

Earthquakes, Reconstruction, and Human Capital: Evidence from the Colombian Coffee Belt Earthquake

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Abstract

This study provides evidence that earthquakes reduce student learning, and tracks how long the loss lasts and which schools recover. Using the universe of secondary school exit examinations over almost two decades together with the annual school census, I estimate a difference-in-differences design that exploits the earthquake that struck the Colombian coffee region in January 1999 and damaged more than a hundred thousand buildings, including schools. Test scores fall during the years of reconstruction, and the loss is concentrated in the epicentral department of Quindío, where it reaches 0.16 standard deviations and fades over nearly a decade. It scales with distance to the epicenter and with recorded destruction, and it is larger and longer lasting in urban schools. The number of test takers falls as well, but their characteristics, school progression, and bounds that account for selection indicate that the losses reflect learning and not changes in exam participation. Comparing public and private schools exposed to the same earthquake, I find that private schools in Quindío lose more of their physical plant and that their test score losses persist at least fifteen years after the earthquake, which is consistent with recovery depending on access to reconstruction funds.

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1 Introduction

Earthquakes are among the most destructive natural disasters. Unlike hurricanes or floods, they arrive without warning and without a season, and in developing economies much of the building stock, including school buildings, is not built to resist them (Shidiqi et al., 2023). The aggregate consequences of natural disasters have been documented in many settings (Aksoy et al., 2025), but evidence on how earthquakes affect what students learn, and for how long, remains limited, since most of it measures years of schooling or a few years after the event.¹ Separating a loss in learning from a loss in schooling requires observing learning directly and for long enough to see whether the gap closes, because a disaster can reduce learning without changing attainment if students stay enrolled but are taught less (Andrabi et al., 2023).

In this paper, I analyze how a major earthquake affected the learning of secondary school students. I study the earthquake that struck the Colombian coffee region in 1999, the most destructive in the country's modern history. It killed 1,185 people, damaged more than a hundred thousand buildings, and cost the equivalent of 2.2 percent of national output. Four days later the government declared a state of emergency in a named set of municipalities and created a reconstruction fund, which operated until 2002. The setting therefore combines a severe shock with a fast, well-documented and geographically bounded reconstruction effort.

I exploit the fact that the emergency decree named specific municipalities to implement a difference-in-differences design. I combine the universe of secondary school exit examinations between 1996 and 2014, roughly ten million student records, with the annual census of Colombian schools, which reports enrollment, promotion, repetition, dropout and the physical plant of every school in the country. I also geocode schools, which allows me to measure exposure by distance to the epicenter and by recorded destruction rather than only by an administrative boundary. Because the department of Quindío was far more severely affected than the municipalities declared elsewhere, I estimate the effect separately for the two groups. Identification requires that affected and unaffected areas would have evolved in parallel in the absence of the earthquake, and I provide evidence in support of that assumption.

The loss is large but not permanent. Test scores in the disaster zone as a whole fall by up to 0.09 standard deviations during the years of reconstruction and are statistically indistinguishable from zero afterward. The loss is concentrated in Quindío, where scores fall by 0.16 standard deviations, close to five percentile points in the national distribution, and the loss fades over nearly a decade. The rest of the disaster zone

¹For the effects of natural disasters on economic growth, see, e.g., Strobl (2011). For labor markets, see Belasen and Polachek (2008), McIntosh (2008), and Groen et al. (2020). For migration, see Boustan et al. (2012), Baez et al. (2017), and Deryugina et al. (2018). Dell et al. (2014) review the relationship between weather and aggregate outcomes.

shows small and statistically insignificant effects. The loss is larger and longer lasting in urban than in rural schools. It also scales with exposure, since it falls monotonically with distance from the epicenter and is larger in municipalities where more schools were demolished. The estimates are robust to the collapse in coffee prices that struck the same region and to letting the data choose the comparison group through a synthetic difference-in-differences estimator.

A natural concern is that these losses reflect who sits the examination rather than what students learned. The number of test takers falls after the earthquake, by 5.7 percent in Quindío and 9.2 percent in the rest of the disaster zone relative to the rest of the country, so the sample of students is not constant. I address this in three steps. First, the observable characteristics of test takers, including gender, age and parental education, do not change consistently. Second, dropout, repetition and enrollment in the school census, and the share of students who sit the examination outside their own municipality, show no pattern that would move the estimate through the sample of test takers. Third, a decomposition shows that the effect in Quindío comes almost entirely from schools observed both before and after the earthquake, and Lee bounds that allow for selection on unobservables remain negative, at $[-0.270, -0.059]$ against a point estimate of -0.153 . The evidence therefore points to learning losses and not to changes in the composition of test takers.

I then turn to the role of reconstruction. Damage and reconstruction spending are strongly correlated across municipalities, so a comparison across places cannot separate what the earthquake did from what the reconstruction undid. I instead exploit the difference between public and private schools exposed to the same earthquake, which holds the shock fixed and varies the likely access to reconstruction funds. Using the physical plant recorded in the school census, I find that private schools in Quindío lose more equipment and more sanitary units than public ones, and that their losses are more persistent. Their test scores follow the same pattern. The private-school path drops strongly and remains depressed at least fifteen years after the earthquake, while the public-school path falls and recovers. This evidence is suggestive, because I cannot observe how the funds were distributed, but it is consistent with the recovery in learning reflecting reconstruction that reached public schools more than private ones.

The first is the literature on the effects of natural disasters on human capital.² This literature finds that disasters can damage human capital by large and lasting amounts (Baez et al., 2010), and most of the evidence measures schooling attainment (Maccini and Yang, 2009; Caruso, 2017). What remains unclear is how long the effects last and through which mechanisms (Baez et al., 2010). Evidence on learning, which is more recent, does not agree on persistence. Sacerdote (2012) finds that students displaced by Hurricanes Katrina and Rita saw declines in test scores in the first year, followed

²Baez et al. (2010) and Paudel (2025) provide reviews of this literature.

by gains among the evacuees from New Orleans who moved to better schools, and [Muñoz-Morales \(2025\)](#) shows that unusually heavy rain in Colombia increased school dropout, with the damage concentrated among rural students. In contrast, [Oppen et al. \(2023\)](#) find that disasters reduce learning across US counties and that the losses persist.

The second strand is the literature on the effects of earthquakes. Most of it measures long-run schooling attainment among cohorts exposed in utero or at school age ([Caruso and Miller, 2015](#); [Tian et al., 2022](#); [Wang et al., 2017](#); [Paudel and Ryu, 2018](#); [Shidqi et al., 2023](#)). Studies of learning, in contrast, observe only a few years after the event. [Andrabi et al. \(2023\)](#) find lower test scores four years after the 2005 earthquake in Pakistan, [Lu et al. \(2023\)](#) find lower college entrance examination scores after the 2008 Wenchuan earthquake, [Di Marcoberardino and Cucculelli \(2026\)](#) find that test scores of primary school students fell for about two years after the 2016 Central Italy earthquake, and [Pacheco-Miranda et al. \(2026\)](#) find higher exit examination scores two years after the 2016 earthquake in Ecuador. Studies of other schooling outcomes follow the event for longer. [Jiménez-Martínez and Jiménez-Martínez \(2025\)](#) find that school attendance fell after the 2010 earthquake in Chile and was no longer affected seven years later, and [Bustelo et al. \(2012\)](#) find that child nutrition and school enrollment fell after the 1999 Colombian earthquake, with the enrollment of older children still lower six years afterward. These studies say little about who recovers. Where they do, they look at household characteristics. [Paudel and Ryu \(2018\)](#) find that high-caste households in Nepal recover while low-caste households do not, and [Andrabi et al. \(2023\)](#) find that children of mothers with primary education are protected from the learning loss.

The third strand is the literature on the role of reconstruction. [Gignoux and Menéndez \(2016\)](#) link the long-run recovery of rural households in Indonesia to rebuilt assets and infrastructure partly funded by aid, and [Liu and Xu \(2021\)](#) suggest that the reconstruction of schools and lower school fees explain gains in girls' education after the 2008 Wenchuan earthquake. [Andrabi et al. \(2023\)](#) show that substantial compensation to households did not prevent learning losses. The closest evidence comes from school-level spending. [Belmonte et al. \(2020\)](#) show that infrastructure funds given to undamaged schools after the 2012 earthquake in Northern Italy raised test scores, and [Pacheco-Miranda et al. \(2026\)](#) attribute about half of the increase in exit examination scores after the Ecuador earthquake to spending and inputs.

This paper contributes to these literatures in three ways. First, it follows a single severe earthquake with annual exit examination scores for fifteen years, which shows how long the loss lasts. The loss fades gradually, with point estimates that return to zero after about nine years in the epicentral department, whereas existing studies of learning cover at most a few years after the event. The loss also reflects learning and not a change in who sits the examination. Second, it shows that recovery differs across schools. The loss is larger and longer lasting in urban schools, unlike the losses from

heavy rain, and it remains until the end of the data in private schools. Third, it uses the difference between public and private schools exposed to the same earthquake as a proxy for access to reconstruction funds. These papers study the effect of resources that schools received, whereas I observe that private schools in Quindío lose more of their equipment and sanitary units and that their test scores remain depressed. Since I cannot observe how the funds were allocated, this evidence is suggestive, but it indicates that whether reconstruction reaches a school may determine whether the loss in learning closes.

The rest of the paper is organized as follows. Section 2 describes the Colombian setting and the earthquake. Section 3 describes the data and the empirical strategy. Section 4 presents the main results. Section 5 asks whether the decline reflects a change in who sits the examination or a change in what students learned. Section 6 turns to the reconstruction, and Section 7 concludes.

2 Background

2.1 The Earthquake

On January 25th, 1999 an earthquake of magnitude 6.2 struck the Colombian coffee belt. The epicenter lay in the municipality of Córdoba, in the department of Quindío. The rupture was about 17 kilometers deep, and the shaking lasted under half a minute. The ground motion and its peaking accelerations, especially recorded in Armenia, Quindío's capital city, met a building stock with little capacity to resist it. This resulted in the most destructive earthquake in the country's modern history.

The official accounting records 1,185 deaths, 8,523 injured and more than 550,000 people affected across an area of some 1,360 square kilometers ([Departamento Nacional de Planeación, 2001](#)). More than a hundred thousand buildings sustained damage. The economic assessment estimated direct and indirect costs at 1,589 million USD, equivalent to 2.2 percent of Colombian output in 1998, of which 68 percent fell on housing and private buildings, 18 percent on physical and social infrastructure (including public schools) and 14 percent on the productive sector ([Comisión Económica para América Latina y el Caribe, 1999](#)).

Figure 1 plots the epicenter, the area around it, the affected municipalities and the location of nearby schools. It also plots two radii, at 25 and 50 kilometers from the epicenter. The department of Quindío fully lies inside the innermost distance band of 25 kilometers, concentrating the biggest damages induced by the earthquake. A substantial share of its building stock, including education facilities, was lost. Appendix Table A1 describes the damages at the municipality level, sorted by distance to the epicenter. The damage was concentrated among municipalities in Quindío, where

128 of the 202 educational establishments recorded as damaged were located, and 49 of the 55 condemned for demolition. Measured by pupils rather than by buildings the concentration is sharper still, since 18,896 of the 19,968 enrolled in establishments marked for demolition sat inside Quindío. Severity varied with proximity even within it, from Córdoba and Montenegro, where every damaged school was demolished, to Armenia, where 31 of 85 were. The rest of the damages was recorded in municipalities in Risaralda, Tolima, and Valle del Cauca, where the damages were of smaller magnitude.

2.2 The Declared Disaster Zone and FOREC

Four days later the national government declared a state of economic, social and ecological emergency in the affected areas through Decree 195 of 1999 ([Presidencia de la República de Colombia, 1999b](#)). The decree declared the Disaster Zone, which comprised 28 municipalities, twelve in Quindío and 16 in four other departments.³ Figure 1 plots the municipalities in the Disaster Zone, which delimit administratively the areas affected by the earthquake. Every school in the Disaster Zone lies within seventy-seven kilometers of the epicenter, none lies beyond a hundred, and ninety-five percent of the schools within twenty-five kilometers are inside it.

This list also determined who received reconstruction money. Decree 195 dictated the transfer of funds from the central government destined to the reconstruction of the affected areas. Column (7) of Appendix Table A1 describes the magnitude of the funds, in Colombian pesos, at the municipality level. That money was administered by the Fondo para la Reconstrucción y Desarrollo Social del Eje Cafetero, known as FOREC. Formally, it was created five days after the earthquake by Decreto 197 de 1999 as an entity of the national order with legal personality and budgetary autonomy ([Presidencia de la República de Colombia, 1999c](#)).⁴

FOREC divided the affected region into zones by level of damage and delegated each to a non-governmental organization, which drew up and executed a zonal action plan together with the local community.⁵ Thirty-one such zonal managements were

³The decree included municipalities spread across the five affected departments, eleven of Quindío's twelve, four municipalities of Risaralda, nine of Valle del Cauca, two of Tolima and one of Caldas. The twelfth Quindío municipality, Génova, was added the following week by Decreto 223 de 1999 ([Presidencia de la República de Colombia, 1999d](#)), which amended the original decree for that purpose, bringing the operative list to twenty-eight municipalities, including the whole of Quindío.

⁴The emergency legislation came in quick succession. Decreto 182 de 1999 declared a national disaster on 26 January ([Presidencia de la República de Colombia, 1999a](#)), Decreto 195 de 1999 declared the state of emergency on 29 January and named the affected municipalities ([Presidencia de la República de Colombia, 1999b](#)), Decreto 197 de 1999 created FOREC on 30 January ([Presidencia de la República de Colombia, 1999c](#)), and Decreto 223 de 1999 added Génova on 5 February ([Presidencia de la República de Colombia, 1999d](#)). [Departamento Nacional de Planeación \(2001\)](#) lists decrees 195 to 198, 209, 228, 258, 350 and 351 of 1999 as the operative set. Congress later passed Leyes 608 and 633 of 2000, which created additional housing subsidies for tenants and tax exemptions intended to reactivate investment in the region ([Congreso de la República de Colombia, 2000](#)).

⁵Financing came 61 percent from the national budget, 38 percent from multilateral credit, and about

established, with the Federación Nacional de Cafeteros taking responsibility for the entire rural sector. The reconstruction was completed in about two and a half years, and FOREC closed on 25 January 2002, exactly three years after the earthquake.

2.3 Secondary Schooling and the High School Exit Exam

Colombian secondary education ends after grade eleven, and essentially every student who completes it takes the secondary school exit exam, *Saber 11*, administered by the Instituto Colombiano para la Evaluación de la Educación (ICFES). The examination is the gateway to tertiary education, since universities admit largely on its basis, and it is a requirement for the award of the secondary school diploma. Because it is close to a census of students, rather than a sample, it can be used to follow cohort after cohort through the years, and it covers students across the whole Colombian territory.⁶

3 Data and Empirical Strategy

3.1 Data

I combine three main data sources to estimate the effect of the earthquake on educational outcomes. First, I employ the secondary school exit exam (i.e., *Saber 11*), from 1996 to 2014. The data contain the scores of about ten million students together with the code of the school in which they were enrolled, its municipality, whether it is public or private, and whether it is located in an urban or a rural area. Test takers also report their parents' education, basic sociodemographic information, and the municipality in which the examination was actually taken, which need not be the municipality of the school. Given the coverage of these background variables, I use them only through 2002, which spans the reconstruction period, whereas the test score data are available through 2014.⁷ Test takers are students seeking a secondary school diploma, not a census of everyone old enough to obtain one, implying that nearly all of the graduating cohort takes the exam.

The exam evaluates test takers in several areas, with an overall test score across these multiple subjects. This result corresponds to the main outcome, which is computed as a percentile rank in the national distribution of the same sitting. Being a rank, it is invariant to any monotonic change in the difficulty of the test, which is a desirable

one percent from international donations ([Departamento Nacional de Planeación, 2001](#)).

⁶Colombian schools follow one of two academic calendars, so *Saber 11* has two editions per year. Calendar A schools run from February to November and their students sit the examination in the second semester of the year, while calendar B schools run from September to June and sit it in the first.

⁷The questionnaire is not asked identically in every year, and coverage of the background module falls sharply after 2002 and is gone by 2005. Appendix Table [A2](#) reports how coverage of each question varies over the period.

property given that the subject scales can move across editions. One standard deviation corresponds to approximately 29 percentile points. Appendix Table A3 describes the data, comparing Quindío, the rest of the disaster zone and the rest of the country in the three years before the earthquake, along with every variable used and its coverage.

Second, I employ the nationwide census of schools, officially named the C-600 form, from 1997 to 2002, covering years prior to the earthquake and to the end of the reconstruction.⁸ These data are gathered by the Colombian National Administrative Department of Statistics (DANE), and every year school directors complete a form collecting information about students, teachers and school facilities. The data record the number of students approved, failed and dropped out in a given academic year, from which I construct school-level rates of promotion, repetition and dropout as ratios to total enrollment at the beginning of the year.⁹ The census also reports the number of teachers and a description of school facilities, which allows school inputs to be tracked alongside student flows.

Third, I geocode schools in order to measure exposure by distance rather than only by administrative boundary. Coordinates are assembled from three registers of Colombian schools that locate 12,236 of the 15,022 schools in the estimation sample, or 81.5 percent. Distances are great-circle distances to the epicenter in the municipality of Córdoba, Quindío.¹⁰ Coverage is close to even across the areas the design compares, at 80 percent in Quindío, 84 percent in the rest of the disaster zone and 81 percent in the rest of the country, so the geocoded subsample is not a differentially selected one.

3.2 Empirical Strategy

The list of twenty-eight municipalities officially declared in the disaster zone tracks exposure to the earthquake. However, damage was far from uniform within that boundary. Quindío, the epicentral department, was severely affected in its entirety, whereas municipalities in the rest of the disaster zone lie further from the epicenter and sustained less damage. I therefore estimate two difference-in-differences (DiD) specifications following these patterns. The first pools all municipalities in the disaster

⁸The census file extends back to 1992, but the data before 1997 are largely missing, and coverage stabilizes in 1997. The 2003 census is unavailable and the questionnaire was redesigned in 2004, so the file covering the earlier years ends in 2002 and a separate one begins in 2004. Data from 2004 onward are not directly comparable.

⁹School transfers are not collected directly before 2004. In the data used here the three reported flows account for total enrollment exactly. Starting 2004, transfers are collected as a separate category.

¹⁰A small number of schools carry coordinates inconsistent with their administrative municipality and are dropped: specifically, those lying more than 25 kilometers from the median position of the other schools in their own municipality. This removes 242 schools, about two percent of those geocoded.

zone and estimates

$$y_{iset} = \sum_{t \neq 1998} \beta_t (D_m \times \mathbf{1}[t]) + \gamma X_i + \delta_s + \lambda_e + \varepsilon_{iset}, \quad (1)$$

where y_{iset} is the outcome of student i in school s sitting edition e in year t , D_m indicates that the school's municipality m was declared a disaster area, X_i contains sex, age and age squared, δ_s are school fixed effects and λ_e are edition fixed effects.¹¹ The year 1998, the last before the earthquake, is the omitted category, so each β_t measures the difference between the disaster zone and the rest of the country in year t relative to that year. Standard errors are clustered at the municipality level, allowing correlations across schools within affected areas.¹²

The second specification includes the same components, but separates the municipalities of the disaster zone in two groups within the same regression,

$$y_{iset} = \sum_{t \neq 1998} \beta_t^Q (Q_m \times \mathbf{1}[t]) + \sum_{t \neq 1998} \beta_t^R (R_m \times \mathbf{1}[t]) + \gamma X_i + \delta_s + \lambda_e + \varepsilon_{iset}, \quad (2)$$

where Q_m indicates a municipality of Quindío and R_m a municipality in the rest of the disaster zone. Both paths are estimated against the same comparison group, the rest of the country, so β_t^Q and β_t^R are comparable in level as well as in shape.¹³ The contrast between them provides a dose-response test, since the more severely affected group should show the larger effect.¹⁴

Identification requires that, absent the earthquake, outcomes in the disaster zone and in the rest of the country would have evolved in parallel. The event-study form of both equations provides a direct check, since the coefficients for 1996 and 1997 measure any pre-existing divergence.

I complement the dynamic estimates with static form specifications. Rather than a coefficient for every year, the post-earthquake period is collapsed into two windows,

$$y_{iset} = \sum_p \beta_p^Q (Q_m \times P_{pt}) + \sum_p \beta_p^R (R_m \times P_{pt}) + \gamma X_i + \delta_s + \lambda_e + \varepsilon_{iset}, \quad (3)$$

¹¹Depending on the outcome, the equation is estimated at the student, school or municipality level, with the corresponding fixed effects.

¹²Standard errors are conservative throughout. I cluster at the municipality level even in specifications where the treatment varies at the school level, since municipality is the level at which the corresponding overall treatment is assigned.

¹³I assess the validity of this comparison group with a synthetic DiD strategy (Arkhangelsky et al., 2021) as a robustness check in Section 4.3.

¹⁴As a robustness check, I also measure exposure by the school's distance from the epicenter and by level of damage. These specifications closely mirror equation (2) rather than provide an independent test, because every geocoded school in Quindío lies within twenty-five kilometers of the epicenter and was among the most affected. Comparing the innermost band against the rest, and comparing by level of damage, is therefore close to comparing Quindío against the rest, but with a dose-response gradient built in.

where P_{pt} indicates that year t falls in window p : 1999–2002, while reconstruction was taking place, and 2002–2014, the long run after it closed.¹⁵ Everything else is as in equation (2), and the years before 1999 remain the omitted category, so each β_p is the average deviation from the pre-earthquake path over that window. The event study in equation (2) shows whether the pre-trends are flat and when any effect appears, while equation (3) provides estimates per window that contrast years during and after reconstruction.

4 Results

4.1 Main Estimates

Figure 2 reports the effect of the earthquake on test scores in the secondary school exit exam. Panel 2a estimates equation (1), pooling every municipality in the disaster zone. No effect is detected prior to 1999, consistent with the parallel trends assumption. For 1999, the point estimate is slightly negative but far from significantly different from zero. After 1999, during the reconstruction period, scores fall by 0.075 standard deviations in 2000 and 0.091 in 2001, both statistically significant, and then return to zero progressively. The point estimate turns positive by 2007, and no year from 2002 onward is statistically distinguishable from zero. These patterns indicate learning losses during the years of reconstruction that vanish after the reconstruction was over.

However, the patterns are not homogeneous everywhere. Panel 2b separates Quindío from the rest of the disaster zone and estimates equation (2). In Quindío, the most affected department, the test score drop reaches 0.16 standard deviations, close to five percentile points in the national distribution. In the rest of the disaster zone, the estimates are small and indistinguishable from zero. In Quindío, the loss is largest during the years of reconstruction and fades gradually afterward. It is statistically significant for about five years, and the point estimates return to zero only after almost a decade.

These effects are larger and more persistent in urban schools. Appendix Table A4 presents the overall estimates of equation (3) and splits the results by urban and rural schools.¹⁶ During reconstruction the loss is 0.18 standard deviations in urban schools and 0.11 in rural schools, and after 2002 it persists in urban schools but not in rural ones.¹⁷ The learning losses from the earthquake are therefore largest and longest lasting

¹⁵The window labelled 1999–2002 runs through 2001, not through 2002. FOREC closed on 25 January of 2002, so 2002 itself belongs to the long run instead. These same two windows are used throughout the paper.

¹⁶The table also reports results for girls and boys, and Appendix Figure A1 presents event study estimates for all of these subgroups.

¹⁷The difference between urban and rural schools in Quindío is statistically significant in both periods

among urban students in the most affected department, which is consistent with the damage being potentially concentrated in urban centers.

4.2 Dosage Effects

The effects plotted in Panel 2b capture a dose-response test for the earthquake's destruction since the learning losses are concentrated in schools where damage was concentrated. Quindío, the department at the epicenter, was severely damaged in its entirety, so it is natural that the results concentrate there, but Quindío is also an administrative region that proxies for proximity to the epicenter and, through it, for the severity of the destruction.

To evaluate whether the effect varies by dosage, I compute the test score effect using alternative measures of exposure and present the results in Table 1. I focus only on 1999 to 2002, the reconstruction period when the main negative effects are present. Columns (1) and (2) restrict the comparison to Quindío against the rest of the country. Column (1) has no controls or fixed effects, whereas column (2) adds school and test-edition fixed effects and the same controls as the main specification. Both give a similar estimate, around -0.153 . Column (3) returns to the pooled disaster zone, Quindío and the rest of the disaster zone together, with the same controlled specification, giving -0.060 . Columns (4) to (7) keep that controlled specification and replace the administrative boundary with alternative measures of exposure. Column (4) uses distance to the epicenter. The effect falls monotonically with distance, from -0.139 within 25 kilometers to -0.037 between 25 and 50, -0.018 between 50 and 100, and -0.013 beyond that, though only the closest band is significant at conventional levels.¹⁸

I then turn to direct measures of destruction, reported by municipality in Appendix Table A1, so exposure can be measured directly rather than inferred from administrative boundaries.¹⁹ Columns (5) to (7) use the share of blocks, schools damaged and schools demolished at the municipality level. Evaluated at the destruction actually observed, all three measures deliver similar magnitudes. The coefficient on the share of schools demolished is -0.925 , which at La Tebaida's 0.231 implies -0.214 , and the coefficient on the share of schools damaged is -0.373 , which at Armenia's 0.556 implies -0.207 .²⁰

($p = 0.004$ during reconstruction and $p = 0.019$ afterwards).

¹⁸Appendix Figure A2a, reports the same specification as an event study, showing no evidence of a pre-trend.

¹⁹The data come from the census of the earthquake zone conducted in February 1999 that enumerated damaged establishments only. The measures used here divide the damaged and the demolished counts by the number of schools the municipality reported to the C-600 school census in 1998, which gives a share of all schools. Appendix Table A1 reports the underlying counts.

²⁰Splitting the municipalities of the disaster zone at the median of measured destruction gives -0.117 above and -0.039 below, and Appendix Figure A2 shows that the two groups are indistinguishable before 1999 and separate immediately afterwards. Appendix Table A5 reports that split in full, together

The effects are more concentrated in municipalities where schools were demolished, capturing directly the more severe exposure to the earthquake. Effects therefore scale with destruction and not merely with the administrative boundary, since places where the damage was worse show larger test score losses.

4.3 Robustness

Two threats could plausibly account for the main results rather than the earthquake. The first is the collapse in coffee prices, which struck the same region over subsequent years. The second is the choice of comparison group, since comparing the disaster zone against the rest of the country might be arbitrary and is not something the data selected.²¹

The Coffee Price Collapse

The disaster zone lies inside the Colombian coffee belt, and international coffee prices collapsed between 1999 and 2002. The collapse was large enough to matter for households in the region ([Miller and Urdinola, 2010](#)). If that shock affected schooling, it would be confounded with the earthquake.

The coffee shock is common to the region, so it can be differenced out by choosing a comparison group that experienced it. Table 2 does this in three ways. Column (1) is the baseline. Column (2) adds fixed effects for year interacted with an indicator for the five coffee departments. Column (3) restricts the sample to those departments. Column (4) restricts it instead to schools with coffee growing in their own neighborhood, so that treated and comparison schools alike lived through the price collapse.²² None of these adjustments moves the estimates by much, and the dose-response pattern is preserved, which indicates that the drop in coffee prices does not confound the main results.

The Choice of Comparison Group

The main specifications compare affected locations to the rest of the country. This control group is very heterogeneous, since it includes all municipalities outside the disaster zone. I evaluate the robustness of the results using the synthetic DiD estimator of [Arkhangelsky et al. \(2021\)](#), which lets the data select and weight comparison units instead.

with the same exercise using schools condemned for demolition rather than blocks, which gives -0.117 against -0.042 . Both pass their pre-earthquake test.

²¹A third threat is potential violations to the stable unit treatment value assumption (SUTVA), because students displaced from the disaster zone could contaminate the comparison group and bias the estimates towards zero. For presentation purposes, I address this in Section 5.

²²Appendix Figure A3 reports the same four specifications as event studies.

The estimator requires a strictly balanced panel, which constrains the window. A school qualifies only if it has test takers sitting the examination in every year, which reduces the sample considerably if accounting from 1996 to 2014. Over 1996 to 2003 the proportion of schools fully observed is 57 percent, retaining 5,334 schools of which 207 are in the disaster zone and 67 in Quindío, and covering 82 percent of the test takers who sat the examination in the disaster zone. That window also spans exactly the pre-earthquake years and the years in which the deficit appears, so it is the one used here.²³

Appendix Figure A4 reports the results. The estimator reproduces the main result closely. For the disaster zone as a whole the effect is indistinguishable from zero. For Quindío the trough is -0.169 in 2000 and -0.158 in 2001, against a trough of -0.165 in Figure 2. The placebo periods are essentially zero in both cases, at four decimal places, which is what the method is designed to deliver and what the raw event studies also show.

Letting the data choose and weight the comparison schools therefore does not change the answer. The null in 1999, the trough in 2000 and 2001, and the size of the Quindío effect relative to the disaster zone as a whole all survive.

5 Learning or Selection?

The test score losses experienced after the earthquake could be explained by changes in the sample of test takers rather than by actual learning losses. I test this by comparing the number of test takers and the number of schools in Quindío, in the rest of the disaster zone, and in the rest of the country. Figure 3 presents the results. Panel 3a shows the raw number of test takers relative to 1998, Panel 3b reports the event study of log test takers per school, and Panel 3c reports the corresponding event study of the number of schools per municipality.²⁴

Test takers rise at a similar pace in all three groups before the earthquake. In 1999 and 2000, the number of test takers in the disaster zone as a whole falls slightly relative to the rest of the country, and after 2000 it falls everywhere, but by more in the disaster zone. Between 1998 and 2002, the number of test takers in Quindío fell by 5.7 percent relative to the rest of the country, and in the rest of the disaster zone by 9.2 percent. This decline reflects fewer test takers in each school and not fewer schools, since the number of schools does not change. The smaller sample of test takers therefore does

²³Standard errors are estimated using 50 bootstrap replications clustered at the municipality level.

²⁴Both event studies regress the outcome on indicators for Quindío and for the rest of the disaster zone interacted with each year, with 1998 omitted and the rest of the country as the comparison group. Panel 3b uses school-by-year data, the log number of test takers, and school and year fixed effects. Panel 3c uses municipality-by-year data, the log of the number of schools with at least one test taker, and municipality and year fixed effects. Standard errors are clustered at the municipality level.

not allow me to rule out sample selection as an explanation for the test score losses.

Whether this selection in the sample biases the estimated learning losses depends on its type and nature. The *type* of selection is determined by who leaves the sample, the best students (negative selection) or the worst (positive selection). The *nature* of selection is how students leave the sample, by dropping out of school or being held back, or by transferring to another school, the latter a violation of the SUTVA assumption. To incorporate both, the DiD estimator can be written as

$$\hat{\beta} = \beta + (b_T - b_C) \quad (4)$$

where β is the true effect of the earthquake on learning, and $b_{g \in \{T,C\}}$ is the shift in the observed average attributable to sample change in the treated and control groups.²⁵

The type of selection determines the direction of this shift whereas the nature of selection determines the magnitude. Regarding type, the difference $b_T - b_C$ is negative when the earthquake induces the best students to leave the treated group (negative selection), in which case the DiD estimator overstates the true loss, and positive when it induces the worst to leave (positive selection), in which case it understates the loss. Regarding nature, dropout and repetition shift only b_T , since the students who leave the sample belong to the treated group, whereas transfers shift both b_T and b_C , since the students who move leave the treated group and enter the comparison group. Transfers therefore produce a larger shift in $b_T - b_C$ than dropout or repetition, but the nature of selection does not affect the sign of the shift.

This section evaluates the terms in equation (4). On the type of selection, I find that the characteristics of the students who sit the exam do not change in a consistent way, implying that selection on observable characteristics is unlikely to shift $b_T - b_C$. On its nature, I find that dropout, grade repetition, and transfers do not rise either. To account for selection on unobservable characteristics, I bound the effect for the fall in test takers, and the estimates remain negative in Quindío under both negative and positive selection. The evidence therefore points to learning losses, and not to changes in who sits the exam, as the driver of the test score effects.

5.1 The Type of Selection

The type of selection refers to who leaves the sample of test takers. If the best students leave, the DiD estimator overstates the learning loss, and if the worst leave, it understates it. Selection on unobservable traits cannot be tested directly, but selection on the characteristics that predict performance can be detected through changes in the composition of test takers. I therefore check whether the characteristics of the students

²⁵Appendix B derives the selection bias formally, allowing for changes in both the type and the nature of selection.

sitting the exam changed after the earthquake. Table 3 reports the results, estimated on a school-by-year panel.²⁶ The first two columns report the static effect on test scores and on the log number of test takers for comparison. The remaining columns report the effect on students' gender and age and on their parents' education.²⁷

No clear compositional change emerges. Of the twelve estimates for the six characteristics in the two groups, only two are statistically different from zero, and both are in the rest of the disaster zone. None of the estimates for Quindío, where the test score effects are concentrated, is statistically different from zero.²⁸

The characteristics of test takers show no consistent change after the earthquake. This suggests that it did not change the type of students who sit the exam along these dimensions, so the component of $b_T - b_C$ that operates through observable characteristics is likely close to zero. The component that operates through unobservable characteristics, however, cannot be tested directly but I address it in Section 5.3.

5.2 The Nature of Selection

The nature of selection refers to how students leave the sample, either by dropping out of school or being held back, or by transferring to another school. Dropout and repetition shift only b_T , since the students leave the treated group. Transfers shift both b_T and b_C , since the students leave the treated group and enter the comparison group, which also violates SUTVA. I examine each in turn.

Dropout. The earthquake could have induced students to drop out and thereby affect the sample of test takers. Column (1) of Table 4 presents DiD estimates using dropout rates from the census of schools, pooling the years after the earthquake into a single post-period, and Appendix Figure A6 reports the dynamic specification. Dropout does not change in Quindío, where the main test score effects are concentrated, and the dynamic estimates show no clear pattern either. It does rise slightly in the rest of the disaster zone, by 0.9 percentage points (significant at 10 percent). These results nonetheless do not indicate that the test score effects in Quindío are driven by a shift in b_T .

Repetition. Students can also leave the sample of test takers without leaving school, if they are held back. Like dropout, repetition removes students from the treated group, so it would shift b_T . Columns (2) and (3) of the same table report promotion and repetition rates. In Quindío, promotion falls by 0.7 percentage points and repetition rises by 0.8

²⁶Appendix Figure A5 presents the corresponding event study estimates.

²⁷I do not include characteristics that the earthquake could have affected directly because they would confound a change in the composition of test takers with a direct effect of the earthquake.

²⁸I also estimated the effect on an indicator for a low-income household. I do not report it in Table 3 because the earthquake could have affected household income directly. Nonetheless, the estimates, which could include such confounding, are small and not statistically different from zero (0.0075 in Quindío and 0.0005 in the rest of the disaster zone).

points, relative to a pre-earthquake repetition rate of 5.3 percent, and neither estimate is statistically different from zero. In the rest of the disaster zone, promotion falls by 1.1 points (significant at 10 percent) and repetition does not change. The dynamic estimates in Appendix Figure A6 show no pre-trends in either group, and the post-earthquake estimates move unevenly from year to year. Grade progression therefore does not suggest either that the earthquake held back enough students to shift b_T in Quindío.

Transfers. If students left schools in the disaster zone for schools in the rest of the country, the research design would violate the stable unit treatment value assumption (SUTVA). I test for this by estimating equation (2) with a dummy for whether the student sat the exit exam outside their own municipality as the outcome. Appendix Figure A7a plots the results. Students in the disaster zone are no more likely to sit the examination outside their own municipality after the earthquake, so displacement across municipal boundaries does not appear to explain the test score effects.

Column (4) of Table 4 and Appendix Figure A7b offer a second check, using enrollment from the school census. If students were displaced by the earthquake, enrollment would fall in the disaster zone and increase elsewhere. In Quindío, the static estimate is not statistically different from zero, and the dynamic estimates show a decline in 1999 followed by increases in 2000 and 2001. In the rest of the disaster zone, enrollment does not change in any year of the window. Enrollment therefore does not fall persistently in the disaster zone, and transfers do not appear to shift b_T or b_C .

5.3 Decomposing and Bounding the Main Effect

The previous results find no evidence of selection on observable characteristics or on school progression. Selection on unobservable traits, however, could still shift $b_T - b_C$ in either direction. Therefore, I provide complementary estimates that bound the effect under the most extreme selection consistent with the fall in test takers. I proceed in two steps. First, I analyze whether the effect comes from schools exiting or entering before and after the earthquake. Consistent with the evidence in Section 5, schools do not explain the main effect. Second, I evaluate what changes in student composition imply directly, by estimating non-parametric bounds on the main effect following Lee (2009). Even accounting for selection on unobservables, the learning losses in Quindío remain negative, with the benchmark estimate of -0.153 lying within bounds of $[-0.270, -0.059]$, which correspond to a loss between 60 percent smaller and 75 percent larger.

Decomposing the Effect

I decompose the main effect by comparing test scores within schools observed before and after the earthquake, and with schools that entered after. Take the simplest

version of the design, a single before-and-after comparison of 1996 to 1998 against 1999 to 2002, displayed in Column (1) of Table 5. Split the schools into continuing schools (C), observed on both sides of the earthquake, entrants (N), which appear only afterwards, and exiters (X), which appear only before. Writing $d \in \{0, 1\}$ for the group being evaluated ($d = 1$ either for Quindío or the rest of the disaster zone, and $d = 0$ for the rest of the country), and writing s_N for the entrants' share of post-earthquake test takers and s_X for the exiters' share of pre-earthquake test takers, the change in each group d decomposes exactly as

$$\underbrace{\bar{y}_{d,\text{post}} - \bar{y}_{d,\text{pre}}}_{\Delta_d} = \underbrace{(\bar{y}_{d,\text{post}}^C - \bar{y}_{d,\text{pre}}^C)}_{\text{within}} + \underbrace{s_N (\bar{y}_d^N - \bar{y}_{d,\text{post}}^C)}_{\text{entry}} + \underbrace{s_X (\bar{y}_{d,\text{pre}}^C - \bar{y}_d^X)}_{\text{exit}}. \quad (5)$$

Since $\beta = \Delta_1 - \Delta_0$, the DiD estimate can be expressed as the sum of the within, entry and exit terms, each computed as a difference between $d = 1$ and $d = 0$.²⁹ Columns (1) to (4) of Table 5 report the decomposition. Column (1) is the raw effect, whereas columns (2) to (4) split it into the within-school change and the contributions of entering and exiting schools.

In Quindío, changes in the sample of schools do not drive the estimate. Of a raw effect of -0.153 standard deviations, continuing schools account for -0.145 and entering schools for -0.008 . In the rest of the disaster zone, the raw effect is close to zero (0.002), but it combines entry of $+0.013$ with a decline of -0.012 among continuing schools. Exit is zero throughout because no school in the disaster zone has test takers only before the earthquake.³⁰

Bounding the Effect

I complement these results by evaluating the sensitivity of the main effects to selection on observable and unobservable traits. Lee (2009) develops bounds for treatment effects under sample selection. If the treatment weakly reduces the probability of being observed for every individual, as the earthquake does here for taking the exam, the effect for those observed regardless of treatment is bounded by trimming the group with more observations, from above and from below, by the differential retention.

Columns (5) to (7) in Table 5 bound the estimate in column (1). Column (5) reports the trimming share, 5.7 percent of the sample in Quindío and 9.2 percent in the rest of

²⁹The decomposition follows the productivity literature, where aggregate change is separated into within-firm improvement and the contribution of entering and exiting firms (Foster et al., 2001; Melitz and Polanec, 2015). Sant'Anna and Xu (2025) give the corresponding treatment for DiD with repeated cross-sections, including a test for whether composition changes at all.

³⁰In the rest of the country, schools with test takers only before the earthquake account for 0.03 percent of pre-earthquake test takers.

the disaster zone, and columns (6) and (7) report the lower and upper bounds.³¹ The bounds on the Quindío effect are $[-0.270, -0.059]$, both negative, although only the lower bound is statistically different from zero. For the rest of the disaster zone they are $[-0.167, 0.158]$, which includes zero.

The bounds do not indicate which end of the range is more likely. However, even under the most extreme selection in either direction, the point estimates of the learning loss in Quindío remain negative, ranging from -0.270 to -0.059 . Only the lower bound is statistically different from zero, but the upper bound is an extreme case that assumes that the top 5.7 percent of students stopped taking the exam because of the earthquake, which is very unlikely. Since there is no evidence of selection in either the observable characteristics or the flows of students, the estimate of -0.153 , which lies well within this range, is the most likely value.

Taken together, these results speak to whether the test score losses reflect changes in the sample of test takers or actual learning losses. The number of test takers falls after the earthquake, but the evidence indicates that the test score losses in Quindío reflect learning losses and not changes in who takes the exam. The characteristics of test takers show no consistent change, dropout, repetition, and transfers do not rise, and bounding the effect for the fall in test takers gives negative point estimates under the most extreme selection in either direction.

6 Reconstruction

The test score losses reported in the previous sections are largest while reconstruction was under way and fade after it. This raises the question of whether reconstruction mediated the effects, which would imply that the funds allowed schools and students to recover. That question is empirically difficult to answer because damage and reconstruction spending are correlated. Appendix Table A1 shows a correlation of 0.57 between the two across the twenty-three municipalities with recorded damage. A comparison across municipalities therefore cannot separate what the earthquake did from what the reconstruction did, since the places that received the money are largely the places that were hit.

Identifying the effect of reconstruction requires variation across schools that experienced the same earthquake but received different amounts of reconstruction. The

³¹The trimming share is the percent drop in the number of test takers between 1998 and the average of 1999 to 2001 in each group, relative to the same change in the rest of the country, computed from the raw totals in Panel 3a. The upper bound replaces the comparison group's post-earthquake mean score with its mean below the $(1 - p)$ quantile, the lower bound with its mean above the p quantile, and the pre-earthquake means are left alone. Scores are standardized within year. Standard errors for the bounds come from the difference-in-differences regression on the trimmed sample, clustered at the municipality level.

public-private margin provides such variation if reconstruction funds were directed more to public than to private school buildings. I do not observe how the funds were allocated, so I assess this premise indirectly. Schools that received more of the money should show smaller losses in infrastructure. Table 6 therefore reports the effects of the earthquake on school infrastructure separately for public and private schools, together with the difference between the two.³²

The infrastructure results are overall consistent with the premise. In Quindío, public schools lose 0.067 on equipment and 0.064 on sanitary units, and private schools lose more, by an additional 0.053 on equipment and 0.090 on sanitary units. The point estimate for built area is also large, with private schools losing an additional 0.143, but it is not statistically different from zero. The difference for classroom area is likewise not statistically different from zero. The event studies add that the private school losses are more persistent, with the estimates for sanitary units and equipment remaining negative through 2000, while those for public schools are smaller. Public and private schools were therefore both affected by the earthquake, but private schools appear to have been hit harder. This is consistent with public schools having received more of the reconstruction money.

If private schools were rebuilt less, their test score losses should also be more persistent. Figure 4 presents this comparison. The private-school path in Quindío drops strongly and remains depressed until 2014, even though the other subgroups fade. The public-school path in Quindío falls and recovers after a few years, following the Quindío path in Panel 2b. Outside Quindío neither sector shows an effect, consistent with a shock too weak to leave much for either kind of school to recover from.

This evidence is suggestive rather than conclusive, because I cannot directly observe how the reconstruction funds were used by public and private schools. Private schools may also differ from public ones in ways unrelated to reconstruction. The public-private contrast nonetheless holds the earthquake fixed and varies only the likely access to reconstruction funds, which makes it informative about the role of reconstruction in the results that follow.

7 Conclusion

A severe earthquake reduced learning among the secondary school students who lived through it. The loss was concentrated where the shock itself was worst, in the department of Quindío, and it scaled with distance to the epicenter and with destruction measured directly rather than only with an administrative boundary. The loss was largest during the years of reconstruction and faded gradually afterward, with point

³²Appendix Figures A8, A9 and A10 show the corresponding event studies for the disaster zone as a whole, and separately for public and private schools in Quindío and in the rest of the disaster zone.

estimates that return to zero after almost a decade in the epicentral department, and it was larger and longer lasting in urban schools. Accounting for the collapse in coffee prices and letting the data choose the comparison group leave the estimates essentially unchanged.

The number of test takers fell after the earthquake, but the evidence points to learning losses and not to a change in who sits the examination. The characteristics of test takers do not change consistently, dropout, repetition and transfers do not rise in a way that would move the estimate, the effect in Quindío comes almost entirely from schools observed both before and after the earthquake, and Lee bounds on the fall in test takers remain negative at both ends.

The comparison between public and private schools provides suggestive evidence on the role of reconstruction. Private schools in Quindío lose more equipment and more sanitary units than public ones and their losses last longer. Their test scores remain depressed fifteen years after the earthquake, whereas those of public schools fall and recover. I cannot observe how reconstruction funds were allocated, so I cannot attribute these differences to unequal access to funds. The pattern is nonetheless consistent with a recovery in learning that tracks how quickly reconstruction reached schools.

Two questions follow. The first is whether school-level records of reconstruction spending, which I have not been able to locate, would confirm that public schools received more of the funds, and how much of the difference in recovery they explain. The second is whether the pairing of a severe shock with a fast, well-funded and geographically targeted reconstruction generalizes beyond this earthquake. The long horizon of this setting shows that a loss in learning can take almost a decade to fade even after a fast reconstruction, and that it can last longer in private schools, which plausibly received fewer reconstruction funds. Whether the same holds in other settings is a question that only comparable natural experiments elsewhere can answer.

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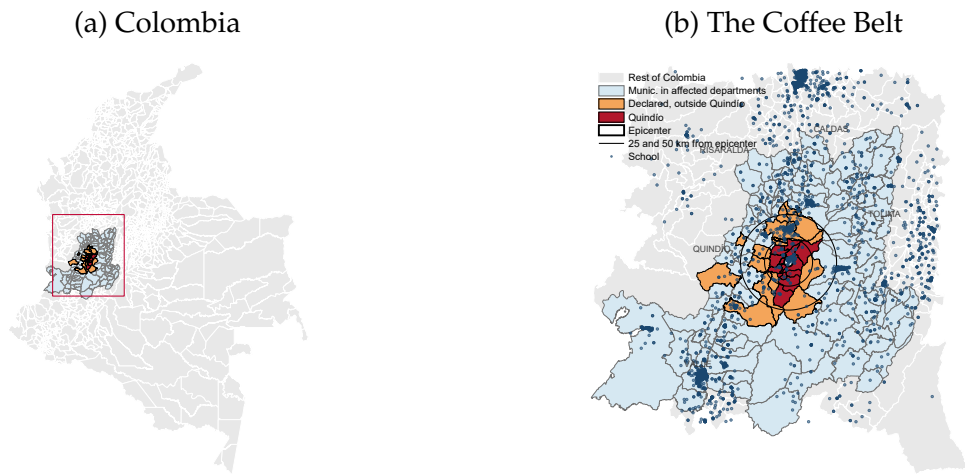
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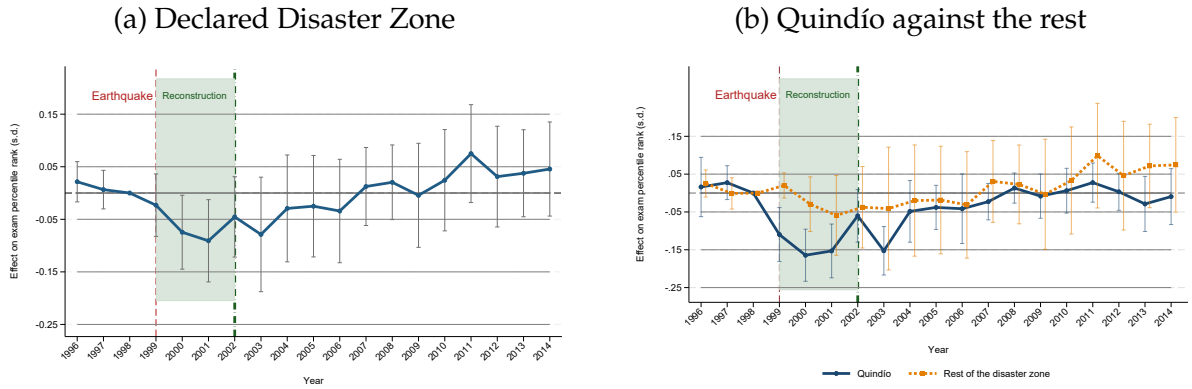
Figures

Figure 1: The Coffee Belt and the Affected Areas



Notes. This figure maps the earthquake region. Panel (a) locates the affected region within Colombia, and the box marks the area shown in panel (b). Panel (b) shows the municipalities of the five affected departments. Municipalities in deep red are the twelve of Quindío, and those in orange are the sixteen declared elsewhere by Decreto 195 de 1999 and its amendment. Municipalities in pale blue are the remaining municipalities of the five departments. Dots are the geocoded schools in the estimation sample. The white marker is the epicenter in Córdoba, Quindío, and the circles are drawn at 25 and 50 kilometers from it. Municipal boundaries come from the 2018 national geostatistical framework.

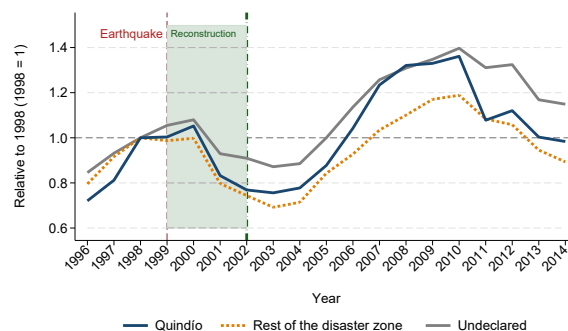
Figure 2: Effect of the Earthquake on Test Scores



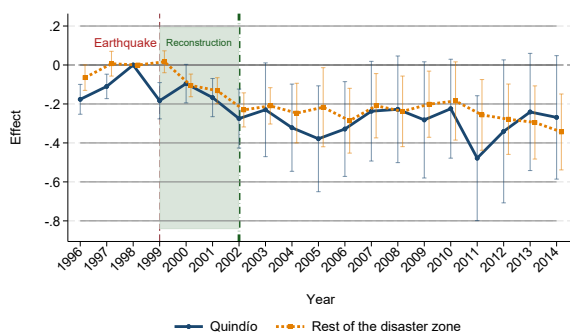
Notes. This figure plots the estimated effect of the earthquake on Saber 11 test scores by year. The sample includes Saber 11 test takers from 1996 to 2014. The outcome variable is the national percentile rank standardized within test edition, where one standard deviation is about 28.6 percentile points. Panel (a) estimates the effect for the municipalities declared disaster areas by Decreto 195 de 1999 and its amendment. Panel (b) estimates, within a single regression, separate effects for the twelve municipalities of Quindío and the sixteen municipalities declared in the other four departments. Both panels are measured against the rest of the country. The specification includes school and test-edition fixed effects and controls for sex, age, age squared and an indicator for missing age. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year. The dashed line marks the earthquake of January 25, 1999. The shaded band marks 1999 to 2002, when the reconstruction program FOREC operated until its closure on January 25, 2002.

Figure 3: Test Takers and Schools in the Secondary School Exit Exam

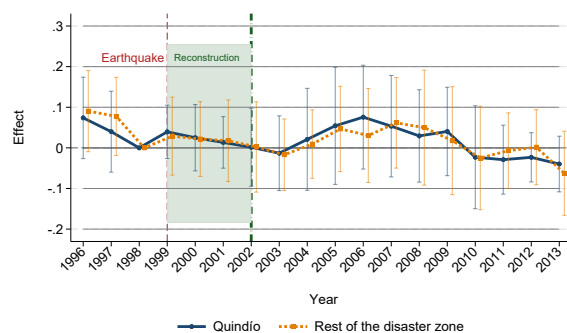
(a) Test Takers Relative to 1998



(b) Log Test Takers per School

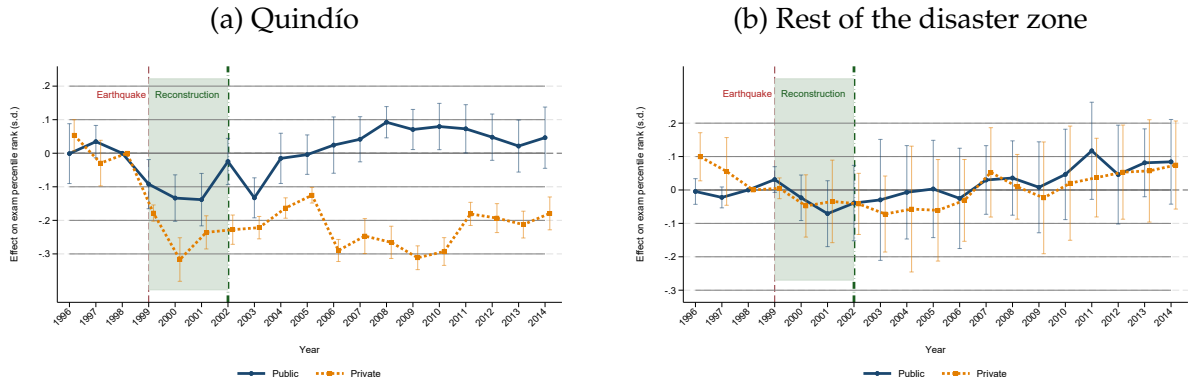


(c) Log Schools per Municipality



Notes. This figure plots the number of test takers and schools in the Saber 11 examination records. Panel (a) plots the number of test takers, summed over the twelve municipalities of Quindío, the sixteen declared municipalities elsewhere and the undeclared municipalities, and divided by the value of each group in 1998. It is a raw total and is not estimated. Panel (b) plots estimated effects on the log number of test takers per school, from a school-by-year panel with school and year fixed effects. Panel (c) plots estimated effects on the log of one plus the number of schools with at least one test taker, from a municipality-by-year panel with municipality and year fixed effects. Panels (b) and (c) separate, within a single regression, Quindío from the declared municipalities elsewhere, with the rest of the country as the comparison group. Standard errors are clustered at the municipality level. Bars are 95 percent confidence intervals and 1998 is the omitted year. In every panel the dashed line marks the earthquake, and the shaded band marks 1999 to 2002, when FOREC operated.

Figure 4: Effect on Test Scores by Public and Private schools



Notes. This figure plots the estimated effect of the earthquake on Saber 11 test scores by year, separately for public and private schools. The outcome variable is the national percentile rank standardized within test edition. Panel (a) shows the twelve municipalities of Quindío and panel (b) shows the sixteen municipalities declared elsewhere, each measured against the undeclared municipalities in the same subsample. The specification includes school and test-edition fixed effects and controls for sex, age, age squared and an indicator for missing age. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year. The dashed line marks the earthquake, and the shaded band marks 1999 to 2002, when FOREC operated.

Tables

Table 1: The Effect under Alternative Measures of Exposure

	(1) Quindío, raw	(2) Quindío	(3) Decree	(4) Distance	(5) Blocks	(6) Schools damaged	(7) Schools demolished
Quindío × 1999–2002	-0.1529*** (0.0517)	-0.1526*** (0.0439)					
Disaster zone × 1999–2002			-0.0598* (0.0343)				
Within 25 km × 1999–2002				-0.1390** (0.0584)			
25 to 50 km × 1999–2002				-0.0366 (0.0239)			
50 to 100 km × 1999–2002				-0.0183 (0.0185)			
100 to 200 km × 1999–2002				-0.0134 (0.0109)			
Share of blocks demolished × 1999–2002					-0.3789** (0.1525)		
Share of schools damaged × 1999–2002						-0.3725*** (0.0333)	
Share of schools demolished × 1999–2002							-0.9248*** (0.1655)
Observations	2,394,115	2,394,090	2,465,458	2,086,019	2,447,008	2,447,008	2,447,008
Municipalities	1,061	1,061	1,077	1,040	1,072	1,072	1,072
School Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Test-Edition Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table presents regression estimates of the effect of the earthquake on Saber 11 test scores under alternative measures of exposure. The outcome variable is the national percentile rank standardized within test edition. The sample includes test takers from 1996 to 2001, and the post-earthquake period labeled 1999–2002 covers the years 1999 to 2001. Each column is a separate regression. Columns (1) and (2) compare Quindío with the rest of the country and exclude the rest of the disaster zone. Column (1) has no controls or fixed effects. Column (2) includes school and test-edition fixed effects and controls for sex, age, age squared and an indicator for missing age. Column (3) pools Quindío and the rest of the disaster zone into a single treatment indicator, with the specification of column (2). Columns (4) to (7) use the specification of column (2) and replace the disaster zone indicator with other measures of exposure. Column (4) uses indicators for distance bands from the epicenter, measured for geocoded schools, with schools beyond 200 kilometers as the omitted category. Column (5) uses the share of a municipality's blocks recorded as demolished. Columns (6) and (7) use the number of educational establishments recorded as damaged and the number condemned for demolition, each divided by the number of schools the municipality reported to the C-600 school census in 1998. Disaster zone municipalities without a damage record are excluded from columns (5) to (7). Standard errors are clustered at the municipality level and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: The Effect under Alternative Treatments of the Coffee Price Collapse

	(1) Baseline	(2) Coffee region by year	(3) Coffee departments only	(4) Schools with coffee
Quindío × 1999–2002	-0.1569*** (0.0424)	-0.1410*** (0.0442)	-0.1325*** (0.0434)	-0.1650*** (0.0511)
Quindío × 2002–2014	-0.0412 (0.0352)	-0.0373 (0.0392)	-0.0019 (0.0374)	-0.0383 (0.0417)
Rest of the disaster zone × 1999–2002	-0.0302 (0.0267)	-0.0162 (0.0287)	-0.0132 (0.0270)	-0.0430 (0.0305)
Rest of the disaster zone × 2002–2014	0.0078 (0.0579)	0.0102 (0.0602)	0.0264 (0.0520)	-0.0085 (0.0607)
Observations	9,791,518	9,791,518	1,807,076	6,409,116
Municipalities	1,103	1,103	142	992
School Fixed Effects	Yes	Yes	Yes	Yes
Test-Edition Fixed Effects	Yes	Yes	Yes	Yes
Coffee-Department-by-Year Fixed Effects	No	Yes	No	No

Notes. This table presents regression estimates of the effect of the earthquake on Saber 11 test scores under alternative treatments of the coffee price collapse. The outcome variable is the national percentile rank standardized within test edition. The sample includes test takers from 1996 to 2014. Each column separates, within a single regression, the twelve municipalities of Quindío from the sixteen other municipalities in the disaster zone, both measured against the rest of the country. The post-earthquake period labeled 1999–2002 covers the years 1999 to 2001. Column (1) is the baseline. Column (2) adds year fixed effects interacted with an indicator for the five coffee departments. Column (3) restricts the sample to schools in those departments. Column (4) restricts the sample to schools with coffee growing in their own neighborhood, measured as a positive harvested area of stimulants in the surrounding cells of the GAEZ agro-ecological grid. All schools in the disaster zone satisfy this definition. All columns include school and test-edition fixed effects and control for sex, age, age squared and an indicator for missing age. Standard errors are clustered at the municipality level and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Household Characteristics and the Composition of Test Takers

	(1) Test score	(2) Log test takers	(3) Female	(4) Age	(5) Mother secondary+	(6) Mother tertiary	(7) Father secondary+	(8) Father tertiary
Quindío × 1999–2002	-0.1556*** (0.0447)	-0.0498** (0.0244)	0.0046 (0.0054)	0.0532 (0.1607)	0.0072 (0.0342)	0.0011 (0.0116)	-0.0285 (0.0466)	-0.0013 (0.0119)
Rest of the disaster zone × 1999–2002	-0.0074 (0.0340)	-0.0877** (0.0340)	0.0065** (0.0030)	0.0281 (0.1026)	-0.0122 (0.0172)	-0.0073* (0.0044)	-0.0346 (0.0226)	-0.0065 (0.0068)
Observations	41,211	41,496	41,496	41,277	41,351	41,351	35,514	35,514
Municipalities	1,071	1,072	1,072	1,071	1,071	1,071	1,071	1,071
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table presents regression estimates of the effect of the earthquake on the composition of test takers. Each column is a separate regression on a school-by-year panel built from the universe of Saber 11 test takers, with school and year fixed effects. Quindío and the rest of the disaster zone enter together and are both measured against the rest of the country. The sample runs from 1996 to 2001, and from 1997 to 2001 in columns (7) and (8), and the post-earthquake period labeled 1999–2002 covers the years 1999 to 2001. The outcome in column (1) is the mean test score of a school's test takers, standardized within test edition. The outcome in column (2) is the log number of test takers. The outcomes in columns (3) and (4) are the share of female test takers and their mean age, both recorded at registration. The outcomes in columns (5) to (8) are the share of test takers whose mother completed secondary education or more, whose mother completed tertiary education, whose father completed secondary education or more, and whose father completed tertiary education, respectively. Secondary education or more includes test takers with tertiary education. Columns (1) and (3) to (8) are weighted by the number of test takers in the cell and column (2) is unweighted. Standard errors are clustered at the municipality level and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: School Progression after the Earthquake

	(1) Dropout	(2) Promotion	(3) Repetition	(4) Log enrollment
Quindío × 1999–2002	-0.0005 (0.0076)	-0.0073 (0.0060)	0.0078 (0.0060)	0.0201 (0.0170)
Rest of the disaster zone × 1999–2002	0.0090* (0.0047)	-0.0113* (0.0063)	0.0023 (0.0038)	0.0020 (0.0157)
Observations	321,876	321,876	321,876	354,588
Municipalities	1,110	1,110	1,110	1,110
School Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Notes. Each column is a regression on the annual school census with school and year fixed effects. The dropout, promotion and repetition rates are the number of students in each category divided by total enrollment at the start of the year. Standard errors are clustered at the municipality level and 1998 is the omitted year. Column (4) is the log of total enrollment, with the same specification. The sample runs from 1997 to 2002. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Decomposing and Bounding the Main Effect to Account for Selection

	Effect (1)	Decomposition			Trimming (5)	Bounds	
		Within (2)	Entry (3)	Exit (4)		Lower (6)	Upper (7)
Quindío	-0.153*** (0.052)	-0.145*** (0.042)	-0.008 (0.013)	0.000	0.057	-0.270*** (0.055)	-0.059 (0.055)
Rest of the disaster zone	0.002 (0.034)	-0.012 (0.031)	0.013*** (0.005)	0.000	0.092	-0.167*** (0.040)	0.158*** (0.042)
Disaster zone	-0.049 (0.042)	-0.054 (0.037)	0.005 (0.007)	0.000	0.081	-0.198*** (0.047)	0.079* (0.048)

Notes. This table presents the decomposition and the bounds of the main effect of the earthquake on Saber 11 test scores for Quindío, for the rest of the disaster zone, and for the disaster zone as a whole. Column (1) computes the effect using the difference-in-differences comparison of 1996 to 1998 with 1999 to 2001, with the rest of the country as the comparison group and scores standardized within year. Columns (2) to (4) decompose column (1) into the contribution of continuing schools, of entering schools and of exiting schools, as defined in Equation 5. The three columns sum to column (1). No school in the disaster zone has test takers only before the earthquake, so the exit term is zero for the three rows and has no standard error. Columns (5) to (7) bound column (1) following Lee (2009). Column (5) is the percent drop in the number of test takers between 1998 and the average of 1999 to 2001, relative to the same change in the rest of the country, and has no standard error. Columns (6) and (7) are the lower and upper bounds, obtained by trimming the post-earthquake score distribution of the rest of the country by that proportion from the bottom and from the top, respectively. Standard errors are clustered at the municipality level and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effects on Physical Plant between Public and Private Schools

	(1) Log built area	(2) Log classroom area	(3) Log sanitary units	(4) Log equipment
Quindío × 1999–2002	-0.0288 (0.0506)	-0.0277 (0.0547)	-0.0642** (0.0319)	-0.0668** (0.0335)
Rest of the disaster zone × 1999–2002	-0.0294 (0.0225)	-0.0127 (0.0262)	-0.0255 (0.0239)	-0.0634*** (0.0179)
Quindío × 1999–2002 × Private	-0.1432 (0.1262)	0.0741 (0.1480)	-0.0901** (0.0356)	-0.0530* (0.0302)
Rest of the disaster zone × 1999–2002 × Private	0.1316*** (0.0351)	0.0425 (0.0516)	-0.0244 (0.0322)	0.0231 (0.0418)
Observations	224,626	218,091	161,446	228,210
Municipalities	1,101	1,101	1,088	1,101
School Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

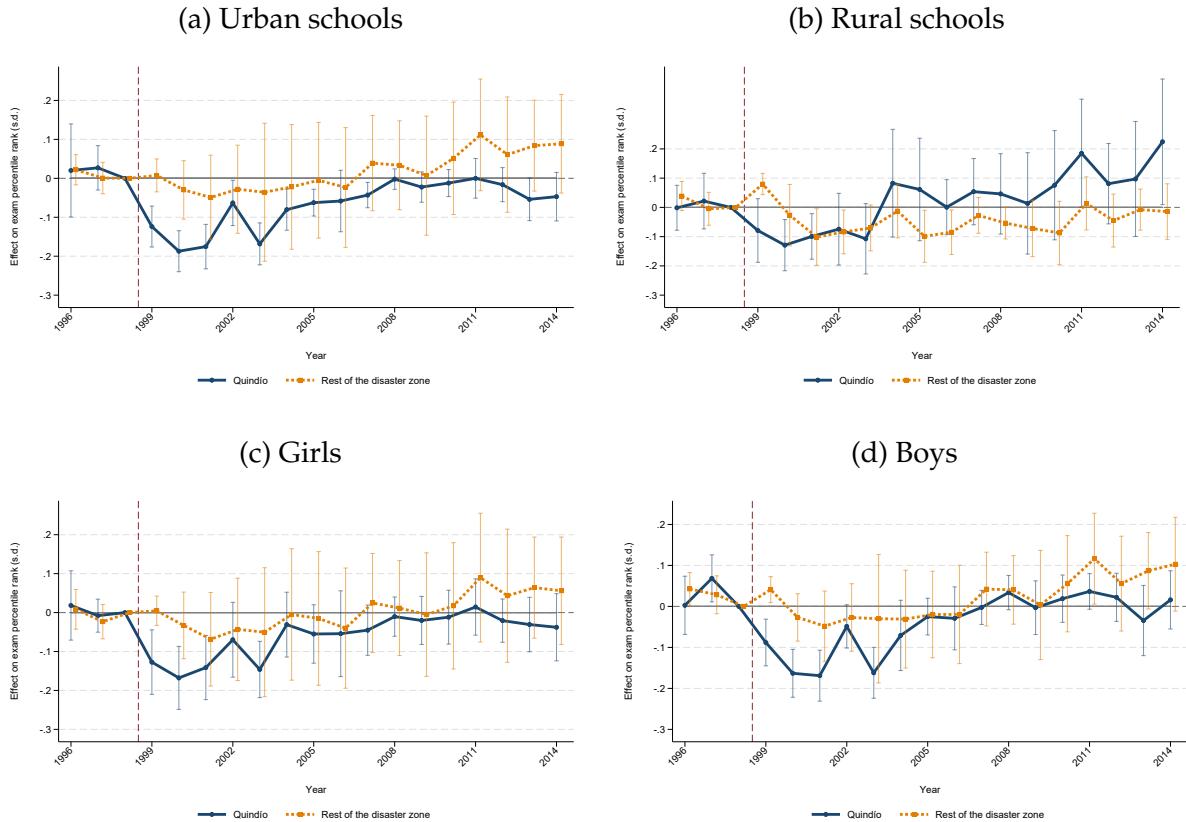
Notes. This table presents regression estimates of the effect of the earthquake on school inputs. Each column is a regression on all schools of the annual school census, 1997 to 2001, with the treatment terms interacted with a private indicator, school fixed effects, year fixed effects that differ by sector, and standard errors clustered at the municipality level. The first two rows are the effects for public schools and the last two rows are the differences between private and public schools, so the effect for private schools is the sum of each pair. The outcomes are the logs of the built area, the classroom area, the number of sanitary units and the number of items of equipment. They are taken from the plant tables of the C-600 census, which record the area of the establishment by type, the number of sanitary units, the number of items of equipment and the facilities the school has. The sanitary-unit table is not available after 2000, so column (3) ends in that year. The post-earthquake period labeled 1999–2002 covers the years 1999 to 2001. The sample ends in 2001 because the number of school records in Quindío falls from 448 in 2001 to 161 in 2002. Standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

A Appendix Figures and Tables

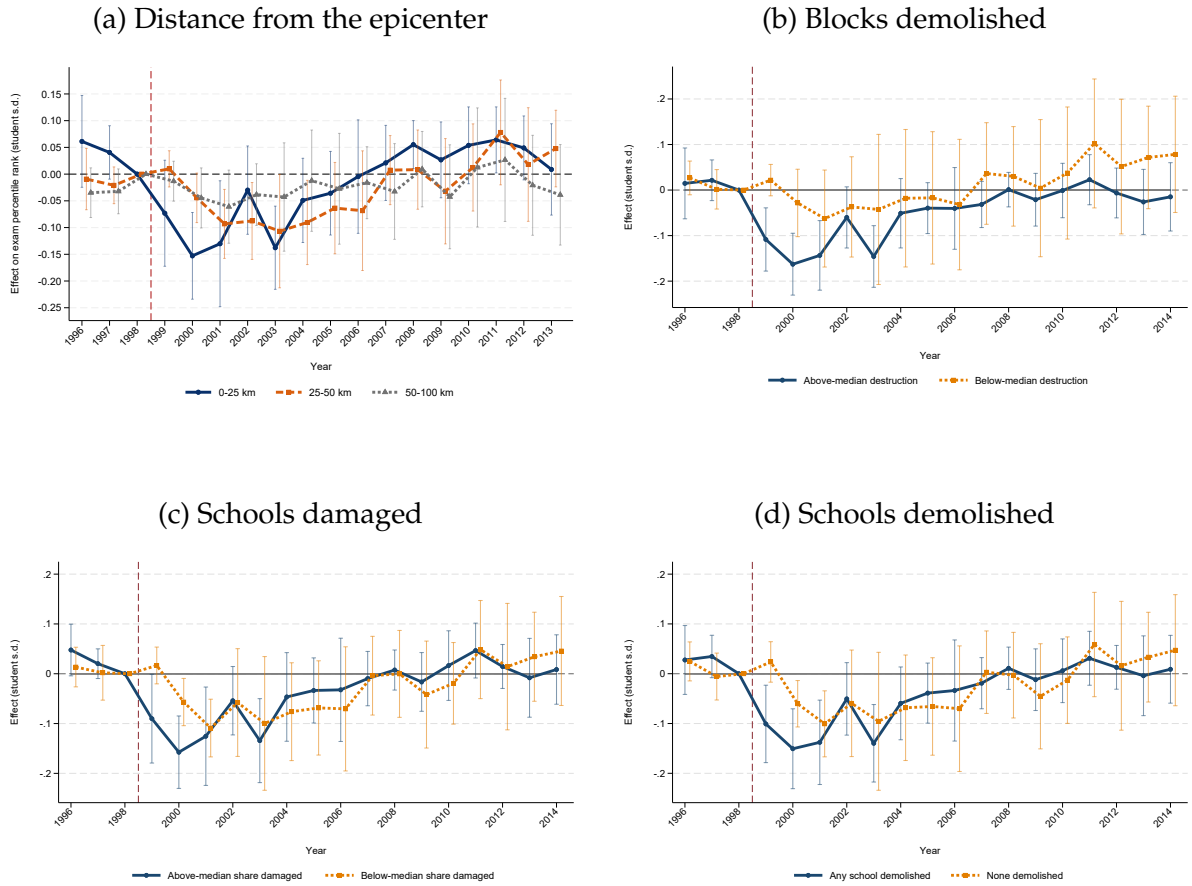
A.1 Appendix Figures

Appendix Figure A1: Test Score Effects of the Earthquake by Subgroups



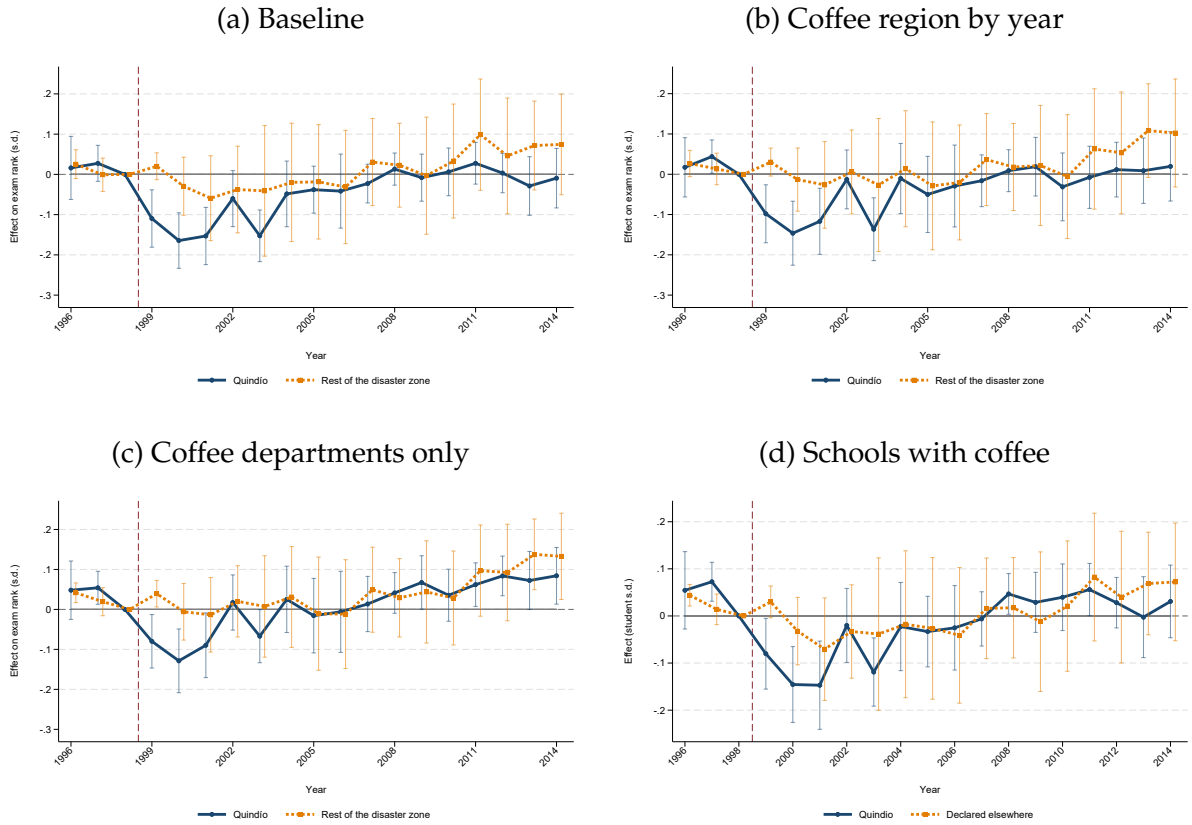
Notes. This figure plots the estimated effect of the earthquake on Saber 11 test scores by year for four subsamples, urban schools in panel (a), rural schools in panel (b), girls in panel (c) and boys in panel (d). The outcome variable is the national percentile rank standardized within test edition. Each panel separates, within a single regression, the twelve municipalities of Quindío from the sixteen declared in the other four departments, both measured against the rest of the country. The specification includes school and test-edition fixed effects and controls for sex, age, age squared and an indicator for missing age, where sex is excluded in panels (c) and (d). Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year, and the dashed line marks the earthquake.

Appendix Figure A2: Test Score Effect by Exposure



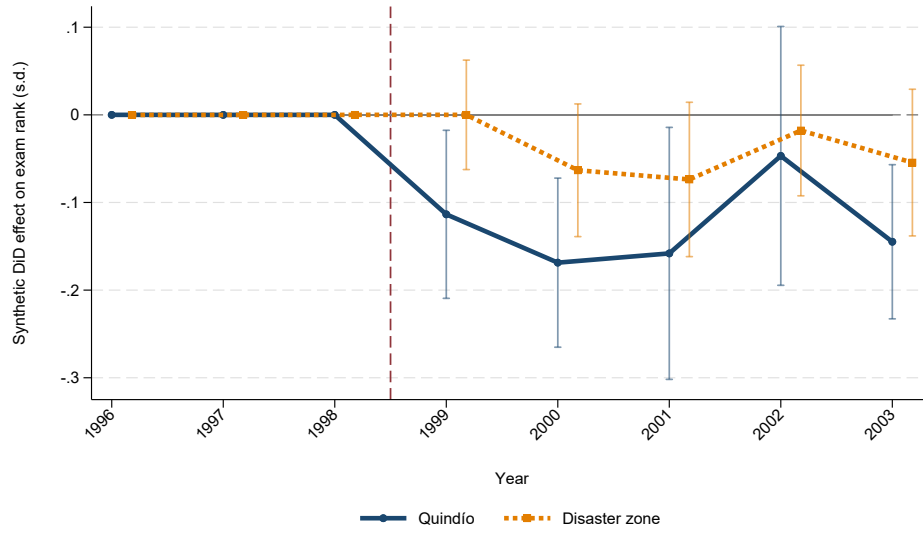
Notes. This figure plots the estimated effect of the earthquake on Saber 11 test scores by year for four measures of exposure. The outcome variable is the national percentile rank standardized within test edition. Panel (a) estimates separate effects for schools in successive distance bands from the epicenter, within a single regression, with schools more than 200 kilometers from the epicenter as the comparison group. Panels (b) to (d) split the declared municipalities into two groups using a measure of destruction recorded in the census of February 1999, and estimate a separate effect for each group against the undeclared municipalities. Panel (b) splits at the median share of a municipality's blocks recorded as demolished. Panel (c) splits at the median of the number of educational establishments recorded as damaged, divided by the number of schools the municipality reported to the C-600 census in 1998. Panel (d) compares municipalities with any establishment condemned for demolition to those with none, using the same denominator. Declared municipalities without a damage record are excluded from panels (b) to (d). The specification includes school and test-edition fixed effects and controls for sex, age, age squared and an indicator for missing age. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year, and the dashed line marks the earthquake.

Appendix Figure A3: Robustness of the Effect to the Coffee Price Collapse



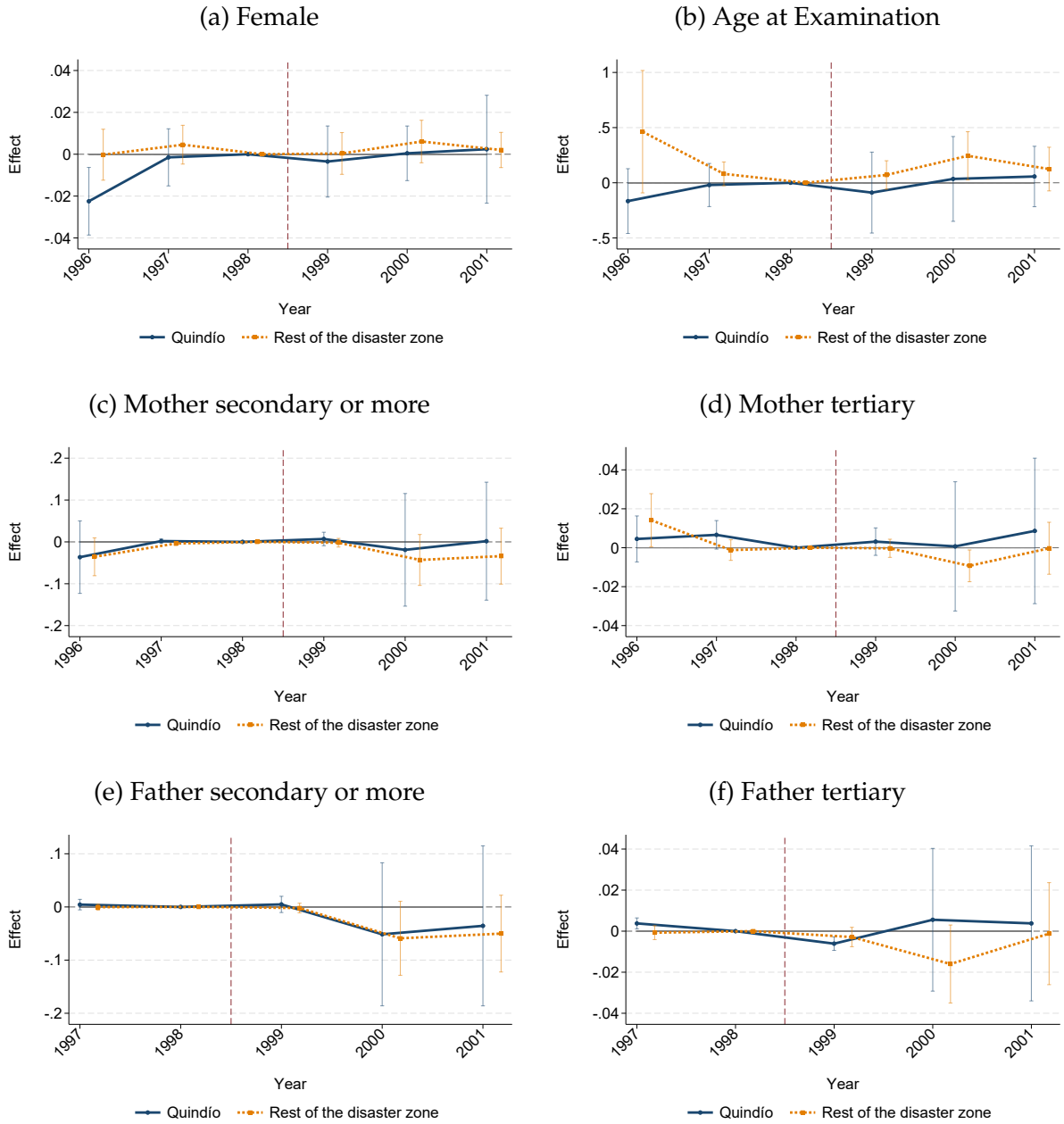
Notes. This figure plots the estimated effect of the earthquake on Saber 11 test scores by year under four treatments of the coffee price collapse. The outcome variable is the national percentile rank standardized within test edition. Each panel separates, within a single regression, the twelve municipalities of Quindío from the sixteen declared elsewhere, both measured against the undeclared municipalities. Panel (a) is the baseline. Panel (b) adds year fixed effects interacted with an indicator for the five coffee departments. Panel (c) restricts the sample to schools in those departments. Panel (d) restricts the sample to schools with coffee growing in their own neighborhood, measured as a positive harvested area of stimulants in the surrounding cells of the GAEZ agro-ecological grid. All schools in the declared zone satisfy this definition. All panels include school and test-edition fixed effects and control for sex, age, age squared and an indicator for missing age. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year, and the dashed line marks the earthquake.

Appendix Figure A4: Synthetic Difference-in-Differences



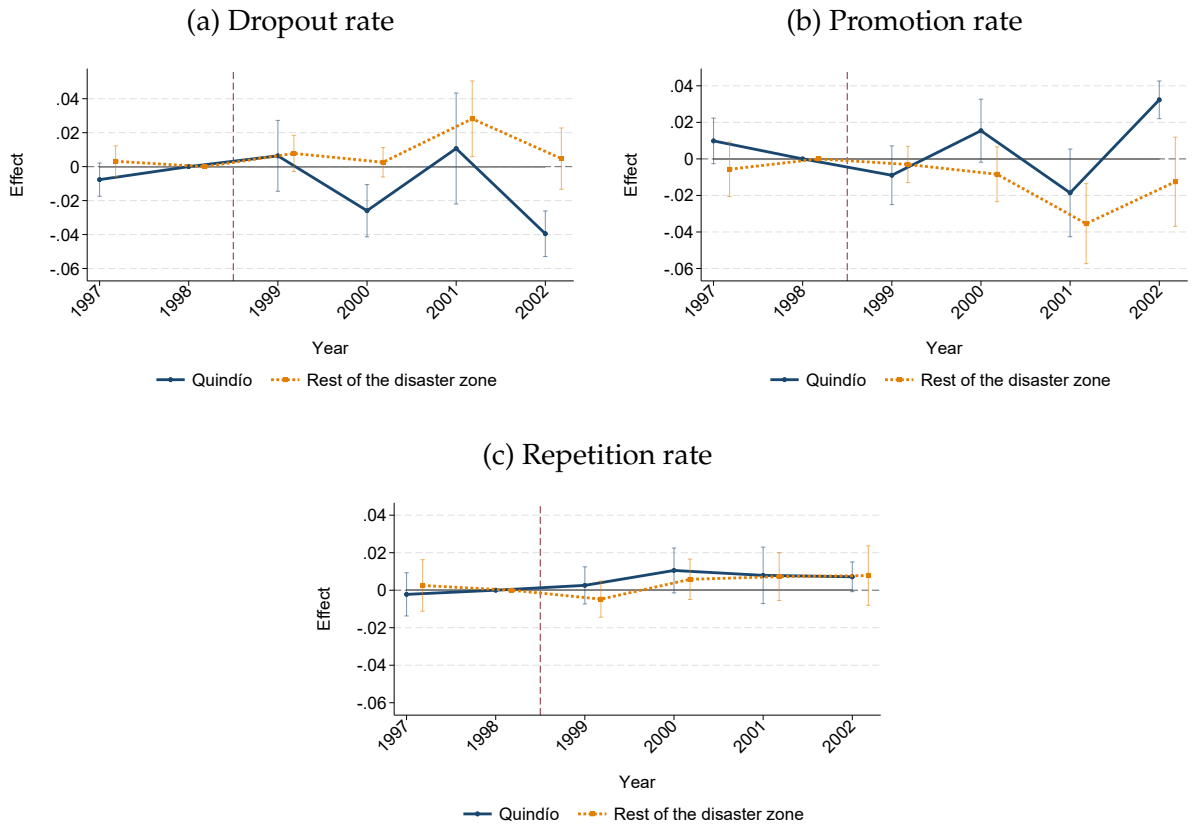
Notes. This figure plots synthetic difference-in-differences estimates of the effect of the earthquake on Saber 11 test scores by year, following [Arkhangelsky et al. \(2021\)](#). The data are a school-by-year panel of the mean examination rank from 1996 to 2003. The estimator assigns weights to comparison schools and to pre-earthquake years. The panel includes the 5,334 schools with test takers in every year of the window, of which 207 are in the declared zone and 67 are in Quindío. The figure shows estimates for Quindío and for the disaster zone as a whole. Standard errors are bootstrapped with 50 replications clustered at the municipality level. The year 1998 is the omitted year, and the dashed line marks the earthquake.

Appendix Figure A5: Characteristics of Test Takers



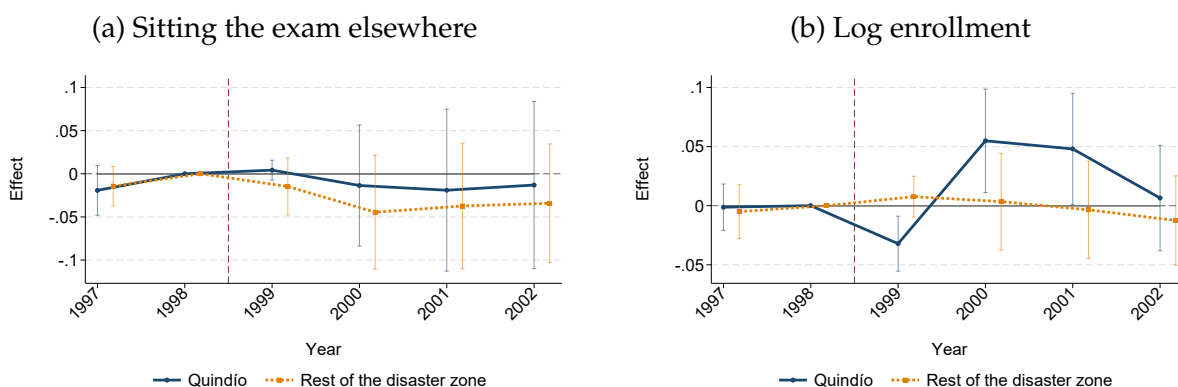
Notes. This figure plots the estimated effect of the earthquake on the characteristics of test takers by year. The outcomes are the share of female test takers in panel (a), mean age at examination in panel (b), the share of test takers whose mother completed secondary education or more in panel (c) and tertiary education in panel (d), and the share of test takers whose father completed secondary education or more in panel (e) and tertiary education in panel (f). Secondary education or more includes test takers with tertiary education. Female and age are recorded at registration. Each panel comes from a school-by-year panel built from the universe of Saber 11 test takers, weighted by the number of test takers in the cell, which separates, within a single regression, Quindío from the declared municipalities elsewhere, both measured against the undeclared municipalities. The specification includes school and year fixed effects. The sample runs from 1996 to 2001, and from 1997 to 2001 in panels (e) and (f). Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year.

Appendix Figure A6: Effects on School Progression



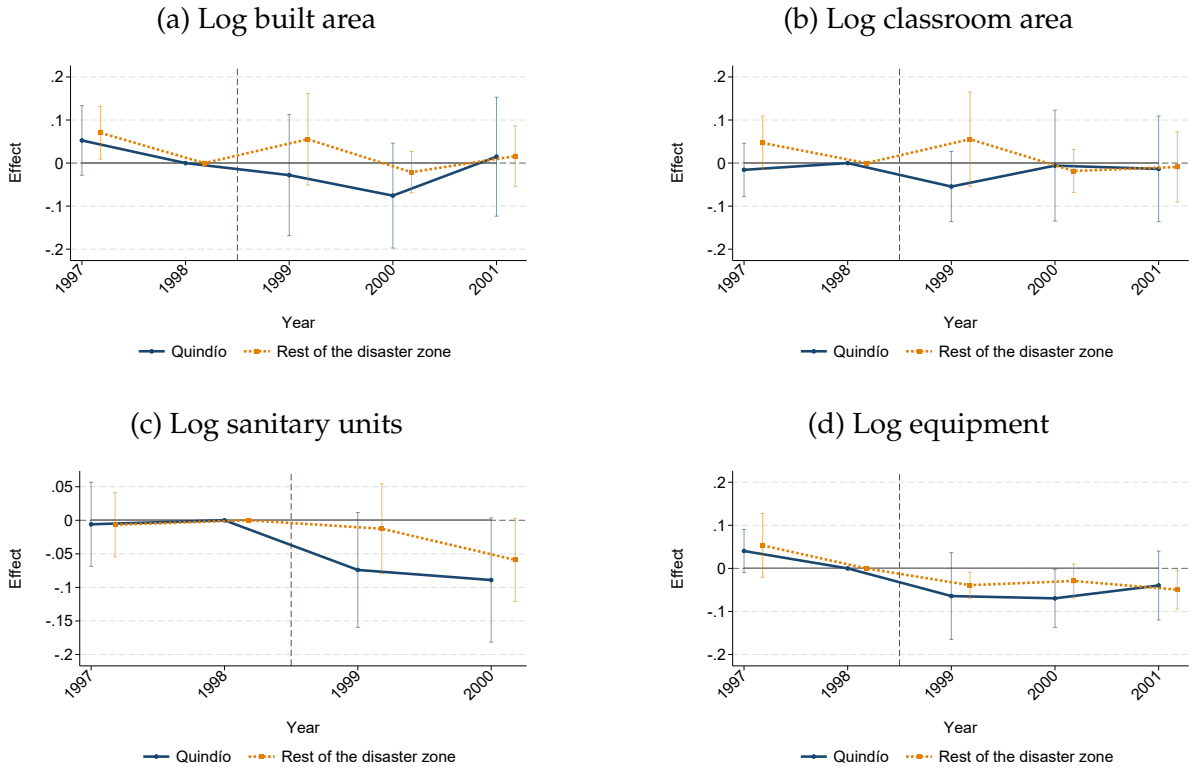
Notes. This figure plots the estimated effect of the earthquake by year on the dropout rate in panel (a), the promotion rate in panel (b) and the repetition rate in panel (c). The data are the annual school census from 1997 to 2002, which covers every school in the country. Each rate is the number of students in each category divided by total enrollment at the start of the year. Each panel separates, within a single regression, the twelve municipalities of Quindío from the sixteen declared elsewhere, both measured against the undeclared municipalities. The specification includes school and year fixed effects. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year.

Appendix Figure A7: Evidence of No Effects on School Transfers



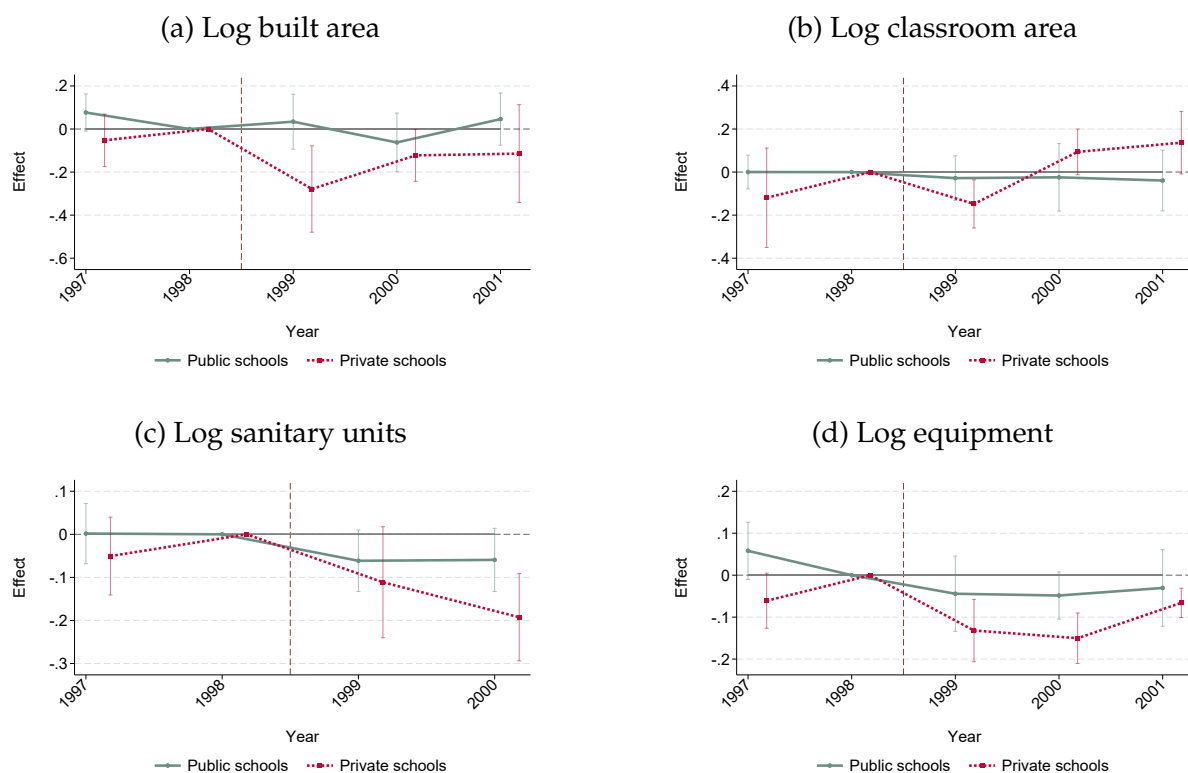
Notes. This figure plots the estimated effect of the earthquake by year on two outcomes. Panel (a) uses a school-by-year panel built from the universe of Saber 11 test takers, weighted by the number of test takers in the cell, with an indicator for sitting the examination outside the student's own municipality as the outcome. Panel (b) uses the annual school census, with the log of enrollment as the outcome, for 1997 to 2002, and includes school and year fixed effects. Both panels separate, within a single regression, Quindío from the declared municipalities elsewhere, with the undeclared municipalities as the comparison group. Standard errors are clustered at the municipality level. Bars are 95 percent confidence intervals, 1998 is the omitted year, and the dashed line marks the earthquake. From 2002 on, the school census merges schools into larger institutions under Ley 715 of 2001.

Appendix Figure A8: Effects on School Inputs and Physical Plant



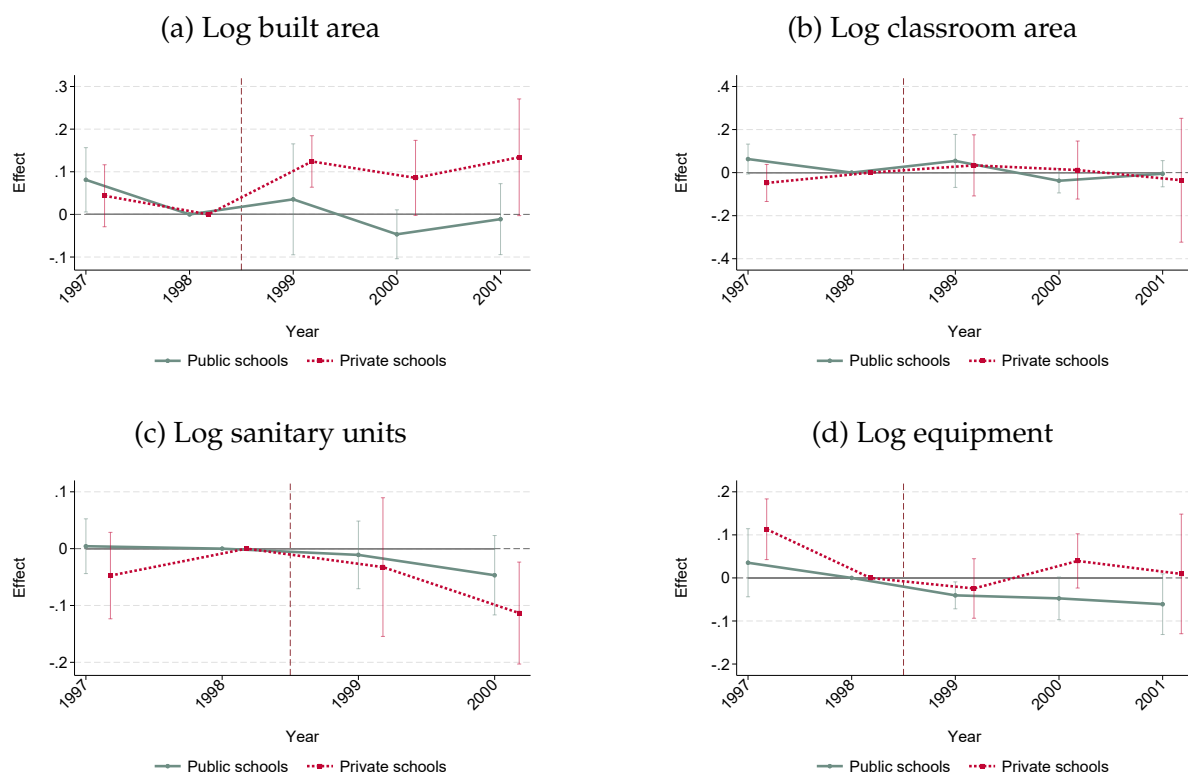
Notes. This figure plots the estimated effect of the earthquake by year on four school inputs from the annual school census. The outcomes are the log of the built area in panel (a), the log of the classroom area in panel (b), the log of the number of sanitary units in panel (c) and the log of the number of items of equipment in panel (d). The sample runs from 1997 to 2001, and panel (c) ends in 2000, the last year with a table of sanitary units. Each panel separates, within a single regression, the twelve municipalities of Quindío from the sixteen declared elsewhere, both measured against the rest of the country, and includes school and year fixed effects. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year.

Appendix Figure A9: Effects on School Inputs and Physical Plant in Quindío



Notes. This figure plots the estimated effect of the earthquake by year on the same four school inputs, separately for public and private schools in Quindío. Each sector is estimated on its own schools and measured against schools of the same sector in the rest of the country. The sample runs from 1997 to 2001, and panel (c) ends in 2000, the last year with a table of sanitary units. The specification includes school and year fixed effects. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year.

Appendix Figure A10: Effects on School Inputs and Physical Plant in Rest of Disaster Zone



Notes. This figure plots the estimated effect of the earthquake by year on the same four school inputs, separately for public and private schools in the sixteen municipalities declared outside Quindío. Each sector is estimated on its own schools and measured against schools of the same sector in the rest of the country. The sample runs from 1997 to 2001, and panel (c) ends in 2000, the last year with a table of sanitary units. The specification includes school and year fixed effects. Bars are 95 percent confidence intervals based on standard errors clustered at the municipality level. The year 1998 is the omitted year.

A.2 Appendix Tables

Appendix Table A1: Damage to Educational Establishments and Reconstruction Contracting within the Disaster Zone

Municipality	Department	Distance (1)	Damaged (2)	Minor (3)	Repair (4)	Demolished (5)	Pupils (6)	Contracting (7)
Armenia	Quindío	4.3	85	14	40	31	9,335	609,907
Calarcá	Quindío	6.9	14	1	5	8	5,030	147,617
Montenegro	Quindío	9.3	1	0	0	1	333	27,551
La Tebaida	Quindío	11.5	7	2	2	3	1,960	30,138
Córdoba	Quindío	12.1	3	0	0	3	797	3,655
Circasia	Quindío	14.6	2	0	1	1	570	17,940
Quimbaya	Quindío	15.7	0	0	0	0	0	25,243
Buenavista	Quindío	16.2	3	3	0	0	0	6,188
Filandia	Quindío	19.6	3	2	0	1	608	8,450
Pijao	Quindío	19.7	3	1	2	0	0	17,993
Salento	Quindío	21.2	5	1	3	1	263	6,860
Ulloa	Valle	23.1	0	0	0	0	0	2,613
Alcalá	Valle	23.8	3	0	3	0	0	3,484
Caicedonia	Valle	26.8	15	8	1	6	1,072	18,422
Cajamarca	Tolima	27.8	1	0	1	0	0	11,563
Obando	Valle	30.9	5	2	3	0	0	4,265
Pereira	Risaralda	34.1	18	16	2	0	0	140,068
Génova	Quindío	34.5	2	2	0	0	0	8,738
La Victoria	Valle	35.2	1	0	1	0	0	3,404
Sevilla	Valle	35.6	14	7	7	0	0	11,227
Dosquebradas	Risaralda	37.3	4	1	3	0	0	9,723
Santa Rosa de Cabal	Risaralda	42.9	7	2	5	0	0	28,393
Marsella	Risaralda	48.9	2	1	1	0	0	5,647
Roncesvalles	Tolima	50.1	0	0	0	0	0	1,743
Argelia	Valle	52.8	3	0	3	0	0	3,772
Chinchiná	Caldas	54.5	0	0	0	0	0	1,187
Bolívar	Valle	67.0	1	1	0	0	0	2,379
Barragán (Tuluá)	Valle	71.9	0	0	0	0	0	174
Total			202	64	83	55	19,968	1,158,344

Notes. Damage to educational establishments recorded by the DANE census of the earthquake zone, conducted in February 1999. Column 1 is the mean straight-line distance from the municipality's geocoded schools to the epicenter, in kilometers, and municipalities are sorted by it. The census enumerated establishments that sustained damage, so column 2 counts damaged establishments rather than all establishments in the municipality, and columns 3 to 5 sum to it exactly. Minor denotes partial damage not requiring works, Repair damage requiring repair, and Demolished establishments condemned. Column 6 counts the pupils enrolled in establishments condemned for demolition. All twenty-eight municipalities in the disaster zone are shown; those with no damaged establishments recorded show zeros in columns 2 to 6. Of the fifty-five establishments condemned, forty-nine are in Quindío and the remaining six are in Caicedonia. Column 7 reports total FOREC reconstruction contracting by municipality, in millions of pesos, drawn from Valencia Barrera's own accounting of the program rather than from the DANE damage census, and covers every sector the program funded rather than education alone. Armenia and Calarcá are each contracted through two separate zonal managements in the source and are summed here.

Appendix Table A2: Share of Test Takers Answering Each Background Question

Question	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
Female	100	100	100	100	100	100	100	100	96	100
Age at examination	85	100	100	100	100	100	100	100	99	100
Sits exam outside school municipality	66	100	100	100	100	100	100	0	0	0
Mother has secondary education or more	50	84	85	83	100	100	100	100	100	100
Mother has tertiary education	50	84	85	83	100	100	100	100	100	100
Father has secondary education or more	47	76	77	75	100	100	100	89	11	0
Father has tertiary education	47	76	77	75	100	100	100	89	11	0

Notes. Each cell is the percentage of test takers *with a non-missing test score* in that year for whom the variable also has a non-missing value, pooling both sittings. The base is the estimation sample rather than every row in the file. Female and age come from the registration record, whether the student sits the exam outside the school's municipality comes from the municipality where the examination was sat, and the other rows come from the ICFES background questionnaire. Mother's and father's education each come from a single block of questions, so the secondary-or-more and tertiary-or-more rows share the same coverage.

Appendix Table A3: Descriptive Statistics of Test Score Data

Variable	Obs.	All, 1996–98		Disaster zone		Quindío		Rest of the country		All years	
		Mean (1)	S.D. (3)	Mean (4)	S.D. (5)	Mean (6)	S.D. (7)	Mean (8)	S.D. (9)	Obs. (10)	Mean (11)
Panel A: Sample description											
Saber 11 Test Score	1,110,607	-0.000	1.000	-0.029	0.956	0.010	0.945	0.001	1.002	9,812,178	-0.000
Female	1,110,606	0.552	0.497	0.550	0.497	0.542	0.498	0.552	0.497	9,792,884	0.542
Age at examination	1,058,198	17.912	3.475	18.014	3.813	17.886	3.507	17.907	3.458	9,741,374	18.068
Sits exam outside school municipality	998,077	0.171	0.376	0.148	0.355	0.190	0.392	0.172	0.377	5,889,935	0.121
Panel B: Household characteristics											
Mother has secondary education or more	822,585	0.853	0.354	0.850	0.357	0.867	0.340	0.853	0.354	9,370,576	0.622
Mother has tertiary education	822,585	0.040	0.195	0.027	0.161	0.030	0.170	0.040	0.197	9,370,576	0.106
Father has secondary education or more	752,383	0.852	0.356	0.851	0.356	0.874	0.332	0.852	0.356	6,984,829	0.590
Father has tertiary education	752,383	0.059	0.235	0.045	0.206	0.046	0.209	0.060	0.237	6,984,829	0.115

Notes. Columns 1 to 9 are Saber 11 test takers in 1996, 1997 and 1998, before the January 1999 earthquake. Obs. is the number of test takers with a non-missing test score and a non-missing value for that row's own variable. The variables in Panel B come from the ICFES background questionnaire. Disaster zone is every municipality named by Decreto 195 de 1999, of which Quindío is the epicentral subset, and Rest of the country is every municipality outside it. The final two columns drop the pre-earthquake restriction and report the same Obs. and Mean over every year each variable is observed, with pre- and post-earthquake years pooled. Scores are standardized on the national distribution within test edition, so their means are differences from the national average of the same sitting.

Appendix Table A4: The Effect of the Earthquake on Exit Examination Performance

	(1) All	(2) Urban	(3) Rural	(4) Girls	(5) Boys
Quindío × 1999–2002	-0.1569*** (0.0424)	-0.1783*** (0.0442)	-0.1102*** (0.0259)	-0.1487*** (0.0465)	-0.1649*** (0.0368)
Quindío × 2002–2014	-0.0412 (0.0352)	-0.0600* (0.0326)	0.0162 (0.0503)	-0.0422 (0.0373)	-0.0430 (0.0335)
Rest of the disaster zone × 1999–2002	-0.0302 (0.0267)	-0.0308 (0.0298)	-0.0260 (0.0269)	-0.0263 (0.0283)	-0.0351 (0.0229)
Rest of the disaster zone × 2002–2014	0.0078 (0.0579)	0.0200 (0.0623)	-0.0636* (0.0326)	0.0145 (0.0645)	0.0037 (0.0488)
Observations	9,791,518	8,363,578	1,427,940	5,309,713	4,481,738
Municipalities	1,103	1,091	939	1,103	1,103
School Fixed Effects	Yes	Yes	Yes	Yes	Yes
Test-Edition Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes. This table presents regression estimates of the effect of the earthquake on Saber 11 test scores for the full sample and for four subsamples. The outcome variable is the national percentile rank standardized within test edition, where one standard deviation is about 28.6 percentile points. Each column is a single regression that separates the twelve municipalities of Quindío from the rest of the disaster zone, both measured against the rest of the country. Column (1) uses the full sample, and columns (2) to (5) use urban schools, rural schools, girls and boys, respectively. All columns include school and test-edition fixed effects and control for sex, age, age squared and an indicator for missing age, where sex is excluded in columns (4) and (5). The post-earthquake periods labeled 1999–2002 and 2002–2014 cover the years 1999 to 2001 and 2002 to 2014. Standard errors are clustered at the municipality level and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A5: The Effect Measured by Destruction rather than by Distance

	(1) Blocks, split	(2) Blocks, cont.	(3) Schools, split	(4) Schools, cont.	(5) Horse race
Above-median destruction × 1999–2002	-0.1437*** (0.0461)				
Below-median destruction × 1999–2002	-0.0181 (0.0262)				
Share of blocks demolished × 1999–2002		-0.3898*** (0.1462)			-0.3389** (0.1592)
Any school demolished × 1999–2002			-0.1446*** (0.0488)		
Damaged, none demolished × 1999–2002			-0.0218 (0.0250)		
Share of damaged schools demolished × 1999–2002				-0.2406* (0.1402)	
Disaster zone × 1999–2002					-0.0176 (0.0239)
Observations	2,465,458	2,465,458	2,465,458	2,465,458	2,465,458
Municipalities	1,077	1,077	1,077	1,077	1,077
School Fixed Effects	Yes	Yes	Yes	Yes	Yes
Test-Edition Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes. This table presents regression estimates of the effect of the earthquake on Saber 11 test scores using measures of destruction. The outcome variable is the national percentile rank standardized within test edition. The sample includes test takers from 1996 to 2001, and the post-earthquake period labeled 1999–2002 covers the years 1999 to 2001. Destruction is measured by municipality from the census of February 1999. Column (1) splits the municipalities in the disaster zone at the median share of blocks recorded as demolished, with each group measured against the rest of the country. Column (2) enters the share of blocks recorded as demolished as a continuous measure. Column (3) compares municipalities with any educational establishment condemned for demolition and municipalities with damaged establishments and none condemned, each measured against the rest of the country. Column (4) enters the share of damaged establishments condemned for demolition as a continuous measure. Column (5) enters the share of blocks demolished and the disaster zone indicator together. All columns include school and test-edition fixed effects and control for sex, age, age squared and an indicator for missing age. Standard errors are clustered at the municipality level and displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Accounting of Selection Bias

Let $\bar{y}_{g,t}$ denote the average test score observed among test takers who take the exam in group $g \in \{T, C\}$ at time $t \in \{\text{pre}, \text{post}\}$, and let $\hat{\beta}$ be the difference-in-differences estimator,

$$\hat{\beta} = (\bar{y}_{T,\text{post}} - \bar{y}_{T,\text{pre}}) - (\bar{y}_{C,\text{post}} - \bar{y}_{C,\text{pre}}). \quad (\text{B1})$$

Write $\bar{y}_{g,\text{post}} = \bar{y}_{g,\text{post}}^* + b_g$, where $\bar{y}_{g,\text{post}}^*$ is the average that would be observed if the post-period sample retained the same composition as the pre-period sample, and b_g is the shift in the observed average attributable to compositional change alone.

Substituting into (B1), with pre-period averages unaffected by selection since the sample composition there is the baseline against which b_g is defined, gives

$$\hat{\beta} = [(\bar{y}_{T,\text{post}}^* - \bar{y}_{T,\text{pre}}) - (\bar{y}_{C,\text{post}}^* - \bar{y}_{C,\text{pre}})] + (b_T - b_C).$$

The bracketed term is the difference-in-differences computed on the composition held fixed at its pre-period state, which parallel trends in $\bar{y}^*$ identifies as the true effect β . Equation (B1) therefore collapses to

$$\hat{\beta} = \beta + (b_T - b_C), \quad (\text{B2})$$

where β is the true learning effect and $b_T - b_C$ is the selection bias.

Dropout. Suppose a share p of the treated group's test takers drop and do not take the exam. Assume also that these leavers have average test scores δ . The sign of δ is not restricted; so $\delta > 0$ describes negative selection of the remaining sample, since the best students are the ones who leave, while $\delta < 0$ describes positive selection, since the worst students leave instead. For a group of size N , removing pN test takers with mean δ shifts the observed mean of the remaining $(1 - p)N$ by

$$b_T^{\text{drop}} = -\frac{p}{1-p} \delta, \quad b_C^{\text{drop}} = 0,$$

since no one reappears in the control group. The resulting bias is

$$\hat{\beta} = \beta + -\frac{p}{1-p} \delta, \quad (\text{B3})$$

with a sign that depends entirely on δ . When $\delta > 0$ (the best leave, or negative selection), the bias is negative, $\hat{\beta}$ is biased downward, and $|\hat{\beta}|$ is an upper bound on $|\beta|$: the estimate overstates the true loss. When $\delta < 0$ (the worse leave, or positive selection), the bias is positive, $\hat{\beta}$ is biased toward zero, and $|\hat{\beta}|$ is a lower bound on $|\beta|$: the estimate understates the true loss.

Transfer (SUTVA violation). Now suppose the same pN leavers do not drop out but instead sit the examination in the control group, of size N_C . The treated group loses the same test takers as before, so $b_T^{\text{trans}} = b_T^{\text{drop}}$. The control group's post-period mean absorbs them:

$$b_C^{\text{trans}} = \frac{pN}{N_C + pN} [(\bar{y}_{T,\text{post}}^* + \delta) - \bar{y}_{C,\text{post}}^*] \approx \frac{pN}{N_C} \delta \quad (\text{B4})$$

for $pN \ll N_C$ and $\bar{y}_{T,\text{post}}^* \approx \bar{y}_{C,\text{post}}^*$ under parallel trends. The resulting bias is

$$b_T^{\text{trans}} - b_C^{\text{trans}} = -\frac{p}{1-p} \delta - \frac{pN}{N_C} \delta, \quad (\text{B5})$$

again signed by δ alone. Negative selection ($\delta > 0$) overstates the true loss and positive selection ($\delta < 0$) understates it, exactly as under dropout. What changes is the magnitude. Dropout distorts only the treated-group term of (B2); transfer distorts both terms, and the two distortions move together rather than offsetting, since $\frac{pN}{N_C} \delta$ and $-\frac{p}{1-p} \delta$ share the sign of $-\delta$. So

$$|b_T^{\text{trans}} - b_C^{\text{trans}}| \geq |b_T^{\text{drop}} - b_C^{\text{drop}}|, \quad (\text{B6})$$

with equality only when $N_C \rightarrow \infty$ (the leavers are a negligible share of the control pool): transfer biases the estimate in the same direction as dropout, only more.

Table B1 summarizes the direction of the bias in all four cases.

Appendix Table B1: Direction of selection bias, by mechanism and by who selects out

	Dropout	Transfer (SUTVA)
Negative selection ($\delta > 0$), best leave	Overstates loss	Overstates loss, by more
Positive selection ($\delta < 0$), worst leave	Understates loss	Understates loss, by more