Overlapping Calendars: Colonial Legacies and the Consequences of Misaligned Timing Institutions

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Abstract

This paper studies the economic consequences of conflicts between timing institutions, the rules that govern when schooling, work, and other activities take place, and local economic conditions. We focus on the overlap between school calendars and local agricultural cycles, and document systematic misalignment between the two across countries in southern latitudes. These patterns reflect institutional legacies, as inherited institutions continue to shape the design of school calendars in previously colonized countries. Exploiting this colonial variation as an instrument, our cross-country estimates indicate that greater overlap between school terms and agricultural labor demand increases school dropout rates. To identify the underlying mechanism, we use school-level data to study a calendar reform in Colombia that exogenously shifted the academic calendar of public schools in two departments, raising the overlap for most schools while reducing it for others. Exploiting school-level variation in the change in overlap induced by the reform, we find that calendar misalignment generates a trade-off between schooling and labor: dropout rises and grade promotion falls where overlap increased, while the same outcomes improve where the reform reduced overlap. Our findings highlight the importance of timing institutions as a determinant of human capital accumulation and suggest that the alignment of institutional calendars with local economic conditions has first-order consequences for schooling in agrarian economies.

Keywords: Seasonality, school calendar, dropout, colonial legacies, rural, Colombia.
JEL codes: I25, J22, J24, O12, O13, O43.

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1. Introduction

Institutions shape economic outcomes not only by what they prescribe, but by when they prescribe it. A foundational insight in development economics is that institutions are key determinants of long-run economic performance (North, 1991; Acemoglu et al., 2001). Most of this literature studies *what* institutions do. Property rights, legal systems, and political authority have received sustained attention as drivers of economic outcomes. Yet, institutions also shape behavior through a second dimension that remains largely unexplored. They determine *when* economic activity takes place.

Timing institutions are everywhere. School calendars dictate when children attend class. Office and business hours shape when adults work. Tax cycles and electoral schedules govern when major civic obligations occur. Each of these arrangements coordinates millions of people around a common temporal structure. When the timing they impose aligns with the rhythm of local economic activity, they go unnoticed. When it does not, they generate frictions with relevant consequences for individual decisions and aggregate outcomes. Despite how common they are, the economic effects of timing institutions remain understudied. This paper provides systematic evidence that timing institutions are quantitatively important for economic development. We focus on a particularly consequential one, the school calendar, and study how its misalignment with local agricultural cycles shapes human capital accumulation in agrarian economies.

To assess the scope and consequences of this institutional misalignment, we begin by documenting cross-country patterns. We assemble a data set of school calendars around the world and find that countries in southern latitudes mirror northern school calendars despite facing different climatic conditions. We then compare these calendars with local agricultural timing across countries and find that many of them exhibit substantial overlap between periods when schools are in session and local harvest seasons. This overlap is larger among countries in southern latitudes and former colonies, which often retain school calendars inherited from their colonizers even when geographic and seasonal conditions differ substantially. These patterns are consistent with a broader view in which institutional transfers persist and continue to shape economic outcomes long after the colonial period ends (Acemoglu et al., 2001), and with evidence that colonial education in particular leaves durable legacies (Bolt and Bezemer, 2009), and suggest that school calendar design in some countries is largely determined by historical institutional factors rather than local economic conditions.

To estimate the causal effect of calendar overlap on school dropout, we instrument using colonial institutional legacies. Specifically, we exploit the interaction between the colonizer's school calendar and the agricultural cycles predicted by a country's geography. We find that a one percentage point increase in overlap raises the share of children out of school by 0.6 to 0.8 percentage points, with the lower end of that range coming from specifications that rely only on variation among former colonies. While these estimates are informative about the global scope of the misalignment, they mask relevant within-country heterogeneity and are subject to potential measurement error.

To address this, we turn to a natural experiment in Colombia that exploits within-country variation. This setting allows us to hold constant country-level factors and exploit granular school-level data to study the mechanism behind the cross-country results directly. The policy reform shifted the academic calendars of public schools in two departments (Nariño and Valle del Cauca) from a September-June schedule, mirroring Northern Hemisphere systems, to the January-November calendar used in the rest of the country. Alignment with the national calendar, however, is not alignment with local harvests. Whether the shift raised or lowered the overlap between schooling and peak agricultural labor demand depended on the crop composition surrounding each school.[1] Importantly, the reform did not alter curricula, assessments, or school resources. It only changed the timing of instruction during the year, raising the overlap by 1.1 percentage points on average among affected schools where it rose, while reducing it in others.[2]

Using administrative school census data covering the universe of public rural schools in Colombia, we estimate the causal effect of calendar misalignment by exploiting school-level variation in the change in overlap induced by the reform. Because the change in overlap depends on the interaction between the new calendar and the crop composition surrounding each school, the reform increased overlap substantially in some areas, left it unchanged in others, and even reduced it in some. Our dose-response design compares schools with different exposure to this change, conditional on school and year fixed effects and on differential trends by baseline municipality characteristics. We find that a one-percentage-point increase in the overlap between the school and agricultural calendars raises the dropout rate by 0.7 percentage points and lowers grade promotion by 0.69 percentage points. Because the reform moved overlap in both directions, the net change across all treated schools is small, about 0.2 percentage points, and the relevant magnitudes are distributional rather than aggregate. Among the 60 percent of treated schools where overlap increased, the average increase was 1.1 percentage points, implying a rise in dropout of about 0.8 percentage points for these schools, roughly 18 percent of their pre-reform dropout rate.[3] Among the 31 percent of schools where the reform reduced overlap, the average reduction was larger, 1.5 percentage points, implying a decline in dropout of about 1.1 percentage points. Complementary synthetic difference-in-differences estimates capture the reform's total effect, including components common to all treated schools regardless of their change in overlap. The effects are persistent over time and concentrated among primary-school-aged children in rural areas.

Our results are robust along several dimensions. The estimates are stable across heterogeneity-robust estimators designed for settings with continuous treatment and heterogeneous effects (de Chaisemartin and D'Haultfœuille, 2020), across control groups ranging from the full country to schools within 40 kilometers of the affected departments' boundary, across clustering

[1] De Roux and Riehl (2022) exploit the same reform to study a different question, namely how the resulting academic break between high school and college affected college enrollment and early-career earnings.

[2] The reform also brought potential benefits, including better coordination with national planning cycles, greater teacher mobility across regions, and smoother transitions to higher education. Our analysis estimates the costs of calendar misalignment but is not designed to evaluate the net welfare effect of unification.

[3] Computed relative to the 2010 dropout rate of 4.8 percent among treated schools whose overlap increased.

the standard errors at the school and municipality level, and across alternative definitions of the treatment variable.

Using the within-Colombia natural experiment, we provide two additional pieces of evidence on the mechanism. First, we exploit the fact that the reform modified the overlap between school and agricultural calendars in both directions. Among schools where the unified calendar increased the conflict between schooling and harvesting, dropout rose and grade promotion fell. Among schools where the new calendar happened to better align with local harvesting cycles, the effects flip sign, with dropout falling and grade promotion rising. This symmetry is difficult to reconcile with explanations based on the disruption of the calendar transition itself or with shocks common to the treated departments, which would move outcomes in both groups in the same direction. Second, we use nationally representative household survey data and a difference-in-differences design to estimate the effect of the reform on child labor. We find that the probability of children engaging in rural work rose by approximately 5 percentage points in affected municipalities following the reform, with the increase concentrated among primary-school-aged children, closely mirroring the patterns observed in schooling outcomes. These results provide direct causal evidence of a time-allocation mechanism, in which greater overlap between academic calendars and agricultural cycles raises the opportunity cost of schooling, leading households to substitute children's schooling with labor supply during peak agricultural periods.

The magnitudes from the two identification strategies are expressed in the same units and are remarkably close. The cross-country instrumental variable estimates imply that a one percentage point increase in calendar overlap raises the share of children out of school by 0.6 to 0.8 percentage points. The within-Colombia dose-response estimates imply that the same increase in overlap raises the annual dropout rate by 0.7 percentage points. The two designs do not estimate the same formal parameter. The cross-country outcome is a stock, the share of primary-school-age children out of school, measured at the national level and identified from colonial variation in inherited calendars, whereas the Colombian outcome is an annual flow, the dropout rate among previously enrolled students in rural public schools, identified from a within-country reform. That two estimates of the schooling cost of overlap, obtained from different populations, outcomes, and sources of variation, agree in sign, significance, and magnitude is precisely what makes the convergence informative. It indicates that calendar alignment is a quantitatively important margin for human capital accumulation rather than an artifact of either design. Timing institutions, and not only the content of institutions, shape economic outcomes.

Our study contributes to three strands of the economics literature. First, it contributes to the broad literature on the economic effects of institutions. While a long-standing literature has documented how institutions shape economic outcomes through their content (North, 1991; Acemoglu et al., 2001), debated the relative importance of institutions and human capital (Glaeser et al., 2004), established the primacy of institutions over geography and trade (Rodrik et al., 2004), examined which specific dimensions of institutions matter most (Acemoglu and Johnson, 2005), and studied how they persist over time (Banerjee and Iyer, 2005; Nunn, 2008;

Dell, 2010; Nunn and Wantchekon, 2011; Michalopoulos and Papaioannou, 2013, 2016; Lowes et al., 2017), far less is known about how the timing prescribed by institutions interacts with local economic activity. A small but growing literature has begun to document such effects. Montero and Yang (2022) show that Catholic patron saint day festivals in Mexico reduce long-run economic development when their fixed dates coincide with peak planting or harvest periods. A complementary set of papers documents how school calendars shape labor market outcomes for parents, particularly mothers (Price and Wasserman, 2024; Graves, 2013; Duchini and Van Effenterre, 2022).

We contribute to this literature by showing that institutional timing is a quantitatively important determinant of economic outcomes. We provide systematic evidence that the timing of school calendars shapes children's human capital accumulation, and that timing institutions can be inherited and persist long after independence, producing more prevalent misalignment in former colonies in southern latitudes. Academic years in many of these countries continue to follow European templates rather than local agrarian cycles, illustrating how colonial institutions shape contemporary outcomes through seemingly minor features. The patterns we document are consistent with colonial inheritance as the reason this calendar misalignment exists and persists. Our estimates quantify its consequences.

Second, we contribute to the education and child labor literature by showing that calendar misalignment is an important and underexplored determinant of school dropout in agrarian settings (Basu and Van, 1998; Baland and Robinson, 2000; Edmonds and Pavcnik, 2005; Beegle et al., 2006; Edmonds, 2007). Existing work largely treats schooling decisions as driven by income shocks and liquidity constraints (Watkins, 2000). The studies closest to our focus examine how the school calendar interacts with agricultural seasonality along particular margins. Dillon (2021) shows that a shift in Malawi's school calendar changed the timing of school-related expenses relative to the harvest, forcing poor households to sell crops early at low prices, and Ito and Shonchoy (2026) show that annual exams coinciding with the harvest season raise school dropout among children from agricultural households in South Asia. We build on this work by treating the overlap between instructional time and agricultural labor demand as an institutional design feature, tracing its colonial origins, and estimating its causal effects on schooling with both cross-country and within-country variation.

We build on this work in ways that are both broader and more precise. Our cross-country analysis provides the first systematic evidence that school calendars are misaligned with local agricultural cycles among former colonies, that this misalignment reflects colonial institutional legacies rather than local conditions, and establishes a causal link between calendar overlap and dropout worldwide. Our within-country design adds unusual precision by exploiting a policy change that unified a dual public school calendar system into a single national calendar, creating a direct and contemporaneous counterfactual. Unlike prior studies, we examine a permanent calendar change covering the entire public school system and trace its effects on dropout and progression. To the best of our knowledge, the Colombian case is the only one in which two parallel public school calendars, formally operated at the national level, were unified through a single policy reform. Furthermore, finding consistent magnitudes across

two independent identification strategies strengthens the case that institutional timing is a first-order and previously overlooked determinant of human capital accumulation.

Two concurrent working papers study related questions. Merfeld (2023) shows that harvest-school overlap mediates the effect of rainfall shocks on child labor across a large set of developing countries. Allen (2024) uses a calendar reform in Malawi in a shift-share design and finds that greater overlap between school and farming calendars reduces grade advancement, corroborating the schooling costs of misalignment in a different setting. Our paper differs in three respects. First, we study the origins of misalignment. The cross-country analysis documents that calendar-harvest overlap is systematically higher in former colonies and traces it to the inheritance of the colonizers' calendars. Second, the Colombian reform generates two-sided variation given that the same policy increased overlap for some schools and reduced it for others, allowing us to show that outcomes deteriorate where alignment worsened and improve where it strengthened, a sign reversal that one-directional designs cannot deliver and that ties the effects to the overlap channel itself. Third, our estimates come from the administrative census of schools, covering the universe of rural public schools over more than ten years.

Third, we contribute methodologically by combining cross-country historical variation with within-country quasi-experimental evidence to study both the existence and the mechanism of the effect. More broadly, we connect historical institutional design to contemporary household behavior through a novel timing channel. At the macro level, we exploit variation in colonizer calendars and geography to instrument for school-harvest overlap and estimate its impact on schooling across countries. At the micro level, we leverage a natural experiment in Colombia that isolates the timing mechanism by shifting the academic calendar without altering other aspects of schooling. Although prior work has examined the effects of instructional time and school schedules (Pischke, 2007; Marcotte, 2007; Goodman, 2014; Bostwick et al., 2022; De Roux and Riehl, 2022), we are the first to link inherited calendar design to local economic conditions and to study its consequences using both cross-country and within-country variation. Together, our findings suggest that institutional timing reforms, such as aligning school calendars with local economic conditions, can substantially improve schooling outcomes at low cost.

The remainder of the paper is organized as follows. Section 2 documents systematic misalignment between school and agricultural calendars across countries, links it to colonial institutional legacies, and presents instrumental variable estimates of the effect of calendar overlap on school dropout. Section 3 describes the institutional setting of the Colombian education system, its academic calendars, and introduces the calendar reform studied in this paper. Section 4 presents the data. Section 5 outlines the empirical strategy. Section 6 presents the main school-level results with their corresponding robustness checks, and Section 7 presents causal evidence on how the calendar reform affected child labor market outcomes. Section 8 reconciles the evidence from both strategies and discusses the scope of the results. Finally, Section 9 concludes.

2. Worldwide Evidence on the Effects of Calendar Overlap

2.1. School Calendars Across Latitudes

School calendars were institutionalized during the 19th century, when European states formalized public primary education through foundational school laws that established centralized, state-regulated systems. These laws created the administrative apparatus through which the structure of the school year, including its term dates and vacations, was subsequently standardized and enforced.[4] These laws required municipalities to maintain public schools and defined the structure of the school year, limiting the time available for work outside the classroom. Compulsory attendance legislation came later, including the Mundella Act (1880) in the UK and the Jules Ferry Laws (1882) in France, but by then the institutional template of the school calendar had already been established (Gold, 2002; Lelièvre, 2021).

Attendance concerns pushed school calendars toward minimizing conflicts with agricultural cycles, with instruction concentrated in periods of low agricultural activity (Pedersen, 2012), although other factors, including religious festivities and urban conditions, also shaped the timing of the school year. As a result, the academic year typically began after the summer, allowing students and their families to participate in harvest activities.[5] Since then, and until current times, most Northern Hemisphere countries have adopted a school calendar that begins in September or October, following the summer, and ends around May or June.

However, the notion of "summer" does not translate uniformly across latitudes, which implies that school calendars need not follow the same structure everywhere. Equatorial countries lack clearly defined seasons, producing substantial within-country variation in climate and harvest schedules. Countries in the Southern Hemisphere experience summer from December to February, which would naturally imply a school calendar offset relative to northern countries.

The adoption of school vacations during the northern summer, therefore, should vary depending on the latitude of the country. We assemble a worldwide dataset of school calendars to document and analyze these patterns, and plot current school calendars by country sorted by latitude in Appendix Figure A.1.[6] Northern countries consistently schedule vacations in July and August. Column 1 of Table 1 confirms this by regressing a dummy for whether the country schedules its main vacation during the northern summer on latitude.[7] As expected, being located at a northern latitude increases the probability of adopting the northern summer

[4] Particularly, France enacted the Loi Guizot in 1833, Belgium the Loi Nothomb in 1842, Spain the Ley Someruelos in 1838, the Netherlands the Schoolwet in 1806, and the United Kingdom the Forster Act in 1870.

[5] Additional evidence suggests that the emergence of summer breaks was also influenced by the aristocratic hunting season in France (Lelièvre, 2021), as well as by urban social habits, budgetary constraints, and the preferences of school officials in the United States (Gold, 2002; Weiss and Brown, 2003).

[6] School calendars were hand-collected from the official websites of the ministries of education, or equivalent government bodies, of each country in our sample, using the most recently available academic calendar at the time of data collection. More details are provided in Appendix B.1.

[7] Latitude is defined as positive values in the north and negative in the south.

vacation.[8]

Table 1: Adoption of the Northern-Hemisphere Summer Vacation

Dependent variable: Main vacation in NH summer			
	(1)	(2)	(3)
Latitude	0.012***		0.014***
	(0.001)		(0.001)
Colony		-0.317***	0.190**
		(0.074)	(0.087)
Observations	120	120	120
Mean dep. var.	0.683	0.683	0.683
R-squared	0.380	0.102	0.400

Notes. The table reports OLS estimates. The dependent variable is an indicator equal to one if a country's main school vacation falls during the northern summer, classified by the authors from each country's school calendar. Latitude is measured in degrees, with positive values north of the equator. *Colony* is an indicator for countries colonized by Belgium, France, the Netherlands, Portugal, Spain, or the United Kingdom. The six colonizing countries are themselves coded as zero. School calendar data were assembled by the authors from national ministries of education. Robust standard errors in parentheses. ***, **, * denote significance at the 1, 5, 10 percent significance level.

2.2. Colonial Origins of the School and Agricultural Calendar Overlap

Despite latitude predicting summer vacation adoption, Appendix Figure A.1 also shows that many countries around the Equator and in southern latitudes mirror the northern calendar regardless of their climatic conditions. Many of these countries were historically colonized and inherited their colonizer's calendar rather than one suited to local conditions. Appendix A.1 documents this misalignment systematically, showing that harvest timing reverses by roughly six months across the equator while the school vacations of former colonies sit progressively farther from local harvests. Column 2 of Table 1 shows that formerly colonized countries are on average less likely to adopt the northern summer vacation. This unconditional difference reflects location rather than behavior, since most former colonies lie near the Equator or in the southern hemisphere, where a northern summer break is climatically unwarranted. Importantly, once we condition on latitude in Column 3, the relationship reverses. Conditional on latitude, a former colony is on average 19 percentage points more likely to schedule its main vacation during the northern summer than a never-colonized country in the same location. Senegal and Thailand illustrate the pattern. The two countries lie at nearly the same latitude, yet Senegal, a former French colony, schedules its long vacation during the northern summer, mirroring the French calendar, while Thailand, which was never colonized, places its long

[8]We exclude Liberia from the analysis. Liberia was established in 1822 by the American Colonization Society as a settlement for freed African Americans and was never colonized by a European power, so it does not map into our classification. It is neither a colony of the European colonizers whose calendar inheritance we study nor a country whose institutions developed without external transplantation. Indeed, Liberia adopted a US-style school calendar, making it a case of institutional inheritance from outside our set of colonizing countries.

break earlier in the year, during its own hottest season. This conditional difference is difficult to explain with geography and is consistent with the inheritance of the colonizers' calendars.

These patterns suggest that school calendars in former colonies may reflect those of the colonizing country rather than the pattern suggested by latitude. Figure 1 plots the monthly share of school days in former colonies and compares them with those of their colonizers (plotted in the first row of the figures). Colonial calendars frequently persist despite substantial differences in climate and agricultural cycles. Countries colonized by France, the United Kingdom, Belgium, and the Netherlands show a high degree of persistence relative to the school calendar of their colonizer. In contrast, Spanish and Portuguese colonies appear to have adapted their school calendars to local conditions. These patterns suggest that school calendars in some southern countries were shaped more by the institutional practices of their colonizers than by local agricultural conditions.

The extent to which colonial calendars persisted seems to depend on the degree of institutional autonomy that former colonies had when their school calendars were established. For example, Spanish and Portuguese colonies, which gained independence early, were able to adapt their calendars to local agricultural conditions. In contrast, countries colonized by France, the United Kingdom, or the Netherlands, which achieved independence after school calendars had already been established by colonial authorities, were more likely to retain the inherited structures. This suggests that the timing of independence played a critical role in shaping the alignment between school calendars and local agricultural cycles.

Figure 2 provides empirical evidence for the relationship between independence timing and calendar persistence. It plots the similarity between the school calendars of colonies and their colonizer against the number of years between the institutionalization of the colonizer's education system and the colony's independence, with positive values indicating independence after institutionalization.[9] We observe a strong positive correlation. Countries that gained independence later were more likely to retain colonial calendar structures, while those that gained independence earlier show less similar calendars.

Two notable examples of this mechanism are Spain and France. Most Spanish colonies, including Colombia which we will emphasize in Section 3, gained independence well before educational systems were formally institutionalized in Spain (in 1838). This early independence allowed these countries to establish their own calendars rather than inherit those of the colonizer. In contrast, former French colonies gained independence only after school systems had already been institutionalized in France and therefore tend to retain calendar structures that closely mirror those of the former colonizer. These examples highlight how the timing of independence shapes the alignment between school calendars and local conditions, driving the broader pattern shown in Figure 2.

[9]Similarity between both school calendars is computed as the share of the year during which students in both the colony and the colonizing country are simultaneously on vacation. We use vacation day overlap rather than school day overlap because the number of instructional days is similar across countries by construction, leaving little variation to exploit. Vacation timing, by contrast, reflects specific institutional choices more likely to carry the imprint of colonial educational transfer.

Figure 1: Monthly School-Day Shares by Colony and Colonizer

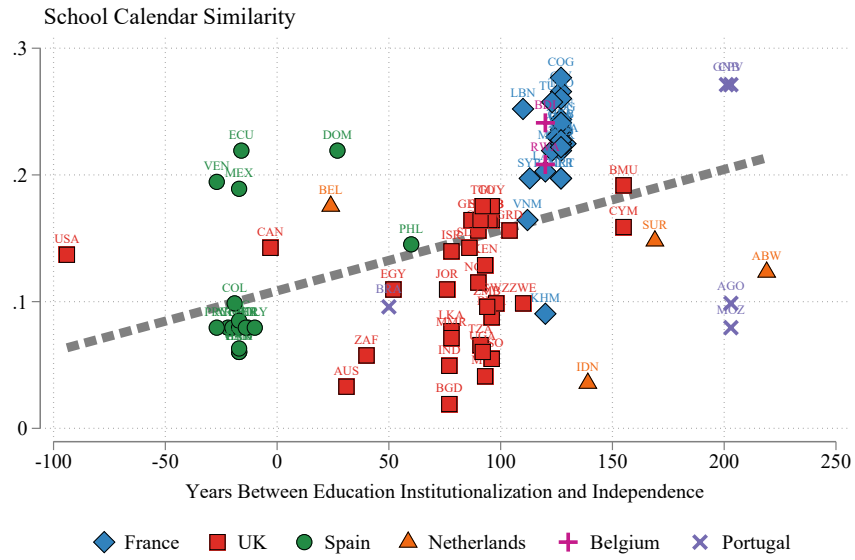


Notes: The figure shows the share of school days in each month. For each country, the information corresponds to the most recent school calendar data available. Source: Own elaboration based on official sources.

2.3. Calendar Overlap and School Dropout

The persistence of colonial school calendars has important consequences for education today. These historical patterns suggest a potential mismatch between school and agricultural calendars, particularly in formerly colonized countries located at latitudes that differ from

Figure 2: Colonial Legacy and School Calendar Overlap



Notes: The y-axis reports the share of the year during which students in both the colony and the colonizing country are simultaneously on vacation. The x-axis displays the difference in years between the independence date of each colony and the date on which the first foundational educational law was enacted in the corresponding colonizer. Positive values indicate that the colony gained independence after the colonizer institutionalized its education system, so larger values correspond to longer exposure to the colonizer's educational framework. Countries with missing independence or education-law dates are excluded. For territories that have not become independent, we use the present year, so their values reflect uninterrupted exposure through today. Source: Own elaboration based on official and historical sources.

those of their colonizers. This mismatch may, in turn, negatively affect present-day educational outcomes by raising the opportunity cost of attending school during periods of peak agricultural labor demand.

We formally evaluate this by computing $O_i \in [0, 1]$, the share of days in the year on which the agricultural and school calendars overlap in each country i:

$$O_i = \frac{|\mathbb{S}_i \cap \mathbb{A}_i|}{|\mathbb{D}|},$$

where $\mathbb{D}$ denotes the set of days in a calendar year, $\mathbb{S}_i \subseteq \mathbb{D}$ denotes the set of days when school is in session in country i, and $\mathbb{A}_i$ denotes the set of days corresponding to the harvesting season of country i's main crop. The cardinality in the intersection of these two sets, $|\mathbb{S}_i \cap \mathbb{A}_i|$, captures the number of days in which students are required to attend school during periods of high agricultural activity.[10] On average, both calendars overlap 15% of the year, corresponding to, roughly, 54 days.[11]

Calendar overlap varies systematically with colonial status and geography. Table 2 reports the results from regressions of calendar overlap on an indicator for former colonial status and countries' latitude. The estimates show that calendar overlap is significantly higher in

[10] Agricultural calendars are constructed based on data from Sacks et al. (2010), which provides crop-specific harvesting dates at the country level. We use the calendar of the main crop in each country, defined as the crop with the largest harvested area among the crops covered by the crop calendar data. More details are provided in Appendix B.1.

[11] Appendix Table A.2 provides descriptive statistics of these measure.

former colonies and significantly lower in countries located at higher latitudes. These results are robust to the inclusion of alternative sets of controls. Although descriptive, they point to systematic misalignment between school calendars and local agricultural cycles in countries that were formerly colonized and are located in southern latitudes.

Table 2: School and Agricultural Calendar Overlap by Colonial Status and Latitude

	Dependent variable: Calendar Overlap					
	(1)	(2)	(3)	(4)	(5)	(6)
Colony	0.091***		0.061***	0.065***	0.066***	0.059**
	(0.016)		(0.023)	(0.024)	(0.025)	(0.025)
Latitude		-0.002***	-0.001*	-0.001**	-0.001*	-0.001*
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	120	120	120	120	120	120
Mean dep. var.	0.15	0.15	0.15	0.15	0.15	0.15
Macroeconomic controls	No	No	No	Yes	Yes	Yes
Demographic controls	No	No	No	No	Yes	Yes
Trade controls	No	No	No	No	No	Yes

Notes. Macroeconomic controls: GDP, consumption share of GDP, and agricultural value added. Demographic controls: population. Trade controls: exports and the total trade volume between each country and the set of colonizers, comprising Belgium, France, Portugal, Spain, the Netherlands, and the UK. ***, **, * denote significance at the 1, 5, 10 percent significance level. Source: own elaboration based on administrative records, World Bank Indicators, Penn World Table version 11.0, and Sacks et al. (2010).

This increased overlap has relevant consequences. It raises the opportunity cost of schooling for rural students, particularly in agrarian economies. When schooling schedules conflict with periods of high labor demand, households may reallocate children's time toward farm work, affecting schooling decisions. To illustrate this relationship, we correlate the measure of calendar overlap with country-level school dropout rates and present the results in Figure 3a.[12] Consistent with this mechanism, countries with greater overlap between school and agricultural calendars exhibit higher school dropout rates.

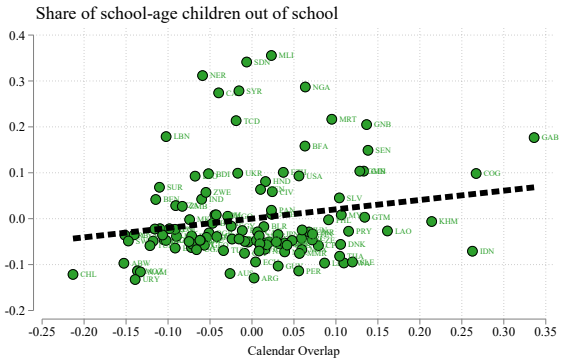
This positive relationship, nonetheless, is almost entirely driven by countries with a colonial legacy. Figure 3b separates countries with and without a colonial past, revealing a steeper gradient among former colonies and little to no correlation among countries with no colonial history.[13] Calendar overlap is associated with school dropout primarily in contexts where the school calendar was inherited rather than locally designed.

These correlations should not be interpreted as causal. Unobserved factors may influence both calendar structure and educational outcomes, generating omitted variable bias. In particular, low institutional quality, itself a legacy of colonialism, may drive both poor calendar design and higher dropout rates, so the colony coefficient in Table 2 and the gradients in Figure 3 likely absorb variation in a wide range of colonial institutional features beyond the school calendar (Rodrik et al., 2004; Acemoglu and Johnson, 2005). The differential gradient in Figure 3b is consistent with our interpretation but does not uniquely identify it, as countries

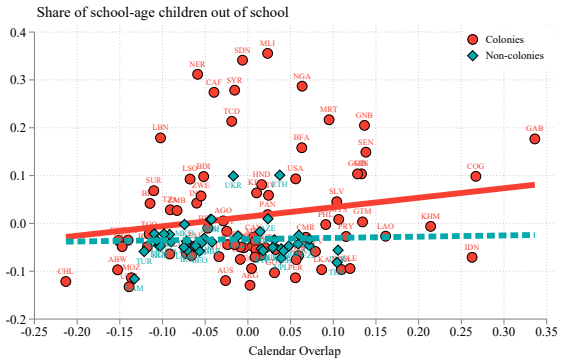
[12] We condition on latitude and GDP to isolate variation that is not driven by geography and income levels.
[13] Appendix Table A.3 shows this relationship when varying the set of controls used.

Figure 3: School Dropout and the Overlap Between School and Agricultural Calendars

(a) All countries



(b) Colonies vs Non-Colonies



Notes: Each panel plots the share of primary-school-age children out of school against the overlap between school and harvesting calendars, with both variables residualized on latitude and GDP. Residuals are computed from pooled regressions on the common estimation sample. In Panel b, fitted lines are estimated separately for former colonies and countries without a colonial past. Source: own elaboration based on administrative records, World Bank Indicators, Penn World Table version 11.0, and Sacks et al. (2010).

with high overlap may simply have lower institutional capacity in general, or non-colonized countries with high overlap may have compensating advantages that attenuate the effect. In addition, dropout rates could themselves affect the design of school calendars, creating the potential for reverse causality.

To estimate causal effects, we exploit historical colonial legacies and instrument calendar overlap in former colonies using the coincidence between the school calendar of the colonizing country and the agricultural calendar predicted by latitude. Let $c(i)$ denote the country that colonized country i and let $\mathbb{L}_i$ denote the set of countries located in the same latitude zone as country i.[14] We construct the following instrument:

$$Z_i = \frac{1}{|\mathbb{D}|} \sum_{d \in \mathbb{D}} S_{c(i),d}\ \hat{p}_{id},$$

where $S_{c(i),d}$ is an indicator equal to one if schools are in session in the colonizing country $c(i)$ on day d, $\hat{p}_{id} \in [0,1]$ is the predicted probability that a country with the agricultural conditions of country i is harvesting on day d, and $\mathbb{D}$ denotes the days of the year. The instrument is thus the expected share of the year during which the colonizer's school calendar

[14] Latitude zones are defined as: Equatorial (0–10°), Tropical (10–23.5°), Subtropical (23.5–35°), Temperate (35–50°), and Cool Temperate (50–66.5°).

coincides with predicted harvesting activity.[15] For countries that were not colonized the instrument takes the value of zero.[16] On average, both calendars overlap 12% of days in the year (see Appendix Table A.2).

Using this instrument, we estimate the following equations by two-stage least squares:

$$\begin{aligned} \mathrm{O}_i &= \alpha_0 + \alpha_1 Z_i + \mathbf{X}_i'\boldsymbol{\pi} + u_i \\ \mathrm{Y}_i &= \beta_0 + \beta \hat{\mathrm{O}}_i + \mathbf{X}_i'\boldsymbol{\gamma} + \varepsilon_i, \end{aligned} \tag{1}$$

where $\mathrm{Y}_i \in [0,1]$ corresponds to the share of primary school age children who are out of school in country i, $\hat{\mathrm{O}}_i$ denotes the predicted overlap between school and agricultural calendars, and $\mathbf{X}_i$ denotes a vector of country level controls, included in both stages.[17] Standard errors are heteroskedasticity-robust.

The coefficient $\hat{\beta}$ in Equation 1 is consistently estimated if the instrument Z_i satisfies the exclusion restriction. This requires that the school calendars of former colonial powers affect school dropout in their former colonies only through the school calendars that these colonies inherited, and not through any other channel. This assumption is plausible because colonial powers originally designed their domestic school calendars to reflect their own climatic and agricultural conditions rather than those of the territories they later administered, making the timing of school terms in the colonizing country unlikely to be directly related to contemporary educational outcomes in former colonies located at very different latitudes. The instrument further combines this colonial school calendar with agricultural cycles predicted by latitude peers, which captures the agricultural conditions that country i would likely face in the absence of colonial institutional influence and prevents the instrument from being mechanically correlated with country-specific agricultural patterns. Together, these two components isolate variation that is specific to calendar timing and should be orthogonal to other dimensions of colonial institutional inheritance such as property rights, legal origin, or state capacity (Rodrik et al., 2004; Acemoglu and Johnson, 2005). One margin of the instrument nonetheless deserves separate scrutiny. Because Z_i equals zero for countries that were never colonized, the full-sample estimates draw in part on the comparison between former colonies

[15] $\hat{p}_{id}$ is constructed as a leave-one-out estimate of the probability that a country harvests on a given day of the year. For each day d, we estimate the probability that a country growing the same main crop as country i and located in the same latitude zone is actively harvesting on day d, using all countries in that peer group except country i itself. Formally, for each cell combination of country j and day d we compute $\hat{p}_{id} = \sum_{j \in \mathbb{L}_i^c \setminus \{i\}} \mathbf{1}[\text{harvest}_{jd}] / (|\mathbb{L}_i^c| - 1)$, where $\mathbb{L}_i^c \subseteq \mathbb{L}_i$ denotes the set of countries sharing both the same main crop and the same latitude zone as country i. When the peer group contains fewer than two countries, we progressively widen the comparison group first to all countries in the same hemisphere growing the same crop, and then to all countries growing the same crop globally. The resulting daily measure $\hat{p}_{id} \in [0,1]$ reflects the predicted probability of harvest on each day of the year for a country with similar agroclimatic conditions to country i, purged of country i's own harvest schedule to avoid mechanical correlation.

[16] In Appendix B.2 we consider a second instrument that combines the interaction between the school calendar of the colonizing country, the agricultural calendar predicted by their latitude, and the degree of exposition to colonial education institutions. This second instrument resembles a shift-share design with both the *shift* and the *share* components being arguably orthogonal. Our results are nearly identical using both designs.

[17] Controls include (i) latitude, (ii) GDP, the consumption share of GDP, and agricultural value added, (iii) population, and (iv) exports in USD and the total trade volume between each country and the set of colonizers, comprising Belgium, France, Portugal, Spain, the Netherlands, and the UK.

and never-colonized countries, a contrast that may correlate with broader institutional legacies. We address this in two ways. Column 5 of Panel A adds an indicator for colonial status to both stages, so that the estimate no longer draws on the difference between colonized and never-colonized countries. Panel B removes the margin by restriction rather than by conditioning, estimating on former colonies alone, so that identification comes only from variation in inherited calendars among colonized countries.

Table 3 presents the results of estimating Equation 1.[18] The estimated coefficients are positive and precisely estimated across specifications. Columns 1 to 4 imply that a one percentage point increase in the overlap between school and agricultural calendars increases school dropout by approximately 0.77 to 0.79 percentage points. Column 5 adds the colonial status indicator and the coefficient falls to 0.56, remaining positive and significant at the 5 percent level, with a first-stage F of 12.6. Panel B restricts the sample to former colonies, where identification relies entirely on variation across the degree of calendar mismatch rather than on the comparison between colonized and non-colonized countries, and the coefficients range from 0.48 to 0.63. The two exercises deliver similar answers, which is what the structure of the instrument leads us to expect. Because Z_i is zero for every never-colonized country, conditioning on colonial status leaves those countries with no residual variation in the instrument, so column 5 recovers essentially the same identifying variation as Panel B while retaining the full sample for the estimation of the controls. We treat these estimates as the more conservative benchmark, since they are immune to any confound operating through colonial status itself. All estimates in both panels remain stable across different sets of controls, supporting a robust causal relationship between calendar overlap and school dropout.

The estimates should be interpreted with the source of identifying variation in mind. The instrument isolates variation in calendar overlap that originates in colonial institutional inheritance, so the estimates are informative about the effect of overlap in countries whose school calendars were shaped by colonial institutions, and are weighted toward countries whose overlap responds most strongly to that inheritance. This is true of every column, and most transparently of those that remove the colonized versus never-colonized contrast, where former colonies supply all of the identifying variation. They need not extrapolate to countries whose calendars were designed domestically. In contexts where inherited school calendars do not align with local agricultural conditions, students face higher opportunity costs of schooling and are more likely to drop out.

These results provide strong causal evidence of the relationship between calendar overlap and school dropout, driven by countries with a colonial past. However, while the instrumental variable strategy addresses potential endogeneity in school calendar design, cross-country estimates remain inherently aggregated and their interpretation might be limited. The analysis relies on national averages that mask substantial within-country heterogeneity in agricultural cycles, school calendars, and schooling decisions. In addition, both agricultural and school calendars might be measured with error at the country level, as harvesting seasons and school schedules often vary across regions within a country. For these reasons, we complement the

[18] The first stage results are presented in Appendix Figure A.3.

Table 3: Effect of Calendar Overlap on Dropout

	Dependent variable: Dropout rate				
	(1)	(2)	(3)	(4)	(5)
Panel A: Full sample					
Calendar Overlap	0.786***	0.781***	0.772***	0.785***	0.558**
	(0.280)	(0.250)	(0.243)	(0.251)	(0.282)
First-stage F	19.58	18.47	18.74	17.49	12.56
Observations	120	120	120	120	120
Panel B: Former colonies					
Calendar Overlap	0.484*	0.591**	0.590**	0.630**	
	(0.289)	(0.293)	(0.285)	(0.305)	
First-stage F	12.81	12.19	12.61	11.18	
Observations	81	81	81	81	
Geographic controls	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes
Trade controls	No	No	No	Yes	Yes
Colonial status control	No	No	No	No	Yes

Notes. The dropout rate is defined as the share of primary school-age children out of school. For each country, we use the latest available value during the 2010–2019 period. Geographic controls: country latitude. Macroeconomic controls: GDP, consumption share of GDP, and agricultural value added. Demographic controls: population. Trade controls: exports and the total trade volume between each country and the set of colonizers, comprising Belgium, France, Portugal, Spain, the Netherlands, and the UK. The colonial status control is an indicator for former colonial status, included in both stages. It applies to Panel A only, since it is constant within the sample of former colonies and therefore drops in Panel B, which has four columns. ***, **, * denote significance at the 1, 5, 10 percent significance level. Source: own elaboration based on administrative records, World Bank Indicators, Penn World Table version 11.0, and Sacks et al. (2010).

cross-country results with micro-level evidence from a within-country natural experiment in Colombia, where a school calendar reform allows us to trace the effect of calendar misalignment while holding other institutional features constant. This design isolates the calendar timing channel from any broader colonial institutional legacy and provides a more direct test of the mechanism we propose.

3. A Natural Experiment: Colombia's School Calendar Reform

Colombia provides a within-country test of the mechanism identified in the cross-country analysis. It is a country near the Equator that provides an ideal historical setting to study the relationship between school and agricultural calendars. The country was colonized by Spain and gained independence in 1819. Spain formally institutionalized its national school calendar only later, which allowed Colombian republican authorities to design their own school schedule after independence.[19] Because of substantial regional variation and political constraints, Colombia adopted a dual school calendar during the twentieth century, in which different areas of the country operated under two opposing school calendars. This system was

[19] We date the institutionalization of Spain's education system to the *Ley de Instrucción Primaria* of 21 July 1838 (*Ley Someruelos*), which established the first functioning liberal primary school system in Spain, requiring municipalities to maintain public schools and introducing teacher certification and provincial supervision of education. This framework was later consolidated by the *Ley de Instrucción Pública* of 9 September 1857 (*Ley Moyano*), which introduced compulsory and free elementary schooling. Since most Spanish colonies, including Colombia, gained independence in the 1810s and 1820s, they did so well before either of these laws was enacted, giving Colombian republican authorities the freedom to design their own school calendar.

unified in 2010 under a single national calendar. This reform increased the overlap between the school calendar and local agricultural harvesting periods in some granular regions of the country. We exploit this institutional change to estimate the effect of school and agricultural calendar overlap on schooling outcomes.

3.1. The Colombian Education System and the Dual School Calendar

Education in Colombia is organized into three levels: (i) primary education (grades 1–5); (ii) lower secondary education (grades 6–9); and (iii) upper secondary education or high school (grades 10–11). Schooling is provided by both public and private institutions. In 2010, approximately 80% of all students were enrolled in public schools, and 20% attended schools located in rural areas.

Unlike countries with centralized and fixed school calendars, the academic year in Colombia can begin either in January and end in November or in September and end in June. The calendar that begins in January and ends in November is known as *Calendar A*, while the calendar that begins in September and ends in June is known as *Calendar B*.[20] Regardless of the calendar, all schools are required to complete 40 weeks of instruction per academic year (Congreso de la República de Colombia, 1994).

Since the early years of the Colombian republic, the academic calendar has undergone substantial transformations. During the colonial period (i.e., before 1819), education in the Spanish colonies was primarily provided by the Church in a decentralized manner and was accessible only to a small elite. In the late eighteenth century, the Spanish Crown introduced the Bourbon reforms to modernize state capacity in the colonies (Chiovelli et al., 2023). As part of these reforms, and influenced by Enlightenment ideas, Carlos III curtailed the Church's influence over education and promoted the expansion of free primary schooling in Spain (Anguita Osuna, 2019). In the colonies, these reforms translated into far more limited changes. During this period, however, the school calendar played little role in shaping educational access.

Following independence in the 1820s, the Colombian state moved early to regulate the school calendar, anchoring school vacations to established national recesses in 1824, defining the structure of the academic year explicitly in the General Law of 1826, and formalizing a nationally uniform two-term structure in the Organic Decree of 1870 (see Appendix C). Through the late nineteenth century the system remained largely standardized around a year running from January or February to November, with occasional institutional exceptions. Regional differentiation emerged in the early twentieth century, when departmental regulations began to prescribe distinct academic years for the southwest, and it is this differentiation that mid-twentieth-century legislation ultimately codified.

The dual calendar structure was formalized in the mid-twentieth century. Decree 75 of

[20] School breaks depend on the adopted calendar. Schools following *Calendar A* have a one-month break between June and July and a longer break between November and January. Schools following *Calendar B* have a one-month break between December and January and a longer break between June and September. Both calendars include one-week breaks around April and October.

1951 established a fixed academic calendar for public schools and formally introduced two regional calendar regimes. Schools in the departments of Nariño and Valle del Cauca were required to begin the school year in October, while schools in the rest of the country started in February. This decree marked the first nationwide recognition of a dual calendar structure, codifying in national law the regional differentiation that had developed over the preceding decades.

Subsequent attempts to standardize class periods nationwide faced substantial opposition and were later revised through Decree 1902 of 1969.[21] The latter decree cited climatic conditions in Valle del Cauca, Nariño, and Cauca that rendered the proposed calendar impractical, as well as requests from principals, teachers, and parents for longer end-of-year vacations and shorter mid-year breaks. This resistance from communities in the southwest suggests that climatic, cultural, and logistical factors played a central role in sustaining regional differentiation in academic calendars. Consequently, since the mid-twentieth century, all public schools in Colombia followed the January–November calendar, except those in the southwest, where public schools traditionally operated under the September–June calendar.[22]

The school calendar reform. Years later, Law 715 of 2001 introduced a decentralization reform that assigned local education authorities responsibility for organizing education provision. This included defining the academic calendar and granted certified municipalities autonomy over education provision.

This increased flexibility led authorities in Nariño and Valle del Cauca to modify the public school calendar because the September–June calendar spanned two fiscal years and complicated budget programming and execution. After 2010, all public schools in these two departments, therefore, were required to transition to the January–November calendar to eliminate this mismatch (De Roux and Riehl, 2022).[23][24] During this transition, public schools maintained the required 40 weeks of instructional time. Private schools, by contrast, have always been free to choose which calendar to adopt nationwide.[25]

Figure 4a plots the share of public schools adopting the September-June calendar in the departments of Nariño and Valle del Cauca (here onward "affected" departments) compared with the rest of the country. Prior to 2010, the period preceding the calendar reform, virtually

[21] Most notably, Decree 1816 of 1967 established uniform academic semesters nationwide, regardless of the calendar followed, the only difference concerned the semester in which schools began. The first semester started on the third Monday of January and lasted 95 instructional days (excluding Saturdays), followed by a mid-year vacation until the Sunday preceding the fourth Monday of July. The second semester began on the fourth Monday of July and also lasted 95 instructional days, with the end-of-year vacation extending until the Sunday preceding the third Monday of January.

[22] Appendix C provides a detailed description of the legal evolution of school calendars since the colonial period.

[23] The transition was implemented through resolutions issued by each certified territorial entity under the authority granted by Law 715 of 2001. Resolution 4143.2.21.4739 of 2008 of the Secretaría de Educación Municipal of Santiago de Cali is one such implementing act, covering public schools in that municipality (Santiago de Cali. Secretaría de Educación Municipal, 2008).

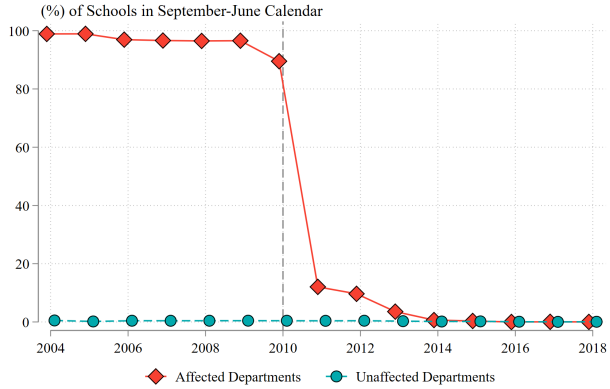
[24] Beyond administrative coordination, unification could have also facilitated teacher mobility across departments and smoother student transitions to higher education. Our analysis estimates the costs of the calendar change but is not designed to evaluate its net welfare effect. We discuss implications about this in Section 8.

[25] De Roux and Riehl (2022) provide a more detailed discussion of the school calendar reform.

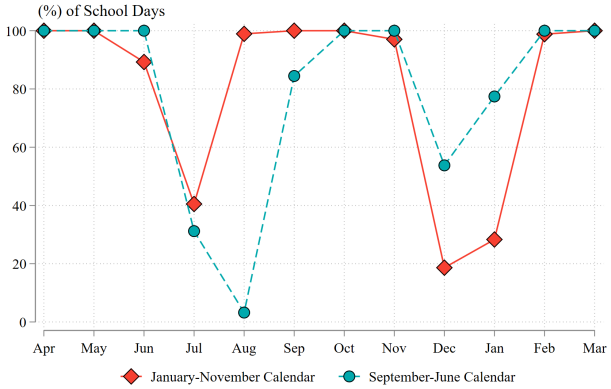
all public schools in the affected departments operated under the September-June calendar, whereas public schools in the rest of the country consistently followed the January-November one. Beginning 2011, and following the implementation of the calendar reform, public schools in Nariño and Valle del Cauca transitioned to the January-November calendar, aligning their academic schedules with those used elsewhere in the country. The transition was rapid but not instantaneous. Close to ninety percent of affected public schools switched in 2011, a share that exceeds 96 percent among the rural public schools that constitute our estimation universe, and the remainder followed over the next years, with full convergence by 2014.[26]

Figure 4: The School Calendar Reform

(a) Schools (%) under Sept.-June Calendar



(b) Calendar Shift in Affected Departments



Notes: Panel (a) shows, for each year, the share of public schools operating under the September–June calendar. The shares are reported separately for schools in Nariño and Valle del Cauca (red dots) and for schools in the rest of the country (blue dots). The rural public schools that constitute our estimation universe transitioned faster than the figure suggests, with 96.7 percent on the January–November calendar in 2011. Panel (b) plots the proportion of school days (i.e., days of instruction) in each month as a share of total monthly workdays. The plotted values correspond to the average across the affected departments of Nariño and Valle del Cauca. Source: Own elaboration based on data from the Colombian School Census (Form C-600) and the Secretaries of Education of Nariño and Valle del Cauca.

The school calendar reform induced changes in the distribution of instructional days across months. Figure 4b presents the average monthly share of instructional days under the September—June calendar prior to the reform and under the January–November calendar adopted thereafter in affected departments. The reform substantially altered the timing of school holidays throughout the year. Prior to the reform, the bulk of vacation time occurred in July and August, with shorter breaks in January and December, closely resembling academic calendars in Northern Hemisphere countries. Following the reform, vacation time became concentrated in January and December, with a markedly shorter mid-year break, primarily in

[26] We define treated schools as those operating on the September-June calendar in every pre-reform year and on the January-November calendar in every post-reform year, so schools with delayed or partial transitions are excluded from the estimation sample. Our estimates should therefore be interpreted as the effect of the reform among schools that adopted the new calendar immediately. Excluded schools are rare, 4.4 percent of rural public schools that operated on the September-June calendar before the reform, and among them delay is concentrated in schools whose overlap *fell* with the reform, the opposite of what strategic delay to avoid increased overlap would imply. The estimates are similar or larger when excluding the transition years 2011 to 2013, consistent with the event-study pattern in which effects grow over the post-reform period, and indicating that the results are not driven by disruption during the transition itself.

July.

As a consequence, schools in the affected departments abruptly modified their academic calendars, altering the timing of instruction in specific regions of the country. Departments not covered by the reform were unaffected and therefore continued to operate under the existing school calendar.

3.2. Agricultural Activity

Colombia is a middle income country where around 23% of the population lives in rural areas and 17% of the workforce is employed in the agricultural sector. Agricultural production varies depending on the area of the country, with coffee, rice, and maize being the most common agricultural products. Affected departments are intensive in the production of stimulants (mainly coffee, although the category also includes tea and cocoa), rice, maize, fruits and nuts, and sugarcane, whereas the rest of the country specializes also in stimulants, maize, rice, and sugarcane but also in yams and roots. Appendix Figure A.4 plots the main crop at the *vereda* level for all the country and highlights the two affected departments.[27]

Colombia is located very close to the Equator, implying that temperature fluctuations do not follow a seasonal pattern. Instead, temperature varies primarily with altitude: lower-lying areas are warmer, while higher-altitude regions are colder. Despite the relative stability of temperatures throughout the year, climatic conditions do vary seasonally in terms of precipitation rather than temperature. As a result, some degree of climate seasonality is present (although to a much lesser extent than in countries with pronounced seasons) while temperature remains relatively constant over the year.

This climatic context, characteristic of equatorial countries, allows for the production of a wide range of agricultural goods depending on the altitude of cultivated areas. Consequently, Colombia exhibits substantial heterogeneity both in the types of crops produced and in the timing of their production. This, in turn, leads to significant variation in agricultural calendars across regions. Appendix Figure A.5 illustrates this heterogeneity by presenting the harvesting calendars of the three main crops in the affected departments. Specifically, we plot the share of each crop's total annual harvesting activity that occurs in each calendar month (see Appendix Figures A.6 and A.7 for detailed information on harvesting calendars in these two departments).

Therefore, harvesting intensity varies substantially across crops and throughout the year. For example, maize harvesting is concentrated mainly in February and around the middle of the year, whereas stimulant crops, predominantly coffee, are harvested primarily between April and June. This heterogeneity is central to our analysis, as it allows us to identify areas

[27] A *vereda* is the smallest administrative territorial division in the rural areas of Colombian municipalities, typically formed by a grouping of land parcels bounded by geographic features and major roadways. According to the 2020 official geographic layer from the Instituto Geográfico Agustín Codazzi (IGAC), Colombia has 33,428 veredas with a median area of 8.9 km^2, ranging from 1.3 km^2 at the 5th percentile to 87 km^2 at the 95th percentile. Size varies enormously across the country, from a median of 5.8 km^2 in Andean departments such as Cundinamarca and Nariño to a median of 442 km^2 in sparsely populated southern departments such as Amazonas and Vichada.

within the affected departments that are differentially exposed to the reform.

4. Data

We combine four data sources to construct a school-level panel containing information on school progression, geographic location, and exposure to agricultural seasonality.

First, we use the school census from 2006 to 2018.[28] This census is compiled annually by the central government through surveys administered to school principals, who report detailed information on enrollment, infrastructure, and teaching staff. In particular, principals indicate the number of students enrolled in the previous year and classify them as currently enrolled on time, repeating a grade, having dropped out, or having transferred to another institution. We complement these data with georeferenced information on school locations.[29]

Second, we compute harvested areas for different crops at the *vereda* level using data from the Global Agro-Ecological Zones (GAEZ) v4, produced by the Food and Agriculture Organization of the United Nations (FAO).[30] The GAEZ data provide information on harvested areas for 25 crop groups, and are constructed on grids of approximately 9 by 9 kilometers at the equator. Because the median vereda in Colombia covers about 9 square kilometers, a typical grid cell spans several veredas. We assign to each vereda the area-weighted average of the grid cells it intersects and match the resulting crop composition to the school census. The crop information surrounding each school is therefore measured at the resolution of the grid rather than of the vereda itself, so nearby schools share similar measured crop portfolios. For this reason, in addition to school-level clustering, we also report standard errors clustered at the municipality level, which allow for arbitrary correlation in measured exposure across nearby schools.

Third, we use information on harvesting calendars at the department–crop level provided by the Rural Agricultural Planning Unit (Unidad de Planificación Rural Agropecuaria, UPRA) of the Colombian Ministry of Agriculture.[31] These data report the timing and intensity of harvesting seasons for each crop in every department.

Fourth, we use administrative records from the Secretaries of Education of Nariño and Valle del Cauca detailing the academic calendars of public schools in these departments for the 2005–2008 period (pre-reform) and the 2011–2015 period (post-reform).

Combining these four data sets we build a school level panel that includes a total of 31,259 schools, representing all the schools in the country. Out of these, 88.6% correspond to public, 59.6% to rural, and 59.4% to rural, public schools (Appendix Figure A.9). The final estimation sample corresponds to the universe of rural public schools observed in every year between 2006 and 2018, yielding a balanced panel of 18,553 schools. Appendix Table A.4 describes the sample.

[28] These data come from Form C-600 of the Colombian Ministry of Education, which is publicly available.
[29] Appendix Figure A.8 plots the affected departments and the spatial distribution of schools in the country.
[30] The data are available at: https://www.gaez.fao.org/.
[31] Available at: https://www.upra.gov.co/es-co/eva/eva-2019.

Overall, enrollment in rural schools declined over time across all departments in the sample. However, the reduction was substantially larger in the treated departments. Enrollment fell by 10% in the affected departments, compared to a decline of only 3% in the rest of the country. Over the same period, dropout rates decreased and passing rates increased across all schools.

5. Empirical Strategy

5.1. Overlap Between School and Agricultural Calendars

The calendar reform modified the degree of overlap between academic and agricultural calendars in the affected departments. To quantify this change, we construct a school-level measure of overlap between the two calendars in four steps:

1. Using department-level school calendars, we compute the proportion of non-instructional days in month m for each affected department d and each academic calendar $c \in \{A, B\}$. We denote this proportion by V_{mdc}.

2. Using department- and crop-specific harvesting calendars, we identify harvesting-intensive months for each crop. We define an indicator variable, $\mathbb{1}_{\{m \in \mathcal{I}_j\}}$, equal to one if month m for crop j falls within the top quartile of the annual distribution of harvesting activity (i.e., the three most intensive months), denoted by $\mathcal{I}_j$, and zero otherwise.[32]

3. Combining these two measures, we define the overlap $O_{sc} \in [0, 1]$ between academic and agricultural calendars for each school s under calendar c as:

$$O_{sc} = \sum_{j \in J} \omega_{sj} \left(\frac{1}{12} \sum_{m=1}^{12} \max \left\{ \mathbb{1}_{\{m \in \mathcal{I}_j\}} - V_{mdc}, 0 \right\} \right). \tag{2}$$

   The term inside the brackets captures the share of instructional days during harvesting-intensive months (i.e., $1 - V_{mdc}$) and is equal to zero during non-intensive months. Averaging this expression over the twelve months of the year yields, for crop j, the share of the calendar year during which instruction and intensive harvesting coincide under calendar c. We then aggregate across crops using ω_{sj}, the share of crop j's harvested area in the *vereda* where school s is located. The resulting measure, $O_{sc} \in [0, 1]$, is the crop-weighted share of the year in which schooling and intensive harvesting overlap, defined on the same scale as the cross-country overlap measure in Section 2. Larger values indicate that more of the school year falls in periods of peak agricultural labor demand, and thus a higher opportunity cost of schooling.

4. Finally, we compute a school-level exposure measure, ΔO_s, defined as the change in overlap induced by the calendar reform:

[32] The selection of the set $\mathcal{I}_j$ might vary depending on how many months we define as harvesting intensive. We provide robustness checks in Section 6.3 varying the definition of $\mathcal{I}_j$ and show that our results do not depend on this.

$$\Delta O_s = O_{sA} - O_{sB}. \tag{3}$$

A positive value of ΔO_s indicates that the post-reform January–November calendar (Calendar A) *increases* the overlap between academic activities and harvesting periods relative to the pre-reform September–June calendar (Calendar B) in school s. A negative value of ΔO_s indicates that the reform *decreases* such overlap.

Intuitively, the overlap measure captures changes in the opportunity cost of schooling faced by students in schools differentially exposed to the calendar reform. Consider a student living in a rural area where potatoes, primarily harvested in August, constitute the dominant crop. Under the pre-reform calendar, most days in August were school holidays, allowing children to participate in harvesting activities during a peak agricultural month. Under the post-reform calendar, August becomes a full instructional month. As a result, the student faces a sharper trade-off between attending school and working during the harvest season. With binding time constraints, this increased overlap raises the opportunity cost of schooling and may lead some students to reduce attendance or drop out altogether.

Appendix Figure A.10 presents the average change in overlap across crops in both affected departments for the main crops in each region. We document substantial heterogeneity in overlap changes across crops and departments. For instance, while the overlap associated with maize harvesting increases markedly in Nariño, it decreases slightly in Valle del Cauca following the reform. This heterogeneity is central to our analysis, as it implies that exposure to the reform depends jointly on the dominant crops in each *vereda* and on department-specific harvesting calendars. Averaging across crops and departments, the reform increased the school-harvest overlap by 1.7 percentage points, as shown by the overall bar at the bottom of Appendix Figure A.10.

5.2. Dose-Response Design

The calendar reform provides a clean source of quasi-experimental variation. The transition was sharp and uniform for all public schools in Nariño and Valle del Cauca after 2011, while schools in the rest of the country were unaffected. However, exposure to the reform varied within affected departments since the overlap between schooling and local agricultural cycles changed by different amounts across schools. Depending on the crop composition of each school's *vereda*, calendar overlap could increase, stay the same, or decrease, leading to differential levels of exposure.

Our main specification exploits this school-level variation in treatment intensity by estimating the following dose-response event-study specification on the panel of rural public schools:

$$y_{st} = \alpha_s + \delta_t + \sum_{k \neq 2010} \tau_k (\Delta O_s \times \mathbf{1}[t = k]) + \sum_c \gamma_{ct} X^c_{m(s)} + \varepsilon_{st}, \tag{4}$$

where y_{st} denotes the outcome of school s in year t, α_s and δ_t are school and year fixed effects, and ΔO_s is the school-level change in calendar overlap defined in Equation 3, set to zero for

schools outside the affected departments. The coefficients of interest, τ_k, trace the effect of the reform per unit of ΔO_s in each year relative to 2010, the last pre-reform year.[33] The terms $X^c_{m(s)}$ denote pre-reform characteristics of the municipality of school s, namely per capita GDP, the number of terrorist attacks, coca cultivation, and municipal public expenditure, each interacted with a full set of year fixed effects. These interactions allow outcomes to trend flexibly according to baseline municipality conditions, absorbing differential trajectories associated with local economic activity, conflict exposure, and fiscal capacity. Standard errors are clustered at the school level in all specifications.[34]

To make the results directly comparable to the cross-country estimates of Section 2, and to improve exposition, we also report a static summary of the effect that replaces the year-specific interactions with a single interaction between ΔO_s and a post-reform indicator:

$$y_{st} = \alpha_s + \delta_t + \tau(\Delta O_s \times \text{Post}_t) + \sum_c \gamma_{ct} X^c_{m(s)} + \varepsilon_{st}. \tag{5}$$

The coefficient τ provides a single per-unit-of-overlap estimate that can be directly compared to the cross-country IV coefficient.

Identification of τ comes from comparing the evolution of outcomes across schools with larger and smaller changes in overlap, conditional on school fixed effects, common year shocks, and the differential trends absorbed by the baseline interactions.[35] This variation combines differences in exposure among affected schools with the comparison between affected and unaffected schools. In Appendix D.3 we absorb the latter margin entirely with treated-by-year fixed effects and obtain nearly identical estimates. The identifying assumption is that, conditional on these controls, outcomes in schools with different values of ΔO_s would have evolved in parallel absent the reform. We test this directly in the pre-reform period and report the coefficients τ_k for $k < 2011$ in all event-study figures.

Our main specification uses standard two-way fixed effects, which is transparent and easy to interpret. With continuous treatments and heterogeneous effects, however, the two-way fixed effects estimand may not recover a convex average of school-level effects. We address this directly by re-estimating all specifications using the heterogeneity-robust estimator of de Chaisemartin et al. (2024). We additionally provide robustness checks by restricting the control group to bordering departments and to progressively narrower distance bands around the boundary of the affected departments. Because all treated schools changed calendars si-

[33] ΔO_s is measured in the same units as the overlap variable in Equation 1, with $\Delta O_s \in [-1, 1]$, so that the coefficients in Equation 4 are expressed in the same units as the cross-country IV estimates in Section 2 when using dropout rate as outcome.

[34] We additionally report municipality-level clustering throughout as a more conservative alternative, since crop composition, and hence the dose, could be correlated within local areas.

[35] The reform also created a one-off academic break during the transition year, which De Roux and Riehl (2022) study in the context of college enrollment. This break, like any shock common to all public schools in Nariño and Valle del Cauca, is not mechanically excluded from the dose coefficient, since the dose is nonzero only for affected schools and its mean among them differs from zero. Appendix D.3 addresses such common components directly by adding a full set of treated-by-year fixed effects, which absorb any shock common to affected schools. The estimates for dropout and grade promotion are essentially unchanged. The sign-reversal analysis in Section 6.2 provides a complementary test, since no common shock can move outcomes in opposite directions across schools whose overlap rose and fell.

multaneously and the dose is continuous, our setting avoids the negative-weighting problems that arise in staggered-adoption designs (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). The results are stable across all estimators and control groups.

The exclusive calendar transition of all public schools in the affected departments would, in principle, allow us to estimate a standard difference-in-differences equation comparing both affected departments with the rest of the country. This specification, however, discards the within-treatment variation in exposure, relies on only two treated units for inference, and uses the simple average of a large and heterogeneous control group as the counterfactual. We therefore complement the dose-response design with aggregate estimates of the reform's total effect using the synthetic difference-in-differences estimator (SDID) of Arkhangelsky et al. (2021) as an additional robustness check. This method partially addresses these concerns by constructing a data-driven weighted combination of untreated schools whose pre-reform trajectory matches that of treated schools. Whereas Equation 4 identifies the effect of the overlap channel from variation in exposure among treated schools, SDID recovers the intent-to-treat effect of the reform on treated departments, including any component common to all treated schools. We present the SDID estimates as part of our robustness analysis and use them to gauge the magnitude of the reform's total effect.

6. School-Level Effects of the Reform

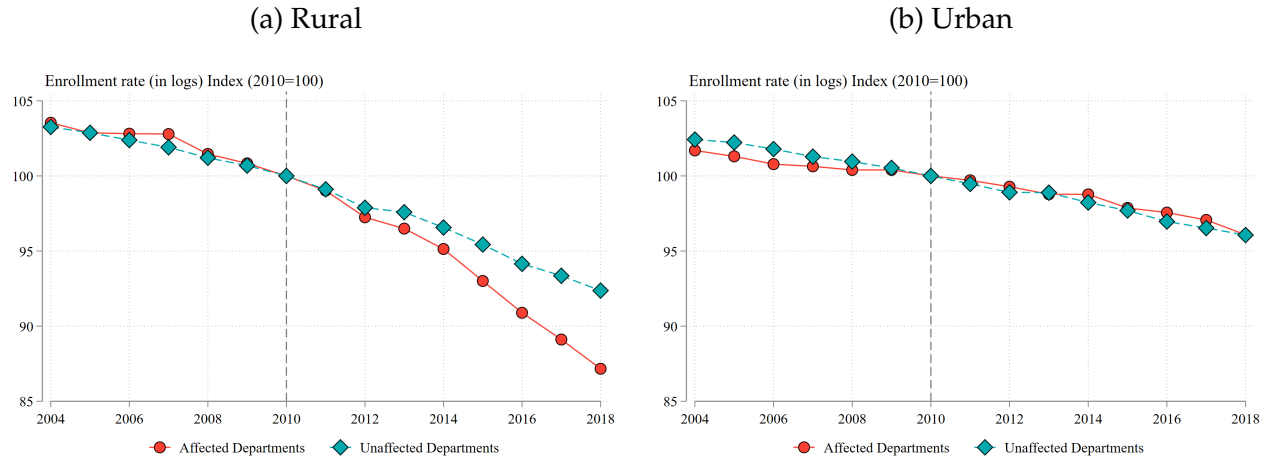
6.1. Effects of the Reform on School Progression

The calendar reform decreased public school enrollment by inducing student dropout and decreasing grade promotion. To see this, we begin by plotting the evolution of school enrollment, normalized to the year prior to the reform, in Figure 5. We present estimates separately for rural (Panel 5a) and urban (Panel 5b) schools. Prior to the reform, school enrollment in affected and unaffected departments evolved in parallel, indicating no differential pre-reform trends. After the reform, we observe a sudden decline in school enrollment among rural schools in affected departments, with the gap widening over time through 2018. We do not see any change in enrollment among urban schools.

Although suggestive, this evidence indicates that the reform decreased rural school enrollment in the affected departments, consistent with the reform increasing average calendar overlap across affected schools, as described in Section 5.1. These plots do not, however, provide causal estimates. Moreover, enrollment is a noisy measure of the overlap channel. As a stock variable, it reflects the cumulative history of prior enrollment and dropout decisions and absorbs scale and composition effects common to all treated schools, such as school mergers, student reshuffling within treated departments, or secular demographic trends in the school-age population driven by changes in fertility or out-migration. Figure 5 indeed shows a negative trend in enrollment across all departments over this period, consistent with such forces operating independently of the reform.

Figure 6 presents the estimates of Equation 4 on dropout rate (Panel a) and grade promo-

Figure 5: Public School Enrollment in Affected and Unaffected Departments



Notes: Each panel plots public school enrollment in the affected departments (Nariño and Valle del Cauca) and in the rest of Colombia, indexed to 100 in 2010, the last pre-reform year, for rural schools (Panel a) and urban schools (Panel b). The vertical line marks the reform. Source: Own elaboration based on data from DANE.

tion rate (Panel b), the flow outcomes that directly capture individual schooling decisions in a given year.[36] Two patterns stand out. First, in the years preceding the reform, the estimated coefficients are small and statistically indistinguishable from zero across all outcomes, indicating that schools with different exposure to the calendar change were not on differential trajectories before 2011. Second, following the reform, outcomes deteriorate sharply in schools where the calendar change generated a larger increase in overlap.[37] Dropout rates rise immediately and remain elevated throughout the post-reform period, whereas grade promotion falls persistently, mirroring the dropout response.

The magnitudes are economically meaningful. The static counterpart of the event-study specification, presented in Panel A of Appendix Table A.5, implies that a one-percentage-point increase in the overlap between the school and agricultural calendars, raises the dropout rate by 0.71 to 0.74 percentage points (closely in line with the aggregate cross-country estimates in Section 2) and lowers grade promotion by 0.65 to 0.69 percentage points.[38] Because the reform moved overlap in both directions, we evaluate these estimates at the subgroup averages rather than at a single aggregate change. Among the 60 percent of treated schools where overlap increased, the average increase of 1.1 percentage points implies a rise in dropout of about 0.8 percentage points and a decline in grade promotion of about 0.8 percentage points, roughly

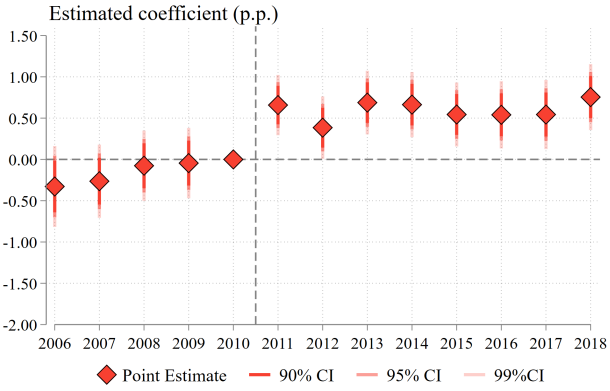
[36] We report the estimates using school enrollment in Appendix Figure A.11 for completeness.

[37] One concern is that the reform induced families to relocate across departments. A child who leaves an affected school is recorded there as a transfer rather than a dropout, so relocation does not mechanically enter our dropout measure, but a child who subsequently enrolls in an unaffected school does enter the control observations, and selective departure could alter the composition of treated schools. The evidence indicates that this margin is inactive. Appendix Figure A.12 shows no increase in municipality-level transfer rates after the reform, and no differential trend between affected and unaffected departments in the share of children who migrated within the previous five years.

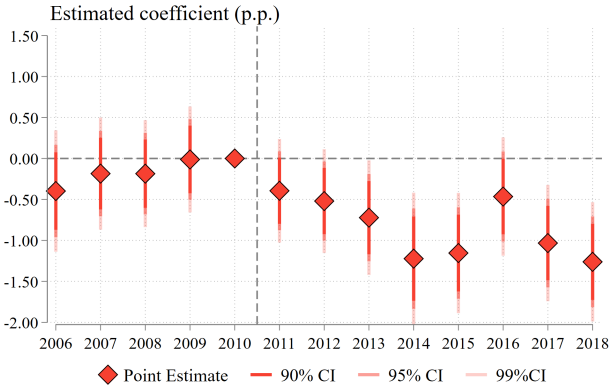
[38] All estimates are statistically significant at the 1 percent level when clustering at the school level. When clustering at the municipality level, the effects on dropout and grade promotion remain significant at the 1 percent level.

Figure 6: Effect of the Calendar Reform by Exposure to Calendar Overlap

(a) Dropout rate



(b) Grade promotion



Notes: The figure plots the estimated coefficients on the interaction between the school-level change in calendar overlap, ΔO_s, and year indicators, relative to the baseline year 2010. Dropout and grade promotion are the shares of students who dropped out or where promoted to the next grade, respectively. Estimates come from the event-study specification Equation 4, which includes school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure). Coefficients are expressed per unit of ΔO_s. The sample includes rural public schools in the affected departments and in the rest of the country, restricted to a balanced panel of schools observed in every year between 2006 and 2018. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the school level. Source: Own elaboration based on data from DANE.

18 percent of the pre-reform dropout rate in these schools. Among the 31 percent of schools where overlap fell, the average reduction of 1.5 percentage points implies improvements of about 1.1 percentage points in both outcomes.

These estimates capture the differential effect of the reform across schools with different exposure to the calendar overlap. Because promotion is measured as a share of enrolled students, the decline in promotion largely reflects the rise in dropout rather than a separate effect on the performance of students who remained in school. When we instead measure promotion conditional on completing the school year, defined as promoted students over the sum of promoted and failed students, we find no response to exposure, and the mechanical contribution of dropout, evaluated at the conditional promotion rate of students who complete the year, accounts for nearly all of the unconditional promotion decline. The effects of calendar overlap therefore operate through the extensive margin of school continuation, with students in highly exposed schools responding to the higher opportunity cost of schooling by leaving school altogether, which also explains the decrease in the stock of rural school enrollment among exposed schools in affected departments.

Heterogeneity of the effect: The effects of the reform are concentrated among primary school students. Panels B and C of Appendix Table A.5 present the dose-response estimates separately for primary and secondary students (Appendix Figure A.13 present the dynamic results). Among primary students, a one-percentage-point increase in calendar overlap raises the dropout rate by 0.83 percentage points and lowers grade promotion by 0.74 percentage points. Among secondary students, in contrast, the estimated effects on dropout and grade promotion are close to zero and statistically insignificant.

This pattern is consistent with the opportunity-cost mechanism. Child labor in agriculture tends to be most prevalent among younger children, who are disproportionately engaged in family subsistence farming (ILO and UNICEF, 2025). Primary school students are therefore the most exposed to agricultural labor demand generated by this type of reforms. The concentration of effects among them is also consistent with the reform accelerating dropout among children at the margin of school continuation, who disproportionately attend primary school.[39] We additionally examine heterogeneity by gender in Appendix Figure A.14, and do not find any substantial difference between male and female students.

6.2. Effects by Direction of the Change in Overlap

The dose-response estimates in Section 6.1 exploit the intensity of exposure to the calendar change. The reform, however, generates an even sharper test of the opportunity-cost mechanism by comparing schools where the reform *increased* overlap with schools where it *decreased* it. If the effects we document operate through the overlap between schooling and agricultural work, rather than through some other feature of the calendar transition, outcomes should deteriorate where overlap rose and improve where overlap fell. Any confounder common to treated departments, by contrast, would affect both groups in the same direction.

We implement this test by splitting treated schools into those with a positive change in overlap ($\Delta O_s > 0$) and those with a negative change ($\Delta O_s < 0$), and estimating the effect of the reform separately for each group using the specification in Equation 4. The resulting coefficients measure the effect per percentage point of overlap change in each group's own direction.[40]

Table 4 presents the estimates for dropout and grade promotion rates, splitting by levels of exposure.[41] The results reveal a clear sign reversal. Among schools where the reform *increased* overlap, a one-percentage-point increase raises dropout by 0.53 percentage points and lowers grade promotion by 0.42 percentage points. Among schools where the reform *reduced* overlap, the effects flip sign. Dropout falls by 1.03 percentage points and grade promotion rises by 1.03 percentage points.

These results provide strong evidence for our mechanism. Schools more exposed to calendar overlap face a deterioration on schooling outcomes. Schools where the overlap decreases display an improvement in those same outcomes. This sign reversal is difficult to reconcile with any confounder common to treated departments and constitutes direct evidence that the overlap between schooling and agricultural labor demand, rather than the calendar change per se, drives the effects on school progression.

[39] We test the time-allocation mechanism directly in Section 7, where we show that the reform raised the probability of children engaging in rural work, with the increase concentrated among primary-school-aged children.

[40] To ensure coefficients are directly comparable across groups, we use the absolute value of the dose as treatment intensity.

[41] The dynamic results are presented in Appendix Figure A.15, where we do not see evidence of existing pre-trends by subgroups.

Table 4: Dose-Response Estimates by Direction of the Overlap Change

	Dropout rate		Grade promotion	
	Positive dose	Negative dose	Positive dose	Negative dose
Calendar overlap ($\lvert\Delta O_s\rvert \times$ Post)	0.531***	-1.031***	-0.420**	1.032***
	(0.096)	(0.181)	(0.178)	(0.241)
Observations	231,855	225,368	231,855	225,368
Baseline controls × year FE	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Notes: The table reports static dose-response estimates of Equation 5 estimated separately for schools whose calendar–harvest overlap increased (positive dose) and decreased (negative dose), each compared against the full all-country control group of rural public schools. The treatment variable is the absolute value of the change in overlap, so coefficients are expressed per unit of $\lvert\Delta O_s\rvert$ in each group's own direction: a negative coefficient in the negative-dose columns indicates that the outcome falls as overlap decreases. All specifications include school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics, on the balanced panel 2006–2018. Standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Own elaboration based on data from DANE.

6.3. Robustness of the School-Level Estimates

We provide additional evidence supporting our main results through three complementary robustness exercises. First, we show that our findings are not sensitive to alternative definitions of harvesting intensity months (i.e., the set defined in $\mathcal{I}_j$). Second, as explained in Section 5.2, we re-estimate the main effects using alternative estimators that account for potential heterogeneity induced by the continuous treatment and progressively more local control groups, yielding 21 estimator-by-control-group combinations per outcome. Third, we complement the dose-response design with an intent-to-treat estimate using the SDID procedure. We provide complementary details about all robustness checks in Appendix D.

Definition of Harvesting Intensity. Our overlap measure in Equation 2 defines harvesting-intensive months as the top quartile of the annual distribution of harvesting activity, corresponding to the three most intensive months of the year. To verify that the results do not depend on this threshold, we re-estimate our main specification using stricter definitions of the dose, constructing ΔO_s from only the single most intensive month and from the two most intensive months. We report the resulting estimates in Appendix Figure D.1.

The effects on school progression are robust across definitions. Dropout rises and grade promotion falls in schools where overlap with the most intensive harvesting periods increased, with estimates that retain their sign, magnitude, and statistical significance under all three definitions. The robustness of the results under progressively stricter definitions indicates that the effects on school progression are driven by the alignment of schooling with peak agricultural activity rather than by the specific threshold used to define it.

Alternative Estimators. Our dose-response design assigns a continuous treatment intensity to every school. Recent econometric work shows that two-way fixed effects estimators with continuous treatments can place negative weights on some treatment effects, potentially yielding estimates that are not a convex combination of the underlying school-level effects (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). To address this, we re-estimate the dose-response specification using three alternative estimators. The

first is the two-way fixed effects specification described in Section 5. The second and third are the normalized and non-normalized versions of the heterogeneity-robust estimator of de Chaisemartin et al. (2024), which expresses the effect per unit of treatment dose.

A second concern is that schools in distant regions of Colombia may provide a poor counterfactual for schools in the affected departments, either because they face different agro-climatic conditions or because they are exposed to different regional shocks. We therefore re-estimate all specifications restricting the control group to progressively more local comparisons, starting with schools in the six departments bordering the affected ones and then narrowing to schools located within 40, 45, 50, 55, and 60 kilometers of the boundary of the affected departments. These restrictions trade sample size for comparability, as nearby schools are more likely to share climatic conditions, crop portfolios, and exposure to local economic shocks.

Appendix D.2 summarizes the full robustness battery in specification-curve form. For each outcome, we plot the 21 point estimates generated by crossing the three estimators with the seven control groups, together with 95 percent confidence intervals clustered at both the school and the municipality level. Each specification is colored according to the p-value of a joint pre-trend test: red indicates the null of parallel pre-trends is not rejected at the 10 percent level, light blue indicates rejection at the 10 but not the 5 percent level, and dark blue indicates rejection at the 5 percent level. In green, we present our main estimate, which corresponds to the one presented in Section 6. The lower panel of each curve identifies the estimator and control group, allowing readers to verify that no single modeling choice drives the results.

The curves show that the estimated effects are stable in sign and magnitude across the entire specification space. Each of the 21 estimates on dropout is positive and statistically significant, ranging from 0.59 to 0.90. The point estimates on grade promotion are uniformly negative. No dropout specification rejects the null of parallel pre-trends at the 10 percent level, and only two of the 21 grade promotion specifications do.[42]

Intent-to-Treat Estimates. Finally, we complement the dose-response evidence with estimates of the reform's effect using the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021), which compares schools in affected departments with a data-driven weighted combination of schools in unaffected ones. Whereas the dose-response design identifies the effect of the overlap channel from within-treatment variation in exposure, the SDID estimates capture the intent-to-treat effect of the reform, including any component common to all schools in affected departments. We present the results in Appendix D.4. The reform raised dropout rates by approximately 1.7 percentage points on average and lowered grade promotion by approximately 2 percentage points. These aggregate estimates exceed the dose-response estimates evaluated at the average overlap change because the two designs recover different estimands. SDID captures the full effect of the reform on treated departments, including components common to all treated schools regardless of their overlap change, whereas the

[42]For completeness, we also report robustness checks for school enrollment, even though, as explained in Section 6.1, this outcome is a stock, measured in logs, and absorbs scale, composition, and demographic effects. The estimates are persistently negative and we fail to reject the null of parallel trends in every case.

dose-response design isolates the component that varies with exposure to the overlap channel.

7. Child Labor Responses to the Reform

The school dropout response documented in Section 6 is consistent with a time-allocation mechanism in which children substitute schooling for agricultural work. We test this directly by examining whether the calendar reform increased child labor, using nationally representative household survey data and a municipality-level difference-in-differences design. We estimate the following specification:

$$E_{i,m\in d,t} = \sum_{k\neq 2010} \tau_k \left(W_m \times \mathbb{1}[t=k]\right) + \alpha_m + \beta_t + \sum_k \left(\mathbb{1}[t=k] \times X_{m,t_0}\right)' \gamma_k + \epsilon_{imt}, \tag{6}$$

where $E_{i,m\in d,t}$ is a dummy variable equal to one if child i in municipality m of department d works in rural activities in year t, and zero otherwise. W_m is a treatment indicator equal to one if municipality m belongs to one of the affected departments. As control municipalities, we use a matched set of untreated departments comparable to the treated ones, namely Risaralda and Sucre.[43] The terms α_m and β_t denote municipality and year fixed effects, while X_{m,t_0} is a vector of predetermined municipality-level characteristics obtained from CEDE–Universidad de los Andes, interacted with year fixed effects to flexibly control for time-varying shocks that may be correlated with both municipality characteristics and child labor outcomes.[44]

Figure 7 reports the estimated τ_t coefficients. Prior to the reform, the probability of children engaging in rural labor evolves in parallel across treated and control municipalities, with coefficients close to zero, supporting the parallel-trends assumption underlying the design. Following the reform, the probability rises sharply in affected municipalities, by approximately 5 percentage points, and remains elevated through the end of the sample period. The persistence of the effect mirrors the dynamics of the dropout response documented in Section 6 and is consistent with a long-lasting reallocation of children's time toward farm work in affected areas.

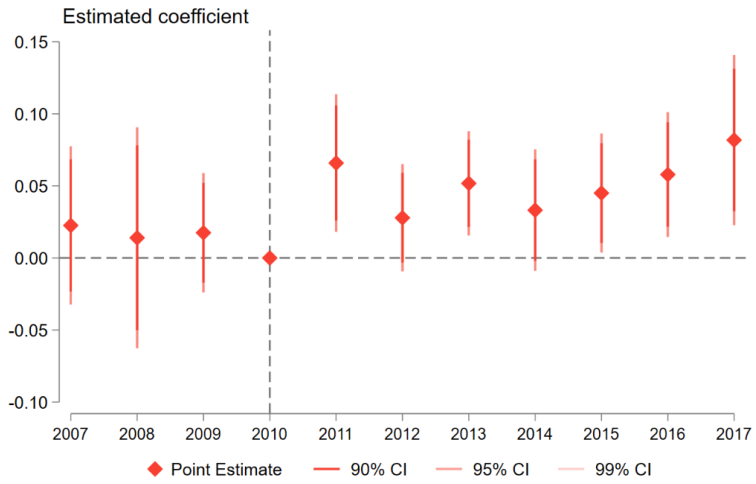
These causal estimates are reinforced by descriptive patterns in the same data. Appendix Figure A.16 provides three complementary views of child labor in rural areas. Panel (a) plots the share of children working in rural activities over time and shows that, in affected departments, this share approximately doubled between the pre-reform period and 2017, while it remained essentially flat in the rest of the country. Panel (b) summarizes this pattern as a raw before-after comparison. The share of primary-school-age children working in rural activities rose by approximately one percentage point in affected departments (about a 20

[43] We first restrict the pool to untreated departments whose primary agricultural products overlap with those of the treated departments (stimulants and rice), and then select the closest departments based on a standardized index of pre-treatment covariates: the share of agriculture in departmental GDP, the log of the rural population, the log of public expenditure, and the log of public revenues. This procedure identifies Risaralda, Sucre, and Arauca. Arauca is excluded because it is not surveyed at the same frequency as the rest of the Colombian departments in the household survey.

[44] The pre-treatment controls include per capita GDP, the number of terrorist attacks at the municipality level, the homicide rate, rural population, and agricultural GDP.

percent increase relative to pre-reform levels) and declined slightly elsewhere. Panel (c) disaggregates the post-reform increase by educational attainment within treated departments and shows that the rise is concentrated among children with completed primary education and incomplete secondary education, which are precisely the margins along which dropout increased in Section 6.

Figure 7: Effect of the Reform on Child Labor



Notes. This figure plots the estimated coefficients τ_t from Equation 6. Source: Own elaboration based on data from DANE.

Taken together, the causal and descriptive evidence suggests that the calendar reform increased the probability that rural children work and reduced the share who remain in school. These results are consistent with a time allocation mechanism in which students substitute schooling for agricultural work. Overall, greater overlap between academic calendars and agricultural cycles appears to raise the opportunity cost of schooling, leading households to substitute children's schooling with labor supply during peak agricultural periods.

8. Reconciling the Evidence

Taken together, our results invite two discussions. The first is how the cross-country and Colombian exercises fit together as identification strategies. The second is what our estimates can and cannot say about the welfare consequences of calendar alignment.

The two strategies. The two empirical exercises complement each other despite an apparent tension. Our identifying experiment comes from a country whose calendars are not a colonial inheritance. Colombia gained independence before Spain institutionalized its school calendar, and its dual calendar system emerged from domestic conditions rather than colonial imposition. The Colombian exercise therefore cannot speak to the origins of calendar misalignment. It isolates, however, the effect of calendar timing when institutions are held constant and the counterfactual is directly observed. To the best of our knowledge, no other country has formally operated a dual public school calendar at the national level and subsequently unified

it through a single policy reform. This is precisely what makes the Colombian exercise a compelling complement to the cross-country evidence.

An identification concern with the cross-country analysis is that colonial legacy variation may capture a broad mismatch between externally imposed institutions and locally appropriate ones, rather than the specific calendar channel we emphasize (Rodrik et al., 2004; Acemoglu and Johnson, 2005). The IV is designed to address this by isolating variation specific to calendar timing and orthogonal to other dimensions of colonial institutional inheritance such as property rights or state capacity. The Colombian evidence complements these results by providing a direct test, since the reform changed only the timing of instruction and left curricula, school resources, and all other institutional features unchanged. Together, the two exercises divide the task. The cross-country analysis documents where calendar misalignment comes from and how it shapes schooling outcomes worldwide, while the Colombian reform provides a controlled setting in which its causal effect can be evaluated holding the institutional environment fixed.

The magnitudes found in both exercises reinforce this interpretation. Both strategies express the schooling cost of a one-percentage-point increase in school-harvest overlap in the same units, and the resulting magnitudes are close. The within-Colombia estimates imply an increase in dropout of 0.71 to 0.74 percentage points. The cross-country estimates bracket this figure. The full-sample specifications that do not condition on colonial status imply 0.77 to 0.79 percentage points, while the specifications that remove the colonized versus never-colonized contrast, either by conditioning on colonial status or by restricting to former colonies, imply 0.48 to 0.63 and are the more conservative benchmark. The Colombian estimate therefore falls between the two cross-country benchmarks. That estimates obtained at different levels of aggregation and from unrelated sources of variation agree in sign and order of magnitude strengthens the case that calendar timing is a quantitatively important determinant of human capital accumulation.

Scope of the results. Our results should not be read as unqualified evidence for realigning every school calendar with local agricultural cycles, as alignment operates along multiple dimensions. Synchronizing school terms with agricultural seasons reduces the opportunity cost of schooling, as our estimates show, but synchronizing calendars across regions eases teacher mobility, transitions to higher education, and administrative coordination, which was the stated rationale for the Colombian unification. Our design isolates the agricultural alignment channel and quantifies its cost but is not equipped to weigh this cost against the coordination benefits that unification may have delivered. The dose-response estimates do, however, sharpen the policy question. Outcomes improved in schools whose alignment the unified calendar happened to increase, so the educational costs we document were not inherent to unification itself but to the choice of which calendar to unify on. A national calendar chosen with harvest timing in mind could in principle have delivered the coordination benefits at a smaller educational cost, though this is more difficult in countries where agricultural activity is heterogeneous and a single calendar cannot simultaneously avoid all harvesting seasons.

9. Conclusions

This paper studies the economic consequences of time conflicts between social institutions and economic activities, focusing on the overlap between school calendars and peak labor demand in agrarian economies. We document that academic calendars across countries in southern latitudes are systematically misaligned with local agricultural cycles, and we show that this misalignment reflects colonial institutional legacies rather than adaptation to local economic conditions. Former colonies that gained independence after their colonizers had institutionalized public schooling tend to retain inherited calendar structures, whereas countries that became independent earlier adapted their calendars to local conditions. Exploiting this colonial legacy as a source of identifying variation, our cross-country instrumental variable estimates indicate that a one percentage point increase in the overlap between school and agricultural calendars raises the share of primary-school-age children out of school by 0.6 to 0.8 percentage points, with effects concentrated among former colonies.

To examine the underlying mechanism, we exploit a calendar reform in Colombia that shifted public schools in Nariño and Valle del Cauca from a September–June to a January–November academic calendar. Because the resulting change in the overlap with local harvesting periods depends on the crop composition surrounding each school, the reform generated continuous variation in exposure, increasing overlap in some *veredas* and reducing it in others. Using administrative school-level data and a dose-response design that exploits this variation, we find that a one-percentage-point increase in calendar overlap raises dropout rates by 0.71 to 0.74 percentage points and lowers grade promotion by 0.69 percentage points. Among the 60 percent of treated schools where the reform increased overlap, the average increase of 1.1 percentage points implies a rise in dropout of about 0.8 percentage points, roughly 18 percent of their pre-reform dropout rate, while schools where overlap fell experienced improvements of comparable magnitude. The estimates are stable across heterogeneity-robust estimators, progressively more local control groups, and clustering levels. Crucially, the effects reverse sign where the reform reduced overlap: schools whose new calendar happened to better align with local harvesting cycles experienced lower dropout and higher grade promotion, a sign reversal that ties the effects to the overlap between schooling and agricultural labor demand rather than to the calendar transition itself. Consistent with a time-allocation mechanism, child labor participation rose in treated departments following the reform, concentrated among primary-school-aged children at the margin of school continuation.

Taken together, the cross-country and within-country evidence point to a common mechanism: when school calendars overlap with periods of high agricultural labor demand, the opportunity cost of schooling rises and households substitute children's schooling with farm work. The two identification strategies express the schooling cost of overlap in the same units and deliver closely aligned magnitudes, 0.8 percentage points of dropout per percentage point of overlap in the cross-country estimates and 0.7 percentage points in the Colombian natural experiment. That two designs drawing on different populations, outcomes, and sources of variation agree in sign, significance, and magnitude indicates that institutional

timing is a quantitatively important and previously overlooked determinant of human capital accumulation.

Our findings have direct policy implications. Much of the existing response to school dropout in developing countries has focused on resource-intensive interventions such as conditional cash transfers or investments in school inputs. Our results suggest that adjusting the timing of the school year to better align with local economic conditions offers a complementary and largely costless margin for improving educational outcomes. More broadly, the evidence highlights that inherited features of institutional design, even those as routine as the timing of the academic year, can have first-order consequences for economic development, and that realigning social institutions with local economic conditions can yield substantial gains for human capital accumulation.

These findings, however, should not be read as an argument against calendar unification per se. The dual calendar system in Colombia was sustained in part by local conditions. Decree 1902 of 1969 justified the September-June calendar in the southwest on climatic and administrative grounds and cited requests from school communities, indicating that regional differentiation reflected local circumstances at least in part. At the same time, our estimates show that the alignment was imperfect, since the switch to the national calendar reduced the school-harvest overlap for roughly a third of affected schools. Regional calendars embodied local conditions imperfectly, and it is precisely this imperfection that generates the two-sided variation our dose-response design exploits. Unification may have brought real benefits, including coordination with national assessment cycles, easier teacher deployment across departments, and smoother pathways to higher education, that our design is not equipped to capture. The reform may therefore have been premature rather than wrong. More broadly, since this paper studies timing institutions rather than school calendars specifically, the same logic applies beyond Colombia. The optimal alignment of a timing institution depends on which dimension, local or system wide, generates the largest gains in a given context, and the broader policy implication is not that countries should fragment their institutions, but that institutional design involves a genuine trade-off between local alignment and system-wide coordination. Quantifying both sides of this trade-off is an important direction for future research.

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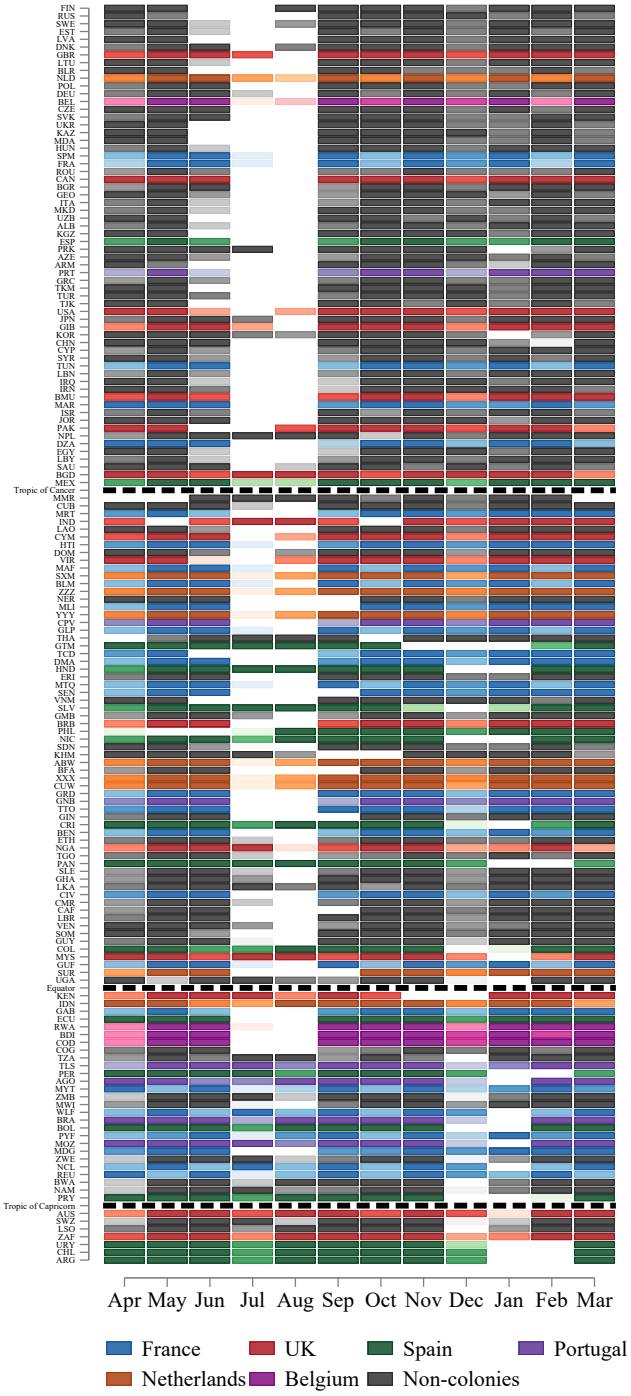
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A. Additional Figures and Tables

A.1. Agricultural and Institutional Timing across Latitudes

Appendix Figure A.1: Worldwide Academic Calendars by Latitude and Colonizer



Notes: The figure shows the share of school days in each month. For each country, the information corresponds to the most recent school calendar data available. Source: Own elaboration based on official sources.

Appendix Figure A.1 shows that school calendars vary with latitude and colonizer. This subsection asks how that institutional variation compares with the variation geography itself generates. Agricultural timing responds to geography. Panel a of Appendix Figure A.2 plots each country's main-crop harvest midpoint against latitude. Harvest timing shifts

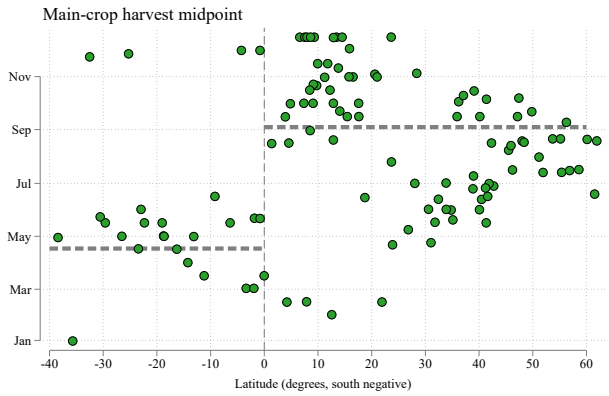
systematically with distance from the equator and, most importantly, flips by roughly six months across it. Countries in the northern tropics concentrate their harvests toward the end of the calendar year, while countries in the mirror bands south of the equator harvest in the first months of the year, reflecting the reversal of solar seasonality between hemispheres. The dashed segments mark the average harvest midpoint in each hemisphere and make the six-month shift explicit.

Institutional timing does not track geography in the same way. Panel b plots, for each country, the distance in months between the midpoint of the long school vacation and the main-crop harvest midpoint. In countries without a colonial past, which designed their own calendars, the long vacation sits close to the harvest, consistent with the historical origins of school calendars described in Section 2. In former colonies the vacation drifts away from the harvest, and the misalignment is largest south of the equator, where the average distance exceeds four months out of a maximum possible six. Notably, this is true even though most southern colonies have moved their long vacation to the austral summer. Because tropical harvests follow rainfall rather than solar seasons, adapting the inherited template to the local summer does not align it with the local harvest. Institutional adjustment followed the logic of the template, placing vacations in summer, rather than the agricultural conditions the original calendars were designed around.

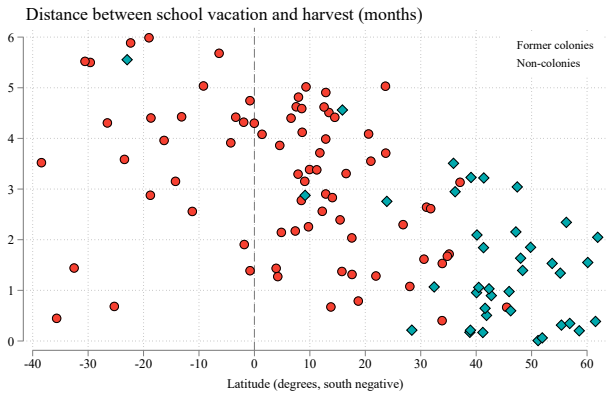
Appendix Table A.1 summarizes the contrast in ten-degree latitude bands. In every band, the average vacation-to-harvest distance is larger in the Southern Hemisphere than in its northern mirror, and it is smallest in the temperate north, where school calendars originated. Together, the two panels illustrate the mechanism underlying our identification strategy. Harvest timing is predictable from geography, which is the premise of the instrument in Section 2.3, while the position of school vacations relative to harvests carries the imprint of institutional history.

Appendix Figure A.2: Agricultural and Institutional Timing across Latitudes

(a) Main-crop harvest midpoint



(b) Distance between vacation and harvest



Notes: Panel a plots the midpoint of each country's main-crop harvest season against latitude, with southern latitudes negative. The horizontal dashed segments mark the average harvest midpoint in each hemisphere. Panel b plots the distance in months between the midpoint of each country's school vacation period and its harvest midpoint, which ranges from zero, when the vacation falls on the harvest, to six months, when it falls in the opposite part of the year. All midpoints and distances are computed treating the calendar year as a circle, so that seasons spanning December and January are measured correctly. The vertical dashed line marks the equator. Source: Own elaboration based on data described in Appendix B.1.

Appendix Table A.1: Harvest and Vacation Timing by Latitude Band

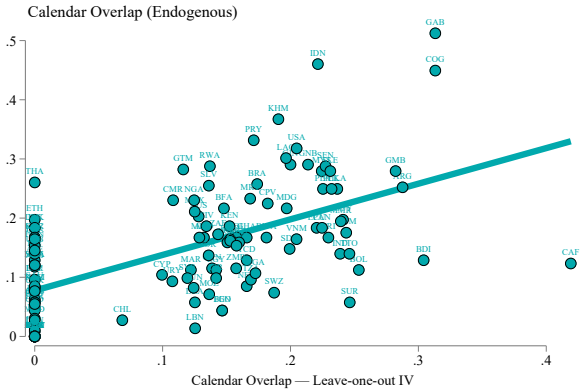
	Northern Hemisphere				Southern Hemisphere			
Latitude band	N	Harvest	Vacation	Gap	N	Harvest	Vacation	Gap
0–10	18	Nov	Sep	3.3	9	Apr	Aug	4.0
10–20	18	Nov	Aug	3.0	7	May	Dec	3.9
20–30	9	Nov	Jul	2.7	6	May	Dec	4.3
30+	43	Aug	Jul	1.4	4	Mar	Jan	2.7

Notes: The table reports, for ten-degree bands of latitude in each hemisphere, the number of sample countries, the typical month in which the main crop is harvested, the typical month of the midpoint of the school vacation period, and the average distance in months between the school vacation and the harvest. The distance ranges from zero, when the vacation falls on the harvest, to six months, when it falls in the opposite part of the year. All midpoints and distances are computed treating the calendar year as a circle, so that seasons spanning December and January are measured correctly. Source: Own elaboration based on data described in Appendix B.1.

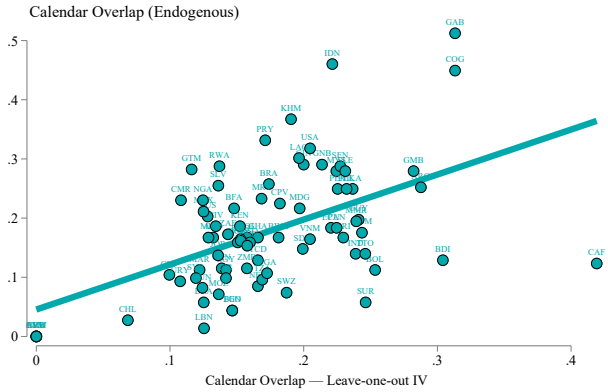
A.2. Tables and Figures

Appendix Figure A.3: First-Stage Relationship

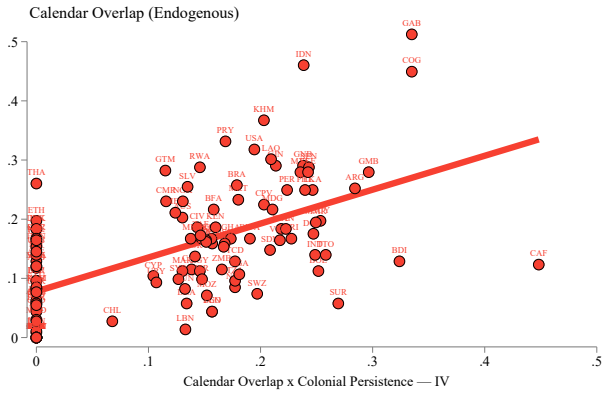
(a) Leave-one-out overlap, Full sample



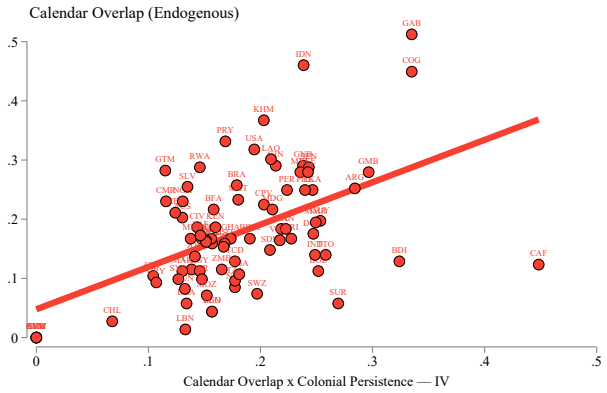
(b) Leave-one-out overlap, Colonies



(c) Overlap weighted by colonial exposure, Full sample

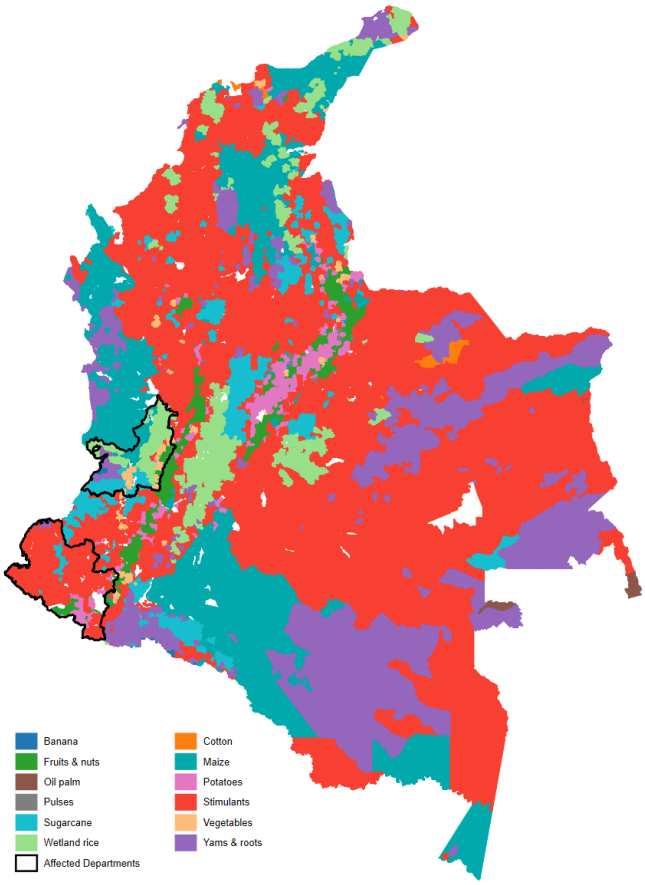


(d) Overlap weighted by colonial exposure, Colonies



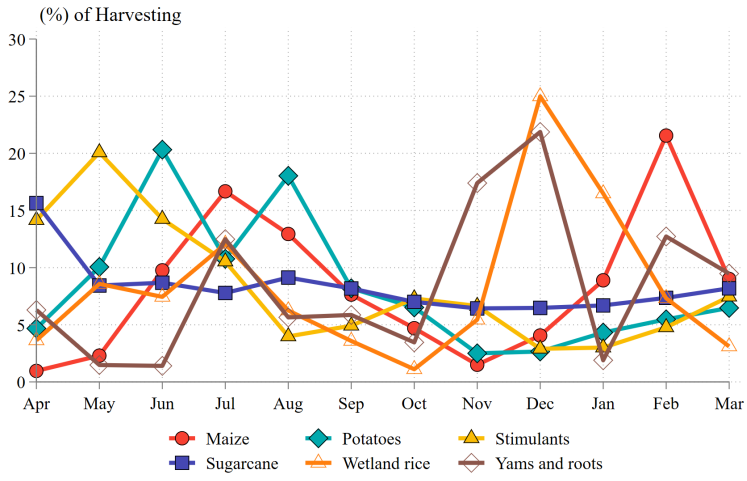
Notes: Each panel plots the observed overlap between school and harvesting calendars, O_i, against the instrument, with each dot representing a country and the solid line a linear fit. Panels a and b use the baseline instrument Z_i, for the full sample and for former colonies. Panels c and d use the overlap weighted by colonial exposure instrument Z_i^C. The figures display the unconditional relationship between the observed overlap and the instruments. Both instruments are expressed as shares of the year, on the same scale as the overlap measure. Source: Own elaboration based on data described in Appendix B.1.

Appendix Figure A.4: Main Crop by *Vereda*



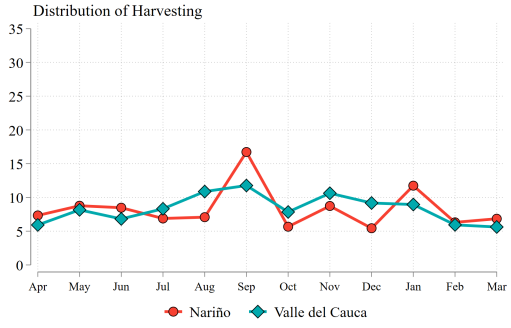
Notes. The figure shows the main harvested crop in each *vereda*. Stimulants include coffee, tea, and cocoa. *Veredas* shown in white indicate areas for which no information is available. Source: Own elaboration based on data from the Global Agro-Ecological Zones (GAEZ) v4 - FAO.

Appendix Figure A.5: Harvesting Calendars for Main Crops in Affected Departments

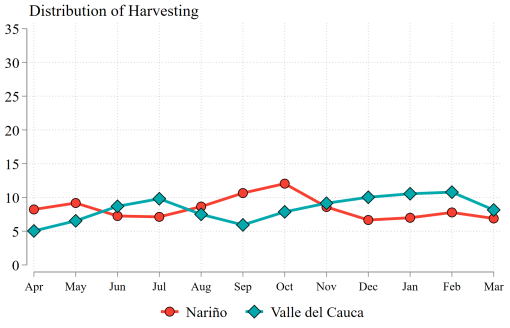


Notes. This figure plots the proportion of days dedicated to harvesting activities in each month, relative to the total annual harvesting for each crop. Stimulants refer to coffee, tea, and cocoa. Data come from the Colombian Ministry of Agriculture.

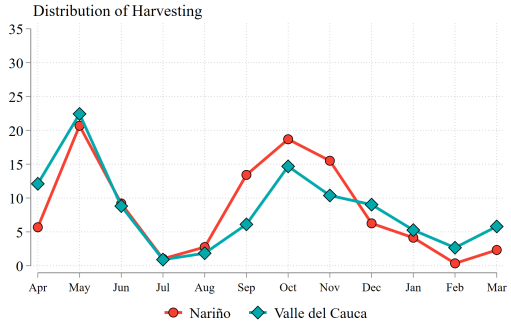
Appendix Figure A.6: Harvesting Calendars - Nariño and Valle del Cauca



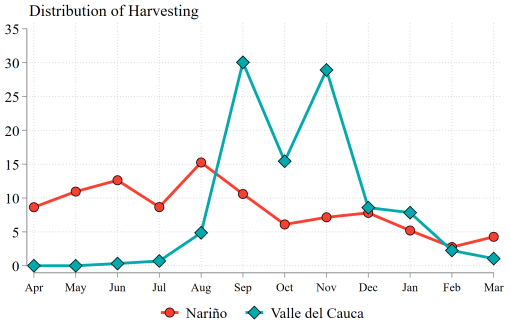
(a) Banana



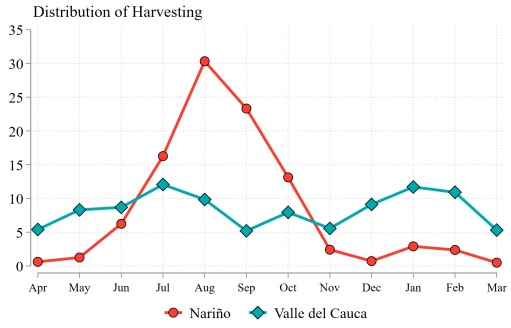
(b) Fruits and Nuts



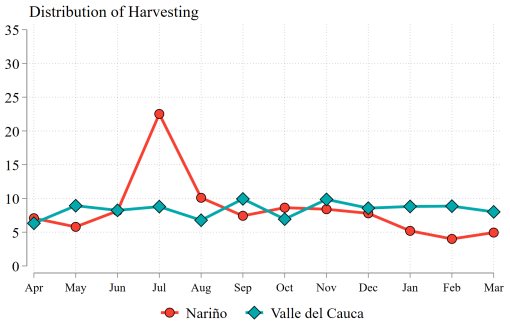
(c) Maize



(d) Potato and Sweet Potato



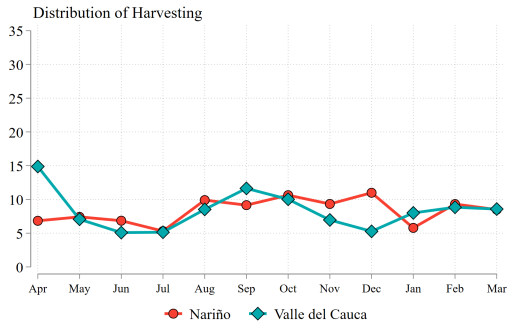
(e) Stimulants



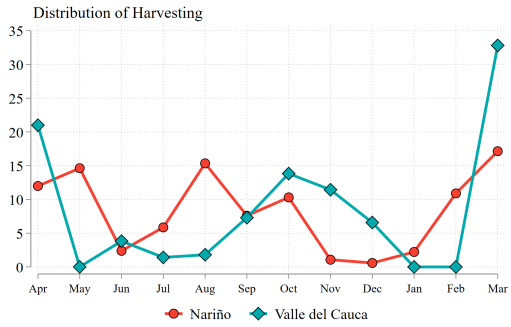
(f) Sugarcane

Notes. The figures show the proportion of days dedicated to harvesting activities in each month, relative to the total annual harvesting for each crop. Source: Own elaboration based on data from the Colombian Ministry of Agriculture.

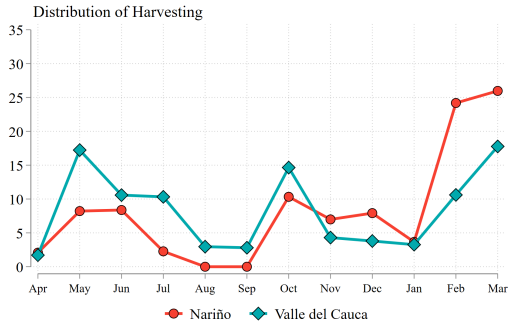
Appendix Figure A.7: Harvesting Calendars - Nariño and Valle del Cauca



(a) Vegetables



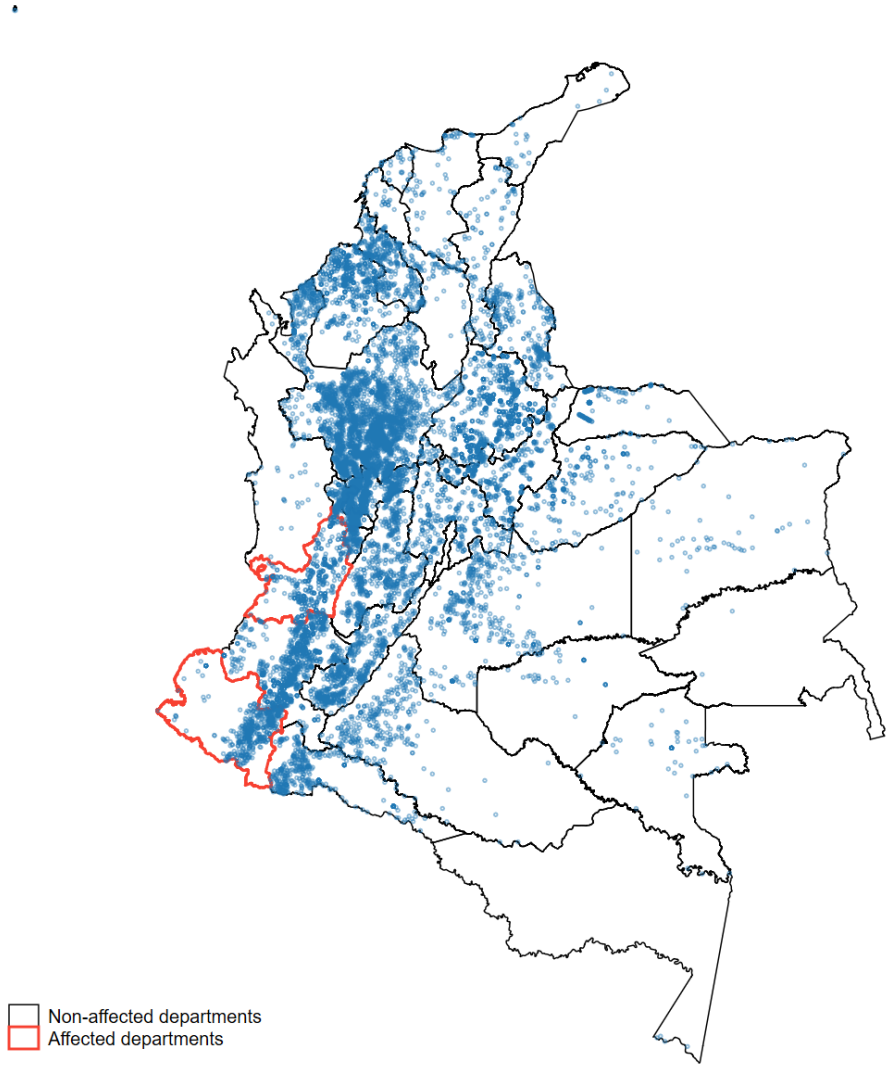
(b) Wetland Rice



(c) Yams and Other Roots

Notes. The figures show the proportion of days dedicated to harvesting activities in each month, relative to the total annual harvesting for each crop. Source: Own elaboration based on data from the Colombian Ministry of Agriculture.

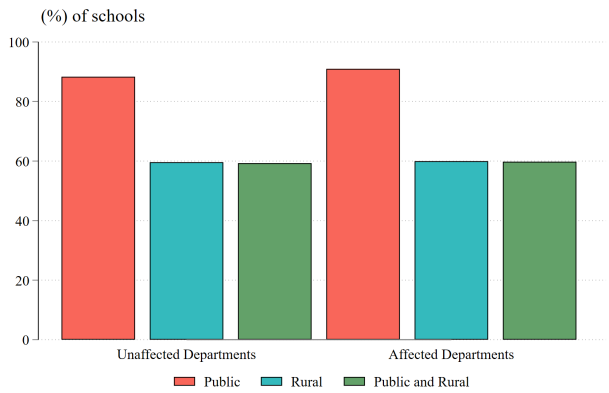
Appendix Figure A.8: Spatial Distribution of Schools in Affected and Unaffected Departments



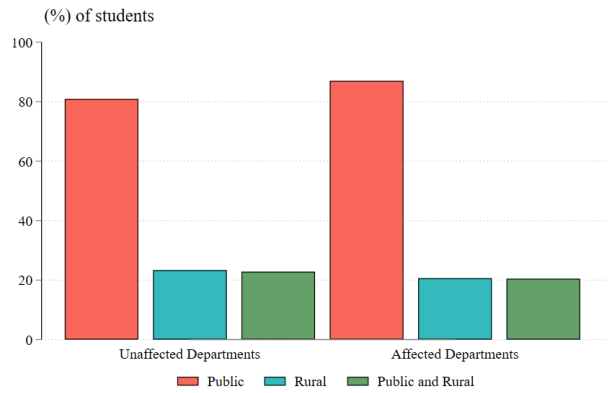
Notes. Each dot represents a school in the estimation sample. Source: Own elaboration based on data from DANE.

Appendix Figure A.9: Proportion of Public and Rural Schools and Students

(a) Schools

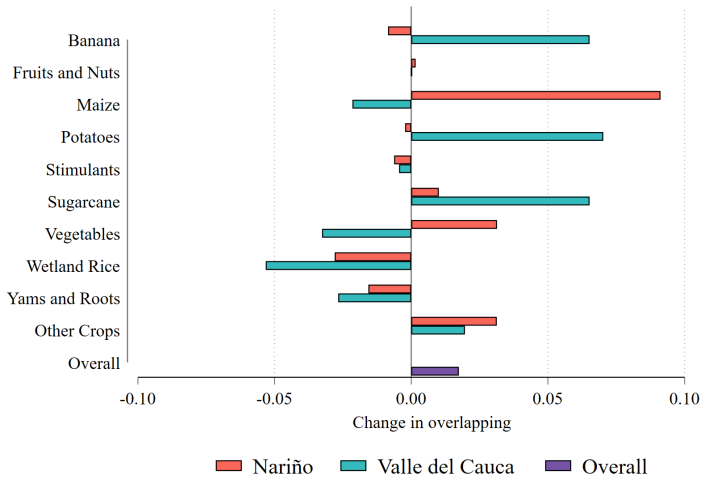


(b) Students



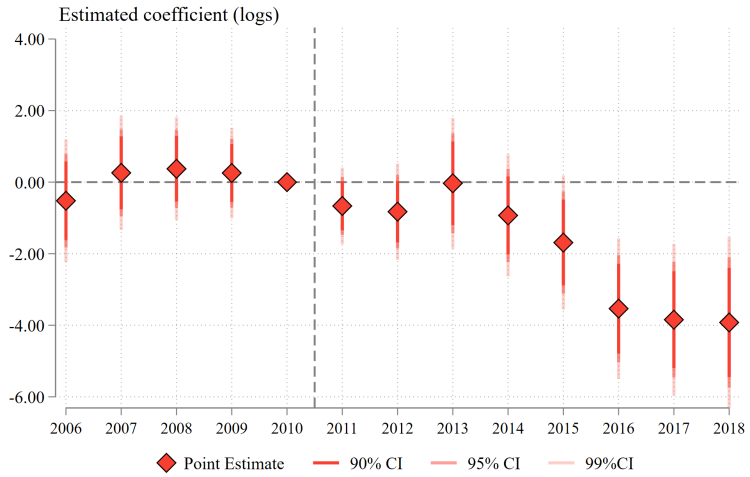
Notes: The figure shows the percentage of schools that are public, rural, or both in 2010, in affected departments (i.e., Nariño and Valle del Cauca), and in the rest of the country. Data come from the Colombian census of schools (Form C-600).

Appendix Figure A.10: Change in Calendar Overlap Induced by the Reform



Notes: The figure plots the average change in overlap between the academic and harvesting calendars before and after the reform, by crop, based on the most harvested crop in each *vereda* where schools are located. The overall average, shown by the purple bar, equals 0.017.

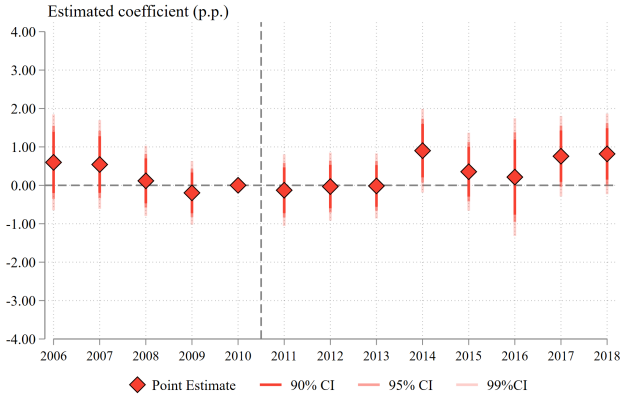
Appendix Figure A.11: Effect of the Reform on School Enrollment



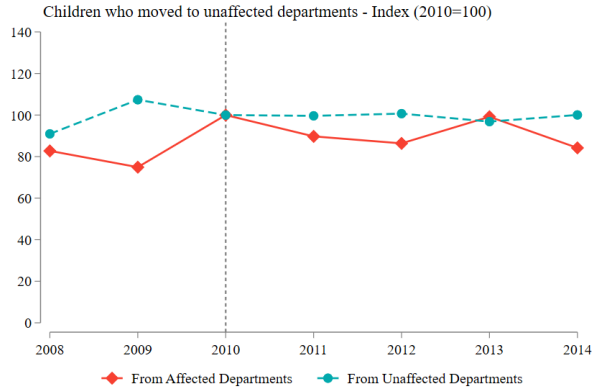
Notes: The figure plots the estimated coefficients on the interaction between the school-level change in calendar overlap, ΔO_s, and year indicators, relative to the baseline year 2010, for school enrollment, measured as the log of the total number of students per school. Estimates come from the event-study specification Equation 4, which includes school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure). Coefficients are expressed per unit of ΔO_s. The sample includes rural public schools in the affected departments and in the rest of the country, restricted to a balanced panel of schools observed in every year between 2006 and 2018. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the school level. Source: Own elaboration based on data from DANE.

Appendix Figure A.12: No Evidence of Reform-Induced Cross-Department Migration

(a) Effects on School Transfer Rate



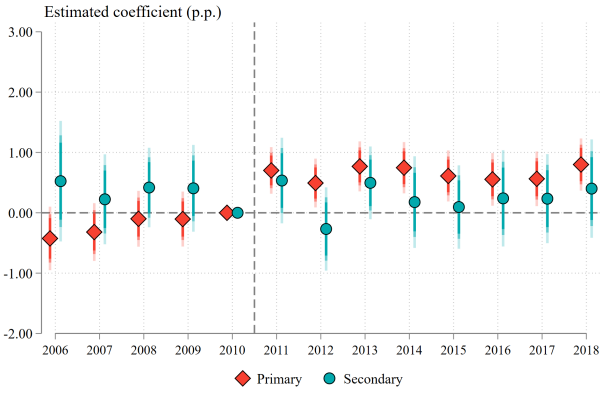
(b) Share of Migrant Children in Time



Notes: Panel A.12a plots the estimated coefficients on the interaction between the municipality-level change in calendar overlap and year indicators, relative to the baseline year 2010, for the transfer rate of rural public schools aggregated to the municipality level. The municipal dose is the baseline-enrollment-weighted average of the school-level change in overlap, ΔO_s. The specification includes municipality and year fixed effects and interactions between year fixed effects and baseline municipality characteristics, estimated on the balanced all-country panel of municipalities between 2006 and 2018. Coefficients are expressed per unit of the dose. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the municipality level. Source: Own elaboration based on data from DANE. Panel (b) plots, among children aged 5 to 15 interviewed in departments other than Nariño and Valle del Cauca, the share who resided in an affected department five years before the interview, using the GEIH migration module for survey years 2013 to 2019. Each observation is dated to the residence reference year, five years before the interview, and the series is indexed to 100 in 2010. Source: Own elaboration based on data from DANE.

Appendix Figure A.13: Heterogeneous Effects of the Reform by Educational Level

(a) Dropout rate



(b) Grade promotion

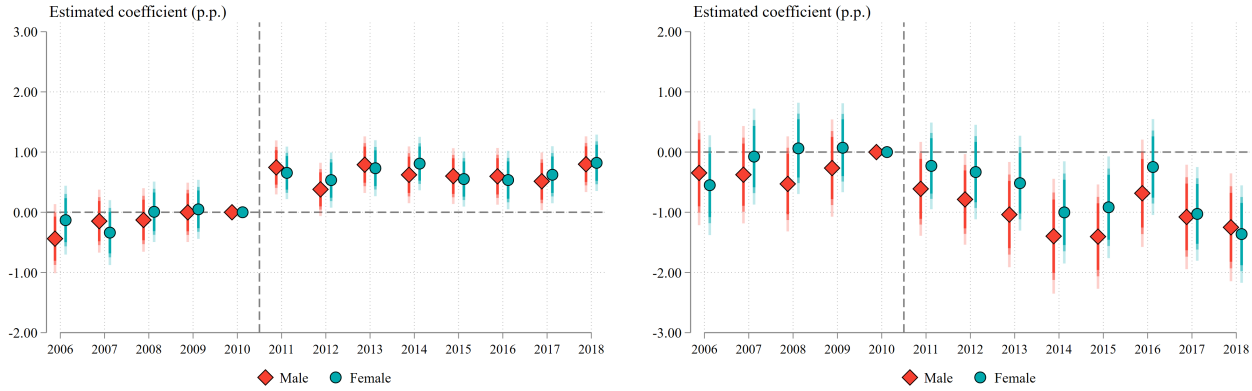


Notes: Each panel plots the estimated coefficients on the interaction between the school-level change in calendar overlap, ΔO_s, and year indicators, relative to the baseline year 2010, from the event-study specification of Equation 4, estimated separately for the outcomes of primary and secondary students. Each estimation includes school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure), and uses the balanced all-country panel of rural public schools for which the group-specific outcome is observed in every year between 2006 and 2018. Coefficients are expressed per unit of ΔO_s. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the school level. Source: Own elaboration based on data from DANE.

Appendix Figure A.14: Heterogeneous Effects of the Reform by Gender

(a) Dropout rate

(b) Grade promotion

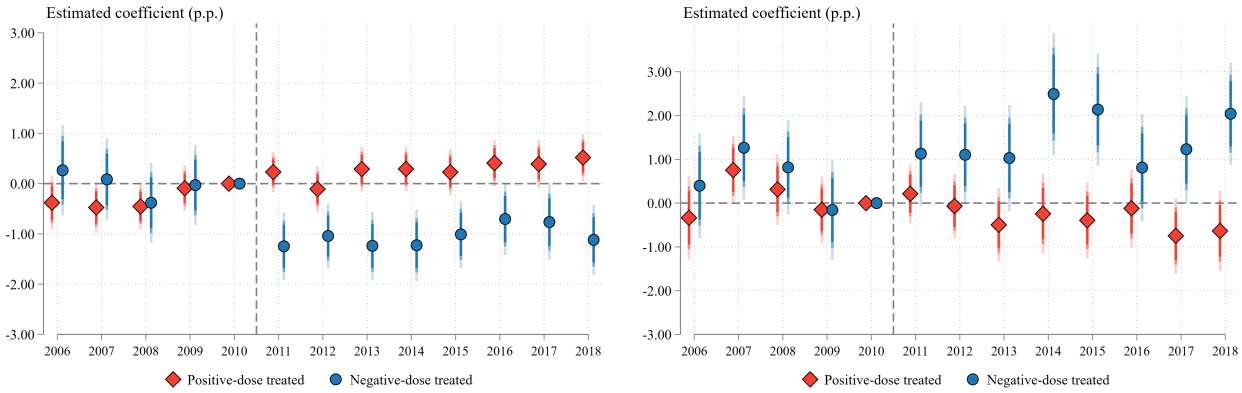


Notes: Each panel plots the estimated coefficients on the interaction between the school-level change in calendar overlap, ΔO_s, and year indicators, relative to the baseline year 2010, from the event-study specification of Equation 4, estimated separately for the outcomes of male and female students. Each estimation includes school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure), and uses the balanced all-country panel of rural public schools for which the group-specific outcome is observed in every year between 2006 and 2018. Coefficients are expressed per unit of ΔO_s. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the school level. Source: Own elaboration based on data from DANE.

Appendix Figure A.15: Effects of the Reform by Direction of the Change in Overlap

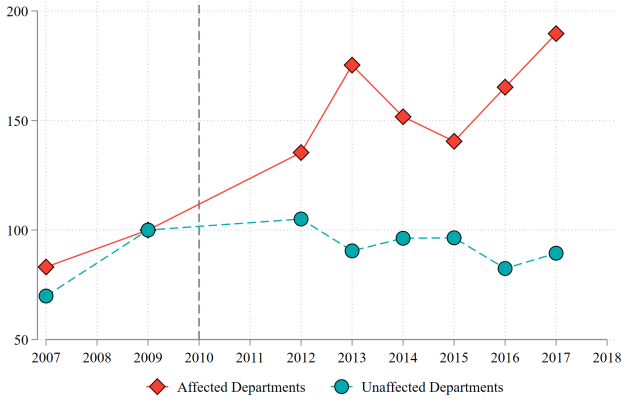
(a) Dropout rate

(b) Grade promotion

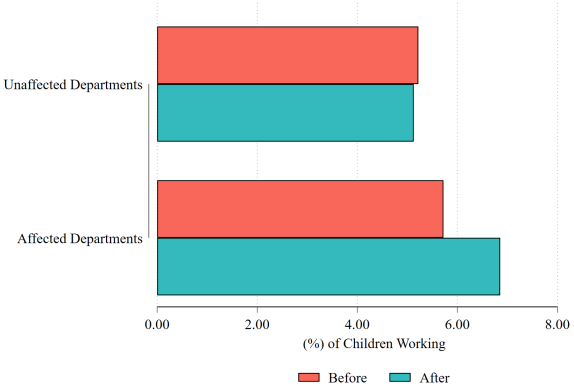


Notes: Each panel plots the estimated dynamic effects of the reform per unit of $|\Delta O_s|$, the absolute value of the school-level change in calendar overlap, interacted with year indicators relative to the baseline year 2010, estimated separately for treated schools whose overlap increased (positive dose) and treated schools whose overlap decreased (negative dose), each compared against the full all-country control group. Coefficients are therefore read in each group's own direction. For the negative-dose group, a negative coefficient indicates that the outcome falls as overlap decreases. Dropout and grade promotion are the shares of students who dropped out or were promoted to the next grade, respectively. Estimates come from the event-study specification of Equation 4, which includes school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure). The sample includes rural public schools in the affected departments and in the rest of the country, restricted to a balanced panel of schools observed in every year between 2006 and 2018. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the school level. Source: Own elaboration based on data from DANE.

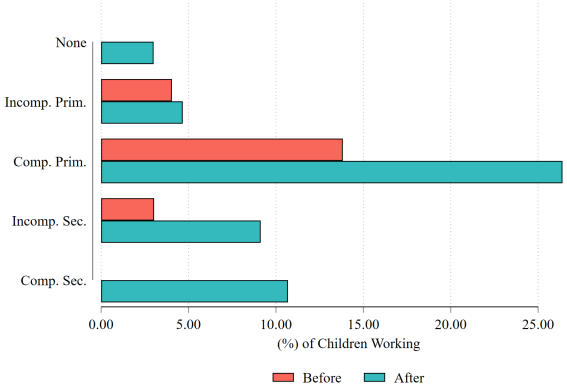
Appendix Figure A.16: Share of Children Working in Agriculture



(a) Evolution over time



(b) In Primary School



(c) By Educational Level

Panel (a) shows the evolution of the share of children working in agriculture and silviculture over time, normalized to 100 in 2009, for Nariño and Valle del Cauca compared to the rest of Colombia. Panel (b) shows the share of children with incomplete or complete primary education working in agriculture and silviculture before and after the academic calendar change, for Nariño and Valle del Cauca compared to the rest of Colombia. Panel (c) shows the share of children working in agriculture and silviculture before and after the academic calendar change, by educational level, for Nariño and Valle del Cauca. Source: Own elaboration based on data from DANE.

Appendix Table A.2: Descriptive Statistics of Key Variables

	Mean	SD	p25	p50	p75	Min	Max
Panel A: All countries							
O_i (overlap)	0.148	0.103	0.075	0.142	0.200	0.000	0.512
Out-of-school share	0.089	0.106	0.016	0.042	0.122	0.000	0.451
Z_i (instrument)	0.117	0.100	0.000	0.136	0.194	0.000	0.419
Z_i^C (exposure-weighted instrument)	0.123	0.105	0.000	0.143	0.203	0.000	0.448
Colony indicator	0.675	0.470	0.000	1.000	1.000	0.000	1.000
Panel B: Colonies (colony = 1)							
O_i (overlap)	0.178	0.103	0.112	0.167	0.249	0.000	0.512
Out-of-school share	0.115	0.117	0.022	0.070	0.176	0.002	0.451
Z_i (instrument)	0.174	0.071	0.136	0.166	0.224	0.000	0.419
Z_i^C (exposure-weighted instrument)	0.182	0.075	0.142	0.173	0.228	0.000	0.448
Panel C: Non-colonies (colony = 0)							
O_i (overlap)	0.087	0.069	0.027	0.082	0.145	0.000	0.260
Out-of-school share	0.033	0.042	0.009	0.019	0.042	0.000	0.202
Z_i (instrument)	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Z_i^C (exposure-weighted instrument)	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes. Z_i is the baseline instrument and Z_i^C the exposure-weighted instrument defined in Appendix B.2. O_i is the observed overlap between the school calendar and the agricultural calendar. Out-of-school share is the share of primary-school-age children out of school. Colony indicator equals 1 if the country was a former colony of Belgium, France, Portugal, Spain, the Netherlands, or the UK, and 0 otherwise. Panel A reports statistics for the full sample, Panel B restricts to former colonies, and Panel C restricts to non-colonies. Source: own elaboration based on administrative records, World Bank Indicators, Penn World Table version 11.0, and Sacks et al. (2010).

Appendix Table A.3: Calendar Overlap and School Dropout Rates by Colonies and Non-Colonies

	Dependent variable: Dropout rate				
	(1)	(2)	(3)	(4)	(5)
Panel A: Main effects					
Calendar Overlap	0.232***	0.167*	0.209**	0.207**	0.209*
	(0.082)	(0.093)	(0.100)	(0.103)	(0.106)
Observations	120	120	120	120	120
Panel B: Colonies					
Calendar Overlap	0.096	0.108	0.180	0.163	0.184
	(0.116)	(0.115)	(0.121)	(0.131)	(0.133)
Observations	81	81	81	81	81
Panel C: Non-colonies					
Calendar Overlap	0.065	0.031	0.026	-0.058	-0.052
	(0.120)	(0.096)	(0.098)	(0.070)	(0.078)
Observations	39	39	39	39	39
Geographic controls	No	Yes	Yes	Yes	Yes
Macroeconomic controls	No	No	Yes	Yes	Yes
Demographic controls	No	No	No	Yes	Yes
Trade controls	No	No	No	No	Yes

Notes. The dropout rate is defined as the share of primary school-age children out of school. For each country, we use the latest available value during the 2010–2019 period. Geographic controls: country latitude. Macroeconomic controls: GDP and consumption share of GDP. Demographic controls: population. Trade controls: exports and the total trade volume between each country and the set of colonizers, comprising Belgium, France, Portugal, Spain, the Netherlands, and the UK. Source: own elaboration based on administrative records, World Bank Indicators, Penn World Table version 11.0, and Sacks et al. (2010).

Appendix Table A.4: Descriptive Statistics

	Before		After	
	Affected	Unaffected	Affected	Unaffected
Panel A: Schooling outcomes				
No. students enrolled	78.66	77.57	70.68	75.42
Dropout rate	0.068	0.078	0.034	0.043
Grade promotion rate	0.844	0.819	0.874	0.847
Schools	1,668	16,885	1,668	16,885
Panel B: Calendar overlap (share of year, 2010)				
Overlap under Calendar A	0.196			
Overlap under Calendar B	0.194			
Change in overlap	0.002			
Change in overlap (crop level)	0.017			
Share with positive change	0.604			
Share with no change	0.092			
Share with negative change	0.305			
Panel C: Sample composition (2010)				
Share rural (all schools)	0.565	0.599		
Share public (all schools)	0.843	0.888		
Share within 40 km of boundary (bordering depts.)	0.769	0.493		
Share within 50 km of boundary (bordering depts.)	0.869	0.605		
Share within 60 km of boundary (bordering depts.)	0.940	0.712		
Panel D: Baseline municipality characteristics (2010)				
GDP per capita (thousands of pesos)	6,734	7,189		
Terrorist attacks	5.44	2.25		
Coca cultivation (ha)	116.92	99.13		
Municipal public expenditure	59,950	34,199		
Panel E: Teachers				
No. teachers	3.81	3.33	3.68	3.55
Share male teachers	0.322	0.307	0.336	0.327
Share teachers with postsecondary	0.721	0.648	0.827	0.751
Panel F: Crop harvesting (veredas)				
Harvested area (ha)	158.3	110.2		
No. of products	9.8	11.1		
Stimulants (ha)	28.2	24.6		
Wetland rice (ha)	26.0	13.0		
Maize (ha)	25.8	16.6		
Veredas	2,400	29,905		

Notes. Affected departments are Nariño and Valle del Cauca. Unaffected departments are the rest of the country. Before and After refer to the periods 2006-2010 and 2011-2018. Panels A and E report the average of each variable across schools in the estimation sample of rural public schools. Panel B reports calendar overlap measures as shares of the calendar year, computed for the baseline year 2010. The school-level change weights crops by each school's surrounding *vereda* crop composition, while the crop-level change is the unweighted mean across crop-by-department cells. Panel C reports the share of rural and public schools among all schools in the census, and the share of schools located within each distance of the boundary of the affected departments. Panel D reports baseline municipality characteristics in 2010, averaged across schools. Panel F reports the average harvested area and the average number of distinct crops across all *veredas* in affected and unaffected departments. Source: Own elaboration based on data from DANE, CEDE, and the Global Agro-Ecological Zones (GAEZ) v4, FAO.

Appendix Table A.5: Effect of Calendar Overlap on Schooling Outcomes: Dose-Response Estimates

	Dropout rate		Grade promotion		Enrollment (logs)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All students						
Calendar overlap ($\Delta O_s \times$ Post)	0.711***	0.740***	-0.654***	-0.691***	-2.169***	-1.984***
	(0.094)	(0.096)	(0.144)	(0.143)	(0.609)	(0.592)
Observations	240,448	240,448	240,448	240,448	240,448	240,448
Panel B: Primary students						
Calendar overlap ($\Delta O_s \times$ Post)	0.802***	0.829***	-0.699***	-0.742***	-2.364***	-2.024***
	(0.100)	(0.102)	(0.153)	(0.152)	(0.592)	(0.578)
Observations	232,999	232,999	232,999	232,999	232,999	232,999
Panel C: Secondary students						
Calendar overlap ($\Delta O_s \times$ Post)	-0.152	-0.079	-0.243	-0.319	1.177	0.582
	(0.190)	(0.181)	(0.337)	(0.324)	(1.189)	(1.158)
Observations	30,979	30,979	30,979	30,979	30,979	30,979
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls $\times$ year FE	No	Yes	No	Yes	No	Yes

Notes: The table reports the static dose-response estimate of Equation 5 on the all-country sample of rural public schools. Odd columns include school and year fixed effects only, even columns add interactions between year fixed effects and baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure). Within each column pair, both specifications are estimated on the same sample of schools with nonmissing baseline controls. Panels B and C estimate the specification on the outcomes of primary and secondary students, each on its own balanced panel. Standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Own elaboration based on data from DANE.

B. Worldwide School and Agricultural Calendars

B.1. Data Construction

In this appendix, we describe the construction of the school and agricultural calendars for each country in our sample, as well as the measure of overlap between them.

School Calendar. We hand-collected academic calendar information from the official websites of ministries of education or equivalent government bodies in each country, using the most recently available calendar at the time of data collection. For each country, we recovered the start and end dates of the academic year, as well as the dates of all main school breaks. We then constructed a day-level indicator variable equal to one if school is in session on a given day of the year and zero otherwise.

For countries with multiple regional calendars, we selected the calendar of the most populous region or capital. Specifically, we used England for the United Kingdom, Berlin for Germany, the Central Region for the Netherlands, the French Community for Belgium, Washington D.C. for the United States, and Ontario for Canada. For Nigeria, India, and Indonesia, however, we deliberately deviated from this convention. Rather than selecting the capital region, we selected the calendar of the region that is the primary producer of the country's main crop as identified in the harvesting data: Kano state for Nigeria (sorghum), West Bengal for India (rice), and Central Java for Indonesia (rice). This choice ensures that the school calendar used in the overlap calculation reflects the institutional environment actually faced by farming households in the main crop-producing areas.

Harvesting Calendar. To construct harvesting calendars, we rely on the crop calendar dataset of Sacks et al. (2010), which reports the start and end dates of planting and harvesting seasons for nineteen major crops in each country. We define each country's main crop as the one with the largest national harvested area, using the harvested-area information distributed with the crop calendar dataset. For crops with multiple growing seasons, we use the main season as designated in the dataset. For a small number of countries not covered by these sources, we complement them with the FAO GIEWS country crop calendars.

Overlap Measure. Based on the school and harvesting calendars described above, we compute for each country the share of the year in which both school is in session and the main crop harvest is underway. Formally, this is the number of days on which both activities overlap, divided by 365. This measure captures the extent to which the institutional school calendar conflicts with the peak labor demand period in agriculture.

Educational Laws. To measure the number of years elapsed between the institutionalization of each colonizer's education system and the independence of each colony, we identify for each colonizer the foundational law that established public primary schooling as a state responsibility. These laws share a common logic: they replaced church-dominated or locally fragmented schooling with a centralized, state-regulated system of primary education, thereby creating the centralized institutional framework through which academic calendars were subsequently standardized and transmitted to colonial territories. We use the dates of these laws as markers of when each colonizer's centralized education system, of which the calendar became a component, was institutionalized.

For France, we use the Loi Guizot of 28 June 1833 (Loi sur l'instruction primaire), which was the first law to create a durable national primary school system, requiring every municipality to maintain at least one school, establishing a mandatory curriculum, and creating a national teacher certification system and a network of teacher-training colleges. For Belgium, we use the Loi Nothomb of 23 September 1842 (Loi organique de l'instruction primaire), which was the founding act of Belgium's education system as an independent state, obliging every municipality to maintain at least one primary school and embedding religious instruction

within the academic calendar. For the Netherlands, we use the Schoolwet van 1806 (Wet voor het Lager Schoolwezen en de Onderwijzers), which, despite being preceded by two short-lived school acts in 1801 and 1803, was the first durable organic law of Dutch primary education, remaining in force for over fifty years and establishing classroom-based instruction, teacher examinations, and national school inspections as the pillars of a secular public school system. For Spain, we use the Ley de Instrucción Primaria of 21 July 1838 (Ley Someruelos) together with its Reglamento of 26 November 1838, which collectively established the first functioning liberal primary school system in Spain, requiring municipalities to maintain public schools, introducing teacher certification, and creating provincial supervision of education.[45] For Portugal, we use the Carta de Lei de 6 de Novembro de 1772, enacted under the Marquis of Pombal, which abolished Jesuit-controlled schooling, created a state-funded network of royal primary schools across Portugal and its empire, and introduced the subsídio literário—a dedicated national tax to fund teachers' salaries—marking the first systematic intervention of the Portuguese state in public primary education.[46] For the United Kingdom, we use the Elementary Education Act of 1870 (the Forster Act), which was the first law to establish a national framework for public primary schooling in England and Wales, filling the gaps left by the existing patchwork of voluntary and church schools by creating elected local school boards empowered to build and maintain publicly funded schools, levy local taxes for education, and—for the first time—work toward universal provision of elementary schooling across the country.

Independence Years. For each country in our sample, we use the year of independence from its corresponding colonizer. In most cases, this corresponds to the year in which the country formally declared or was granted independence. For a small number of cases, the year we use requires clarification. For Bangladesh, we use 1947—the year of the partition of British India and the creation of Pakistan, of which Bangladesh (then East Pakistan) was a part—rather than 1971, the year of Bangladesh's independence from Pakistan, since our interest is in the duration of exposure to the British institutional framework. For the Philippines, we use 1898—the year Spain ceded the territory following the Spanish-American War—rather than 1946, the year of formal independence from the United States, since our colonizer of interest is Spain and 1898 marks the effective end of Spanish institutional influence over the country. For the Dominican Republic, we use 1865—the year of its final independence from Spain following the War of Restoration—rather than 1844, the year of its independence from Haiti, since our colonizer of interest is Spain. In all three cases, the years we use correspond to the moment at which the country ceased to be under the direct institutional influence of the colonizer whose education system we are studying, which is the relevant date for our analysis.

B.2. Robustness to the Instrumental Variable Strategy

We provide robustness checks to our results in Section 2.3 by introducing a second instrument that exploits the interaction between the school calendar of the colonizing country, the agricultural calendar predicted by their latitude, and the degree of exposition to colonial

[45] The institutional framework established by the *Ley Someruelos* was later consolidated and extended by the *Ley de Instrucción Pública* of 9 September 1857 (*Ley Moyano*), which introduced compulsory and free elementary schooling and integrated primary, secondary, and higher education into a single national system. While the *Ley Moyano* is often cited as the more comprehensive foundational law of Spanish education, we use the 1838 law as our reference date as it marks the first effective state intervention in primary schooling.

[46] Subsequent nineteenth-century reforms, notably those of 1822, 1835, and 1844, sought to reorganize and expand primary schooling in Portugal under liberal constitutional governments, but were largely left incomplete due to the country's severe economic difficulties and political instability during this period. We nonetheless retain the 1772 Pombaline reform as our reference date, as it constitutes the original act by which the Portuguese state assumed direct responsibility for public education, displacing the Jesuit institutional model that had governed schooling in Portugal and its overseas territories since the sixteenth century.

education institutions. Let $c(i)$ denote the country that colonized country i and let $\mathbb{L}_i$ denote the set of countries located at latitudes similar to that of country i. We construct the following instrument:

$$Z_i^C = \left(\frac{1}{|\mathbb{D}|} \sum_{d \in \mathbb{D}} S_{c(i),d} \; \hat{p}_{id} \right) \times \frac{Y_i^{indep}}{Y_{c(i)}^{educ}},$$

where $S_{c(i),d}$ is an indicator equal to one if schools are in session in the colonizing country $c(i)$ on day d, $\hat{p}_{id} \in [0,1]$ is the leave-one-out predicted probability that a country with the agricultural conditions of country i is harvesting on day d, as defined in Section 2.3, and the final term is the ratio of the year in which country i gained independence to the year in which the colonizer institutionalized its school calendar. The ratio exceeds one for colonies that remained under colonial rule after the colonizer's calendar was institutionalized, and increases with the length of that post-institutionalization exposure, so the instrument places relatively more weight on countries whose school systems developed while the colonizer's calendar template was already in place. Using this instrument, we estimate Equation 1.

This alternative instrument combines two sources of variation in a manner similar to a shift-share design (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2021). Because the instrument is the product of the two components, its validity rests on the excludability of the interaction itself, as we discuss below. The plausibility of that requirement, in turn, draws on the properties of each component.

The *share* component corresponds to the same used in Section 2.3, and quantifies the overlap that would arise if former colonies adopted the school calendar of their colonizer while facing agricultural conditions determined by their latitude. This term isolates variation in school calendars inherited from the colonizer while accounting for local agricultural cycles. Its exogeneity is plausible because colonial powers originally designed their domestic school calendars to reflect their own climatic and agricultural conditions rather than those of the territories they later administered. Moreover, by using agricultural calendars derived from countries located at similar latitudes, the instrument captures agricultural conditions predicted by geography rather than country-specific institutional choices.

The *shift* component captures the extent to which colonial institutions influenced the determination of school calendars in country i. Specifically, it reweights the inherited calendar variation according to the timing of independence relative to the educational reform in the colonizing country. Countries that gained independence later were more likely to inherit the colonizer's calendar, while those that became independent earlier had greater scope to adopt calendars adapted to local conditions. This timing is plausibly exogenous to current educational outcomes because the date of colonial independence relative to the timing of educational reforms in the colonizing country was largely determined by geopolitical and historical forces unrelated to contemporary school dropout.

Taken together, this second instrument captures the overlap that country i would have experienced if it had adopted the colonizer's calendar while facing agricultural conditions determined by its latitude, with relatively more weight placed on countries that remained under colonial rule after the colonizer institutionalized its calendar. The validity of this instrument requires that the interaction itself be excludable. Conditional on the controls, the product of the colonizer-calendar overlap and the length of colonial exposure must affect dropout only through the inherited school calendar, which is a different, and not weaker, requirement than the exogeneity of either component alone. Its plausibility rests on the same logic as the baseline instrument, with exposure duration entering as an intensity weight. Colonizers institutionalized their calendars for their own conditions, and a longer period of colonial rule after that institutionalization gave the transplanted calendar more time to take root. Because the specifications control for latitude directly, the instrument's identifying variation does not reduce to geographic location. We therefore read the estimates in Table B.1

as a robustness check under an alternative exposure weighting rather than as an independently identified estimate.

Table B.1 presents the results of the estimations using this version of the instrument. In the full sample, the coefficients are precisely estimated and range from 0.81 to 0.84, close to the baseline estimates. Among former colonies, the coefficients remain positive and statistically significant, ranging from 0.56 to 0.68, somewhat below the full-sample estimates but consistent with them in sign and significance. Our results are therefore robust to this alternative exposure weighting of the instrument.

Appendix Table B.1: Effect of Calendar Overlap on School Dropout Rates

	Dependent variable: Dropout rate				
	(1)	(2)	(3)	(4)	(5)
Panel A: Full sample					
Calendar Overlap	0.837***	0.821***	0.811***	0.824***	0.615**
	(0.292)	(0.259)	(0.252)	(0.259)	(0.290)
First-stage F	18.35	17.59	17.92	16.79	11.71
Observations	120	120	120	120	120
Panel B: Former colonies					
Calendar Overlap	0.558*	0.649**	0.646**	0.684**	
	(0.302)	(0.301)	(0.293)	(0.313)	
First-stage F	11.67	11.34	11.76	10.50	
Observations	81	81	81	81	
Geographic controls	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes
Trade controls	No	No	No	Yes	Yes
Colonial status control	No	No	No	No	Yes

Notes. The dropout rate is defined as the share of primary school-age children out of school. For each country, we use the latest available value during the 2010–2019 period. Geographic controls: country latitude. Macroeconomic controls: GDP and consumption share of GDP. Demographic controls: population. Trade controls: exports and the total trade volume between each country and the set of colonizers, comprising Belgium, France, Portugal, Spain, the Netherlands, and the UK. Source: own elaboration based on administrative records, World Bank Indicators, Penn World Table version 11.0, and Sacks et al. (2010).

C. Historical Evolution of the Academic Calendar in Colombia

The academic calendar in Colombia has undergone substantial transformations since the early nineteenth century. These changes reflect shifting priorities in educational administration, evolving degrees of centralization, regional heterogeneity, and growing recognition of climatic and institutional differences across the country. From an initially centralized system that tightly regulated vacations and examinations nationwide, Colombia progressively moved toward differentiated academic calendars across regions—later formalized as Calendar A and Calendar B—before adopting a decentralized framework that allows territorial entities to define calendars within national guidelines.

C.1. Colonial Period (Before 1820s)

During the colonial period, particularly before the eighteenth century, education in Spain and its colonies was primarily provided by the Church in a decentralized manner. Since the sixteenth century, instruction was largely delivered by Catholic religious orders—such as Jesuits, Franciscans, and Dominicans—who focused mainly on teaching Christian doctrine and basic literacy in Indigenous languages, while learning Spanish was often optional. Education was typically delivered in *escuelas de primeras letras*, institutions that provided basic instruction by introducing children to Christian principles and values, teaching them to read, familiarizing them with the four basic arithmetic operations, and developing writing skills through exemplars and guided practice (Anguita Osuna, 2019). Over time, however, strong pressure (especially from archbishops, the Council of the Indies, and Crown officials) emerged to promote instruction in Spanish, as Indigenous languages were increasingly viewed as obstacles to the diffusion of Christianity.

During the eighteenth century, the Spanish Crown introduced the Bourbon reforms to modernize state capacity in the colonies (Chiovelli et al., 2023). As part of these reforms, and influenced by Enlightenment ideas, Carlos III promoted educational reforms that reduced the Church's influence over schooling and expanded free primary instruction in Spain (Anguita Osuna, 2019), and colonial authorities pursued parallel efforts to broaden educational access in the Americas (Herrera, 2004; García, 2005). This process gained momentum after the suppression of the Society of Jesus, which entailed the expulsion of Jesuit members from Spain and its colonies in 1767. Nevertheless, education remained accessible only to a small elite, with broad segments of society excluded (García, 2005). Instructional institutions were scarce in the Kingdom of New Granada (which encompassed present-day Colombia), as evidenced by the presence of only one free *escuela de primeras letras* in Santa Fe, the capital of the kingdom and the site of present-day Bogotá (García, 2005). Importantly, school calendars during this period played little role in shaping educational access.

C.2. Early Republican Regulation and Centralization (1820s–1870)

The landscape of Colombian society in the early years of national consolidation highlighted the need for a new and more structured educational system. Independence leaders promoted the creation of civic educational institutions, such as *colegios* and public schools, framed within republican ideals of citizenship and Enlightenment thought, although resources and administrative capacity were limited. Efforts were made to implement laws and establish institutions that expanded access and provided practical instruction, but their reach remained uneven, and religious actors continued to exert significant influence (Báez Osorio, 2006).

Historical overviews suggest that school calendars were not a major policy concern in the early republic (Báez Osorio, 2006). Educational provision was still being organized, and

formal structures such as fixed academic calendars were not yet nationally institutionalized.

Nonetheless, the earliest national regulation potentially affecting the academic calendar dates to 1824, when Vice President Francisco de Paula Santander issued a decree establishing that school vacations should coincide with the recesses observed by courts and tribunals, as defined in the Law of October 12, 1821. Under Articles 70 and 71 of that law, judicial activity was suspended only during major religious feast days, Holy Week, and December. Consequently, school vacations were institutionally anchored to these same periods.

This framework was reinforced by the General Law of 1826, which explicitly defined the structure of the academic year. Article 112 stipulated that annual vacations would run from the conclusion of examinations in November until January 1, and that no additional recesses would be permitted beyond religious feast days and Easter. Contemporary correspondence illustrates that this reform curtailed earlier local practices. In December 1826, students from the *Colegio de Boyacá* protested the elimination of a traditional vacation period running from late July to mid-October, which was replaced by a single end-of-year recess in December following the enactment of Santander's legislation. The governor of Boyacá responded that the matter fell under central authority, and the reform was ultimately reaffirmed.

This episode indicates that, prior to the reforms promoted under Santander's administration, academic calendars in at least some institutions followed a timing that differed substantially from the post-1826 national standard, including a long mid-year interruption with instruction extending across the calendar year. Because the colonial record described above contains no formalized term schedule, whether this earlier arrangement derived from Spanish-period practice cannot be established from the available sources. What the episode documents is that, from 1826 onward, such local arrangements were explicitly displaced by a centrally imposed calendar more closely resembling the January-November structure.

The emphasis on standardization continued with the Organic Decree of Public Instruction of 1870, which formalized a system of two academic terms per year, each lasting five months. Schools were to open on January 10 and July 10 and close on May 31 and November 30, with vacations in June and December. This decree established a nationally uniform structure that would remain influential for decades.

C.3. Consolidation and Incremental Adjustments (1880s–1890s)

Late nineteenth-century sources indicate both continuity and incremental adjustments within this centralized framework. Institutional correspondence from 1888 shows that intermediate examinations were held in late July, followed by short vacations, while final examinations took place in November. National decrees in 1892 and subsequent regulations consistently defined the academic year as running from February (or January) to November, with vacations concentrated in December–January.

However, some institutional and regional variation emerged. For example, the *Colegio Público de Barranquilla*, established in 1892, followed an academic year running from August to June. Regulations for normal schools and primary schools in 1893 retained the February–November or January–November structure but clarified enrollment periods, examination timing, and paid vacation months. These variations, however, remained exceptions within an otherwise standardized national system.

C.4. Departmental Differentiation in the Early Twentieth Century (1900s–1940s)

By the early twentieth century, departmental regulations increasingly reflected differentiated academic timing. Resolutions from 1906 illustrate this divergence clearly. In the Department of Cauca, which at the time still included the territory that in 1910 became the

department of Valle del Cauca, secondary and professional institutions were required to operate on an academic year running from October 1 to July 20. By contrast, in the Department of Huila the school period was fixed from January 10 to November 30, with additional vacations during Holy Week and in late July.

A significant event occurred in 1913, when the Governor of Antioquia decreed that the academic year should begin in mid-September and end in June. The decree explicitly justified this structure by reference to pedagogical considerations, climatic conditions, and the convenience of students pursuing studies abroad.

Regional differentiation became increasingly pronounced in the southwest. Decree 1457 of 1939 required the Rural Normal School of Popayán to suspend classes at the end of July and resume them in October. Decree 3087 of 1945 further confirmed that institutions in the southwest operated under a calendar distinct from that followed in other regions. By this period, Colombia effectively maintained multiple regional academic calendars, though they had not yet been formally labeled.

C.5. Formal Recognition of Dual Calendars (1950s–early 1960s)

The regional division of academic calendars became explicit and institutionalized in the mid-twentieth century. Decree 0075 of 1951 clearly differentiated academic timing across regions: colleges in Valle del Cauca and Nariño were to implement the new curriculum beginning in October, while institutions in the rest of the country would do so beginning in February. This marked the first nationwide decree to formally recognize a dual calendar structure aligned with regional geography.

An attempt to unify the system followed with Decree 3109 of 1956, which mandated the unification of calendars beginning in September 1958. However, subsequent amendments—particularly Decree 1254 of 1957—introduced transitional adjustments, reflecting practical difficulties in implementing uniformity.

Rather than eliminating differentiation, reforms in the early 1960s codified it more precisely. Decree 45 of 1962 and Decree 1710 of 1963 established detailed academic periods, vacations, and examination schedules separately for the central-eastern and south-western regions, covering both secondary and primary education. These distinctions were further reinforced by Decree 2720 of 1963, which formally distinguished Calendar A (central-eastern region) and Calendar B (south-western region) and explicitly allowed schools to choose which calendar to follow.

C.6. Attempts at Unification and Reinstatement of the Dual System (Late 1960s–1980s)

A renewed effort to impose national uniformity came with Decree 1816 of 1967, which standardized instructional days and vacation lengths nationwide while allowing calendars to differ only in start dates. This reform was implemented gradually between 1968 and 1969.

However, the reform proved unsustainable. Decree 1902 of 1969 reversed course, citing reduced end-of-year vacations, administrative constraints in educational planning, the need for mid-year teacher training, climatic conditions in the southwest, and requests from school communities. The decree reinstated a clear territorial division: Calendar B for Nariño, Cauca, Valle del Cauca, and Putumayo, and Calendar A for the rest of the country.

This dual structure persisted through the 1970s and was reaffirmed in subsequent regulations. Decree 691 of 1972 established a provisional calendar following a nationwide suspension of activities, while Decree 174 of 1982 maintained Calendars A and B for both public and private institutions at the preschool and primary levels.

C.7. Decentralization and Contemporary Framework (2000s–Present)

A major institutional shift occurred with the decentralization reforms of the early 2000s. Law 715 of 2001 assigned departments responsibility for organizing education provision in non-certified municipalities, including defining the academic calendar, while granting certified municipalities autonomy over education provision. Decree 1850 of 2002 required certified territorial entities to issue annual academic calendars for all public institutions within their jurisdiction, explicitly allowing calendars to account for regional economic conditions and institutional traditions.

While this framework preserved the historical distinction between Calendars A and B, it also enabled greater coordination and gradual convergence in academic timing, particularly in certified and urban jurisdictions. As a result, contemporary academic calendars reflect both long-standing regional differentiation and increased institutional coordination within a decentralized governance structure.

D. Robustness of the School-Level Estimates

We provide additional evidence supporting our main results through several complementary exercises. We begin by showing that the effects on school progression are insensitive to alternative definitions of harvesting intensity. Then, we re-estimate the dose-response effects using alternative estimators that are robust to treatment-effect heterogeneity, restrict the control group to progressively more local comparisons, test the identifying assumption of parallel pre-trends across every specification, and summarize the full battery in specification-curve form. Finally, we present aggregate synthetic difference-in-differences estimates of the reform's intent-to-treat effects.

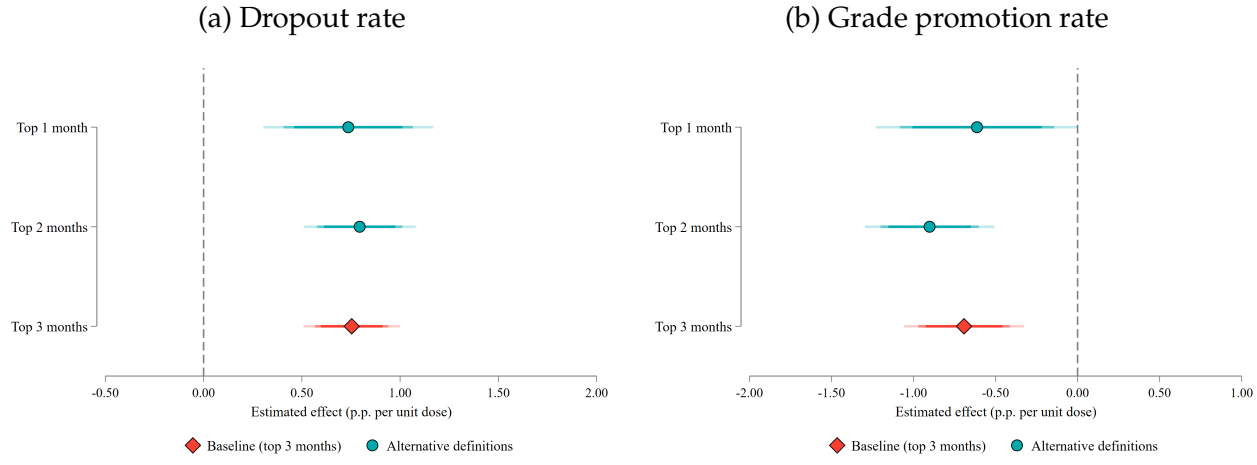
D.1. Alternative Definitions of Harvesting Intensity

Recall from Section 5.1 that the overlap measure for school s under calendar c is

$$O_{sc} = \sum_{j \in J} \omega_{sj} \left(\frac{1}{12} \sum_{m=1}^{12} \max \left\{ \mathbb{1}_{\{m \in \mathcal{I}_j\}} - V_{mdc}, 0 \right\} \right), \tag{D.1}$$

where $\mathcal{I}_j$ corresponds to the three most intensive harvesting months, the top quartile of the annual distribution of harvesting for crop j. This threshold is arbitrary. We therefore re-estimate our main specification varying $\mathcal{I}_j$ to include only the two most intensive months and only the single most intensive month. Appendix Figure D.1 reports the results, with the baseline measure included as a reference. The effects retain their magnitude, sign, and remain statistically significant under all three definitions.

Appendix Figure D.1: Effects Varying the Definition of Harvesting Intensive Months ($\mathcal{I}_j$)



Notes: Each panel reports the static dose-response estimate of Equation 5 under alternative definitions of harvesting intensity, for the dropout rate (Panel a) and the grade promotion rate (Panel b). In each panel, the dose ΔO_s is constructed from the single most intensive harvesting month of the year (Top 1), the two most intensive months (Top 2), and the three most intensive months (Top 3), the latter corresponding to the baseline definition used throughout the paper and displayed as a diamond. All specifications are estimated on the balanced all-country panel of rural public schools between 2006 and 2018 and include school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics. Horizontal bars indicate 90%, 95%, and 99% confidence intervals based on standard errors clustered at the school level. Source: Own elaboration based on data from DANE.

D.2. Specification Curves

We estimate the static dose-response effect of the calendar reform for each of our outcomes (and, for completeness, also for school enrollment, measured in logs) under every combination of three estimators and seven control groups, for a total of 21 specifications per outcome.

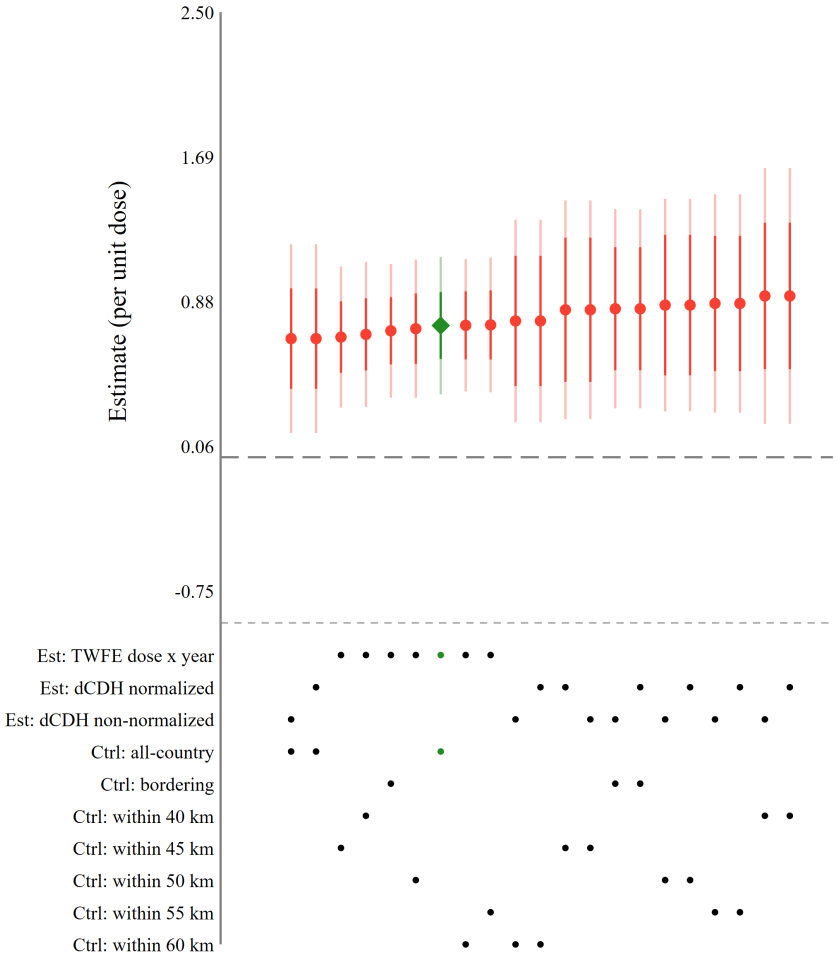
The three estimators are: (i) the two-way fixed effects (TWFE) specification described in Section 5; (ii) the heterogeneity-robust estimator of de Chaisemartin et al. (2024) in its normalized version, which expresses the effect per unit of treatment dose; and (iii) the same estimator in its non-normalized version. All estimators include school and year fixed effects. Trend adjustment enters each estimator in its natural form. The TWFE specification interacts year fixed effects with baseline municipality characteristics (per capita GDP, terrorist attacks, coca cultivation, and municipal public expenditure), while the de Chaisemartin et al. (2024) estimator conditions on nonparametric cells defined by within-sample median splits of the same characteristics.

The seven control groups are: all rural public schools in the country; schools in the six departments bordering the treated ones; and schools located within 40, 45, 50, 55, and 60 kilometers of the boundary of the treated departments, based on georeferenced school locations.

Figures D.2, D.3, and D.4 present the resulting specification curves for dropout, grade promotion, and log enrollment. In each figure, the upper panel plots the 21 point estimates sorted by magnitude, with nested 95 percent confidence intervals clustered at the school level (solid) and at the municipality level (translucent). The lower panel indicates the estimator and control group corresponding to each specification. Each specification is colored according to the outcome of its joint pre-trend test: specifications in red do not reject the null hypothesis of no differential pre-trends at the 10 percent level, specifications in light blue reject at the 10 but not the 5 percent level, and specifications in dark blue reject at the 5 percent level.

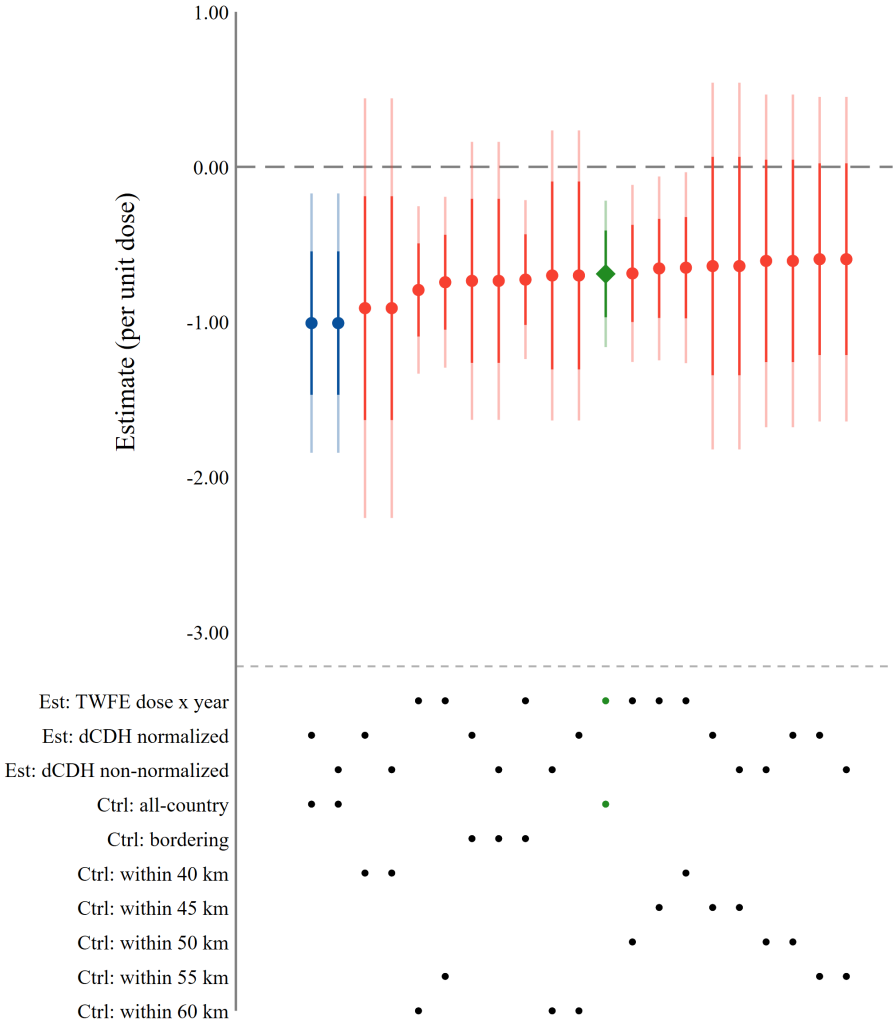
Three patterns emerge. First, the effect on dropout is positive and statistically significant at the 5 percent level in all 21 specifications, at both clustering levels, with estimates ranging from 0.59 to 0.90 percentage points per percentage point of overlap. Second, the effect on grade promotion is negative in all 21 specifications and statistically significant for most of them. Third, the effect on enrollment is negative in all specifications, ranging from 0.7 to 4.2 percent per percentage point of overlap, and statistically significant at conventional levels with school-level clustering in 20 of them. The only insignificant estimate corresponds to the narrowest control group (schools within 40 kilometers of the treated boundary), where the sample is smallest. No estimator or control group produces estimates that deviate from these patterns, indicating that the results are not an artifact of any particular modeling choice.

Appendix Figure D.2: Specification Curve: Dropout Rate



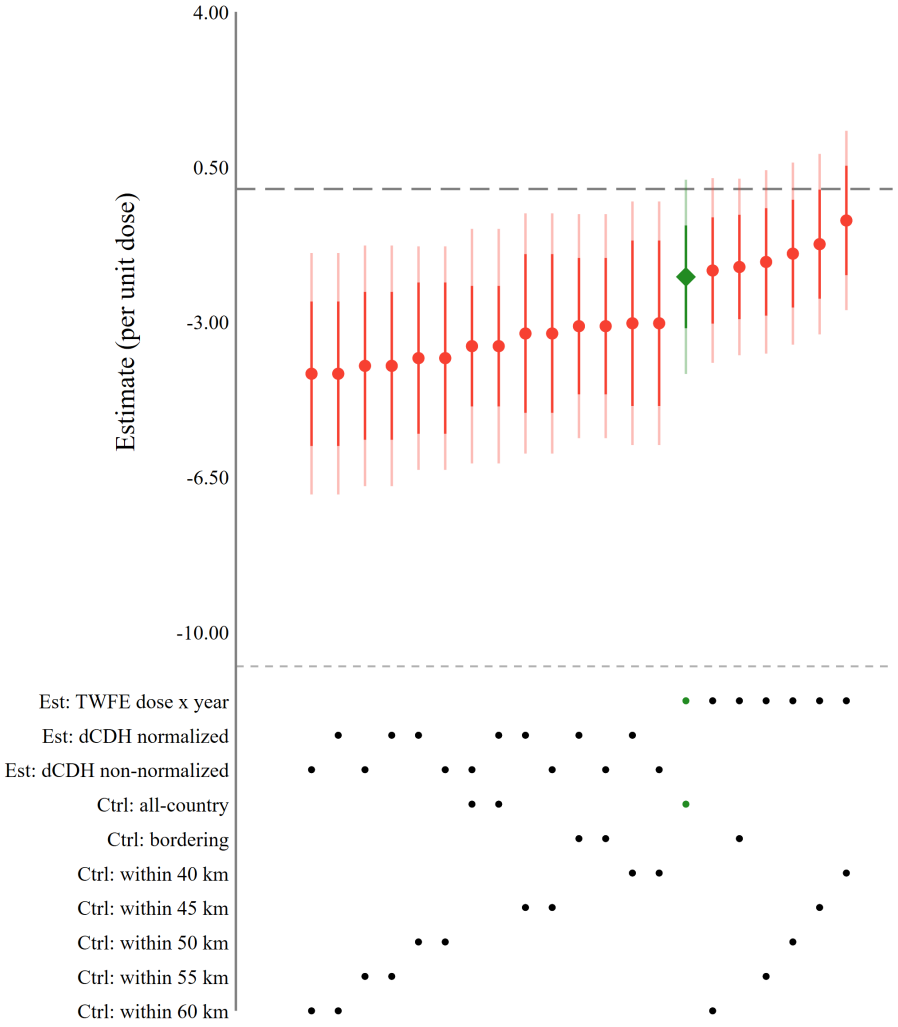
Notes: The upper panel plots the 21 static dose-response estimates for the dropout rate obtained by crossing three estimators with seven control groups, sorted by magnitude. Estimates are expressed per unit of the change in calendar overlap, ΔO_s. Solid vertical bars are 95% confidence intervals clustered at the school level; translucent bars are 95% confidence intervals clustered at the municipality level. The lower panel indicates the estimator and control group of each specification. Specifications in red do not reject the null hypothesis of no differential pre-trends at the 10 percent level; specifications in light blue reject at the 10 but not the 5 percent level; specifications in dark blue reject at the 5 percent level. Source: Own elaboration based on data from DANE.

Appendix Figure D.3: Specification Curve: Grade Promotion



Notes: The upper panel plots the 21 static dose-response estimates for the grade promotion rate. Estimates are expressed per unit of the change in calendar overlap, ΔO_s. Solid vertical bars are 95% confidence intervals clustered at the school level; translucent bars are 95% confidence intervals clustered at the municipality level. The lower panel indicates the estimator and control group of each specification. Specifications in red do not reject the null hypothesis of no differential pre-trends at the 10 percent level; specifications in light blue reject at the 10 but not the 5 percent level; specifications in dark blue reject at the 5 percent level. Source: Own elaboration based on data from DANE.

Appendix Figure D.4: Specification Curve: Enrollment (in logs)



Notes: The upper panel plots the 21 static dose-response estimates for log enrollment. Estimates are expressed per unit of the change in calendar overlap, ΔO_s. Solid vertical bars are 95% confidence intervals clustered at the school level; translucent bars are 95% confidence intervals clustered at the municipality level. The lower panel indicates the estimator and control group of each specification. Specifications in red do not reject the null hypothesis of no differential pre-trends at the 10 percent level; specifications in light blue reject at the 10 but not the 5 percent level; specifications in dark blue reject at the 5 percent level. Source: Own elaboration based on data from DANE.

D.3. Common Shocks and Within-Treated Identification

Because the dose ΔO_s is set to zero for all schools outside the affected departments, the dose-response coefficient combines variation in exposure among affected schools with the comparison between affected and unaffected schools. A post-reform shock common to all schools in Nariño and Valle del Cauca is therefore not mechanically excluded from the estimates. Table D.1 addresses this concern directly. It adds a full set of treated-by-year fixed effects to the baseline specification, absorbing any shock common to affected schools and identifying the coefficient exclusively from variation in exposure among them. The estimates for dropout and grade promotion are essentially unchanged, 0.75 and -0.75 per percentage point of overlap, indicating that the comparison with unaffected schools contributes little to the headline effects. The enrollment estimate falls by about half, consistent with enrollment absorbing scale and demographic components common to affected departments rather than reflecting the overlap channel alone.

Appendix Table D.1: Identification from Within-Treated Variation

	Dropout rate (1)	Grade promotion (2)	Enrollment (logs) (3)
$\Delta O_s \times$ Post, baseline specification	0.740***	-0.691***	-1.984***
	(0.096)	(0.143)	(0.592)
$\Delta O_s \times$ Post, adding treated $\times$ year FE	0.751***	-0.747***	-1.016*
	(0.100)	(0.147)	(0.571)
Observations	240,448	240,448	240,448
Baseline controls $\times$ year FE	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

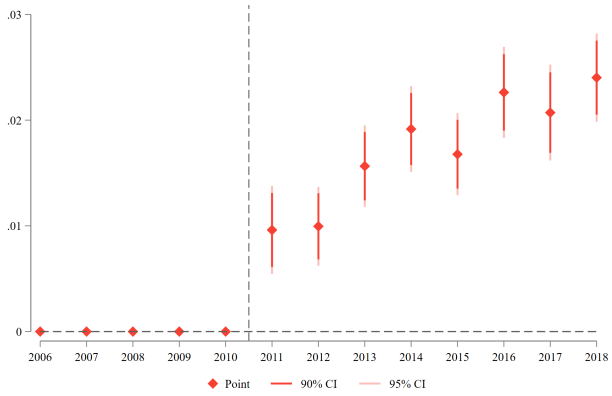
Notes: The table probes whether shocks common to affected departments contribute to the dose-response estimates. The first row reports the baseline static specification. The second row adds a full set of treated-by-year fixed effects, which absorb any post-reform shock common to schools in Nariño and Valle del Cauca and identify the coefficient exclusively from variation in exposure among affected schools. All specifications include school and year fixed effects and interactions between year fixed effects and baseline municipality characteristics, on the balanced panel 2006–2018. Standard errors clustered at the school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Own elaboration based on data from DANE.

D.4. Aggregate Effects: Synthetic Difference-in-Differences

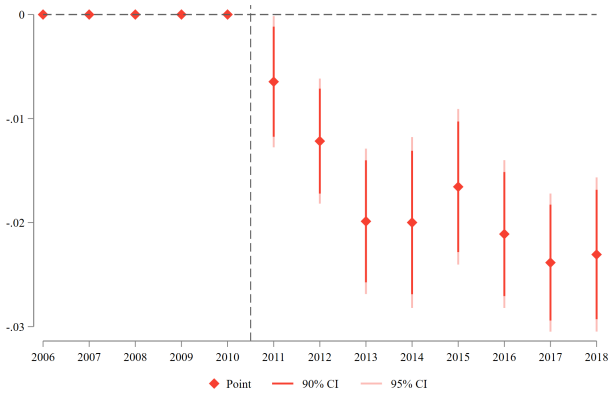
We complement the dose-response estimates with aggregate estimates of the reform's total effect using the synthetic difference-in-differences estimator described in Section 5 (Arkhangelsky et al., 2021), applied to the same balanced panel of rural public schools as our main specification. Whereas the dose-response design identifies the effect of the overlap channel from variation in exposure across schools, the SDID estimates capture the intent-to-treat effect of the reform on treated departments relative to a data-driven synthetic control. Appendix Figure D.5 presents the estimated dynamic paths for the three outcomes. We observe similar effects as with the dose-response strategy. Because treatment is assigned at the department level and only two departments are treated, formal inference at the level of treatment assignment is necessarily uninformative in this design, and school-level intervals understate uncertainty by treating schools as independent draws. We therefore read these aggregate estimates as descriptive, serving to gauge the magnitude of the reform's total effect. The paper's inferential claims rest on the dose-response design, where identification comes from variation in exposure across schools within the affected departments and standard errors can be clustered across roughly one hundred municipalities.

Appendix Figure D.5: Aggregate Effects of the Calendar Reform: Synthetic Difference-in-Differences

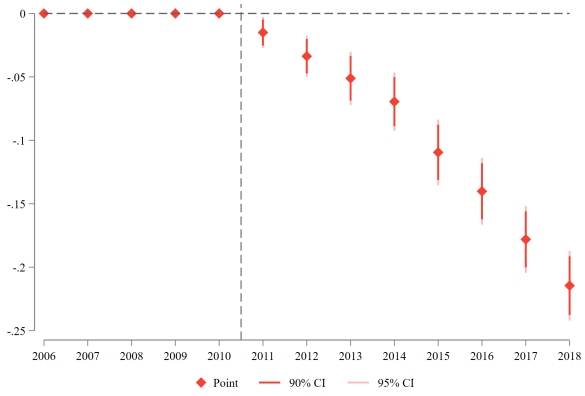
(a) Dropout rate



(b) Grade promotion



(c) Enrollment



Notes: Each panel plots the estimated dynamic effects of the reform in each post-reform year, defined as the difference between treated schools and the synthetic control net of the time-weighted pre-reform mean difference, estimated on the balanced all-country panel of rural public schools used in the main specification. The synthetic control is constructed following the methodology described in Section 5. Vertical bars indicate 90%, 95%, and 99% confidence intervals based on bootstrapped standard errors clustered at the school level with 100 repetitions. Source: Own elaboration based on data from DANE.