Do college students benefit from placement into higher-achieving classes?

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Abstract. We examine a unique admission system at a flagship Colombian university that tracked admitted students into high- and lower-ability classes in the same majors. In a regression discontinuity design, we find that marginal admits to the high-ability classes were less likely to pass their first-year courses, and less likely to earn a college degree in the long-run. These results suggest that college students may benefit when their academic preparation is high *relative* to that of their classmates, particularly at schools where there is a substantial risk of failing courses.

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In many education systems, students are grouped by ability into different classrooms or schools. This practice—commonly known as "tracking"—is often contentious; proponents highlight the benefits of targeted instruction, while critics argue that tracking can exacerbate inequality (Betts, 2011). In both sides of this debate, it is typically assumed that, at a minimum, students who are placed into high-ability classrooms will be better off, either because they interact with higher-achieving peers or because they experience a more supportive learning environment. Indeed, the most compelling research on within-school tracking finds large achievement gains for students who are placed into high-ability classes (Duflo, Dupas, and Kremer, 2011; Vardardottir, 2013; Card and Giuliano, 2016).

This research—along with the vast majority of the tracking literature—focuses on K–12 students, and yet ability grouping across schools and classrooms is also pervasive in higher education. At many colleges, dropout rates are high, and initial course grades are explicitly used as a gatekeeper for upper-level coursework. In such settings, there may still be advantages to taking classes with high-achieving peers, but individuals may also benefit when their academic preparation is high *relative* to that of their classmates (Arcidiacono et al., 2016). There is little research on tracking in higher education because data on coursework and grades is less readily available. Further, it is challenging to identify the impacts of tracking in college since students have substantial discretion on where they go to school and what they study.

In this paper, we ask whether college students benefit from placement into higher-achieving classrooms within the same school and major. The setting for our paper is a flagship public university in Cali, Colombia called "Univalle" (*Universidad del Valle*). Univalle is widely perceived to be the top public college in its region, and its students are high-achieving relative to the average Colombian high school graduate. But despite Univalle's selectivity and reputation, many of its students fail courses and drop out. In our data, for example, the median enrollee in Univalle's engineering programs scored at the 94^{th} percentile of the national high school exit exam. Yet 80 percent of students in these programs failed at least one of their first-year courses, and only 40 percent earned a degree.

Our analysis exploits a unique admission system at Univalle that tracked applicants into higher- and lower-achieving cohorts of the same major. In a typical year, students apply to specific majors at Univalle in cohorts that begin in either the fall or the spring, and admission is based solely on scores from a national standardized exam. But from 2000–2003, several of Univalle's architecture, business, and engineering programs used admission scores to track students into separate fall and spring cohorts. The 60 highest-scoring applicants were admitted to a fall cohort, and the next 60 applicants were admitted to a spring cohort of the same program. This tracking led to large differences in cohort mean ability for students on the margin of admission to the fall or spring cohort. On average, enrollees in the fall cohorts of these programs scored 10 percentile points higher on the national exam than enrollees in the spring cohort. Because admission cohorts often take courses together, tracking also led to large differences in the mean ability of individuals' first-year classmates.

There are numerous mechanisms through which tracking may have impacted the outcomes of Univalle students. The fall and spring Univalle cohorts took the same courses and had access to similar educational resources, including the same faculty. Yet the stark differences in the composition of their first-year classmates may have impacted their learning through multiple channels. There is evidence that students benefit from interacting with more able peers (Sacerdote, 2001) and that families prefer schools with higher-achieving students (Abdulkadiroğlu et al., 2020). On the other hand, students may exert less effort if they have a low class rank or receive failing grades (Elsner and Isphording, 2017; Murphy and Weinhardt, 2020). Tracking may also have influenced professors' level of instruction (Duflo et al., 2011) or grading behavior (Hanna and Linden, 2012). Lastly, admits to the spring cohorts also had a one-semester delay before enrolling, and they may have worked or prepared for college in the interim.

We use a regression discontinuity (RD) design to estimate the net effect of these different mechanisms on the academic and longer-run outcomes of students on the margin of the high- and lower-ability cohorts. We collected admission records for students who applied to the Univalle programs with tracking in 2000–2003. We match these data to transcript records to measure classmate characteristics and grades in every course that students took at Univalle. Lastly, we match our data to a national higher education census and to social security records to examine impacts on educational attainment and formal sector earnings measured through 2012. Our RD design identifies the reduced-form impact of admission to a higher-ability fall cohort for students with admission scores near the tracking threshold. Since Univalle is the top choice for many applicants, most admits in our sample accepted their admission offer, and there was no discontinuity in the likelihood of enrolling at the tracking threshold.

Our main finding is that marginal admits to the high-ability cohorts had *lower* grades and graduation rates than students just below the tracking threshold. Crossing the tracking threshold reduced students' grades in their first-year courses by 0.2 GPA points (roughly the difference between a B+ and a B), and, more importantly, led to a five percentage point decline in the likelihood of passing first-year courses. In the longer-run, marginal admits to the high-ability cohorts passed four fewer courses on average, and they were nearly nine percentage points less likely to earn a degree from Univalle or *any* other college. Consistent with the negative impacts on educational attainment, we also find negative point estimates on early-career formal sector employment and earnings, but these estimates are too noisy to draw strong conclusions.

Our data do not allow us to conclusively distinguish between different mechanisms, but our results suggest that students who just missed placement in the high-ability cohort benefited from the fact that they were better prepared academically than many of their classmates. Although grade curving is not standard policy at Univalle, faculty may have adjusted their grades and/or instruction levels when they taught the lower-ability spring cohorts. The negative impacts of tracking were more pronounced among students who earned lower scores on pre-college tests that were *not* used in admissions; these students were more likely to be near the very bottom of the class ability distribution if they were admitted to a fall cohort. Lastly, we also find that male students had more negative impacts of admission to the high-ability cohorts, consistent with research that finds that male students' effort is more responsive to their relative class rank (Murphy and Weinhardt, 2020; Elsner et al., 2021).

We contribute to research on the economics of tracking in education (Slavin, 1987). As Betts (2011) emphasizes, the implementation of tracking varies widely across education systems; ability grouping can occur across- or within-schools, and may or may not be accompanied by differences in curricula and resources. Some papers in this literature ask how the design or adoption of tracking policies impact average achievement or inequality in achievement (Dustmann, 2004; Hanushek and Woessmann, 2006; Brunello and Checchi, 2007; Malamud and Pop-Eleches, 2011; Cortes and Goodman, 2014). Other work asks whether students benefit from placement into gifted programs (Bui et al., 2014; Dougherty et al., 2017) or elite schools (Jackson, 2010; Pop-Eleches and Urquiola, 2013; Abdulkadiroğlu et al., 2014; Dobbie and Fryer, 2014; Dustmann et al., 2017).

Our paper is most closely related to research that examines the outcomes of students who are on the margin of high- and lower-ability classes within the same schools. Our findings differ substantially from papers that examine this type of tracking at the K–12 level (Duflo et al., 2011; Vardardottir, 2013; Card and Giuliano, 2016), suggesting that exposure to higher-ability classmates may be more harmful when preparation is important for academic progression, and when failing is a real possibility. Our results are more consistent with those in Ribas et al. (2020)—who examine a Brazilian university that used a tracking system similar to that at Univalle—but the authors find impacts on major switching, whereas we find impacts on degree completion.

More broadly, our findings are informative for higher education policies that group students into schools and classes by ability. Such policies include the use of placement exams for tracking community college students into college-level or remedial courses (Bailey et al., 2010; Martorell and McFarlin Jr, 2011; Scott-Clayton, 2012; Bergman et al., 2021), and the use of GPA thresholds for determining enrollment in STEM majors (Bleemer and Mehta, 2021). Similarly, there are ongoing debates about the desirability of using standardized exam scores for admissions to selective colleges (Rothstein, 2004; Riehl, 2022). Our results show that a student's relative academic preparation can be an important determinant of their success in college, consistent with other work that emphasizes the importance of the student/college "match" (Arcidiacono et al., 2014, 2016; Dillon and Smith, 2020). Of course, ability grouping is only one component of these policies, as they also cause students to take different courses or attend schools with different resources. But our results suggest that colleges may wish to devote extra resources to help relatively less-prepared students succeed.

The paper proceeds as follows. Section 1 describes our data, context, and Univalle's tracking admissions system. Section 2 presents our identification strategy. Section 3 presents our main results on grades, graduation rates, and earnings. Section 4 discusses mechanisms and heterogeneity. Section 5 concludes.

1. Data and context

1.1. **Univalle.** The setting for our paper is *Universidad del Valle*, or "Univalle," as it is often known. Univalle is a public flagship university in Cali, Colombia—the country's third largest city and the capital of the Valle del Cauca region. Like most Colombian flagships, Univalle offers a wide range of undergraduate and graduate programs and is much less expensive than comparable private colleges. These features make Univalle the largest and most selective university its region, and one of the more selective colleges in the country.[1]

Each year Univalle offers admission to roughly 50 undergraduate majors that we refer to as "programs." As in many countries, prospective college students in Colombia apply to both an institution and a major. Admissions at Univalle are based solely on a student's performance on a national standardized exam called the ICFES.[2] The ICFES is similar to the U.S. SAT exam, but it is taken by nearly all high school graduates in the country. Admissions are determined by a program-specific weighted average of scores on different ICFES subject tests. The highest scoring applicants are admitted, with admission cutoffs determined by the number of available slots in each program.

1.2. **Data sources.** Our analysis uses two administrative datasets from Univalle:

(1) Lists of applicants to Univalle's undergraduate programs from 2000–2003. These lists contain each applicant's admission score and admission outcome.
(2) Transcript records for all students in our sample of programs who enrolled in Univalle. The data contain course names, dates, and grades for all classes that each student took at the flagship through 2017, as well as their graduation outcome.

We combine these records with three national individual-level administrative datasets:

[1] Univalle is often ranked roughly 10$^{\text{th}}$ in national rankings of Colombian universities; see, for example: https://www.webometrics.info/es/latin_america_es/colombia.
[2] The ICFES exam is now called Saber 11, but we use the name from the time period of our data.

(1) Records from the ICFES national standardized college admission exam that include all students who took the exam in 1998–2003. These data contain students' test scores on approximately eight different exam subjects and their demographic characteristics.
(2) Records from the Ministry of Education on students who enrolled in nearly all colleges in the country between 1998–2012.[3] These records contain each student's institution, program of study, date of entry and exit from college, and graduation outcome.
(3) Earnings records from the Ministry of Social Protection for the years 2008–2012. These data contain monthly earnings and days of employment for any college enrollee working in the formal sector.

We link the Univalle and administrative data sources using individuals' names, birthdates, and ID numbers. Appendix B provides details on the data, merge process, and resulting sample. Our final dataset allows us to observe admission scores, college choices, graduation outcomes, and earnings for all Univalle applicants, even if they attended another university. We observe college courses and grades only for applicants who enrolled in Univalle. We discuss this potential sample selection concern below.

1.3. **Tracking admissions.** Univalle, like many Colombian colleges, offer programs that begin in both January and August. Semi-annual admissions are the norm in Colombia because high schools also operate on two different academic calendars (de Roux and Riehl, 2022).[4] Throughout the paper, we use the term "cohort" to refer to the group of students who *began* a program in the same year and semester. Univalle typically conducts admissions separately for the fall and spring cohorts of each program, i.e., students apply separately to each cohort depending on when they wish to begin the program.

From 2000–2003, however, Univalle used a unique form of tracking admissions for several programs. Table 1 gives an example of tracking for Univalle's architecture program. In 2003, 426 students applied to the architecture cohort that would begin in the fall of that year. Univalle computed admission scores that were weighted averages of applicants' ICFES subject scores. The top 60 students based on this score were admitted to the fall cohort (August 2003). The next 62 students were also admitted, but to an architecture cohort that began in the spring (January 2004). All other applicants were rejected. We call this "tracking admissions" because students were tracked into fall and spring cohorts by a measure of academic preparation (admission scores).

[3] College admissions in Colombia are decentralized; students apply to individual schools and each institution determines its own criteria. Nonetheless the Ministry tracks enrollment and graduation at almost all colleges.
[4] Most public high schools in Colombia start the school year in January, while some elite private high schools begin in August. During the period of our data, however, nearly all public *and* private high schools in the Valle del Cauca region operated on the August calendar. Thus Univalle would offer its most popular programs in both semesters, while less popular programs would typically be offered only in the fall semester.

Eleven different Univalle programs used tracking admissions during 2000–2003. Architecture, accounting (daytime and nighttime), and business administration (daytime and nighttime) had tracking admissions for each year in 2000–2003. Foreign trade and five engineering programs each used tracking for a single year during this time period. All other Univalle programs did not use tracking; they were either offered only once per year or else had separate admissions for fall and spring cohorts.[5] Appendix Table A1 provides details on applications and admissions for each program in our sample.

1.4. **Sample.** Our sample includes students who applied to these 11 Univalle programs in the year(s) in which they used tracking admissions. We include only students who were *admitted* to either a fall or spring cohort, and thus exclude rejected applicants (e.g., those below the second threshold in Table 1). For heterogeneity analyses, we group programs into three areas based on their faculty organization within the university—architecture, business, and engineering—as students in these program groups have similar characteristics and take similar courses. We drop 94 individuals who were admitted through reserved quotas for indigenous and military applicants, as their admission status was not determined by the general tracking cutoff.[6] Appendix B provides details on our sample.

Panel A of Table 2 shows application and admission statistics for the programs in our sample. Our sample includes 46 application pools across all programs and years, where "application pool" refers to the set of students who applied to the same program at the same time and thus faced the same tracking cutoff. From 2000–2002, programs had two separate application pools each fall because the ICFES exam underwent a major reform in 2000 (Riehl, 2022). Univalle allowed students to apply using either old or new exam scores, and had separate cutoffs for the two groups. After 2002, applicants could only use post-reform ICFES scores. Roughly 6,700 students applied to programs with tracking admissions in these years. Nearly 3,200 applicants were admitted, with admissions divided roughly equally between the fall and spring cohorts. As we show below, most students accepted their admission offer since Univalle is considered the top college in the area. This led to cohorts of 54 students per program on average.

Panel B of Table 2 describes the characteristics of students who enrolled in Univalle. The average enrollee scored at the 79$^{\text{th}}$ percentile of the national ICFES exam, and the median enrollee scored at the 85$^{\text{th}}$ percentile. Roughly half of enrollees were female, although women

[5] The adoption of tracking admissions was a department-level decision, and thus not all programs adopted tracking. Anecdotally, the switch to tracking was driven by a desire to reduce the amount of admission work, but all programs in our sample had switched back to semi-annual admissions by 2005.

[6] Roughly three-quarters of students admitted through reserved quotas began in the fall cohort, and one-quarter began in the spring cohort.

were overrepresented in business programs and underrepresented in architecture and engineering programs. Business students tended to be older, and they had lower socioeconomic status (SES) as measured by mother's education.

Panel C of Table 2 displays summary statistics on the academic performance and labor market outcomes of Univalle enrollees. Despite Univalle's selectivity, it is common for students to fail courses, and many students do not earn a degree. Only one-third of Univalle enrollees in our sample passed *all* of their first-year required courses on the first try, and roughly half of all students did not graduate.[7] This suggests that students have significant uncertainty about their suitability for college coursework at the time of enrollment, although other financial and personal factors affect the likelihood of degree completion. The last row of Table 2 shows total formal sector earnings during the period of 2008–2012 (converted to 2012 U.S. dollars). The average Univalle enrollee in our sample earned roughly $18,500 during this five year period, and mean earnings were much higher in business and engineering programs than in architecture.[8]

1.5. **Potential mechanisms.** There are a variety of potential channels through which Univalle's tracking admissions may have impacted students' outcomes. As we show below, tracking led to large differences in the mean ability of an individual's cohort and classroom peers. This difference may have impacted individuals' outcomes through peer effects on their learning, broadly defined. Many education models assume higher peer mean ability raises individual achievement (e.g., Epple and Romano, 1998), and this is often an implicit assumption in debates on tracking (Slavin, 1987). There is evidence that students benefit from interacting with more able peers (Sacerdote, 2001), including in the context of tracking (Duflo et al., 2011). High-achieving peers may also raise individual motivation; Card and Giuliano (2016) provide evidence that placement into high-ability classes can reduce negative peer pressures. On the other hand, other research finds that students exert less effort if they have a low class rank (Murphy and Weinhardt, 2020; Elsner et al., 2021), which could reduce learning for individuals who are near the bottom of the ability distribution in high-achieving classrooms.

Univalle's admission system was a form of within-school and -major tracking, and thus in many ways, students in high- and lower-ability classes had access to similar educational inputs. For example, the fall and spring cohorts were supported by the same financial

[7] In the Ministry of Education data, 43 percent of students who enrolled in 2000 in *any* Univalle program had graduated by 2012. This is about the same as the average graduation rate across all other Colombian colleges in this dataset.

[8] Table 2 shows that the mean Univalle enrollee in our sample earned $18 U.S. dollars per day of formal employment over 2008–2012. This is slightly below the average daily earnings measured across all 2000–2003 college enrollees in our national Ministry of Education data ($20 U.S. dollars per day), although there is significant heterogeneity in earnings across fields of study and geographic areas.

resources, student services, and administrators. As we show below, these students also took many of the same courses since they were required for the major. Our data do not include information on professor identity, but we suspect that the fall and spring cohort courses in our sample were largely taught by the same faculty (although frequently in multiple sections). In Colombia, it is common for faculty to teach the same courses in both the fall and spring semesters since semi-annual admissions are the norm. We reviewed course catalogues from 2005–2021 for Univalle's economics program (which is not in our sample) and found that when first-year required courses were offered in both semesters, they were taught by the same faculty member 73 percent of the time.[9]

While the curriculum and faculty were likely similar for the fall and spring cohorts, Univalle professors may still have adjusted their teaching or assignments in the two cohort groups. Our discussions with Univalle administrators suggest that their faculty, like at many colleges, have autonomy over how they teach and evaluate students. Thus faculty may have adjusted the content of their lectures or exams in response to the different ability distributions in the fall and spring cohorts. Duflo et al. (2011) provide direct evidence that teachers adapt their level of instruction in response to tracking. Such responses may have affected students' learning in the classroom or their study behavior, and they may also have impacted students' grades even if there were no learning effects.

Another potential mechanism is responses in professors' grading behavior. Grade curving is not explicit policy at Univalle, and thus we do not think that our results below are the result of professors using fixed curves. We spoke with a former Univalle undergraduate student who told us that he had never heard of a grade curve at Univalle. Further, we reviewed 52 syllabi from Univalle's 2017 architecture courses, and none explicitly mentioned a curve.[10] It is nonetheless possible that—conditional on student performance—some Univalle faculty adjusted their grades in response to tracking.[11] For example, faculty may have adjusted grades upward when they realized that mean ability in the spring cohort was lower than they were used to.

A final category of potential mechanisms is related to timing of Univalle enrollment. In our setting, students who were tracked into the lower-ability spring cohorts had to wait six months before beginning their program at Univalle. Academic breaks between high school and college are common in Colombia; the majority of college students do not begin immediately after high school graduation (de Roux and Riehl, 2022). Thus we suspect that

[9] We accessed these course catalogues in February 2022 at: https://socioeconomia.univalle.edu.co/economia.
[10] We accessed these syllabi in February 2022 at: https://arquitectura.univalle.edu.co/pensum-arquitectura.
[11] Hanna and Linden (2012) present evidence that teachers' grading behavior changes depending on the characteristics of their students.

most students who were admitted to the spring cohort did not find the delay to be unusually onerous. Nonetheless, the enrollment delay may have increased or reduced students' academic preparation through a variety of channels. Some research highlights the potential for learning loss during academic breaks (Cooper et al., 1996), while other work finds that students who are old for their grade perform better on exams (Bedard and Dhuey, 2006; Black et al., 2011). It is possible that students took college prep courses during the break, although we suspect this was uncommon. It is more likely that students worked during the interim period. As we discuss in Section 3.3, many students work before and during college, which can affect the time and money they have available to support their their studies.

We now turn to our analysis of how tracking affected students' academic and labor market outcomes. We revisit the mechanisms that may explain our findings in Section 4.1.

2. Identification

2.1. **Regression discontinuity specification.** Univalle's tracking admission system lends itself to a regression discontinuity (RD) design. A large body of research uses RD designs to analyze the effects of attending a more selective university or field of study (e.g., Hoekstra, 2009; Kirkebøen et al., 2016). This is similar to analyzing the lower threshold in Table 1 because rejected applicants often attend less selective programs. In this paper, we focus instead on the upper threshold in Table 1. This allows us to compare students who were admitted to the same college and major, and ask how marginal admission to the higher-ability track affected their outcomes.

Our empirical specification is the stacked RD regression:

$$Y_{ip} = \pi D_{ip} + \alpha^b x_{ip} + \alpha^a D_{ip} x_{ip} + \gamma_p + \epsilon_{ip} \quad \text{if } |x_{ip}| \leq h^Y. \tag{1}$$

The dependent variable, Y_{ip}, is an outcome for individual i in application pool p, where p is defined by an applicant's program, year, and whether they applied with old or new ICFES scores (see Section 1.4). A small proportion of individuals i appear in our sample twice because they reapplied to Univalle, and some of our outcome variables may differ within-individual depending on the application pool (e.g., program enrollment and grades).[12] The running variable, x_{ip}, is an individual's rank in their application pool based on their admission score (e.g., column A in Table 1). We normalize x_{ip} to increase in admission scores and so the last student above the tracking threshold in each pool has $x_{ip} = 0$. D_{ip} is an indicator for having a score above the tracking threshold ($x_{ip} \geq 0$), and thus gaining admission to the

[12] Individuals who reapplied to Univalle typically did so because they declined their admission offer or dropped out of another Univalle program. Repeat applicants comprise approximately two percent of our sample. Other outcome variables are defined at the individual-level rather than the individual $\times$ application pool level (e.g., any college degree or total formal earnings).

higher-ability fall cohort of that program. We include application pool fixed effects, γ_p, and a linear spline in the running variable, x_{ip} and $D_{ip}x_{ip}$.

We focus on the effects of marginal admission to a higher-ability cohort by restricting our regressions to applicants near the tracking threshold. Our benchmark regressions include only applicants whose admission ranks are within h^Y positions of the tracking threshold. We define the bandwidth h^Y separately for each outcome variable Y using the methodology in Calonico et al. (2019), which includes an adjustment for covariates (in our case, application pool dummies). Our benchmark regressions follow the default options in Calonico et al. (2014)'s Stata package by weighting observations with a triangular kernel. Appendix Table A7 and Appendix Figure A2 show our main results using other bandwidths and kernels; we discuss the sensitivity of our findings to these choices below.

The coefficient of interest, π, estimates the effect of marginal admission to a higher-ability cohort pooling across all programs and cohorts in our sample. We also estimate equation (1) separately for the architecture, business, and engineering groups defined in Table 2. We cluster standard errors at the individual level to address correlation in outcomes from repeat applicants.[13]

2.2. **Balance tests.** The main identification assumption is that individuals near the tracking threshold do not have perfect control of their admission score (Lee and Lemieux, 2010). Although students likely have an idea about the program's quota and standards, there is uncertainty in the final admission decision stemming from other applicants. A violation of the identification assumption would arise if, for example, students could petition the admission officer to move the tracking cutoff. This is unlikely given the formulaic nature of admissions.

As a test of this assumption, Figure 1 shows how individual characteristics vary across the tracking threshold. The y-axis in both panels is a student's *predicted* first-year GPA, which is estimated from an OLS regression of first-year GPA on individual covariates including gender, age, national exam score, and socioeconomic traits. This dependent variable combines many pre-determined characteristics into a single index measured in GPA units. The x-axis is a student's (normalized) rank in their application pool, x_{ip}. Dots are means of the dependent variable in five rank bins, and lines are predicted values from separate local linear regressions above and below the threshold.

We find no evidence of a discontinuous change in individual characteristics at the tracking threshold. The sample in Panel A of Figure 1 includes all applicants, and the continuity of predicted GPA suggests that the admissions committee did not manipulate applicant ranks. In Panel B, the sample includes only students who enrolled in Univalle. This balance test is

[13] For our results on grades, we have also tried clustering standard errors at the classroom level to allow for unobserved correlation in grades due to the professor or other classroom shocks. In most cases, the individual-level standard errors that we report in Table 4 are larger than classroom-level standard errors.

important because our data only contain grades for Univalle enrollees. The characteristics of enrollees do not change significantly at the tracking threshold, and the predicted GPAs of applicants and enrollees are similar overall. The results in Figure 1 are corroborated by McCrary (2008) density tests (Appendix Figure A1) and covariate balance tests (Appendix Tables A2–A3). Applicant and enrollee traits are similar because the vast majority of applicants accepted their admission offer, as we show in the next section.

2.3. **Enrollment outcomes and cohort peer characteristics.** Panel A of Table 3 shows how tracking affected students' starting cohorts at Univalle. This panel uses four dependent variables: 1) an indicator for being admitted to and enrolling in a fall Univalle cohort; 2) an indicator for being admitted to and enrolling in a spring Univalle cohort; 3) an indicator for enrolling in the cohort that the applicant was admitted to (either fall or spring); and 4) an indicator for enrolling in *any* Univalle cohort (regardless of which cohort the applicant was admitted to).[14] Column (A) shows the mean of each dependent variable for applicants 1–5 positions below the tracking threshold, and column (B) displays estimates of the RD coefficient π from equation (1). Columns (C)–(E) present separate RD coefficients for architecture, business, and engineering programs.

The results in Panel A show that Univalle's tracking admissions affected the cohorts that applicants enrolled in but not their overall likelihood of attending Univalle. The fraction of applicants who were admitted to and enrolled in a fall cohort increases by 86 percentage points at the tracking threshold (from a base of zero below the threshold). 85 percent of below-threshold applicants enrolled in a spring cohort, and thus there is no discontinuity in overall enrollment at the threshold. Panels A–B of Figure 2 show these results graphically using RD plots that are similar to those in Figure 1. We find large discontinuities in fall cohort enrollment in each of architecture, business, and engineering, and no significant effects on the overall enrollment rate.

Panel B of Table 3 shows that admission to the fall cohort significantly increased the mean ability of an individual's cohort peers. In the first row of this panel, the dependent variable is the mean ICFES percentile in an individual's college cohort, where this percentile is defined relative to all students who took the national exam in a given year.[15] Students below the tracking threshold enrolled in cohorts where the average student scored at the 77$^{\text{th}}$ percentile (column A). On average, mean cohort ability was 10 percentile points for marginal admits to the fall cohorts (column B).[16] (See also Panel C of Figure 2.)

[14] All dependent variables in Panel A of Table 3 are equal to one only if the applicant enrolled in the specific program that they applied to at Univalle.
[15] We use individuals' average score across all core subjects of the ICFES exam.
[16] In architecture, the effects on mean cohort ability appear smaller because admission scores were based partially on a separate architecture exam administered by Univalle. We do not observe this architecture

An increase in peer ability meant that applicants just above the tracking threshold were lower in their cohort's ability distribution. We illustrate this in Panel B of Table 3 (and in Panel D of Figure 2) using individuals' percentile rank in their cohort based on admission scores as a dependent variable. Individuals just below the tracking threshold are at the 90$^{\text{th}}$ percentile of their cohort's admission score distribution on average (column A), while individuals just above the threshold are among the lowest ranked students in their cohort (column B).[17] This is a mechanical effect of tracking admissions, and it arises in all three program groups (columns C–E).

While Univalle applicants were tracked into cohorts based on a measure of ability, this meant that their cohort peers also differed with respect to other characteristics. We illustrate this in Panel B of Table 3 using three other mean cohort characteristics as dependent variables: proportion female, mean age at application, and proportion with a college educated mother. For each characteristic we find a statistically significant discontinuity at the tracking threshold, but the magnitudes are more modest than those for mean cohort ability. In addition, these magnitudes vary across architecture, business, and engineering programs depending on how admission scores related to individual characteristics.

In sum, the vast majority of applicants in our sample attended the same college program, but tracking induced large differences in the mean ability of students in their cohorts.

3. Main results

This section presents our main results on how tracking affected the academic and longer-run outcomes of Univalle students. We first examine effects on individuals' classmate characteristics and grades in first-year courses (Table 4). Next, we examine longer-run effects on educational attainment and formal sector earnings (Table 5). We show RD graphs for several of our main outcomes in Figure 3. Finally, we examine the timing of our graduation and earnings effects (Figure 4).

3.1. **First-year course grades.** We begin by showing how tracking affected individuals' performance in their first-year courses at Univalle. We focus on first-year courses that are required for the major since new enrollees typically take these courses with peers from their cohort. In our data, we define first-year required courses as those were taken by at least 75 percent of a cohort's graduates, and in which the modal graduate took the course in

exam score, and thus effects on peer ability appear smaller when measured solely by the ICFES. By contrast, business and engineering admission scores were based only on ICFES subject scores.

[17] Cohort rank does not fall perfectly from one to zero at the threshold because a few students enroll in cohorts other than the one they were admitted to.

their first year.[18] In some cases these courses were offered in several sections, but the large majority of students in any classroom were from the same cohort. On average, first-year classrooms contained 36 students, and 77 percent of an individual's classmates were from their own program and cohort (Appendix Table A5). We exclude other elective courses because students had discretion on whether and when to take them.

At Univalle, unlike at many top universities in the U.S., it common for students to fail courses—particularly early in their college careers. Appendix Table A6 shows the most common first-year required courses in each Univalle program group and their average pass rates. These include introductory courses related to the major such as Intro to Accounting and Physics I, as well as math courses like Calculus and Geometry. Passing rates range from 60–80 percent for most courses, with math classes typically having lower pass rates. More than half of all engineering students in our sample failed Calculus I, despite the fact that the median enrollee scored at the 94$^{\text{th}}$ percentile on the ICFES exam.

Panel A of Table 4 shows that tracking affected the mean ability of an individual's classmates in their first-year required courses at Univalle. This panel uses the same dependent variables as in Panel B of Table 3, except we compute mean peer characteristics at the *classroom* level rather than the cohort level. Regressions are at the applicant $\times$ class level, with an observation for each applicant's first attempt at each first-year course. We find that admission to a fall cohort led to a 9.7 percentile point increase in the mean ICFES score in an individual's first-year classes (column B). Crossing the tracking threshold caused individuals to fall from roughly the 80$^{\text{th}}$ to the 20$^{\text{th}}$ percentile of the classroom ability distribution as measured by ICFES scores. The last three rows of Panel A show that tracking also affected the gender, age, and SES composition of individual's first-year classmates, although these coefficients are smaller in magnitude than the cohort-level estimates from Table 3.

Panel B of Table 4 shows our first main result: marginal admits to the high-ability cohorts received *lower* first-year grades.[19] In the first row, the dependent variable is the applicant's numerical grade in their first attempt at each course. Crossing the tracking threshold reduced applicants' first-year grades by 0.18 points on average (column B). (See also Panel A of Figure 3.) Colombian grades are on a 0–5 scale with 0.1 point increments (see Appendix Figure A3), and 0.2 points is roughly the difference between a B+ and a B on the U.S. scale. Marginal admission to a high-ability cohort reduced grades in each of our three

[18] We use a data-driven method to define first-year required courses since program requirements changed slightly over time. Our results are similar using other definitions, or including all first-year courses. Appendix Table A5 shows that students took nine first-year required courses on average.

[19] As in Panel A, regressions in Panel B are at the applicant $\times$ class level with one observation for each first-year course. The one exception is the last row; for this, the dependent variable is first-year GPA measured across all attempts at all first-year required courses, and regressions are at the applicant level.

program groups (columns C–E), although the architecture and engineering estimates have large standard errors.

More importantly, we find that marginal admits to high-ability cohorts were also less likely to *pass* first-year courses. In the second row of Panel B of Table 4, the dependent variable is an indicator for receiving a passing grade on the first attempt at each course, which is a value of 3 or above on the Colombian scale. Tracking decreased the course passing rate by roughly six percentage points on average (column B), with large and statistically significant effects in each of architecture, business, and engineering (columns C–E). This effect is a 33 percent increase in the mean course failure rate (18 percent). (See also Panel B of Figure 3.)

Our results on first-year grades and passing rates are robust to different RD specifications. Panel A of Appendix Table A7 shows that we find negative and statistically significant effects of admission to the high-ability cohorts using RD bandwidths that are both 0.5 and 2 times the benchmark CCT bandwidth. We also find similar results using a uniform (as opposed to triangular) kernel. Panels A–B of Appendix Figure A2 shows that our estimates remain negative and significant across a wide range of bandwidths, and our estimates become even more negative at the most narrow bandwidths.

The remaining rows in Panel B of Table 4 show that most students who were induced to fail first-year course by tracking never managed to pass the course. For this we use three dependent variables: 1) an indicator equal to one if the applicant ever retook each course; 2) an indicator for *ever* passing the course; and 3) applicants' GPA across all of their first-year required courses. We find that crossing the tracking threshold led to a 4.5 percentage point increase the likelihood of retaking first-year courses, suggesting that many students who failed on their first attempt tried again to pass the course. Yet the effect of tracking on the likelihood of *ever* passing each course (−4.5pp) is only slightly smaller in magnitude than the effect on passing in the first attempt (−5.8pp). Averaged across all programs and courses, marginal admission to a high-ability cohort reduced applicants' first-year GPA by 0.24 points.

3.2. **Program completion and educational attainment.** We next ask how tracking affected the likelihood that individuals earned a degree from their Univalle program and their overall educational attainment. Students cannot graduate from Univalle without passing courses that are required for their major, and so the results in Table 4 suggest that admission to a high-ability cohort was likely to also reduce the rate of degree completion. Yet this relationship is not mechanical because the students who were induced to fail first-year courses by tracking may have dropped out of the program anyway. Further, some individuals who dropped out may have enrolled in other programs at Univalle or other colleges.

We examine effects on educational attainment in Panel A of Table 5 using four dependent variables: 1) the total number of courses passed at Univalle; 2) the number of semesters of full-time Univalle enrollment; 3) an indicator for completing the Univalle program; and 4) an indicator for earning *any* college degree.[20] These first three variables use our transcript data from Univalle, which allows us to observe individuals' course performance and degree completion through 2017. The last variable combines the Univalle graduation records with our Ministry of Education data that covers nearly all Colombian colleges. Thus this variable is an indicator for completing the Univalle program by 2017 or another college program by 2012. Table 5 has the same structure as Table 4; column (A) show the mean of each dependent variable for applicants just below the tracking threshold, and columns (B)–(E) show RD coefficients for all programs and for our three program groups. Panels C–D of Figure 3 display RD graphs for courses passed and Univalle graduation.

We find that admission to a high-ability cohort reduced applicants' educational attainment as measured by each outcome variable. Column (B) in Table 5 shows that marginal admits to the high-ability cohorts passed 4.2 fewer courses at Univalle on average, and they had 0.7 fewer semesters of full-time enrollment. Across all programs and cohorts, crossing the tracking threshold reduced the likelihood of graduating from the Univalle program by 8.6 percentage points, which is a 15 percent reduction from the graduation rate below the threshold. The point estimate for the likelihood of earning *any* college degree is similar in magnitude, which shows that most individuals who were induced to drop out of Univalle by tracking did not successfully complete a degree from another program. As in Table 4, our point estimates are largest in magnitude in Univalle's architecture program (column C), although these estimates have very large standard errors. We find negative and statistically significant effects in our largest program group, business, and negative but imprecise estimates in engineering (columns D–E).

Our results for educational attainment are marginally significant, and thus should be interpreted with some caution. There is graphical evidence that courses passed and degree completion decrease at the tracking threshold in Panels C–D of Figure 3, but these graphs are noisy because both outcomes are only moderately correlated with admission ranks. This weak correlation arises because many factors affect whether students successfully complete a college degree, including financial resources, personal motivation, and outside employment options. These characteristics may be hard to measure at the time that applicants apply to Univalle. In addition, Univalle may have other objectives that lead it to use exam scores for admissions despite their low predictive power for program completion (e.g., cost, fairness, or screening based on potential employment outcomes).

[20] We define full-time semesters as those in which individuals took four or more courses at Univalle.

Nonetheless, we take our results as evidence that tracking in college programs can reduce educational attainment for marginal admits to the high-ability tracks. Our results on degree completion in Table 5 are even *larger* in magnitude than the effects on first-year passing rates in Table 4.[21] Further our educational attainment results are robust to a range of RD bandwidths and to the choice of kernel (see Panel B in Appendix Table A7 and Panels C–D of Appendix Figure A2).

3.3. **Formal employment and earnings.** We now ask how tracking affected the labor market outcomes of Univalle applicants. Our labor market data cover the universe of workers at firms that were registered with the Ministry of Social Protection during the calendar years 2008–2012. This allows us to observe earnings and days of employment for any Univalle applicant who worked at one of these firms, but the data come with two important caveats. First, we measure labor market outcomes early in individuals' careers; since our sample includes students who applied to Univalle in 2000–2003, we only observe earnings 5–12 year after application, depending on the cohort. As we discuss below, the benefits of a college education may only be beginning to materialize at this point, particularly for individuals who take a while to graduate. Second, we do not observe earnings or employment in the informal sector, which is a large part of the Colombian economy. College educated individuals are less likely to be informally employed than the typical Colombian worker, but the attainment of a college degree may impact the likelihood of formal employment.

Given these caveats, we define our two main labor market outcomes so that they are cleanly measured for all individuals in our sample. Our first dependent variable is an indicator for having formal earnings in *any* year in 2008–2012; in other words, it is simply an indicator for appearing in our earnings data. Second, we follow Clark and Martorell (2014) in measuring *total* formal sector earnings over 2008–2012. For this, we convert an individual's earnings in each year to 2012 U.S. dollars, and then sum earnings across the five years. This measure includes zeroes for individuals who have no formal sector earnings in all years. We define two other outcome variables that are conditional on formal employment: 1) the log of the individual's total days of employment from 2008–2012; and 2) the individual's log mean daily earnings over this period.[22] The former variable provides a measure of labor supply conditional on employment, while the latter variable is our best measure of a skill price for

[21] Appendix Table A12 shows that tracking also affected the composition of students' classmates in their *upper-level* required courses. These magnitudes are smaller than the effects on peer composition in first-year courses (Panel A of Table 4) because there is more variation in the timing at which students take upper-level course. But this shows that marginal admits to the high-ability cohorts continued to take courses with higher-ability peers if they persisted beyond the first year. We do not find significant effects of tracking on grades in upper-level courses, although these effects are not cleanly estimated because tracking affected the set of students that persisted to take these courses.

[22] For log mean daily earnings, we convert annual earnings to 2012 U.S. dollars in each year, divide by the number of employment days in that year, average across any years with earnings, and then take logs.

the individual's labor. We present RD estimates for these labor market outcomes in Panel B of Table 5 and in Panels E–F of Figure 3.

We find that admission to a high-ability Univalle cohort is associated with negative but imprecisely estimated labor market outcomes. Column B of Table 5 shows that RD coefficient for any formal sector earnings is −11.5 percentage points ($p = 0.02$), which is a 14 percent decrease from the formal employment rate for applicants just below the tracking threshold (82.1pp). The RD estimate for total formal earnings is negative at −\$1,916, but it is not statistically significant ($p = 0.35$). The magnitude of the earnings effect is roughly nine percent of the below threshold mean, suggesting that the tracking effect on total earnings is driven primarily by the effect on formal employment. We also find a negative and insignificant effect of crossing the tracking threshold on daily earnings (−0.066 log points).

The negative employment and earnings effects in Panel B of Table 5 are consistent with the negative effects of tracking on educational attainment in Panel A. A college degree is likely to increase access to formal sector jobs, and to improve individuals' job opportunities within the formal sector. Our results do not capture any earnings in the informal sector, but informal jobs pay much lower wages on average.

Nonetheless, we cannot draw any strong conclusions from our main labor market estimates given the imprecision of our RD coefficients. Although the formal employment effect in Table 5 is large and statistically significant, the graphical evidence for this effect is not compelling because of the noisy relationship between formal employment and admission scores (Panel E of Figure 3). Further, the formal employment and total earnings estimates are more sensitive to the choice of RD bandwidth than the estimates for academic outcomes. The employment and earnings RD estimates are negative across all bandwidths, but they vary considerably in magnitude and statistical significance (see Panel C in Appendix Table A7 and Panels E–F of Appendix Figure A2).

3.4. **Timing of tracking effects.** An important fact in interpreting our academic and labor market results is that many students take a long time to graduate from Univalle. Most of Univalle programs in our sample have on-time durations of five years, but the majority of students who earn a degree do not finish on time.[23] Many students have to retake failed classes in order to complete the program, and most students work while they are in college.

Figure 4 shows how our RD estimates change when we measure graduation and labor market outcomes at different lengths of time since students applied to Univalle. The x-axis in each panel represents the number of years since Univalle application, e.g., zero represents the year 2000 for applicants who applied in Fall 2000. The red dashed line plots the mean

[23] On-time completion is five years in all programs except Foreign Trade (4.5 years) and nighttime business/accounting (six years).

of each dependent variable for students just below the tracking threshold (pooling across all cohorts and programs). The green solid line plots this mean effect plus the RD estimate, which is the π coefficient from a separate estimation of equation (1) for outcomes defined only in that year. Vertical dashed lines are 95 percent confidence intervals for the RD coefficient. In Panel A, the outcome variable is an indicator for completing the Univalle program by that year. The other outcome variables in Figure 4 are are any formal earnings in that year (Panel B), total formal earnings in that year (Panel C), and log daily earnings conditional on employment in that year (Panel D).[24] Appendix Table A9 presents RD estimates for each of our outcome variables in Table 5 measured at different durations since Univalle application.

Panel A of Figure 4 shows that applicants just below the tracking threshold graduated from Univalle at higher rates than above-threshold applicants, but this effect took a long time to materialize. Students began graduating 4–5 years after application, and the RD estimate for admission to the high-ability cohorts is *positive* in these years. This reflects the six-month enrollment delay for applicants below the threshold. Below-threshold graduation rates caught up to and surpassed those for above-threshold students over the next several years. Yet even ten years later, the effect of tracking on program completion is less than two-thirds of its final magnitude (-5.4pp vs. -8.7pp). Thus among the set of students who earned a Univalle degree *only because* they were admitted to a lower-ability cohort, many took a long time to graduate.

Panel B of Figure 4 shows how tracking affected formal employment rates over time. We note two features of Panel B in particular. First, more than half of students in our sample are formally employed in *all* years, even though many had not yet finished college coursework. Second, the negative effect of crossing the tracking threshold on formal employment arises *early* in students' careers. The gap in formal employment rates between below- and above-threshold applicants is large (but noisily estimated) measured 5–8 years after application, and closes to near zero by 12 years after application. In Colombia, students often work during college, and many schools hold career fairs to help students obtain internships. Thus students who were induced to drop out of Univalle by tracking may have had less access to jobs at formal firms early in their careers.[25] Unfortunately, the timing of our earnings data does not allow us to investigate employment during the initial years of Univalle enrollment. But the results in Panel B of Figure 4 suggests that our main results in Table 5 do not reflect a long-run effect of tracking on the likelihood of formal employment.

[24] In Panels B–D of Figure 4, the composition of cohorts and programs changes along the x-axis because we only observe labor market outcomes over a five-year window for each applicant.

[25] Another possible explanation for the employment patterns in Panel B of Figure 4 is that students just below the tracking threshold may have begun jobs during the six-month period before they enrolled in Univalle, and then kept these jobs during their college careers.

By contrast, Panels C–D of Figure 4 show that the negative earnings effects of tracking arise primarily *later* in individuals' careers. For both total formal earnings (Panel C) and log daily earnings (Panel D), the gap between below- and above-threshold applicants is close to zero measured 5–8 years after application, and then widens up to the end of our sample window. For example, Appendix Table A9 shows that the RD estimate on log daily earnings measured 11–12 years after application (-0.181 log points) is much larger than the daily earnings effect measured over the entire 2008–2012 period (-0.066 log points), and it is statistically significant at $p < 0.05$.

The time profile of the earnings results in Panels C–D of Figure 4 matches the time profile of the graduation results in Panel A. If students who benefitted academically from enrolling in lower-ability cohorts took a long time to graduate, one might also expect that the earnings benefits would only appear in the longer-run. Yet these longer-run outcomes are measured in only a subset of our sample, and our data do not allow us to investigate whether these earnings effect persisted beyond 12 years. Thus we take the results in Figure 4 as only suggestive evidence that tracking impact students' careers in the labor market.

4. Mechanisms

Our results in Section 3 showed that students who were marginally admitted to high-ability cohorts at Univalle had lower course grades and graduation rates. This section discusses the mechanisms that could explain these results. We present suggestive evidence that student ability and effort play a role in our findings by showing heterogeneity with respect to prior test scores and gender.

4.1. **Revisiting potential mechanisms.** We begin by updating our discussion of potential mechanisms in Section 1.5 based on the results from Section 3. From the mechanisms outlined above, the following are consistent with our findings:

(1) If peer effects on learning are important in our setting, our results would suggest that students near the tracking margin learned *less* in higher-ability classrooms. This is consistent with effort responses to class rank (Murphy and Weinhardt, 2020; Elsner et al., 2021), as, for example, in the invidious comparison model of peer effects in Hoxby and Weingarth (2005).
(2) Professors may have adjusted their teaching levels in lower-ability classrooms in a way that benefitted the highest-ranking students in the class. In their experiment in Kenyan primary schools, Duflo et al. (2011) present evidence that teachers' instruction level is closer to the top students in lower-ability classes than to the bottom

students in higher-ability classes.[26] This can arise if professors target their teaching to students with the highest ability.

(3) Conditional on student learning and performance, professors may have curved grades upward in the lower-ability spring cohorts.[27]

(4) Students in the spring cohort may have been better prepared for college as a result of the six-month enrollment delay. For example, students may have studied, rested, or earned income to support their education in the interim.

Our data do not allow us to conclusively distinguish between these different mechanisms; for instance, we do not have information on professor identity or on individuals' employment histories prior to college. But we present suggestive evidence on several mechanisms by examining heterogeneity in tracking effects in the next subsection.

4.2. **Heterogeneity.** We first examine heterogeneity in the effects of admission to a high-ability cohort with respect to the student's *own* ability. There are several reasons why the impacts of tracking could vary with individual ability or achievement. Card and Giuliano (2016) find that the benefits of placement into high-achieving elementary school classes are concentrated among Black and hispanic students. These minority students may have lower achievement than white students with similar test scores owing to other disparities in the education system. The authors' results suggest that minority students benefit from higher expectations and reduced barriers to achievement in high-ability classes. On the other hand, lower-ability students may have more negative impacts of placement into advanced classes. It may be particularly harmful to be at the bottom of the classroom ability distribution, where the risk of failing is highest. For example, if lower-ability students receive failing grades on midterm exams, they may become discouraged and reduce effort for the rest of the semester.

To test for heterogeneity in tracking effects by ability, we follow Abdulkadiroğlu et al. (2014)'s insight that admission scores are noisy measures of ability, and thus there is variation in ability even conditional on admission scores. In our data, we take advantage of the fact that we observe scores on *each* subject component of the ICFES exam for most applicants. In particular, our data includes scores on an elective component of the ICFES exam that

[26] Duflo et al. (2011) also find evidence of a positive effect of peer ability, and they argue that these two factors offset to produce no discontinuity at the tracking threshold.

[27] To examine the role of curving, we compare grade distributions in first-year classrooms that had mostly fall or mostly spring cohort students. Appendix Figure A4 shows that there is significant variation in both the mean and standard deviation of grades across these two cohort groups, which is inconsistent with the notion that (potentially different) professors maintained fixed curves. Appendix Figure A5 shows that the overall grade distributions across all first-year classes are quite similar between the fall and spring cohorts, but this does not necessarily imply curving. Nonetheless, individual professors may have adjusted grades in response to tracking, and this is hard to disentangle from instructional responses or differences in student performance.

was not used in the computation of admission scores by any Univalle program in our sample. For this elective component, students could choose between tests on environmental science, violence and society, and mass media and culture. Although these are non-standard topics, the correlation between elective scores and the mean score on core ICFES subjects is roughly 0.4. We compute the median score on this elective component across all students in our sample, and estimate equation (1) separately for applicants above and below the median.

Columns (A)–(C) of Table 6 presents results for students with above- and below-median ability on the ICFES elective component. We use the same dependent variables related to first-year grades (Panel A), educational attainment (Panel B), and formal employment and earnings (Panel C) as in Tables 4–5. Column (A) shows RD estimates for above-median students (higher ability), and column (B) shows results for below-median students (lower-ability). Column (C) reports the p value from a test of equality of the above- and below-median coefficients.

We find that the negative impacts of placement into the high-achieving cohorts are concentrated among lower-ability students. Lower-ability students were roughly 10 percentage points more likely to fail their first-year courses on the initial attempt when they crossed the tracking threshold, and they were 6.6 percentage points more likely to *never* pass these courses. By contrast, we find no statistically significant impacts of tracking on first-year grades for students with above-median elective scores. This finding is corroborated by quantile RD regressions in Appendix Table A11, which show that tracking effects on first-year GPA are more negative at the lowest quantiles. The negative impacts of tracking on educational attainment are also driven by lower-ability students (Panel B), and we find evidence of declines in these students' formal employment and earnings (Panel C). In many cases we cannot statistically distinguish between the above- and below-median RD coefficients (column C). Nonetheless, these results show that the negative impacts of tracking were concentrated among students who were near the bottom of the ability distribution in the high-achieving cohorts.

Columns (D)–(F) of Table 6 show that the negative effects of admission to a high-ability cohort were also more pronounced among male students. These columns are similar to columns (A)–(C), but we estimate RD regressions separately for female (column D) and male (column E) applicants. The effect on initial course passing rates is three percentage points more negative for male students than for female students (-8.0pp vs. -5.2pp), while the effects on retaking first-year courses are similar (-4.9pp vs. -4.3pp). Thus male students were more likely to fail first year courses as a result of tracking, and less likely to retake courses after failing. As a result, the tracking effect on *ever* passing first-year courses is almost entirely driven by men. We also find that tracking had more negative impacts on the educational attainment of male students (Panel B). One possible explanation for these results is that effort helps to offset the negative effects of more able classmates, and men have relatively higher effort costs of schooling (Goldin et al., 2006). It is important to note, however, that none of our male and female RD estimates are statistically different at conventional levels (column F).

5. Conclusion

In most K–12 school systems, students are grouped by ability or achievement for the purpose of targeting instruction in the classroom. This "tracking" takes different forms around the world. In Colombia, students attend either "academic" or "technical" high schools that prepare them for different career paths in college and beyond. Many European countries use a similar form of across-school tracking, but there is wide variation in the age at which students are tracked. In the United States, tracking often occurs *within* schools by grouping students into classrooms with different levels of instruction. There is a large literature that asks how tracking affects students' academic achievement, and the vast majority of these papers focus on tracking at the K–12 level (Betts, 2011).

Yet tracking is also pervasive in higher education. Both of our institutions practice a form of within-school and -major tracking. At Cornell University in the U.S., potential physics majors can opt for an advanced introductory class that is pitched to students who are "comfortable with a deeper, somewhat more abstract approach." At Universidad de Los Andes in Colombia, students who are "better prepared" can apply to take a honors-level differential calculus class instead of the standard course.[28] There is limited evidence on whether college students benefit from taking such courses with higher-achieving peers.

Our paper aimed to partly fill this gap by examining the outcomes of college students on the margin of placement into high- and lower-achieving classes. We exploited a unique tracking policy at a selective flagship university in Colombia called "Univalle," where students are high-achieving but failing courses is common. In this context, we found that marginal admits to high-ability classes were more likely to fail courses than similar students who took classes with lower-achieving peers. Placement in high-ability classes also reduced the likelihood that these students earned a college degree, suggesting that it had longer-run consequences for their careers.

Our findings show that college students may benefit not only from absolute levels of academic preparation, but also when they are better prepared *relative* to their classmates. Our results suggest that relative preparation is particularly important in settings where failing courses and dropping out is a real possibility. These findings run counter to the

[28] See the course catalogues at: https://classes.cornell.edu/browse/roster/SP21/class/PHYS/1116; http://uniandes.smartcatalogiq.com//es-ES/2018/Catalogo/Cursos/MATE/1000/MATE-1204.

common criticism that tracking can lead to stigma for students who are placed into lower-ability tracks (Slavin, 1987).[29] While these stigma effects may exist in some settings, students in competitive education environments may also be discouraged when they receive low grades in initial courses. Concerns about relative preparation may partly explain why disadvantaged students are often less likely to apply to selective colleges (Hoxby and Avery, 2013; Dillon and Smith, 2017).

A broader question is whether tracking students into college courses can help boost *aggregate* completion rates. In Colombia, roughly half of students who begin a college program do not earn a degree, and reducing college dropout rates is a policy priority (Carranza and Ferreyra, 2019). In our data from Univalle, degree completion rates were similar in the fall and spring cohorts despite large differences in incoming test scores (see Panel D of Figure 3). Our analysis focused on students on the margin of high- and lower-ability classes, and thus do not directly speak to the aggregate effects of tracking. We hope future research will shed more light on the efficiency and distributional effects of tracking in higher education.

[29] However, empirical work does not find much evidence of discouragement effects in the context of community college placement exams (Martorell et al., 2015; Scott-Clayton and Rodriguez, 2015).

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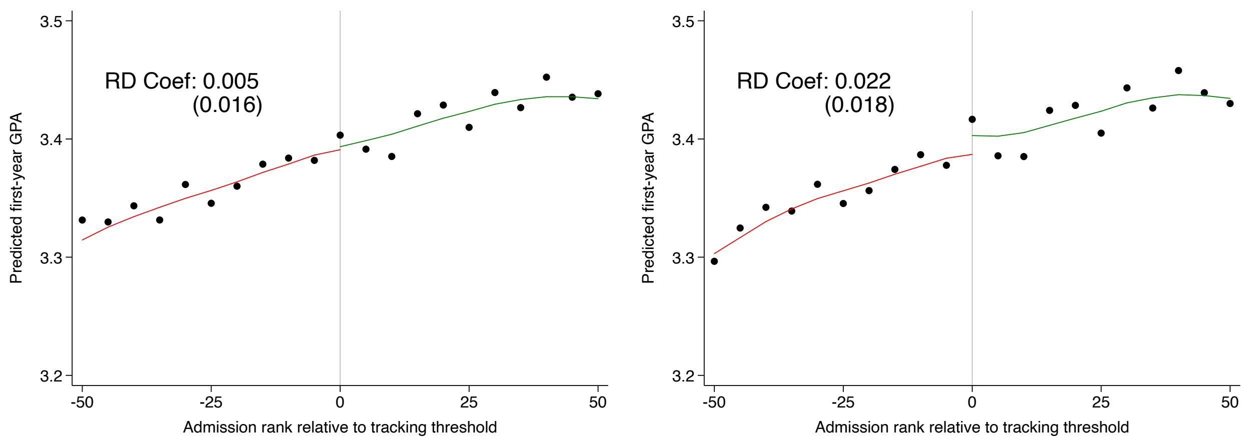
Figures and tables

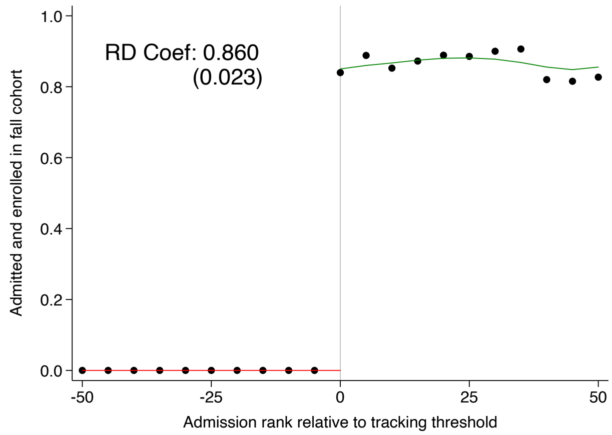


Panel A. All applicants

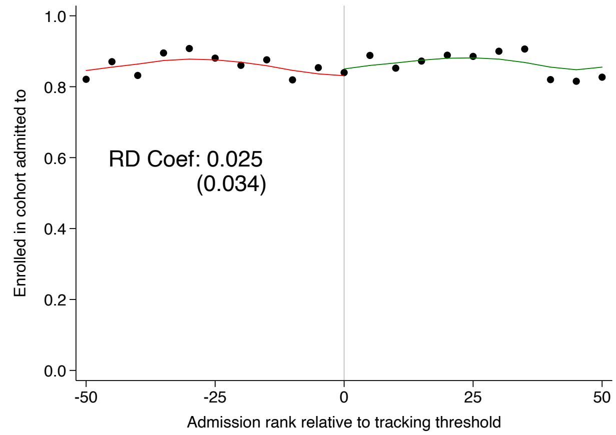
Panel B. Univalle enrollees

Figure 1. RD balance tests using a GPA index based on individual characteristics

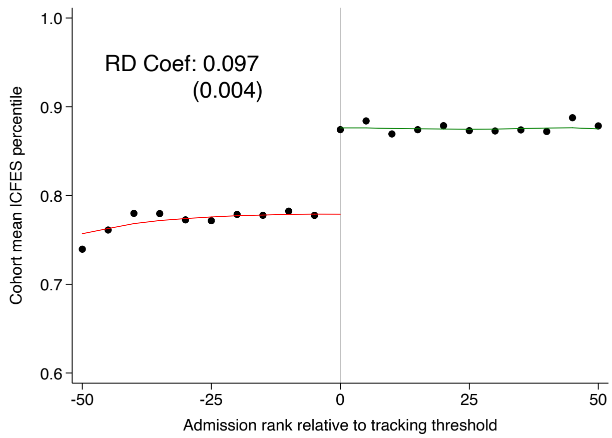
Notes: This figure presents RD graphs that test for balance at the tracking threshold with respect to observable characteristics. The x-axis is a student's admission rank normalized so that zero is the first rank above the tracking threshold. The y-axis is the predicted value from a regression of an applicant's first-year GPA on individual characteristics (age; years since high school graduation; ICFES percentile; and indicators for gender, a secondary educated mother, a college educated mother, a secondary educated father, a college educated father, and family income above 2x minimum wage). The sample for Panel A includes all applicants who were admitted to Univalle programs with tracking admissions, as shown in column (A) of Table 2. Panel B includes only the subset of these students who enrolled in Univalle. Dots are means in five-rank bins. Lines are local linear regressions estimated separately above and below the tracking threshold; these regressions use a triangular kernel and the same Calonico et al. (2019) bandwidth as in our benchmark RD specification for this dependent variable. Each panel displays the RD coefficient and standard error from this benchmark regression (see Appendix Tables A2–A3).



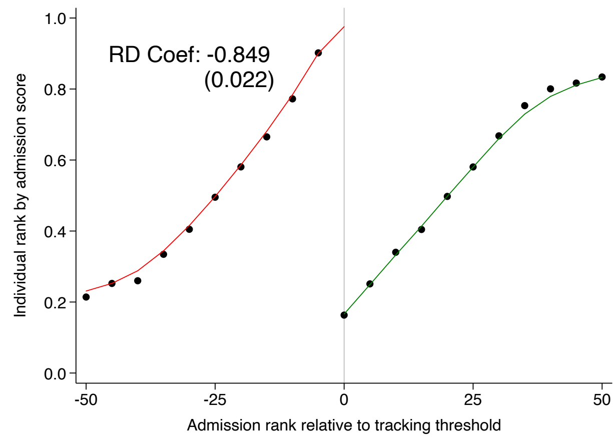
Panel A. Admitted and enrolled in fall cohort



Panel B. Enrolled in cohort admitted to



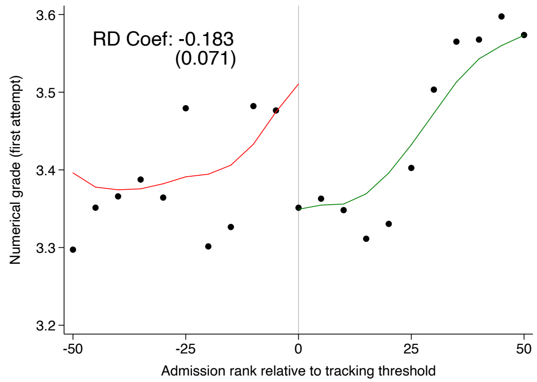
Panel C. Cohort mean ICFES percentile



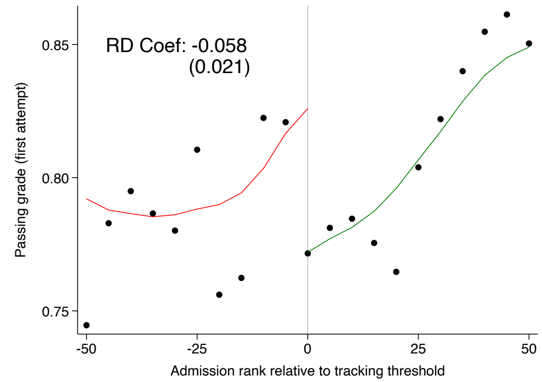
Panel D. Individual rank by admission score

FIGURE 2. Univalle program enrollment and cohort ability

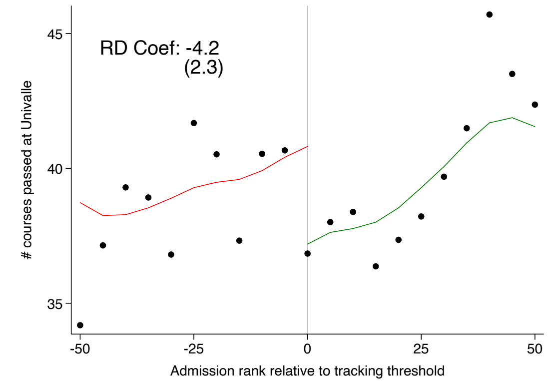
Notes: This figure contains RD graphs (as in Figure 1) using four different dependent variables: A) an indicator for being admitted to and enrolling in a fall Univalle cohort; B) an indicator for enrolling in the Univalle cohort that the applicant was admitted to; C) the mean ICFES percentile in the applicant's college cohort; and D) the applicant's rank in their Univalle cohort based on their admission score. The sample for Panels A–C includes all applicants who were admitted to Univalle programs with tracking admissions, as shown in column (A) of Table 2. Panel D includes only the subset of these students who enrolled in Univalle. Each panel displays the RD coefficient and standard error from the benchmark specification for that dependent variable (see Table 3).



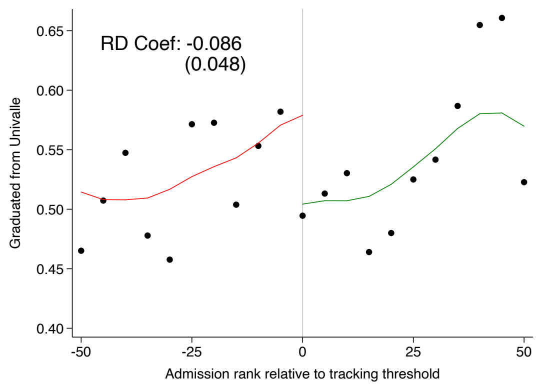
Panel A. Numerical grade in first-year courses



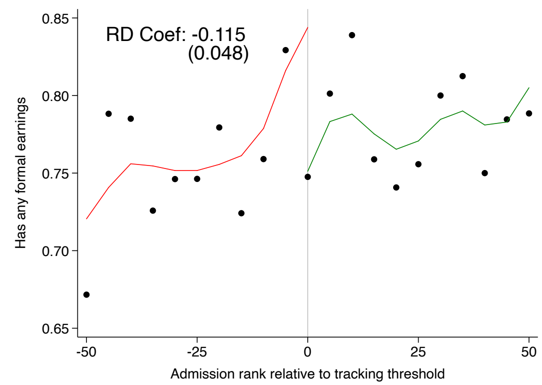
Panel B. Passing grade in first year courses



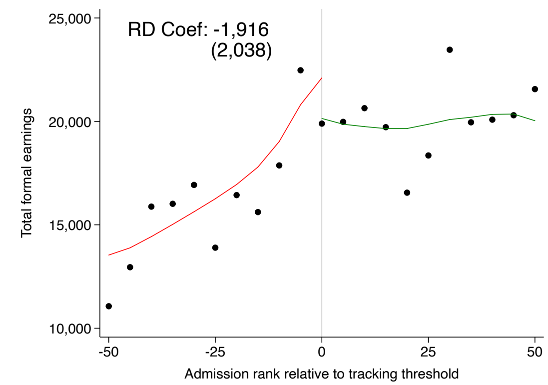
Panel C. Number of courses passed at Univalle



Panel D. Graduated from Univalle



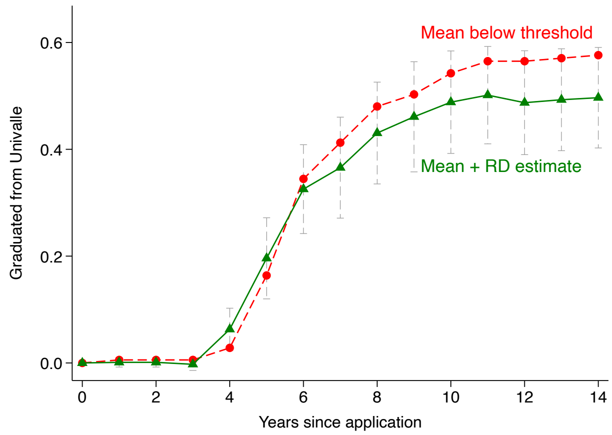
Panel E. Has any formal earnings in 2008–12



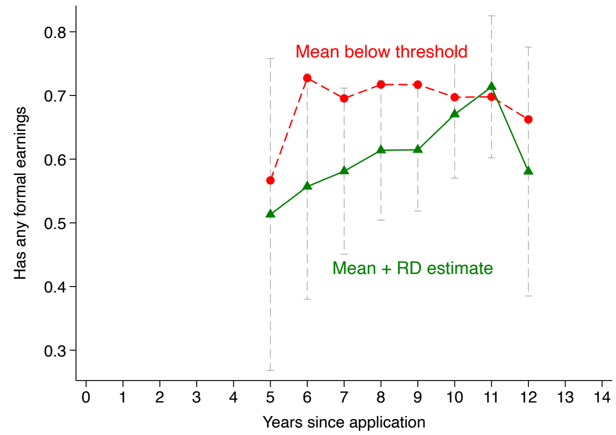
Panel F. Total formal earnings in 2008–12

FIGURE 3. Academic and labor market outcomes

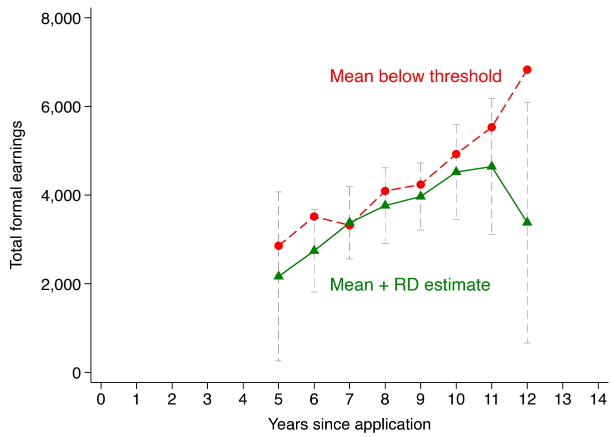
Notes: This figure contains RD graphs (as in Figure 1) using six different dependent variables: A) the numerical grade in the applicant's first attempt at each first-year required course; B) an indicator for a passing grade in the applicant's first attempt at each first-year required course; C) the total number of courses that the applicant passed at Univalle; D) an indicator for graduating from the Univalle program; E) an indicator for appearing in our earnings data in any year in 2008–2012; and F) the applicant's total formal sector earnings in 2008–2012 (converted to 2012 U.S. dollars). Panels A–B use applicant × course level data using only applicants who enrolled in Univalle. Panels C–F use applicant-level data; Panels C–D include only Univalle enrollees, while Panels E–F include all Univalle admits. Each panel displays the RD coefficient and standard error from the benchmark specification for that dependent variable (see Tables 4–5).



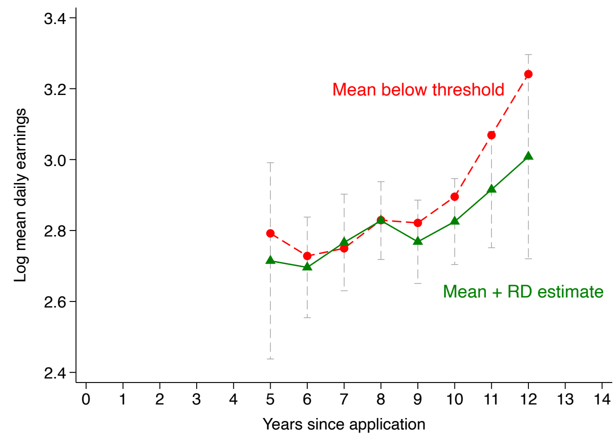
Panel A. Graduated from Univalle



Panel B. Has any formal earnings



Panel C. Total formal earnings



Panel D. Log mean daily earnings

FIGURE 4. RD coefficients by years since Univalle application

Notes: This figure displays RD estimates for Univalle graduation and labor market outcomes by years since application. The x-axis in each panel represents the number of years since Univalle application. The red dashed line plots the mean of each dependent variable for applicants with admission ranks between -5 and -1. The green solid line plots this mean effect plus the RD estimate, which is the π coefficient from a separate estimation of equation (1) for outcomes defined only in that year. Vertical dashed lines are 95 percent confidence intervals for the RD coefficient using standard errors clustered at the individual level. The dependent variables are: A) an indicator for graduating from the Univalle program by that year; B) an indicator for appearing in our earnings data in that year; C) the applicant's formal sector earnings in that year (converted to 2012 U.S. dollars); and D) the applicant's log mean daily earnings (converted to 2012 U.S. dollars). The sample for Panel A includes enrollees in Univalle programs with tracking admissions. The sample for Panels B–C includes all Univalle admits. The sample for Panel D includes the subset of these admits who were formally employed in that year.

Table 1. Tracking admissions example — Fall 2003 applicants to architecture

(A) Admit rank	(B) Admit score	(C) Admission decision
1	404.16	
...	...	Admitted to Fall 2003 cohort
60	315.75	
61	315.05	
...	...	Admitted to Spring 2004 cohort
132	259.14	
133	258.94	
...	...	Rejected
426	14.01	

Notes: This table provides an example of Univalle's tracking admissions for applicants to the Fall 2003 architecture programs. Applicants were first ranked based on their admission score, which is a weighted average of an applicant's ranks on each ICFES subject score (where the lowest scoring applicant has rank one). Column (A) shows the applicant's rank, and column (B) shows their admission score. Column (C) shows their admission decision: applicants in the first 60 positions were admitted to the Fall 2003 cohort; applicants in the next 62 positions were admitted to the Spring 2004 cohort; and the remaining applicants were rejected.

Table 2. Summary statistics for programs with tracking admissions

	(A) All programs	(B) Architecture	(C) Business	(D) Engineering
Panel A. Application and admission statistics				
# programs	11	1	5	5
# application pools	46	7	29	10
# applicants	6,544	1,440	4,028	1,076
# admitted students	3,059	446	1,992	621
# admitted students (fall cohort)	1,534	217	984	333
# admitted students (spring cohort)	1,525	229	1,008	288
Mean cohort size	54	50	54	56
Panel B. Mean characteristics of Univalle enrollees				
ICFES percentile	0.79	0.80	0.77	0.87
Female	0.48	0.33	0.60	0.18
Age at application	19.33	18.64	19.70	18.56
College educated mother	0.28	0.46	0.20	0.40
Panel C. Mean outcomes of Univalle enrollees				
Mean first-year GPA	3.39	3.02	3.55	3.10
Passed all first-year courses on first try	0.33	0.30	0.38	0.21
Graduated from Univalle	0.53	0.53	0.57	0.40
Has any formal earnings in 2008–2012	0.81	0.76	0.83	0.80
Total formal earnings in 2008–2012	18,692	11,193	19,063	22,852
Mean daily earnings over 2008–2012	17.98	15.84	17.22	22.13

Notes: This table presents summary statistics for applicants to Univalle programs and cohorts with tracking admissions, as described in Section 1.4 and Appendix B. Column (A) displays statistics for all programs. Columns (B)–(D) display statistics for three program groups: architecture, business, and engineering. These program groups include the following programs, with the years in which each program used tracking admissions in parentheses:

- Architecture: Architecture (2000–2003);
- Business: Accounting daytime (2000–2003); Accounting nighttime (2000–2003); Administration daytime (2000–2003); Administration nighttime (2000–2003); Foreign Trade (2003);
- Engineering: Chemical Engineering (2000); Electrical Engineering (2000); Electronic Engineering (2000); Materials Engineering (2001); Mechanical Engineering (2001).

Panel A displays statistics on the number of applicants, admitted students, and mean cohort size. Our main sample includes all students who were admitted to these programs, as shown in the four row of Panel A. Panel B displays mean characteristics of students who enrolled in Univalle. Panel C shows mean outcomes for Univalle enrollees. Total formal earnings are converted to 2012 U.S. dollars and include zeroes for individuals with no earnings (see Section 3.3). Mean daily earnings are also in 2012 U.S. dollars, but exclude zeroes.

Table 3. Tracking effects on Univalle enrollment and cohort characteristics

	(A)	(B)	(C)	(D)	(E)
	Mean below threshold	RD coefficients			
Dependent variable	All programs	All programs	Architecture	Business	Engineering
Panel A. Enrolled in Univalle program					
Admitted and enrolled in fall cohort	0.000	0.860***	0.763***	0.857***	0.906***
		(0.023)	(0.088)	(0.027)	(0.051)
Admitted and enrolled in spring cohort	0.854	−0.832***	−0.821***	−0.859***	−0.817***
		(0.024)	(0.085)	(0.028)	(0.074)
Enrolled in cohort admitted to	0.854	0.025	−0.002	−0.002	0.094
		(0.034)	(0.112)	(0.038)	(0.087)
Enrolled in any cohort	0.883	0.029	0.016	0.007	0.054
		(0.030)	(0.100)	(0.036)	(0.083)
N (Enrolled in cohort admitted to)	205	3,059	446	1,992	621
Panel B. Cohort characteristics					
Mean ICFES percentile	0.778	0.097***	0.054***	0.111***	0.080***
		(0.004)	(0.014)	(0.006)	(0.012)
Individual rank by admission score	0.902	−0.849***	−0.779***	−0.838***	−0.937***
		(0.022)	(0.104)	(0.029)	(0.041)
Proportion female	0.484	−0.038***	0.044**	−0.057***	−0.016
		(0.004)	(0.021)	(0.005)	(0.012)
Mean age at application	19.116	0.254***	0.232	0.237***	0.382
		(0.069)	(0.219)	(0.041)	(0.233)
Proportion college educated mother	0.267	0.021***	0.007	0.009*	0.060**
		(0.006)	(0.032)	(0.005)	(0.024)
N (Individual rank)	177	2,703	379	1,799	525

Notes: This table displays RD estimates for the effects of admission to a higher-ability cohort on Univalle enrollment and cohort characteristics.

In Panel A, the dependent variables measure enrollment in the Univalle program that the applicant applied to. In the first three rows, the dependent variables are indicators for enrolling in the cohort that the applicant was admitted to (fall only, spring only, and both jointly). The last row measures enrollment in any Univalle cohort of that program, regardless of which cohort the applicant was admitted to. The sample includes all Univalle admits.

In Panel B, the dependent variables are mean characteristics of the applicant's college cohort (defined by their school, program, and semester of enrollment). The sample includes all Univalle admits who enrolled in a college in the Ministry of Education records. For individual rank by admission score, the sample includes only Univalle enrollees.

Column (A) displays means of each dependent variable for applicants with admission ranks between −5 and −1. Column (B) displays estimates of the RD coefficient π from equation (1) using each dependent variable. Columns (C)–(E) displays π coefficients from separate estimations for the architecture, business, and engineering program groups. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Tracking effects on classmate characteristics and grades in first-year courses

	(A)	(B)	(C)	(D)	(E)
	Mean below threshold	RD coefficients			
Dependent variable	All programs	All programs	Architecture	Business	Engineering
Panel A. First-year classmate characteristics					
Mean ICFES percentile	0.748	0.097^{***}	0.042^{**}	0.107^{***}	0.092^{***}
		(0.004)	(0.017)	(0.005)	(0.010)
Individual rank by admission score	0.805	-0.657^{***}	-0.345^{***}	-0.692^{***}	-0.679^{***}
		(0.019)	(0.090)	(0.022)	(0.037)
Proportion female	0.470	-0.015^{**}	0.050	-0.029^{***}	-0.005
		(0.007)	(0.034)	(0.007)	(0.013)
Mean age at application	19.252	0.147^{***}	0.347^{**}	0.181^{***}	-0.150
		(0.053)	(0.175)	(0.062)	(0.178)
Proportion college educated mother	0.271	0.029^{***}	0.014	0.014^{**}	0.077^{***}
		(0.007)	(0.030)	(0.006)	(0.017)
N (Individual rank)	1,490	23,841	3,503	16,054	4,284
Panel B. First-year grades					
Numerical grade (first attempt)	3.476	-0.183^{***}	-0.685^{***}	-0.193^{**}	-0.162
		(0.071)	(0.257)	(0.083)	(0.187)
Passing grade (first attempt)	0.821	-0.058^{***}	-0.239^{**}	-0.052^{**}	-0.129^{*}
		(0.021)	(0.105)	(0.022)	(0.067)
Retook course	0.074	0.045^{***}	0.132^{**}	0.036^{***}	0.136^{***}
		(0.012)	(0.054)	(0.012)	(0.047)
Ever passed course	0.875	-0.045^{**}	-0.183^{*}	-0.032	-0.089
		(0.022)	(0.109)	(0.022)	(0.066)
First-year GPA	3.358	-0.238^{**}	-0.794^{**}	-0.292^{**}	0.012
		(0.096)	(0.371)	(0.115)	(0.216)
N (Numerical grade)	1,490	23,841	3,503	16,054	4,284

Notes: This table displays RD estimates for the effects of admission to a higher-ability cohort on classmate characteristics and grades in first-year required courses. We define first-year required courses as those were taken by at least 75 percent of a cohort's graduates, and in which the modal graduate took the course in their first year.

In Panel A, the dependent variables are mean characteristics of the applicant's classmates in their first-year courses. In Panel B, the dependent variables measure the applicant's academic performance in those courses. In both panels, regressions are at the individual $\times$ class level with an observation for each individual's first attempt at each first-year course. The one exception is for the dependent variable "first-year GPA," for which regressions are at the individual level. The sample for all regressions includes only Univalle enrollees.

Column (A) displays means of each dependent variable for applicants with admission ranks between -5 and -1. Column (B) displays estimates of the RD coefficient π from equation (1) using each dependent variable. Columns (C)–(E) displays π coefficients from separate estimations for the architecture, business, and engineering program groups. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5. Tracking effects on educational attainment and labor market outcomes

	(A)	(B)	(C)	(D)	(E)
	Mean below threshold	RD coefficients			
Dependent variable	All programs	All programs	Archi-tecture	Business	Eng-ineering
Panel A. Educational attainment					
# courses passed at Univalle	40.667	-4.238^{*}	-13.422	-4.972^{*}	-5.967
		(2.288)	(9.045)	(2.594)	(6.309)
# full-time semesters at Univalle	7.554	-0.725^{*}	-1.881	-0.880^{**}	-0.894
		(0.387)	(1.526)	(0.438)	(1.081)
Graduated from Univalle	0.582	-0.086^{*}	-0.211	-0.140^{**}	-0.030
		(0.048)	(0.173)	(0.062)	(0.118)
Any college degree	0.634	-0.087^{*}	-0.178	-0.109^{*}	-0.031
		(0.048)	(0.159)	(0.061)	(0.112)
N (Any college degree)	205	3,059	446	1,992	621
Panel B. Employment and earnings over 2008–2012					
Has any formal earnings	0.829	-0.115^{**}	-0.153	-0.108^{*}	-0.062
		(0.048)	(0.146)	(0.056)	(0.084)
Total formal earnings	22,470	−1,916	−7,959	−2,492	−3,591
		(2,038)	(5,098)	(2,490)	(6,285)
Log # formal employment days	6.771	0.094	-0.511^{*}	0.120	0.249
		(0.100)	(0.290)	(0.130)	(0.236)
Log mean daily earnings	2.883	-0.066	0.020	-0.075	-0.067
		(0.051)	(0.161)	(0.060)	(0.140)
N (Has any formal earnings)	205	3,059	446	1,992	621

Notes: This table displays RD estimates for the effects of admission to a higher-ability cohort on educational attainment and labor market outcomes.

In Panel A, the dependent variables in the first three rows measure: number of passed courses, number of full-time semesters of enrollment (i.e., semesters with four or more courses), and program completion at Univalle. These regressions include only Univalle enrollees, and outcomes are measured through 2017. In the last row of Panel A, the sample includes all Univalle admits, and the dependent variable is an indicator for graduating from the Univalle program by 2017 or from another college in the Ministry of Education records by 2012.

In Panel B, the dependent variables are labor market outcomes measured from 2008–2012. The sample for the first two rows includes all Univalle admits, while the sample for the last two rows includes only individuals who appear in our earnings data. Total formal earnings are converted to 2012 U.S. dollars and include zeroes for individuals with no earnings (see Section 3.3).

Column (A) displays means of each dependent variable for applicants with admission ranks between -5 and -1. Column (B) displays estimates of the RD coefficient π from equation (1) using each dependent variable. Columns (C)–(E) displays π coefficients from separate estimations for the architecture, business, and engineering program groups. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6. Heterogeneity in tracking effects by ability and gender

	(A)	(B)	(C)	(D)	(E)	(F)
	RD coefficients by ability			RD coefficients by gender		
Dependent variable	Above median	Below median	p value diff	Women	Men	p value diff
Panel A. First-year grades						
Numerical grade (first attempt)	−0.002	−0.240**	0.133	−0.138	−0.191**	0.733
	(0.106)	(0.097)		(0.120)	(0.083)	
Passing grade (first attempt)	−0.006	−0.104***	0.063	−0.052	−0.080***	0.552
	(0.036)	(0.031)		(0.033)	(0.029)	
Retook course	0.029	0.057***	0.300	0.043***	0.049***	0.775
	(0.018)	(0.019)		(0.016)	(0.016)	
Ever passed course	0.020	−0.066**	0.099	−0.013	−0.078**	0.181
	(0.034)	(0.032)		(0.031)	(0.031)	
N (Numerical grade)	11,152	10,833	21,985	11,877	11,964	23,841
Panel B. Educational attainment						
# courses passed at Univalle	−0.185	−5.704	0.314	−2.639	−5.686*	0.526
	(3.479)	(3.569)		(3.144)	(3.159)	
Graduated from Univalle	−0.002	−0.064	0.593	−0.070	−0.113*	0.674
	(0.077)	(0.074)		(0.068)	(0.066)	
Any college degree	0.056	−0.129*	0.055	−0.041	−0.124**	0.377
	(0.061)	(0.066)		(0.067)	(0.058)	
N (Any college degree)	1,433	1,368	2,801	1,459	1,600	3,059
Panel C. Employment and earnings over 2008–2012						
Has any formal earnings	0.003	−0.154***	0.081	−0.024	−0.107**	0.345
	(0.060)	(0.055)		(0.065)	(0.050)	
Total formal earnings	326	−5,022**	0.183	−4,127	−2,835	0.777
	(2,952)	(2,540)		(3,020)	(3,086)	
N (Has any formal earnings)	1,433	1,368	2,801	1,459	1,600	3,059

Notes: This table displays heterogeneity in the effects of admission to a higher-ability cohort by ability and gender.

Columns (A)–(B) estimate RD regressions separately for students with above- and below-median ability. Ability is defined by the applicant's score on the ICFES elective component, which is not used in the computation of Univalle's admission scores in any program. We split the sample based on the median elective component score across all Univalle admits. Column (C) reports the p value from a test of equality of the RD coefficients in columns (A)–(B).

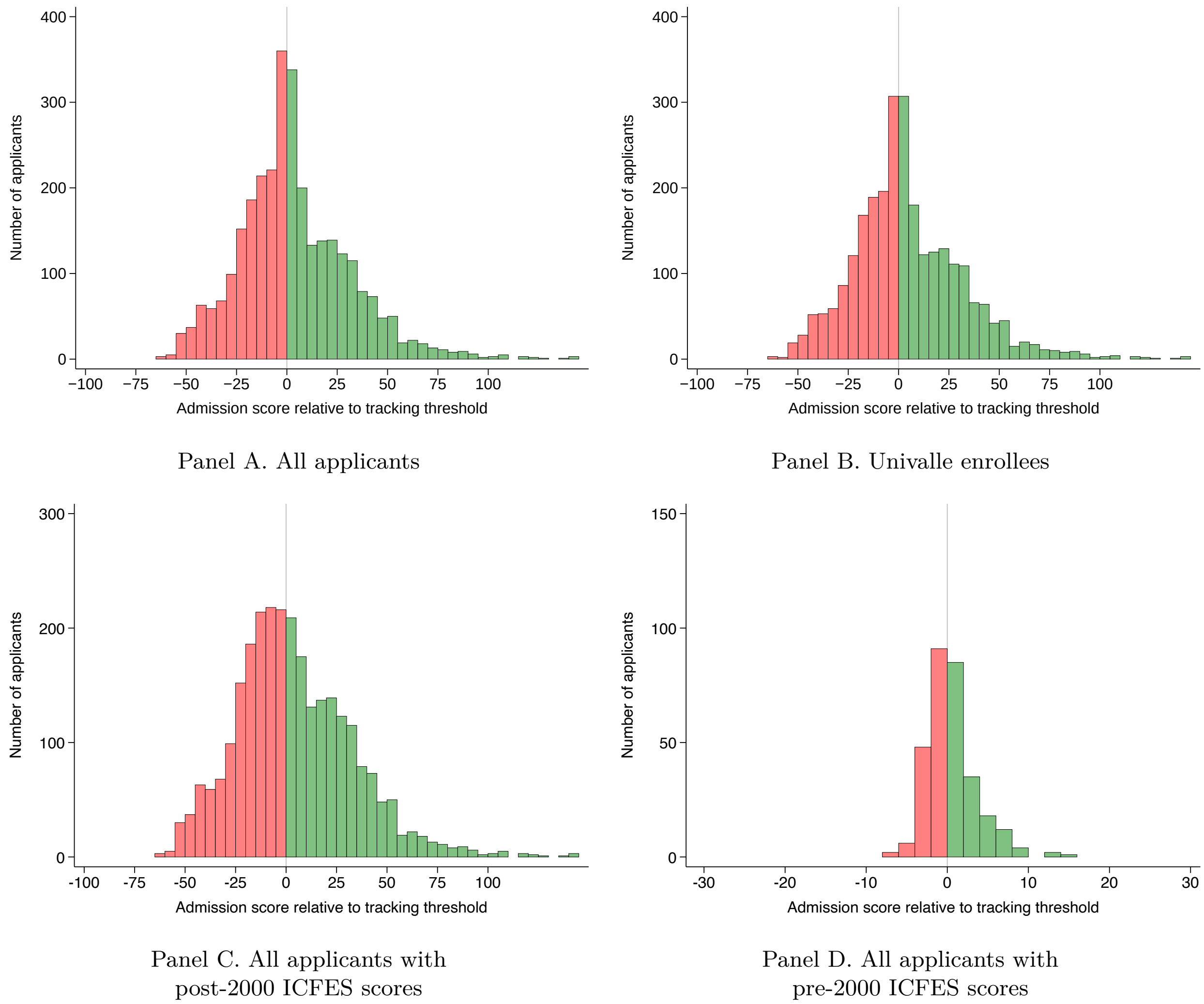
Columns (D)–(E) estimate RD regressions separately for female and male students. Column (F) reports the p value from a test of equality of the RD coefficients in columns (D)–(E).

All dependent variables, samples, and RD regressions are defined in the same way as in column (B) of Tables 4–5. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix — For Online Publication

A. Appendix figures and tables



Panel A. All applicants

Panel B. Univalle enrollees

Panel C. All applicants with post-2000 ICFES scores

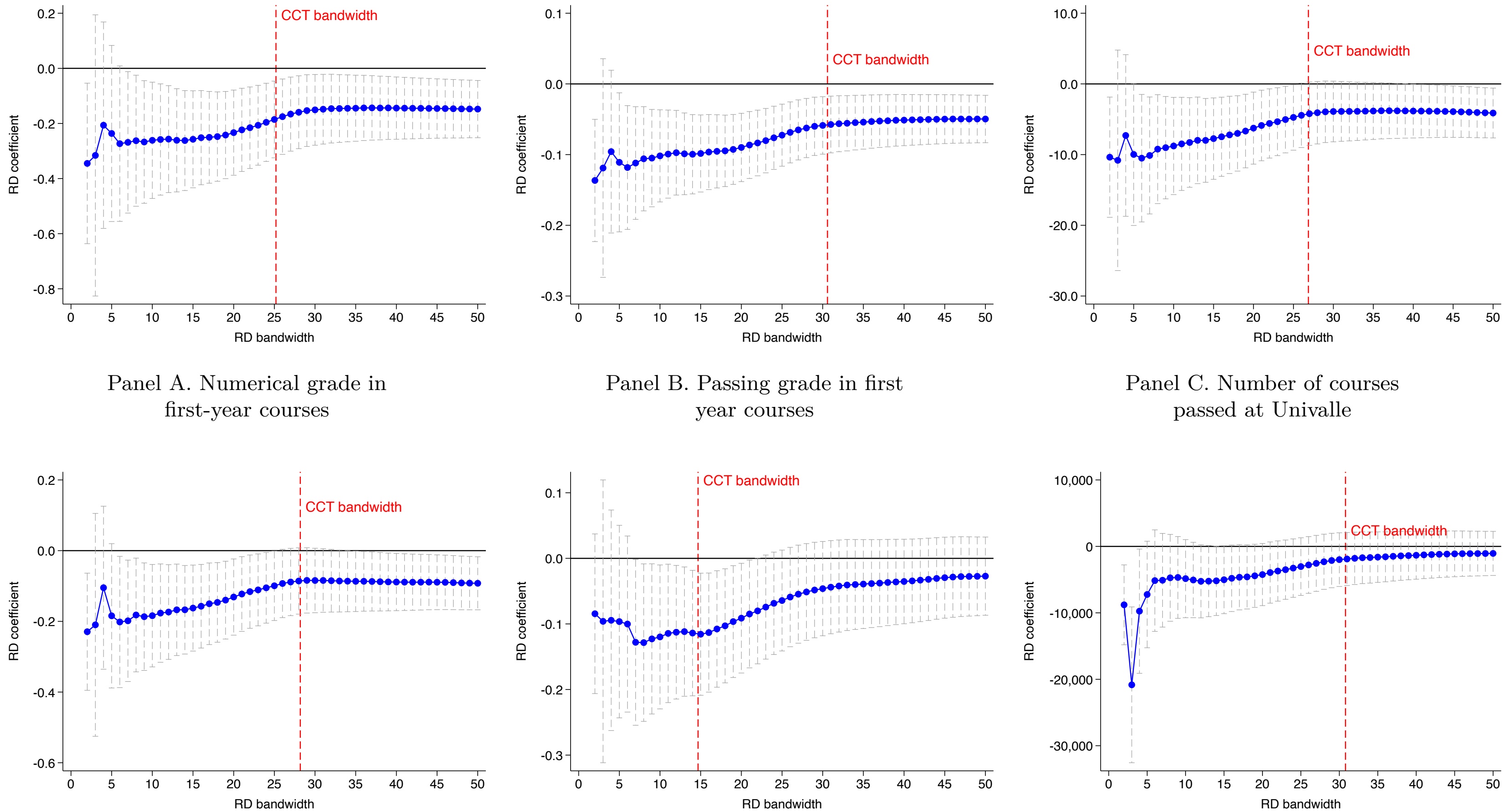
Panel D. All applicants with pre-2000 ICFES scores

Figure A1. Density of admission scores relative to the tracking threshold

Notes: This table displays density of admission scores normalized relative to Univalle's tracking threshold. The x-axis in each panel is a student's admission score (e.g., column B in Table 1) normalized so that zero is the first rank above the tracking threshold. The y-axis shows the number of applicants within five unit bins.

Panel A shows the distribution of admission scores for all Univalle admits. Using the McCrary (2008) density test, the estimated discontinuity—i.e., the log difference in height at the threshold—is 0.011 with a standard error of 0.073. Panel B shows the distribution of admission scores for students who enrolled in Univalle. The estimated density discontinuity is 0.054 with a standard error of 0.078.

In Panels A–B, the heaping of admission scores near the threshold arises because Univalle allowed applicants to apply with scores from the pre-2000 version of the ICFES exam for several years after the exam reform (see Section 1.3). Many of these admission pools had a small number of applicants. Panels C–D show the distribution of admission scores for applicants who took the post-2000 and pre-2000 ICFES exams. Univalle's admission scores are a weighted average of an applicant's ranks in their admission pool based on each ICFES subject score. Thus the variance of admission scores is smaller in the admission pools that used pre-2000 ICFES scores.



Panel A. Numerical grade in first-year courses

Panel B. Passing grade in first year courses

Panel C. Number of courses passed at Univalle

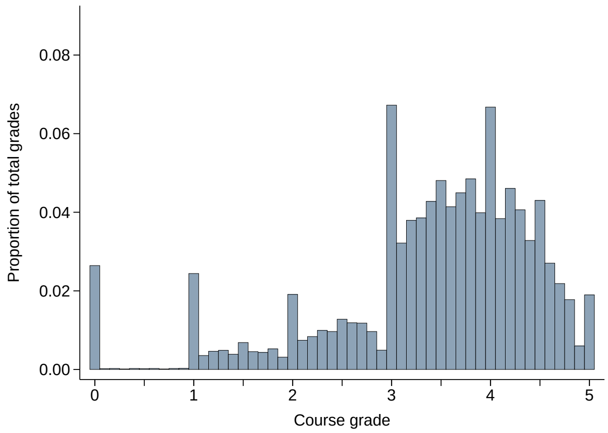
Panel D. Graduated from Univalle

Panel E. Has any formal earnings in 2008–12

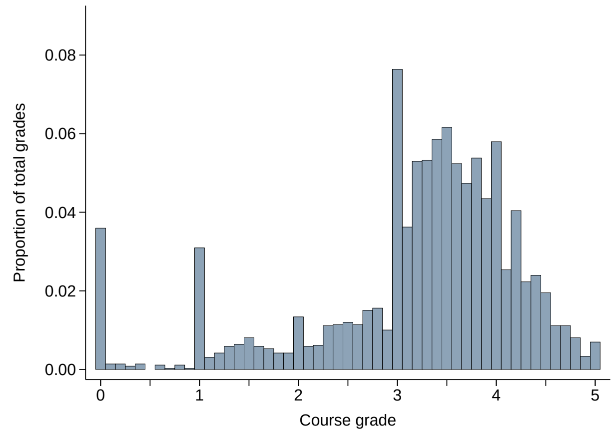
Panel F. Total formal earnings in 2008–12

FIGURE A2. Tracking effects by RD bandwidth — All programs

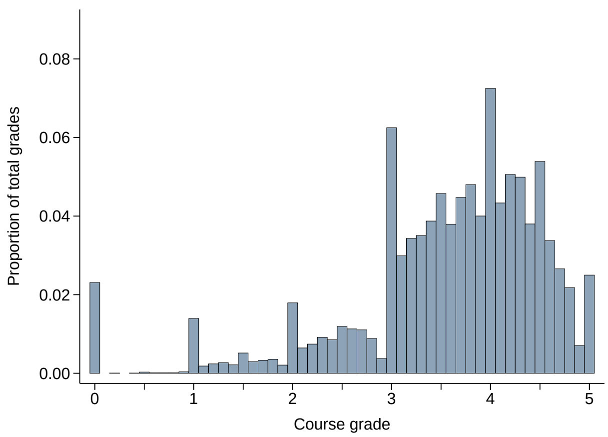
Notes: This figure shows how our RD estimates for the effects of admission to a higher-ability cohort vary with the RD bandwidth. In each panel, the blue marker represents the RD coefficient estimated using a triangular kernel and the bandwidth depicted on the x-axis, which we allow to range from 2–50 admission ranks. The dashed lines around each point estimate are 95 percent confidence intervals using standard errors clustered at the individual level. We use the same six outcome variables as in Figure 3. The red dashed vertical line denotes the Calonico et al. (2019) bandwidth in our benchmark specification, as in Tables 4–5.



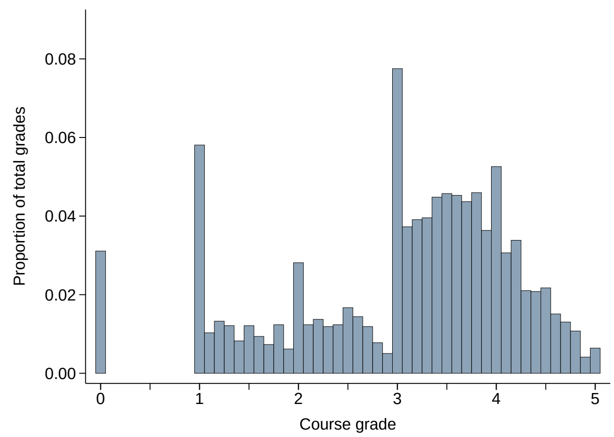
Panel A. All programs



Panel B. Architecture



Panel C. Business

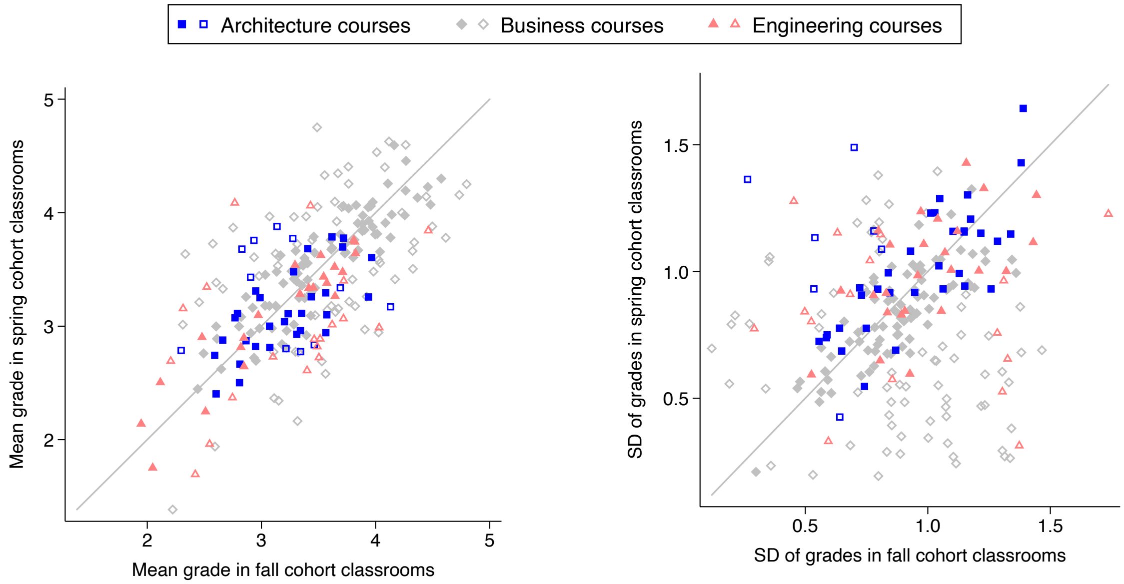


Panel D. Engineering

Figure A3. Grade distributions in first-year required courses

Notes: This figure shows grade distributions in first-year required courses at Univalle. The graphs include an observation for each individual's first attempt at each course. Colombian college grades are on a 0–5 scale at 0.1 point increments, with 3 or above denoting a passing grade. The height of each bar is the number of grades for each 0.1 point increment as a proportion of all grades.

Panel A shows the grade distribution in all courses across all programs. Panels B–D show the grade distributions for courses in architecture, business, and engineering programs.



Panel A. Mean grade

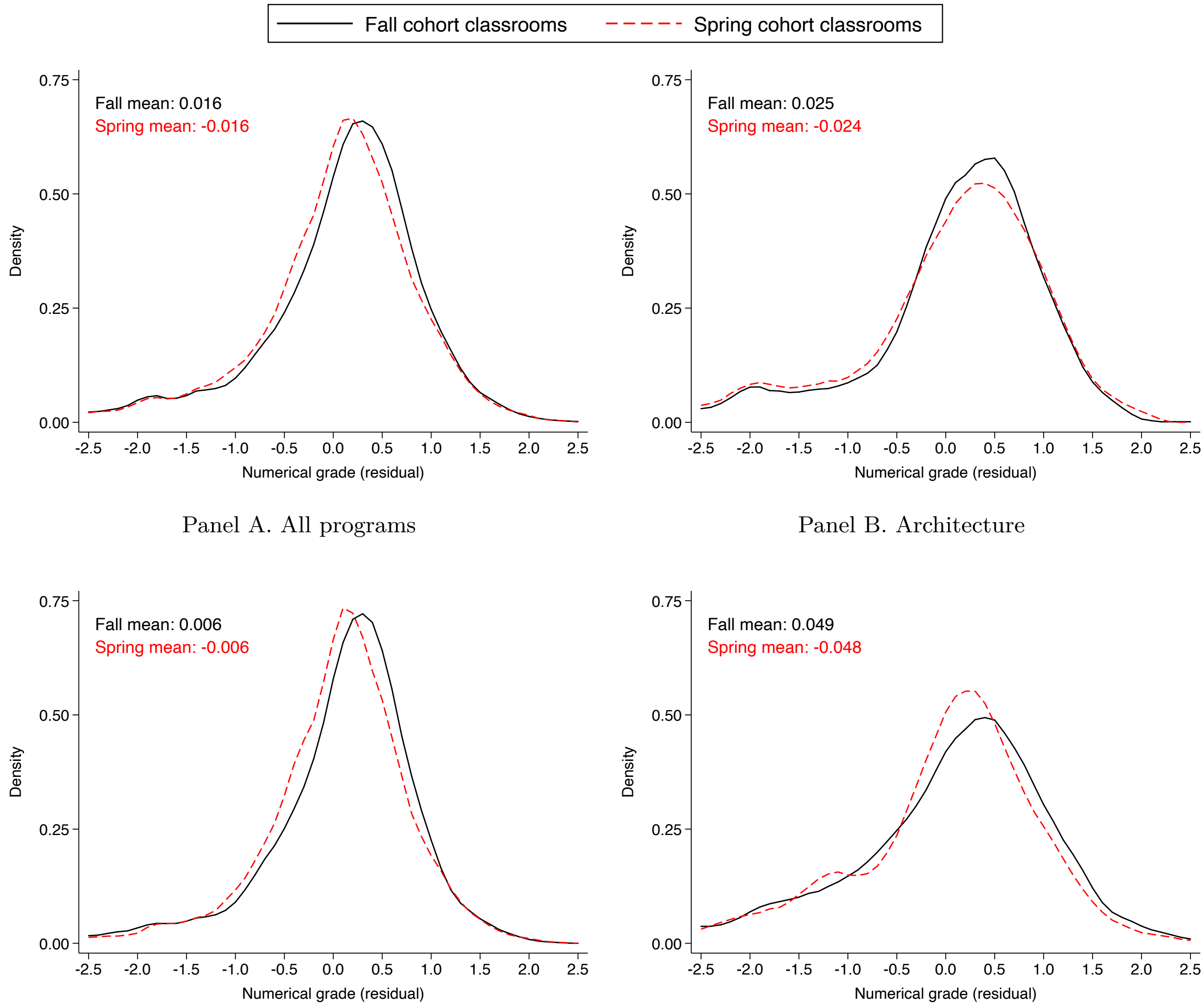
Panel B. Standard deviation of grades

FIGURE A4. Mean and SD of grades in first-year fall and spring cohort classrooms

Notes: This figure plots grade means and standard deviations in first-year required courses. We assign the classrooms of each course to a program and cohort, where cohorts are defined by enrollment year and semester. "Fall cohort classrooms" are those in which at least 50 percent of students were from the fall cohort of a given program. "Spring cohort classrooms" are those in which at least 50 percent of students were from the spring cohort of a given program. We omit classrooms with no cohort majority or with fewer than ten students. 591 classrooms fit these criteria.

In Panel A, dots depict the mean grade at the program/cohort/course level. The x-axis is the mean grade in fall cohort classrooms. The y-axis is the mean grade in the subsequent spring cohort's classrooms of the same program and course. There are 456 program/cohort/course cells, and thus 228 dots. Panel B is similar to Panel A, but dots depict the standard deviation of grades at the program/cohort/course level.

Blue squares are architecture courses. Grey diamonds are business courses. Red triangles are engineering courses. Hollow symbols indicate that grade means/standard deviations are statistically different in fall and spring cohort classrooms at $p < 0.05$. We reject identical means in 41 percent of courses, and we reject identical standard deviations in 40 percent of courses.



Panel A. All programs

Panel B. Architecture

Panel C. Business

Panel D. Engineering

FIGURE A5. Grade distributions in first-year fall and spring cohort classrooms

Notes: This figure plots grade distributions in first-year required courses. We assign the classrooms of each course to a program and cohort, where cohorts are defined by enrollment year and semester. "Fall cohort classrooms" are those in which at least 50 percent of students were from the fall cohort of a given program. "Spring cohort classrooms" are those in which at least 50 percent of students were from the spring cohort of a given program. We omit classrooms with no cohort majority or with fewer than ten students. 591 classrooms fit these criteria.

We demean grades at the program × enrollment year level, and then plot the grade distributions in each panel. The black solid line depicts the demeaned grade distribution in fall cohort classrooms, and the red dashed line depicts the demeaned grade distribution in spring cohort classrooms. Panel A shows the distributions across all programs. Panels B–D show the distributions in architecture, business, and engineering programs.

Table A1. Univalle programs with tracking admissions

		(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
			Apply pools	Total applied	Total admitted		Mean cohort size		Grad rate
Group	Program	Years		Fall	Fall	Spring	Fall	Spring	
Arch	Architecture	2000–03	7	1,440	217	229	50	50	0.53
Bus	Accounting (day)	2000–03	7	912	270	289	67	64	0.58
	Accounting (night)	2000–03	7	911	193	190	46	44	0.53
	Business Admin (day)	2000–03	7	1,159	285	287	68	62	0.63
	Business Admin (night)	2000–03	7	923	194	193	45	44	0.49
	Foreign Trade	2003	1	123	42	49	45	40	0.64
Eng	Chemical Engineering	2000	2	228	72	58	59	49	0.50
	Electrical Engineering	2000	2	128	65	60	60	52	0.41
	Electronic Engineering	2000	2	399	68	60	58	56	0.54
	Materials Engineering	2001	2	120	64	56	59	54	0.30
	Mechanical Engineering	2001	2	201	64	54	56	59	0.24
	Total	2000–03	46	6,544	1,534	1,525	56	53	0.53

Notes: This table provides details on applications, admissions, and enrollment in the Univalle programs in our sample. Column (A) shows the years that each program in our sample used tracking. Column (B) shows the number of application pools in these programs and years. In 2000–2002, each program had separate admissions for students applying with pre- and post-2000 ICFES scores. In 2003, all applicants had to submit post-2000 exam scores.

Column (C) shows the total number of applicants (all applicants applied in the fall). Columns (D)–(E) show the total number of students admitted to the fall and spring cohorts. Columns (F)–(G) show the mean number of students who enrolled in each cohort. Column (H) shows each program's graduation rate across all cohorts.

Table A2. Balance tests — Univalle admits

	(A)	(B)	(C)	(D)	(E)
	Mean below threshold	RD coefficients			
Dependent variable	All programs	All programs	Architecture	Business	Engineering
Predicted GPA	3.382	0.005	0.008	−0.007	0.025
		(0.016)	(0.052)	(0.021)	(0.026)
Female	0.395	0.035	0.126	−0.008	0.098
		(0.041)	(0.139)	(0.060)	(0.088)
Age	20.106	0.077	1.191	0.141	−0.771
		(0.295)	(0.914)	(0.421)	(0.471)
Years since HS graduation	1.720	0.243	1.264	0.063	−0.026
		(0.219)	(0.905)	(0.256)	(0.469)
ICFES percentile	0.828	−0.010	−0.011	−0.017	−0.003
		(0.014)	(0.051)	(0.020)	(0.023)
Secondary educated mother	0.647	−0.054	−0.163	−0.027	−0.085
		(0.045)	(0.130)	(0.060)	(0.114)
College educated mother	0.267	−0.001	0.246	−0.021	−0.077
		(0.041)	(0.158)	(0.048)	(0.129)
Secondary educated father	0.675	−0.063	−0.012	−0.024	−0.185
		(0.056)	(0.156)	(0.068)	(0.151)
College educated father	0.344	−0.028	0.406**	−0.039	−0.196
		(0.057)	(0.174)	(0.065)	(0.134)
Family income > 2x min wage	0.316	−0.035	−0.057	−0.003	−0.111
		(0.048)	(0.155)	(0.057)	(0.133)
N (Predicted GPA)	142	2,256	345	1,444	467
N (Female)	205	3,059	446	1,992	621
p value: Jointly zero		0.846	0.001	0.998	0.430

Notes: This table displays RD balance tests for all Univalle admits in our sample.

In the first row, the dependent variable is the predicted value from a regression of an applicant's first-year GPA on all individual characteristics listed in the remaining rows (age; years since high school graduation; ICFES percentile; and indicators for gender, a secondary educated mother, a college educated mother, a secondary educated father, a college educated father, and family income above 2x minimum wage). All other rows use these individual characteristics as dependent variables in separate regressions.

Column (A) displays means of each dependent variable for applicants with admission ranks between −5 and −1. Column (B) displays estimates of the RD coefficient π from equation (1) using each dependent variable. Columns (C)–(E) displays π coefficients from separate estimations for the architecture, business, and engineering program groups. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

The bottom row of the table reports p values from F tests that the coefficients on all covariates (except predicted GPA) are equal to zero.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3. Balance tests — Univalle enrollees

	(A)	(B)	(C)	(D)	(E)
	Mean below threshold	RD coefficients			
Dependent variable	All programs	All programs	Architecture	Business	Engineering
Predicted GPA	3.378	0.022	0.118**	−0.004	0.041
		(0.018)	(0.057)	(0.022)	(0.031)
Female	0.395	0.049	0.215	−0.014	0.169*
		(0.044)	(0.165)	(0.067)	(0.094)
Age	19.900	0.175	1.375	0.310	−0.680
		(0.308)	(1.104)	(0.441)	(0.516)
Years since HS graduation	1.743	0.075	0.041	0.014	−0.118
		(0.214)	(0.557)	(0.269)	(0.552)
ICFES percentile	0.821	−0.011	0.004	−0.018	−0.007
		(0.015)	(0.058)	(0.021)	(0.026)
Secondary educated mother	0.641	−0.035	−0.255*	0.011	−0.076
		(0.048)	(0.138)	(0.063)	(0.124)
College educated mother	0.269	−0.005	0.180	−0.012	−0.059
		(0.043)	(0.193)	(0.051)	(0.142)
Secondary educated father	0.652	−0.026	0.032	0.021	−0.165
		(0.061)	(0.182)	(0.075)	(0.166)
College educated father	0.312	−0.002	0.493**	−0.020	−0.178
		(0.060)	(0.193)	(0.069)	(0.144)
Family income > 2x min wage	0.317	−0.033	−0.072	0.007	−0.118
		(0.050)	(0.168)	(0.058)	(0.140)
N (Predicted GPA)	127	2,057	308	1,342	407
N (Female)	177	2,703	379	1,799	525
p value: Jointly zero		0.966	0.000	0.997	0.359

Notes: This table displays RD balance tests for Univalle enrollees in our sample. This table is identical to Appendix Table A2, except the sample for each regression is restricted to students who enrolled in Univalle.

In the first row, the dependent variable is the predicted value from a regression of an applicant's first-year GPA on all individual characteristics listed in the remaining rows (age; years since high school graduation; ICFES percentile; and indicators for gender, a secondary educated mother, a college educated mother, a secondary educated father, a college educated father, and family income above 2x minimum wage). All other rows use these individual characteristics as dependent variables in separate regressions.

Column (A) displays means of each dependent variable for applicants with admission ranks between −5 and −1. Column (B) displays estimates of the RD coefficient π from equation (1) using each dependent variable. Columns (C)–(E) displays π coefficients from separate estimations for the architecture, business, and engineering program groups. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

The bottom row of the table reports p values from F tests that the coefficients on all covariates (except predicted GPA) are equal to zero.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4. Tracking effects on sample attrition — All programs

Dependent variable	(A) N in full sample	(B) N with variable defined	(C) RD coefficients for prob. of observing variable: CCT bwidth for Pr(observed)	(D) RD coefficients for prob. of observing variable: CCT bwidth for Dep. Var.
Panel A. First-year grades				
Numerical grade (first attempt)	26,988	24,055	−0.022	−0.008
			(0.022)	(0.020)
Passing grade (first attempt)	26,988	24,055	−0.022	−0.000
			(0.022)	(0.018)
Panel B. Educational attainment				
# courses passed at Univalle	3,059	2,703	0.025	0.026
			(0.034)	(0.033)
# full-time semesters at Univalle	3,059	2,703	0.025	0.026
			(0.034)	(0.033)
Graduated from Univalle	3,059	2,703	0.025	0.027
			(0.034)	(0.032)
Any college degree	3,059	3,059		
Panel C. Employment and earnings over 2008–2012				
Has any formal earnings	3,059	3,059		
Total formal earnings	3,059	3,059		
Log # formal employment days	3,059	2,363	-0.115^{**}	−0.048
			(0.048)	(0.037)
Log mean daily earnings	3,059	2,363	-0.115^{**}	-0.068^{*}
			(0.048)	(0.040)

Notes: This table describes sample attrition for our main outcome variables.

Panel A includes outcome variables from Table 4 (Panel B), which are defined at the individual × class level with an observation for each individual's first attempt at each first-year course. Column (A) shows the number of observations in the full sample for these outcomes, which includes all Univalle enrollees × all first-year required courses in each program. Column (B) shows the number of individual × class observations in which we observe a grade.

Panels B–C include outcome variables from Table 5, which are defined as the individual level. Column (A) shows the number of observations in the full sample for these outcomes, which includes all Univalle admits. Column (B) shows the number of individuals for which each outcome variable is defined, which is either the full sample of admits, the sample of Univalle enrollees, or the sample of Univalle admits who appear in our earnings data.

Columns (C)–(D) report estimates of the RD coefficient π from equation (1) for dependent variables that are indicators for observing each outcome variable. Column (C) uses the Calonico et al. (2019) bandwidth computed for the binary indicator for observing that variable, while column (D) uses the Calonico et al. (2019) from the RD regression that uses that outcome as the dependent variable. Both columns use triangular kernels. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5. Summary statistics for first-year required courses

		(A)	(B)	(C)	(D)	(E)	(F)
Group	Program	Total # students	# courses per student	# students in classroom	% of classmates from own cohort	Mean grade	% pass
Arch	Architecture	385	9.1	36.4	0.64	3.16	0.75
Bus	Accounting (day)	510	9.8	41.0	0.82	3.48	0.81
	Accounting (night)	351	8.6	36.5	0.77	3.34	0.77
	Business Admin (day)	506	9.8	36.9	0.80	3.68	0.87
	Business Admin (night)	345	6.9	34.1	0.80	3.54	0.84
	Foreign Trade	84	8.1	33.1	0.80	3.59	0.85
Eng	Chemical Engineering	103	8.2	31.6	0.80	3.16	0.73
	Electrical Engineering	109	8.4	29.9	0.74	2.99	0.67
	Electronic Engineering	98	9.1	31.7	0.68	3.27	0.78
	Materials Engineering	109	7.3	26.2	0.84	2.90	0.60
	Mechanical Engineering	103	8.2	25.0	0.85	2.87	0.61
	Total	2,703	9.2	35.7	0.77	3.38	0.79

Notes: This table displays summary statistics on first-year required courses for each program in our sample. We define first-year required courses as those were taken by at least 75 percent of a cohort's graduates, and in which the modal graduate took the course in their first year.

Column (A) is the total number of students in our sample who took any first-year required course. Column (B) is the average number of first-year required courses taken by each student. Column (C) is the average number of classmates in individual's first-year courses. Column (D) is the average proportion of these students who are from the individual's own cohort, where cohort is defined by program and starting semester. Columns (E) and (F) are the mean grade and mean course passing rate across all first-year required courses.

Table A6. Examples of first-year required courses

Architecture			Business			Engineering		
Course	Took course	Pass rate	Course	Took course	Pass rate	Course	Took course	Pass rate
Graphics I	0.95	0.62	Basic Mathematics	0.94	0.63	Calculus I	0.91	0.48
Projection Geometry	0.94	0.62	Calculus	0.89	0.72	Vector Geometry	0.94	0.56
Workshop Project I	0.97	0.68	Intro to Accounting	0.98	0.78	Linear Algebra	0.93	0.58
Intro to Technology	0.93	0.76	Computing I	0.96	0.92	Physics I	0.91	0.65
Theory I	0.94	0.81	Colombian Politics	0.93	0.94	Calculus II	0.90	0.70

Notes: This table shows the five most common first-year required courses in each of the three program groups in our sample: architecture, business, and engineering (see Table 2). We define first-year required courses as those that were taken by 75 percent or more of a cohort's graduates, and for which the modal graduate took the course in their first year. "Took course" is the proportion of a cohort's graduates who took the course. "Pass rate" is the proportion of course enrollees who passed the class.

Table A7. Tracking effects by RD bandwidth and kernel — All programs

	(A)	(B)	(C)	(D)	(E)	(F)
		Triangular kernel			Uniform kernel	
	Bandwidth	RD coefficients by bandwidth choice				
Dependent variable	CCT	CCT	0.5× CCT	2× CCT	CCT	30
Panel A. First-year grades						
Numerical grade (first attempt)	25.199	-0.183^{***}	-0.260^{***}	-0.148^{***}	-0.240^{***}	-0.126^{**}
		(0.071)	(0.097)	(0.053)	(0.079)	(0.060)
Passing grade (first attempt)	30.586	-0.058^{***}	-0.098^{***}	-0.049^{***}	-0.080^{***}	-0.045^{**}
		(0.021)	(0.028)	(0.016)	(0.024)	(0.019)
Effective N (Numerical grade)		12,259	6,708	20,415	8,408	14,327
Panel B. Educational attainment						
# courses passed at Univalle	26.876	-4.238^{*}	-7.986^{***}	-4.225^{**}	-5.765^{**}	-3.780^{*}
		(2.288)	(3.067)	(1.760)	(2.566)	(2.015)
# full-time semesters at Univalle	26.453	-0.725^{*}	-1.311^{**}	-0.646^{**}	-0.764^{*}	-0.593^{*}
		(0.387)	(0.520)	(0.296)	(0.420)	(0.337)
Graduated from Univalle	28.187	-0.086^{*}	-0.167^{***}	-0.094^{**}	-0.077	-0.084^{*}
		(0.048)	(0.064)	(0.037)	(0.052)	(0.043)
Any college degree	25.339	-0.087^{*}	-0.128^{**}	-0.090^{**}	-0.086^{*}	-0.089^{**}
		(0.048)	(0.065)	(0.036)	(0.051)	(0.041)
Effective N (Any degree)		1,605	888	2,656	1,228	1,865
Panel C. Employment and earnings over 2008–2012						
Has any formal earnings	14.676	-0.115^{**}	-0.128^{**}	-0.048	-0.059	-0.022
		(0.048)	(0.063)	(0.037)	(0.042)	(0.035)
Total formal earnings	31	$-1{,}916$	$-4{,}917^{*}$	-948	$-3{,}090$	$-1{,}037$
		(2,038)	(2,578)	(1,591)	(2,311)	(1,906)
Log # formal employment days	29.123	0.094	0.136	0.065	0.051	0.093
		(0.100)	(0.133)	(0.080)	(0.109)	(0.093)
Log mean daily earnings	24.173	-0.066	-0.067	-0.052	-0.066	-0.056
		(0.051)	(0.065)	(0.040)	(0.060)	(0.044)
Effective N (Has earnings)		1,004	581	1,813	1,228	1,865

Notes: This table shows how our RD estimates vary with the choice of kernel and bandwidth. All dependent variables, samples, and RD regressions are defined in the same way as in column (B) of Tables 4–5. Column (A) shows the Calonico et al. (2019) bandwidth in our benchmark RD specification, and column (B) shows the RD coefficient from this specification. Columns (C)–(D) show RD coefficients using bandwidths that are 0.5x and 2x the value in column (A). Column (E) shows RD coefficients using the Calonico et al. (2019) and a uniform (as opposed to triangular) kernel. Column (F) uses a uniform kernel and a constant bandwidth of 30 across all outcome variables. The sample sizes correspond to the Effective N (the number of observations within the RD bandwidth) as opposed to the total sample for each variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8. Tracking effects by running variable — All programs

	(A)	(B)	(C)
	RD coefficients by running variable		
Dependent variable	Admission ranks	Admission scores	Normalized admission scores
Panel A. First-year grades			
Numerical grade (first attempt)	-0.183^{***}	-0.210^{***}	-0.201^{**}
	(0.071)	(0.079)	(0.095)
Passing grade (first attempt)	-0.058^{***}	-0.082^{***}	-0.077^{***}
	(0.021)	(0.025)	(0.028)
N (Numerical grade)	23,841	23,841	23,841
Panel B. Educational attainment			
# courses passed at Univalle	-4.238^{*}	-5.912^{**}	-6.541^{**}
	(2.288)	(2.498)	(2.697)
# full-time semesters at Univalle	-0.725^{*}	-0.989^{**}	-1.051^{**}
	(0.387)	(0.427)	(0.450)
Graduated from Univalle	-0.086^{*}	-0.130^{**}	-0.127^{**}
	(0.048)	(0.055)	(0.057)
Any college degree	-0.087^{*}	-0.113^{*}	-0.112^{*}
	(0.048)	(0.058)	(0.062)
N (Any college degree)	3,059	3,059	3,059
Panel C. Employment and earnings over 2008–2012			
Has any formal earnings	-0.115^{**}	-0.102^{**}	-0.109^{**}
	(0.048)	(0.051)	(0.053)
Total formal earnings	−1,916	−3,647	−3,125
	(2,038)	(2,381)	(2,321)
Log # formal employment days	0.094	0.080	0.127
	(0.100)	(0.114)	(0.141)
Log mean daily earnings	−0.066	−0.051	−0.035
	(0.051)	(0.052)	(0.056)
N (Has any formal earnings)	3,059	3,059	3,059

Notes: This table shows how our RD estimates vary with the choice running variable. All dependent variables, samples, and RD regressions are defined in the same way as in column (B) of Tables 4–5. Column (A) shows our benchmark RD estimates, which use admission ranks normalized to zero at the tracking threshold as the running variable. Column (B) uses admission *scores* rather than admission ranks, as in column (B) of Table 1. Column (C) is the same as column (B), except admission scores are further standardized to SD one within the set of students who applied to the same program in the same year. All RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9. Timing of tracking effects — All programs

Dependent variable	(A) RD coefficients by years since application: 5–6 years	(B) 7–8 years	(C) 9–10 years	(D) 11–12 years
Panel A. Means below threshold				
# courses passed at Univalle	7.751	1.395	0.944	0.220
# full-time semesters at Univalle	1.373	0.186	0.130	0.028
Graduated from Univalle	0.345	0.480	0.542	0.565
Any college degree	0.351	0.502	0.585	0.620
Has any formal earnings	0.758	0.746	0.761	0.719
Total formal earnings	4,814	6,164	8,442	9,314
Log # formal employment days	5.498	5.670	5.916	5.713
Log mean daily earnings	2.745	2.799	2.855	3.128
Panel B. RD coefficients				
# courses passed at Univalle	-2.950^{***}	-0.307	-1.086^{***}	-0.065
	(0.718)	(0.303)	(0.411)	(0.119)
# full-time semesters at Univalle	-0.542^{***}	-0.085^{*}	-0.167^{**}	-0.016
	(0.125)	(0.052)	(0.072)	(0.020)
Graduated from Univalle	-0.018	-0.056	-0.060	-0.080
	(0.042)	(0.050)	(0.050)	(0.050)
Any college degree	-0.026	-0.061	-0.063	-0.077
	(0.039)	(0.046)	(0.050)	(0.049)
Has any formal earnings	-0.178^{**}	-0.110^{**}	-0.045	0.010
	(0.085)	(0.055)	(0.042)	(0.053)
Total formal earnings	$-1{,}384^{*}$	-381	-535	$-1{,}782$
	(800)	(649)	(804)	(1,313)
Log # formal employment days	-0.010	0.061	0.094	0.067
	(0.169)	(0.096)	(0.113)	(0.118)
Log mean daily earnings	-0.048	0.008	-0.065	-0.181^{**}
	(0.075)	(0.055)	(0.058)	(0.087)
N (Has any formal earnings)	1,229	3,059	3,059	1,830

Notes: This table shows how our RD estimates for educational attainment and labor market outcomes vary when we measure outcomes at different lengths of time since Univalle application.

All dependent variables, samples, and RD regressions are similar to those in column (B) of Table 5, but we measure outcomes at 5–6, 7–8, 9–10, and 11–12 years since individuals applied to Univalle, as listed in the column header. "Graduated from Univalle" and "Any college degree" are indicators for graduation by the last of the two years in each time range. "Has any formal earnings" is an indicator for appearing in our earnings data in either year in that each time range. All other variables are totals or averages over each two year period.

Panel A displays means of each dependent variable for applicants with admission ranks between −5 and −1. Panel B displays estimates of the RD coefficient π from equation (1) using each dependent variable in a sample that includes all programs. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10. Tracking effects by cohort — Architecture & business programs

	(A)	(B)	(C)	(D)	(E)	(F)
	Architecture & business (excluding foreign trade)					Foreign trade
Dependent variable	All cohorts	2000 cohort	2001 cohort	2002 cohort	2003 cohort	2003 cohort
Panel A. Univalle enrollment						
Enrolled in cohort admitted to	−0.004	−0.010	0.120	−0.232***	0.098	0.374
	(0.035)	(0.067)	(0.079)	(0.086)	(0.107)	(0.243)
Predicted GPA (if enrolled)	−0.001	0.006	−0.064	0.003	0.060	0.108
	(0.023)	(0.049)	(0.048)	(0.036)	(0.070)	(0.145)
N (Enrolled)	2,347	493	716	637	501	91
Panel B. First-year grades						
Numerical grade (first attempt)	−0.287***	−0.430*	−0.093	−0.273*	−0.094	−0.186
	(0.088)	(0.257)	(0.158)	(0.139)	(0.195)	(0.237)
Passing grade (first attempt)	−0.087***	−0.116	−0.056	−0.077*	−0.040	−0.155**
	(0.025)	(0.073)	(0.049)	(0.045)	(0.061)	(0.063)
N (Numerical grade)	18,875	3,645	5,888	5,501	3,841	682
Panel C. Educational attainment						
# courses passed at Univalle	−6.593**	−5.915	−6.571	−12.213**	3.624	−19.903**
	(2.779)	(6.730)	(5.031)	(4.786)	(8.190)	(9.941)
Any college degree	−0.111*	−0.224*	0.046	−0.202*	−0.092	−0.504*
	(0.059)	(0.135)	(0.110)	(0.114)	(0.154)	(0.302)
N (Any college degree)	2,347	493	716	637	501	91
Panel D. Employment and earnings over 2008–2012						
Has any formal earnings	−0.114**	−0.056	0.016	−0.284***	−0.120	−0.059
	(0.056)	(0.114)	(0.086)	(0.094)	(0.123)	(0.316)
Total formal earnings	−3,845	−9,257	519	−5,649	−1,213	−19,707
	(2,456)	(5,989)	(4,526)	(3,825)	(5,839)	(13,361)
N (Has any formal earnings)	2,347	493	716	637	501	91

Notes: This table shows how our RD estimates vary across cohorts for architecture and business programs. All dependent variables, samples, and RD regressions are defined in the same way as in column (C)–(D) of Tables 3–5. Column (A) shows RD coefficients in a sample that includes all architecture and business programs except for foreign trade. Columns (B)–(E) present RD coefficients estimated separately for each of the 2000–2003 cohorts in these programs. Column (F) shows RD coefficients for foreign trade, which has only a 2003 cohort in our data. All RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11. Quantile RD effects for first-year GPA

	(A)	(B)	(C)	(D)	(E)
	Dependent variable: First-year GPA				
	Mean/qtile below threshold	RD coefficients			
RD estimate	All programs	All programs	Architecture	Business	Engineering
Benchmark RD coefficient	3.358	−0.238**	−0.794**	−0.292**	0.012
		(0.096)	(0.371)	(0.115)	(0.216)
10^{th} quantile	2.080	−0.359***	−0.790	−0.230*	−0.305
		(0.103)	(0.733)	(0.138)	(0.207)
20^{th} quantile	2.910	−0.133*	−0.838	−0.179*	−0.128
		(0.076)	(1.141)	(0.097)	(0.333)
30^{th} quantile	3.246	−0.187***	−1.283*	−0.212**	−0.090
		(0.068)	(0.663)	(0.086)	(0.297)
40^{th} quantile	3.420	−0.158***	−1.180**	−0.166**	−0.084
		(0.060)	(0.522)	(0.066)	(0.234)
50^{th} quantile	3.544	−0.120**	−0.831**	−0.120	−0.115
		(0.057)	(0.362)	(0.085)	(0.136)
60^{th} quantile	3.673	−0.053	−0.672**	−0.022	−0.077
		(0.033)	(0.298)	(0.064)	(0.123)
70^{th} quantile	3.827	−0.103**	−0.743**	−0.087	−0.098
		(0.046)	(0.308)	(0.062)	(0.116)
80^{th} quantile	3.991	−0.062*	−0.865***	−0.063	−0.189
		(0.037)	(0.151)	(0.065)	(0.163)
90^{th} quantile	4.167	−0.062	−0.710***	−0.059	−0.038
		(0.067)	(0.116)	(0.077)	(0.118)
N (First-year GPA)	176	2,703	385	1,796	522

Notes: This table displays quantile RD estimates for the effects of admission to a higher-ability cohort on mean GPA in first-year required courses.

The first row replicates our benchmark RD estimates for first-year GPA (i.e., the last row of Panel B in Table 4). All other rows report estimates from quantile RD regressions, where the quantile is listed in the first column. Column (A) displays the quantile of first-year GPA for applicants with admission ranks between −5 and −1. Columns (B) show RD coefficients for each quantile in regressions that include Univalle enrollees in all programs. Columns (C)–(E) show quantile RD coefficients separately for architecture, business, and engineering programs. All regressions use a triangular kernel and the same Calonico et al. (2019) bandwidth as in the benchmark regressions in the first row. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12. Tracking effects on classmate characteristics and grades in *upper-level* courses

	(A)	(B)	(C)	(D)	(E)
	Mean below threshold	RD coefficients			
Dependent variable	All programs	All programs	Architecture	Business	Engineering
Panel A. First-year classmate characteristics					
Mean ICFES percentile	0.784	0.047***	0.030*	0.054***	−0.011
		(0.004)	(0.016)	(0.005)	(0.016)
Individual rank by admission score	0.647	−0.317***	−0.179***	−0.347***	−0.226***
		(0.019)	(0.059)	(0.022)	(0.043)
Proportion female	0.530	0.008	0.056**	0.006	0.004
		(0.008)	(0.023)	(0.008)	(0.022)
Mean age at application	19.355	0.080	0.305	0.119	−0.265
		(0.074)	(0.268)	(0.084)	(0.265)
Proportion college educated mother	0.241	0.007	0.007	0.018***	−0.056
		(0.009)	(0.032)	(0.006)	(0.051)
N (Individual rank)	3,568	53,495	5,812	41,175	6,508
Panel B. Upper-level course grades					
Numerical grade (first attempt)	3.670	0.057	−0.155	0.022	0.310**
		(0.049)	(0.131)	(0.054)	(0.132)
Passing grade (first attempt)	0.888	0.013	−0.003	−0.004	0.038
		(0.012)	(0.038)	(0.014)	(0.065)
Retook course	0.073	−0.013	−0.005	−0.003	−0.068
		(0.009)	(0.033)	(0.008)	(0.042)
Ever passed course	0.953	0.001	−0.028*	−0.011	0.010
		(0.009)	(0.016)	(0.010)	(0.046)
Upper-level GPA	3.491	0.000	−0.169	−0.027	0.231
		(0.092)	(0.205)	(0.105)	(0.302)
N (Numerical grade)	3,568	53,495	5,812	41,175	6,508

Notes: This table displays RD estimates for the effects of admission to a higher-ability cohort on classmate characteristics and grades in upper-level required courses. This table is similar to Table 4, except we measure outcomes in upper-level rather than first-year required courses. We define upper-level required courses as those were taken by at least 75 percent of a cohort's graduates, and in which the modal graduate took the course *after* their first year.

In Panel A, the dependent variables are mean characteristics of the applicant's classmates in their upper-level courses. In Panel B, the dependent variables measure the applicant's academic performance in those courses. In both panels, regressions are at the individual × class level with an observation for each individual's first attempt at each upper-level course. The one exception is for the dependent variable "upper-level GPA," for which regressions are at the individual level. The sample for all regressions includes only Univalle enrollees.

Column (A) displays means of each dependent variable for applicants with admission ranks between −5 and −1. Column (B) displays estimates of the RD coefficient π from equation (1) using each dependent variable. Columns (C)–(E) displays π coefficients from separate estimations for the architecture, business, and engineering program groups. RD regressions use a triangular kernel and the Calonico et al. (2019) bandwidth computed for each sample and dependent variable. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B. Empirical appendix

This appendix provides details on our data and analysis sample.

We use three individual-level administrative datasets from 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*. The data were provided by the agency that administers the exam, and it contains all students who took the exam in 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. 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 dataset provided by the ICFES testing agency on a college exit exam called *Saber Pro* (formerly ECAES). This national exit exam became a requirement for graduation from any higher education institution in 2009. Column (A) in Table B1 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.[30] 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 include all universities but are missing a few technical colleges.[31] Overall, 96 percent of exit exam takers attend colleges that appear in the Ministry of Education records.

Finally, we use administrative earnings records from the Ministry of Social Protection for the years 2008–2012. The records are from the Ministry's electronic tax record system called *Planilla Integrada de Liquidación de Aportes* (PILA). Our data include monthly earnings for any individual who worked at a firm that was registered with the Ministry in these years. Our main earnings measure is average daily earnings, which we compute by by dividing

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

[31] 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*).

Table B1. 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.

total annual earnings by the number of formal employment days in the year. We also use an indicator for appearing in the earnings dataset as a measure of formal employment.

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.[32] 39 percent of the 1998–2004 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.[33] 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 2004.[34] The Ministry of Social Protection merged their earnings records into the Ministry of Education dataset using using ID numbers, birth dates, and names as well.

We combine the administrative data with two datasets provided by Universidad del Valle:

[32] 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.

[33] The gross tertiary enrollment rate ranged from 23 percent to 28 percent between 1998 and 2004 (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.

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

Table B2. Analysis sample

		(A)	(B)	(C)	(D)	(E)	(F)	(G)
		# applied			# admitted			
Group	Program	Total	Reserved quotas	General track	Total	Reserved quotas	General track	Merge rate
Arch	Architecture	1,488	48	1,440	465	19	446	0.989
Bus	Accounting (day)	928	16	912	575	16	559	0.996
	Accounting (night)	921	10	911	393	10	383	0.987
	Business Admin (day)	1,171	12	1,159	584	12	572	0.995
	Business Admin (night)	940	17	923	403	16	387	0.995
	Foreign Trade	126	3	123	94	3	91	0.989
Eng	Chemical Engineering	233	5	228	135	5	130	1.000
	Electrical Engineering	129	1	128	126	1	125	0.992
	Electronic Engineering	403	4	399	132	4	128	1.000
	Materials Engineering	120	0	120	120	0	120	0.992
	Mechanical Engineering	209	8	201	126	8	118	0.992
	Total	6,668	124	6,544	3,153	94	3,059	0.993

Notes: Column (A) shows the number of Univalle applicants to the programs and years with tracking admissions (see Appendix Table A1). Column (B) shows the number of students who applied through reserved quotas for indigenous or military applicants. Column (C) shows the number of general track applicants, which is the difference between columns (A) and (B). Column (D) shows the total number of admitted students. Column (E) shows the number of students who were admitted through reserved quotas. Column (F) shows our main analysis sample, which includes students who were admitted through the general track. Column (G) shows the proportion of students in our main sample (column F) who were matched to any of our administrative datasets using the method described in the text.

(1) Lists of applicants to Univalle's undergraduate programs from 2000–2003.
(2) Transcript records for all students in our sample of programs who enrolled in Univalle.

Our sample includes Univalle applicants to the programs and years with tracking admissions, as listed in Appendix Table A1. Column (A) of Table B2 shows the total number of applicants to the programs and cohorts in our sample. Column (B) shows that 124 students applied to these programs through reserved quotas for disadvantaged groups (e.g., indigenous students or military applicants). The remaining applicants applied through the general track, as shown in column (C). Columns (D)–(F) show the number of applicants who were admitted overall and through each track. Column (F) contains our main analysis sample, which includes only students admitted through the general track. Our regressions focus on the subset of these applicants who were near the tracking threshold, as defined by the Calonico et al. (2019) bandwidths for each outcome variable.

We merge the Univalle application and transcript data into the administrative data using applicants' full names. Since roughly 85 percent of applicants enrolled in the Univalle program they were admitted to, most individuals match uniquely on name, program, and cohort. Most applicants who enrolled in other programs also match uniquely on full name. In cases with duplicate names, we use information from the administrative records on individuals' exam cohorts and high school location to identify the correct match; most Colombian students stay in region for college and apply shortly after taking the ICFES entrance exam. Through this process we are able to match over 99 percent of individuals in our main analysis sample to our administrative records, as shown in column (G) of Table B2.