest, L_{mt}, is an indicator for programs and cohorts with large quotas (the solid symbols in Figure 4). Column (C) reports π coefficients in which we define L_{mt} as the total number of admits in each program/cohort divided by 60 (the y-axis in Figure 4). Column (D) is similar to column (B), but we "stack" our dataset so that the π coefficients are identified *only* by comparing programs with quota expansions to those without expansions. We combine the six "treated" STEM programs into three groups based on the cohort(s) in which their quotas expanded: 1) Biology (Fall 1999–2002); 2) Chemical, Electrical, and Electronic Eng. (Fall 2000); and 3) Mechanical and Systems Eng. (Fall 2001). We then create three datasets that include all 12 "control" STEM programs plus the treated programs in each group. Lastly, we stack these datasets and estimate the DD or RDDD specification with all covariates (except L_{mt}) interacted with dummies for each dataset.

Regressions are at the program/cohort level with observations weighted by the inverse squared standard errors of the means (Panel A) and RD coefficients (Panel B). Parentheses contain standard errors clustered at the program/cohort level.

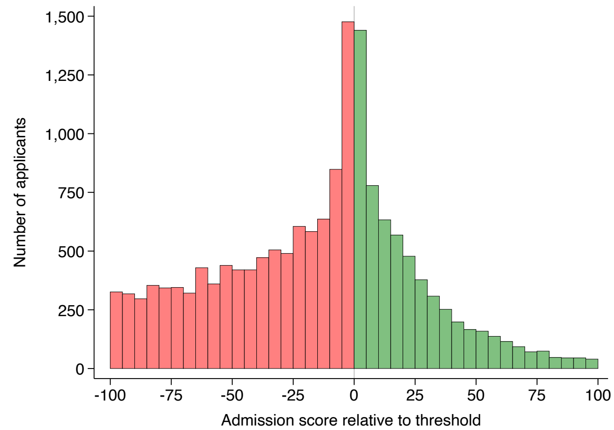
Appendix — For Online Publication

Outline:

A. Appendix figures and tables



Panel A. STEM applicants

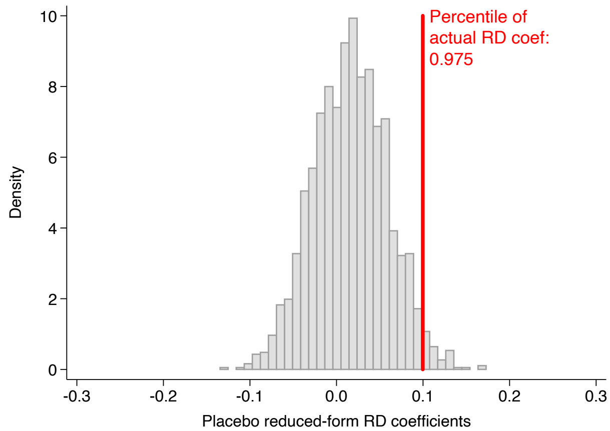


Panel B. Other applicants

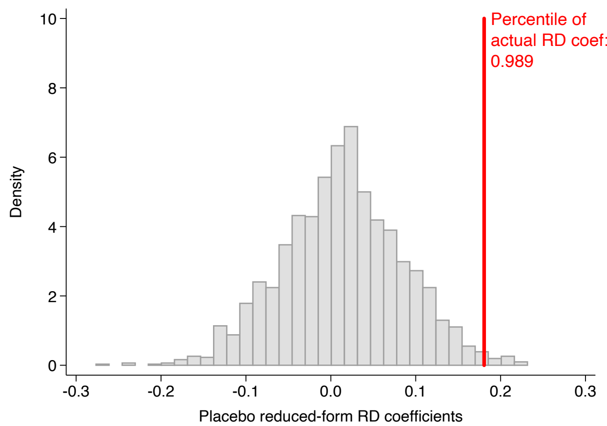
Figure A1. Density of admission scores relative to the threshold

Notes: This figure shows the density of admission scores relative to the admission thresholds. The x-axis is a student's admission score normalized to zero at the threshold. The y-axis shows the number of applicants within five unit bins of the admission score. The graphs are limited to those with normalized scores between -100 and 100.

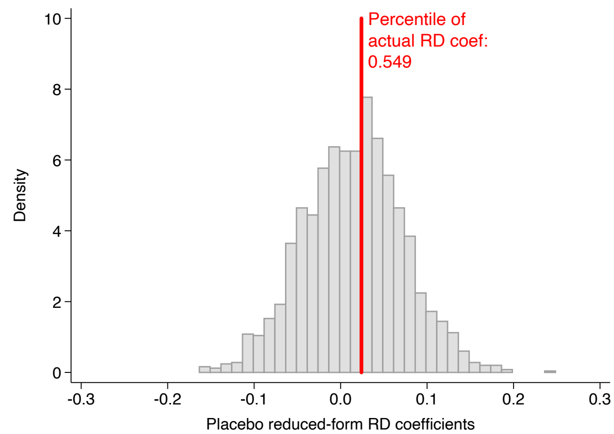
Panel A shows the distribution of admission scores for applicants to Univalle STEM programs. Using the McCrary (2008) density test, the estimated discontinuity—i.e., the log difference in height at the threshold—is -0.049 with a standard error of 0.030. Panel B shows the distribution of admission scores for applicants to non-STEM Univalle programs. The estimated density discontinuity is 0.017 with a standard error of 0.026.



Panel A. All STEM applicants



Panel B. Less-prepared STEM applicants



Panel C. More-prepared STEM applicants

FIGURE A2. Placebo RD estimates for log monthly earnings — STEM applicants

Notes: This figure displays placebo RD estimates for STEM applicants' log monthly earnings.

We follow Beuermann and Jackson (2022)'s method of generating these placebo RD estimates (see their Appendix Figure A2). First, we randomly choose an admission rank as the placebo cutoff in each application pool. We then estimate our reduced-form RD regression (equation 1) with log monthly earnings as the dependent variable, and we define the placebo running variable, x_{ip}, and above-threshold indicator, D_{ip}, relative to the placebo cutoffs.

The gray bars in each graph plot the distribution of 2,000 placebo reduced-form RD coefficients estimated using this method. The sample for Panel A includes all STEM applicants. The samples for Panels B and C include less- and more-prepared STEM applicants, respectively. In each graph, the vertical red lines depict the actual reduced-form RD coefficients for log monthly earnings, and we report the percentile of these actual coefficients in the placebo distribution. The actual reduced-form RD earnings coefficients are 0.100 for all STEM applicants, 0.181 for less-prepared STEM applicants, and 0.024 for more-prepared STEM applicants.

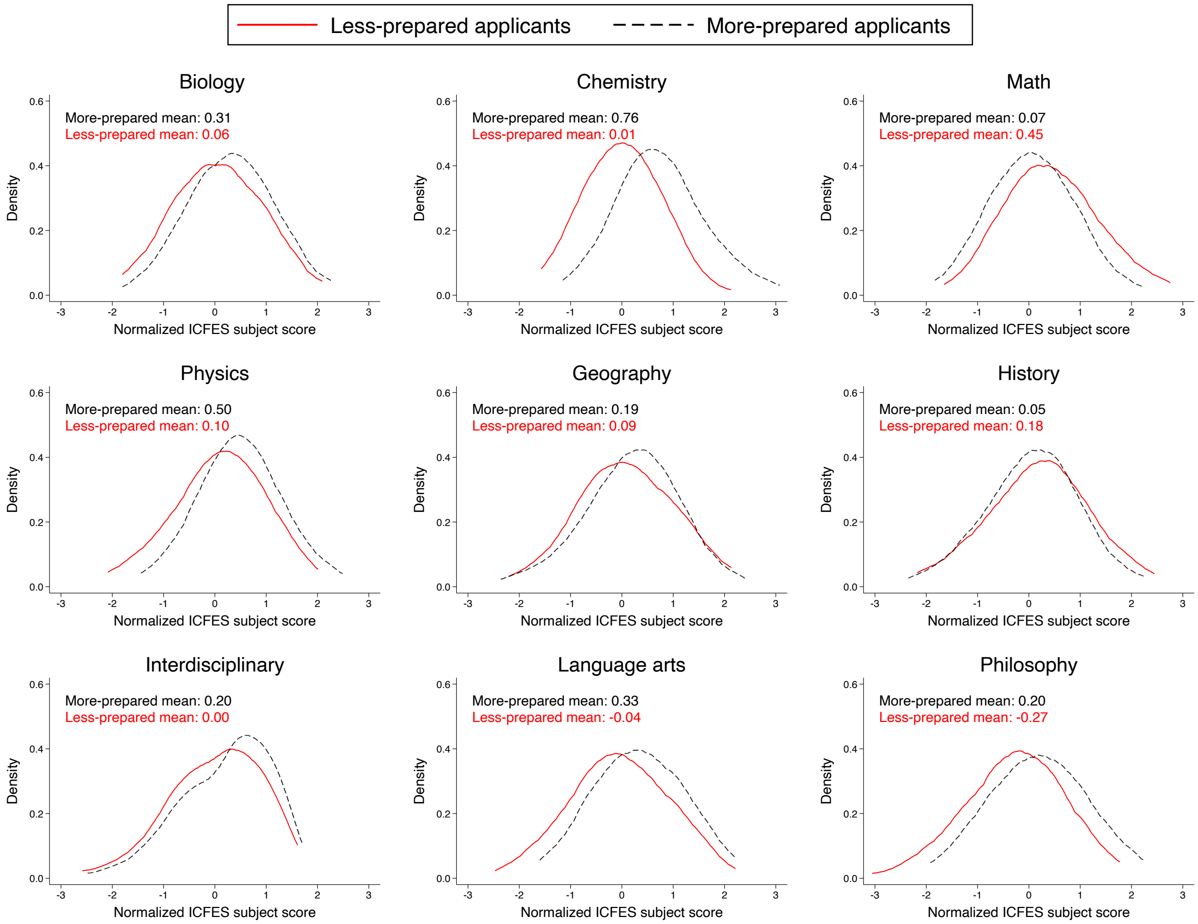
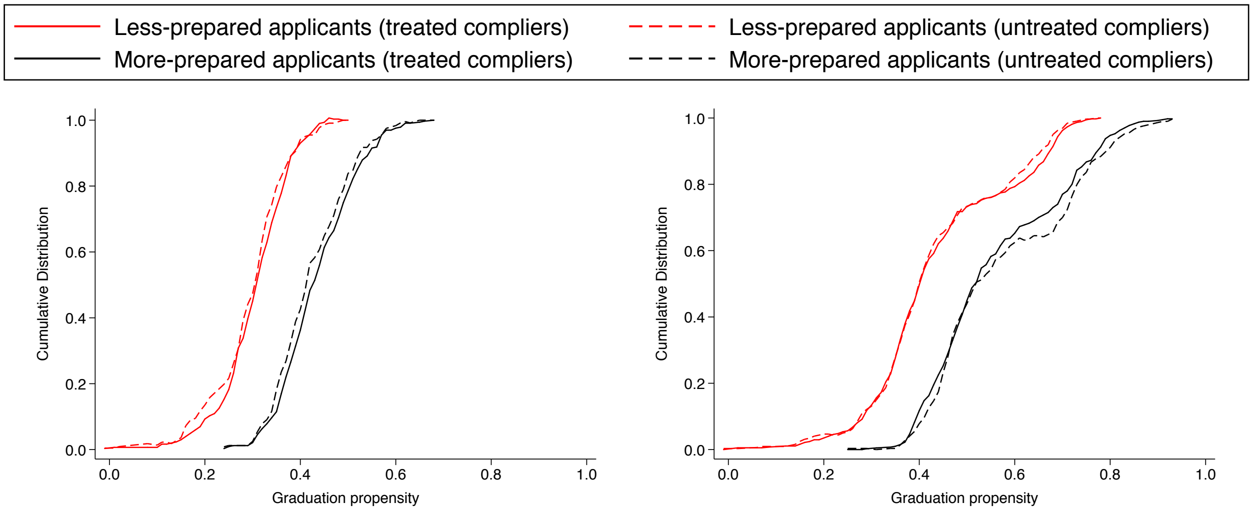


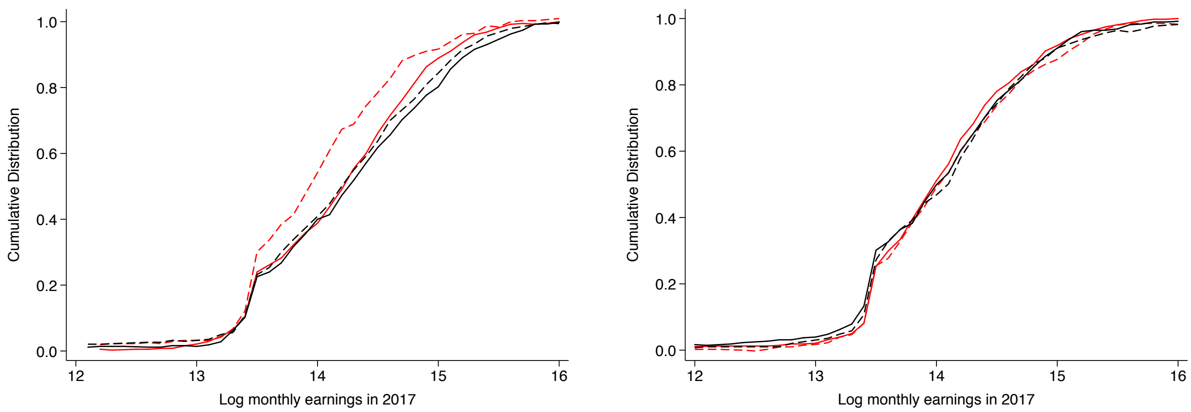
FIGURE A3. ICFES subject score distributions for marginal STEM applicants by academic preparation

Notes: This figure plots distributions of ICFES subject scores for marginal STEM applicants by academic preparation. The sample includes applicants to Univalle STEM programs who were within five positions of the admission thresholds and who took the post-2000 version of the ICFES exam. Each graph shows score distributions for a different ICFES subject exam, as indicated in the graph title. Subject scores are normalized to be mean zero and standard deviation one for the full sample of applicants to all Univalle programs. Solid red lines show score distributions for less-prepared applicants, and black dashed lines show score distributions for more-prepared applicants. Each graph reports the mean normalized score in the less- and more-prepared samples.



Panel A. Graduation propensity
(STEM applicants)

Panel B. Graduation propensity
(Non-STEM applicants)



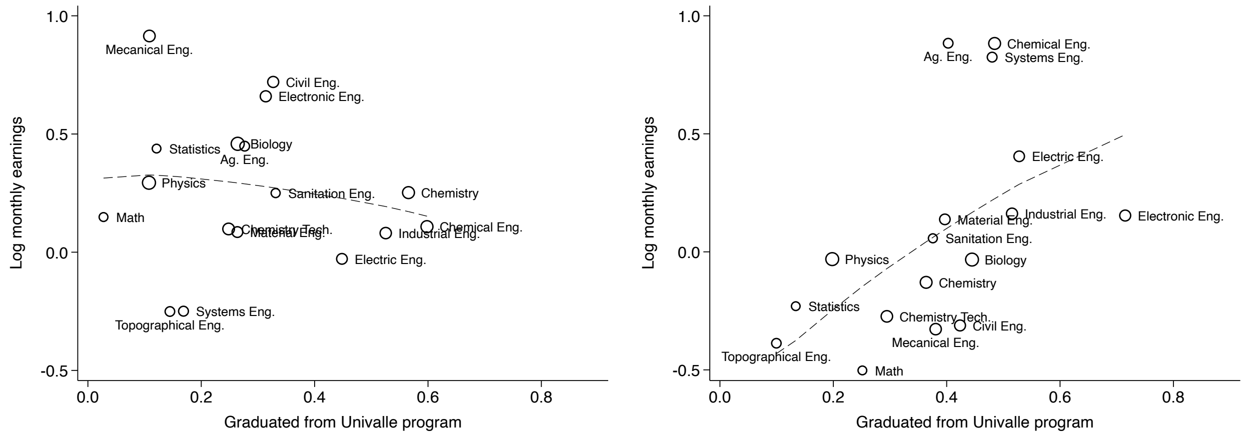
Panel C. Log monthly earnings
(STEM applicants)

Panel D. Log monthly earnings
(Non-STEM applicants)

FIGURE A4. Abadie (2002) Cumulative Distributions of Treated and Untreated Compliers

Notes: This figure presents cumulative distribution functions (CDFs) for treated and untreated compliers following Abadie (2002). "Treated" and "untreated" are defined by admission to Univalle (above and below the admission threshold). "Compliers" are applicants who would have enrolled in the Univalle program they applied to if and only if they were admitted. We compute CDFs separately for treated and untreated compliers and for less- and more-prepared applicants defined by graduation propensity (see Section 4.1), as indicated by the legend.

The sample includes applicants to STEM programs (Panels A and C) and non-STEM programs (Panels B and D) who are within 10 positions of the admission thresholds. Panels A–B show CDFs of graduation propensity. Panels C–D show CDFs of log monthly earnings in 2017.



Panel A. Less-prepared applicants

Panel B. More-prepared applicants

FIGURE A5. Earnings and graduation RD coefficients for each Univalle STEM program

Notes: This figure plots 2SLS RD estimates for graduation rates and earnings returns for each of Univalle's 18 STEM programs. The x-axis in each panel is the program's graduation rate for marginal enrollees, which is β coefficient from separate estimation of equations (1)–(2) with an indicator for graduating as the dependent variable. The y-axis in each panel is the earnings return for marginal enrollees, which is the 2SLS RD estimate of β for each program with log monthly earnings in 2017 as the dependent variable. Panel A shows estimates for less-prepared applicants and Panel B shows estimates for more-prepared applicants. We define our less- and more-prepared samples as described in the notes to Table 5. Dashed lines show the non-parametric relationships between the earnings and graduation coefficients.

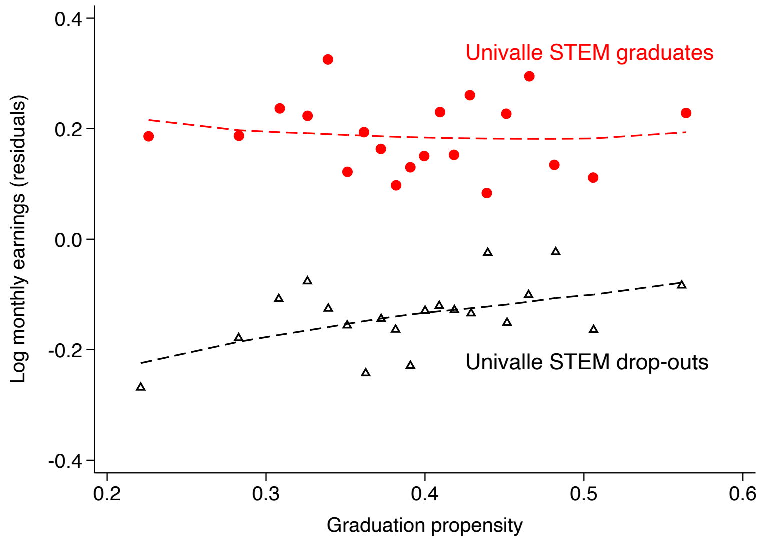
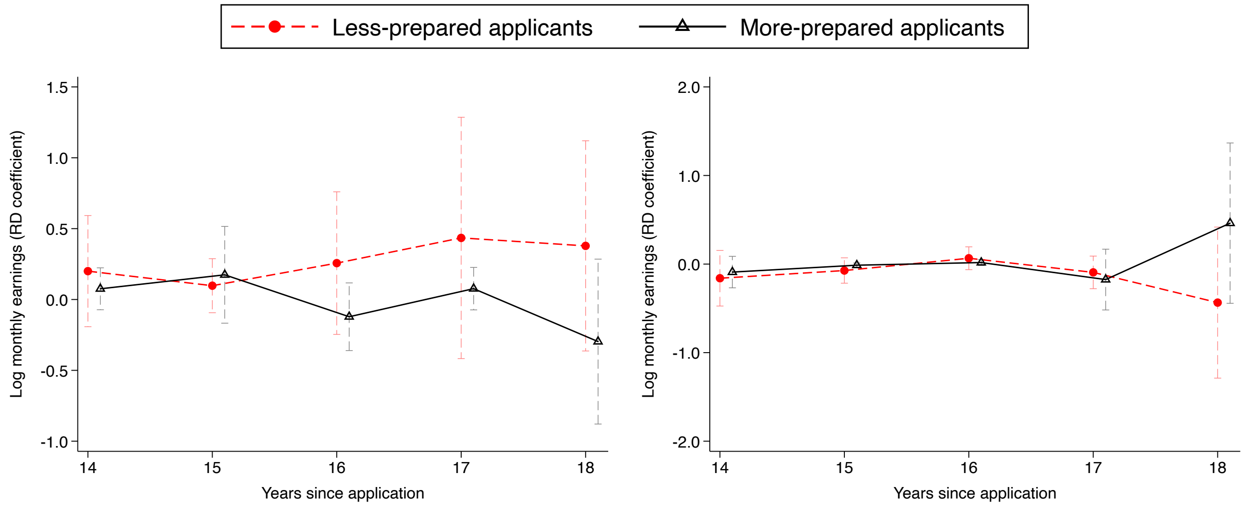


Figure A6. Log earnings residuals for Univalle graduates and dropouts

Notes: This figure is similar to Panel A of Figure 3, but the dependent variable is earnings residuals rather than raw earnings. We plot earnings residuals (y-axis) by graduation propensity (x-axis) for students who enrolled in Univalle's STEM programs. These residuals are generated from a regression of log monthly earnings in 2017 on a vector of individual covariates (gender, age, and dummies for high schools, mother's education categories, father's education categories, family income bins, and ICFES exam years). Markers depict means in ventiles of graduation propensity, with red circles representing students who completed the Univalle STEM program and hollow triangles representing students who dropped out. Dashed lines are predicted values from local linear regressions.

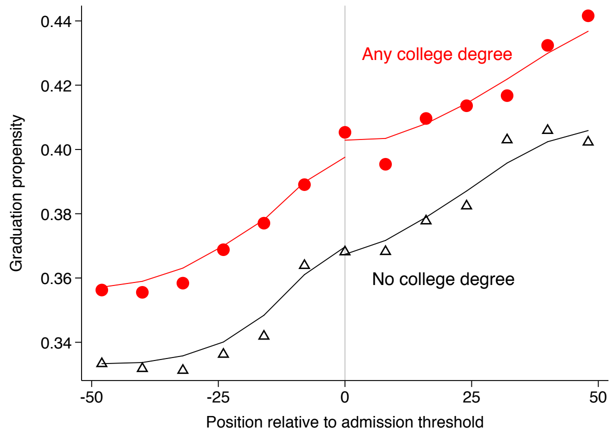


Panel A. STEM applicants

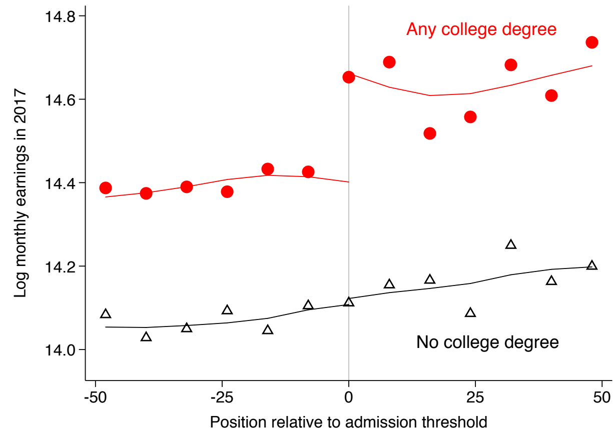
Panel B. Non-STEM applicants

Figure A7. Earnings returns to Univalle enrollment by years since application

Notes: This figure plots RD estimates of the earnings returns to Univalle enrollment estimated separated by years since application. We use our 2SLS RD specification (1)–(2) with log monthly earnings in 2017 as the dependent variable and estimate this regression separately for each application year in Fall 1999 through Spring 2004. The y-axis in each panel represents the RD coefficients. The x-axis represents years since application, defined as 2017 minus the year of the fall term of each academic year (e.g., 18 years since application includes applicants in Fall 1999 and Spring 2000). Panel A shows estimates for applicants to STEM programs and Panel B shows estimates for applicants to non-STEM programs. Red circles show RD coefficients for less-prepared applicants, and hollow black triangles show estimates for more-prepared applicants. We define our less- and more-prepared samples as described in the notes to Table 5. Dashed vertical lines are 95 percent confidence intervals using standard errors clustered at the individual level.



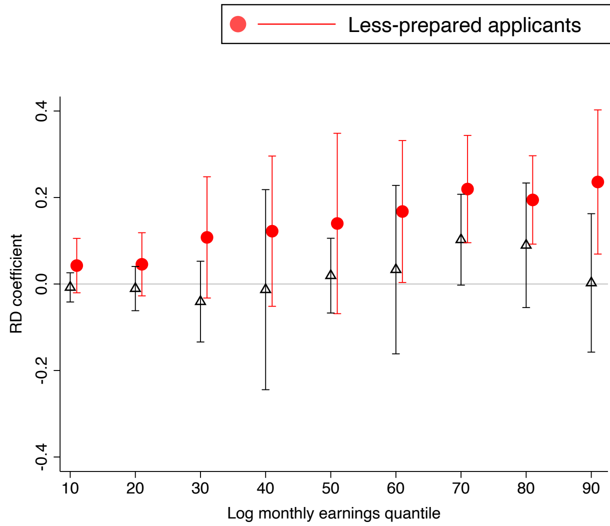
Panel A. Graduation propensity



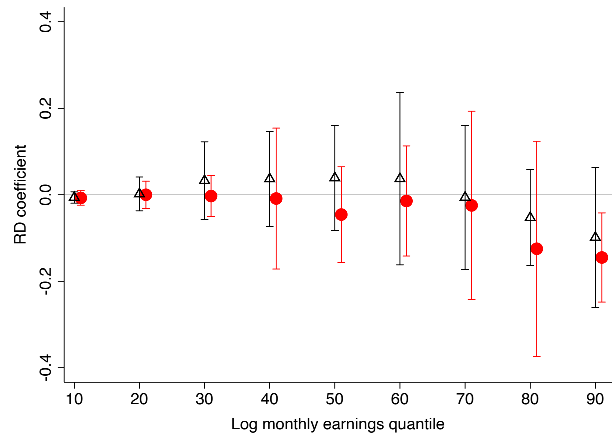
Panel B. Log monthly earnings in 2017

FIGURE A8. STEM returns by college degree attainment

Notes: This figure plots academic preparation and earnings for STEM applicants based on whether or not they ultimately earned a college degree. The x-axis in each panel is an applicant's position in their application pool normalized to zero at the threshold. The y-axis is an individual's graduation propensity (Panel A) or log monthly earnings in 2017 (Panel B). The sample includes all STEM applicants within 50 positions of the admission threshold. Markers show the means of each variable in 8-rank bins of the admission score. Red circles include applicants who earned *any* college degree, regardless of whether it was at Univalle. Hollow triangles include those who did not. Lines are local linear regressions estimated separately above and below the thresholds for each sample.



Panel A. STEM applicants



Panel B. Non-STEM applicants

FIGURE A9. RD quantile regressions for log monthly earnings

Notes: This figure presents reduced form RD quantile estimates of the effects of admission to Univalle programs. The x-axis in each panel is the quantile of log monthly earnings. The y-axis shows the estimated RD coefficient at each quantile. We estimate these coefficients using the reduced form RD specification (1) with log earnings as the dependent variable. Markers show the point estimates and vertical bars show 95% confidence intervals. Panel A shows estimates for STEM applicants and Panel B show estimates for applicants to non-STEM programs. Hollow triangles show estimates for more-prepared applicants and red circles show estimates for less-prepared applicants.

Table A1. Robustness to RD specification — STEM applicants

	(A)	(B)	(C)	(D)	(E)
Bandwidth:	$h = 30$	$h = 15$	$h = 45$	CCT	$h = 30$
Kernel:	Uniform	Uniform	Uniform	Uniform	Triang.
Panel A. All applicants					
Enrolled in Univalle program	0.746	0.716	0.766	0.729	0.730
	(0.015)	(0.021)	(0.012)	(0.017)	(0.016)
Graduated from Univalle program	0.344	0.338	0.338	0.335	0.341
	(0.020)	(0.029)	(0.017)	(0.020)	(0.023)
Employed in formal sector in 2017	0.040	0.046	0.039	0.055	0.051
	(0.027)	(0.040)	(0.023)	(0.025)	(0.030)
Log monthly earnings in 2017	0.133	0.074	0.101	0.141	0.119
	(0.061)	(0.088)	(0.049)	(0.060)	(0.069)
N	6,699	3,789	8,994	5,215	6,519
Panel B. Less-prepared applicants					
Enrolled in Univalle program	0.726	0.697	0.750	0.692	0.709
	(0.022)	(0.031)	(0.018)	(0.026)	(0.024)
Graduated from Univalle program	0.288	0.271	0.274	0.281	0.281
	(0.029)	(0.042)	(0.024)	(0.030)	(0.032)
Employed in formal sector in 2017	0.019	0.024	0.027	0.027	0.034
	(0.042)	(0.062)	(0.035)	(0.049)	(0.047)
Log monthly earnings in 2017	0.244	0.176	0.142	0.235	0.257
	(0.094)	(0.143)	(0.075)	(0.100)	(0.105)
N	3,306	1,850	4,348	2,456	3,215
Panel C. More-prepared applicants					
Enrolled in Univalle program	0.761	0.743	0.777	0.766	0.750
	(0.021)	(0.029)	(0.017)	(0.018)	(0.023)
Graduated from Univalle program	0.375	0.383	0.386	0.386	0.377
	(0.028)	(0.040)	(0.023)	(0.023)	(0.031)
Employed in formal sector in 2017	0.044	0.067	0.040	0.050	0.059
	(0.037)	(0.053)	(0.031)	(0.035)	(0.040)
Log monthly earnings in 2017	0.032	0.017	0.057	0.034	0.013
	(0.083)	(0.116)	(0.068)	(0.072)	(0.089)
N	3,390	1,937	4,642	4,177	3,301

Notes: This table displays RD coefficients for STEM applicants with different bandwidths and kernels. Panel A shows estimates for our full sample of STEM applicants and Panels B–C show estimates separately for less- and more-prepared applicants. The specifications are the same as in Tables 3 and 5, but we vary the bandwidth or kernel as indicated in the column header. Column (A) replicates our benchmark results from those tables, which use an RD bandwidth of 30 positions and a uniform kernel. Columns (B) and (C) use bandwidths of 15 and 45 positions. Column (D) uses the RD bandwidth from the benchmark method of Calonico et al. (2014) estimated separately for each sample and outcome variable. Column (E) uses a triangular kernel with a bandwidth of 30 positions.

Parentheses contain standard errors clustered at the individual level.

Table A2. Robustness to RD specification — Non-STEM applicants

	(A)	(B)	(C)	(D)	(E)
Bandwidth:	$h = 30$	$h = 15$	$h = 45$	CCT	$h = 30$
Kernel:	Uniform	Uniform	Uniform	Uniform	Triang.
Panel A. All applicants					
Enrolled in Univalle program	0.784	0.765	0.788	0.785	0.771
	(0.013)	(0.018)	(0.011)	(0.012)	(0.014)
Graduated from Univalle program	0.498	0.514	0.499	0.501	0.509
	(0.018)	(0.024)	(0.015)	(0.014)	(0.020)
Employed in formal sector in 2017	0.035	0.034	0.018	0.022	0.028
	(0.025)	(0.035)	(0.021)	(0.020)	(0.027)
Log monthly earnings in 2017	−0.047	−0.009	−0.070	−0.050	−0.058
	(0.051)	(0.071)	(0.043)	(0.038)	(0.056)
N	7,664	4,439	10,026	8,203	7,476
Panel B. Less-prepared applicants					
Enrolled in Univalle program	0.808	0.799	0.811	0.804	0.801
	(0.018)	(0.025)	(0.015)	(0.017)	(0.020)
Graduated from Univalle program	0.475	0.495	0.491	0.485	0.488
	(0.025)	(0.034)	(0.022)	(0.019)	(0.027)
Employed in formal sector in 2017	0.038	0.072	0.001	0.018	0.040
	(0.036)	(0.049)	(0.030)	(0.032)	(0.038)
Log monthly earnings in 2017	−0.093	−0.054	−0.094	−0.086	−0.112
	(0.074)	(0.108)	(0.061)	(0.058)	(0.082)
N	3,773	2,208	4,870	4,170	3,681
Panel C. More-prepared applicants					
Enrolled in Univalle program	0.765	0.740	0.770	0.773	0.746
	(0.019)	(0.027)	(0.015)	(0.016)	(0.021)
Graduated from Univalle program	0.519	0.533	0.507	0.516	0.527
	(0.027)	(0.037)	(0.023)	(0.021)	(0.029)
Employed in formal sector in 2017	0.032	−0.014	0.032	0.024	0.017
	(0.035)	(0.051)	(0.030)	(0.026)	(0.039)
Log monthly earnings in 2017	−0.026	−0.051	−0.067	−0.063	−0.066
	(0.074)	(0.106)	(0.062)	(0.055)	(0.079)
N	3,884	2,225	5,149	4,797	3,788

Notes: This table displays RD coefficients for non-STEM applicants with different bandwidths and kernels. Panel A shows estimates for our full sample of non-STEM applicants and Panels B–C show estimates separately for less- and more-prepared applicants. The specifications are the same as in Table 3 and Appendix Table A6, but we vary the bandwidth or kernel as indicated in the column header. Column (A) replicates our benchmark results from those tables, which use an RD bandwidth of 30 positions and a uniform kernel. Columns (B) and (C) use bandwidths of 15 and 45 positions. Column (D) uses the RD bandwidth from the benchmark method of Calonico et al. (2014) estimated separately for each sample and outcome variable. Column (E) uses a triangular kernel with a bandwidth of 30 positions.

Parentheses contain standard errors clustered at the individual level.

Table A3. RD balance tests

	(A)	(B)	(C)	(D)	(E)	(F)
	STEM applicants			Non-STEM applicants		
Dependent variable	All	Less-prepared	More-prepared	All	Less-prepared	More-prepared
Panel A. Balance tests using individual characteristics						
ICFES percentile	0.002	0.004	0.003	0.003	0.001	0.004
	(0.005)	(0.008)	(0.006)	(0.007)	(0.010)	(0.008)
Age	0.022	0.006	0.032	−0.155	−0.180	−0.125
	(0.114)	(0.172)	(0.146)	(0.120)	(0.196)	(0.156)
College educated father	−0.011	−0.019	−0.003	0.003	−0.053	0.033
	(0.024)	(0.035)	(0.034)	(0.022)	(0.032)	(0.030)
College educated mother	0.021	0.004	0.046	0.003	−0.020	0.016
	(0.022)	(0.033)	(0.032)	(0.020)	(0.030)	(0.029)
Family income > 2x min wage	0.016	0.001	0.021	0.035	0.006	0.047
	(0.023)	(0.033)	(0.032)	(0.021)	(0.031)	(0.029)
Female	−0.024	−0.019	−0.031	−0.004	0.008	−0.004
	(0.020)	(0.029)	(0.029)	(0.020)	(0.030)	(0.028)
N	6,699	3,309	3,391	7,664	3,780	3,888
p value: Jointly zero	0.693	0.980	0.622	0.566	0.606	0.707
Panel B. Balance tests using predicted outcomes						
Enrolled in Univalle program	0.007	0.009	0.009	0.002	0.003	−0.001
	(0.007)	(0.011)	(0.009)	(0.006)	(0.009)	(0.008)
Graduated from Univalle program	−0.006	−0.006	−0.005	0.002	−0.005	0.008
	(0.005)	(0.008)	(0.007)	(0.005)	(0.007)	(0.006)
Employed in formal sector in 2017	−0.000	−0.001	0.000	−0.001	−0.001	−0.001
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Log monthly earnings in 2017	−0.002	−0.005	0.002	0.003	−0.007	0.009
	(0.007)	(0.011)	(0.009)	(0.006)	(0.009)	(0.007)
N (w/ all characteristics defined)	5,031	2,419	2,605	5,345	2,573	2,765

Notes: This table displays RD balance tests. We estimate our reduced form RD specification (1) using the dependent variable listed in the row header and display the θ coefficient. In Panel A, the dependent variables are individual characteristics, and the last row reports p values from F tests that the coefficients on all characteristics are jointly equal to zero. In Panel B, the dependent variables are predicted outcomes based on individual characteristics. To define these predicted outcomes, we regress the outcome listed in the row header on all of the covariates from Panel A and application pool dummies. We estimate these regressions separately for STEM and non-STEM applicants and take the predicted values. We then use these predicted outcomes as dependent variables in the balance tests in Panel B.

Columns (A)–(C) include applicants to STEM programs and columns (D)–(F) include non-STEM applicants. Columns (A) and (D) include all applicants to these programs. Columns (B) and (E) include less-prepared applicants. Columns (C) and (F) include more-prepared applicants. We define our less- and more-prepared samples as described in the notes to Table 5.

Parentheses contain standard errors clustered at the individual level.

Table A4. Formal and informal sector monthly earnings in 2017

	(A)	(B)	(C)	(D)	(E)	(F)	(G)
				Mean earnings (1000s of COP)		SD of earnings (1000s of COP)	
Education/industry group	N	Prop. employed	Prop. in formal sector	Formal sector	Informal sector	Formal sector	Informal sector
Panel A. By highest degree completed							
High school degree	29,204	0.78	0.50	1,069	670	650	527
Technical degree	12,710	0.82	0.70	1,237	742	780	694
Bachelor's degree	10,440	0.86	0.80	2,459	1,291	2,338	1,562
All high school and above	52,354	0.80	0.60	1,435	732	1,398	716
Panel B. By industry of employment							
A. Agriculture and livestock	917	1.00	0.30	980	552	451	513
B. Fishing	37	1.00	0.09	790	527	545	265
C. Mining	241	1.00	0.81	3,371	663	4,160	389
D. Manufacturing	4,691	1.00	0.68	1,264	723	1,331	621
E. Electricity, gas and water utilities	408	1.00	0.96	1,506	617	1,299	147
F. Construction	1,962	1.00	0.41	1,590	844	1,784	485
G. Wholesale and retail	9,688	1.00	0.50	1,218	706	1,318	821
H. Hotels and restaurants	2,659	1.00	0.39	1,352	705	1,251	541
I. Transportation and communications	3,878	1.00	0.48	1,328	820	1,325	536
J. Financial organizations	1,059	1.00	0.88	1,909	1,236	2,030	1,911
K. Real estate and business	3,766	1.00	0.70	1,618	984	1,495	1,085
L. Public administration and defense	2,719	1.00	0.99	1,846	1,255	1,129	882
M. Education	2,326	1.00	0.88	1,601	517	1,422	438
N. Social and health services	3,588	1.00	0.87	1,344	705	931	873
O. Other community services	2,550	1.00	0.39	1,138	628	779	558
P. Domestic services	801	1.00	0.13	918	664	362	686

Notes: This table shows formal and informal sector earnings in 2017 from the GEIH Colombian household survey (*Gran Encuesta Integrada de Hogares*). The sample includes all individuals surveyed between January 2017 and December 2017 who were born between 1980–1986 and whose highest degree is high school, technical college, or university. Panel A presents summary statistics by individuals' highest degree. Panel B displays statistics by workers' industry of employment, defined by the section categories in the third revision of the CIIU economic activity codes (*Clasificación Industrial Internacional Uniforme*).

Column (A) shows the number of surveyed individuals. Column (B) shows the proportion employed at the time of the survey. Column (C) shows the proportion of employed individuals who worked in the formal sector; we define formally-employed workers as those who either: 1) have a written contract; or 2) run a business that is registered with a government agency. This is our best approximation of the definition of formal employment that we use throughout the paper, which is having earnings at a firm that is tracked by the Ministry of Social Protection. Column (D) shows mean monthly earnings for formal sector workers in thousands of Colombian Pesos and column (E) shows mean monthly earnings for informal sector workers. Columns (F)–(G) show the standard deviation of monthly earnings in the formal and informal sectors. All statistics in columns (B)–(G) are computed using survey weights.

Table A5. Pre-2000 ICFES subject score weights in admission scores and outcomes

(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
	STEM programs				Other programs			
ICFES subject (Pre-2000 exam)	Admit score	Grad-uated	Exit score	Log earnings	Admit score	Grad-uated	Exit score	Log earnings
Biology	0.13	−0.38	0.03	−0.03	0.06	−0.09	0.17	0.08
Chemistry	0.10	1.97	0.09	1.01	0.05	1.20	0.01	0.15
Math aptitude	0.22	−0.10	0.22	−0.17	0.13	−0.03	0.46	0.28
Math knowledge	0.19	0.11	0.02	0.55	0.06	−0.54	−0.55	0.11
Physics	0.12	−0.08	0.05	0.46	0.07	0.12	0.27	0.07
Language arts	0.13	−0.37	0.44	0.00	0.34	0.19	0.68	−0.09
Social science	0.11	−0.14	0.15	−0.82	0.29	0.16	−0.04	0.40
Quantitative subjects	0.76	1.51	0.41	1.82	0.38	0.65	0.36	0.69
Qualitative subjects	0.24	−0.51	0.59	−0.82	0.62	0.35	0.64	0.31
Mean absolute deviation from admit score		0.58	0.15	0.60		0.33	0.23	0.16
N (enrollees w/ outcome)	1,007	1,007	302	742	1,042	1,042	372	743

Notes: This table shows how subject scores on the ICFES exam relate to four outcomes: 1) the Univalle admission score; 2) an indicator for graduating from the Univalle program; 3) scores on a field-specific college *exit* exam called *Saber Pro* (formerly *ECAES*); and 4) log monthly earnings in 2017. We regress each outcome variable on the nine ICFES subject scores using all Univalle enrollees in our sample who took the pre-2000 version of the ICFES. (See Table 4 for analogous results using post-2000 ICFES exam takers.) We run these regressions separately for each of Univalle's 48 programs and normalize the estimated coefficients to sum to one. Columns (A)–(D) show the subject weights for each outcome averaged across Univalle's 18 STEM programs. Columns (E)–(H) show the subject weights for each outcome averaged across Univalle's 30 non-STEM programs. We report the sum of the weights for quantitative subjects (biology, chemistry, math, and physics) and qualitative subjects (geography, history, interdisciplinary, language arts, and philosophy). We also report the mean absolute deviation between the average admission score weights (columns A and D) and the average weights for each other outcome (columns B–D and F–H). The last row shows the number of Univalle enrollees for which each outcome is defined.

Table A6. Heterogeneity in returns to Univalle enrollment by academic preparation
Applicants to non-STEM programs

	(A)	(B)	(C)	(D)	(E)
	Less-prepared applicants		More-prepared applicants		
Dependent variable	Mean below threshold	2SLS coef	Mean below threshold	2SLS coef	p value diff
Panel A. First stage					
Enrolled in Univalle program	0.077	0.808	0.134	0.765	0.105
		(0.018)		(0.019)	
N		3,773		3,884	
Panel B. 2SLS regressions					
Graduated from Univalle program	−0.000	0.475	0.000	0.519	0.235
		(0.025)		(0.027)	
Employed in formal sector in 2017	0.674	0.038	0.699	0.032	0.899
		(0.036)		(0.035)	
Log monthly earnings in 2017	14.172	−0.093	14.142	−0.026	0.524
		(0.074)		(0.074)	
N (with earnings defined)		2,654		2,771	
Panel C. Log monthly earnings returns with imputed informal earnings					
$\beta^{\text{Informal}} = 0$	13.979	−0.032	13.956	0.014	
$\beta^{\text{Informal}} = 0.133$	13.979	0.006	13.956	0.050	
$\beta^{\text{Informal}} = 0.266$	13.979	0.045	13.956	0.086	
N		3,773		3,884	

Notes: This table displays RD coefficients from separate regressions for less-prepared (columns A–B) and more-prepared (columns C–D) applicants to Univalle's non-STEM programs. We define the less- and more-prepared samples using a leave-cohort-out version of the graduation ICFES score weights from column (F) of Table 4. Specifically, for each program m and cohort t, we regress an indicator for Univalle graduation on the ICFES subject scores in a sample that includes all enrollees in program m in cohorts *other than* t. We take the predicted values from this regression as a measure of the graduation propensity of applicants to program m and t. Lastly, we regress graduation propensity on individuals' admission ranks with application pool dummies and take the residuals from this regression. Less-prepared applicants are those with below median residuals of graduation propensity in their application pool. More-prepared applicants are those with above median residuals.

Columns (A) and (C) present means of each dependent variable for applicants who were just below the admission thresholds. In Panel A, these columns show means over all applicants who were 1–5 positions below the thresholds. In Panels B–C, these columns show control complier means estimated following Katz et al. (2001).

Columns (B) and (D) present RD coefficients using samples of applicants within 30 positions of the admission thresholds. Panel A displays reduced-form RD coefficients, θ, from equation (1). Panel B displays 2SLS RD coefficients, β, from equation (2) using the dependent variable listed in the row header. Panel C displays 2SLS RD coefficients for log monthly earnings in which we impute values for individuals with missing earnings; see the notes to Table 3 for details on this imputation method. We estimate the 2SLS RD specification (1)–(2) using log monthly earnings including these imputed values as the outcome and display the θ coefficients in Panel C.

Column (E) displays the p value from an F test for equality of the RD coefficients in columns (B) and (D).

Parentheses contain standard errors clustered at the individual level.

TABLE A7. Heterogeneity in returns to Univalle STEM enrollment using alternative measures of academic preparation

	(A)	(B)	(C)	(D)	(E)
	Less-prepared applicants		More-prepared applicants		
Dependent variable	Mean below threshold	2SLS coef	Mean below threshold	2SLS coef	p value diff
Panel A. Graduation propensity in national administrative data					
Enrolled in Univalle program	0.158	0.728	0.141	0.762	0.263
		(0.022)		(0.021)	
Graduated from Univalle program	−0.000	0.267	0.000	0.397	0.001
		(0.029)		(0.028)	
Employed in formal sector in 2017	0.706	0.041	0.715	0.012	0.609
		(0.042)		(0.038)	
Log monthly earnings in 2017	13.959	0.253	14.356	0.004	0.044
		(0.091)		(0.085)	
N		3,307		3,390	
Panel B. Predicted log monthly earnings					
Enrolled in Univalle program	0.138	0.751	0.161	0.736	0.602
		(0.021)		(0.022)	
Graduated from Univalle program	0.000	0.290	0.000	0.393	0.012
		(0.028)		(0.029)	
Employed in formal sector in 2017	0.714	0.004	0.701	0.051	0.410
		(0.041)		(0.039)	
Log monthly earnings in 2017	14.009	0.188	14.316	0.092	0.435
		(0.087)		(0.089)	
N		3,306		3,390	

Notes: This table displays RD coefficients for less- and more-prepared STEM applicants using different measures of academic preparation. The specifications and dependent variables are the same as in Table 5, but we define our less- and more-prepared samples using two different methods. In Panel A, we define graduation propensity using our national Ministry of Education data rather than our sample of Univalle applicants. We regress an indicator for graduating from any college program on the ICFES subject scores in a sample that includes all 1998–2003 ICFES exam takers except for those who appear in our Univalle sample. We estimate this regression separately for each program area in the Ministry's data, and we take the predicted values from this regression as a measure of the graduation propensity for our Univalle sample based on the area of the program the applicant applied to. We then define our less- and more-prepared samples in the same way as in Table 5. In Panel B, we define academic preparation based on predicted earnings rather than graduation propensity within our Univalle sample. The method is the same as in Table 5, except we use log monthly earnings in 2017 rather than an indicator for Univalle graduation to define the two samples.

Columns (A)–(B) show results for less-prepared applicants defined in these two ways, and columns (C)–(D) show results for more-prepared applicants. Columns (A) and (C) present means of each dependent variable for applicants who were just below the admission thresholds. Columns (B) and (D) present RD coefficients using samples of applicants within 30 positions of the admission thresholds. Column (E) displays the p value from an F test for equality of the RD coefficients in columns (B) and (D).

Parentheses contain standard errors clustered at the individual level.

Table A8. Heterogeneity in returns to STEM enrollment by ICFES year and academic preparation

	(A)	(B)	(C)	(D)	(E)	(F)
	All levels of academic preparation		Less-prepared applicants		More-prepared applicants	
Dependent variable	Pre-2000	Post-2000	Pre-2000	Post-2000	Pre-2000	Post-2000
Panel A. Graduation and earnings						
Graduated from Univalle program	0.466	0.323	0.401	0.259	0.529	0.371
	(0.051)	(0.024)	(0.072)	(0.035)	(0.075)	(0.034)
Log monthly earnings in 2017	0.095	0.111	0.335	0.194	−0.124	0.039
	(0.168)	(0.072)	(0.264)	(0.111)	(0.210)	(0.099)
Panel B. Enrollment in college programs						
Enrolled in any STEM BA program	0.559	0.550	0.600	0.603	0.543	0.499
	(0.059)	(0.027)	(0.088)	(0.041)	(0.081)	(0.037)
Enrolled in any BA program	0.320	0.260	0.393	0.302	0.275	0.210
	(0.053)	(0.025)	(0.083)	(0.039)	(0.069)	(0.034)
Enrolled in any college program	0.236	0.142	0.245	0.173	0.246	0.104
	(0.049)	(0.022)	(0.075)	(0.035)	(0.066)	(0.030)
Panel C. Log mean earnings in college program						
Mean earnings in college	0.054	0.026	0.063	0.041	0.040	0.012
	(0.024)	(0.011)	(0.036)	(0.016)	(0.032)	(0.015)
Mean earnings in major	0.082	0.095	0.085	0.131	0.081	0.064
	(0.029)	(0.013)	(0.043)	(0.020)	(0.041)	(0.018)
Mean earnings in college/major	0.197	0.154	0.232	0.187	0.162	0.125
	(0.034)	(0.015)	(0.048)	(0.021)	(0.051)	(0.021)
N	1,062	4,912	521	2,434	541	2,477

Notes: This table displays heterogeneity in the returns to Univalle STEM enrollment by academic preparation and applicants' version of the ICFES exam. The sample, specifications, and dependent variables are the same as in Tables 5–6. In column (A), the sample includes applicants who took the pre-2000 version of the ICFES exam (1998–1999 cohorts). Column (B) includes applicants with post-2000 ICFES scores (2000–2003 cohorts). Columns (C)–(D) include less-prepared applicants with pre- and post-2000 ICFES scores. Columns (E)–(F) include more-prepared applicants with pre- and post-2000 ICFES scores. We define our less- and more-prepared samples as described in the notes to Table 5.

All columns displays 2SLS RD coefficients β from equations (1)–(2). Panel A shows effects of Univalle STEM enrollment on Univalle graduation and log monthly earnings in 2017, as in Panel B of Table 5. Panel B shows effects of Univalle STEM enrollment on college program characteristics using our national higher education census data, as in Panel A of Table 6. Panel C shows effects of Univalle STEM enrollment on log mean earnings in an applicant's college and/or major, as in Panel C of Table 6. Parentheses contain standard errors clustered at the individual level.

Table A9. Heterogeneity in returns to STEM enrollment by gender and academic preparation

	(A)	(B)	(C)	(D)	(E)	(F)
	All levels of academic preparation		Less-prepared applicants		More-prepared applicants	
Dependent variable	Men	Women	Men	Women	Men	Women
Panel A. Graduation and earnings						
Graduated from Univalle program	0.306	0.422	0.232	0.373	0.348	0.469
	(0.025)	(0.034)	(0.037)	(0.051)	(0.036)	(0.050)
Log monthly earnings in 2017	0.173	0.061	0.287	0.141	0.032	0.071
	(0.081)	(0.096)	(0.127)	(0.147)	(0.105)	(0.144)
Panel B. Enrollment in college programs						
Enrolled in any STEM BA program	0.479	0.635	0.540	0.625	0.411	0.646
	(0.029)	(0.037)	(0.044)	(0.055)	(0.039)	(0.050)
Enrolled in any BA program	0.271	0.252	0.304	0.315	0.229	0.187
	(0.027)	(0.035)	(0.042)	(0.053)	(0.035)	(0.048)
Enrolled in any college program	0.168	0.151	0.171	0.203	0.151	0.102
	(0.024)	(0.032)	(0.037)	(0.047)	(0.032)	(0.044)
Panel C. Log mean earnings in college program						
Mean earnings in college	0.034	0.017	0.052	0.040	0.019	−0.001
	(0.012)	(0.014)	(0.017)	(0.022)	(0.016)	(0.020)
Mean earnings in major	0.090	0.074	0.101	0.113	0.078	0.031
	(0.014)	(0.017)	(0.022)	(0.026)	(0.019)	(0.025)
Mean earnings in college/major	0.170	0.135	0.198	0.172	0.149	0.096
	(0.016)	(0.021)	(0.023)	(0.028)	(0.022)	(0.031)
N	4,558	2,132	2,225	1,066	2,329	1,045

Notes: This table displays heterogeneity in the returns to Univalle STEM enrollment by academic preparation and gender. The sample, specifications, and dependent variables are the same as in Tables 5–6. In column (A), the sample includes male applicants, and column (B) includes female applicants. Columns (C)–(D) include less-prepared male and female applicants. Columns (E)–(F) include more-prepared male and female applicants. We define our less- and more-prepared samples as described in the notes to Table 5.

All columns displays 2SLS RD coefficients β from equations (1)–(2). Panel A shows effects of Univalle STEM enrollment on Univalle graduation and log monthly earnings in 2017, as in Panel B of Table 5. Panel B shows effects of Univalle STEM enrollment on college program characteristics using our national higher education census data, as in Panel A of Table 6. Panel C shows effects of Univalle STEM enrollment on log mean earnings in an applicant's college and/or major, as in Panel C of Table 6. Parentheses contain standard errors clustered at the individual level.

Table A10. Heterogeneity in returns to STEM enrollment by program type and academic preparation

	(A)	(B)	(C)	(D)	(E)	(F)
	All levels of academic preparation		Less-prepared applicants		More-prepared applicants	
Dependent variable	Eng.	N. Sci.	Eng.	N. Sci.	Eng.	N. Sci.
Panel A. Graduation and earnings						
Graduated from Univalle program	0.377	0.289	0.308	0.253	0.418	0.316
	(0.026)	(0.032)	(0.037)	(0.047)	(0.036)	(0.044)
Log monthly earnings in 2017	0.190	0.035	0.249	0.236	0.133	−0.142
	(0.080)	(0.094)	(0.122)	(0.151)	(0.106)	(0.131)
Panel B. Enrollment in college programs						
Enrolled in any STEM BA program	0.480	0.623	0.538	0.631	0.418	0.605
	(0.029)	(0.035)	(0.043)	(0.055)	(0.041)	(0.044)
Enrolled in any BA program	0.269	0.269	0.307	0.310	0.219	0.222
	(0.027)	(0.033)	(0.040)	(0.053)	(0.036)	(0.042)
Enrolled in any college program	0.165	0.171	0.200	0.162	0.126	0.163
	(0.024)	(0.030)	(0.037)	(0.046)	(0.032)	(0.040)
Panel C. Log mean earnings in college program						
Mean earnings in college	0.025	0.036	0.044	0.047	0.007	0.021
	(0.012)	(0.013)	(0.017)	(0.020)	(0.018)	(0.016)
Mean earnings in major	0.135	0.006	0.180	−0.010	0.097	0.013
	(0.014)	(0.016)	(0.021)	(0.026)	(0.020)	(0.021)
Mean earnings in college/major	0.181	0.126	0.213	0.142	0.149	0.111
	(0.016)	(0.018)	(0.022)	(0.028)	(0.024)	(0.024)
N	4,385	2,314	2,163	1,143	2,220	1,170

Notes: This table displays heterogeneity in the returns to Univalle STEM enrollment by academic preparation and type of STEM program. The sample, specifications, and dependent variables are the same as in Tables 5–6. In column (A), the sample includes applicants to Univalle's Engineering programs. Column (B) includes applicants to Univalle's Natural Science programs. Columns (C)–(D) include less-prepared applicants to Engineering and Natural Science programs. Columns (E)–(F) include more-prepared applicants to Engineering and Natural Science programs. We define our less- and more-prepared samples as described in the notes to Table 5. See the notes to Table 2 for the Univalle Engineering and Natural Science programs included in our sample.

All columns displays 2SLS RD coefficients β from equations (1)–(2). Panel A shows effects of Univalle STEM enrollment on Univalle graduation and log monthly earnings in 2017, as in Panel B of Table 5. Panel B shows effects of Univalle STEM enrollment on college program characteristics using our national higher education census data, as in Panel A of Table 6. Panel C shows effects of Univalle STEM enrollment on log mean earnings in an applicant's college and/or major, as in Panel C of Table 6. Parentheses contain standard errors clustered at the individual level.

Table A11. Graduation propensity, GPA, and log earnings by year in program for Univalle STEM graduates

Panel A. Relationship between GPA and graduation propensity by year of course

	(A)	(B)	(C)	(D)	(E)
	Dependent variable				
Covariate	Year 1 GPA	Year 2 GPA	Year 3 GPA	Year 4 GPA	Year 5 GPA
Constant	3.010	3.320	3.418	3.244	3.686
	(0.198)	(0.145)	(0.160)	(0.198)	(0.128)
Graduation propensity	1.588	0.731	0.378	0.491	0.439
	(0.448)	(0.337)	(0.353)	(0.453)	(0.282)
N	152	152	152	152	152

Panel B. Relationship between log earnings and GPA by year of course

	(A)	(B)	(C)	(D)	(E)
	Dependent variable				
Covariate	Log earnings	Log earnings	Log earnings	Log earnings	Log earnings
Year 1 GPA	0.456				
	(0.145)				
Year 2 GPA		0.555			
		(0.183)			
Year 3 GPA			0.451		
			(0.185)		
Year 4 GPA				0.376	
				(0.141)	
Year 5 GPA					0.474
					(0.191)
N	121	121	121	121	121

Notes: This table shows the relationship between graduation propensity, Univalle GPA, and log earnings for students who completed a Univalle STEM degree. The sample and definition of Univalle GPA is the same as in Panel B of Figure 3. The sample includes graduates from the 2000 and 2001 cohorts of five Univalle engineering programs for which we have transcript data: Chemical, Electrical, Electronic, Materials, and Mechanical Engineering. To compute GPA, we include only courses that were required for the major and we group courses based on the modal year in the program in which students take them. See the text in Section 4.4 for details on the transcript data and grades at Univalle.

Panel A shows results from regressions of GPA in each year on graduation propensity. Panel B shows results from regressions of log monthly earnings in 2017 on GPA in each year. All regressions include program × enrollment cohort fixed effects. Parentheses contain standard errors clustered at the individual level.

Table A12. Number of students admitted to Univalle programs by cohort

Quota variation	Program	Aug 1999	Jan 2000	Aug 2000	Jan 2001	Aug 2001	Jan 2002	Aug 2002	Jan 2003	Aug 2003	Jan 2004
		Number admitted by semester of application									
Panel A. STEM programs											
Program expansions	Biology	**101**		**99**		**82**	43	**92**	45	53	62
	Systems Eng.	62		82		**126**		61		63	
Tracking admissions	Chemical Eng.	61		**130**		66		43	41	39	36
	Electrical Eng.	56		**127**		57		45	51	49	45
	Electronic Eng.	64		**141**		71		54	44	55	46
	Mechanical Eng.	62		67		**123**		56	50	44	42
Minimal	Other programs (mean)	60	45	62	44	62	50	63	47	63	46
Panel B. Non-STEM programs											
Tracking admissions	Accounting (day)		25	**97**		**194**		**178**		**96**	
	Accounting (night)			**99**		**101**		**95**		**93**	
	Architecture	49	35	**102**		**125**		**100**		**132**	
	Business (day)	51		**106**		**196**		**184**		**100**	
	Business (night)		48	**105**		**103**		**89**		**90**	
	Foreign Trade							54		**92**	
Minimal	Other programs (mean)	38	12	51	39	50	42	48	40	47	60

Notes: This table shows the number of students in our sample who were admitted to Univalle programs in each application cohort. Columns denote the semester of application, which we observe from August (Fall) 1999 to January (Spring) 2004. Panel A includes STEM programs, as depicted in Figure 4. Panel B includes non-STEM programs.

The first six rows in each panel show programs in which the admission quotas changed significantly during this time period. In STEM, this includes two programs with class size expansions (Biology and Systems Engineering) and four programs that used "tracking" admissions (Chemical, Electrical, Electronic, and Mechanical Engineering). In all six non-STEM programs with significant quota variation, the increase in quotas was due to tracking admissions. The last row in each panel shows the mean number of admits for the other programs in our sample without significant quota variation during this time period. See Section 5.1 for details on program expansions and tracking admissions.

Bold numbers are cohorts that we define as having large quotas for our binary measure of L_{mt} (see Section 5.2).

Table A13. Single-step regressions for effects of quota expansions on returns to Univalle STEM enrollment

Dependent variable	(A) Mean below threshold	(B) Effect of quota expansion: Large quota (binary)	(C) Effect of quota expansion: 60 extra admits (integer)	(D) Effect of quota expansion: Stacked DD (binary)
Panel A. Characteristics of applicants at threshold (DD coefficients)				
Graduation propensity	0.375	−0.092	−0.088	−0.092
		(0.015)	(0.012)	(0.015)
Female	0.311	0.120	0.107	0.122
		(0.064)	(0.051)	(0.066)
College educated mother	0.351	−0.181	−0.104	−0.199
		(0.073)	(0.068)	(0.068)
Family income > 2x min wage	0.622	−0.098	−0.127	−0.099
		(0.075)	(0.053)	(0.073)
N		657	657	1,479
Panel B. Returns to Univalle enrollment (RDDD coefficients)				
Enrolled in Univalle program	0.149	0.153	0.149	0.146
		(0.058)	(0.061)	(0.059)
Graduated from Univalle program	−0.000	−0.125	−0.116	−0.132
		(0.072)	(0.057)	(0.079)
Employed in formal sector in 2017	0.704	0.033	0.041	0.009
		(0.110)	(0.088)	(0.111)
Log monthly earnings in 2017	14.168	0.401	0.211	0.347
		(0.216)	(0.206)	(0.205)
N		6,699	6,699	14,901

Notes: This table shows how the characteristics (Panel A) and outcomes (Panel B) of applicants near the admissions threshold for Univalle's STEM programs changed when the quotas increased. This table is similar to Table 7, except we estimate the DD or RDDD coefficients in a single-step using individual-level observations.

Panel A presents results from difference-in-differences (DD) regressions using a sample of STEM applicants whose admission scores were 1–5 positions below the thresholds. Column (A) shows the mean of each dependent variable, and columns (B)–(D) show π coefficients from equation (3) estimated at the individual-level in this sample.

Panel B presents results from our RD difference-in-differences (RDDD) specification using all STEM applicants whose admission scores were within 30 positions of the thresholds. Column (A) shows control complier means for each dependent variable estimated following Katz et al. (2001). Columns (B)–(D) show π coefficients from a single-step 2SLS RDDD specification, which we derive by plugging equation (3) into our first-step 2SLS specification (1)–(2). Our single-step 2SLS RDDD specification is:

$$E_{ip} = \theta_p D_{ip} + \alpha_p x_{ip} + \psi_p D_{ip} x_{ip} + \gamma_p + \epsilon_{ip} \quad \text{if } |x_{ip}| \leq h$$

$$Y_{ip} = \left(\tilde{\gamma}_m + \tilde{\gamma}_t + \pi L_{mt}\right) E_{ip} + \tilde{\alpha}_p x_{ip} + \tilde{\psi}_p D_{ip} x_{ip} + \tilde{\gamma}_p + \tilde{\epsilon}_{ip} \quad \text{if } |x_{ip}| \leq h.$$

Column (B) reports estimates of π in which the variable of interest, L_{mt}, is an indicator for programs and cohorts with large quotas (the solid symbols in Figure 4). Column (C) reports π coefficients in which we define L_{mt} as the total number of admits in each program/cohort divided by 60 (the y-axis in Figure 4). Column (D) is similar to column (B), but we "stack" our dataset so that the π coefficients are identified *only* by comparing programs with quota expansions to those without expansions. See the notes to Table 7 for details on this stacking procedure.

Regressions are at the individual level. Parentheses contain standard errors clustered at the program/cohort level.

Table A14. Effects of quota expansions on returns to non-STEM enrollment

	(A)	(B)	(C)	(D)
		Effect of quota expansion		
Dependent variable	Control complier mean	Large quota (binary)	60 extra admits (integer)	Stacked DD (binary)
Panel A. Characteristics of marginally-admitted compliers (DD coefficients)				
Graduation propensity	0.505	−0.106	−0.038	−0.106
		(0.051)	(0.026)	(0.051)
Female	0.592	−0.233	−0.063	−0.230
		(0.198)	(0.073)	(0.200)
College educated mother	0.346	−0.188	−0.023	−0.193
		(0.110)	(0.057)	(0.114)
Family income > 2x min wage	0.554	0.008	0.080	0.002
		(0.130)	(0.067)	(0.129)
N (# program/cohorts)		130	130	239
Panel B. Returns to Univalle enrollment (RDDD coefficients)				
Enrolled in Univalle program	0.106	−0.049	−0.030	−0.046
		(0.146)	(0.056)	(0.144)
Graduated from Univalle program	−0.000	−0.128	−0.072	−0.130
		(0.125)	(0.080)	(0.126)
Employed in formal sector in 2017	0.690	−0.074	0.012	−0.085
		(0.200)	(0.101)	(0.208)
Log monthly earnings in 2017	14.158	−0.375	−0.054	−0.398
		(0.325)	(0.283)	(0.319)
N (# program/cohorts)		130	130	239

Notes: This table shows how the characteristics (Panel A) and outcomes (Panel B) of applicants near the admissions threshold for Univalle's non-STEM programs changed when the quotas increased. This table is similar to Table 7, except the sample includes non-STEM applicants.

In both panels, column (A) shows control complier means for each dependent variable estimated following Katz et al. (2001). In Panel A, columns (B)–(D) show π coefficients from equation (3) in which the dependent variables are program/cohort-specific mean complier characteristics. In Panel B, columns (B)–(D) show π coefficients from equation (3) in which the dependent variables are program/cohort-specific RD coefficients, β_{mt}, from our 2SLS specification (1)–(2).

Column (B) reports estimates of π in which the variable of interest, L_{mt}, is an indicator for programs and cohorts with large quotas (the bold numbers in Appendix Table A12). Column (C) reports π coefficients in which we define L_{mt} as the total number of admits in each program/cohort divided by 60 (the number values in Appendix Table A12). Column (D) is similar to column (B), but we "stack" our dataset so that the π coefficients are identified *only* by comparing programs with quota expansions to those without expansions. We combine the six "treated" non-STEM programs into two groups based on the cohort(s) in which their quotas expanded: 1) Accounting, Architecture, and Business (Fall 2000–2003); and 2) Foreign Trade (Fall 2003). We then create two datasets that include all 24 "control" non-STEM programs plus the treated programs in each group. Lastly, we stack these datasets and estimate the DD or RDDD specification with all covariates (except L_{mt}) interacted with dummies for each dataset.

Regressions are at the program/cohort level with observations weighted by the inverse squared standard errors of the means (Panel A) and RD coefficients (Panel B). Parentheses contain standard errors clustered at the program/cohort level.

Table A15. Effects of STEM quota expansions on college program and degree characteristics

	(A)	(B)	(C)	(D)	(E)
		Effect on mean below threshold (DD coefficients)		Effect on returns to STEM enrollment (RDDD coefficients)	
Dependent variable	Mean below threshold	Large quota (binary)	60 extra admits (integer)	Large quota (binary)	60 extra admits (integer)
Panel A. Enrollment in college programs					
Enrolled in any STEM BA program	0.564	−0.177	−0.150	0.241	0.248
		(0.079)	(0.073)	(0.070)	(0.068)
Enrolled in any BA program	0.766	−0.161	−0.143	0.175	0.174
		(0.068)	(0.058)	(0.091)	(0.067)
Enrolled in any technical program	0.202	0.051	0.029	0.041	0.090
		(0.067)	(0.055)	(0.098)	(0.076)
Enrolled in any college program	0.838	−0.066	−0.069	0.092	0.123
		(0.069)	(0.059)	(0.082)	(0.054)
N (# program/cohorts)		104	104	104	104
Panel B. Log mean earnings in college program					
Mean earnings in college	14.081	−0.067	−0.048	0.045	0.040
		(0.029)	(0.028)	(0.038)	(0.032)
Mean earnings in major	14.122	−0.023	−0.024	0.010	0.038
		(0.045)	(0.039)	(0.050)	(0.040)
Mean earnings in college/major	14.131	−0.099	−0.084	0.053	0.053
		(0.049)	(0.039)	(0.046)	(0.044)
N (# program/cohorts)		104	104	104	104

Notes: This table shows how the college enrollment outcomes of applicants near the admissions threshold for Univalle's STEM programs changed when the quotas increased. Panel A shows effects on college program characteristics using our national higher education census data, as in Panel A of Table 6. Panel B shows effects on log mean earnings in an applicant's college and/or major, as in Panel C of Table 6.

Column (A) shows the mean of each dependent variable for STEM applicants 1–5 positions below the thresholds. Columns (B)–(E) show π coefficients from equation (3). In columns (B)–(C), the dependent variables are the mean outcomes of STEM applicants whose admission scores were 1–5 positions below the thresholds in each program/cohort (as in Panel A of Table 7). In columns (D)–(E), the dependent variables are program/cohort-specific RD coefficients, β_{mt}, from our 2SLS specification (1)–(2) estimated in a sample of all STEM applicants whose admission scores were within 30 positions of the thresholds (as in Panel B of Table 7). Columns (B) and (D) report estimates in which the variable of interest, L_{mt}, is an indicator for programs and cohorts with large quotas (the solid symbols in Figure 4). Columns (C) and (E) report estimates in which we define L_{mt} as the total number of admits in each program/cohort divided by 60 (the y-axis in Figure 4).

Regressions are at the program/cohort level with observations weighted by the number of observations (columns B–C) and the inverse squared standard errors of the RD coefficients (columns D–E). Parentheses contain standard errors clustered at the program/cohort level.

TABLE A16. Effects of quota expansions on returns for top Univalle STEM enrollees

	(A)	(B)	(C)	(D)
		Effect of quota expansion		
Dependent variable	Mean in small cohorts	Large quota (binary)	60 extra admits (integer)	Stacked DD (binary)
Panel A. Characteristics of top enrollees (DD coefficients)				
Graduation propensity	0.403	0.018	0.012	0.022
		(0.007)	(0.005)	(0.006)
Female	0.199	−0.004	−0.020	−0.008
		(0.022)	(0.019)	(0.028)
College educated mother	0.409	0.014	−0.000	0.014
		(0.045)	(0.039)	(0.042)
Family income > 2x min wage	0.620	−0.015	−0.030	−0.016
		(0.041)	(0.035)	(0.044)
N (# program/cohorts)		106	106	238
Panel B. Returns to Univalle enrollment (DD coefficients)				
Graduated from Univalle program	0.364	0.071	0.024	0.078
		(0.045)	(0.030)	(0.050)
Employed in formal sector in 2017	0.772	0.043	0.046	0.036
		(0.046)	(0.040)	(0.046)
Log monthly earnings in 2017	14.363	−0.004	0.009	0.016
		(0.076)	(0.060)	(0.083)
N (# program/cohorts)		106	106	238

Notes: This table shows how the characteristics (Panel A) and outcomes (Panel B) of highly-ranked Univalle STEM enrollees changed when the quotas increased. The specifications and outcome variables are similar to those in Table 7, but we use a sample of "top enrollees" in Univalle's STEM programs. To define this sample, we first compute the minimum rank of a student who was admitted and enrolled in each Univalle program and cohort. We then compute the *maximum* of these minimum ranks across all cohorts for each program. Our top enrollee sample includes all students who enrolled in the Univalle STEM program to which they applied and whose rank was higher than this maximum rank. Thus this sample contains students whose admission ranks were high enough such that they could have enrolled in *any* cohort of their program, regardless of the quota size.

The dependent variables are the mean characteristics (Panel A) and outcomes (Panel B) of top enrollees in each program/cohort. Column (A) shows the mean of each dependent variable, and columns (B)–(D) show π coefficients from equation (3). Column (B) reports estimates of π in which the variable of interest, L_{mt}, is an indicator for programs and cohorts with large quotas (the solid symbols in Figure 4). Column (C) reports π coefficients in which we define L_{mt} as the total number of admits in each program/cohort divided by 60 (the y-axis in Figure 4). Column (D) is similar to column (B), but we "stack" our dataset so that the π coefficients are identified *only* by comparing programs with quota expansions to those without expansions. See the notes to Table 7 for details on this stacked specification.

Regressions are at the program/cohort level with observations weighted by the inverse squared standard errors of the means. Parentheses contain standard errors clustered at the program/cohort level.

B. Theoretical appendix

This section presents a framework that illustrates the mechanisms through which the returns to enrolling in a selective STEM program can vary with a student's academic preparation.

We consider a population of high school graduates indexed by i with pre-college academic preparation α_i. Students can choose from a large number of college programs $p \in P$, where programs are defined by both an institution and a field of study. The set P also includes the option of not enrolling in college at all, which we denote by $p = 0$. For simplicity, our framework assumes that academic preparation, α_i, is unidimensional. In our empirical analysis, we allow individuals to have different levels of preparation for different college programs p.

We define the following potential outcomes that describe an individual's returns to enrolling in each program:

- Let v_{ip}^e represent individual i's potential skill value added from *enrolling* in program p. This term reflects, for example, the skills an individual learns in first-year courses.
- Let g_{ip} denote individual i's potential graduation outcome in program p. In other words, $g_{ip} = 1$ for individuals who would successfully complete the program if they enrolled and $g_{ip} = 0$ for individuals who would drop out.
- Let v_{ip}^g represent the additional skill that individual i would gain if they *graduate* from program p.

We assume $v_{ip}^e \geq 0$ and $v_{ip}^g \geq 0$ for all p and that $v_{i0}^e = v_{i0}^g = 0$ for the option of not attending college. Importantly, each of these three potential outcomes can depend on an individual's academic preparation, α_i.

After college, individuals enter a competitive labor market and earn a wage equal to their skill. Under the above assumptions, individual i's potential log wage from enrolling in program p is given by:

$$w_{ip} = \alpha_i + v_{ip}^e + g_{ip} v_{ip}^g. \tag{B1}$$

An individual's wage is equal to $\alpha_i + v_{ip}^e + v_{ip}^g$ if they complete program p and it is equal to $\alpha_i + v_{ip}^e$ if they drop out of the program.

Our empirical estimates pertain to a population of "compliers" for a selective STEM program that we denote by s. By "compliers," we mean a group of students who would enroll in program s if and only if they are offered admission. If these students are not admitted, they enroll in their next-choice program that we denote by $c(i) \in P$. Next-choice programs can vary across individuals in the complier group, and they may differ from program s in institution and/or field of study.

We begin by examining the average wage returns to *enrolling* in Univalle's STEM program in Section 3. We denote this return by $E[w_{is} - w_{i,c(i)}]$, where the expectation is defined over all compliers who are close to Univalle's admission threshold. Using the wage equation (B1) and that fact that g_{ip} is binary, this return is given by:

$$\begin{aligned} E[w_{is} - w_{i,c(i)}] &= E[v^e_{is} - v^e_{i,c(i)}] + \Big\{E[v^g_{is}|g_{is} = 1] - E[v^g_{i,c(i)}|g_{i,c(i)} = 1]\Big\}E[g_{is}] \\ &\quad + E[v^g_{i,c(i)}|g_{i,c(i)} = 1]E[g_{is} - g_{i,c(i)}] \end{aligned} \tag{B2}$$

Our results in Sections 4–5 show how the returns to enrolling in a Univalle STEM program vary with academic preparation. In notation this estimand is $dE[w_{is} - w_{i,c(i)}|\alpha_i = \alpha]/d\alpha$—the change in the mean wage return to program s from an increase in academic preparation, α. Using equation (B2) and letting $E_\alpha[x] \equiv E[x|\alpha_i = \alpha]$ denote the expected value of a variable x conditional on academic preparation level $\alpha_i = \alpha$, this term is given by:

(B3)

$$\begin{aligned} \frac{dE_\alpha[w_{is} - w_{i,c(i)}]}{d\alpha} &= \underbrace{\frac{dE_\alpha[v^e_{is} - v^e_{i,c(i)}]}{d\alpha}}_{\text{Term 1}} + \underbrace{\frac{dE_\alpha[v^g_{is}|g_{is} = 1]}{d\alpha}E_\alpha[g_{is}]}_{\text{Term 2}} - \underbrace{\frac{dE_\alpha[v^g_{i,c(i)}|g_{i,c(i)} = 1]}{d\alpha}E_\alpha[g_{i,c(i)}]}_{\text{Term 3}} \\ &\quad + \underbrace{\Big\{E_\alpha[v^g_{is}|g_{is} = 1] - E_\alpha[v^g_{i,c(i)}|g_{i,c(i)} = 1]\Big\}\frac{dE_\alpha[g_{is}]}{d\alpha}}_{\text{Term 4}} + \underbrace{E_\alpha[v^g_{i,c(i)}|g_{i,c(i)} = 1]\frac{dE_\alpha[g_{is} - g_{i,c(i)}]}{d\alpha}}_{\text{Term 5}} \end{aligned}$$

Our RD analysis of the returns to Univalle's STEM programs yields three main results. First, there is a positive mean earnings return to enrolling in these STEM programs for marginal admits (Table 3). Second, STEM graduation rates at Univalle increase with academic preparation (Tables 5 and 7), while the effect of enrollment on the probability of earning any college degree does not differ significantly by academic preparation (Table 6). Third, mean earnings returns to enrolling in these STEM programs *decrease* with academic preparation (Tables 5 and 7).

These results lead us to explore the mechanisms through which less-prepared students can have larger earnings returns to selective STEM programs. All else equal, earnings returns increase with the probability of graduating, but there are three reasons why returns can be larger for less-prepared students despite lower graduation rates. We summarize these three mechanisms in the following proposition.

Proposition. *Suppose that:*

(i) The skill return to graduating from program s is non-negative for all levels of academic preparation,

$$E_\alpha[v^g_{is}|g_{is} = 1] - E_\alpha[v^g_{i,c(i)}|g_{i,c(i)} = 1] \geq 0;$$

(ii) Graduation rates in program s are increasing in academic preparation,

$$\frac{dE_\alpha[g_{is}]}{d\alpha} > 0;$$

(iii) Relative graduation rates between program s and next-choice programs are unrelated to academic preparation,

$$\frac{dE_\alpha[g_{is} - g_{i,c(i)}]}{d\alpha} = 0.$$

Then if the wage return to enrolling in program s is <u>*decreasing*</u> *in academic preparation,* $dE_\alpha[w_{is} - w_{i,c(i)}]/d\alpha < 0$*, at least one of the following conditions must hold:*

(a) There is a skill return to enrolling in program s that decreases with academic preparation,

$$\frac{dE_\alpha[v^e_{is} - v^e_{i,c(i)}]}{d\alpha} < 0;$$

(b) Less-prepared students choose counterfactual programs with less degree value added,

$$\frac{dE_\alpha[v^g_{i,c(i)}|g_{i,c(i)} = 1]}{d\alpha} > 0;$$

(c) Less-prepared students have greater value added to a degree from program s,

$$\frac{dE_\alpha[v^g_{is}|g_{is} = 1]}{d\alpha} < 0.$$

This proposition follows from inspection of equation (B3). Conditions (i) and (ii) ensure that Terms 4 and 5 are non-negative. Mechanisms (a)–(c) determine the sign of Terms 1–3 since $E_\alpha[g_{is}] \geq 0$ and $E_\alpha[g_{i,c(i)}] \geq 0$.

We explore the empirical evidence on these three mechanisms in Sections 4.3–4.5 and in Section 5.5.

C. Empirical appendix

C.1. **Data and merging.** This section provides details on our data sources and merging.

Our base dataset includes lists of all applicants to Universidad del Valle's undergraduate programs from Fall 1999 to Spring 2004 (Univalle, 2017). These data were provided by Univalle, and they include the program/cohort that applicants applied to, their admission scores, and their admission decisions.

We combine the Univalle application records with three individual-level administrative datasets provided by the Colombian government. The first dataset includes records from Colombia's national standardized college entrance exam, which was formerly called the ICFES exam and is now called *Saber 11* (ICFES, 2013a). The data were provided by the agency that administers the exam and it contains all students who took the exam between 1998–2003. The ICFES exam is also used by the Colombian government for high school accountability, so it is taken by nearly every high school graduate in the country. The main variables of interest are individuals' scores on each exam subject and demographic characteristics.

The second administrative dataset includes enrollment and graduation records from the Ministry of Education (SPADIES, 2013). These records include the institution, program of study, and graduation outcome for students who enrolled in college between 1998–2012. The Ministry's records cover almost all colleges in Colombia, although it omits a few schools due to their small size or inconsistent reporting. To describe the set of colleges that are included in the Ministry of Education records, we use another administrative dataset from a college exit exam called *Saber Pro* (ICFES, 2013b). This national exit exam is administered by the same agency that runs the ICFES college admission exam and it became a requirement for graduation from any higher education institution in 2009. Column (A) in Table C1 depicts the 310 colleges that have any exit exam takers in these administrative records in 2009–2011. These colleges are categorized into the Ministry of Education's five types of higher education institutions, which are listed in descending order of their on-time program duration.[38] Column (B) shows the number of exit exam takers per year. The majority of exam takers are from university-level institutions, with fewer students from technical colleges. Column (C) shows the fraction of these 310 colleges that appear in the Ministry of Education records that we use in our analysis. These proportions are weighted by the number of exam takers depicted in column (B). Column (C) shows that the Ministry of Education records

[38] Most programs at universities require 4–5 years of study, while programs at Technical/Professional Institutes typically take 2–3 years.

Table C1. Higher education institutions in the Ministry of Education records

	(A) Number of colleges	(B) Number of exit exam takers/year	(C) Prop. of colleges in records
University	122	134,496	1.00
University Institute	103	53,338	0.88
Technology School	3	2,041	1.00
Technology Institute	47	15,092	0.82
Technical/Professional Institute	35	11,408	0.99
Total	310	216,375	0.96

Notes: Column (A) depicts the number of colleges that have *Saber Pro* exit exam takers in 2009–2011 using administrative records from the testing agency. Colleges are categorized into the Ministry of Education's five higher education institution types. Column (B) shows the number of 2009–2011 exam takers per year. Column (C) shows the proportion of colleges that appear in the Ministry of Education records, where colleges are weighted by the number of exit exam takers.

include all universities but are missing a few technical colleges.[39] Overall, 96 percent of exit exam takers attend colleges that appear in the Ministry of Education records.

The third administrative dataset includes earnings records collected by the Ministry of Social Protection (PILA, 2019). The records are from the Ministry's electronic tax record system called *Planilla Integrada de Liquidación de Aportes* (PILA). Our data include monthly earnings in 2017 for any individual who worked at a firm that was registered with the Ministry. Our main income measure is average monthly earnings, which we compute by dividing total annual earnings by the number of employment months in 2017. We also use an indicator for appearing in the PILA dataset as a measure of formal employment.

We merge the Univalle application data into the ICFES data using applicants' full names. Since the ICFES exam is required for admission to Univalle, most applicants appear in the ICFES administrative dataset. Most individuals match uniquely on name, but in cases with duplicate names we use information on ICFES exam cohort and high school location to identify the correct match.[40] Through this process, we are able to match 84 percent of individuals in the Univalle application data to the ICFES records, as shown in columns (A)–(B) in Table C4 below. The vast majority of non-matches occur because individuals took the ICFES exam prior to 1998, when our records begin.[41]

[39] The largest omitted institutions are the national police academy (*Dirección Nacional de Escuelas*) and the Ministry of Labor's national training service (*Servicio Nacional de Aprendizaje*).

[40] If there are duplicates, we select the individual who took the ICFES exam prior to Univalle application and who attended a high school in the Valle del Cauca region. If these criteria do not identify a unique ICFES exam taker, we consider the applicant to be a non-match.

[41] Many Colombians wait a year or more after high school before applying to college.

We merge the ICFES and Ministry of Education datasets using individuals' national ID numbers, birth dates, and names. We define a match from this merge as observations that have either: 1) the same ID number and a fuzzy name match; 2) the same birth date and a fuzzy name match; or 3) an exact name match for a name that is unique in both records.[42] 39 percent of the 1998–2003 ICFES exam takers appear in the Ministry of Education records, which is comparable to the higher education enrollment rate in Colombia during the same time period.[43] A better indicator of merge success is the percentage of college enrollees that appear in the admission exam records because all domestic college students must take the exam. We match 91 percent of enrollees who took the admission exam between 1998 and 2003.[44]

Lastly, the combined dataset from the above merges was matched to the PILA earnings records by the Colombian statistical agency *Departamento Administrativo Nacional de Estadística* (DANE). DANE also merged these datasets using national ID numbers, names, and birth dates. The fraction of individuals in the 1998–2003 ICFES exam cohorts who were matched to the 2017 earnings dataset is 56 percent. To benchmark this merge rate, we use Colombian household survey data (GEIH) on individuals in the 1981–1987 birth cohorts with at least a high school degree (GEIH, 2019). In this population, the fraction of individuals who worked and had a contract for their employment was also 56 percent in 2017. This suggests that the DANE merge identified nearly all individuals in our sample with formal sector jobs.

C.2. **Analysis sample.** This section provides details on the sample we use for our analysis.

Our sample includes all of Univalle's bachelor's degree programs where we can identify the effects of admission. Our initial dataset includes applicants to 74 different degree programs from Fall 1999 to Spring 2004. We exclude 26 of these programs from our sample for one of two reasons, as shown in Table C2. First, we exclude technical/professional programs to focus on bachelor's degree attainment (column C). Second, we exclude programs with fewer than two cohorts in which any applicant was rejected (column E), which is necessary for our RD difference-in-differences design. Excluded programs tend to attract fewer applicants and

[42] Nearly all students in these records have national ID numbers, but Colombians change ID numbers around age 17. Most students in the admission exam records have below-17 ID numbers (*tarjeta*), while most students in the college enrollment and earnings records have above-17 ID numbers (*cédula*). Merging using ID numbers alone would therefore lose a large majority of students.

[43] The gross tertiary enrollment rate ranged from 23 percent to 28 percent between 1998 and 2003 (World Bank World Development Indicators, available at: http://data.worldbank.org/country/colombia). This rate is not directly comparable to our merge rate because not all high school aged Colombians take the ICFES exam. About 70 percent of the secondary school aged population was enrolled in high school in this period. Dividing the tertiary enrollment ratio by the secondary enrollment ratio gives a number roughly comparable to our 39 percent merge rate.

[44] Approximately 16 percent of students in the Ministry of Education records have missing birth dates, which accounts for most of the non-matches.

Table C2. Programs excluded from sample

(A)		(B)	(C)	(D)	(E)	(F)
Faculty area	#	Program	Degree level	Application cohorts	Cohorts with rejects	Total applied
Engineering	1	Environmental Management	Technical	4	2	538
	2	Food Science	Technical	1	1	195
	3	Forest Protection	Technical	1	0	12
	4	Information Systems	Technical	1	1	201
	5	Soil and Water Conservation	Technical	4	2	253
Health	6	Prehospital Care	Technical	4	2	3,014
Humanities	7	Geography	Bachelor's	2	1	97
	8	Philosophy	Professional	2	2	106
	9	Physical Education	Professional	2	1	316
	10	Political Studies	Bachelor's	3	1	337
	11	Recreation (night)	Bachelor's	2	1	112
	12	Teaching (Biology & Chemistry)	Bachelor's	2	0	44
	13	Teaching (Elem. Math, day)	Bachelor's	1	0	34
	14	Teaching (Elem. Math, mixed)	Bachelor's	1	0	30
	15	Teaching (Elem. N. Science, day)	Bachelor's	1	1	138
	16	Teaching (Elem. N. Science, mixed)	Bachelor's	1	0	13
	17	Teaching (Math & Physics, day)	Bachelor's	1	1	65
	18	Teaching (Math & Physics, mixed)	Bachelor's	1	0	18
	19	Teaching (Modern Languages, day)	Bachelor's	1	1	39
	20	Teaching (Modern Languages, night)	Bachelor's	1	0	37
	21	Teaching (Phys. Ed. & Health)	Bachelor's	2	1	111
	22	Teaching (Physical Education, day)	Bachelor's	1	1	55
	23	Teaching (Physical Education, mixed)	Bachelor's	2	0	43
	24	Teaching (Physical Math)	Bachelor's	1	0	23
	25	Teaching (Popular Education)	Bachelor's	1	1	45
Integrated arts	26	Music	Bachelor's	1	1	110
Total			Bachelor's	44	21	5,986

Notes: Columns (A)–(B) list the Univalle programs that we exclude from our sample and their faculty areas at the university. Column (C) reports the program's degree level (technical, professional, or bachelor's). Column (D) shows the total number of application cohorts from August 1999 to January 2004. Column (E) shows the number of cohorts during this period in which any applicant was rejected. Column (F) shows the total number of applicants during this period.

were offered only a few times during our data period. Our sample includes the remaining 48 degree programs listed in Appendix Table C3.

Table C4 shows the applicants to these 48 programs that we include in our sample. Column (A) shows that our initial dataset includes 20,001 applicants to the STEM programs in our sample (Panel A) and 29,041 applicants to other programs (Panel B). We exclude applicants for the three reasons shown in columns (B)–(D) of Table C4. First, we drop applicants who do not appear in our ICFES dataset (column B), as described in Section C.1. Second, we exclude applicants in special disadvantaged admission groups who were not subject to Univalle's primary admission thresholds (column C). During this time period, Univalle maintained

Table C3. Programs included in sample

Group	Faculty area (A)	#	Program (B)	Application cohorts (C)	Total applied (D)	Main RD sample (E)
STEM	Engineering	1	Agricultural Engineering	6	532	313
		2	Chemical Engineering	7	1,220	478
		3	Civil Engineering	7	590	405
		4	Electrical Engineering	7	717	380
		5	Electronic Engineering	7	1,027	407
		6	Industrial Engineering	7	1,183	423
		7	Materials Engineering	6	857	379
		8	Mechanical Engineering	7	849	443
		9	Sanitary Engineering	4	541	274
		10	Statistics	5	627	254
		11	Systems Engineering	5	1,758	323
		12	Topographical Engineering	6	517	306
	N. sciences	13	Biology	8	2,021	567
		14	Chemical Technology (day)	3	883	238
		15	Chemical Technology (night)	3	295	212
		16	Chemistry	7	1,073	473
		17	Math	3	481	259
		18	Physics	9	851	565
Other	Administration	19	Accounting (day)	5	845	250
		20	Accounting (night)	4	758	274
		21	Business (day)	5	1,065	275
		22	Business (night)	5	770	299
		23	Foreign Trade	2	359	107
	Health	24	Audiology	5	579	294
		25	Bacteriology	5	1,657	301
		26	Dentistry	5	818	286
		27	Medicine	5	2,551	327
		28	Nursing	5	1,149	261
		29	Occupational Therapy	5	889	286
		30	Physical Therapy	5	1,742	297
	Humanities	31	History	4	531	190
		32	Recreation	2	228	123
		33	Social Work	4	1,016	233
		34	Teaching (Elem. S. Science)	2	154	108
		35	Teaching (Foreign Lang., day)	2	188	114
		36	Teaching (Foreign Lang., night)	2	107	93
		37	Teaching (History)	4	596	213
		38	Teaching (Literature)	4	588	260
		39	Teaching (Philosophy)	4	411	261
		40	Teaching (Social Science)	3	171	139
	Integrated arts	41	Architecture	6	1,346	311
		42	Communication	5	356	268
		43	Dramatic Arts	9	363	336
		44	Teaching (Music)	5	571	332
		45	Visual Arts	5	423	280
	S. sciences	46	Economics	9	983	585
		47	Psychology	5	1,264	323
		48	Sociology	4	961	238
Total				242	39,461	14,363

Notes: Columns (A)–(B) list each Univalle program in our sample and its faculty area (see Section 2.2). Column (C) shows the total number of application cohorts from August 1999 to January 2004. Column (D) reports the total number of applicants in our sample and column (E) shows the number of applicants within 30 positions of the admission thresholds.

Table C4. Analysis sample

	(A)	(B)	(C)	(D)	(E)	(F)
		Excluded applicants				
	All applicants	Missing ICFES scores	Special admission group	No rejected applicants	Full sample	RD sample
Panel A. STEM applicants						
Ability percentile	0.783		0.811	0.838	0.780	0.842
Age	18.713		19.544	18.997	18.686	18.947
College educated father	0.426		0.355	0.454	0.427	0.440
College educated mother	0.361		0.330	0.358	0.361	0.373
Family income > 2x min wage	0.576		0.470	0.612	0.576	0.599
Female	0.357		0.274	0.312	0.360	0.319
N	20,001	3,077	310	592	16,022	6,699
Panel B. Other applicants						
Ability percentile	0.735		0.778	0.846	0.733	0.810
Age	18.923		19.735	20.596	18.879	19.353
College educated father	0.408		0.431	0.370	0.408	0.424
College educated mother	0.344		0.375	0.300	0.344	0.354
Family income > 2x min wage	0.560		0.498	0.609	0.560	0.589
Female	0.637		0.539	0.541	0.641	0.588
N	29,041	4,746	462	394	23,439	7,664

Notes: Column (A) shows the total number of applicants to the 48 Univalle programs in our sample (see Appendix Table C3). Column (B) shows the number of applicants who do not appear in the ICFES dataset. Column (C) lists the number of students who were admitted through special quotas for disadvantaged groups. Column (D) shows the number of applicants to program/cohort pairs in which no applicants were rejected. Column (E) shows our full analysis sample, which is equal to column (A) minus the applicants in columns (B)–(D). Column (F) shows the subset of applicants from column (E) who are within 30 positions of the admission threshold in their application pool.

Panel A includes applicants to Univalle's STEM programs and Panel B includes applicants to non-STEM programs. Demographic characteristics are not reported in column (B) because these variables come from the ICFES dataset.

special admission quotas for disabled, indigenous, and military applicants. Third, we drop applicants from cohorts where no applicants were rejected (column D), which is necessary for our RD strategy. After these restrictions, our sample includes 16,022 STEM applicants and 23,439 applicants to other programs.

Most of our regressions focus on the subset of applicants whose admission scores are within h ranks of the tracking threshold. Our benchmark model uses $h = 30$, which is roughly the mean of the Calonico et al. (2014) bandwidths across all dependent variables. Column (F) shows that this RD sample includes 6,699 STEM applicants and 7,664 applicants to other programs. Applicants in our RD sample tend to have higher pre-college ability than those in the full sample. In addition, these applicants come from slightly more advantaged socioeconomic backgrounds, and are less likely to identify as female.