# A Framework for Assessing the Distributional Effects of School Closures

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**Abstract**

Declining student populations and fiscal pressures are forcing public education systems to consolidate schools. But which schools should governments close, who bears the costs, and could alternative decisions reduce inequalities without sacrificing educational quality? I develop and estimate a framework combining spatial school choice and educational production to address these questions, using Puerto Rico's large-scale school closures during 2013–17. Drawing on linked student-teacher administrative records and geographic data, I distinguish school and teacher contributions to achievement and estimate how families substitute among schools following closures. The results reveal substantial inequalities in geographic access and educational quality. Rural-poor students account for approximately half of modeled displacement but bear 62 percent of total access losses. Moreover, changes in estimated school effectiveness are approximately 0.10 test-score standard deviations less favorable for rural-poor students than for urban non-poor students. Counterfactual analyses reveal important policy trade-offs. Prioritizing closures near surviving schools substantially reduces access losses for rural-poor students without significantly improving their exposure to effective schools. Reallocating displaced teachers yields limited gains, while better information about school effectiveness could improve families' choices. These findings demonstrate why evaluating school consolidation requires considering both the accessibility and educational effectiveness of alternative schools.

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*Keywords* school closures, school consolidation, school value-added, teacher value-added, school choice, distance costs, rural schools, access to schools, Puerto Rico

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# 1 Introduction

Demographic change poses difficult challenges for the provision of public education. Declining birth rates and population movements leave governments facing difficult decisions about how to reorganize school networks, particularly when falling enrollment leads to the underutilization of educational infrastructure and increasing pressure on public budgets (Sunderman and Payne, 2009). School consolidation presents a particularly challenging trade-off: closing underutilized schools can reduce operating costs and create opportunities to reallocate scarce educational resources more effectively (Brummet, 2014, Engberg et al., 2012, Steinberg and MacDonald, 2019). However, these decisions also disrupt students' educational trajectories, alter their access to schools, and change the distribution of educational opportunities across communities (Hanushek et al., 2004, Bobonis et al., 2026). These consequences can be especially important for disadvantaged families, who may have limited access to alternative schools. Understanding how school consolidation affects educational opportunities, and how these effects differ across students and communities, is therefore central to designing policies that balance fiscal considerations with educational effectiveness and equity.

The consequences of school consolidation for student achievement are theoretically ambiguous. School closures can generate substantial disruptions for displaced students and the schools that receive them. Previous research documents adverse academic consequences of school switching (Hanushek et al., 2004) and large inflows of new students into receiving schools (Imberman et al., 2012, Özek, 2021). Closures may also disrupt peer and community networks, with potentially lasting consequences for students and their families (Brummet, 2014, Steinberg and MacDonald, 2019). At the same time, consolidation can provide opportunities for students to attend more effective schools, learn alongside higher-achieving peers, and receive instruction from more effective teachers (Abdulkadiroğlu et al., 2026, Bobonis et al., 2026).[1] Whether students benefit from consolidation therefore depends not only on the disruption associated with displacement but also on the educational opportunities available at the schools they subsequently attend. Moreover, the consequences of consolidation are likely to vary substantially across school systems, depending on their overall performance, the geographic distribution of schools, and the alternatives available to displaced families. Related evidence from school choice policies, including private school vouchers, documents substantial variation in effects across institutional contexts and education systems (Epple et al., 2017).

A growing empirical literature examines the consequences of school closures for student enrollment and

[1] A substantial literature documents the benefits of exposure to higher-achieving peers (Sacerdote, 2011), more effective schools (Angrist et al., 2017), and higher-value-added teachers (Chetty et al., 2014b).

achievement.[2] Although these reduced-form evaluations provide essential evidence on the effects of consolidation policies and can inform future decisions about their design, their findings reflect the particular school closure, the alternatives available to displaced students, and the resulting allocation of educational resources. These settings cannot be readily extrapolated to alternative consolidation policies because changing which schools close also changes families' substitution opportunities, their enrollment decisions, and how teachers are allocated across the remaining schools. Evaluating alternative consolidation policies therefore requires a framework that explicitly accounts for the interactions between school closures, families' school choices, and educational production. This paper develops such a framework to examine how alternative consolidation decisions shape the distribution of educational opportunities across students and communities.

I study these questions in Puerto Rico, where a sharp decline in the school-age population led to the closure of approximately 44 percent of operating public schools between the 2009–10 and 2019–20 academic years (NCES, 2021b). Puerto Rico provides a particularly informative setting for studying school consolidation. Its public education system operates within the institutional framework of the United States but serves a population characterized by substantially lower incomes, widespread poverty, and considerable geographic disparities. Moreover, the large-scale consolidation affected schools in both rural and urban areas, with substantial variation in the distance and educational effectiveness of the alternatives available to displaced students.[3] This makes it a particularly informative setting for examining how the consequences of school consolidation differ across socioeconomic groups and locations and offering lessons for other school systems facing demographic decline and fiscal pressures. The scale of the consolidation – the analysis focuses on 334 public school closures between 2013 and 2017 – along with detailed administrative records linking students, teachers, and schools, offers a rare opportunity to examine how closure consequences depend on the organization of the remaining school network.

I develop and estimate a framework that combines student school choice, which captures substitution across schools in response to closures, with an educational production model. The framework distinguishes the contributions to student achievement associated with individual schools from those of teachers, who can

[2] Estimating these effects presents an important empirical challenge, as schools are often selected for closure based on low academic performance, potentially confounding comparisons between displaced and nondisplaced students (Sunderman and Payne, 2009, Engberg et al., 2012, Steinberg and MacDonald, 2019). Studies addressing these selection concerns generally find relatively small average effects on student achievement, but substantial heterogeneity depending on the relative performance of the schools students leave and enter (Brummet, 2014, Steinberg and MacDonald, 2019).

[3] Caraballo-Cueto (2023) examines Puerto Rico's 2017 and 2018 school closures and finds negative effects on displaced students' academic achievement, particularly in primary schools. In related joint work covering Puerto Rico's closure waves through 2018, Bobonis et al. (2026) find no persistent negative effects on average achievement and document persistent reductions in public school enrollment and heterogeneous achievement effects associated with the performance of closed schools relative to nearby schools and the teacher inputs displaced students encounter. These findings highlight the importance of the educational alternatives available to students following closure.

be reassigned following a closure, and separately measures the accessibility of public school alternatives available to families. I use this framework to address three questions. First, how do realized school closures change the accessibility and educational effectiveness of the public schools available to students, and how are these changes distributed across socioeconomic and geographic groups? Second, could alternative decisions about which schools to close have improved the distribution of educational opportunities? Third, to what extent could reallocating teachers or providing families with information about school effectiveness mitigate the consequences of consolidation? By explicitly modeling families' substitution across schools and distinguishing school-specific from portable educational inputs, the framework allows me to evaluate alternative consolidation decisions and their distributional implications.

The empirical framework has two components. First, I estimate an educational production model that separates school contributions to student achievement (i.e., organizational and institutional inputs, as well as the broad group of peers) from teacher value added. Using longitudinal records linking students to their teachers in mathematics, Spanish, and English, I exploit student and teacher movements across schools to estimate these contributions while accounting for persistent differences across students. This distinction is particularly important in the context of school consolidation. Although teachers from closed schools can potentially be reassigned to other schools, the educational contributions associated with the institutions themselves cannot simply be transferred with them. Separating these inputs allows me to assess how closures change students' exposure to educational effectiveness and to evaluate the potential gains from alternative teacher reassignments.

Second, I estimate a school choice model that characterizes how families substitute among public schools when their existing schools close. I combine observed enrollment shares, displaced students' destination choices, and information on the geographic distribution and socioeconomic characteristics of families to estimate the attractiveness and accessibility of alternative schools. The model allows families to differ in their willingness or ability to travel and incorporates substitution outside the local public school network. Importantly, it distinguishes schools' attractiveness to families from their contributions to student achievement. This separation allows me to measure accessibility losses from removing schools from the network without assuming that families' enrollment decisions necessarily reflect differences in educational effectiveness. Together, the two components provide a framework for evaluating how alternative closure decisions affect both the geographic accessibility and the educational effectiveness of the remaining school options.

The results reveal substantial heterogeneity in the educational inputs associated with different schools. The estimated standard deviation of school contributions to student achievement is approximately 0.30 standard deviations of test scores, considerably larger than the dispersion in schools' average teacher value

added, of 0.075 to 0.089 standard deviations. This difference highlights the importance of school-specific contributions to educational production. Moving students and teachers from one institution to another does not necessarily preserve the educational inputs associated with the school that closes. Consequently, evaluating consolidation policies requires comparing the effectiveness of the schools selected for closure with that of the alternatives available to displaced students, rather than relying exclusively on enrollment, school size, or the characteristics of their teachers.

I find substantial inequalities in the accessibility of public schooling following consolidation. The nearest surviving school is, on average, approximately 3.4 kilometers from closed rural schools, compared with 2 kilometers from closed urban schools. The school choice model indicates that rural-poor students experience accessibility losses that are 84 percent larger per displaced student than those of urban non-poor students, measured in kilometer-equivalent units. Although rural-poor students account for approximately half of predicted displacement, they bear nearly 62 percent of the total modeled accessibility losses. These findings illustrate how decisions about which schools to close can disproportionately affect geographically isolated and disadvantaged communities, even when closures are motivated by system-wide enrollment declines.

These disparities in accessibility are accompanied by differences in the educational effectiveness of the schools students leave and enter. Changes in estimated school contributions to achievement, net of teacher value added, are approximately 0.10 standard deviations less favorable for rural-poor students than for urban non-poor students.[4] In contrast, the corresponding difference in schools' average teacher value added is $+0.006$ standard deviations, with a 95 percent interval from $-0.055$ to 0.067 that excludes a difference as large as the one in school contributions. At the point estimates, the disparities in educational inputs associated with realized closures therefore reflect contributions specific to schools rather than the effectiveness of their teachers. Taken together, the results show that rural-poor students face a disproportionate burden along two distinct dimensions: greater losses in access to public schools and less favorable changes in the educational effectiveness of the schools they attend.

Motivated by these findings, I use the estimated framework to examine whether alternative consolidation decisions could have reduced these disparities. I first compare counterfactual closure rules that vary the criteria used to select schools for closure, while holding the number of closures fixed within municipalities, closure waves, and enrollment-size categories. I then use the school choice model to predict how

[4] Here there is a pronounced gradient across the four groups. Rural-poor students move to schools whose estimated effectiveness—combining the contribution of the school itself and the value added of its teachers—is on average 0.060 standard deviations lower than that of the schools they leave. The corresponding change is essentially zero for rural non-poor students ($-0.005$ standard deviations) and urban poor students (+0.002), while urban non-poor students experience a modest improvement of 0.034 standard deviations. Thus, the difference between rural-poor and urban non-poor students is approximately 0.094 standard deviations.

displaced students would sort among the surviving schools under each alternative policy. Rules that place greater weight on proximity to surviving schools can reduce accessibility losses for rural-poor students. For example, prioritizing closures based on proximity to alternative schools, weighted by enrollment, reduces modeled accessibility losses per displaced rural-poor student by approximately 10 percent relative to the realized closures. A rule that combines low academic performance with the availability of a nearby alternative generates a 6 percent reduction in modeled accessibility losses and a 0.080 standard deviation more favorable change in school contributions for rural-poor students. However, after accounting for model uncertainty and multiple comparisons, the differences in school contributions relative to realized closures are not statistically significant. These findings suggest that alternative closure rules could improve geographic accessibility, while improvements in educational effectiveness remain less certain.

I also examine the trade-off between the number of schools closed and the accessibility losses imposed on disadvantaged communities. Among the 315 closures considered in the counterfactual analysis, keeping open the 47 schools with the most distant surviving alternatives—15 percent of these closures—would avoid approximately 30 percent of the total modeled accessibility losses borne by rural-poor students. This finding illustrates the potential value of incorporating geographic accessibility into consolidation decisions. A relatively small reduction in the number of closures, targeted toward geographically isolated schools, could substantially reduce the accessibility burden on disadvantaged students, although the fiscal implications depend on the operating costs of keeping these schools open.

Finally, I examine whether alternative allocations of teachers and students could improve educational opportunities conditional on the realized school closures. I consider the reassignment of 626 displaced teachers across teaching positions in receiving schools, holding students' observed classroom assignments fixed. Reallocations that restrict teachers to positions within their municipalities generate small and statistically insignificant estimated gains. In contrast, allowing teacher assignments across the island produces an estimated gain of 0.018 standard deviations in test scores per displaced rural-poor student. This exercise provides an optimistic benchmark that abstracts from teacher mobility constraints and institutional assignment rules. I also simulate how students' school choices might change if families placed greater weight on information about the effectiveness of surviving schools. These simulations suggest potential improvements in educational exposure, although their magnitude depends on assumed behavioral responses and receiving-school capacity. Together, these exercises distinguish the consequences of selecting schools for closure from the scope to mitigate those consequences through subsequent reallocations of students and teachers.

This paper contributes to three related literatures. *First*, it contributes to the growing literature on the educational consequences of school closures and consolidations. Previous studies emphasize the heteroge-

neous effects of displacement depending on the performance of origin and destination schools, as well as the disruption of peer and community networks (Brummet, 2014, Steinberg and MacDonald, 2019, Bobonis et al., 2026). In Puerto Rico, Caraballo-Cueto (2023) reports negative achievement effects from the 2017 and 2018 school closures, particularly in primary schools. In Pakistan, Andrabi et al. (2025) find that the public schools closed in a consolidation program had low enrollment but not low school value-added. A related structural literature examines alternative consolidation decisions and the trade-offs faced by policymakers. Epple et al. (2018) model school closure decisions that anticipate parental responses and consider objectives related to school quality, student retention, and demographic composition. More recent work evaluates school closure and capacity replacement policies (Abdulkadiroğlu et al., 2026) and alternative closure criteria incorporating distributional safeguards (Chrzan and Pearman, 2026). I complement this literature by explicitly measuring how alternative consolidation decisions redistribute two distinct dimensions of educational opportunity—public school accessibility and educational effectiveness—across socioeconomic and geographic groups. The analysis also distinguishes changes in closure selection from adjustments in the allocation of students and teachers after schools close.

*Second*, the paper contributes to the literature on educational production and the allocation of educational resources. A growing body of work emphasizes the importance of distinguishing the contributions of schools from those of teachers and other educational inputs. Mansfield (2015) and Jackson (2013) use teacher mobility to separate teacher value added from school-specific factors; where teachers are not observed in more than one school, school value-added includes the average effect of its teachers (Bau and Das, 2020, Andrabi et al., 2025). Bates et al. (2025) combine educational production with teacher and principal choices to evaluate allocation policies. Building on this literature, I distinguish educational inputs that are specific to schools from those that can potentially be transferred through teacher reassignment. This distinction is especially relevant for consolidation policy because closing a school eliminates the institution while leaving some of its educational resources available for reallocation. By incorporating separate measures of school contributions and teacher value added into the evaluation of school consolidation, the paper extends existing frameworks that do not explicitly distinguish these margins of adjustment (Epple et al., 2018).

*Third*, the paper contributes to the broader literature on the spatial provision of public services and the distributional consequences of withdrawing local facilities. Studies of hospital closures and service reorganization use patient choice models to evaluate how removing facilities changes access, costs, and service quality (Capps et al., 2010, Gravelle et al., 2024). Related research on school choice emphasizes the importance of distinguishing schools' attractiveness to families from their effectiveness in producing student achievement (Walters, 2018, Abdulkadiroğlu et al., 2020). I bring these perspectives together to study how

school consolidation changes the geographic accessibility and educational effectiveness of public schooling. The results demonstrate that the consequences of withdrawing local public services depend not only on the characteristics of the facilities removed, but also on the quality and accessibility of the alternatives available to affected populations. More broadly, they highlight why the design of public service networks under demographic decline should account for the distribution of opportunities across communities, rather than focusing exclusively on aggregate utilization or operating costs.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the school consolidation process in Puerto Rico. Section 3 presents the administrative and geographic data, and Section 4 provides descriptive evidence on school closures and displaced students. Sections 5 and 6 introduce the school choice and educational production framework and the estimation strategy. Section 7 presents the empirical findings, while Section 8 evaluates alternative consolidation and resource allocation policies. Section 9 concludes.

# 2 Institutional Background

This section describes the public school system, the school closures implemented from 2013 to 2017, and what followed for students and teachers. One Department decided every closure, and teachers were placed by a process separate from the one that placed students. The analysis measures access within the municipality of the closed school. This is a maintained set of alternatives, not a restriction on every student's observed enrollment.

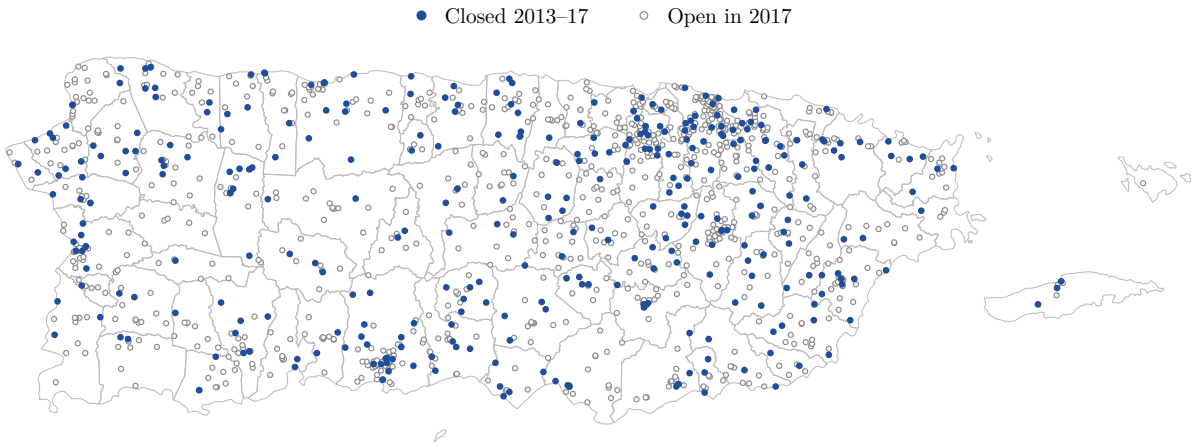
## 2.1 Public Schools and the Closures of 2013-17

The Puerto Rico Department of Education (PRDE) runs every public school on the island. It is both the state education agency and the only local education agency (PRDE, 2017a). I separate students into alternative options across all 78 municipalities in Puerto Rico, which are geographic units without separate school budgets. Therefore, every closure studied here was a decision of PRDE alone.

The public education contracted sharply over the decade. Public enrollment fell by about a third between fall 2010 and fall 2018. This decline in the school-age population led PRDE to close 44 percent of operating public schools between the 2009-10 and 2018-19 academic years(NCES, 2021a,b). Both cover all grades and a longer period than this paper aims to study. Online Appendix Figure A1 shows schools, enrollment, and closures by year in the PRDE's administrative records for grades 1 to 8. The analysis covers 334 school closures in 2013-17, including 156 in 2017 (Section 3.1). Figure 1 maps the schools that closed and those

still open in 2017. A closure of 2017 is a school whose last operating year was the 2016-17 academic year and whose students enrolled elsewhere in 2017-18. Under this setting, 334 schools closed in 2013-17, which are 8, 70, 59, 41 and 156 in the five years, respectively (Section 3.1 gives the rule behind the count). The 2017 closures alone displaced about half of the students studied in this paper, 17,907 of 34,975 (Section 3.3). Figure 1 maps the schools that closed and those still open in 2017.

**Figure 1.** Schools closed in 2013-17 and schools open




*Notes.* Each mark is a public school that serves a grade with a standardized test. Filled marks show the 334 schools closed in 2013-17. A closure is dated by the school's last operating year (8, 70, 59, 41, 156 in 2013 to 2017). Hollow marks show the 935 schools open in 2017. Coordinates are those of the school file, rounded to 0.001 degree, and municipal outlines come from the Census Bureau's cartographic boundary file. Table 1 holds the counts by year. Its 1,274 open schools are those open in any year of 2013-17.

Hurricane María made landfall on September 20, 2017 (National Hurricane Center, 2017). The schools in the 2017 closure cohort last operated in 2016-17. However, their displaced students entered new schools in 2017-18, when the storm damaged schools and interrupted instruction (Nelson et al., 2020). This distinction is important to compare schools' characteristics before closure with students' destinations afterward. The paper does not study the closures that followed the storm. Therefore, I fixed which schools closed, what their status was the year before, and how far each was from the nearest surviving school before the storm. I included school choice for students who enrolled in their new schools in the 2017-18 academic year, when the storm damaged schools and interrupted instruction (Nelson et al., 2020). I do not study the closures that came after the storm.

PRDE issued written closure procedures in July 2013 and revised them in April 2015, November 2015, and March 2017 (PRDE, 2013, 2015a,c, 2017c). The department's criteria for selecting a school to close include enrollment, infrastructure, academic performance, operating costs, and location. The April 2015 procedure required for a list of schools with initial enrollment of 100 or fewer students as one step in a broader evaluation to determine closure policies. The 2015 procedures also required assessing receiving

schools' space, staffing, and transport. The March 2017 circular required consultation before the Secretary's decision. These documents show formal procedures. However, they do not describe how the Department applied each criterion to each closure.

### 2.2 Placement, Transport and Teachers after a Closure

Families can request the destination school alongside administrative placement. A parent notice published in May 2015 named a recommended receiving school but allowed families to request another school offering the appropriate grade, subject to available places. The student's record would be transferred automatically to the recommended school if a family did not return its preregistration form (PRDE, 2015b). The August 2017 enrollment instructions allowed families to request a school and grade. However, it gives school directors authority to approve enrollment and transfer a student to another school. Families could request transport through the enrollment form. Directors have the authority to approve or reject transport after approving enrollment (PRDE, 2017d). These procedures give a role to both family requests and administrative decisions in the destinations observed in the records.

A separate personnel procedure organized teacher reassignment. The circular published in January 2017 called for considering receiving school capacity. It also ordered displaced teachers within job categories by permanent appointment date, experience, and qualifications. However, receiving school incumbents were excluded from that seniority comparison. It also provided for transfers across municipalities (PRDE, 2017b). These are the documented rules for 2017, rather than evidence that the same procedure operated in every earlier wave. The separate processes for students and teachers motivate the distinction between school and teacher value-added in Section 5.

## 3 Data

The paper links the Department's enrollment, student, teacher and school records to population and geographic data. These sources describe the schools removed from the network, the schools displaced students entered, and the choices available to families in the model. Because their populations differ, Online Appendix Table A2 maps the sources to the empirical objects. Online Appendix Table A1 lists the variable constructions.

### 3.1 Department of Education Records

The Department provided four extracts of its administrative records under a data use agreement. I link information at the school level by school code and school year, and link the student records to teacher records by student identifier and school year.

The first extract, the enrollment extract, counts students enrolled in each grade at each public school serving grades 1 to 8, from 2012-13 to 2018-19. It gives the number of students in each grade of a closed school in its last year, each school's size, and the earlier maximum enrollment that Section 8 uses as the number of seats. It holds 5,949 school years for 2013-17. Of these school years, 334 are the last operating years of schools that closed during 2013-17, and 5,615 are years in which the school remained open. These observations cover 1,274 schools.

The second extract is the student records. It follows students enrolled in public schools in each academic year. It records each student's school, grade of enrollment, municipality and ZIP code of residence, the PRDE's family poverty record, and test scores in math, Spanish, and English for grades 3 to 8. The record of family poverty is the PRDE's dummy variable for a family with low income. I use the student's most frequent recorded status across years for missing dummies (Online Appendix A.5). Because the records follow students, they show whether a displaced student entered the next year's public school or left the municipality's public schools.

The third extract, the teacher links, contains 3,082,690 records, each linking a student to their teachers in math, Spanish, and English for a given school year from 2009-10 to 2022-23. I use the time frame until 2018-19 since the records are not available through COVID period. Unique identifiers follow each teacher across subjects and schools. The links show where teachers were employed at closed schools afterward. Section 5 separate what moves with teachers from what stays with a school in terms of what they contribute to the student test scores. Estimation of the production model uses a narrower scored sample. Its source records include current and lagged scores in all three subjects and satisfy the valid class and largest class filters. The production estimate uses this selection, although its equation includes student effects rather than lagged score controls. The input contains almost no grade 3 observations and mainly covers grades 4-8. Applying the estimated teacher value-added to grade 3 students requires portability across these grades. Presence in the broad teacher links does not mean that a teacher has estimated teacher value-added.

The fourth extract is the school file, which describes each school and its characteristics. The records

include the school's coordinates in each year, the grades it serves, the PRDE's rural or urban designation, and its last operating year. 334 schools serving at least one grade with a standardized test closed in 2013-17.[5]

The records lack three things. First, homes are observed only at the ZIP code level. Second, the supplied extracts include no school operating costs, so Section 8 compares closure lists at fixed counts. The separate fiscal illustration for fewer closures is conditional on the cost assumptions in Online Appendix F. Third, there is no transportation record (Section 2.2).

### 3.2 Puerto Rico Community Survey

The Puerto Rico Community Survey is the Census Bureau's American Community Survey for the island, and I use two of its public products. The first gives each block group's school-age population. I call a block group rural when its population density falls below a threshold set to match the Department's rural designation of schools, because the Census urban share equals 1.0 in the median block group and so cannot separate rural from urban areas on the island (Online Appendix A.5). The block groups locate the rural population near each school, which defines the four groups of Section 3.3. They are also the residences of the families in the choice model, linked to schools by routes on the OpenStreetMap road network.

The second product is the Census Bureau's 2013-17 file of individual records for Puerto Rico, released for public use and covering five years, obtained from its public PUMS archive. It records students' enrollment (public school, private school, or none) and their family's income against the Census poverty line. Among students aged 8 to 14 in public schools, 67.1 percent (0.4) are classified as poor by income. Students classified as poor by income are less often outside public schools, with 8.7 percent in a private school or not enrolled compared with 42.6 percent of other students. Income poverty is not the Department's family poverty record, which marks about four in five students in the schools studied here as poor. Section 5.2 uses the two measures for different jobs. The survey is not linked to the student records student by student.

### 3.3 Displaced Students, the Four Groups, and the Two Measures

Displaced students are the students in grades 1 to 7 enrolled in a closed school in its last operating year. The enrollment extract gives 36,109 of them. The analysis uses the 34,975 with rural and poor shares, enrolled in 323 of the 334 closed schools. Their counts are 495, 6,877, 6,073, 3,623 and 17,907 for the closures of

[5] I study 334 (8, 70, 59, 41 and 156 by year) schools, leaving out five schools with a last operating year in the window that served no grade with a test and have no rows in the enrollment extract. The 34,975 displaced students with group shares were enrolled in 323 of the 334. Section 8 chooses 315 of the 334 again under other sets of closures and holds the other 19 closed in every set. These 323 schools are 314 of the 315 and 9 of the 19.

2013 to 2017 (Table 1, Panel A and Online Appendix A.2). Because the counts come from enrollment, they include students who later left the public schools.

**Table 1.** Closures, displaced students, and schools the year before

*Panel A. Closures and displaced students*

| Last operating year | Schools closed (1) | Students displaced (2) | Percent of displaced students: Rural poor (3) | Rural nonpoor (4) | Urban poor (5) | Urban nonpoor (6) |
|---|---|---|---|---|---|---|
| 2013 | 8 | 495 | 39.1 | 16.4 | 27.0 | 17.5 |
| 2014 | 70 | 6,877 | 40.8 | 9.8 | 42.8 | 6.7 |
| 2015 | 59 | 6,073 | 37.4 | 8.0 | 47.0 | 7.6 |
| 2016 | 41 | 3,623 | 47.1 | 9.0 | 39.5 | 4.5 |
| 2017 | 156 | 17,907 | 38.7 | 8.3 | 44.8 | 8.2 |
| 2013-17 | 334 | 34,975 | 39.7 | 8.7 | 44.0 | 7.6 |

*Panel B. Public schools in the year before*

| | Closed 2013-17 (7) | Open (8) | Closed minus open (9) |
|---|---|---|---|
| Enrollment | 165.2 | 290.0 | −124.8 |
| | | | (5.6) |
| Grades offered | 5.19 | 5.36 | −0.17 |
| | | | (0.10) |
| Rural share of the catchment | 0.52 | 0.43 | 0.09 |
| | | | (0.02) |
| Poor share of students | 0.84 | 0.82 | 0.02 |
| | | | (0.01) |
| Km to the nearest other school | 2.67 | 2.61 | 0.06 |
| | | | (0.10) |
| Average test score, SD | −0.007 | 0.037 | −0.044 |
| | | | (0.031) |
| School years | 334 | 5,615 | 5,949 |
| School years with an average test score | 326 | 4,297 | 4,623 |
| Schools | 334 | 1,274 | 1,281 |

*Notes.* A closure is dated by its last operating year. Displaced students are those in grades 1-7 of a closed school in that year. The 34,975 displaced students with group shares were enrolled in 323 of the 334 closed schools. Columns (3)-(6) split a school's students by its catchment's rural share and students' poor share. Panel B gives school means in the year before. Column (7) holds a closed school once and (8) a school in every year of 2013-17 in which it did not close, so 327 of (8)'s 1,274 schools closed later and are also in (7). Column (9) is (7) minus (8), with standard error clustered by municipality. Counts cover each school once. For the closures of 2013, characteristics are those of the last operating year, since no file for 2011-12 exists, and there is no average test score. In (7), the rural share of the catchment exists for 324 schools. Km refers to the nearest other school of the municipality serving a common grade. Average test score is the mean of the school's students.

A second flow from the student records contains 32,514 displaced students, from kindergarten to grade 8, with a recorded next public school. They are not a subset of the 34,975, since the grades and the extracts differ (Online Appendix Table A2). Requiring group information and coverage of school value-added leaves 19,927 records for the destination comparison using school value-added alone. Students absent from public

school records have no observed destination, so these comparisons show those who enroll again in selected public schools in the system.

The choice model represents 28,024 displaced students in 885 affected markets. Rural-poor families account for 50.0 percent of displaced students, compared with 39.7 percent in the total enrollment sample. Online Appendix D.6 reconciles these populations. The difference in their composition is not an additional effect of closure.

The four groups of displaced students are rural-poor, rural non-poor, urban poor, and urban non-poor. Rural is the share of the population near the closed school that lives in rural block groups, weighting each block group by its population and by $\exp(-d/2)$, where $d$ is its distance in km from the school. Poor is the family poverty record. In the enrollment counts, a school's students are split by the school's rural share and by its share of students with the poverty record, so group counts are not whole numbers, and the poor and non-poor students of one school share its distances. The four groups are 39.7, 8.7, 44.0 and 7.6 percent of the 34,975, so rural-poor students number about 13,900. The student records use the same rural share and the student's own poverty record, so the groups keep their names in both flows. Model families instead take their residence block group's rural indicator, while the exit estimates use the origin school's Department designation. Online Appendix Table A3 compares these three constructions of rural.

Access, the first measure, is the distance in km from the closed school to the nearest surviving school in the same municipality serving the student's grade. I multiply the distance along a straight line by the median ratio of road distance to distance along a straight line for the closed school's class, 1.52 for rural and 1.42 for urban schools, for 279,759 routes on the road network. For this adjustment, a school is rural when at least half of its nearby population lives in rural block groups. I also report the share of students with no surviving school within 4.8 km, or three miles. This serves as a benchmark for reporting longer travel distances, so it is not an established transport or policy threshold for Puerto Rico. Both statistics start at the closed school. They do not start at a student's home, so neither changes a student's trip to school. The student records carry their own distance between the closed school and the destination school, whose mean absolute distance difference is 0.47 km from this construction (Online Appendix A.4). Therefore, I only compare distances within one construction.

The second measure, a school's average test score, is the mean score of its students with a test score in grades 3 to 8 in the year before closure. Scores in math, Spanish, and English are normalized within subject, grade, and year to mean zero and standard deviation one, and SD denotes a standard deviation of these scores. Standardization removes differences in score means and scales across subjects, grades, and years. It does not by itself establish comparability across the transition from the Pruebas Puertorriqueñas

de Aprovechamiento Académico (PPAA) to the META-PR test, first given in 2015-16 (PRDE, 2016, p. 4). I measure the average for the same schools used in the distance comparisons, including the school closed, the nearest surviving school, the municipality's surviving schools weighted by enrollment, and the school entered. Its standard deviation across schools is 0.52 SD. Average test score is not available for the schools closed in 2013. An average shows which students attend a school and what the school and its teachers add, which Section 5 separates.

The primary comparisons cover the 2013-17 closure waves and students' movements in the following school year, through 2017-18. I identify and report checks in the earlier frame. The Online Appendix includes tables by year. Another side note is that no displaced student's own test score is an outcome in the closure analysis. Therefore, the paper compares origin and destination schools and their estimated school value-added. Section 4 describes the two raw measures for the four groups.

# 4 Descriptive Evidence

This section describes which schools closed, compares the schools displaced students left and entered a new school, and measures access to surviving schools. It also summarizes the exit estimate used to calibrate the choice model.

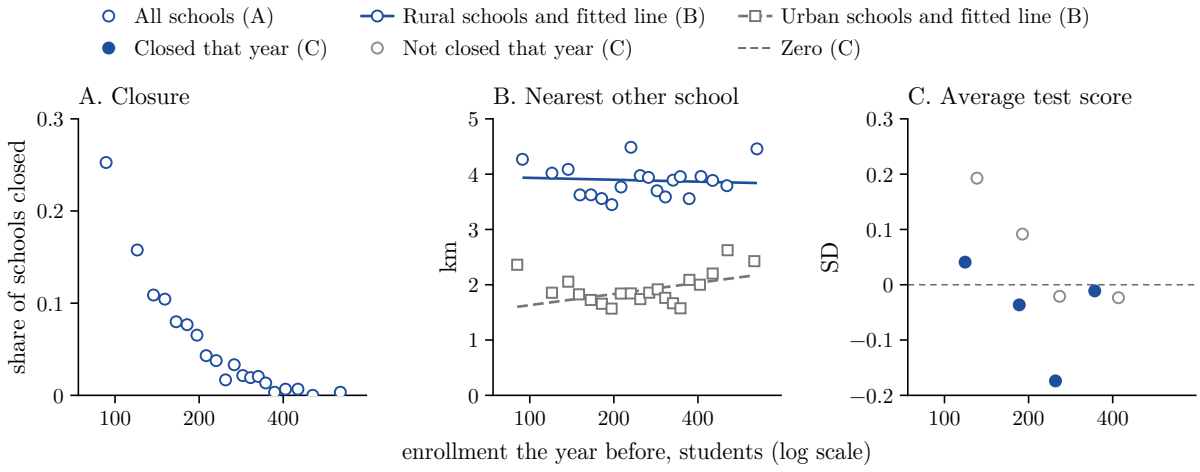
## 4.1 Which Schools Closed

Closed schools were small. In the year before closing, a closed school enrolled 165.2 students on average, against 290.0 in the school years of schools that stayed open, a difference of $-124.8$ (5.6) (Table 1, Panel B).

Closure followed enrollment. In a conditional logit of closure within municipality and year, which also includes the school's distance to its nearest neighbor, its rural and poor shares and its enrollment trend, the coefficient on log enrollment is $-3.02$ (0.22) (Online Appendix Table B1). Figure 2, Panel A shows the pattern without a model. School years are ordered by enrollment into 20 equal bins. The closure rate is 25.3 percent in the smallest bin, against 5.3 percent across all school years.

Closed schools had higher measured rural and poor shares, while their unconditional differences in average scores and distance to the nearest school are imprecisely estimated. The share of the nearby population living in rural block groups was 0.52 around closed schools against 0.43 around open ones, a difference of 0.09 (0.02). The share of students with the family poverty record was 0.84 against 0.82, a difference of 0.02 (0.01). The difference in average test score is $-0.044$ SD (0.031), and the nearest other school of the munic-

**Figure 2.** Enrollment, closure, and the two measures




*Notes.* School years of public schools, 2013-17, by enrollment the year before (5,930 in A, 315 of them of schools closed that year, 5,861 in B and 4,605 with an average test score in C). A point is the mean over a bin of school years. Panels A and B use the same 20 bins of 280 to 313 school years. Panel B gives km (a straight line times a road ratio) to the nearest other school of the municipality serving each of its grades, weighted by the school's enrollment in each grade. Rural means a mostly rural catchment, and a line has the area's slope on log enrollment with effects for closure years, through its mean. Panel C shows schools closed that year (filled) and not closed (hollow), by fifth of enrollment. A fifth with fewer than 10 closed schools is pooled with the fifth below, and scores are in SD. An average reflects a school's students as well as the school. The 19 closures held in every list of Section 8 are not among these school years (5,930 of the 5,949 of Table 1). Online Appendix B.4 reports the slopes by area.

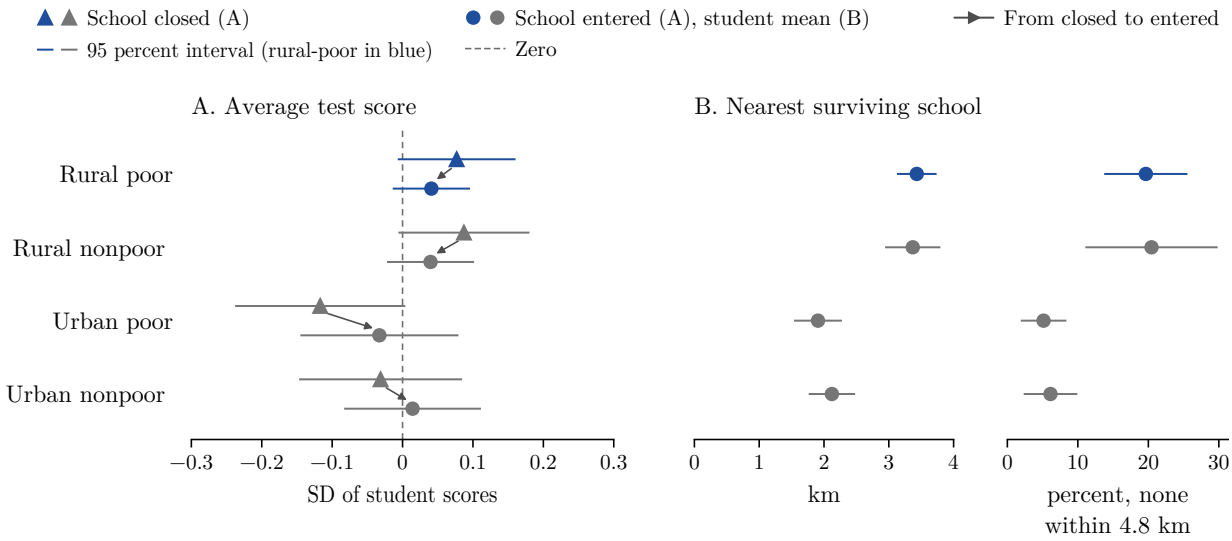
ipality stood 0.06 km (0.10) farther from closed schools. Neither difference can be distinguished from zero. Enrollment carried the clearest pattern. Among schools of similar size, however, the schools that closed had lower average test scores than those that stayed open (Figure 2, Panel C). What each group of students lost depended on where the closed schools stood, both on the map and relative to schools with higher or lower average test scores.

## 4.2 Average Test Scores of the Schools Closed and Entered

Average test scores show a gap between rural-poor students and all other displaced students, mainly reflecting the schools that closed. I compare the average test score of the school a student left with three surviving alternatives. These are the nearest school, the average of the municipality's surviving schools weighted by enrollment, and the school the student entered. Every comparison is the surviving school minus the school closed, so a negative number means a lower average score than the school closed. The rural-poor contrast is the value for rural-poor students minus the value for all other displaced students. Figure 3, Panel A draws, for each group, the average test score of the school closed and of the school entered.

The schools that rural students lost had average scores above the island's mean, and at the point estimate those that urban poor students lost were below it (Table 2, Panel A). Among the 34,191 displaced students

**Figure 3.** Average test scores of closed and entered schools and distance to the nearest surviving school by group




*Notes.* Closures of 2013-17, with groups defined by the student's poverty record and the closed school's rural share. Panel A gives the average test score of the school closed and of the school entered, each the year before the closure, in SD, for the 14,791 student records of reenrolled students with both scores. Averages reflect a school's students as well as the school. Panel B gives km from the closed school (not the home) to the nearest surviving school of the municipality serving the grade (a straight line times a road ratio), and the percent of students with none within 4.8 km (3 miles), for all 34,975 displaced students. Lines give 95 percent intervals, with standard errors clustered on the closed school's municipality. For rural poor minus all other displaced students, the values are 0.155 (0.056) for the closed school, 0.058 (0.045) for the entered school, −0.097 (0.042) for entered minus closed and 1.29 (0.20) km. Table 2 holds the numbers.

with an average test score for the school closed and the nearest surviving school, rural-poor students' closed schools scored 0.153 SD (0.061) above those of all other displaced students. Before any student moved, the municipality's surviving schools scored below the school closed for rural students and above it for urban poor students at the point estimates, a rural-poor contrast of −0.105 (0.036). Against the nearest surviving school alone, the rural-poor contrast is −0.072 (0.053), which cannot be distinguished from zero.

At the point estimates, urban poor students entered schools with higher average scores than the schools they left, while both rural groups entered schools with slightly lower average scores. Neither rural group's change is distinguishable from zero. On the 14,791 records of students who enrolled again with an average test score for both schools, the school entered minus the school closed is −0.036 SD (0.046) for rural-poor students and −0.047 (0.052) for rural non-poor students. For urban poor and urban non-poor students it is 0.084 (0.037) and 0.045 (0.050). The rural-poor contrast of −0.097 SD (0.042) indicates a less favorable change than that of all other displaced students. The rural minus urban difference is −0.117 (0.049). Against urban non-poor students alone, who are 7.6 percent of the displaced, the difference of −0.081 (0.058) cannot be distinguished from zero. Although rural-poor students' own raw change cannot be distinguished from zero, the raw result shows a gap between groups.

**Table 2.** Average test scores and distances for closed, nearest surviving and entered schools

| | Displaced students | | | | | Difference | | |
|---|---|---|---|---|---|---|---|---|
| | All (1) | Rural poor (2) | Rural nonpoor (3) | Urban poor (4) | Urban nonpoor (5) | Rural minus urban (6) | Rural poor minus all other (7) | Rural poor minus urban nonpoor (8) |
| *Panel A. Average test score, SD of student scores* | | | | | | | | |
| Displaced students | | | | | | | | |
| School closed | 0.002 | 0.094 | 0.152 | −0.117 | 0.043 | 0.199 | 0.153 | 0.051 |
| | (0.052) | (0.039) | (0.060) | (0.067) | (0.093) | (0.066) | (0.061) | (0.091) |
| Nearest surviving school | 0.022 | −0.021 | −0.035 | 0.081 | −0.031 | −0.089 | −0.072 | 0.010 |
| minus school closed | (0.043) | (0.051) | (0.069) | (0.048) | (0.092) | (0.059) | (0.053) | (0.096) |
| Municipality's surviving | 0.019 | −0.044 | −0.086 | 0.102 | −0.013 | −0.137 | −0.105 | −0.031 |
| schools minus school closed | (0.033) | (0.035) | (0.053) | (0.037) | (0.054) | (0.039) | (0.036) | (0.059) |
| Displaced, with scores | 34,191 | 13,600 | 2,954 | 15,098 | 2,539 | 34,191 | 34,191 | 16,139 |
| Students who reenrolled in a school of their set of alternatives | | | | | | | | |
| School closed | −0.013 | 0.077 | 0.087 | −0.117 | −0.031 | 0.184 | 0.155 | 0.108 |
| | (0.048) | (0.043) | (0.048) | (0.062) | (0.059) | (0.063) | (0.056) | (0.060) |
| School entered | 0.007 | 0.041 | 0.040 | −0.033 | 0.014 | 0.067 | 0.058 | 0.027 |
| | (0.037) | (0.028) | (0.032) | (0.057) | (0.050) | (0.051) | (0.045) | (0.045) |
| School entered | 0.020 | −0.036 | −0.047 | 0.084 | 0.045 | −0.117 | −0.097 | −0.081 |
| minus school closed | (0.036) | (0.046) | (0.052) | (0.037) | (0.050) | (0.049) | (0.042) | (0.058) |
| Reenrolled, with scores | 14,791 | 6,217 | 1,210 | 6,353 | 1,011 | 14,791 | 14,791 | 7,228 |
| *Panel B. Distance from the closed school, km* | | | | | | | | |
| Displaced students | | | | | | | | |
| To the nearest | 2.66 | 3.43 | 3.37 | 1.91 | 2.12 | 1.48 | 1.29 | 1.31 |
| surviving school | (0.18) | (0.16) | (0.22) | (0.19) | (0.18) | (0.22) | (0.20) | (0.22) |
| Percent with none | 12.3 | 19.6 | 20.4 | 5.1 | 6.1 | 14.5 | 12.2 | 13.5 |
| within 4.8 km | (2.3) | (3.0) | (4.8) | (1.6) | (1.9) | (3.2) | (2.5) | (3.1) |
| Displaced students | 34,975 | 13,900 | 3,051 | 15,379 | 2,646 | 34,975 | 34,975 | 16,545 |
| Students who reenrolled in a school of their set of alternatives | | | | | | | | |
| To the school entered | 3.88 | 4.70 | 4.74 | 2.98 | 3.35 | 1.67 | 1.43 | 1.35 |
| | (0.17) | (0.16) | (0.21) | (0.14) | (0.16) | (0.20) | (0.20) | (0.20) |
| School entered minus | 1.33 | 1.37 | 1.61 | 1.23 | 1.44 | 0.15 | 0.06 | −0.07 |
| nearest surviving school | (0.06) | (0.08) | (0.11) | (0.06) | (0.09) | (0.08) | (0.07) | (0.10) |
| Percent entering the | 41.0 | 44.6 | 37.2 | 38.7 | 37.9 | 4.8 | 6.1 | 6.6 |
| nearest surviving school | (2.2) | (2.3) | (3.6) | (3.0) | (3.2) | (3.3) | (2.8) | (3.6) |
| Reenrolled students | 15,025 | 6,345 | 1,221 | 6,436 | 1,023 | 15,025 | 15,025 | 7,368 |

*Notes.* Closures of 2013-17. Panel A reports a school's average test score the year before the closure, in SD of student scores (none for the closures of 2013). The municipality row gives the mean over the municipality's surviving schools, weighted by enrollment. Reenrolled refers to student records of displaced students found the next year in a surviving school of their set of alternatives (Section 2.2), grouped by the student's own poverty record. Panel B uses distance as in Figure 3. The reenrolled rows use the student records' km between schools, another construction. Groups follow Table 1. N gives rounded sums of shares, which need not add up, and a difference column counts both sides. Standard errors in parentheses are clustered on the closed school's municipality. Those of (1), (6), (7), which pool groups, let the mix of groups vary. There are 34,975 displaced students in 323 closed schools.

The gap sits in the schools that closed more than in the schools entered. On the same 14,791 records, the schools that rural-poor students left scored 0.155 SD (0.056) above those of all other displaced students, and the schools they entered 0.058 (0.045) above. Average test scores also fall with school size. Among open schools, the smallest fifth by enrollment averaged 0.193 SD and the largest fifth −0.040 (Figure 2, Panel C). Low enrollment did not imply low average test scores in these data. These patterns are associations.

### 4.3 Who Lost Access and Why

Rural students faced more distant surviving alternatives. The nearest surviving school is located 3.43 km (0.16) from the closed school for rural-poor students and 3.37 km (0.22) for rural non-poor students, compared with 1.91 km (0.19) and 2.12 km (0.18) for urban poor and urban non-poor students (Figure 3, Panel B and Table 2, Panel B). Rural students' nearest surviving school was 1.48 km (0.22) farther than urban students'. Moreover, the far tail is rural as well. For 19.6 percent of rural-poor and 20.4 percent of rural non-poor students, no surviving school lay within 4.8 km, against 5.1 and 6.1 percent of urban poor and urban non-poor students. The share for rural students is 14.5 percentage points (3.2) higher. Within rural areas, the difference of 0.06 km (0.12) between poor and non-poor students in distance to a surviving school is small and cannot be distinguished from zero. This comparison reflects only how poverty shares differ across schools, since a school's poor and non-poor students share its distance (Section 3.3). The gap in this distance measure based on schools primarily separates rural from urban students. Online Appendix Figure B1 draws the full distributions behind these means.

The rural distance gap is consistent with rural schools lying farther apart. A rural school's nearest other school of the municipality serving its grades was 3.89 km away on average, against 1.93 km for an urban school. The rural mean is larger in each of the 20 enrollment bins of Figure 2, Panel B. Small schools are also more often rural, accounting for about two thirds of the school years in the smallest bin compared with about three in ten in the largest.

Small schools are more isolated across the island, although the relation within a municipality cannot be distinguished from zero. Km to the nearest other school falls with log enrollment with a slope of −0.306 (0.117) given year effects, and of −0.059 (0.124) within a municipality and year (Online Appendix B.4 reports the slopes by area). In the closure logit of Section 4.1, the coefficient on log km to the nearest other school is −0.09 (0.11). Thus no association between distance and closure was detected after controlling for enrollment. That logit measures a school's distance to its nearest neighbor before any closure, considering one school at a time. The random lists of Section 8.2 measure the distance to the nearest surviving school a year after closures, which depends on which neighbor schools close together.

These patterns link the rural access gap to the geography of small schools. As a result, they do not identify the effect of school size, which this paper's design does not vary. Allowing the nearest surviving school to lie in another municipality, on the same 34,975 students, lowers the distance for rural-poor students from 3.43 to 3.10 km and for all others from 2.15 to 1.98 km. The rural-poor minus others gap falls from 1.29 to 1.12 km (0.18), while the rural-poor share beyond 4.8 km falls from 19.6 to 12.9 percent (Online Appendix Table B2). The municipal boundary accounts for part of the distance gap, but the gap remains when alternatives across it are included.

### 4.4 Where Displaced Students Went

Most students who enrolled again in a public school within the choice set did not enter the nearest surviving school. Among 15,025 displaced students in the student records who enrolled again in a surviving school in their set of alternatives, 41.0 percent (2.2) entered the nearest one (Table 2, Panel B). The shares were 44.6, 37.2, 38.7, and 37.9 percent for rural-poor, rural non-poor, urban poor, and urban non-poor students. Of all displaced students who re-enrolled in the choice model's extract of student records, 19.5 percent entered a school outside the set of alternatives, the most common among the closed school's other grades, and 11.9 percent entered another school outside the set. These destinations lie outside the choice model's set of public schools (Online Appendix A.2). On the same 15,025 records, the school entered was 1.33 km (0.06) farther from the closed school than the nearest surviving school, and this extra distance was only 0.15 km (0.08) larger for rural than for urban students. Among these students who enrolled again in a public school within the choice set, the additional distance to the school entered contributed little to the gap between rural and urban students at the point estimates. These distances use the students' own km between schools, so I compare them only with each other.

Some students left the municipality's public schools, and students of rural schools left more often. An event study with the estimator of Sun and Abraham (2021) compares students displaced by the closures of 2013-16, in grades 3 to 7, with matched students whose schools stayed open. In the first year after a closure, the probability of leaving the municipality's public schools rose by 8.5 percentage points (1.5) (Online Appendix A.6). Of this rise, 3.2 points (0.9) are students no longer in the public records and 5.3 points (1.2) are students enrolled in another municipality's public school. The rise was 12.0 points (2.5) for students of schools the Department designates as rural and 3.2 points (1.2) for students of urban schools. Students absent from the public records may have entered a private school, left the island or stopped attending, but the records do not distinguish these possibilities.

These facts motivate the two models that follow. Average test scores show a less favorable change

for rural-poor students than for other displaced students. However, the average mixes students' persistent characteristics with what the school and its teachers add. The score model of Section 5.1 separates student effects, teacher value-added, and school value-added under its maintained assignment assumptions. On the access cost side of the closure margin, distance to the nearest surviving school is not the variable that values the lost option. Most students who enrolled again in a public school within the choice set did not enter the nearest school. Some students also left the municipality's public schools. The choice model uses these enrollment patterns to value the lost public school option under maintained assumptions about placement, transport, and family choice. Students' distance responses and outside options differ across groups. The model's outside option is also calibrated to the estimated exit (Section 5.2).

# 5 A Model of Test Scores and School Choice

This section defines the two margins for any set of closures, realized or not. The score model of Section 5.1 separates a school's contribution to test scores from those of its students and teachers. This procedure defines the school value-added that the second margin compares. The choice model of Section 5.2 describes where displaced families enroll. Consequently, the choice mode prices the option a closure removes and places the students of closures that did not happen. Section 5.3 defines both margins with the two models.

## 5.1 Student Effects, Teacher Value-Added and School Value-Added

Let $i$ index students, $s$ subjects (mathematics, Spanish and English), $t$ school years, $k$ teachers and $j$ schools. Write $k(i,s,t)$ for the student's teacher in subject $s$ and year $t$, and $j(i,t)$ for the school the student attends. The score model is

$$Y_{ist} = a_i + \mu_{k(i,s,t),s} + \phi_{j(i,t)} + e_{ist}, \tag{1}$$

where $Y_{ist}$ is the test score normalized within subject, grade and year. The student effect $a_i$ is common to the three subjects, $\mu_{ks}$ is teacher value-added for teacher $k$ in subject $s$, and $\phi_j$ is the school value-added. A subject intercept would be absorbed by the teachers' value-added, so the equation has none. The error combines a term for a student and subject with shocks common to a classroom in a given year and to a school in a given year, $e_{ist} = \nu_{ist} + \zeta_{c(i,s,t)} + \eta_{j(i,t),t}$. Scores are therefore correlated within a classroom and within a school in a given year. All effects are in SD. School value-added is common to all students, without estimated differences by student type.

The school value-added is the part of a school's contribution to test scores that does not move with its teachers. When a school closes, its teachers are reassigned to other schools and take their value-added with

them (Section 2.2). A displaced student leaves the closed school behind and enters a school with a different school value-added. For school $j$ in year $t$, let $n_{jt,ks}$ count the scored observations for each student and subject taught by teacher $k$ in subject $s$, and let $n_{jt} = \sum_{k,s} n_{jt,ks}$. School-average teacher value-added and school effectiveness are

$$q_{jt} = \sum_{k,s} \frac{n_{jt,ks}}{n_{jt}} \mu_{ks}, \qquad \Psi_{jt} = \phi_j + q_{jt}.$$

As an average exposure measure, school effectiveness combines school value-added with school-average teacher value-added, which weights teacher value-added by the school's scored observations for each student and subject. Students at the same school need not have the same teachers. The teacher reassignment exercise based on classrooms instead follows each displaced student's linked teachers. A teacher's value-added is assumed portable across the schools where the teacher works. Section 7.1 bounds a component specific to a teacher and a school.

The school value-added is constant over the years of the sample. It measures a school's average contribution to test scores over those years, net of the effects of its students and teachers. It bundles inputs that stay with the school, such as its facilities, location and organization, with the school's average class sizes and peers over the sample. Equation (1) does not separate these parts, and class sizes and peers change when a school receives displaced students. I hold the school value-added of the surviving schools fixed under every set of closures, although their intake changes. This is a maintained restriction, so this study does not estimate how a school responds to the students it receives.

Teachers enter the score model so that the school value-added is net of them. Online Appendix E.4 describes a reassignment of displaced teachers with the realized closures held fixed. I do not model teacher reassignment under other closure lists. School value-added and teacher value-added describe performance in the three subjects tested, and effects on test scores need not match effects on other outcomes, for teachers (Jackson, 2018) or for schools (Beuermann et al., 2023).

## 5.2 School Choice after a Closure

The choice model gives the probability that a displaced family enters each surviving school or leaves the municipality's public schools. It has two uses. First, it prices the option a closure removes, the value to a family of the school that closed, in km. Second, Section 8 uses its probabilities to place the students of closures that did not happen, after a check on the realized closures (Section 6.2).

A market $m$ is a municipality, a grade, and a year. Its choice set $\mathscr{J}_m$ contains the public schools of the municipality that serve the grade, the set of alternatives of Section 2.2. The outside option, $j = 0$, pools

private school, not enrolling, and a public school in another municipality. The records observe homes only to ZIP code, so families are placed at census block groups, each carrying the survey's population weights (Section 3.2). For a family $i$ in market $m$, the utility of school $j \in \mathscr{J}_m$ is

$$u_{imj} = \delta_{mj} + (1-\rho)\,\widetilde{\lambda}_i\, d_{ij}^{p} + \pi I_i + \varepsilon_{imj}, \qquad \widetilde{\lambda}_i = \widetilde{\lambda}_0 + \widetilde{\lambda}_R R_i + \widetilde{\lambda}_P P_i, \tag{2}$$

and the utility of the outside option is $u_{im0} = \varepsilon_{im0}$. Here $d_{ij}$ is the routed distance in km from the block group's centroid to school $j$, and the power $p = 0.8$ is fixed before estimation. $R_i$ is a dummy variable that flags a rural block group, $P_i$ is the PRDE's poverty record, and $I_i$ is income poverty as the survey measures it. The school mean utility $\delta_{mj}$, which captures school attractiveness, is common to all families in the market and is recovered from enrollment shares. It holds everything about a school, including school effectiveness, that all families equally value.

The errors follow a nested logit with one nest for the municipality's public schools and the outside option alone (Cardell, 1997, Train, 2009). $0 \le \rho < 1$ is the nest parameter. Therefore. the coefficients $\widetilde{\lambda}$ are in units within the nest, which keeps the distance coefficients comparable as $\rho$ changes. With $\widetilde{\delta}_{mj} = \delta_{mj}/(1-\rho)$, the choice probabilities are

$$r_{ij} = \frac{\exp\left(\widetilde{\delta}_{mj} + \widetilde{\lambda}_i d_{ij}^{p}\right)}{D_i}, \quad D_i = \sum_{l \in \mathscr{J}_m} \exp\left(\widetilde{\delta}_{ml} + \widetilde{\lambda}_i d_{il}^{p}\right), \quad s_{i0} = \frac{1}{1 + e^{\pi I_i} D_i^{1-\rho}}, \quad s_{ij} = (1 - s_{i0})\, r_{ij}.$$

Here $r_{ij}$ is the probability of school $j$ for a family that stays in the municipality's public schools, and $s_{ij}$ is its unconditional probability. I integrate both over block groups and poverty with the survey's weights.

Poverty enters through two measures that do different jobs. The family poverty record shifts the distance coefficient. Income poverty shifts attachment to public schools through $\pi$, the added pull of the municipality's public schools, against the outside option, for families classified as poor by income.

The distance coefficient, $\widetilde{\lambda}_i$, captures effective distance costs. It reflects the cost of travel, including the busing, placement, and information arrangements families faced after a closure (Section 2.2). The data do not separate these factors from each other. Pricing the lost option assumes that enrollment shows what a school is worth to a family under those arrangements. Placing students under other closures assumes that the same arrangements would have operated. A richer specification adds $(b_R R_i + b_P P_i)\Psi_j$ to the utility within the nest, so that rural and poor families may respond to school effectiveness differently from other families. The response common to all families stays in $\delta_{mj}$. This specification sets $b_R = b_P = 0$, a restriction explained in Section 6.2. Online Appendix D reports the richer specification.

### 5.3 The Two Margins for Any Set of Closures

A set of closures names the schools that close in each year. Its displaced students are the students in grades 1 to 7 of a closed school in its last operating year. For the realized closures, they are the 34,975 students of Section 3.3. Each margin is defined per displaced student of the set, by group, and the number of students the set displaces is reported beside it.

Access is measured in two ways. The first needs no model. It is the km from the closed school to the nearest surviving school of the municipality serving the student's grade, and beside it the share of students with no such school within 4.8 km (Section 3.3). The second prices the lost option with the choice model. Let $C_m$ be the closed schools of market $m$ and $r_{iC} = \sum_{j \in C_m} r_{ij}$ the probability that family $i$ attended one of them. Removing $C_m$ lowers the family's expected best utility within the nest by $\log D_i - \log D_i^{(-C_m)} = -\log(1 - r_{iC})$. Dividing by $r_{iC}$ gives the loss per displaced student, $\ell_i$, which I express in km as follows.

$$\ell_i(C_m) = \frac{-\log\left(1 - r_{iC}\right)}{r_{iC}}, \qquad \left|\widetilde{\lambda}_i\right| \left[ \left(d_* + x_i\right)^p - d_*^p \right] = \ell_i(C_m), \qquad d_* = 3.9 \text{ km}. \tag{3}$$

The value $x_i$ in kilometer equivalents is the extra distance, beyond a school $d_*$ away, that would cost the family as much as the lost option. Because distance enters with the power $p$, I solve for $x_i$ instead of dividing $\ell_i$ by the absolute value of the distance coefficient. The kilometer equivalent is a value, not a journey. The value of the family's own match with the closed school does not depend on the map and is included in this measure. Because $\ell_i \geq 1$, every displaced family loses at least one unit of utility within the nest. At the estimates the closed schools' lower school mean utilities offset most of that part, so the loss in kilometer equivalents is mostly its distance part (Online Appendix D.5). This measure values only the options inside the municipality's public schools. Conclusions drawn from school choices can reverse when families' beliefs are inaccurate, as Kapor et al. (2020) show for beliefs about admission chances. It values the lost option under the information families had. Because the specification used here sets $b_R = b_P = 0$, no estimate of school value-added enters the access numbers. I exclude markets in which no public school survives because their loss is not finite. None of the markets under the realized closures is of this kind.

For a displaced student $i$ who left the closed school $o(i)$ and entered school $d(i)$, the change in school value-added is

$$\Delta_i = \phi_{d(i)} - \phi_{o(i)} = \left(\phi_{d(i)} - \bar{\phi}_i\right) - \left(\phi_{o(i)} - \bar{\phi}_i\right), \qquad \bar{\phi}_i = \frac{\sum_{j \in \mathscr{S}_i} E_j \, \phi_j}{\sum_{j \in \mathscr{S}_i} E_j}. \tag{4}$$

Here $\mathscr{S}_i$ is the set of surviving schools of the student's municipality that serve the student's grade, and $E_j$ is their enrollment in that grade before the closure. The first bracket is the term for the school entered and

the second is the term for the school closed. Both compare a school with the average surviving school of the municipality. A negative change means that the school entered compares less favorably with the school closed. This comparison concerns schools rather than a change in any student's test score. Using $\Psi$ counts the closed school's teachers in its last operating year and the entered school's teachers in the year after closure on top of school value-added since $\Psi$ includes school-average teacher value-added. Because the school value-added averages each school's class sizes and peers over the sample, the change compares the two schools as they operated over the sample, not the inputs a displaced student received after the move. A closure changes both, since some classmates move together and the school entered receives new students.

The school entered is recorded only for the realized closures. This is the reason why the paper uses three versions of the change in school value-added. All three have the same sign, which is a school minus the closed school. The convention I use in this study is that the school value-added change is negative when less favorable. The recorded change uses the school a student entered in the student records, for the realized closures (Section 7.2). The change before students sort again uses the average surviving school of the municipality, $\bar{\phi}_i$, minus the school closed. Therefore, it is the closed school term with its sign reversed. This measure needs no destination, so it is defined for any set of closures and changes only through which schools close and which survive. The change placed by the choice model, or placed change, replaces the recorded school entered with the schools the model places each student in, weighted by the model's probabilities. It counts the students in markets the model covers. Section 8.1 states which students each comparison of Section 8 counts.

The two margins stand side by side. Km and SD are never added, weighted against each other, or put in a ratio. The reason is that this study is limited on having price that would convert one into the other. Together, the two definitions are the framework that Section 7 applies to the realized closures and Section 8 to closures that did not happen. Section 6 shows what variation in the closures identifies each model.

# 6 Empirical Strategy

The score model and the choice model draw on different variation from the same closures. Students and teachers who change schools identify the school value-added and the teachers' value-added (Section 6.1). The schools that displaced students enrolled identify the distance aversion. The exit effect of Section 4.4 sets the nest parameter (Section 6.2).

### 6.1 School and Teacher Value-Added

I estimate equation (1) by least squares with the student effects absorbed. A student observed with different teachers compares those teachers, conditional on fixing the student's permanent characteristics. Students who change schools, along with the teachers who teach in more than one school, provide variation for separating school value-added from teachers' value-added, as established in the comparison based on mobility of Abowd et al. (1999) and applied to teachers and schools by Mansfield (2015) and Jackson (2013). Closures add such moves using changes of schools by displaced students and the reassignment of the teachers of a closed school to new schools. Estimation is restricted to the connected set of schools that these moves link. Connectedness alone does not establish separate identification of every contrast in the joint student, teacher and school model. Online Appendix C.2 checks the targets' rank condition using the full sample design and finds five additional null directions involving 17 teacher value-added terms for combinations of teacher and subject. These leave the school value-added contrasts and the school-average teacher value-added contrasts by school and year for the closures of 2013-17 unchanged. The check does not cover the designs for the two halves. School value-added is estimated for 97.3 percent of the 334 closed schools.

**Assumption 1** (Assignment and shocks to test scores)**.** *Let $\mathscr{A}$ collect the assignments of students to teachers and schools in every year of the sample. Conditional on the student effects, teacher value-added, and school value-added, the error in equation* (1) *has mean zero as follows.*

$$\mathbb{E}\left[e_{ist} \mid \mathscr{A}, a, \mu, \phi\right] = 0 \quad \textit{for every observed } (i,s,t).$$

Assumption 1 allows sorting on the permanent effects. A Department that closes schools with persistently low scores, or families who choose schools with large school value-added, do not bias the estimates. The assumption excludes moves and closures that respond to transitory shocks, a central concern in value-added estimation (Rothstein, 2010). Families' sorting on student-specific gains would also violate it; joint models of school choice and achievement allow such selection (Walters, 2018, Abdulkadiroğlu et al., 2020), and the forecast tests below check for it. Although a closure removes the option to stay, it does not assign a student at random to a school or a teacher.

Three threats bear on the comparison of the school closed with the school entered, and each has its own version of the estimates. First, the Department may have closed schools after a bad year, which would put a transitory shock into the closed school's value-added. I check the closing schools' residuals in the years before closure (Section 7.1). I also estimate the school value-added again for each year of closures without its last operating year and the year before, the years surrounding closure decisions. Second, displaced

students' own later test scores help estimate the effects of both the school they left and the school they entered, so a shock common to those students would enter both sides of the change. The version without displaced students' records leaves them out from the year of the move on. Third, closed schools are small, and their effects carry more noise. I estimate every effect on two halves of the school years, split so that no school year shock enters both halves, and use the covariance across halves. Estimates from the full sample use all school years.

Noise also widens the spread of estimated effects. I correct the variances using the estimator of Kline et al. (2020) that leaves observations out, adapted to shocks shared within a classroom and year or a school and year. The variances of those shocks are calibrated inputs, and Online Appendix C.3 reports how the corrected variances move with them. As a further check, I replace the student effects with lagged test scores, which imposes a different assumption on assignment (Chetty et al., 2014a, Bau and Das, 2020). On grades 4 to 8, the estimates of teachers' value-added from the two specifications correlate at 0.849 (Online Appendix C.5).

I test the assumption out of sample on displaced students, following the forecast tests of Chetty et al. (2014a) and Angrist et al. (2017) and their use with school switchers and closures by Bau and Das (2020) and Andrabi et al. (2025). The effects are estimated separately on two halves of the school years, after removing displaced students' records from the year of their move on. For each student, the half that contains neither the closed school's last operating year nor the entered school's first year provides the instrument for the other half's estimate, which removes the attenuation from estimation noise. Unbiased estimates give a forecast coefficient of one. For the change in school value-added across the move, the coefficient is 0.89 (0.12), with a 95 percent interval of 0.64 to 1.25 from a sign-flip test over municipalities, and the change in scores in the year before the move gives a placebo coefficient of 0.20 (0.21) (Online Appendix Table C4). Because families chose the school entered, I also instrument the change with the one implied by the closure alone, the enrollment-weighted average of the municipality's surviving schools minus the closed school, which gives 0.66 (0.18), with an interval of 0.16 to 1.15. For the change in school effectiveness the coefficient is 0.72 (0.09), below one. The value-added of a student's own teachers forecasts the change in that student's score in each subject with a coefficient of 1.32 (0.46), with an interval of 0.55 to 2.68. These tests concern the inputs a student receives, not their division between the school and its teachers, which rests on the moves of students and teachers that identify equation (1). All school value-added reported in the paper is estimated within the sample and labeled by version.

## 6.2 Identification and Checks for the Choice Model

Enrollment shares alone cannot separate the distance coefficient from a school's mean utility because the school mean utilities $\delta_{mj}$ can be chosen to match the shares for any value of the distance coefficient (Berry, 1994). What separates them is where displaced students went. Given the school mean utilities, the distance coefficient sets how fast the share of displaced students entering a surviving school falls with that school's distance rank. The moments describe where the 15,025 displaced students enrolled again. They measure shares entering the second, third, fourth plus fifth, and sixth or farther surviving school, ranked by distance from the closed school. They are computed in each of the four groups, and the share entering the nearest school is implied. This uses the information in substitution choices emphasized by Berry et al. (2004), observed here after an actual closure. The model's families are the block group populations in 3,023 markets, each a municipality, grade and year. Of these markets, 805 contain displaced students whose next school is recorded, and 885 have a closed school among the choices. The rural difference in the distance coefficient also rests on where rural families live across the catchments of closed schools, since the records give the rural share of a closed school's surroundings, not each student's home.

Attachment to public schools, $\pi$, is identified by the share of students classified as poor by income among students in public schools in the survey, 67.1 percent (0.4). A single moment sets the nest parameter by requiring the model's exit when closed schools leave the choice set to equal the exit effect of Section 4.4, 8.5 points in the year after a closure. The nest parameter is therefore just identified, and matching exit is not a test of the model. The exit effect is measured in grades 3 to 7 and on the closures of 2013-16, and it is applied to displaced students in grades 1 to 7 and to the closures of 2017, whose next school year followed Hurricane María. Adding the closures of 2017 gives an effect of 7.6 points (Online Appendix A.6).

**Assumption 2** (Stable rankings)**.** *Conditional on the school a family attended and on its characteristics in the choice model, a closure does not change how the family ranks the surviving schools and the outside option. A displaced family enters its best remaining option under the same draws of $\varepsilon$ that made it choose the closed school.*

Assumption 2 lets the Department choose closures on enrollment and other school attributes. It rules out closures that change information, transport, or placement in ways the model does not represent. That matters most for who chose the receiving school, which is documented only for the closures of 2015 and 2017 (Section 2). If the Department in effect assigned students, the rank shares describe placement, and the distance coefficient cannot be interpreted as a family's own distance cost. The records show more concentration than the model. For each closed school and grade, take the surviving school that the closed

school's other grades in the same grade span entered most in excess of the model. That school received 46 percent of the students who enrolled again for whom it is defined, against 21 percent in the model, with the excess appearing in all four groups (Online Appendix D.2). Possible explanations include assignment to a receiving school, bus routes, siblings in other grades, or errors in the model's predicted residential locations and in the distance cost. The existing records cannot separate these explanations. This is why Section 5.2 interprets the distance coefficient as component that captures effective distance costs.

The records say little about whether poor or rural families respond to school effectiveness differently from other families. Among displaced students, the school effectiveness of the school entered differs between poor and non-poor students by $-0.008$ SD (0.022) in urban areas and $+0.013$ (0.016) in rural areas. The rural difference in the response to school effectiveness cannot be separated from the rural difference in the distance coefficient in the proposed choice model. I restrict both differences to zero, $b_R = b_P = 0$, and the response common to all families remains in the school mean utilities. I chose this restriction after seeing the results. In the richer specification, the poor difference is $-0.063$ (0.398), and the confidence set for the rural difference has no lower end over the range searched. A test that both differences are zero gives $p = 0.59$ (Online Appendix D.3). Under the restriction, differences between groups in where the model places students come from their locations, distance costs, attachment to public schools and the school mean utilities, rather than different responses to school effectiveness.

I estimate the model by sequential generalized method of moments (GMM) (Newey, 1984). Given the nest parameter, four parameters (the three distance coefficients and attachment) minimize a weighted distance between 17 moments and their targets, comprising the 16 rank shares and the share classified as poor by income. The nest parameter then solves the exit equation, and its confidence set comes from inverting that equation. The standard errors stack the two steps and use the full derivative, since the limiting distribution of GMM changes when the moments are not matched exactly (Hall and Inoue, 2003). I check the fit using two untargeted quantities, the km from the closed school to the school entered and exit by the class of the closed school (Section 7.3). Online Appendix D.1 gives the moments, the weights and the covariance.

Section 8 uses the model for its second job, placing the students of closures that did not happen, and I check that use on the realized closures first. The model's error is the school value-added, or the school effectiveness, of the school a student entered in the records minus that of the school the model places the student in. On the realized closures it is $-0.041$ SD (0.024) for rural-poor students and $-0.026$ (0.020) for all displaced students. At the point estimates, the model places students in schools with larger school value-added than those they entered (Online Appendix Table D3, Panel A). I regress this error on the closed

school's size band, its distance to the nearest surviving school and the number of surviving schools, and I apply the regression to the closures that each set in Section 8 would make instead. For all displaced students and for rural-poor students, these features predict a change in the error of at most 0.007 SD. Changes in the error through other features are not measured. A placed contrast is read as a difference in where students go only if its size, less a tolerance for the model's error, is distinguishable from zero given its standard error (Online Appendix E.5). The tolerance is specific to each set and at least 0.02 SD (Online Appendix Table D3). It also covers the change predicted when the closed school's average test score and its enrollment are added to the features. This is a sensitivity allowance, not an estimated bound on all sources of counterfactual error. The policy intervals and simultaneous bound are conditional on the allowance covering the true differential model error for each list, as well as on the stated uncertainty calculation.

The check has four limits. First, the check and its thresholds were set after the model's error on the realized closures was known. Second, the comparison uses the displaced students whose choices the choice model was estimated on, although the school value-added of the schools they entered was not used in that estimation. Third, the schools entered after the closures of 2017 are those of 2017-18, the school year the storm disrupted (Section 2.1). Fourth, the standard errors hold the estimates of the choice model fixed. Section 7 reports what both models estimate under these assumptions.

# 7 Results

The estimates match both the raw facts of Section 4 and locate the gap in school value-added in the schools that closed. This section reports school value-added and teachers' value-added (Section 7.1) and the change in school value-added and where its gap between groups sits (Section 7.2). It then presents the choice model and the access loss it prices (Section 7.3) before considering the two margins together (Section 7.4).

## 7.1 School Value-Added and Teacher Value-Added

Schools differ more than their teaching staff. Measured by the covariance of estimates on two halves of the school years, the SD of school value-added is 0.303 (0.017), against 0.075 (0.032) for school-average teacher value-added (Table 3, Panel A). Individual teachers' value-added has an SD of 0.203 (0.008), although most of that spread cancels when averaged over a school's teachers. With the correction that leaves observations out instead, the SDs are 0.361 for school value-added and 0.089 for school-average teacher value-added. The estimates use school years 2011-19 and comprise 3,452,029 observations at the student, subject and year

level, covering 395,030 students in 1,274 schools, with 19,484 teacher value-added terms for combinations of teacher and subject.

**Table 3.** School value-added and teacher value-added

| | Across schools | | Across teacher subject pairs |
|---|---|---|---|
| | School value-added | School-average teacher value-added | Teacher value-added |
| *Panel A. Standard deviation, in test score SD* | | | |
| Across halves of the school years | 0.303 | 0.075 | 0.203 |
| | (0.017) | (0.032) | (0.008) |
| without displaced students | 0.311 | 0.078 | 0.198 |
| Corrected for noise | 0.361 | 0.089 | 0.225 |
| *Panel B. Reliability across halves of the school years* | | | |
| First split | 0.565 | 0.087 | 0.29 |
| Second split | 0.561 | 0.059 | 0.24 |
| Without displaced students | 0.549 | 0.084 | 0.26 |
| Observations (student, subject, year) | | 3,452,029 | |
| Students | | 395,030 | |
| Units (schools, teacher subject pairs) | 1,274 | 1,274 | 19,484 |

*Notes.* Test scores of school years 2011-19, with full sample estimates of the model with student fixed effects, teacher value-added by subject and school value-added. School-average teacher value-added is the mean over a school's teachers. The estimates across halves use the covariance of estimates on two halves of the school years, with standard errors in parentheses from a bootstrap over schools. None is computed for the other rows. The two splits are two random divisions of the school years, and Panel A uses the first. The version without displaced students leaves out their records after the closure. The correction removes estimation noise. This row excludes 17 teacher value-added coefficients by subject in five unidentified components, using the remaining 19,467 coefficients for the teacher variance and renormalizing each school's teacher value-added weights over the remaining teachers. All 1,274 schools remain. The exclusion changes the corrected SDs of teacher value-added, school-average teacher value-added and school effectiveness by +0.00010, +0.00001 and +0.00003. School value-added is unaffected and the rounded values are unchanged. The rows across halves and reliability estimates were not recomputed by this check. Ranges across noise inputs before exclusion are 0.34 to 0.38, 0.00 to 0.15 and 0.14 to 0.28 by column. Online Appendix C.2 gives the scope of the full sample rank and ridge checks. Reliability is Spearman-Brown reliability of the full sample estimate. School value-added estimates correlate 0.803 across periods and 0.78 across the two tests. Drift specific to teachers and a component for the interaction of teacher and school in value-added are each at most 0.10 SD.

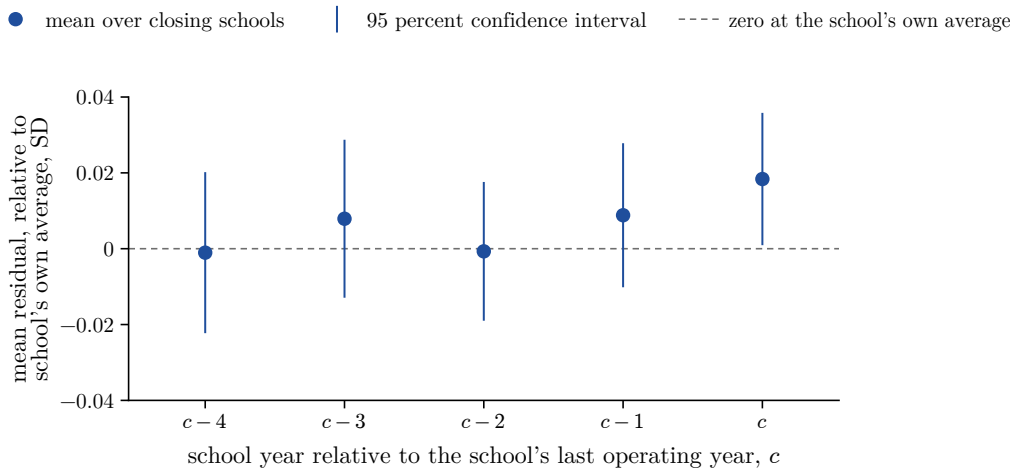
Rankings of school value-added are more reproducible across splits than rankings based on school-average teacher value-added. Across two random splits of the school years, the reliability of the school value-added is 0.565 and 0.561, against 0.087 and 0.059 for school-average teacher value-added (Table 3, Panel B). Without displaced students' records from the year of their move onward, the components are similar, with an SD of school value-added across halves of 0.311 and reliability of 0.549. Section 7.2 reports the change in school value-added in the same version.

School value-added estimates are correlated across periods and tests. School value-added estimates from different periods correlate at 0.803, and those estimated on the two tests the Department used correlate at

0.78. A teacher's value-added drifts by at most about 0.10 SD over the sample. A component specific to a teacher and a school has an SD of at most 0.10, with a point estimate of zero, a range that includes the match effects estimated for teachers who change schools voluntarily (Jackson, 2013) (Table 3, notes).

The pooled test before closure does not detect a fall in residual test scores. Figure 4 plots, for the closures of 2013-17, the closing schools' mean residual test score relative to each school's own average, from four years before the last operating year to that year. A closure that followed a bad year would show as negative residuals in the last years. The joint test that the residuals of the last three years are zero gives $p = 0.16$, compared with $p = 0.38$ when measured against the municipality's schools that never closed in the same years. Online Appendix Figure C1 reports checks across closure periods, since the pooled test does not rule out differences between them.

**Figure 4.** Checks of school value-added using test score residuals before closure




*Notes.* Schools closed in 2013-17, by last operating year $c$ (245 to 316 schools a point). Dots give the mean over schools of the school year's mean test score residual less the school's own average over its years. Lines give 95 percent intervals, with standard errors clustered by municipality. Residuals are from the score model (student fixed effects, teacher value-added by subject and school value-added), full sample estimates. They average zero within a school, so the figure shows timing, not a level. The joint test that the means at $c-2$, $c-1$ and $c$ are zero gives $p = 0.16$, or $p = 0.38$ against the municipality's schools that never closed in the same year. Figure C1 gives the closure periods separately. The estimates of the score model are in Table 3.

## 7.2 The Change in School Value-Added and Where the Gap Sits

The change was least favorable for rural-poor students. With the estimates from the full sample, the school entered minus the school closed is −0.079 SD (0.036) for rural-poor students and −0.032 (0.035) for rural non-poor students. It is +0.011 (0.027) and +0.018 (0.025) for urban poor and urban non-poor students (Table 4). Against all other displaced students, the rural-poor contrast is −0.084 (0.031). Against urban non-

poor students it is −0.097 (0.037), and rural students' change differs from urban students' by −0.082 (0.036). In an additional comparison within rural areas, the change was 0.047 SD (0.020) less favorable for poor than for non-poor students, while the distance difference between the two rural groups was not distinguishable from zero (Section 4.3). Including school-average teacher value-added, the change in school effectiveness gives a rural-poor contrast against all other displaced students of −0.065 (0.030) on the 19,164 records that have both.

These comparisons need the school a student entered, which only the student records give. Of the 32,514 students displaced in 2013-17 with a recorded next public school, 20,422 have their own poverty record and a rural share for the closed school. Of these, 19,982 have school value-added estimates at both schools, and 19,927 have a municipality average to compare with (Online Appendix Table A2). The groups follow each student's own poverty record and the closed school's rural share, and the records leave out students who exited the public schools, more often after rural closures (Section 4.4).

On the common sample of 19,164 records, I separate the change in school value-added from the change in school-average teacher value-added. Relative to urban non-poor students, the rural-poor change is −0.101 SD (0.038) in school value-added and +0.006 SD (0.031, wild cluster $p = 0.85$) in school-average teacher value-added, giving −0.094 SD (0.036) in school effectiveness. Out of sample, differences in school effectiveness forecast about 70 percent of the corresponding differences in score changes (Section 6.1). The components add before rounding. At the point estimates, the less favorable change in school effectiveness reflects school value-added. The teacher contrast is imprecise and does not establish equality. The contrast based on school value-added alone of −0.097 above uses the larger sample, so it cannot be combined with this school-average teacher value-added contrast for the common sample. These comparisons describe inputs associated with the schools students left and entered, rather than causal effects of closure on their own achievement.[6]

The two versions that address the threats of Section 6.1 leave the gap where it is. Without displaced students' records, the contrast is −0.097 (0.041) on 14,525 of the 19,927 records. Without the years sur-

[6] A completed sensitivity calculation for production estimation adds variance from the first estimation stage to the variance clustered by municipality in quadrature using $\sqrt{\mathrm{SE}_{\mathrm{CR1}}^2 + \mathrm{SE}_{\mathrm{FS}}^2}$. It uses the recorded production noise model, holding the sample, exposure weights and teacher rosters fixed. Table 4 reports these sensitivity SEs in brackets. Under a normal approximation, the rural-poor contrasts against all other and urban non-poor students have $p = 0.0195$ and 0.0268 for school value-added, and $p = 0.0432$ and 0.0194 for school effectiveness. All four remain below 0.05 under this calculation. The rural-poor level of the school value-added has $p = 0.0504$, slightly above 5 percent. These are p values under a normal approximation, separate from the reported p values from wild cluster inference. The sensitivity for school value-added uses the larger sample for school value-added alone and was not computed for the common sample's school value-added contrast, −0.101. The calculation does not estimate covariance between sampling and production estimation or establish unconditional coverage or a guaranteed upper bound.

**Table 4.** The change in school value-added, by group

| | Displaced students | | | | | Difference | | |
|---|---|---|---|---|---|---|---|---|
| | All (1) | Rural poor (2) | Rural nonpoor (3) | Urban poor (4) | Urban nonpoor (5) | Rural minus urban (6) | Rural poor minus all other (7) | Rural poor minus urban nonpoor (8) |
| School value-added relative to the municipality's average surviving school, SD | | | | | | | | |
| School closed | −0.017 | 0.045 | −0.013 | −0.066 | −0.077 | 0.102 | 0.104 | 0.122 |
| | (0.027) | (0.035) | (0.038) | (0.031) | (0.028) | (0.037) | (0.031) | (0.036) |
| School entered | −0.046 | −0.034 | −0.045 | −0.056 | −0.059 | 0.020 | 0.020 | 0.025 |
| | (0.015) | (0.020) | (0.026) | (0.016) | (0.016) | (0.020) | (0.016) | (0.021) |
| Change, school entered minus school closed, SD | | | | | | | | |
| School value-added | −0.029 | −0.079 | −0.032 | 0.011 | 0.018 | −0.082 | −0.084 | −0.097 |
| | (0.026) | (0.036) | (0.035) | (0.027) | (0.025) | (0.036) | (0.031) | (0.037) |
| Sensitivity SE | - | [0.040] | [0.042] | [0.033] | [0.033] | - | [0.036] | [0.044] |
| School effectiveness | −0.021 | −0.060 | −0.005 | 0.002 | 0.034 | −0.057 | −0.065 | −0.094 |
| | (0.026) | (0.032) | (0.029) | (0.030) | (0.031) | (0.034) | (0.030) | (0.036) |
| Sensitivity SE | - | [0.035] | [0.035] | [0.033] | [0.035] | - | [0.032] | [0.040] |
| Raw measures on these records | | | | | | | | |
| Average test score, | 0.003 | −0.057 | −0.119 | 0.079 | 0.033 | −0.140 | −0.100 | −0.090 |
| entered minus closed, SD | (0.037) | (0.038) | (0.068) | (0.040) | (0.073) | (0.047) | (0.044) | (0.073) |
| Nearest surviving school, | 2.73 | 3.52 | 3.49 | 1.93 | 2.13 | 1.55 | 1.32 | 1.38 |
| km from the closed school | (0.19) | (0.18) | (0.23) | (0.19) | (0.19) | (0.23) | (0.21) | (0.24) |
| Student records | 19,927 | 8,071 | 1,825 | 8,487 | 1,544 | 19,927 | 19,927 | 9,615 |

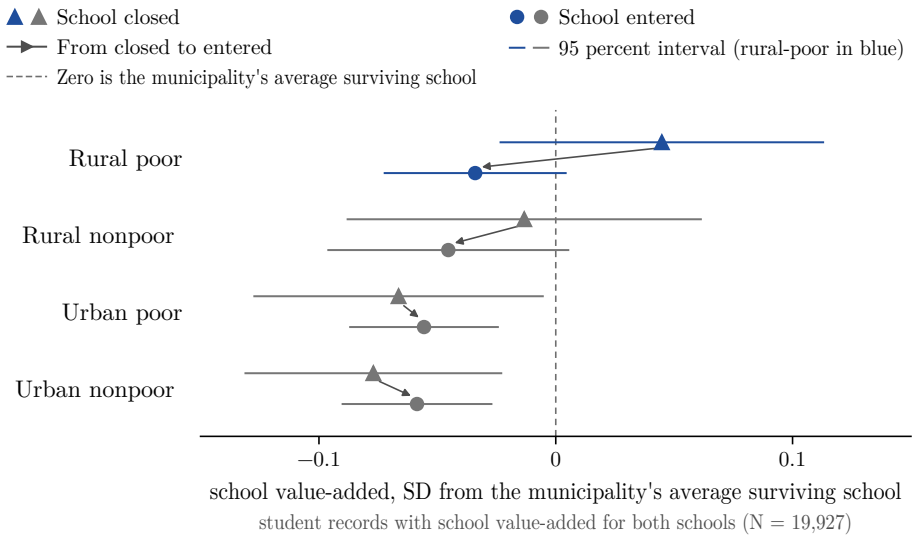
*Notes.* Records of students displaced by closures of 2013-17 (32,514 with a next public school, 19,927 with school value-added for both schools and surviving schools to average, as in Table A2). The first two rows give each school's value-added less the municipality's mean school value-added among surviving schools, weighted by enrollment. Change means entered minus closed, with a negative value less favorable. School effectiveness adds the school-average teacher value-added (19,164 records). Raw measures on these records comprise average test scores (18,671 records) and km (19,925). These are the full sample estimates of Table 3. Online Appendix C gives the versions without displaced students and without each closure wave's last operating year and the year before. N gives group sums of weights, and a difference column counts both sides. Standard errors in parentheses are clustered by municipality. Those of (1), (6), (7), which pool groups, let the mix of groups vary. Brackets on the two change rows give an uncertainty sensitivity SE, $\sqrt{\text{SE}_{\text{CR1}}^2 + \text{SE}_{\text{FS}}^2}$, adding first stage production variance under the recorded noise model with the sample, exposure weights and teacher rosters fixed. Dashes denote uncomputed cells. This calculation does not estimate covariance between sampling and production estimation or establish unconditional coverage or a guaranteed upper bound. Rural poor minus rural nonpoor in the change in school value-added is −0.047 (0.020).

rounding closure decisions, it is −0.086 (0.042), against −0.083 (0.029) with the estimates from the full sample on the same 19,308 records. The level is less stable than the gap. Without those years, the change for all displaced students is −0.079 (0.033), against −0.021 (0.023) with them, while the contrast barely moves. That is why the paper's object is the gap and not the rural-poor level (Online Appendix C.8). The raw counterpart of the gap agrees. On 18,671 of the same records, comparing the average test score of the school entered with that of the school closed gives a rural-poor contrast of −0.100 (0.044) against all other students. Against urban non-poor students, the smallest group, the raw contrast is −0.090 (0.073) and cannot be distinguished from zero.

The gap mainly reflects the schools that closed. Relative to the average surviving school of the munic-

ipality, the schools that rural-poor students left stood at +0.045 SD (0.035), and those of rural non-poor students at −0.013 (0.038). The schools of urban poor and urban non-poor students stood at −0.066 (0.031) and −0.077 (0.028) (Figure 5). Every group entered schools that stood below the average surviving school, by 0.034 to 0.059 SD at the point estimates. The rural-poor contrast is thus 0.104 (0.031) in the term for the school closed and 0.020 (0.016) in the term for the school entered, and the change subtracts the first from the second.

**Figure 5.** The closed and entered schools behind the gap by group




*Notes.* Closures of 2013-17, using the 19,927 student records of Table 4, grouped by the student's own poverty record and the closed school's rural share. Each school's estimated school value-added (full sample estimates, Table 3) is shown relative to the municipality's mean school value-added among surviving schools, weighted by enrollment, shown by the dashed line. The triangle marks the school closed and the circle the school entered, with the arrow joining them. Lines give 95 percent confidence intervals, with standard errors clustered on the municipality of the closed school. Rural poor minus all other displaced students is 0.104 (0.031) for the school closed and 0.020 (0.016) for the school entered. Table 4 holds the numbers.

School size goes with about a fifth of the gap against urban non-poor students. Within a municipality, smaller schools have larger estimated school value-added. The gradient is −0.114 (0.038) per log point of enrollment, estimated jointly with grade span and catchment breadth across 1,257 schools. I multiply this gradient by the change in log enrollment from the school closed to the school entered. The product differs between rural-poor and urban non-poor students by −0.021 (0.008), against a gap of −0.098 in the change itself on those records. The gradient is an association (Section 5.1). So I do not attribute the rest of the gap to school size or to any other characteristic.

### 7.3 The Choice Model and the Access Loss

Rural families have lower estimated effective distance costs than urban families. On the other hand, poor families have slightly higher effective distance costs compared to non-poor students. These coefficients need not measure physical travel costs alone. The coefficient within the nest on km to the power 0.8 is $-2.785$ (0.577) for urban non-poor families. The rural difference is $+1.219$ (0.637), and the poor difference is $-0.202$ (0.094) (Table 5, Panel A). Among non-poor families, the rural coefficient is 0.56 (0.15) of the urban one. The ratio's interval excludes one, although the 95 percent interval of the rural difference itself includes zero. In urban areas, the poor coefficient is 1.07 (0.04) of the non-poor one. Attachment to public schools for families classified as poor by income is 1.411 (0.062). The nest parameter is 0.847 (0.031), with a confidence set from 0.783 to 0.904.

The model comes close on how far from the closed school displaced students enrolled, but not on which school they entered. The model's mean km from the closed school to the school entered, which is not targeted, lies between 0.08 to 0.34 km below the data across the four groups. The data and model are based on the same 13,058 records and on the construction in Section 3.3, not on the students' own km in Table 2 (Table 5, Panel B). The share entering the nearest surviving school is overpredicted in all four groups by 5.2 and 9.7 points for rural-poor and rural non-poor students and by 3.5 and 2.5 points for urban poor and urban non-poor students. The share entering the second nearest school is underpredicted.

The test of overidentifying restrictions of the 17 moments gives $p = 0.038$. One possible reason is that the covariance of the targets is treated as known. Also, two of the 16 rank moments, the second nearest shares of the two rural groups, lie more than two of the displayed standard errors from their targets (Online Appendix Table D1). Exit is also misplaced by origin. After rural closures, the exit is 12.0 (2.5) points in the data against 9.2 in the model. However, after urban closures, it is 3.2 (1.2) against 7.9. That is why the paper uses the model's exit only in the aggregate.

Priced by the choice model, the access loss is again a rural loss. Per displaced student, it is 1.84 (0.11) kilometer equivalents for rural-poor students and 1.91 (0.15) for rural non-poor students. It is 0.97 (0.08) for urban poor and 1.00 (0.09) for urban non-poor students (Table 6). These figures cover the 28,024 displaced students of the 885 markets in which a closed school was among the choices. The model types families by their block group's rural indicator, not by the closed school's rural share, so its four groups do not coincide with those of the km measure (Online Appendix D.6). Their standard errors come from the delta method in the five coefficients, clustered by municipality, and hold the inputs listed in the table's notes fixed.

The model also gives rural-poor students a larger access loss in kilometer equivalents. The loss of rural-

**Table 5.** Choice model estimates and fit

*Panel A. Estimates*

| | Estimate | Standard error |
|---|---|---|
| Distance coefficient within the nest, $km^{0.8}$ | | |
| Urban nonpoor family | −2.785 | 0.577 |
| Rural family, difference | 1.219 | 0.637 |
| Poor family, difference | −0.202 | 0.094 |
| Leaving the municipality's public schools | | |
| Nest parameter | 0.847 | 0.031 |
| Attachment to public schools for an income poor family | 1.411 | 0.062 |
| Implied ratio of effective distance costs | | |
| Rural over urban family (nonpoor) | 0.56 | 0.15 |
| Poor over nonpoor family (urban) | 1.07 | 0.04 |
| Overidentification test (17 moments, 4 parameters), p | 0.038 | |
| Markets (cells by municipality, grade and year) | 3,023 | |

*Panel B. The model against the data*

| | Data | Standard error | Model |
|---|---|---|---|
| Entered the nearest surviving school, percent (implied by the targeted shares) | | | |
| Rural poor | 44.6 | 2.3 | 49.7 |
| Rural nonpoor | 37.2 | 3.6 | 46.9 |
| Urban poor | 38.7 | 3.0 | 42.2 |
| Urban nonpoor | 37.9 | 3.2 | 40.4 |
| Km to the school entered (not targeted) | | | |
| Rural poor | 4.73 | 0.14 | 4.65 |
| Rural nonpoor | 4.83 | 0.19 | 4.62 |
| Urban poor | 2.91 | 0.16 | 2.73 |
| Urban nonpoor | 3.28 | 0.17 | 2.94 |
| Left the municipality's public schools after a closure, percentage points (not targeted) | | | |
| Rural closed schools | 12.0 | 2.5 | 9.2 |
| Urban closed schools | 3.2 | 1.2 | 7.9 |
| Reenrolled students (shares) | 15,025 | | |
| Reenrolled records with a model value (km) | 13,058 | | |
| Students in the exit estimates | 7,138 | | |

*Notes.* Nested logit of the school a displaced student entered, with leaving the municipality's public schools as the outside option. In Panel A, a family's distance coefficient within the nest sums the rows that apply, and the exponent is fixed. Poor denotes the Department's family poverty record, while income poor denotes survey income poverty. All families respond alike to school effectiveness, a restriction chosen after the results (Table D2). The nest parameter rests on one moment, the effect of a closure on leaving. The test's p value is simulated from its first order null distribution with the covariance of the targets treated as known. Ratios use the delta method. In Panel B, Model is predicted at the estimates and Data standard errors are clustered by municipality. Km from the closed school is a straight line times a road ratio, on the 13,058 reenrolled records with a model value, both weighted by students. Leaving uses the event study of closures of 2013-16, by the Department's rural tag (Model uses catchment).

**Table 6.** The access loss priced by the choice model

| | Displaced students | | | | | | |
|---|---|---|---|---|---|---|---|
| | Rural poor (1) | Rural nonpoor (2) | Urban poor (3) | Urban nonpoor (4) | Rural poor over urban nonpoor (5) | Rural poor minus urban nonpoor (6) | Students (7) |
| Km to the nearest surviving school | 3.43 (0.16) | 3.37 (0.22) | 1.91 (0.19) | 2.12 (0.18) | 1.62 (0.15) | 1.31 (0.22) | 34,975 |
| Km equivalents in the choice model | 1.84 (0.11) | 1.91 (0.15) | 0.97 (0.08) | 1.00 (0.09) | 1.84 (0.16) | 0.84 (0.12) | 28,024 |

*Notes.* Closures of 2013-17. The first row reports km from the closed school to the nearest surviving school of its municipality that serves the grade, as in Table 2, for all displaced students. The second reports the fall in a family's expected best utility among the municipality's public schools (within the nest) when the closed schools leave its choice set, at the estimates of Table 5. It expresses this fall as the extra distance that costs the family as much and covers the 28,024 displaced students of the 885 markets with a realized closure. The two rows rest on different students. The model types families by their block group's rural indicator, not by the closed school's rural share (Online Appendix D.6). Standard errors appear in parentheses. Those in the first row are clustered by municipality. Those in the second use the delta method in the coefficients, clustered by municipality, with enrollment shares, the set of closed schools, population weights, the reference distance and the distance power held fixed.

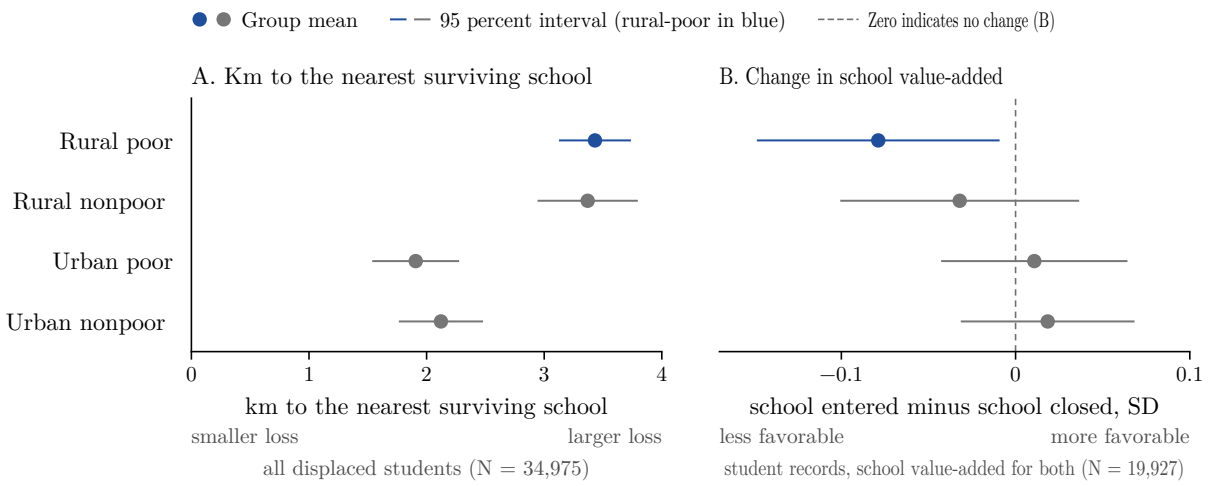
poor students is 1.84 (0.16) times that of urban non-poor students, against a ratio of 1.62 (0.15) in km to the nearest surviving school. The difference between the two groups is 0.84 (0.12) kilometer equivalents. The price ratio depends largely on the conversion itself. Rural families' estimated effective distance costs are lower, so a similar loss of utility converts into more km. In the reported sensitivity exercise for utility within the nest, the ordering of the two groups' losses is not determined over the confidence set for the rural difference in the distance coefficient (Online Appendix D.5), so the paper's claim about priced access is the ordering in kilometer equivalents, besides the ordering in km of Section 4.3, which needs no conversion. Across the variants of the choice model in Online Appendix D.5, the ratio lies between 1.78 and 1.85. Over the confidence sets for the rural difference in the distance coefficient, it lies between 1.4 and 2.1, above one at every point evaluated.

## 7.4 The Two Margins Together

Rural-poor students stood on the less favorable side of both margins, in two units that the paper keeps apart (Figure 6). In km, rural students' nearest surviving school was farther than urban students' (Panel A). The choice model prices this difference, so rural-poor students' loss is again larger. In school value-added, their change was 0.084 SD (0.031) less favorable than that of all other displaced students (Panel B).

The two panels rest on different records of the same closures. Panel A covers all 34,975 displaced students, grouped by the closed school's rural and poor shares. Panel B covers the 19,927 student records of Table 4, grouped by each student's own poverty record and the closed school's rural share. The access gap

**Figure 6.** Distance to the nearest surviving school and changes in school value-added by group




*Notes.* Closures of 2013-17. Panel A gives mean distance from the closed school to the nearest surviving school of the municipality that serves the student's grade, a straight line times a road ratio by class of school. It uses all 34,975 displaced students, grouped by each closed school's rural and poor shares. Panel B gives the estimated school value-added of the school entered minus that of the school closed, in SD of test scores, for the 19,927 student records of Table 4, grouped by the student's own poverty record and the closed school's rural share. The dashed line marks zero, or no change. The two panels rest on different records of one set of closures. Rural poor minus all other displaced students is 1.29 (0.20) km in A (1.32 (0.21) on B's records) and −0.084 (0.031) SD in B. Circles show group means and lines show 95 percent confidence intervals, with standard errors clustered on the municipality of the closed school. Table 2, Panel B, and Table 4 hold the numbers.

is similar for both. Rural-poor students' nearest surviving school was 1.29 km (0.20) farther than that of all other students among the 34,975, and 1.32 km (0.21) farther in the student records (Tables 2 and 4).

Two patterns motivate Section 8. More distant rural alternatives suggest that keeping isolated schools open could significantly reduce the distance rural students face. The concentration of the school value-added gap in the closed schools motivates comparing alternative closure lists on the value-added margin as well. The descriptive decomposition does not determine how students would sort under a different list. Section 8 compares sets of closures at the schools they close before students sort again, and also with students placed by the choice model.

# 8 Counterfactuals

This section first varies which schools close, conditional on the PRDE's number of closed schools in each municipality and year. It then varies how many close and, with the closure list fixed, where displaced students and teachers go. Section 8.1 states what every comparison holds fixed, and Sections 8.2 to 8.4 take the decisions in order.

## 8.1 Design and What Is Held Fixed

A set of closures changes two things for a group. First, how many students are displaced, and second, how each margin changes per displaced student. The tables report both margins. The total effect can be calculated using both the number of displaced students and the margin per displaced student when both refer to the same population and use the same weights. The mean of a destination school on covered origin schools cannot be multiplied by the count of all displaced students. We have two sets of closure lists: one that displaces fewer rural-poor students, and one that creates a lower margin effect for each of them.

Each list at the Department's counts uses only what the Department recorded the year before a closure. Each record includes a school's enrollment, its place on the map, and its average test score. The 19 closures held in every list stay closed, and the other 315 of the 334 closures are chosen again. Each list closes as many schools as the PRDE did in every municipality, year, and size band. The comparable pool includes schools open in the closure year that are not larger than the largest school closed, conditional on being within the estimation core with eligible grade spans. The school selection procedure for each list excludes specialized schools, held closures, and protected earlier receiving schools (Online Appendix E.1). Each size band is defined as being in one of the thirds of school size among that year's closed schools. Moreover, each year starts from the schools left open by the realized closures of earlier years. So a comparison describes one year's choice at a time.

Five lists vary whether the choice uses the map, average test scores, both, or neither (Online Appendix Table E1). They are named *smallest first*, *smallest first with an alternative within 4.8 km*, *lowest average test scores first*, *lowest average test scores first with an alternative within 4.8 km*, and *nearest alternative first*. Each score list is chosen on one half of the school years and measured on the other. A random list draws from every municipality, year, and size band, with as many comparable schools as closed in that specific municipality-year-size-band cohort. Holding counts within size bands holds enrollment roughly fixed to meet one of the PRDE's stated criteria (Section 2.1). This setting allows random lists to isolate which comparable schools closed. The setting draws 1,000 random lists from the reference, spreading across possible choices rather than a sampling interval.

The choice model and estimates of school value-added supply a placed value for 32,398 of the 34,975 displaced students using the enrollment weights of the realized closures. This process covers 93.7 percent of the rural-poor weight and 91.9 percent of the other weight. On this basis, 1,628 students (4.7 percent) have no grade in the choice model's defined market. Also, 858 (2.5 percent) students do not have the closed school value-added. Small remaining exclusions account for the rest. Section 6.2 checks the placements on

realized closures. 18,834 of 19,927 records have a placed value on the separate sample of student records of Table 4. The 1,093 records (5.5 percent) without any placed value show a different recorded rural-poor contrast: −0.282 SD (0.115) versus −0.074 (0.031) for the covered records.

The population used to calculate averages varies by measure. Distances are averaged over the students displaced by that list for each closure list. Differences relative to the realized closures are averaged over a fixed population. For changes measured before students re-sort, this is the students displaced by the realized closures. This provides a common basis for comparing these changes across lists, although the lists displace different students. Figure 7 shows the change per student displaced by each list. The whole section compares rural-poor students with all other displaced students. For access cost, all other students include rural non-poor students, whose loss matches that of rural-poor students and urban students. Online Appendix Table E2 gives the four groups for random lists.

I hold the rest of the system as it was in the year of closure when comparing closure lists. Surviving schools keep their school value-added, teachers, and other students. These schools do not respond to the displaced students coming from closed schools. In the counterfactual setting, the residence of modeled displaced students does not change. So they choose as the students on whose enrollment shares built the model's school mean utilities.

The calculations assume that the move from closed to the destination school do not make any disruption. Closure lists must preserve grade coverage and pass an aggregate screen for spare enrollment capacity. The capacity is relaxed where necessary to accommodate the realized closures. Once a list is selected, the choice model allocates students without seat constraints at individual destinations. Each list closes the same number of schools, but the number of removed seats and the fiscal savings may differ. The proxy for removed seats is based on historical enrollment in grades 3 to 8, while displaced students are counted in grades 1 to 7. So, these two do not form a capacity comparison. On this proxy, the realized closures remove 35,455 seats and the five lists 29,272 to 35,007. The lists displace 33,242 to 37,339 students, against 34,975 (Table 7, Panel A). Savings enter only in Section 8.3.

The two score lists and the place under other lists were added after the results were seen. The exercise with fewer closures and the rule setting standard errors for the change before students sort again were also added afterward. Online Appendix E.1 describes the closure rules and the scope of the comparisons. Statements of direction are adjusted for multiple comparisons within sets specified before each set's own results were seen (Online Appendix E.5).

**Table 7.** Both margins by group for closure lists at the Department's counts

*Panels A and B. Seats, students displaced and km to the nearest surviving school*

| | A. Students and seats | | | B. Km to the nearest surviving school | |
|---|---|---|---|---|---|
| | Seat proxy grades 3-8 (1) | Students displaced (2) | Percent rural poor (3) | Rural poor (4) | All other (5) |
| Realized list | 35,455 | 34,975 | 39.7 | 3.43 | 2.15 |
| *Smallest first* | 29,272 | 33,242 | 43.6 | 3.79 | 2.25 |
| *Smallest first, alternative within 4.8 km* | 30,134 | 33,698 | 39.8 | 3.11 | 2.07 |
| *Lowest average test scores first* † | 35,007 | 36,850 | 40.9 | 3.68 | 2.18 |
| *Lowest average test scores first, alternative within 4.8 km* † | 34,855 | 37,339 | 37.9 | 3.11 | 2.01 |
| *Nearest alternative first* | 32,979 | 35,217 | 34.3 | 2.67 | 1.50 |

*Panels C and D. Change in school value-added, school entered or surviving minus school closed, SD*

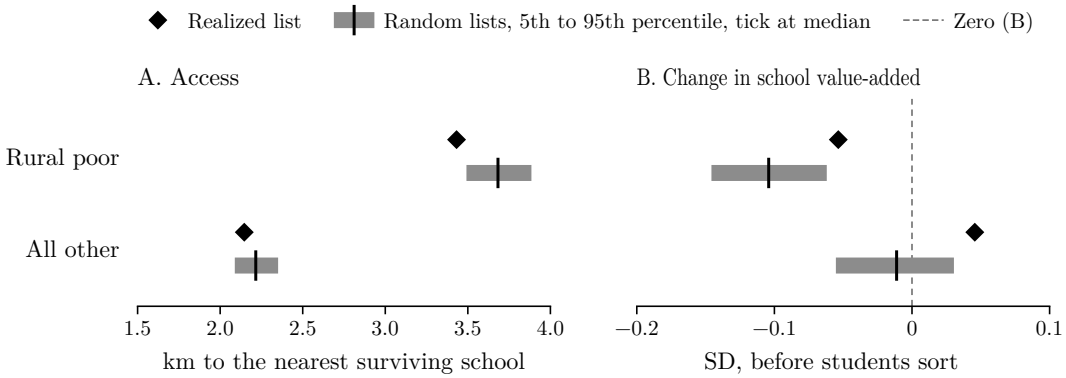
| | C. Before students sort again | | D. Placed by the model | |
|---|---|---|---|---|
| | Rural poor (6) | All other (7) | Rural poor (8) | All other (9) |
| Realized list | −0.054 | 0.046 | −0.021 | 0.024 |
| | (0.041) | (0.029) | (0.044) | (0.033) |
| Difference from the realized list | | | | |
| *Smallest first* | −0.098 | −0.089 | −0.093 | −0.068 |
| | (0.039) | (0.030) | (0.036) | (0.036) |
| *Smallest first, alternative within 4.8 km* | −0.081 | −0.084 | −0.085 | −0.068 |
| | (0.035) | (0.029) | (0.034) | (0.034) |
| *Lowest average test scores first* † | 0.048 | 0.094 | 0.072 | 0.141 |
| | (0.042) | (0.032) | (0.052) | (0.046) |
| *Lowest average test scores first, alternative within 4.8 km* † | 0.057 | 0.104 | 0.080 | 0.144 |
| | (0.036) | (0.034) | (0.047) | (0.048) |
| *Nearest alternative first* | −0.054 | −0.028 | −0.062 | −0.008 |
| | (0.035) | (0.030) | (0.040) | (0.034) |

*Notes.* Closures of 2013-17, with lists as in Table E1. Panel A uses closed schools' earlier maximum enrollment in grades 3 to 8 as the seat proxy. Students displaced are in grades 1 to 7, so the columns are not a capacity comparison. Panel B gives km per student each list displaces, as in Table 2, which gives the realized list's standard error. None is computed for the other lists. Panel C gives surviving schools' mean school value-added, weighted by enrollment, minus the closed school's, per student the realized list displaced. Standard errors are the largest of four imputation rules (Table E11), a rule changed after the results were seen, under which *smallest first*'s rural-poor difference (standard error 0.039, against 0.035) is no longer distinguishable from zero. Panel D is per student the choice model places (93.7 percent of the realized list's rural poor). Standard errors combine the delta method in the model's coefficients with the noise in the school value-added, and the completed 200-draw numerical check retains these intervals under the maintained noise construction (Online Appendix E.5). The † lists were added after the results were seen, ranked on one half of the school years and measured on the other. Panel B pools the two directions, while C and D average them. No adjustment for multiple comparisons rejects a list in C or D.

## 8.2 Different Schools at the Department's Counts

Random lists of comparable schools reproduce the rural gap in distance (Figure 7, Panel A). Across 1,000 random lists, the median distance to the nearest surviving school per student displaced is 3.68 km for rural-poor students, 3.60 for rural non-poor, 1.96 for urban poor and 2.13 for urban non-poor students. The realized closures give 3.43, 3.37, 1.91 and 2.12. They lie at the 1.2nd and 2.2nd percentiles of the random lists for the two rural groups, and at the 28.2nd and 47.9th for the two urban groups (Online Appendix Table E2). Thus the rural distance gap also appears when closures are drawn from this comparable pool at the realized counts. The low pooled percentiles of the realized rural distances are driven mainly by 2017 and do not describe every earlier wave.

**Figure 7.** The realized list among random lists at the Department's counts




*Notes.* Closures of 2013-17. A random list draws comparable schools and closes, in every municipality, year and size band, as many as closed there. The 19 held closures close under each list. The diamond marks the realized list, the bar spans the 5th to 95th percentile of 1,000 random lists (a spread across lists, not a sampling interval), and the tick marks their median. Both panels are per student each list displaces. Panel A gives km from the closed school to the nearest surviving school of the municipality. Panel B gives the municipality's mean school value-added among surviving schools, weighted by enrollment (full sample estimates, Table 3), minus the school closed, before students sort again, with negative values less favorable. All other comprises the three groups other than rural poor. Table E2, Panels A and D, holds related figures. Its middle 95 percent runs from the 2.5th to the 97.5th percentile. Panel B's levels are not read in the text because they move with the version of the school value-added (Section 7.2).

Random lists also give a gap of similar size in the change in school value-added before students sort again (Figure 7, Panel B). Under the realized closures, this change is 0.099 SD less favorable for rural-poor students than for all other displaced students. Across random lists, the gap averages 0.093 SD per student displaced, and the middle 95 percent of lists lie between 0.041 and 0.141 (2.5th to 97.5th percentiles, with Figure 7's bars marking the 5th to 95th).

At the Department's counts, the lists that use the map lower distance for rural-poor students. Their nearest surviving school is 3.43 km away under the realized closures, 3.11 km under *smallest first with an*

*alternative within 4.8 km* and 2.67 km under *nearest alternative first* (Table 7, Panel B). The share with none within 4.8 km falls from 19.6 percent to 9.1 and 10.7 percent (Online Appendix Table E1). Summed over the rural-poor students each list displaces, distance falls by 12.5 percent [2.5, 21.0] and 32.4 percent [23.0, 41.2], with 95 percent intervals in brackets (Online Appendix Table E1, notes). In the choice model, the access loss per rural-poor student is 1.84 (0.11) kilometer equivalents under the realized closures and 1.79 (0.11) and 1.65 (0.12) under the two lists that use the map but not scores, figures that carry no statement of direction (Online Appendix Table E3). Under *smallest first*, which uses neither the map nor scores, the figures are 3.79 km and 2.20 (0.09) kilometer equivalents. Lists also change who is displaced. *Nearest alternative first* displaces 35,217 students, 34.3 percent of them rural poor, against 34,975 and 39.7 percent (Panel A).

Before students sort again, no list's change in school value-added for rural-poor students can be distinguished from the realized closures' after adjusting for the five comparisons (Table 7, Panel C). The realized change is −0.054 SD (0.041) per rural-poor student displaced. This figure and the gap of 0.099 SD above are per student the realized closures displaced, the 34,037 students of the 315 closures chosen again, whereas Section 7.2's term for the school closed, +0.045 for rural-poor students and 0.104 for the contrast, is on the 19,927 student records of Table 4. At the point estimates, the differences from the realized closures are −0.098 (0.039) for *smallest first*, −0.081 (0.035) with an alternative within 4.8 km and −0.054 (0.035) for *nearest alternative first*, and +0.048 (0.042) and +0.057 (0.036) for the two score lists. A closed school without a school value-added estimate counts as zero in these figures, and this coverage differs between lists (Online Appendix E.1).

Placed by the choice model, the students in each list would show the same result (Table 7, Panel D). The realized closures give rural-poor students a placed change of −0.021 SD (0.044). The recorded change in Section 7.2, −0.079 (0.036), covers other students, and its difference from the placed change includes the model's error (Section 6.2). The lists' contrasts with the realized closures range from −0.093 to +0.080 SD. None of these estimations can be distinguished from zero given the model's error, with each list carrying a tolerance of 0.020 to 0.033 SD. No list's placed change for rural-poor students differs from the realized closures' by more than 0.233 SD, conditional on the standard errors from the delta method and the two halves and on the stated allowance covering each list's differential placement error (95 percent, simultaneous over the five lists, as reported in Online Appendix Table E5). The bound is nearly three times the gap of Section 7.2. Therefore, the data cannot rank the lists on this margin.

Changes in sorting contribute little to the placed contrast for the size and map lists, so nearly all comes from which schools close. For *smallest first*, −0.110 SD (0.034) comes from the schools closed and +0.017

(0.014) from the schools entered (Online Appendix Table E5). For the two score lists, the terms for the schools entered are larger by about 0.04 to 0.06 SD. Therefore, the access and school value-added comparisons describe distinct consequences of changing the closure list. In the choice model, access loss per displaced rural-poor student is 10.2 percent below the realized list under *nearest alternative first* and 6.0 percent below it under *lowest scores first with an alternative within 4.8 km*. *Lowest scores first* without that distance priority is 6.0 percent above it (Online Appendix Table E4). These are comparisons of the different students each list displaces. Here and in Section 8.3, the standard errors for placed students combine the delta method in the model's coefficients with the noise in the school value-added. A completed numerical check with 200 draws retains their reported intervals under the maintained coefficient and noise distributions based on the two halves. It checks numerical propagation, not the validity of those distributions or the counterfactual model (Online Appendix E.5).

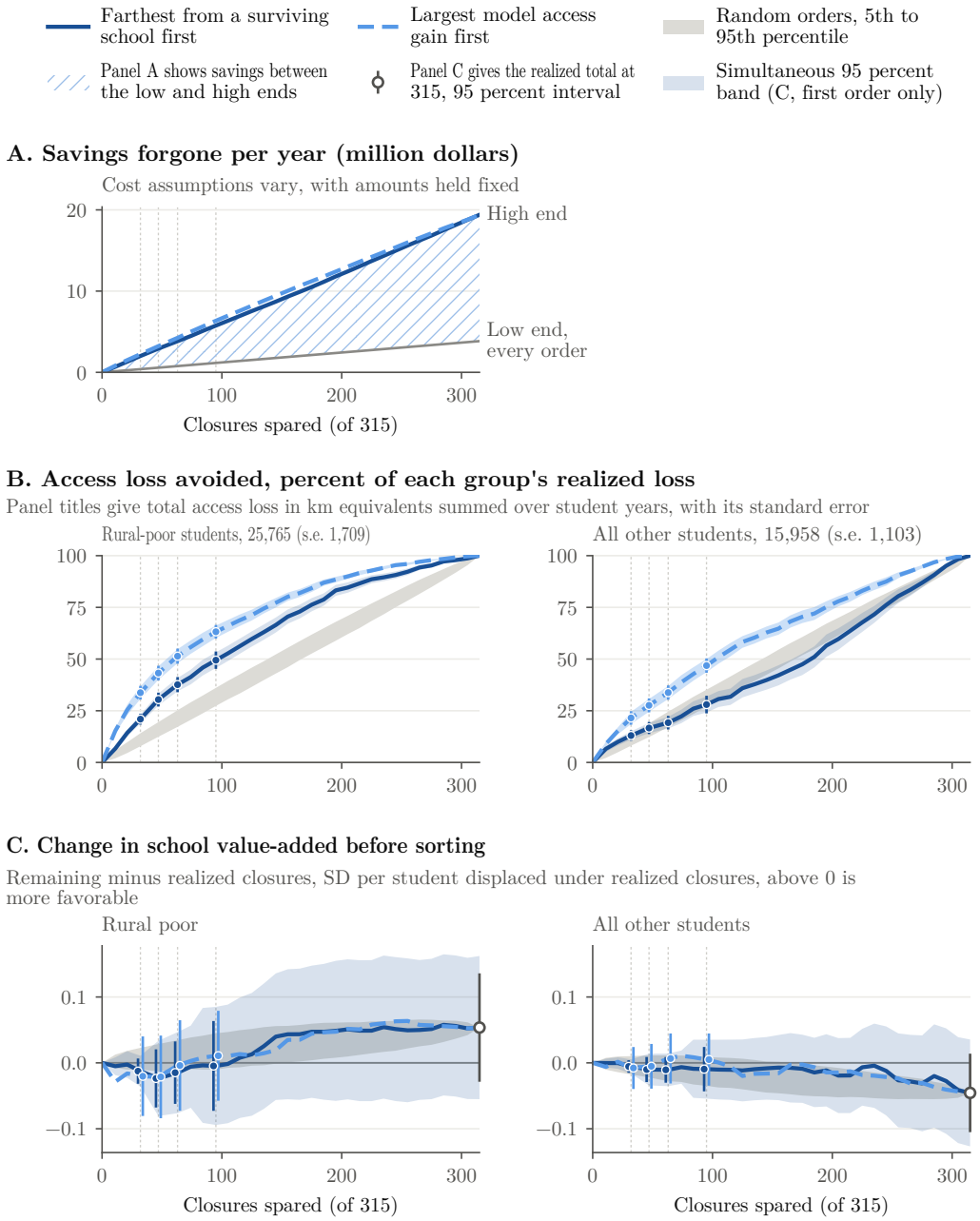
Pooled over the waves and driven mainly by 2017, the Department's closures left rural students nearer a surviving school than almost all random choices from this comparable pool at its counts. This statement is conditional on the comparison design. The Department did not compute school value-added, and none of these facts shows what it weighed.

### 8.3 Closing Fewer Schools

Closing fewer schools keeps some realized closures open and gives up their savings, making it a different decision. From the 315 closures chosen again in Section 8.2, with the held closures still closed, I spare closures one at a time in two orders. "Farthest from a surviving school first" spares first the closure whose students, in its grade with the most distant alternative, were farthest from a surviving school serving that grade. The Department could have computed it from the map. "Largest model access gain first" spares at each step the closure that most reduces the choice model's access loss for all students. It requires the model and is chosen and measured at the same estimates. One thousand random orders of the same closures give a reference band, the 5th to 95th percentiles at each count. I read the paths at 32, 63 and 95 closures spared, a tenth, a fifth and three tenths of the 315. I also read them at 47, where the first order brings the share of rural-poor students with no surviving school within 4.8 km close to zero (Figure 8).

Keeping open the 47 closures farthest from a surviving school nearly empties the far tail. The share of rural-poor students with no surviving school within 4.8 km falls from 19.6 to 0.14 percent, and the remaining closures displace 30,441 students instead of 34,975. The 47 closures spared avoid 30.4 percent [27.7, 33.2] of rural-poor students' access loss in the choice model and 16.6 percent [14.3, 19.0] of all other students', about a quarter in all (Figure 8, Panel B). For rural-poor students, the share avoided is 20.9, 37.6, and 49.5

**Figure 8.** Savings forgone, access loss avoided and changes in school value-added with fewer closures




*Notes.* The horizontal axis counts closures of 2013-17 kept open, of 315. *Farthest from a surviving school first* spares first the closure whose students were farthest from one. *Largest model access gain first* spares the closure that most reduces the choice model's access loss (the Department could not compute it). The exercise and its orders are exploratory, with inference restricted to the families in Online Appendix E.5. Gray gives the 5th to 95th percentile of 1,000 random orders (a spread, not sampling error). In Panel A, the low end is $12,197 a closure (every order), and the high end adds $121.16 a student and $31,544 a closure for staff posts. Panel B uses 28,024 students of the choice model. Access counts each student once, while savings recur yearly. Bars give pointwise 95 percent intervals at 32, 47, 63 and 95, and shading gives simultaneous 95 percent bands (C gives the first order only, as in Figure E1). In Panel C, standard errors are the largest of four imputation rules (Table E11), a rule changed after the results were seen. More than half of the variance is imputed at 95 under the first order and at 7 of 8 estimates under the second.

percent at 32, 63, and 95 closures spared. "Largest model access gain first" avoids more of the model's loss, the measure it is chosen on, reaching 43.2 percent [40.3, 46.2] for rural-poor students at 47. It leaves 10.4 percent of them with no surviving school within 4.8 km. For all students, both orders avoid more access loss than the 95th percentile of the random orders at each count.

The change in school value-added before students sort again does not move by an amount the data can distinguish (Figure 8, Panel C). With 47 closures spared in the order "Farthest from a surviving school first," the remaining closures' change differs from the realized closures' by −0.024 SD (0.022) for rural-poor students and −0.010 (0.009) for all other students, per student the realized closures displaced. At 32, 63 and 95 the rural-poor differences are −0.013 (0.009), −0.015 (0.023) and −0.005 (0.034). None can be distinguished from zero after adjusting for the four counts. Against random orders of the same closures, this path lies below their median at all four counts, and at 47 at their 0.4th percentile, a spread across orders rather than sampling error (Online Appendix E.3). With students placed by the choice model, per student the realized closures displaced, the rural-poor differences are −0.010 (0.011), −0.016 (0.020), −0.009 (0.022) and −0.005 (0.034) at 32, 47, 63 and 95. None can be distinguished from zero given the model's error (Online Appendix Table E6).

Keeping schools open forgoes savings that recur every year (Figure 8, Panel A). I count a closure's savings at the two ends of a range of cost assumptions and treat both ends as fixed amounts. The low end is $12,197 a year, the fixed part of a school's facilities spending net of what a closed building still costs. The high end adds $121.16 per student enrolled in the closure's last year and $31,544 for staff posts (1.06 posts per closure), about $60,600 at the median closed school of 139 students. All 315 closures save $3.84 million to $19.39 million a year on this account. Keeping open the 47 farthest from a surviving school forgoes $0.57 million to $2.93 million a year, about 15 percent at either end. The high end assumes that a released post is not refilled (Online Appendix F).

Dividing savings forgone by access loss avoided gives the breakeven value of one kilometer equivalent of access for one student for one year. With 47 closures spared under "Farthest from a surviving school first," it is $55 [$52, $58] at the low end and $279 [$265, $294] at the high end, for all students. Across 32 to 95 closures spared, it runs from $52 to $67 at the low end and from $269 to $334 at the high end (Online Appendix Table E6). The intervals carry the sampling error in access only. Access counts each displaced student once, for the school year of displacement, while savings recur every year. Students who

would have entered the closed schools in later years are not counted. Savings are also gross of any added cost of transport, which would lower the breakeven values.[7]

### 8.4 Holding the Closures Fixed

With the closure list fixed, I first examine school effectiveness within each displaced student's observed distance and then reassign the teachers of a selected pool among observed posts. In the benchmark for student placement, wider distance limits permit larger estimated gains. Teacher reassignment instead holds every student's school and distance fixed.

**Reach.** Without going farther from the closed school than the school they entered, rural-poor students could have entered somewhat more effective schools, although their estimated gain is smaller than urban non-poor students' at the point estimates (Table 8). For each displaced student's student record, I find the most effective surviving school with a seat, capped at the earlier maximum enrollment, no farther from the closed school than the school entered, holding every other student's school fixed. The school is chosen on one half of the school years, and its school effectiveness, the school value-added plus school-average teacher value-added, is measured on the other. For rural-poor students, the school found is 0.048 SD more effective [0.028, 0.073] and 0.31 km nearer the closed school, with 79 percent of their records keeping the school entered. For urban non-poor students, the gain is 0.067 [0.031, 0.102], at 0.37 km nearer. For rural-poor students, one more km raises the gain to 0.064 [0.040, 0.090]. Any surviving school of the municipality raises it to 0.204 [0.146, 0.269], at 3.1 km [2.4, 3.9] more. The exercise uses 12,564 student records. Evaluating each student's alternative separately while holding every other student's school fixed does not construct a jointly feasible reassignment of the whole cohort.

**Teachers.** I hold students' schools and classrooms fixed and reassign displaced teachers among 626 observed posts, within municipality, year, subject profile, and grade band. The original objective weights each post's scored caseload by one plus its school's model rural-poor share, without specifically targeting displaced students' classes. Scored on each displaced student's linked classroom, this assignment changes 201 posts and gives rural-poor students an estimated gain of 0.00033 SD per displaced student, with a 95 percent interval of $[-0.00262, 0.00383]$ (Online Appendix Table E9). The interval includes zero. These results con-

[7] Online Appendix E.3 adds two orders that rank closures by past test scores and the map, which the Department held.

**Table 8.** School effectiveness and distance within reach by group

| | Rural poor | Urban nonpoor |
|---|---|---|
| *Panel A. Gain in school effectiveness, SD* | | |
| No farther than the school entered | 0.048 | 0.067 |
| | [0.028, 0.073] | [0.031, 0.102] |
| Within one more km | 0.064 | 0.099 |
| | [0.040, 0.090] | [0.057, 0.141] |
| Anywhere in the municipality | 0.204 | 0.224 |
| | [0.146, 0.269] | |
| *Panel B. Change in km to the school entered* | | |
| No farther than the school entered | −0.31 | −0.37 |
| Within one more km | −0.24 | −0.20 |
| | [−0.33, −0.16] | |
| Anywhere in the municipality | 3.1 | 2.87 |
| | [2.4, 3.9] | |

*Notes.* The realized closures and every other student's school are held fixed. Each displaced student's record is evaluated at the most effective surviving school serving the grade and satisfying the distance restriction. Seats follow earlier maximum enrollment, with a floor admitting observed allocations. This accounting screen is individual rather than a jointly feasible cohort assignment. Schools are selected on one half of the school years and their school effectiveness, school value-added plus school-average teacher value-added, is measured on the other, with directions reversed and averaged. Distance runs from the closed school, and its change compares the assigned school with the school entered. The sample contains 12,564 student records with rural-poor weight 5,369. Brackets give 95 percent bootstrap intervals over municipalities. Entries without brackets are point estimates only. Online Appendix E.4 gives construction details.

cern a selected pool that teaches 21.5 percent of displaced rural-poor students' linked observations in tested subjects, rather than the scope for reassigning all teachers. [8]

Allowing exchanges across municipalities and directing the assignment at the observed classes of displaced rural-poor students gives larger point estimates in this exercise. Retaining municipality restrictions with the objective targeting displaced students gives 0.00250 SD $[-0.00094, 0.00674]$. Exchanges across the island give 0.00773 $[-0.00311, 0.01864]$ with the original objective and 0.01830 $[0.00775, 0.03004]$ with the objective targeting displaced students. These are conditional benchmarks using the same teachers, posts, classes, and intervals specific to each design, without adjusting across designs or intervals for their differences. The calculations use classes observed in the next year. They do not establish implementable gains after accounting for teachers' residences, preferences, priority rules, and implementation costs.

Across the three decisions, access moves with which schools close and how many, although the data cannot rank the resulting changes in school value-added. With closures fixed, the benchmark for student placement offers modest estimated gains within observed distances. The local teacher exercise gives a

[8] The denominator is 22,475 displaced students who enrolled again in a public school in the same municipality in 2013-17, with rural-poor weight 8,647.4. For a student with links to destination teachers, the outcome averages change over the tested subjects with such links. Changes are zero for teachers outside the pool and for students without eligible destination links. Online Appendix E.4 describes pool selection and the uncertainty calculation. It also reports an information scenario under an assumed response to a report card.

small, imprecise gain, while wider exchanges and a different assignment objective give larger conditional benchmarks without changing students' schools or distances.

# 9 Conclusion

Puerto Rico's closures of 2013-17 changed access to public schooling and exposure to educational inputs unevenly. Within the municipality, the nearest surviving school stood about 3.4 km from rural students' closed schools, compared with about 2 km for urban students. Rural-poor students' recorded change in estimated school value-added was 0.084 SD less favorable than all other displaced students' and 0.097 SD less favorable than urban non-poor students' on the larger school value-added sample. The contrasts mainly show differences in the schools that closed relative to their local alternatives. These separate accounts of geographic access and educational inputs describe how closures expose students to access and quality costs.

The policy comparisons separate closure selection, closure counts, and subsequent allocation. Proximity-based lists reduce rural-poor students' distance to surviving schools, conditional on PRDE's decision about the number of closed schools. However, the comparisons of school value-added remain too imprecise to rank the lists. Keeping open the 47 most isolated schools under the implemented policy avoids 30.4 percent of rural-poor students' total modeled access loss. These comparisons hold closure counts constant. This study also specifies which closures are spared, given the study's limitation on access to Puerto Rico Department of Education savings accounts over school consolidation policy.

A separate student placement exercise finds an estimated 0.048 SD improvement in rural-poor students' school effectiveness in the same closure rule implemented by PRDE using teacher reallocation. The scope for reallocating teacher inputs depends on which teachers and posts can change assignment. The original exercise restricted to each municipality yields a small estimated gain. A benchmark that allows the same restricted teacher pool to move across municipalities and targets displaced rural-poor students' classes generates larger point estimates. It uses observed classroom assignments and abstracts from teachers' locations, priority rules, and willingness to move.

The analysis holds inputs at surviving schools and much of the system fixed. In this study, I leave spillovers, resource responses, and adjustment over longer horizons unmodeled. Separating local access, school value-added, and portable teacher value-added clarifies what different consolidation policies can change for displaced students across their spatial and demographic heterogeneity. Reallocating resources and preserving a nearby school address different margins, which enrollment alone does not measure adequately.

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