DISRUPTED ACADEMIC CAREERS: THE RETURNS TO TIME OFF AFTER HIGH SCHOOL

NICOLÁS DE ROUX*
EVAN RIEHL+

MAY 2020

ABSTRACT. This paper asks how academic breaks after high school affect individuals' college and labor market outcomes. We exploit a policy that altered academic calendars in two regions of Colombia, which caused thousands of high school graduates to have to wait an extra semester to start college. Using administrative data and a synthetic control design, we show that the academic break caused many students to forgo enrolling in college at all. High-ability students who did not attend college had lower earnings seven years later, but forgoing college had little effect on earnings for lower-ability students.

*de Roux: Department of Economics, Universidad de los Andes, Calle 19A No 1-37 Este, W 823, Bogotá, Colombia (email: nicolas.de.roux@uniandes.edu.co).
+Riehl (corresponding author): Department of Economics, Cornell University, 266 Ives Hall, Ithaca, NY 14853, United States (email: eriehl@cornell.edu).
For useful comments we thank Peter Arcidiacono, Peter Bergman, Christopher Hansman, Nandita Krishnaswamy, W. Bentley MacLeod, Jonah Rockoff, Juan E. Saavedra, Seth Sanders, Judith Scott-Clayton, and Miguel Urquiola. We are grateful to Luis Omar Herrera Prada for invaluable assistance with the data. All errors are our own.

1. Introduction

For many young adults, the transition from high school to college is not contiguous. In the U.S., roughly one-quarter of college students take at least one year off after high school before enrolling. Academic breaks are even more common in developing countries, where students are often uncertain about whether to attend college. Time away from school may increase the opportunity cost or relative disutility of schooling (Castleman and Page, 2015; Perez-Arce, 2015), which can discourage individuals from going to college. On the other hand, academic breaks may allow individuals to learn about their returns to college (Manski, 1993), and thus make better choices on whether to enroll or what type of degree to pursue.

This paper asks how academic breaks after high school affect individuals' college and labor market outcomes. To identify these effects, we exploit a government policy that altered academic calendars in two regions of Colombia, which forced thousands of high school graduates to have to wait an extra semester to start college. We find that the calendar shift caused the college enrollment rate to fall by eight percent in affected regions relative to other areas of the country. This suggests that time away from school caused some students to forgo college. High-ability students who were induced to skip college had lower earnings seven years later, but the labor market effects were muted for students with less academic preparation. These results show that academic breaks can reduce college enrollment rates for individuals with both high and lower returns to further schooling.

Our paper is motivated by the worldwide push to raise college enrollment rates. In many developing countries, policies to promote college attendance have contributed to a dramatic rise in tertiary enrollment in recent decades. These policies are based on the assumption that a college education is essential in a modern economy, and that attending college should be the default option for many high school graduates. But college is a risky investment, especially in countries where school quality varies significantly. Students who lack the academic preparation to succeed in college can potentially benefit from time off after high school, which breaks the inertia of going to college and allows them to explore their career options.

The setting for our paper is the country of Colombia, where academic breaks between high school and college are the norm. About 50 percent of Colombian high school graduates enroll in college, but the vast majority of enrollees take time off before starting college. As in the U.S., time gaps before college are more common for individuals with lower test scores and disadvantaged backgrounds. This fact suggests that there may be a causal link between time off and the propensity to enroll in college at all, although this link is difficult to isolate because students typically choose whether or not to take an academic break.

To identify the causal effects of time off from school, we exploit Colombia's unique system of academic calendars and a policy that altered these calendars. Colombian high schools operate on two different schedules; some schools begin the academic year in January, while others start in September. Most public schools use the January calendar, but in two regions, the public school year historically began in September. From 2008–2010, these regions transitioned public schools to the January calendar to align with the rest of the country. This altered the academic term at roughly 400 high schools that we call *switching schools*, which include all local public schools plus some private schools that followed suit. Roughly 100 private high schools in the area chose to remain on the September calendar. These *staying schools* included most of the regions' elite high schools, and their students typically had higher test scores and socioeconomic backgrounds than students at switching schools.

The calendar shift caused students at both switching and staying schools to experience an academic break between high school and college. Colombian colleges typically offer programs in both January and September, so most students can start college right after graduation regardless of their high school's schedule. At switching schools, however, the calendar transition caused the 2009 cohort to graduate just after the start of the September college term, and thus students had to wait until the next semester to enroll. Colleges in the affected regions also responded to the new academic calendar by moving some programs from a September to a January start date. This affected the post-2009 cohorts at staying schools because students who were interested in a college program that changed calendars had to wait an extra semester to enroll.

We use administrative data on high school graduates in both affected and other regions to examine the college and labor market effects of these academic breaks. We match data from Colombia's high school exit exam—which includes nearly all high school graduates in the country from 2002–2011—to records from the higher education census and social security ministry. This allows us to observe enrollment at almost all Colombian colleges through 2012 and formal sector earnings in 2017. Our identification strategy is a synthetic control approach (Abadie et al., 2010), which matches high schools in the affected regions to schools in other regions with similar pre-transition college enrollment patterns.

We first show that the calendar shift sharply reduced the number of students who started college right after high school. Relative to comparison schools, the *immediate* college enrollment rate fell by about five percentage points at both switching and staying schools. The decline in immediate enrollment is the first stage for our analysis, as it shows that some students in the affected cohorts experienced an academic break before potential college entry.

We find that many of the affected students never enrolled in college after the academic break. The students who experienced a break as a result of the calendar shift could have enrolled in college one semester later. At both switching and staying schools, however, our estimates suggest that only about half of the affected students began college over the next few years. Individuals with disadvantaged academic and socioeconomic backgrounds were more likely to forgo college after the time gap. Although we can only observe college entry within four years of the transition, we find that catch-up enrollment occurs only in the first two years. This suggests that the academic break caused a permanent decrease in college attendance rates. We also present evidence against other potential explanations for the enrollment decline, including changes in student preparation or international enrollment.

Lastly, we show that the labor market effects of the academic break differed significantly between switching and staying schools. At staying schools, the cohorts affected by the calendar shift were less likely to be working in the formal sector seven years later, and they earned five percent less per month conditional on employment. In combination with the enrollment effects, these estimates suggest that the affected students at staying schools would have earned twice as much on average had they attended college.

At switching schools, however, we find little difference in labor market outcomes between students who experienced the time gap and comparison students in other regions. In the affected cohort at switching schools, we observe at most a small decrease in formal employment rates in 2017, and no systematic difference in formal sector earnings. In addition, the calendar shift caused only a small decrease in college *persistence* rates, which suggests that many of the affected students would have dropped out of college had they enrolled. Although our data only include early-career earnings, these results suggest that the affected students at switching schools would have had lower returns to college than those at staying schools, consistent with their lower academic preparation.

In sum, our results show that academic breaks can cause individuals to forgo enrolling in college, but that the affected individuals can vary significantly in their returns to further schooling. This shows that time away from school can increase the perceived or opportunity costs of education for some individuals for whom college was likely to be a good investment. On the other hand, academic breaks may also benefit other individuals with relatively low returns by sparing them the time and financial costs of college.

Our paper relates to work on the effects of delayed college enrollment. This work has shown that disadvantaged students are more likely to postpone college (Rowan-Kenyon, 2007; Wells and Lynch, 2012), and that delayed enrollment is associated with lower completion rates (Bozick and DeLuca, 2005). Our paper is most similar to research that asks how breaks after high school affect the probability of enrolling in college at all. This includes work on deferred university admission (Perez-Arce, 2015) and on the "summer melt"—a phenomenon in which students change their minds about attending college during the summer after high school (Castleman and Page, 2014). Our results corroborate these studies in showing that academic breaks can decrease educational attainment, but our paper is distinct in examining the earnings impacts of forgoing college.

Our paper also contributes to work on small costs that can prevent individuals from going to college (Lavecchia et al., 2016). These college barriers include financial aid forms (Bettinger et al., 2012), standardized test access and fees (Bulman, 2015; Pallais, 2015), and complexities in admissions or aid (Avery and Kane, 2004; Dynarski and Scott-Clayton, 2006; Hoxby and Turner, 2013; Dynarski et al., 2018). Our paper highlights another seemingly small cost—short academic breaks before college—that is especially important in developing countries. Most papers in this literature are unable to examine labor market outcomes for individuals affected by these costs. We show that individuals who forgo college after academic breaks can vary significantly in their returns to further schooling. This is consistent with work that finds substantial heterogeneity in the returns to college (Willis and Rosen, 1979; Carneiro et al., 2011; Barrow and Malamud, 2015), and it suggests that some individuals may benefit from an academic break by learning about their returns (Jensen, 2010; Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2015).

Lastly, our paper relates to work on the mechanisms underlying socioeconomic gaps in college enrollment (Bailey and Dynarski, 2011; Malamud and Pop-Eleches, 2011; Hoxby and Avery, 2013; Black et al., 2015; Page and Scott-Clayton, 2016). We show that students from disadvantaged backgrounds are more likely to take an academic break after high school, and also more likely to forgo college after a break. Thus, policies that encourage students to start college right after high school may help reduce socioeconomic gaps in college enrollment (Castleman and Page, 2015; Carrell and Sacerdote, 2017). If the goal is to reduce inequality in labor market outcomes, however, our results stress the importance of policies that address gaps in academic preparation before students reach the end of high school (Riehl, 2019).

The paper proceeds as follows. Section 2 describes the prevalence of academic breaks after high school and the mechanisms through which they can affect college enrollment. Section 3 describes how the Colombian calendar shift led many students to experience an academic break after high school. Section 4 shows how the time gap affected college enrollment rates, and Section 5 presents its effects on labor market outcomes. Section 6 concludes.

2. Academic breaks between high school and college

Many students continue directly onto college after high school, but it is also common to take an academic break before starting college. The prevalence of academic breaks can be seen by comparing *immediate* and *overall* college enrollment rates. The immediate enrollment rate is the fraction of high school graduates who enter college exactly one semester after graduating. The overall enrollment rate measures the fraction who ever attend college.

Figure 1 measures immediate and overall college enrollment using panel data from the U.S. and Colombia. The dashed line depicts 1995–2002 high school graduates in the U.S. National Longitudinal Survey of Youth 1997 (NLSY). The x-axis is the number of years since high school graduation, and the y-axis shows the fraction of individuals who had ever enrolled in college by that year. The immediate college enrollment rate is slightly below 60 percent for NLSY graduates, as indicated by the data point at 0.5 years after graduation. Overall enrollment rises to nearly 80 percent after nine years. Thus more than one-quarter of U.S. college enrollees experienced an academic break before starting.

Academic breaks are even more prevalent in Colombia. The solid line in Figure 1 depicts college enrollment for 2002 high school graduates in Colombia using administrative data described below. College enrollment is less common overall in Colombia than in the U.S., but immediate enrollment rates are especially low. Only 14 percent of students started college in the semester after high school graduation, while 44 percent began within nine years. Thus more than two-thirds of college enrollees took an academic break before starting.

Figure 2 shows that academic breaks are more common for the types of students who are less likely to enroll in college at all. We divide the U.S. and Colombian samples into 16 groups based on age, mother's education, and quartiles of academic ability, as indicated in the legend. For each group, the x-axis depicts the overall college enrollment rate, and the y-axis shows the fraction of college students who waited more than one semester to enroll. Panel A shows this relationship in the NLSY, and Panel B uses the Colombian administrative data. In both countries, there is a strong negative relationship between overall and delayed enrollment; the types of students who are less likely to attend college at all are also more likely to delay conditional on enrolling. Both delaying and forgoing college are more common for students with lower test scores and socioeconomic backgrounds, and for older students.[1]

Academic breaks are likely to be more common in developing countries, which tend to have a higher fraction of students who are over-age for their grade. Appendix Figure A1 shows evidence of this pattern using OECD data that includes several Latin American countries. For 38 countries with available data in 2017, there is a negative relationship between GDP/capita and the *variance* of age at tertiary enrollment. This suggests that students in developing countries are more likely to delay college entry, although there is significant residual variation in this relationship.[2]

This paper asks whether academic breaks after high school affect the likelihood that individuals enroll in college at all. In a standard human capital model (Becker, 1964), experiences during an academic break could affect an individual's schooling decision in several ways. Individuals may learn about the earnings potential of their current degree by searching for work or taking a job during the break. Time off can increase the opportunity cost of college for individuals who find particularly well-paying jobs (Perez-Arce, 2015). Academic breaks

[1] We find that gender is not a significant predictor of delayed enrollment in either country.

[2] Other national institutions can affect the prevalence of delayed college enrollment, such as military service requirements or academic tracking (Hanushek and Woessmann, 2006).

may change one's view on the relative disutility of school versus work, or alter one's willingness to wait for future earnings. If the time gap is long enough, it may also shorten an individual's post-schooling career, thus reducing the amount of time to reap the returns to further education. In short, an individual's experiences during an academic break could either increase or decrease their expected net benefits of schooling.

If time gaps do affect individuals' choices on whether to enroll in college, it is important to understand whether the effects are driven primarily by changes in expected costs or expected returns. If academic breaks increase the perceived costs of education, this can prevent individuals from enrolling in college even if they have high returns to doing so. Conversely, individuals may forgo college after a break because they learned that they have relatively low returns. In this case, academic breaks may actually help individuals by sparing them the time and costs of college.

The remainder of this paper exploits a policy change in Colombia that provides an opportunity to examine how academic breaks after high school affect students' college enrollment decisions and subsequent labor market outcomes.

3. An academic calendar shift in Colombia

This section gives background on the high school and college systems in Colombia and our related data sources. It then describes a government policy that altered the academic calendars in two regions, which created an unusual time gap between high school graduation and potential college entry for thousands of students.

3.1. **Academic calendars and college admissions.**

A unique feature of the high school system in Colombia is that schools operate on two different academic calendars. The large majority of schools start the academic year in January and conclude in November. This schedule is common because it gives the longest break during the Christmas season. A small number of private high schools begin in September and finish in June. These schools use this calendar in part because it aligns with that of U.S. and European colleges. Throughout this paper, we refer to these two schedules as the *January* and *September* calendars.

To enter college, Colombian students are required to take a national standardized entrance exam called the ICFES.[3] The ICFES exam is similar to the SAT in the U.S., but its results are also used to evaluate high schools. As a result, nearly all high school graduates take the exam, even if they do not plan to attend college. The exam is offered semiannually near the end of the last year of high school (11^{th} grade) on each calendar. Scores are returned within a few weeks, and students can use them to apply to college for the next semester.

[3] The ICFES exam is now called *Saber 11*, but we use the name that matches the period of our data.

Because there are two high school calendars, Colombian colleges also enroll students twice per year. Nearly all colleges offer enrollment cohorts in both January and September, and aggregate enrollment counts are similar in the two semesters. The Colombian higher education market has many public and private colleges with varying selectivity and degree durations. As in many countries, applicants apply to school/major pairs that we refer to as "programs." There is no centralized admission system; students apply to individual programs, and colleges determine their own selection criteria.

3.2. **Data sources.** This paper uses three individual-level administrative data sources:

(1) Records from the ICFES testing agency on all 11^{th} graders who took the college entrance exam in 2002–2011. These data contain each student's high school, background characteristics, and exam scores.
(2) Ministry of Education records on students who began in college in 2002–2012. These data cover almost all Colombian higher education institutions, and include each student's college, program of study, and dates of enrollment and graduation/drop-out.
(3) Earnings records from the Ministry of Social Protection for the year 2017. These data contain monthly earnings for any individual in the other two datasets that worked at a firm registered with the Ministry.

We merge these datasets using national ID numbers, birth dates, and names. This merge defines our main measure of college enrollment, which is an indicator for appearing in the Ministry of Education records. Appendix B.1 describes the merge and match rates, and Appendix B.2 gives details on the institutions covered by the Ministry of Education records.

We also use data on labor force participation by age and region from a monthly Colombian household survey called the *Gran Encuesta Integrada de Hogares* (GEIH).

3.3. **An academic calendar shift in two regions.** Almost all public high schools in Colombia start the academic year in January, but in two regions the public school year historically began in September. These two regions are Valle del Cauca and Nariño, which are the third and eighth largest of Colombia's 33 administrative regions. The capital of Valle del Cauca, Cali, is the country's third most populous city. We call these two regions the *affected regions*. Anecdotally, public schools in the affected regions used the September calendar because private schools developed first and adopted this schedule.

In 2008, Valle del Cauca and Nariño transitioned public schools to the January calendar to be consistent with all other public schools in the country. This change was motivated by a desire to align the public academic calendar with the federal fiscal year. The transition occurred over two years by adding extra instructional periods and mid-term holidays, as we describe further below. The academic calendar shift was complete by January 2011.

Although the policy affected public high schools, some local private schools also changed from the September to the January calendar. Other private schools chose not to change schedules. We use the term *switching schools* to refer to the public and private schools that switched calendars. We use *staying schools* to refer to the private schools that stayed on the September calendar. Our analysis excludes high schools that did not exist for all years of our data (2002–2011). We also omit any school that ever operated on a "flexible" calendar, in which students can begin the school year in either semester. Appendix B.3 provides details on our sample of high schools.

Table 1 describes the schools in our sample. Columns (A)–(C) include switching and staying schools in the affected regions, and columns (D)–(E) include public and private schools in other regions. Switching schools include 315 public schools and 114 private schools; in total they had over 26,000 graduates per year in 2002–2011. All 94 staying schools are private, and they graduated roughly 4,000 students per cohort.

Graduates from staying schools were significantly more advantaged than those from switching schools as measured by institution and individual characteristics. 97 percent of staying schools received the exam agency's high or superior rank in any year, compared with half of switching schools. Staying schools were more likely to offer academic as opposed to technical-level education. The average student at staying schools scored at the 74^{th} percentile on the ICFES exam, and 64 percent of students had mothers who attended college. Switching school graduates scored near the median of the ICFES exam on average, and fewer than one-quarter had college educated mothers. The differences between switching and staying schools were similar to the disparities between public and private schools in other regions.

Graduates from staying schools were also much more likely to enroll in college. In the 2002–2006 cohorts, 30 percent of staying school graduates started college one semester after taking the ICFES exam, and the enrollment rate rose to 58 percent after six years. Immediate and overall college enrollment rates were roughly half of these magnitudes in switching schools.

The last two rows of Table 1 show mean labor market outcomes in 2017. Slightly more than half of high school graduates were employed in the formal sector, defined as working at a firm that is registered with the Ministry of Social Protection. Conditional on formal employment, graduates earned roughly 5,000 U.S. dollars in 2017 on average. Both employment rates and earnings were significantly higher in staying schools and in private schools in other regions.

The academic calendar shift caused students at both switching schools and staying schools to experience an academic break between high school graduation and potential college entry, but for different reasons. The next two sections describe the causes of this break.

3.4. **An academic break at switching schools.** One cohort at switching schools experienced an unusual academic break because the calendar transition delayed their graduation.

Figure 3 shows the transition from the September to January calendars for the 2007–2012 cohorts at switching schools.[4] The grey bars depict instructional periods in the last year of high school. Prior to 2009, students began the year in September, took the ICFES exam in April/May, and graduated in June. They were eligible to begin college in September of their graduation year, as indicated by the white boxes.

Graduates in the 2009 cohort started senior year and took the ICFES exam on the typical schedule, but they finished high school three months later than usual. This occurred through one extra month of classes and two additional mid-term breaks. 2010 graduates also had an extra month of instruction, but their school year began and ended several months later. The transition to the January calendar was complete by 2011.

Figure 3 shows that the transition created an unusual four month gap between high school and potential college entry for the 2009 cohort at switching schools. This cohort graduated just after the start of the September college semester, so many students could not begin college until the following January. Below we measure college enrollment relative to the date of the ICFES exam since the 2009 cohort took the test at the same time as in previous cohorts. This allows us to measure exposure to the academic break by computing the fraction of students who began college one semester after the exam. We also consider other potential transition effects such as changes in instruction time.

3.5. **An academic break at staying schools.** Some students at staying schools also experienced a time gap after graduation because *colleges* responded to the public calendar transition. In Colombia, many college programs are offered semiannually, but some enroll students only once per year. Prior to 2009, most annual programs in the affected regions were offered in September because this aligned with the academic calendars of nearly all local high schools. With the calendar shift, most students now finished in November, and thus colleges in the affected regions shifted some annual programs to the January cohort.

Figure 4 illustrates this shift in college program timing. Panel A plots the number of college programs offered in the September cohort. The solid line includes programs at colleges in the affected regions, and the dashed line includes college programs in other regions. The y-axis shows the number of programs offered in each year relative to the supply of programs in 2008. In both areas, the number of September programs increased by roughly 50 percent from 2002–2008, which reflects both the opening of new colleges and new majors at existing colleges. From 2009–2011, however, the number of September programs declined in the affected regions, falling behind program growth in other regions.

[4] Figure 3 shows academic calendars from public resolutions by the government in Cali, Valle del Cauca. Resolutions from the region of Nariño show a similar transition. See Appendix B.4 for details.

Panel B shows that the decline in September programs in affected regions occurred because colleges shifted some programs to the January calendar. This panel plots the total number of programs offered in *both* the September and January cohorts. The two areas exhibit similar growth in the total number of programs in 2009–2011. Thus colleges in the affected regions altered the timing of some programs without a significant change in the annual quantity.

The shift from September to January programs affected the timing of college entry for the 2009–2011 cohorts at staying schools. These cohorts finished high school in June, so individuals had to wait an extra semester if they wanted to apply to a program that switched to January enrollment. Thus some staying school students experienced a six month academic break before potential college entry, induced in this case by changes in college calendars.

3.6. **Effects on immediate college enrollment.** This section shows that the calendar transition reduced the fraction of students who began college immediately after graduation at both switching and staying schools. This is the first stage for our empirical analysis, as it shows that some students experienced an academic break after high school graduation.

Figure 5 plots the immediate college enrollment rate for graduates in the affected regions. The x-axis depicts students' graduation cohorts, or equivalently, the year they took the ICFES college entrance exam. The y-axis shows the fraction of students who enrolled in college one semester after taking the entrance exam. The solid line includes graduates from switching schools (columns (A)–(B) in Table 1). The dashed line depicts graduates from staying schools (column (C) in Table 1).

Figure 5 shows that the calendar shift reduced immediate college enrollment at both switching and staying schools. At switching schools, the fraction of students who enrolled one semester after the entrance exam rose from nine to 13 percentage points in 2002–2008, and then dropped by five percentage points in the 2009 graduation cohort. This aligns with the time gap in Figure 3; many 2009 graduates at switching schools did not finish high school until after the start of the September college semester.[5] At staying schools, immediate enrollment also increased from 2002–2008, and then fell over the 2009–2011 cohorts. This matches the timing of the college program shift in Figure 4, which suggests that some graduates had to wait to enroll in certain programs.

In sum, the calendar shift created an academic break between high school graduation and potential college entry for some students in the affected regions. The next section asks how this time gap impacted the likelihood that these students enrolled in college at all.

[5] The immediate college enrollment rate did not fall to zero in the 2009 cohort at switching schools, which suggests that some students or schools did not follow the academic calendar depicted in Figure 3.

4. Academic breaks and college enrollment

This section shows that the academic break after high school caused many students in the affected regions to forgo enrolling in college. We first describe our empirical specification and present the main effects on college enrollment. We then consider alternative explanations and argue that the results are most likely driven by students' extra time out of school. We show that the academic break had a larger impact on students from disadvantaged academic and socioeconomic backgrounds. Lastly, we provide evidence that the enrollment declines are driven by increases in labor force participation during the break.

4.1. **Empirical specification.** To examine how the academic break affected college enrollment, we use the differences in differences regression

$$y_{ihc} = \gamma_h + \gamma_c + \beta \text{Transition}_{hc} + \epsilon_{ihc}. \tag{1}$$

The dependent variable, y_{ihc}, is a college enrollment outcome for individual i in high school h and cohort c. Cohorts are defined by the year of graduation, or equivalently, the year that individuals took the ICFES college entrance exam.[6] The regression includes high school dummies, γ_h, and cohort dummies, γ_c. The variable of interest, Transition_{hc}, is an indicator for students who were potentially exposed to an academic break as a result of the calendar transition. Following Section 3, this variable is equal to one for the 2009 cohort at switching schools and the 2009–2011 cohorts at staying schools. Transition_{hc} equals zero for all other cohorts in the affected regions, and for all graduates from high schools in other regions.

A key implementation choice is selecting high schools in other regions that serve as good comparisons for switching and staying schools. For this we follow work on synthetic controls (Abadie et al., 2010) by matching high schools based on their pre-transition college enrollment rates. We adapt these methods to address the fact that there is a large set of potential high schools for both the "treatment" and "control" groups.

We begin by selecting samples of high schools in other regions that have similar characteristics to switching and staying schools. For switching schools, our control group includes public high schools in other regions (column (D) in Table 1). Our comparison group for staying schools includes private high schools in other regions (column (E) in Table 1).

We then compute synthetic control weights so that the distribution of high schools in the comparison samples matches pre-transition college enrollment rates in the affected regions. To compute these weights, we first calculate the college enrollment rate in two cohort groups at each school: 2002–2003 graduates, and 2007–2008 graduates. These are the earliest and

[6] We define cohorts using *academic* years; for example, the 2009 cohort includes students who took the ICFES exam in fall 2008 or spring 2009. This allows us to measure enrollment effects for an extra semester since most students in other regions take the ICFES exam in the fall.

latest pre-transition cohorts that we observe in the data. Next, we form groups of high schools $g \in \{1, \ldots, 25\}$ that appear in the same quintiles of both 2002–2003 and 2007–2008 enrollment rates. Lastly, we compute weights for each school in other regions based on the distributions of groups g in both areas. These weights equal the proportion of affected region graduates that are in group g, divided by the proportion of graduates in other regions that are in group g. These weights rebalance the sample of other region schools so that it matches the distribution of college enrollment rates in affected schools in both 2002–2003 and 2007–2008.[7] Appendix Tables A1–A2 provide details on this synthetic control procedure and show the resulting weights for comparisons schools.

We estimate equation (1) separately for switching and staying schools with observations in other regions weighted by these weights. Our dependent variables y_{ihc} measure college enrollment at different lengths of time since individuals took the ICFES exam. College enrollment is an indicator for attending any institution in the Ministry of Education dataset. The coefficient of interest, β, describes how enrollment rates changed in the transition cohorts of affected regions relative to the same cohorts in other regions. The main identification assumption is that outcomes would have evolved similarly in affected and other regions absent the calendar shift. To test this assumption and the validity of our synthetic control approach, we show event study graphs that plot β_c coefficients for each cohort c. We also estimate specifications that include a linear cohort trend for each region r (i.e., $\gamma_r \times c$ terms).

We cluster standard errors at the region level because identification comes fundamentally from regional policies. Colombia has 33 administrative regions, which raises potential concerns about a small number of clusters. To address this, Appendix Tables A4–A5 present results from region-cohort level regressions that use a t-distribution for inference. This approach follows Donald and Lang (2007), who recommend "between-group" estimators in settings with a large number of observations per cluster but few treated clusters (see also Cameron and Miller, 2015). These regressions yield larger standard errors but do not measurably affect the patterns of statistical significance for our main results.

4.2. **Effects on college enrollment.** This section shows that exposure to an academic break after high school caused many students to forgo college for at least the next few years.

Column (A) in Table 2 shows the effects of the academic calendar transition at switching schools. Each coefficient is an estimate of β from a separate regression (1) using the dependent variable listed in the row header. The sample includes 2002–2009 graduates from switching schools and public schools in other regions, with observations in other regions weighted by the synthetic control weights.

[7] In other words, our synthetic control method constructs a comparison group of schools that matches both the level of college enrollment and its trend in the pre-transition periods.

The first row of column (A) shows the proportion of switching school graduates who experienced an academic break. The dependent variable in this regression is an indicator for enrolling in college one semester after taking the ICFES entrance exam. Immediate college enrollment fell by five percentage points in the 2009 cohort at switching schools relative to comparison schools. This is consistent with a time gap from the calendar transition (Figure 3), and with the enrollment patterns in Figure 5.

Students who experienced an academic break could have started college in the following January, but the remaining rows in column (A) show that many did not enroll. These regressions use dependent variables that measure enrollment up to four years after the ICFES exam, which is the maximum length of time we can observe for all cohorts. In the 2009 cohort, the enrollment gap between switching and comparison schools fell by about 50 percent over the next two years and then flattened out. This suggests that roughly half the students who experienced an academic break did not subsequently enroll in college. Four years later, the enrollment rate for the 2009 cohort at switching schools was almost three percentage points lower than in comparison schools. This is roughly an eight percent decline relative to the pre-transition enrollment rate (Table 1).

Panels A–B in Figure 6 plot event study coefficients that correspond to the results in column (A) of Table 2. These graphs depict β_c coefficients that we estimate by interacting Transition_{hc} with dummies for graduation cohorts. We omit the 2008 cohort dummy, so the coefficients measure the difference in enrollment rates between affected and other regions in cohort c relative to that in the 2008 cohort. Panel A shows that immediate enrollment trended similarly in affected and unaffected regions from 2002–2008, and then fell sharply in the 2009 cohort at switching schools. Panel B shows a similar pattern for college enrollment measured four years after the ICFES exam, with a smaller but significant longer-term effect.

Column (B) of Table 2 shows that the enrollment results for switching schools are robust to the inclusion of region-specific cohort trends. These regressions are similar to those in column (A), but we include a linear term for graduation cohort interacted with dummies for each of Colombia's 33 administrative regions. Including cohort trends increases standard errors but does not measurably change point estimates. This helps to validate our synthetic control approach, and shows that outcomes for the 2009 cohort were a clear deviation from pre-transition trends.

Columns (C)–(D) in Table 2 show how the calendar transition affected college enrollment at staying schools. These regressions are similar to those in columns (A)–(B), but the sample includes 2002–2011 graduates from staying schools and private schools in other regions. Column (C) estimates equation (1), and column (D) adds region-specific cohort trends.

Exposure to an academic break also reduced college enrollment rates at staying schools. In our benchmark specification (column (C)), the calendar shift reduced the immediate college enrollment rate by seven percentage points in the 2009–2011 cohorts at staying schools. This is consistent with students waiting to enroll in college programs that switched to the January calendar (Figure 4). Staying school students would have been able to enroll in these programs within a year of graduation, yet two years later, the enrollment rate was still five percentage points lower in the affected cohorts. This suggests that the majority of the affected students did not enroll in college, leading to an eight percent decline in the pre-transition enrollment rate. The point estimates are slightly smaller but still significant in regressions with cohort trends (column (D)).

Panels C–D in Figure 6 display the event study coefficients that correspond to column (C) of Table 2. Panel C shows that immediate college enrollment rates at staying schools fell sharply beginning in the 2009 cohort, consistent with the timing of the college program shift in Figure 4. Panel D shows that enrollment rates recovered slightly after two years, but were still substantially lower in staying schools than in comparison schools.

Columns (E)–(F) in Table 2 replicate columns (C)–(D) excluding the 2011 cohort, which allows us to measure enrollment outcomes for an extra year. The estimated enrollment effects are lower in magnitude since the 2011 cohort experienced the largest effects of the college program shift (Figure 4). But there is little evidence that enrollment rates in the affected cohorts caught up to those at comparison schools between the second and third post-graduation years. This suggests that the academic break likely caused a permanent decrease in college enrollment rates at staying schools.

4.3. **Alternative hypotheses.** A potential concern in attributing the enrollment decline to the academic break is that students may have been affected by other elements of the calendar transition. At switching schools, for example, the 2009 school year had fewer instructional days prior to the date of the ICFES college entrance exam (Figure 3). This may have reduced students' likelihood of taking the test or impacted their performance.

Appendix Table A3 shows that the calendar transition did not significantly affect the number of ICFES exam takers or their test scores. This table estimates regressions similar to equation (1) with the number of exam takers and their scores as dependent variables. In the transition cohorts, the total number of exam takers did not change significantly in affected regions relative to other regions. With 95 percent confidence, these regressions also rule out effects on mean ICFES exam scores of more than two percentile points at both switching and staying schools.[8] Further, changes in college preparedness are unlikely to explain the results at staying schools because they did not change academic calendars.

[8] Appendix Table A3 also shows that there is no systematic evidence of changes in other student characteristics that could explain the college enrollment declines in Table 2.

Another potential explanation for our results is that the academic break may have caused some students to go abroad for college. Some students—particularly those at staying schools—may have preferred to immediately enroll in an international college rather than wait to go to a school in the affected regions. Since our data only include Colombian colleges, this behavior would look like a decline in college enrollment in our analysis.

There are several reasons why international enrollment is unlikely to have a significant impact on our results. OECD data from 2012 show that only 1.6 percent of Colombian tertiary students were enrolled abroad, and this figure includes both undergraduate and masters students (OECD, 2014, Table C4.5).[9] It would therefore take a dramatic increase in the propensity to study abroad to fully explain our results, which are equivalent to an eight percent decline in the college enrollment rate. Further, students in the affected regions could also have attended colleges in other parts of the country, which would typically be less costly than studying abroad. Appendix Table A6 shows no evidence that affected students were more likely to enroll in colleges outside of their region. Lastly, below we show that the enrollment declines were larger for students with lower socioeconomic status and lower ability, who are less likely to consider studying abroad. Thus changes in international enrollment are unlikely to explain our results.

We also consider the possibility that the enrollment effects were driven by changes in college admission rates. The calendar shift affected the timing of potential college entry for thousands for students in the affected regions as the majority of schools moved to the January calendar. Colleges also responded by moving some programs to the January calendar (Figure 4). Both of these changes may have affected admission rates to some college programs.

Changes in admission rates cannot fully explain the enrollment effects at switching schools because we also observe a decrease in the likelihood that students enrolled in non-selective colleges. The top colleges in the affected regions are selective, but there are a large number of colleges that are open enrollment conditional on ability to pay and minimum ICFES test scores. Appendix Table A7 shows that switching school graduates in the affected cohorts were less likely to enroll in *both* selective and non-selective colleges, with slightly larger effects for non-selective colleges. These effects are unlikely to be attributable to changes in the probability of admission since these colleges rarely reject students.[10]

Admission effects may have been important for graduates from staying schools, but if anything, these students' admission chances likely increased as a result of the calendar shift.

[9] Roughly half of Colombians who studied in the U.S. in 2015 were in graduate programs (IIE, 2019). Thus the fraction of Colombian *undergraduates* who enrolled abroad in 2012 was likely less than 1.6 percent.

[10] At staying schools, the enrollment effects were driven primarily by reduced enrollment in selective colleges (see Appendix Table A7). This reflects the fact that staying school graduates were *ex ante* more likely to enroll in selective colleges. Further, the college programs that shifted to the January calendar (Figure 4) were mostly programs in specialized subjects that are offered only once per year by selective colleges.

Prior to the transition, nearly all students in the affected regions graduated from high school in June. From 2009–2011, the large majority of students in these regions moved back to a November graduation date, which left staying school students with a much smaller graduation cohort. The number of September college programs fell slightly during these years (Panel A in Figure 4), but not by nearly the same magnitude as the decrease in cohort size. Thus competition for immediate enrollment in college programs likely decreased for graduates from staying schools. Furthermore, Panel B in Figure 4 suggests that the total quantity of college programs was unaffected by the transition, so there was little change in the ratio of graduates to college slots at an annual level. Thus enrollment effects are more likely to be driven by changes in program timing than changes in competitiveness.

In sum, the pattern of results suggests that the decreases in college enrollment at switching and staying schools was most likely driven by the academic break after high school.

4.4. **Heterogeneity in responsiveness to the academic break.** This section shows that academic breaks had a greater impact on the college enrollment decisions of students from disadvantaged backgrounds.

We examine heterogeneous effects using an index of college enrollment propensity based on student characteristics. To compute this index, we regress an indicator for enrolling in college within six years of graduation on a large vector of individual covariates in a sample of pre-transition cohorts (2002–2006). These covariates include fixed effects for gender, deciles of ICFES exam scores, age in years, and individuals' high schools.[11] Following Abadie et al. (2018), we use a leave-one-out estimator for this index to reduce bias from in-sample stratification based on an outcome variable. We use the estimates from this regression to compute the predicted probability of college enrollment for all individuals in our sample. Appendix Table A8 shows the covariates for our college propensity index and their coefficients.

Table 3 displays summary statistics by quartile of the college propensity index for the 2002–2006 cohorts. We define quartiles separately for switching and staying schools (and their comparison groups) so that each quartile has the same number of students within our two samples. In both the switching and staying school samples, students in the top quartile had higher ICFES exam scores, more educated mothers, and were more likely to finish high school at the on-time age. Top quartile students were also more likely to be working in the formal sector in 2017, and they earned nearly twice as much as students in the bottom quartile. However, students in the same quartiles at switching and staying schools differ dramatically. For example, staying school graduates in the bottom quartile have mean characteristics that are similar to switching school graduates in the third quartile.

[11] The data do not include an individual measure of socioeconomic status (SES) for all cohorts, but our index is highly correlated with SES because school choice in Colombia is strongly related to family background.

Table 4 shows effects of the academic break on college enrollment by quartile of the college propensity index. This table displays β coefficients from equation (1) estimated separately for each quartile. Columns (A)–(C) present results for the 2002–2009 cohorts at switching schools, and columns (D)–(F) show results for the 2002–2011 cohorts at staying schools.

At switching schools, the calendar transition had a larger effect on immediate enrollment for graduates with higher college-going propensities. In column (A) of Table 4, the dependent variable is an indicator for enrolling in college one semester after the ICFES exam. The calendar shift reduced the probability of immediate enrollment by more than eight percentage points for switching school graduates in the top quartile of college propensity, and by less than two percentage points for those in the bottom quartile. These effect sizes are similar to the variation in mean pre-transition enrollment rates across quartiles, suggesting that the time gap had roughly proportional effects on immediate enrollment.

Among students who experienced one semester break, however, those with lower college propensities were more likely to forgo enrolling over the next four years. Column (B) shows that the transition reduced the four-year college enrollment rate by less than four percentage points for graduates in the top quartile of college propensity. This suggests that more than half of students in the top quartile who were exposed to the time gap ultimately enrolled in college. By contrast, the magnitudes of the immediate and four-year enrollment effects are similar for students in the bottom quartile, suggesting that these students were more likely to forgo college after the break. We illustrate this in column (C), which shows the ratios of the coefficients in columns (B) and (A).[12] The effects of the academic break on college enrollment decrease monotonically across quartiles of college propensity.

Columns (D)–(F) of Table 4 show similar patterns of responsiveness to the time gap at staying schools. The calendar transition had roughly equal effects on immediate enrollment across quartiles of the college propensity index (column (D)), but graduates in the bottom two quartiles were significantly more likely to forgo college for the next two years (column (E)). Column (F) shows the ratios of the coefficients in columns (E) and (D), which suggest that about 80 percent of students in the bottom two quartiles who experienced an academic break did not subsequently enroll. By contrast, about half of affected students in the top two quartiles subsequently enrolled in college.

The results in Table 4 suggest that academic breaks after high school have a greater influence on individuals who are on the margin of whether or not to attend college. In our data, these marginal students are older, more disadvantaged, and have lower exam scores. This suggests that academic breaks may have a compounding effect on socioeconomic gaps

[12] Column (C) shows estimates from two stage least squares regressions based on equation (1). The dependent variable is four-year college enrollment, and we use Transition_{hc} as a instrument for immediate enrollment.

in college enrollment; disadvantaged students are both more likely to take time off after high school (Figure 2), and also more likely to forgo college after these breaks.

4.5. **Potential mechanisms.** This section discusses potential mechanisms through which time off after high school may have caused some students to forgo college.

In the human capital model of Becker (1964), academic breaks could alter an individual's schooling decision by changing either her expected returns or her expected costs. It is unlikely that the Colombian calendar shift had a significant effect on individuals' potential earnings with a college education. The transition reduced individuals' future working careers by six months at most, and the corresponding loss in discounted returns is small relative to estimates of the college earnings premium. In Colombia, college graduates earn more than twice as much as high school graduates on average, which is even larger than estimates of the U.S. college earnings premium (Autor, 2014).

The academic break more likely affected individuals' perceived costs of college or their expected earnings with a high school degree. Experiences during the break may have changed individuals' expectations on both of these factors. Students may have searched for a job during the break and learned that their high school degree had higher earnings potential than they anticipated. Job search or time off may have also altered the opportunity costs of a college degree, the perceived effort costs of completing a college degree, or willingness to wait for the earnings returns to college.

Figure 7 provides suggestive evidence on these channels using labor force participation data from a monthly Colombian household survey (GEIH). These graphs show how the academic calendar shift affected the timing of high school graduation and labor force entry. The dashed line in both panels includes individuals in the affected regions who turned 17 years old in the pre-transition years (2007–2008). The solid line includes individuals who turned 17 in the first year of the calendar transition (2009). Age is a noisy measure of graduation cohort in Colombia because many students are behind schedule, but it is predictive enough to detect initial effects of the calendar transition in the survey data.[13]

Panel A shows how the calendar shift affected the timing of high school graduation. The x-axis displays quarters beginning in October before the cohort year, which is the start of the final year of high school for 17 year olds with on-time schooling. The y-axis plots the fraction of individuals in each cohort group with a high school degree. The graduation rate for the 2007–2008 cohorts jumps from 10 to 40 percent in the July quarter, but it does not reach 40 percent until October for the 2009 cohort. This reflects the delayed academic calendar for 2009 graduates in switching schools (Figure 3).

[13] We cannot separate switching and staying school graduates in the survey data, but the results in Figure 7 are likely driven by switching schools because they contained the large majority of students in these regions.

Panel B provides evidence that labor force participation increased during the time gap for the 2009 cohort. This panel is similar to Panel A, but the y-axis plots the fraction of individuals in each cohort group who were either employed or looking for work. In both cohort groups, the labor force participation rate was roughly 20 percent in the months prior to on-time high school graduation. Labor force entry increased as students finished high school, but it was higher in the 2009 cohort than in the 2007–2008 cohorts by the end of the year. October–December was the period between high school graduation and potential college enrollment for many students in the 2009 cohort. This suggests that some 2009 graduates looked for work or took up jobs during the time gap. Labor force participation rates remained higher in the 2009 cohort through the start of the next year, consistent with the longer-term college enrollment effects in Table 2.[14]

Figure 7 provides evidence that some of the affected students engaged with the labor market during the academic break. These experiences may have been beneficial if individuals learned that they had higher-than-expected earnings with a high school degree, and thus relatively low returns to college. On the other hand, labor force participation may have increased the perceived cost of college or its opportunity cost. Unfortunately, we do not have administrative earnings data for all individuals during the transition years, so we cannot directly examine how job attainment affected the college enrollment decision. However, we can examine *ex post* outcomes to see whether individuals who did not attend college would have had high returns to doing so. We now turn to this analysis.

5. Academic breaks and the returns to college

This section examines the returns to college for students affected by the Colombian calendar shift. We show that the affected students at staying schools would have been likely to persist in college had they enrolled, and they had lower earnings seven years later. At switching schools, the affected students would have been more likely to drop out, and the transition had limited effects on labor market outcomes. These results suggest that the academic break caused some students with high returns to college to forgo enrolling, but it also reduced enrollment for individuals with lower returns.

5.1. **Effects on college persistence.** This section shows that college persistence rates for staying school students who were affected by the calendar shift would likely have been higher than those for switching school students.

In analyzing the potential returns to college for those affected by the calendar shift, a first question is whether these individuals would have been likely to succeed in college. College

[14] Appendix Table A10 shows that the increase in labor force participation in the affected cohorts is statistically significant in regressions that also include 17 year olds in other regions as a comparison group.

dropout is a major issue in Colombia; roughly 50 percent of college enrollees do not earn a degree. If most of the affected individuals would have dropped out, it is possible that they were better off not having spent time and money on college.

Table 5 presents results on the likely college persistence of students affected by the calendar transition. Each coefficient is an estimate of β from a separate regression (1) using the dependent variable listed in the first column. In the top row, the dependent variables are indicators for enrolling in college within one or two years of the ICFES exam, as shown in the column header. This replicates estimates from Table 2. The dependent variables in the other rows are indicators for enrolling by that year *and* remaining in college for at least 0.5–2.5 years. If students affected by the academic break were likely to have persisted in college, the magnitudes of the enrollment and persistence effects should be similar. If most affected students would have dropped out anyway, these coefficients should tend toward zero, leaving no differential change in college persistence rates between affected and other regions.

Column (A) in Table 5 suggests that more than half of the affected students at switching schools would have dropped out of college. The first row shows that the academic break caused a 3.8 percentage point decline in the probability of enrolling in college within one year, which repeats the estimate from column (A) of Table 2. The second row shows that the probability of enrolling *and* staying beyond one semester was only three percentage points lower in the affected cohort. The magnitude of the persistence effect 2.5 years after college entry is less than 50 percent of the initial enrollment effect. Although we cannot observe persistence outcomes beyond 2.5 years in our data, this result suggests that more than half of the students who did not enroll after the time gap would have likely dropped out of college.

Column (B) shows a similar rate of expected persistence for switching school students who did not enroll within two years after the academic break. The time gap caused a 2.7 percentage point decrease in the probability of starting college within two years, and a 1.5 percentage point decline in the likelihood of enrolling and persisting beyond three semesters. The persistence rate given enrollment within two years is similar to that for enrollment within one year. This suggests that for switching school students, the effect of the academic break on college *graduation* was likely less than half of the enrollment effect.

Columns (C)–(D) in Table 5 show that the calendar shift had a much larger effect on college persistence rates at staying schools. These regressions include only 2002–2010 cohorts at staying and comparison schools because we cannot observe college persistence for 2011 graduates. The first row in column (C) shows that exposure to the academic break reduced the probability of starting college within one year by four percentage points at staying schools. The magnitude of this effect is slightly *larger* for college persistence outcomes; the probability of enrolling and staying beyond 1.5 years was 4.3 percentage points lower in the affected cohorts. Column (D) shows a similar pattern of persistence after enrolling within two years, but we can only observe persistence beyond one semester for this group. These results suggest that, if anything, staying school graduates who did not enroll in college because of the academic break would have been *more* likely to persist than the typical college enrollee.

5.2. **Effects on labor market outcomes.** This section shows that staying school students affected by the calendar shift had lower formal employment rates and earnings seven years later. The labor market effects for switching school students are mixed and smaller in magnitude. This suggests that the affected students at staying schools would have had large positive returns to college, while switching school graduates would have had smaller returns.

We examine the labor market effects of the calendar shift using data from the Ministry of Social Protection for the year 2017. These records provide monthly earnings for any individual in our other datasets who worked at a firm that was registered with the Ministry. Importantly, the data do not cover individuals working in the informal sector, which is a substantial portion of the Colombian labor market. Part of the return to a college education in Colombia may include access to formal sector firms.[15] We therefore examine effects on both formal employment—defined as appearing in the Ministry's data—and on earnings. Formal employment is likely to be associated with higher earnings, as mean hourly wages are roughly 50 percent higher in the formal sector.[16] Below we discuss how the absence of data on informal earnings could alter our conclusions.

It is also important to note that our data only allow us to measure early-career earnings for the affected cohorts. The on-time duration for most university-level programs in Colombia is five years, and many students take longer to graduate. For the high school cohorts affected by the calendar shift (2009–2011), students who went on to complete college were either recent graduates or still enrolled in 2017. Many Colombian students work part-time during college, and enrolling in college alone often improves access to formal sector firms. This suggests that our results are likely to understate the returns to college for those who enrolled.[17]

With these factors in mind, Table 6 shows how the calendar transition affected individuals' labor market outcomes. Each coefficient is an estimate of β from equation (1) using the dependent variable in the first column. Our dependent variables include an indicator for formal employment and, conditional on employment, the logs of average monthly and total

[15] In our data, formal sector employment rates for the 2002–2003 exam cohorts are 78 percent for college graduates, 68 percent for college dropouts, and 44 percent for individuals who did not attend college. Individuals who do not appear in our earnings data are either informally employed or out of the labor force.
[16] This wage gap is our calculation using a Colombian household survey (GEIH), where we define formal workers as those who have a contract for their employment.
[17] Note also that we measure labor market outcomes at different levels of potential experience for the 2002–2011 cohorts. Thus identification relies on parallel trends as defined by potential experience profiles.

annual earnings. Columns (A)–(B) present results for switching schools, and columns (C)–(D) show results for staying schools. We estimate the benchmark specification (1) in columns (A) and (C), and we add region-specific cohort trends in columns (B) and (D).

Columns (A)–(B) show that the calendar shift had limited labor market effects for switching school graduates. The formal sector employment rate declined by roughly 1.5 percentage points at switching schools relative to comparison schools from a base of about 50 percentage points (Table 1). This estimate is similar with and without linear cohort trends. The transition effects on monthly and annual earnings are two percent in magnitude at most, and the signs and statistical significance vary across specifications.

Panels A–B in Figure 8 show event study graphs for the labor market effects at switching schools. The difference in formal employment rates between switching and comparison schools fell by about two percentage points in the 2009 cohort (Panel A). This was a deviation from the trend in previous cohorts, which suggests a small causal effect of the academic break on the probability of formal employment. There is little evidence that the calendar transition affected annual earnings conditional on employment (Panel B).

By contrast, columns (C)–(D) in Table 6 show that the calendar shift had large negative effects on labor market outcomes for staying school graduates. Formal employment rates declined in the 2009–2011 cohorts at staying schools relative to comparison schools, although the magnitude of the effect falls significantly with the inclusion of region-specific trends. Among individuals with formal sector jobs, average monthly earnings fell by about five percent in the affected cohorts, and annual earnings decreased by roughly ten percent.

Panels C–D in Figure 8 present event study graphs for the labor market effects at staying schools. Panel C shows that switching and comparison schools had significantly different pre-trends in formal employment rates. Thus although formal employment declined further in the 2009–2011 cohorts at staying schools, it is less clear that this effect was driven by the calendar transition. By contrast, Panel D shows that annual earnings evolved similarly in staying and comparison schools from 2002–2008, and then fell sharply in the staying school cohorts that were affected by the calendar shift. This is consistent with the fact that the earnings results in Table 6 are more robust to the inclusion of cohort trends.

These results suggest that the affected students at staying schools would have had large returns to enrolling in college. The academic break reduced their monthly earning by about five percent, which is similar in magnitude to the college enrollment and persistence effects in Tables 2 and 5. This suggests a return to college of roughly one log point, which is close to the earnings gap between college and high school graduates in Colombian household survey data. The estimated effects on annual earnings are even larger, suggesting that the affected cohorts also became less attached to the labor market. The reduction in formal employment is less clearly attributable to the calendar shift, but if anything, this effect would likely cause us to understate the earnings effects since informal wages are typically much lower.

Conversely, the results in Table 6 and Figure 8 suggest smaller returns to college for affected students at switching schools. There is evidence that the academic break caused a small reduction in formal employment rates, but the earnings effects are mixed and close to zero. These lower returns to college are consistent with Table 5, which suggests that many of the affected students at switching schools would likely have dropped out of college.

5.3. **Heterogeneity in returns.** This section shows that students with higher college propensity had more negative returns to forgoing college after the academic break.

Table 7 shows heterogeneity in the effects of the calendar shift on college persistence and labor market outcomes. This table displays β coefficients from equation (1) estimated separately for each quartile of the college propensity index, defined as in Section 4.4. The dependent variable for each regression is listed in the column header. Columns (A)–(C) present results for switching schools, and columns (D)–(F) show results for staying schools.[18]

Columns (A)–(C) show that the aggregate effects at switching schools mask considerable heterogeneity in labor market returns. In particular, the academic break had large negative effects for students in the top quartile of college propensity. Column (A) shows that the probability of enrolling in college and persisting 1.5 years fell by five percentage points among top quartile students at switching schools. This effect is similar in magnitude to the enrollment effects in Table 4, suggesting that many of these individuals would have been likely to persist in college had they enrolled. Columns (B)–(C) show that top-quartile students in the affected cohort also had lower formal employment rates and earnings. Exposure to an academic break reduced these students' annual earnings by roughly six percent, which suggests that they would have had large positive returns to college.

By contrast, switching school students in the lower quartiles of college propensity had zero or even positive returns to the academic break. The calendar transition had a much smaller effect on college persistence rates for switching school students in the bottom three quartiles (column (A)), and it had little effect formal employment rates (column (B)). In the middle two quartiles, affected students actually had *higher* annual earnings in 2017 (column (C)). Although our data include only early-career earnings, the results suggest that students with a lower college propensity had lower returns to attending college.

At staying schools, exposure to the academic break had negative labor market effects across the range of college propensities. Column (D) in Table 7 shows that the effects on college persistence are slightly larger for students with higher college propensity, but persistence

[18] The regressions in Table 7 include region-specific cohort trends to address the pre-trend in formal employment rates for staying schools (Figure 8).

rates declined in all four quartiles. Column (F) shows that annual earnings also fell across the distribution of student types, with slightly more negative effects in the bottom quartile in this case. These results suggest that most of the affected students at staying schools would have had large positive returns to enrolling in college.

5.4. **Interpretation.** Tables 6 and 7 show significant variation in how the academic break affected individuals' labor market outcomes. This section discusses potential explanations for this heterogeneity.

One explanation for the variation in labor market outcomes is that the affected students differed widely in academic preparation. The results in Table 6 suggest that the affected students at staying schools had higher returns to enrolling in college than those at switching schools. Since staying schools were mostly elite private high schools, their graduates had higher test scores and more advantaged socioeconomic backgrounds on average (Table 1). Staying school students may have been more likely to succeed in college had they enrolled—either for academic or financial reasons. This hypothesis is consistent with the finding that the calendar shift had a larger effect on college persistence rates at staying schools than at switching schools. It is also consistent with the fact that switching school students with the highest college propensity also had negative returns to the academic break (Table 7).

Another potential explanation for this heterogeneity is that the affected students would have enrolled in different types of colleges or programs. Appendix Tables A7–A9 show how the enrollment effects in Table 2 vary by college selectivity and program average earnings. At staying schools, the effects were driven primarily by decreased enrollment in selective colleges and programs with above-median average earnings. At switching schools, however, enrollment declines were larger at non-selective colleges and programs with lower mean earnings. Thus the academic break may have caused staying school students to forgo enrolling in programs with high returns, while many of the affected students at switching schools would have enrolled in programs with lower earnings value added.

Variation in the types of programs that affected students would have attended is consistent with the different causes of the academic break. The staying school students who experienced an academic break were those that wanted to enroll in a college program that shifted to the January calendar. These college programs were mostly speciality majors at selective colleges, which typically have high average earnings. By contrast, the switching school time gap resulted from a delayed graduation date, which was more likely to affect all students regardless of their college preferences.

A final possibility is that the heterogeneity in labor market outcomes is partially driven by data constraints.[19] Our data only include early-career earnings, which would likely cause us

[19] We thank an anonymous referee for highlighting this issue.

to understate the returns to college for those who attended. This would lead to an *upward* bias in our point estimates for earnings, since individuals who did not attend college after the academic break got a jump start on their careers. This bias could be larger for disadvantaged students if those who attended college took longer to graduate, or it could be smaller since they were less likely to persist in college. It is hard to conclusively determine the long-run effects without more data, so it is important to emphasize that our analysis speaks most directly to how the academic break affected individuals' early career paths.

In sum, we draw two conclusions from the results in Tables 6–7. First, an academic break after high school caused some students with high returns to college to forgo enrolling. By 2017, these individuals were already earning less than a comparison group with higher college attendance rates, so it is reasonable to suspect that the earnings gap may only increase with time. Second, the academic break also reduced enrollment rates for some students who would have dropped out, and therefore would likely have had lower returns to college.

6. Conclusion

Education systems vary in the degree to which they permit flexibility in the transition between schooling levels. In many European countries, for example, students are separated into academic or vocational tracks at younger ages, which provides a default postsecondary option at the end of high school (Hanushek and Woessmann, 2006). In the U.S. and Colombia, by contrast, students have more flexibility in choosing which colleges to apply to after high school. More flexible education systems can lead to indecision in the transition from high school to college, and thus create breaks in students' academic careers.

This paper showed that academic breaks after high school can lead some students to forgo enrolling in college altogether. To isolate this channel, it exploited a policy that altered academic calendars in two regions of Colombia, which caused some students to experience a one semester break before potential college entry. This brief time gap led to an eight percent reduction in college enrollment rates in the affected regions, with the largest effects coming from students with disadvantaged academic and socioeconomic backgrounds.

The Colombian calendar shift had heterogeneous effects on individuals' career paths. High-ability students who did not attend college because of the transition had significantly lower earnings than comparison students in other regions, which suggests they would have had high returns to further schooling. The calendar shift had less impact on the early-career earnings of lower-ability students, suggesting that they would have had lower returns to college.

These results show that policies that ease the transition between schooling levels can reduce inequality in college enrollment, but the implications for labor market inequality are more complicated. Policies that clarify academic standards and simplify the application process, for example, may increase immediate college enrollment rates and help to close socioeconomic gaps in college attendance. However, the findings in this paper suggest that some students may benefit from an academic break if it allows them to learn that they have relatively low returns to schooling. To reduce earnings inequality, therefore, it is important to address inequities at earlier schooling levels so that students finish high school with sufficient preparation to succeed in college.

References

Abadie, A., M. M. Chingos, and M. R. West (2018). Endogenous stratification in randomized experiments. *Review of Economics and Statistics 100*(4), 567–580.

Abadie, A., A. Diamond, and J. Hainmueller (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association 105*(490), 493–505.

Autor, D. H. (2014). Skills, education, and the rise of earnings inequality among the 'other 99 percent'. *Science 344*(6186), 843–851.

Avery, C. and T. J. Kane (2004). Student perceptions of college opportunities. the Boston COACH program. In *College choices: The economics of where to go, when to go, and how to pay for it*, pp. 355–394. University of Chicago Press.

Bailey, M. J. and S. M. Dynarski (2011). Gains and gaps: Changing inequality in U.S. college entry and completion. National Bureau of Economic Research.

Barrow, L. and O. Malamud (2015). Is college a worthwhile investment? *Annual Review of Economics 7*(1), 519–555.

Becker, G. S. (1964). *Human Capital: A Theoretical and Empirical Analysis, With Special Reference to Education*. University of Chicago Press.

Bettinger, E. P., B. T. Long, P. Oreopoulos, and L. Sanbonmatsu (2012). The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment. *The Quarterly Journal of Economics 127*(3), 1205–1242.

Black, S. E., K. E. Cortes, and J. A. Lincove (2015). Apply yourself: Racial and ethnic differences in college application. National Bureau of Economic Research.

Bozick, R. and S. DeLuca (2005). Better late than never? delayed enrollment in the high school to college transition. *Social Forces 84*(1), 531–554.

Bulman, G. (2015). The effect of access to college assessments on enrollment and attainment. *American Economic Journal: Applied Economics 7*(4), 1–36.

Cameron, A. C. and D. L. Miller (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources 50*(2), 317–372.

Carneiro, P., J. J. Heckman, and E. J. Vytlacil (2011). Estimating marginal returns to education. *The American Economic Review 101*(6), 2754–2781.

Carrell, S. and B. Sacerdote (2017). Why do college-going interventions work? *American Economic Journal: Applied Economics 9*(3), 124–51.

Castleman, B. L. and L. C. Page (2014). A trickle or a torrent? understanding the extent of summer "melt" among college-intending high school graduates. *Social Science Quarterly 95*(1), 202–220.

Castleman, B. L. and L. C. Page (2015). Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? *Journal of Economic Behavior & Organization 115*, 144–160.

Donald, S. G. and K. Lang (2007). Inference with difference-in-differences and other panel data. *The Review of Economics and Statistics 89*(2), 221–233.

Dynarski, S., C. Libassi, K. Michelmore, and S. Owen (2018). Closing the gap: The effect of a targeted, tuition-free promise on college choices of high-achieving, low-income students. NBER working paper.

Dynarski, S. M. and J. E. Scott-Clayton (2006). The cost of complexity in federal student aid: lessons from optimal tax theory and behavioral economics. *National Tax Journal 59*(2), 319–357.

Hanushek, E. A. and L. Woessmann (2006). Does educational tracking affect performance and inequality? differences-in-differences evidence across countries. *The Economic Journal 116*(510), C63–C76.

Hoxby, C. and S. Turner (2013). Expanding college opportunities for high-achieving, low income students. Stanford Institute for Economic Policy Research Discussion Paper.

Hoxby, C. M. and C. Avery (2013). Missing one-offs: The hidden supply of high-achieving, low-income students. Brookings Papers on Economic Activity.

IIE (2019). Open doors report on international educational exchange. https://www.iie.org/Research-and-Insights/Open-Doors/Data/International-Students/Places-of-Origin.

Jensen, R. (2010). The (perceived) returns to education and the demand for schooling. *The Quarterly Journal of Economics 125*(2), 515–548.

Lavecchia, A. M., H. Liu, and P. Oreopoulos (2016). Behavioral economics of education: Progress and possibilities. In *Handbook of the Economics of Education*, Volume 5, pp. 1–74. Elsevier.

Malamud, O. and C. Pop-Eleches (2011). School tracking and access to higher education among disadvantaged groups. *Journal of Public Economics 95*(11-12), 1538–1549.

Manski, C. F. (1993). Adolescent econometricians: How do youth infer the returns to schooling? In *Studies of Supply and Demand in Higher Education*, pp. 43–60. University of Chicago Press.

OECD (2014). Education at a glance 2014: OECD indicators. http://dx.doi.org/10.1787/eag-2014-en.

Page, L. C. and J. Scott-Clayton (2016). Improving college access in the United States: Barriers and policy responses. *Economics of Education Review 51*, 4–22.

Pallais, A. (2015). Small differences that matter: Mistakes in applying to college. *Journal of Labor Economics 33*(2), 493–520.

Perez-Arce, F. (2015). Is a dream deferred a dream denied? college enrollment and time-varying opportunity costs. *Journal of Labor Economics 33*(1), 33–61.

Riehl, E. (2019). Fairness in college admission exams: From test score gaps to earnings inequality. Working Paper.

Rowan-Kenyon, H. T. (2007). Predictors of delayed college enrollment and the impact of socioeconomic status. *The Journal of Higher Education 78*(2), 188–214.

Stinebrickner, R. and T. R. Stinebrickner (2014). A major in science? initial beliefs and final outcomes for college major and dropout. *Review of Economic Studies 81*(1), 426–472.

Wells, R. S. and C. M. Lynch (2012). Delayed college entry and the socioeconomic gap: Examining the roles of student plans, family income, parental education, and parental occupation. *The Journal of Higher Education 83*(5), 671–697.

Willis, R. J. and S. Rosen (1979). Education and self-selection. *Journal of Political Economy 87*(5, Part 2), S7–S36.

Wiswall, M. and B. Zafar (2015). Determinants of college major choice: Identification using an information experiment. *The Review of Economic Studies 82*(2), 791–824.

Figures

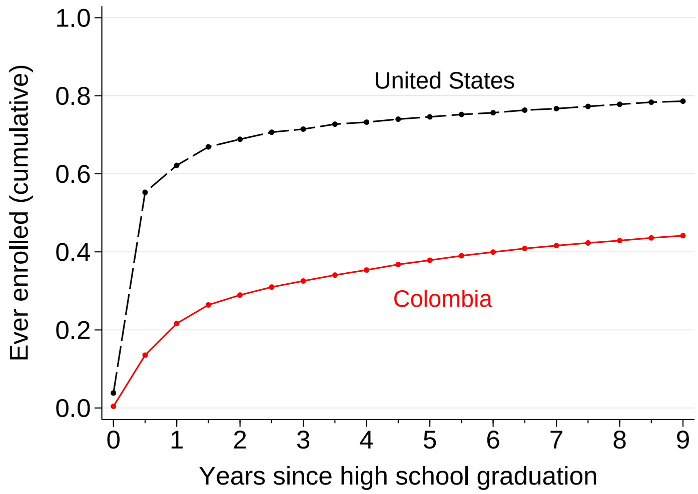
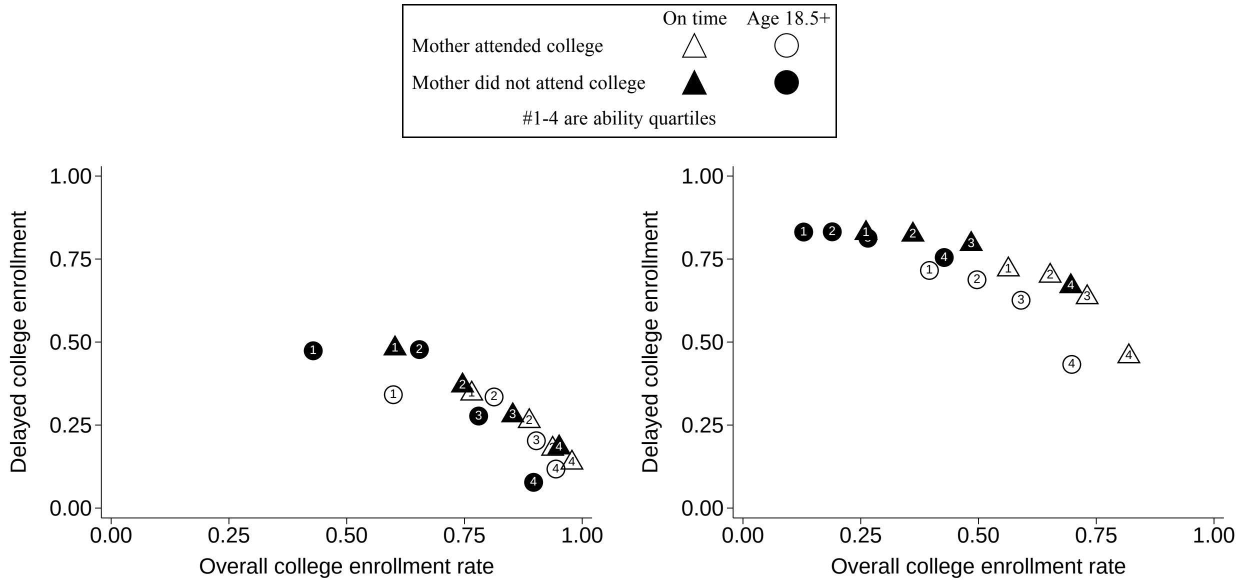


Figure 1. College enrollment timing in the U.S. and Colombia

Notes: This figure shows the proportion of individuals who ever enrolled in a higher education institution (y-axis) by the number of years since high school graduation (x-axis). Enrollment at zero years includes individuals reported to begin college in or before the month of high school graduation. The U.S. sample (dashed line) includes individuals in the 1997 NLSY cross-section and over-sample with a high school graduation year in 1995–2002. Observations are weighted by 2011 panel weights and aggregated into six month bins. The Colombian sample (solid line) includes any 11^{th} grader who took the national college entrance exam in 2002. See Section 3 for details on the Colombian data.



Panel A. United States

Panel B. Colombia

FIGURE 2. Delayed college enrollment by ability, socioeconomic status, and age

Notes: This figure plots delayed and overall college enrollment rates for groups of individuals defined by age at high school graduation, socioeconomic status, and ability, as depicted in the legend. The samples include individuals in the U.S. and Colombian datasets from Figure 1 with non-missing values of the variables described below. The x-axis is the proportion of individuals in each group who enrolled in college within nine years of high school graduation. The y-axis is the proportion of college enrollees in each group who began more than six months after graduation.

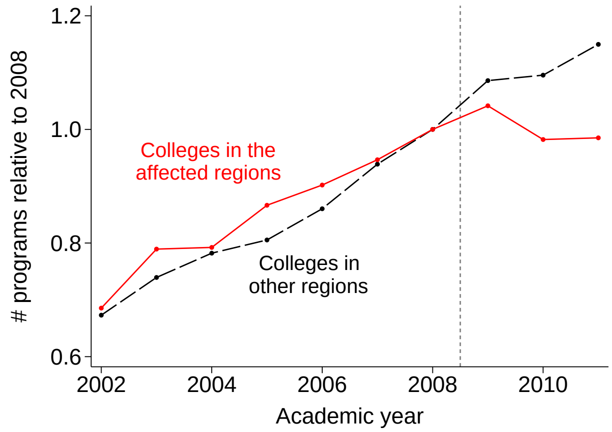
In Panel A, age is calculated at the end of the month prior to graduation. "Mother attended college" means a graduate's mother has some college education. Ability is defined by quartiles of the Armed Services Vocational Aptitude Battery math/verbal percentile within the sample. All calculations use 2011 panel weights.

In Panel B, age is calculated at the end of August in the college entrance exam year. "Mother attended college" means a graduate's mother has any degree above basic secondary. Ability is defined by quartiles of the aggregate entrance exam percentile within the sample. See Table 1 for details on these variables.

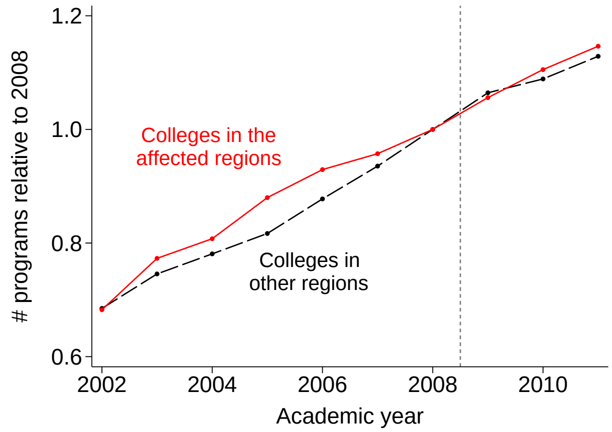
FIGURE 3. Calendar transition and a time gap at switching schools

Notes: This figure shows public high school calendars based on 2006–2012 resolutions from the Secretary of Education in Cali, Valle del Cauca. Schedules are approximate based on half-month increments; there are small yearly differences in start/end dates and ICFES timing. See Appendix B.4 for details.

Grey bars are instructional periods in the last year of high school. Gaps are break periods. White boxes represent the first potential college semester. ICFES indicates the timing of the college entrance exam.



Panel A. September cohort



Panel B. Both cohorts

FIGURE 4. Number of college programs relative to 2008

Notes: This figure shows the number of programs with at least one enrollee in the Ministry of Education records by academic year. Programs are defined by an institution and program name. Academic years include September of the year on the x-axis and January of the next year. The y-axis is the number of programs in that year divided by the number of programs in 2008, computed separately for affected regions (solid lines) and other regions (dashed lines).

Panel A shows the number of September programs. Panel B shows the number of September and January programs.

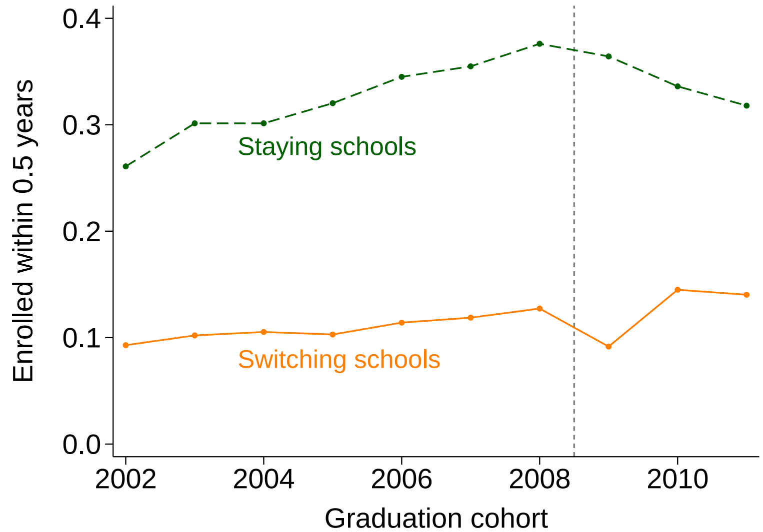
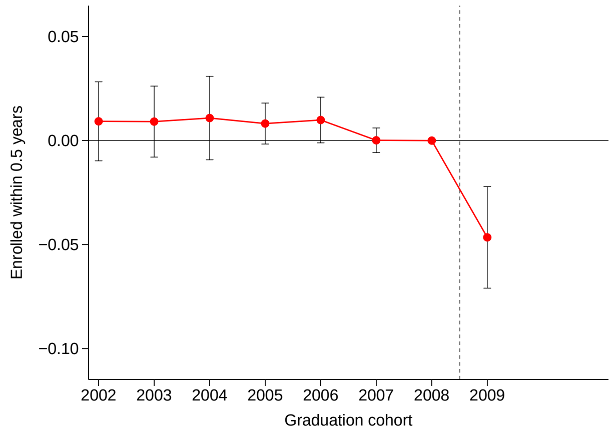
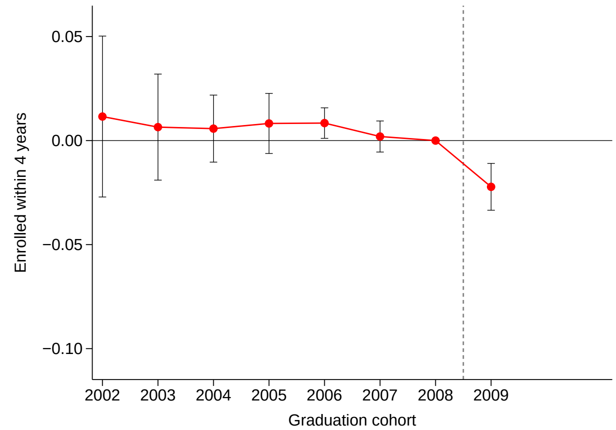


FIGURE 5. Immediate college enrollment rates at switching and staying schools

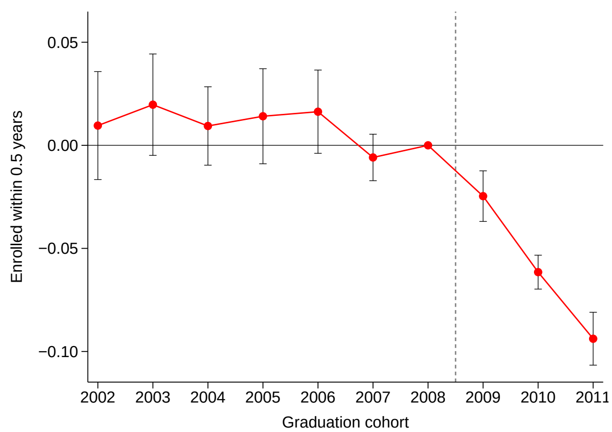
Notes: The y-axis is the proportion of students who enrolled in any college in the Ministry of Education records one semester after taking the ICFES exam. The x-axis is the student's graduation cohort, or equivalently, the year of the ICFES exam. The solid line includes graduates from switching schools (columns (A)–(B) in Table 1). The dashed line includes graduates from staying schools (column (C) in Table 1).



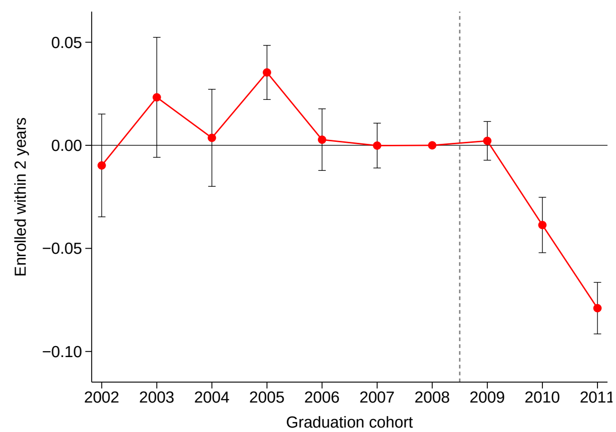
Panel A. Enrolled within 0.5 years
Switching schools



Panel B. Enrolled within 4 years
Switching schools



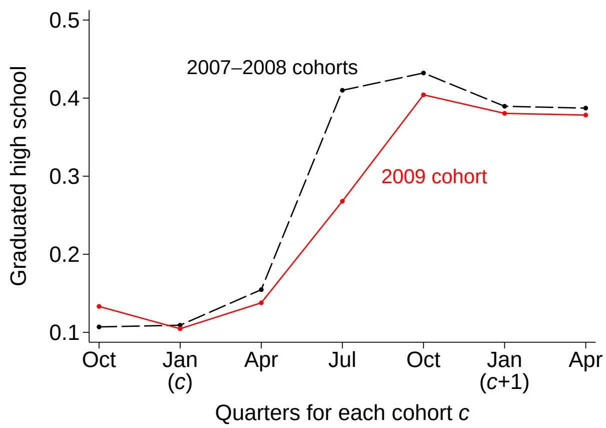
Panel C. Enrolled within 0.5 years
Staying schools



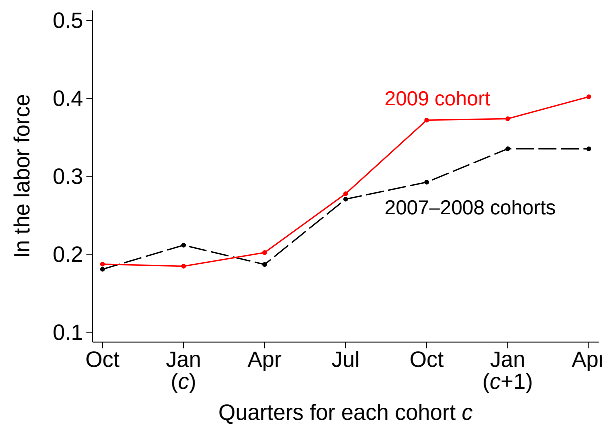
Panel D. Enrolled within 2 years
Staying schools

FIGURE 6. Event study coefficients — College enrollment by years since the ICFES exam

Notes: This figure plots event study coefficients based on regression (1). These coefficients are on the interaction of Transition_{hc} with dummies for graduation cohorts c, omitting the 2008 cohort interaction. The dependent variables are college enrollment within t years of the ICFES exam, as shown in the panel title. The sample for Panels A–B is 2002–2009 graduates at switching schools and public schools in other regions. The sample for Panels C–D is 2002–2011 graduates at staying schools and private schools in other regions. All regressions are estimated with synthetic control weights. Dashed lines are 95% confidence intervals with standard errors clustered at the region level.



Panel A. High school graduation



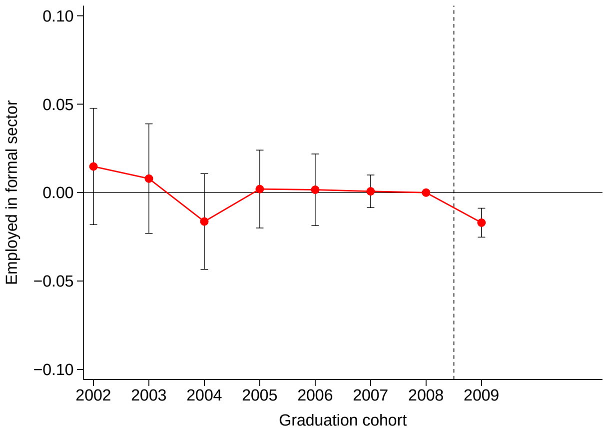
Panel B. Labor force participation

FIGURE 7. High school graduation and labor force participation for 17 year olds in the affected regions

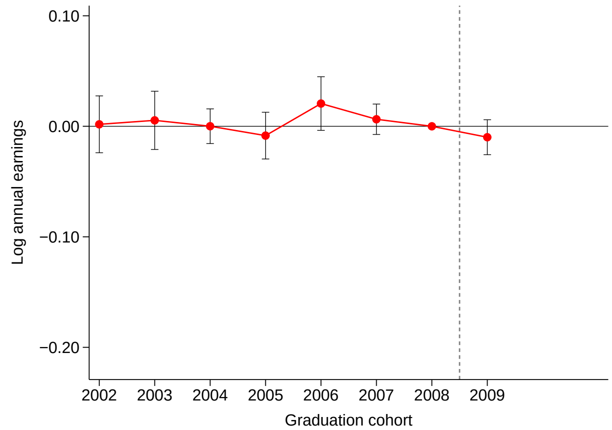
Notes: The figure plots high school graduation and labor force participation rates for cohorts in the affected regions who turned 17 in 2007–2009. Data are from the 2007–2010 monthly urban (*cabecera*) and rural (*resto*) GEIH household surveys. The sample includes individuals in the affected regions who were born in 1990–1991 (dashed lines) or 1992 (solid line), excluding anyone who identifies as the household head.

The x-axis in both panels combines monthly surveys into three-month periods beginning in October before the cohort year and ending in June after the cohort year (e.g., 10/2008 through 6/2010 for the 2009 cohort). 2006 surveys are not available, so Oct–Dec 2006 values for the 2007 cohort are equal to Oct–Dec 2007 values for the 2008 cohort plus the average difference between the 2007 and 2008 cohorts in Jan–Jun of the cohort year.

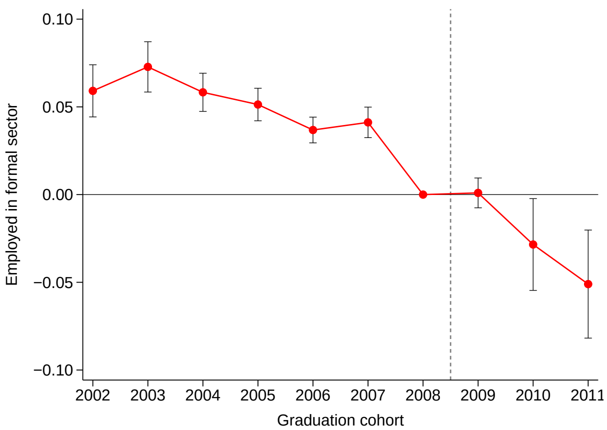
In Panel A, the y-axis is the fraction of each cohort group with a high school degree or above. In Panel B, the y-axis is the fraction of individuals that are either employed (*ocupados*) or unemployed (*desocupados*). Panel B is lagged one month because survey questions refer to last month's labor force activity. Observations are weighted by the sum of survey weights across all months within cells defined by region, age, gender, urban/rural survey, and secondary educated mother.



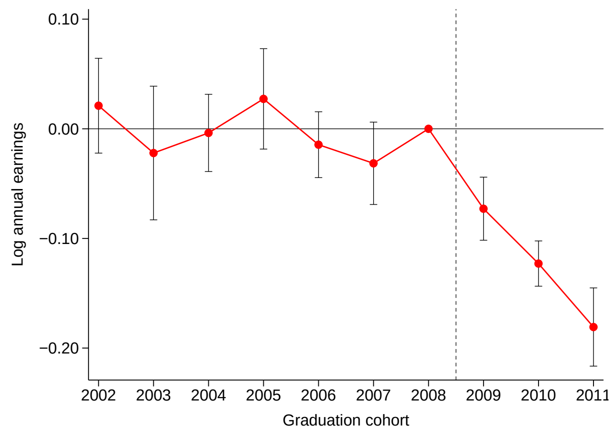
Panel A. Employed in formal sector
Switching schools



Panel B. Log annual earnings
Switching schools



Panel C. Employed in formal sector
Staying schools



Panel D. Log annual earnings
Staying schools

FIGURE 8. Event study coefficients — Labor market outcomes in 2017

Notes: This figure plots event study coefficients based on regression (1), defined as in Figure 6. In Panels A and C, the dependent variable is an indicator for appearing in the Ministry of Social Protection records in 2017. In Panels B and D, the dependent variable is log total earnings in 2017 excluding zeros. The sample for Panels A–B is 2002–2009 graduates at switching schools and public schools in other regions. The sample for Panels C–D is 2002–2011 graduates at staying schools and private schools in other regions. All regressions are estimated with synthetic control weights. Dashed lines are 95% confidence intervals with standard errors clustered at the region level.

Tables

Table 1. Summary statistics by high school type

	(A)	(B)	(C)	(D)	(E)
	Affected regions			Other regions	
	Switching schools		Staying schools		
	Public	Private	Private	Public	Private
# high schools	315	114	94	2,582	1,189
# graduates per year	21,133	5,315	3,787	175,734	57,453
School earned high/superior rank	0.47	0.57	0.97	0.56	0.89
Academic school	0.62	0.55	0.79	0.81	0.87
ICFES entrance exam percentile	0.51	0.54	0.74	0.49	0.67
Mother attended college	0.13	0.25	0.64	0.13	0.49
Age 18 or less	0.74	0.83	0.83	0.71	0.82
Enrolled in college 0.5 years after ICFES exam	0.09	0.13	0.30	0.09	0.30
Enrolled in college within 3 years	0.27	0.36	0.56	0.28	0.58
Enrolled in college within 6 years	0.30	0.39	0.58	0.31	0.61
Employed in formal sector in 2017	0.45	0.51	0.59	0.54	0.64
Annual earnings in 2017 (USD)	4,014	4,518	6,258	4,320	6,059

Notes: The sample includes 11^{th} graders who took the ICFES exam in 2002–2011 and attended high schools with exam takers in every year. We omit high schools that have a "flexible" academic calendar or that change calendars in any year besides those for the calendar transition in the affected regions. See Appendix B.3 for details on the sample.

Columns (A)–(C) show high schools in Nariño and Valle del Cauca; columns (D)–(E) include high schools in all other regions. Switching schools are those that operated on the January calendar in 2010 and/or 2011, and on the September calendar in all other years. Staying schools are those that operated on the September calendar in all years. Private schools are those that are listed as private in any year; public schools are listed as public in all years.

A school is high/superior rank if it ever received one of the exam agency's top three (of seven) ranks in 2002–2008. Academic school means a school is listed as academic, academic & technical, or normal in any of these years.

ICFES percentiles are relative to all 11^{th} grade exam takers in the same year and are calculated using the average of the biology, chemistry, language, mathematics, philosophy, and physics scores. Mother attended college equals one if a graduate's mother has any degree above basic secondary; this variable is only available for the 2008–2011 cohorts. Age is calculated at the end of August in the exam year.

College enrollment within t years of the ICFES is defined as a graduate's first appearance in the Ministry of Education college records (see Appendix B.2). These averages are calculated using 2002–2006 graduates.

Employed in the formal sector is an indicator for appear in the Ministry of Social Protection dataset in 2017. Annual earnings is the average total earnings in that year conditional on employment, converted to 2017 U.S. dollars.

Table 2. Effects of the calendar transition on college enrollment

	(A)	(B)	(C)	(D)	(E)	(F)
	Switching schools (2002–2009 grads)		Staying schools (2002–2011 grads)		Staying schools (2002–2010 grads)	
Dependent variable	Bench-mark	Region trends	Bench-mark	Region trends	Bench-mark	Region trends
Enrolled within 0.5 years	-0.053^{***}	-0.046^{***}	-0.070^{***}	-0.047^{***}	-0.053^{***}	-0.039^{***}
	(0.008)	(0.014)	(0.008)	(0.004)	(0.009)	(0.003)
Enrolled within 1 year	-0.038^{***}	-0.039^{***}	-0.059^{***}	-0.040^{***}	-0.040^{***}	-0.031^{***}
	(0.005)	(0.010)	(0.010)	(0.004)	(0.010)	(0.004)
Enrolled within 2 years	-0.027^{***}	-0.027^{***}	-0.048^{***}	-0.031^{***}	-0.026^{**}	-0.021^{***}
	(0.006)	(0.009)	(0.010)	(0.004)	(0.010)	(0.004)
Enrolled within 3 years	-0.026^{***}	-0.023^{**}			-0.026^{***}	-0.020^{***}
	(0.006)	(0.010)			(0.009)	(0.004)
Enrolled within 4 years	-0.028^{***}	-0.022^{**}				
	(0.006)	(0.010)				
N	1,587,250	1,587,250	494,198	494,198	443,958	443,958
Cohort trends by region		Y		Y		Y

Notes: This table displays β coefficients from separate regressions (1). The dependent variables are college enrollment within t years of the ICFES exam, as shown in the first column. The sample for columns (A)–(B) is 2002–2009 graduates at switching schools and public schools in other regions (columns (A)–(B) and (D) in Table 1). The sample for columns (C)–(D) is 2002–2011 graduates at staying schools and private schools in other regions (columns (C) and (E) in Table 1). The sample for columns (E)–(F) is similar to that in columns (C)–(D), but it omits the 2011 cohort.

Columns (A), (C), and (E) estimate equation (1) as specified. Columns (B), (D), and (F) add a linear cohort trend for each region r (i.e., $\gamma_r \times c$ terms). All regressions are estimated with synthetic control weights (see Appendix Tables A1–A2). Parentheses contain standard errors clustered at the region level.

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

Table 3. Summary statistics by quartile of college propensity

	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
	Switching schools				Staying schools			
	Top Q	Q3	Q2	Q1	Top Q	Q3	Q2	Q1
ICFES entrance exam percentile	0.76	0.54	0.40	0.26	0.88	0.77	0.60	0.40
Mother attended college	0.31	0.16	0.09	0.04	0.63	0.55	0.41	0.22
Age 18 or less	0.94	0.83	0.68	0.29	0.96	0.83	0.77	0.61
Enrolled in college within 6 years	0.64	0.37	0.21	0.09	0.77	0.67	0.58	0.37
Employed in formal sector in 2017	0.67	0.57	0.50	0.46	0.73	0.71	0.67	0.60
Annual earnings in 2017 (USD)	6,287	4,789	4,227	3,598	8,603	7,778	6,154	4,664

Notes: The table shows summary statistics by quartile of the college propensity index. College propensity is the predicted value from a regression of enrolling in college within six years of graduation on a large vector of individual covariates in the 2002–2006 cohorts. See Appendix Table A8 for details.

Columns (A)–(D) include switching schools and their comparison schools. Columns (E)–(H) include staying schools and their comparison schools. We define quartiles separately for the two samples, with observations in the comparison groups weighted by the synthetic control weights. Variables are defined as in Table 1.

Table 4. Enrollment effects by college propensity

	(A)	(B)	(C)	(D)	(E)	(F)
	Switching schools (2002–2009 grads)			Staying schools (2002–2011 grads)		
Quartile of college enrollment index	Enrolled within 0.5 years	Enrolled within 4 years	Effect of time gap (B)/(A)	Enrolled within 0.5 years	Enrolled within 2 years	Effect of time gap (E)/(D)
Top quartile	-0.086^{***}	-0.039^{***}	0.456^{**}	-0.073^{***}	-0.032^{***}	0.441^{***}
	(0.022)	(0.010)	(0.183)	(0.010)	(0.005)	(0.078)
Quartile 3	-0.069^{***}	-0.035^{**}	0.505^{***}	-0.054^{***}	-0.030^{*}	0.557^{***}
	(0.005)	(0.013)	(0.181)	(0.016)	(0.016)	(0.130)
Quartile 2	-0.042^{***}	-0.029^{***}	0.694^{***}	-0.086^{***}	-0.063^{***}	0.733^{***}
	(0.004)	(0.010)	(0.195)	(0.012)	(0.014)	(0.065)
Bottom quartile	-0.017^{***}	-0.020^{***}	1.170^{***}	-0.067^{***}	-0.061^{***}	0.905^{***}
	(0.002)	(0.004)	(0.170)	(0.007)	(0.009)	(0.070)
N	1,587,250	1,587,250	1,587,250	494,198	494,198	494,198

Notes: This table displays coefficients from separate regressions for each quartile of the college propensity index, defined as in Table 3. Columns (A)–(B) and (D)–(E) report β coefficients from equation (1). The dependent variables for these regressions are college enrollment within t years of the ICFES exam, as shown in the column header. Columns (C) and (F) report IV coefficients from two stage least squares regressions based on equation (1). The dependent variables are the same as in columns (B) and (E), and we use Transition$_{hc}$ as a instrument for college enrollment within 0.5 years of the ICFES. All regressions are estimated with synthetic control weights.

The sample for columns (A)–(C) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (D)–(F) is 2002–2011 graduates at staying schools and private schools in other regions. Parentheses contain standard errors clustered at the region level.

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

Table 5. Effects of the calendar transition on college persistence

	(A)	(B)	(C)	(D)
	Switching schools (2002–2009 grads)		Staying schools (2002–2010 grads)	
Dependent variable	Enrolled within 1 year	Enrolled within 2 years	Enrolled within 1 year	Enrolled within 2 years
Enrolled in college	-0.038^{***}	-0.027^{***}	-0.040^{***}	-0.026^{**}
	(0.005)	(0.006)	(0.010)	(0.010)
Enrolled and persisted 0.5 years	-0.031^{***}	-0.023^{***}	-0.045^{***}	-0.037^{***}
	(0.005)	(0.006)	(0.008)	(0.009)
Enrolled and persisted 1 year	-0.027^{***}	-0.020^{***}	-0.046^{***}	
	(0.004)	(0.005)	(0.007)	
Enrolled and persisted 1.5 years	-0.022^{***}	-0.015^{***}	-0.043^{***}	
	(0.004)	(0.004)	(0.007)	
Enrolled and persisted 2 years	-0.019^{***}			
	(0.003)			
Enrolled and persisted 2.5 years	-0.017^{***}			
	(0.003)			
N	1,587,250	1,587,250	443,958	443,958

Notes: This table displays β coefficients from separate regressions (1). In the first row, the dependent variables are college enrollment within t years of the ICFES exam, as shown in the column header. In other rows, the dependent variables are indicators for enrolling in college within t years of the ICFES exam *and* persisting in college for y years, as shown in the first column. The sample for columns (A)–(B) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (C)–(D) is 2002–2010 graduates at staying schools and private schools in other regions. All regressions are estimated with synthetic control weights. Parentheses contain standard errors clustered at the region level.

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

Table 6. Effects of the calendar transition on labor market outcomes in 2017

	(A)	(B)	(C)	(D)
	Switching schools (2002–2009 grads)		Staying schools (2002–2011 grads)	
Dependent variable	Bench-mark	Region trends	Bench-mark	Region trends
Employed in formal sector	−0.018	−0.013***	−0.073***	−0.020***
	(0.013)	(0.004)	(0.011)	(0.005)
Log monthly earnings	0.022***	0.006	−0.051***	−0.041***
	(0.007)	(0.007)	(0.008)	(0.007)
Log annual earnings	−0.014	−0.016	−0.118***	−0.091***
	(0.013)	(0.011)	(0.009)	(0.010)
N (full sample)	1,587,250	1,587,250	494,198	494,198
N (if formally employed)	863,744	863,744	312,653	312,653
Cohort trends by region		Y		Y

Notes: This table displays β coefficients from separate regressions (1). The dependent variables, as listed in the first column, are: 1) an indicator for appearing in the Ministry of Social Protection records in 2017; 2) average log monthly earnings in 2017 if formally employed; 3) log total earnings in 2017 if formally employed.

The sample for columns (A)–(B) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (C)–(D) is 2002–2011 graduates at staying schools and private schools in other regions.

Columns (A) and (C) estimate equation (1) as specified. Columns (B) and (D) add a linear cohort trend for each region r (i.e., $\gamma_r \times c$ terms). All regressions are estimated with synthetic control weights. N (full sample) is the number of observations for the regressions in row 1. N (if formally employed) is the number of observations for the regressions in rows 2 and 3. Parentheses contain standard errors clustered at the region level.

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

Table 7. Labor market effects by college propensity

	(A)	(B)	(C)	(D)	(E)	(F)
	Switching schools			Staying schools		
Quartile of college enrollment index	Enroll & persist 1.5 years	Formal employment	Log annual earnings	Enroll & persist 1.5 years	Formal employment	Log annual earnings
Top quartile	-0.050^{***}	-0.026^{*}	-0.062^{***}	-0.045^{***}	-0.000	-0.050^{**}
	(0.014)	(0.013)	(0.010)	(0.007)	(0.010)	(0.019)
Quartile 3	-0.017	-0.004	0.038^{***}	-0.062^{***}	-0.019	-0.073^{***}
	(0.013)	(0.007)	(0.011)	(0.012)	(0.018)	(0.012)
Quartile 2	-0.017^{***}	-0.004	0.076^{**}	-0.019^{***}	-0.004	-0.052^{***}
	(0.004)	(0.011)	(0.029)	(0.007)	(0.011)	(0.016)
Bottom quartile	-0.006^{**}	0.008	-0.023	-0.033^{***}	-0.033^{***}	-0.114^{***}
	(0.002)	(0.008)	(0.016)	(0.004)	(0.004)	(0.020)
N	1,587,250	1,587,250	863,744	443,958	494,198	312,653
Cohort trends by region	Y	Y	Y	Y	Y	Y

Notes: This table displays β coefficients from separate regressions (1) for each quartile of the college propensity index, defined as in Table 3. The dependent variables, as listed in the column header, are: 1) an indicator for enrolling in college within one year of the ICFES exam *and* persisting in college for 1.5 years; 2) an indicator for appearing in the Ministry of Social Protection records in 2017; and 3) log total earnings in 2017 if formally employed.

The sample for columns (A)–(C) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (E)–(F) is 2002–2011 graduates at staying schools and private schools in other regions. The sample for column (D) is similar to that in columns (E)–(F), but it omits the 2011 cohort.

All regressions are estimated with synthetic control weights and include a linear cohort trend for each region r (i.e., $\gamma_r \times c$ terms). Parentheses contain standard errors clustered at the region level.

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

Appendix — For online publication

A. Appendix figures and tables

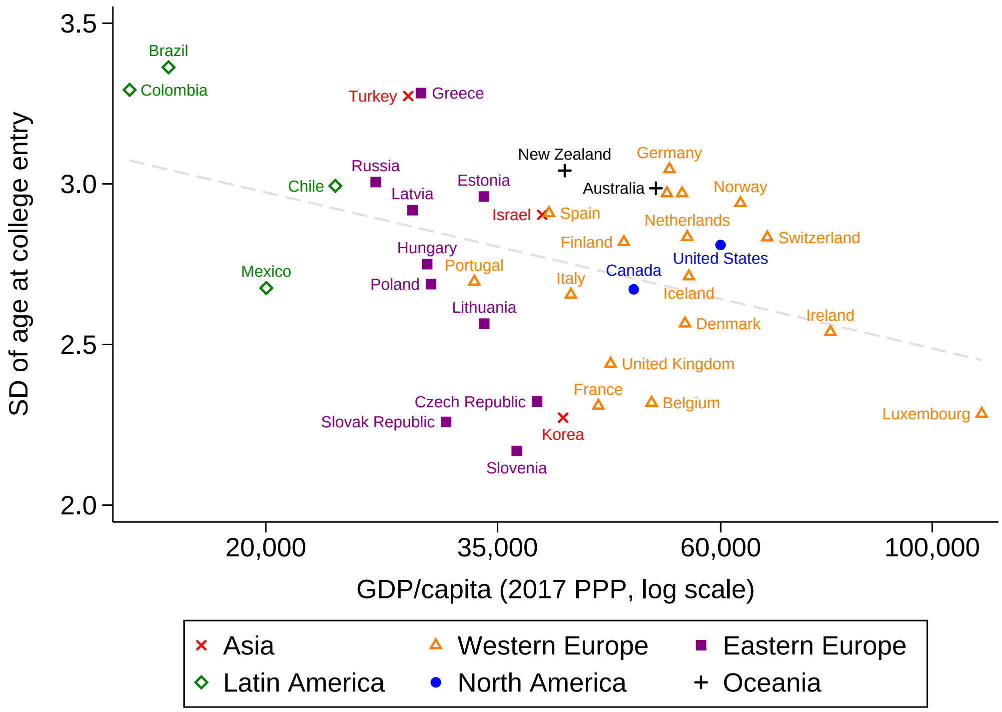


FIGURE A1. St. deviation of age at college entry and GDP/capita

Notes: This figure plots the difference between the average ages of new tertiary entrants and secondary graduates for OECD countries with data in 2011 plus Colombia.

For OECD countries, data are from http://stats.oecd.org. Bars depict the difference between the average ages of new entrants to level A tertiary programs and graduates from upper secondary general programs. In the U.S., upper secondary includes all education programs because there is no distinction between general and vocational programs. For Colombia, the bar depicts the age difference between entrants to university-level programs and 11^{th} grade college entrance exam takers in 2011 using the data described in Section 3. All secondary and tertiary averages are calculated using students with ages 29 or below; above this, the OECD data only report ages in bins.

Table A1. Synthetic control weights — Switching schools

(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Quintiles of college enrollment		Switching schools in affected regions			Public schools in other regions			Synthetic weight
2002–2003 cohorts	2007–2008 cohorts	# schools	# students	% of all students	# schools	# students	% of all students	(E)/(H)
1	1	112	26,442	14.47%	581	156,985	13.06%	1.11
1	2	21	5,768	3.16%	187	59,193	4.92%	0.64
1	3	9	2,046	1.12%	57	16,326	1.36%	0.82
1	4	1	84	0.05%	20	5,423	0.45%	0.10
1	5				11	4,455	0.37%	0.00
2	1	40	13,865	7.59%	167	60,513	5.03%	1.51
2	2	34	14,042	7.68%	215	101,159	8.41%	0.91
2	3	14	8,516	4.66%	122	58,909	4.90%	0.95
2	4	7	2,148	1.18%	39	14,422	1.20%	0.98
2	5	1	60	0.03%	8	3,430	0.29%	0.12
3	1	13	4,155	2.27%	38	10,669	0.89%	2.56
3	2	19	13,004	7.12%	135	62,339	5.19%	1.37
3	3	15	5,257	2.88%	174	98,523	8.19%	0.35
3	4	10	8,675	4.75%	112	63,208	5.26%	0.90
3	5	4	1,562	0.85%	21	9,604	0.80%	1.07
4	1				12	2,725	0.23%	0.00
4	2	8	3,197	1.75%	40	17,603	1.46%	1.19
4	3	19	12,691	6.94%	109	68,113	5.67%	1.23
4	4	29	21,170	11.58%	165	108,788	9.05%	1.28
4	5	14	4,956	2.71%	66	37,693	3.14%	0.86
5	1	1	623	0.34%	4	507	0.04%	8.08
5	2	2	273	0.15%	1	691	0.06%	2.60
5	3	1	640	0.35%	11	6,235	0.52%	0.68
5	4	10	6,474	3.54%	60	46,515	3.87%	0.92
5	5	45	27,111	14.83%	227	188,249	15.66%	0.95
Total		429	182,759	100.00%	2,582	1,202,277	100.00%	

Notes: This table describes our computation of synthetic control weights for switching schools.

For each high school h, we first compute the college enrollment rate within four years of the ICFES exam for 2002–2003 graduates, r_h^{02}, and for 2007–2008 graduates, r_h^{08}. Columns (A)–(B) show quintiles of r_h^{02} and r_h^{08} in a sample that includes switching schools and public high schools in other regions.

Column (C) shows the number of switching schools in each group $g \in \{1, \ldots, 25\}$ defined by the two sets of quintiles. Column (D) shows the total number of switching school graduates in each group. Column (E) shows the proportion of switching school graduates in each group, which equals the value in column (D) divided by the sum of column (D). Columns (F)–(H) show the same statistics for public schools in other regions.

Our synthetic control weights for switching schools are the ratio of values in columns (E) and (H), which we report in column (I). All switching school regressions in this paper are estimated with observations from public schools in other regions weighted by these synthetic control weights.

Table A2. Synthetic control weights — Staying schools

(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Quintiles of college enrollment		Staying schools in affected regions			Private schools in other regions			Synthetic weight
2002–2003 cohorts	2007–2008 cohorts	# schools	# students	% of all students	# schools	# students	% of all students	(E)/(H)
1	1	13	1,851	6.93%	263	64,856	16.03%	0.43
1	2	2	1,131	4.24%	58	14,194	3.51%	1.21
1	3				12	3,174	0.78%	0.00
1	4				2	403	0.10%	0.00
1	5				1	322	0.08%	0.00
2	1	5	746	2.79%	55	14,834	3.67%	0.76
2	2	15	4,150	15.54%	98	32,326	7.99%	1.95
2	3	12	5,595	20.95%	56	21,067	5.21%	4.02
2	4	5	840	3.15%	20	5,059	1.25%	2.52
2	5				10	1,569	0.39%	0.00
3	1	1	178	0.67%	11	2,912	0.72%	0.93
3	2	12	2,640	9.89%	58	17,025	4.21%	2.35
3	3	8	3,475	13.01%	54	22,382	5.53%	2.35
3	4	3	1,004	3.76%	56	26,987	6.67%	0.56
3	5	2	280	1.05%	21	8,679	2.15%	0.49
4	1				1	122	0.03%	0.00
4	2	5	1,886	7.06%	29	10,970	2.71%	2.60
4	3	1	105	0.39%	65	24,501	6.06%	0.06
4	4	4	1,856	6.95%	69	32,174	7.95%	0.87
4	5	1	281	1.05%	44	15,371	3.80%	0.28
5	1	2	217	0.81%	2	362	0.09%	9.08
5	2	2	282	1.06%	7	1,720	0.43%	2.48
5	3	1	185	0.69%	23	5,642	1.39%	0.50
5	4				48	16,769	4.14%	0.00
5	5				126	61,154	15.12%	0.00
Total		94	26,702	100.00%	1,189	404,574	100.00%	

Notes: This table describes our computation of synthetic control weights for staying schools.

For each high school h, we first compute the college enrollment rate within four years of the ICFES exam for 2002–2003 graduates, r_h^{02}, and for 2007–2008 graduates, r_h^{08}. Columns (A)–(B) show quintiles of r_h^{02} and r_h^{08} in a sample that includes staying schools and private high schools in other regions.

Column (C) shows the number of staying schools in each group $g \in \{1, \ldots, 25\}$ defined by the two sets of quintiles. Column (D) shows the total number of staying school graduates in each group. Column (E) shows the proportion of staying school graduates in each group, which equals the value in column (D) divided by the sum of column (D). Columns (F)–(H) show the same statistics for private schools in other regions.

Our synthetic control weights for staying schools are the ratio of values in columns (E) and (H), which we report in column (I). All staying school regressions in this paper are estimated with observations from private schools in other regions weighted by these synthetic control weights.

Table A3. Balance tests

	(A)	(B)	(C)	(D)	(E)	(F)
	Switching schools (2002–2009 grads)			Staying schools (2002–2011 grads)		
Dependent variable	*N*	Mean	DD coef	*N*	Mean	DD coef
# graduates in region	264	5,666	−82	250	3,268	−5
			(149)			(56)
ICFES exam percentile	1,595,482	0.486	0.005	612,409	0.658	0.001
			(0.009)			(0.004)
Female	1,595,480	0.544	−0.001	612,409	0.517	0.016***
			(0.009)			(0.005)
Age at time of ICFES	1,593,053	17.764	−0.044	611,895	17.276	0.042
			(0.094)			(0.025)
No high school fee	1,539,175	0.589	0.063	573,522	0.130	−0.014
			(0.039)			(0.016)

Notes: This table displays results from balance tests based on regression (1). The dependent variable for each balance test is listed in the first column. "# graduates in region" is the number of ICFES exam takers in each region and graduation cohort. "No high school fee" is an indicator equal to one if the student did not pay tuition for high school, and zero if the student had any positive tuition fee.

Regressions in the first row are at the region-cohort level; these regressions include region dummies, cohort dummies, and a treatment variable, Transition_{rc}, which is an indicator for affected regions and post-transition cohorts. These regressions are weighted by the sum of the synthetic control weights in each region-cohort. In all other rows, regressions are at the individual level as specified in equation (1), and are estimated with synthetic control weights.

The sample for columns (A)–(C) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (C)–(D) is 2002–2011 graduates at staying schools and private schools in other regions.

Columns (A) and (D) show the number of observations for each regression. Columns (B) and (E) show means of the dependent variable in the 2002–2008 cohorts. Columns (C) and (F) show the β coefficients from equation (1). Parentheses contain standard errors clustered at the region level.

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

Table A4. Region-cohort level regressions — Switching schools

	(A)	(B)	(C)	(D)
	Region-cohort means		Means after demeaning by high school	
Dependent variable	Bench-mark	Region trends	Bench-mark	Region trends
Enrolled within 0.5 years	-0.055^{***}	-0.043^{***}	-0.053^{***}	-0.046^{***}
	(0.008)	(0.011)	(0.008)	(0.011)
Enrolled within 1 year	-0.040^{***}	-0.034^{***}	-0.038^{***}	-0.039^{***}
	(0.009)	(0.010)	(0.009)	(0.010)
Enrolled within 2 years	-0.029^{***}	-0.020^{*}	-0.027^{**}	-0.027^{**}
	(0.010)	(0.010)	(0.010)	(0.010)
Enrolled within 3 years	-0.028^{***}	-0.015	-0.026^{**}	-0.023^{**}
	(0.010)	(0.010)	(0.010)	(0.010)
Enrolled within 4 years	-0.030^{***}	-0.014	-0.028^{***}	-0.022^{**}
	(0.010)	(0.009)	(0.010)	(0.009)
Enrolled and persisted 0.5 years	-0.032^{***}	-0.033^{***}	-0.031^{***}	-0.037^{***}
	(0.008)	(0.009)	(0.008)	(0.008)
Enrolled and persisted 1 year	-0.029^{***}	-0.032^{***}	-0.028^{***}	-0.036^{***}
	(0.007)	(0.008)	(0.007)	(0.008)
Enrolled and persisted 1.5 years	-0.024^{***}	-0.027^{***}	-0.023^{***}	-0.031^{***}
	(0.007)	(0.007)	(0.007)	(0.007)
Enrolled and persisted 2 years	-0.020^{***}	-0.024^{***}	-0.019^{***}	-0.027^{***}
	(0.006)	(0.007)	(0.006)	(0.006)
Enrolled and persisted 2.5 years	-0.018^{***}	-0.022^{***}	-0.018^{***}	-0.025^{***}
	(0.006)	(0.006)	(0.006)	(0.006)
Employed in formal sector	-0.019	-0.009	-0.018	-0.013^{**}
	(0.014)	(0.006)	(0.013)	(0.006)
Log monthly earnings	0.015	0.008	0.023^{**}	0.005
	(0.011)	(0.011)	(0.011)	(0.010)
Log annual earnings	-0.023	-0.013	-0.012	-0.017
	(0.017)	(0.017)	(0.016)	(0.017)
N	264	264	264	264
Cohort trends by region		Y		Y

Notes: This table displays β coefficients from the region-cohort level regression:

$$\bar{y}_{rc} = \gamma_r + \gamma_c + \beta \text{Transition}_{rc} + \epsilon_{rc}.$$

$\bar{y}_{rc}$ is the mean of the variable in the first column for region r and cohort c (see Tables 2, 5, and 6 for variable definitions). Regressions include region dummies, γ_r, cohort dummies, γ_c, and the variable of interest, Transition_{rc}, which is an indicator for the 2009 cohort in the affected regions. In columns (A)–(B), $\bar{y}_{rc}$ is the raw region-cohort mean. In columns (C)–(D), $\bar{y}_{rc}$ is the region-cohort mean after first demeaning the variable by high school.

The sample is 2002–2009 graduates at switching schools and public schools in other regions. Columns (A) and (C) estimate the above regression as specified. Columns (B) and (D) add a linear cohort trend for each region r (i.e., $\gamma_r \times c$ terms). In all regressions, observations are weighted by the sum of the synthetic control weights in each region-cohort. Parentheses contain the maximum of OLS and robust standard errors. Asterisks report statistical significance levels from a $t(33-2)$ distribution, where 33 is the total number of regions.

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

Table A5. Region-cohort level regressions — Staying schools

	(A)	(B)	(C)	(D)
	Region-cohort means		Means after demeaning by high school	
Dependent variable	Bench-mark	Region trends	Bench-mark	Region trends
Enrolled within 0.5 years	-0.063^{***}	-0.040^{**}	-0.070^{***}	-0.047^{**}
	(0.015)	(0.018)	(0.017)	(0.019)
Enrolled within 1 year	-0.051^{***}	-0.034^{*}	-0.058^{***}	-0.040^{*}
	(0.017)	(0.020)	(0.018)	(0.022)
Enrolled within 2 years	-0.039^{**}	-0.026	-0.047^{**}	-0.032
	(0.018)	(0.022)	(0.020)	(0.023)
Enrolled and persisted 0.5 years	-0.039^{*}	-0.040^{*}	-0.045^{*}	-0.045
	(0.021)	(0.023)	(0.025)	(0.027)
Enrolled and persisted 1 year	-0.039^{**}	-0.046^{**}	-0.045^{**}	-0.051^{**}
	(0.016)	(0.018)	(0.019)	(0.022)
Enrolled and persisted 1.5 years	-0.037^{**}	-0.046^{***}	-0.043^{**}	-0.050^{***}
	(0.013)	(0.016)	(0.015)	(0.018)
Employed in formal sector	-0.069^{***}	-0.018	-0.073^{***}	-0.019
	(0.013)	(0.016)	(0.014)	(0.016)
Log monthly earnings	-0.043^{**}	-0.040	-0.051^{***}	-0.039
	(0.018)	(0.027)	(0.018)	(0.028)
Log annual earnings	-0.108^{***}	-0.089^{**}	-0.119^{***}	-0.087^{**}
	(0.027)	(0.041)	(0.027)	(0.040)
N	250	250	250	250
Cohort trends by region		Y		Y

Notes: This table displays β coefficients from the region-cohort level regression:

$$\bar{y}_{rc} = \gamma_r + \gamma_c + \beta \text{Transition}_{rc} + \epsilon_{rc}.$$

$\bar{y}_{rc}$ is the mean of the variable in the first column for region r and cohort c (see Tables 2, 5, and 6 for variable definitions). Regressions include region dummies, γ_r, cohort dummies, γ_c, and the variable of interest, Transition_{rc}, which is an indicator for the 2009–2011 cohorts in the affected regions. In columns (A)–(B), $\bar{y}_{rc}$ is the raw region-cohort mean. In columns (C)–(D), $\bar{y}_{rc}$ is the region-cohort mean after first demeaning the variable by high school.

The sample is 2002–2011 graduates at staying schools and private schools in other regions. Columns (A) and (C) estimate the above regression as specified. Columns (B) and (D) add a linear cohort trend for each region r (i.e., $\gamma_r \times c$ terms). In all regressions, observations are weighted by the sum of the synthetic control weights in each region-cohort. Parentheses contain the maximum of OLS and robust standard errors. Asterisks report statistical significance levels from a $t(25-2)$ distribution, where 25 is the total number of regions that have private high schools.

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

Table A6. Effects of the calendar transition on college enrollment by college location

Dependent variable	(A)	(B)	(C)	(D)	(E)	(F)
	Switching schools (2002–2009 grads)			Staying schools (2002–2011 grads)		
	Any college	College in same region	College out of region	Any college	College in same region	College out of region
Enrolled within 0.5 years	-0.053^{***}	-0.040^{***}	-0.013^{***}	-0.070^{***}	-0.060^{***}	-0.011
	(0.008)	(0.009)	(0.002)	(0.008)	(0.008)	(0.009)
Enrolled within 1 year	-0.038^{***}	-0.025^{***}	-0.013^{***}	-0.059^{***}	-0.050^{***}	-0.008
	(0.005)	(0.007)	(0.004)	(0.010)	(0.010)	(0.010)
Enrolled within 2 years	-0.027^{***}	-0.021^{***}	-0.006^{*}	-0.048^{***}	-0.040^{***}	-0.008
	(0.006)	(0.007)	(0.003)	(0.010)	(0.010)	(0.011)
Enrolled within 3 years	-0.026^{***}	-0.021^{***}	-0.005			
	(0.006)	(0.007)	(0.004)			
Enrolled within 4 years	-0.028^{***}	-0.024^{***}	-0.004			
	(0.006)	(0.007)	(0.004)			
N	1,587,250	1,587,250	1,587,250	494,198	494,198	494,198
Dep. variable means for pre-transition cohorts in affected regions						
Enrolled within 0.5 years	0.110	0.089	0.021	0.322	0.270	0.052
Enrolled within 2 years	0.268	0.206	0.061	0.571	0.468	0.103
Enrolled within 4 years	0.332	0.252	0.080			

Notes: This table displays β coefficients from separate regressions (1). The dependent variables are college enrollment within t years of the ICFES exam, as shown in the first column. In columns (A) and (D), the dependent variable equals one if the student enrolled in any Colombian college by that time. In columns (B) and (E), the dependent variable equals one only if the student enrolled in a college located in the same region where she attended high school. In columns (C) and (F), the dependent variable equals one only if the student enrolled in a college located in a region other than the one where she attended high school.

The sample for columns (A)–(C) is 2002–2009 graduates at switching schools and public schools in other regions (columns (A)–(B) and (D) in Table 1). The sample for columns (D)–(F) is 2002–2011 graduates at staying schools and private schools in other regions (columns (C) and (E) in Table 1).

All columns estimate equation (1) as specified with synthetic control weights (see Appendix Tables A1–A2). The bottom three rows show the means of each dependent variable for the 2002–2008 cohorts in affected regions. Parentheses contain standard errors clustered at the region level.

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

Table A7. Enrollment effects by college selectivity

	(A)	(B)	(C)	(D)
	Switching schools (2002–2009 grads)		Staying schools (2002–2011 grads)	
Dependent variable	Non-selective college	Selective college	Non-selective college	Selective college
Enrolled within 0.5 years	-0.028^{***}	-0.025^{***}	-0.022^{***}	-0.048^{***}
	(0.004)	(0.009)	(0.006)	(0.004)
Enrolled within 1 years	-0.023^{***}	-0.015^{**}	-0.006	-0.053^{***}
	(0.007)	(0.006)	(0.009)	(0.006)
Enrolled within 2 years	-0.017^{**}	-0.009	0.006	-0.054^{***}
	(0.008)	(0.006)	(0.009)	(0.006)
Enrolled within 3 years	-0.018^{**}	-0.008		
	(0.008)	(0.006)		
Enrolled within 4 years	-0.018^{**}	-0.010		
	(0.008)	(0.007)		
N	1,587,250	1,587,250	494,198	494,198

Notes: This table displays β coefficients from separate regressions (1). The dependent variables are indicators for enrolling in a selective or non-selective college (listed in the column header) within t years of the ICFES exam (listed in the first column). We define college selectivity using program-level application data from the Ministry of Education (http://www.mineducacion.gov.co/sistemasdeinformacion/1735/w3-article-212400.html). We calculate the admission rate for each technical- and university-level program with a non-zero number of applicants and admitted students in the 2007–2008. The admission rate equals the total number of admitted students divided by the total number of applicants over this time period. "Selective colleges" are those below the median admission rate across all colleges. "Non-selective colleges" are those above the median admission rate or that do not appear in the application data.

The sample for columns (A)–(B) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (C)–(D) is 2002–2011 graduates at staying schools and private schools in other regions. All regressions are estimated with synthetic control weights. Parentheses contain standard errors clustered at the region level.

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

Table A8. Computation of college propensity index

(A)	(B)	(C)	(D)	(E)
	Switching schools		Staying schools	
Covariate	Coef	SE	Coef	SE
Constant	0.166	(0.002)	0.409	(0.005)
Female	−0.009	(0.001)	−0.014	(0.002)
Age 14 or less	0.038	(0.005)	0.002	(0.009)
Age 15	0.150	(0.002)	0.089	(0.004)
Age 16	0.087	(0.001)	0.046	(0.002)
Age 18	−0.059	(0.001)	−0.040	(0.003)
Age 19	−0.084	(0.002)	−0.071	(0.004)
Age 20	−0.099	(0.002)	−0.105	(0.007)
Age 21	−0.106	(0.003)	−0.128	(0.011)
Age 22	−0.116	(0.003)	−0.134	(0.016)
Age 23 or more	−0.105	(0.002)	−0.097	(0.009)
ICFES decile 2	0.019	(0.002)	0.038	(0.005)
ICFES decile 3	0.036	(0.002)	0.055	(0.005)
ICFES decile 4	0.055	(0.002)	0.075	(0.005)
ICFES decile 5	0.076	(0.002)	0.095	(0.005)
ICFES decile 6	0.106	(0.002)	0.126	(0.005)
ICFES decile 7	0.143	(0.002)	0.151	(0.005)
ICFES decile 8	0.196	(0.002)	0.178	(0.005)
ICFES decile 9	0.278	(0.002)	0.216	(0.005)
ICFES top decile	0.413	(0.002)	0.266	(0.005)

Notes: This table describes how we compute the college propensity index for Tables 4 and 7.

We first estimate the regression:

$$y_{ihc} = \gamma_c + \gamma_h + \psi X_i + e_{ihc}.$$

y_{ihc} is an indicator for enrolling in college within six years of graduation. γ_c and γ_h are fixed effects for graduation cohorts c and high schools h. X_i is a vector of covariates for individual i, which includes the dummy variables listed in column (A): gender, deciles of ICFES exam scores, and age in years at the time of ICFES. We estimate this regression separately for switching and staying schools (and their comparison groups) using the 2002–2006 cohorts. Columns (B) and (D) show the coefficient vectors $\hat{\psi}$ from these regressions. Columns (C) and (E) show robust standard errors.

Following Abadie et al. (2018), we use a leave-one-out estimator for the propensity index to reduce bias from in-sample stratification based on an outcome variable. For this, we compute the residual $\hat{e}_{ihc} = y_{ihc} - \hat{\psi} X_i$ from the above regression. We then compute the leave-one-out mean residual at each school, $\hat{\bar{e}}_{-ih}$, which is the mean of $\hat{e}_{ihc}$ for all students at high school h other than individual i.

Our college propensity index, $\hat{y}_{ih}$, is the predicted value from this regression using the leave-one-out mean residual, i.e., $\hat{y}_{ih} = \hat{\psi} X_i + \hat{\bar{e}}_{-ih}$. We define quartiles of $\hat{y}_{ih}$ separately for switching and staying schools (and their comparison groups), which we use for the regressions in Tables 4 and 7.

Table A9. Enrollment effects by program average earnings

	(A)	(B)	(C)	(D)
	Switching schools (2002–2009 grads)		Staying schools (2002–2011 grads)	
Dependent variable	Low earning programs	High earning programs	Low earning programs	High earning programs
Enrolled within 0.5 years	-0.029^{***}	-0.023^{***}	-0.018^{***}	-0.052^{***}
	(0.004)	(0.005)	(0.006)	(0.003)
Enrolled within 1 years	-0.026^{***}	-0.011^{***}	-0.014^{**}	-0.045^{***}
	(0.004)	(0.003)	(0.007)	(0.004)
Enrolled within 2 years	-0.018^{***}	-0.007^{*}	-0.015^{**}	-0.034^{***}
	(0.004)	(0.004)	(0.007)	(0.005)
Enrolled within 3 years	-0.020^{***}	-0.005		
	(0.004)	(0.004)		
Enrolled within 4 years	-0.020^{***}	-0.007^{*}		
	(0.004)	(0.004)		
N	1,587,250	1,587,250	494,198	494,198

Notes: This table displays β coefficients from separate regressions (1). The dependent variables are indicators for enrolling in a high or low earning program (listed in the column header) within t years of the ICFES exam (listed in the first column). We compute mean earnings in each program using log average earnings in 2017 for students from the 2002–2006 high school graduation cohorts. "High earning programs" are those above the median average earnings in their region. "Low earning programs" are those below the median average earnings in their region.

The sample for columns (A)–(B) is 2002–2009 graduates at switching schools and public schools in other regions. The sample for columns (C)–(D) is 2002–2011 graduates at staying schools and private schools in other regions. All regressions are estimated with synthetic control weights. Parentheses contain standard errors clustered at the region level.

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

Table A10. Effects of the calendar transition on high school graduation and labor force participation in survey data

	(A)	(B)
	Dependent variable	
Affected regions & cohorts in ...	Graduated high school	In the labor force
July−September 2009	-0.149^{***}	-0.011
	(0.036)	(0.036)
October−December 2009	-0.054	0.109^{*}
	(0.057)	(0.053)
January−June 2010	-0.037	0.057^{*}
	(0.036)	(0.029)
N	1,506	1,504
R^2	0.868	0.816
# regions	24	24
Dependent var. mean	0.372	0.244

Notes: This table displays coefficients that correspond to the high school graduation and labor force participation results in Figure 7. Data are from the 2007–2010 monthly urban (*cabecera*) and rural (*resto*) GEIH household surveys. The sample includes individuals in all regions in the 1990–1992 birth cohorts, excluding anyone who identifies as the household head; this defines cohorts, c, that turned 17 in 2007–2009. The sample includes months $t = 1$ to $t = 21$ for each cohort, where $t = 0$ is the start of 11^{th} grade for most high schools in the region. This means that $t = 0$ in September before the cohort year in the affected regions, and $t = 0$ in February of the cohort year in other regions. 2006 surveys are not available, so values at $t \leq 3$ are missing for the 2007 cohort in the affected regions.

The table shows β_q coefficients from the regression:

$$\bar{y}_{rct} = \gamma_{rc} + \gamma_{rt} + \gamma_{ct} + \beta_q \text{Transition}_{rc} + \epsilon_{rct},$$

where $\bar{y}_{rct}$ is a mean outcome in region r, cohort c, and month t. The regressions include region-cohort dummies, γ_{rc}, region-month dummies, γ_{rt}, and cohort-month dummies, γ_{ct}. The treatment variable, Transition_{rc}, is an indicator equal to one for the 2009 cohort in the affected regions. We allow the treatment coefficient, β_q, to vary with quarters q that capture the quarter of typical high school graduation ($t \in$ 10–12), the academic break for the 2009 cohort ($t \in$ 13–15), and the period following the academic break ($t \in$ 16–21).

In column (A), the dependent variable is the fraction of each region-cohort-month cell with a high school degree or above. In column (B), the dependent variable is the fraction of each region-cohort-month cell that is either employed (*ocupados*) or unemployed (*desocupados*). This variable is lagged one month because survey questions refer to last month's labor force activity.

Observations are weighted by the sum of survey weights across all months within cells defined by region, age, gender, urban/rural survey, and secondary educated mother. Parentheses contain the maximum of OLS and robust standard errors. Dependent variable means are calculated from the 2007–2008 cohorts.

Inference uses a $t(24-4)$ distribution. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B. Empirical appendix

B.1. **Data merging.** The main analysis of this paper uses three administrative datasets:

(1) Records from the testing agency on all 11^{th} graders who took the ICFES college entrance exam in 2002–2011.
(2) Ministry of Education records on students who began in college in 2002–2012.
(3) Earnings records from the Ministry of Social Protection for the year 2017.

We merge datasets (1) and (2) using national ID numbers, birth dates, and names. Nearly all students in both datasets have national ID numbers, but Colombians change ID numbers around age 17. Most students in the ICFES records have the below-17 ID number (*tarjeta*), while the majority of students in the college enrollment records have the above-17 ID number (*cédula*). Merging using ID numbers alone would therefore lose a large majority of students. Instead, we merge observations with 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.

Merge quality is important because our main dependent variable—enrolling in college—is an indicator for a student's appearance in the enrollment dataset. 41 percent of the 2002–2011 ICFES exam takers appear in the enrollment records, which is comparable to the higher education enrollment rate in Colombia during the same time period.[20] A better indicator of merge success is the percentage of college enrollees that appear in the ICFES records because all domestic college students must take the exam. Among enrollees who took the ICFES exam between 2002 and 2011, we match 91 percent.[21]

The combined dataset (1)–(2) was matched to dataset (3) by the Colombian statistical agency *Departamento Administrativo Nacional de Estadística* (DANE). To address the age-17 ID change, DANE also merged these datasets using national ID numbers, names, and birth dates. The fraction of individuals in the 2002–2011 ICFES exam cohorts who were matched to the 2017 earnings dataset is 52 percent. To benchmark this merge rate, we use Colombian household survey data (GEIH) on individuals in the 1985–1994 birth cohorts with at least a high school degree. In this population, the fraction of individuals who worked and

[20] The gross tertiary enrollment rate grew from 25 percent to 43 percent between 2001 and 2012 (World Bank World Development Indicators, https://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. Roughly 20 percent of the secondary school aged population is not enrolled in high school. This would cause our merge rate to be higher than the World Bank's tertiary enrollment rate.

[21] The enrollment records contain age at time of ICFES for some students, which allows us to calculate the year they took the ICFES exam. Approximately 16 percent of students in the enrollment dataset have missing birth dates, which accounts for the majority of observations we cannot merge. Some duplicate matches arise because students took the ICFES exam more than once, though we erroneously match a small number of students with the same birth date and similar names.

Table B1. Higher education institutions in Ministry of Education records

	(A)	(B)	(C)
	# colleges	# exit exam takers/year	% colleges in records
University	122	134,496	100.0%
University Institute	103	53,338	87.6%
Technology School	3	2,041	100.0%
Technology Institute	47	15,092	82.1%
Technical/Professional Institute	35	11,408	98.9%
Total	310	216,375	95.6%

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 percentage of colleges that appear in the Ministry of Education records, where colleges are weighted by the number of exit exam takers.

had a contract for their employment was 54 percent in 2017. This suggests that the DANE merge identified nearly all individuals in our sample with formal sector jobs.

B.2. **Colleges in the Ministry of Education records.** This section describes the colleges that are included in the Ministry of Education records (dataset (2) in Section 3.2). For this we use another administrative dataset from a college exit exam called *Saber Pro* (formerly ECAES). This national exam is administered by the same agency that runs the ICFES entrance exam. The 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 normative program duration.[22] 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 included all Universities but are missing a few colleges that provide more technical training.[23] Overall, 96 percent of exit exam takers attend colleges that appear in the Ministry of Education records.

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

[23] 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 B2. Construction of high school sample

	(A) Unbalanced panel only	(B) Flexible calendar	(C) Final sample
# high schools	8,798	391	4,294
Missing high school	0.07	0.00	0.00
Total # of students	1,815,297	228,587	2,634,245
# students per school & year	20.63	58.46	61.35
ICFES exam percentile	0.47	0.41	0.54

Notes: The sample is 11^{th} graders who took the ICFES in 2002–2011. Column (A) includes high schools that have zero exam takers in any year in 2002–2011. It also includes schools for which we cannot cleanly merge in location and academic calendar information, schools that are listed in different departments or municipalities over time, and observations with missing school information. Column (B) includes high schools that are listed with a "flexible" academic calendar in any year, affected region schools that change calendars before 2010, and schools in other regions that ever change calendars. Column (C) includes the remaining high schools that have exam takers in every year.

# of students per school & year is the total number of students in 2002–2011 divided by the number of high schools divided by 10. ICFES percentiles are relative to all 11^{th} grade exam takers in the same year and are calculated using the average of the scores from the six core components that did not change in 2002–2011: biology, chemistry, language, mathematics, philosophy, and physics.

Another potential issue is that the Ministry of Education's institution coverage has been increasing over time. This could affect the main results if there are differential changes in coverage across regions. Panel B of Figure 4, however, suggests that this is not an issue. This panel depicts the number of college-program pairs that appear in the Ministry of Education records in each academic year. The number of programs is increasing over time due to both program growth and increasing data coverage. But there is no evidence of differential increases between affected and unaffected regions.

In sum, the main results in the paper are likely driven by students forgoing college altogether rather than switching to institutions that are not tracked by the Ministry of Education.

B.3. **Sample of high schools.** This section describes how we select our sample of high schools. Our sample excludes high schools that have zero ICFES exam takers in any year between 2002 and 2011. This includes schools for which we cannot cleanly merge in location and academic calendar information.[24] We also drop high schools that are listed in different departments or municipalities over time. This first set of excluded schools includes the 8,798 schools shown in column (A) of Table B2. It also includes seven percent of all exam takers who have no high school information. These excluded schools are typically either new high schools or less-established private schools that went out of business.

Second, we exclude high schools that are listed as having a "flexible" academic calendar in any year in 2002–2011. A flexible calendar means that students can begin the school

[24] We identify high schools by numeric school IDs, but the ICFES records do not contain these IDs in 2008–2009. We must therefore rely on high school name in these two years, which causes us to drop some high schools that have the same name and time of day as another school.

year in either semester. We also omit schools in the affected regions that change calendars before 2010, and schools in other regions that change calendars in any year. These schools were likely more able to adapt to the academic calendar shift. This excludes the 391 schools shown in column (B) of Table B2.

Our final sample includes the remaining 4,294 high schools in column (C) of Table B2 (see also Table 1). These schools have ICFES exam takers in every year from 2002–2011, and they contain 56 percent of all high school graduates during this time period. Schools in our sample have 61 students per cohort on average and are larger than excluded schools. Their students also perform better on the ICFES entrance exam.

B.4. **Timing of academic calendar transition.** Figure 3 shows the transition from the September to January calendars for the 2007–2012 cohorts at switching schools. We obtained information on transition timing from the following public resolutions by the Secretary of Education in Cali, Valle del Cauca:

- Resolution No. 4211.2.21.2482 of 2006;[25]
- Resolution No. 4143.2.21.4739 of 2008;[26]
- Resolution No. 4143.2.21.5589 of 2010;[27]
- Resolution No. 4443.0.21.10752 of 2010;[28]
- Resolution No. 4143.0.21.9142 of 2012.[29]

Resolutions from the Secretary of Education in the department of Nariño show a similar calendar transition. See the following resolutions:

- Resolution No. 1913 of 2007;[30]
- Resolution No. 1977 of 2008;[31]
- Resolution No. 2800 of 2009;[32]
- Resolution No. 5005 of 2011;[33]
- Resolution No. 4939 of 2011.[34]

[25] Accessed on March 18, 2020 at: http://www.cali.gov.co/descargar.php?id=2946.
[26] Accessed on March 18, 2020 at: http://www.cali.gov.co/lgbt/descargar.php?id=2918.
[27] Accessed on March 18, 2020 at:
https://pregoneustaquiano.blogspot.com/2010/07/aclaracion-la-resolucion-de.html.
[28] Accessed on March 18, 2020 at:
http://files.iejuandeampudia.webnode.com.co/200000170-12ad01302c/manual%20convivencia%20SEP2011.pdf.
[29] Accessed on March 18, 2020 at: https://pregoneustaquiano.blogspot.com/2012/02/.
[30] No longer available online; please contact the authors for a copy.
[31] No longer available online; please contact the authors for a copy.
[32] Accessed on March 18, 2020 at: http://www.actiweb.es/ietasandiego/archivo1.pdf.
[33] Accessed on March 18, 2020 at:
http://www.sednarino.gov.co/SEDNARINO12/index.php/es/normatividad1/category/20-resoluciones.
[34] Accessed on March 18, 2020 at:
http://www.sednarino.gov.co/SEDNARINO12/index.php/es/normatividad1/category/20-resoluciones.