Online Appendix

A Framework for Assessing the Distributional Effects of School Closures

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A Data

This appendix documents the data behind Sections 2 to 4. It covers the variable inventory (Appendix A.1), the two flows of displaced students (Appendix A.2), schools, enrollment, and closures by year (Appendix A.3), the construction of distance (Appendix A.4), the groups and the survey (Appendix A.5), and the event study of exit (Appendix A.6).

A.1 Variable Inventory

Table A1 lists each variable the paper uses, the dataset it comes from, how it is built, and where it is used. Five conventions apply throughout. First, a school year is labeled by the calendar year in which it ends, and a closure by the school's last operating year. Thus a closure of 2017 is a school that last operated in 2016-17, whose students enrolled elsewhere in 2017-18. Second, I harmonize grades to actual grades before applying restrictions, using each source field's convention. The production file's test grade codes for each subject are two above the actual grade. In the raw enrollment file, *GRADE_ID_FK* gives the actual grade, while *GRADE_INT* is two below it. Third, the grade of enrollment defines displacement and the choice model's markets, while the grade of the test defines the subject, grade and year within which a score is normalized. For 0.3 percent of students, the test grade differs from the grade of enrollment. Fourth, a school's total enrollment appears on each of its grade rows in the enrollment extract, so I take it once for each school and year and never sum it across grades. Fifth, scores are merged on the student and the school year, never on the grade. The student identifier matches between the student records and the teacher links for 98.1 percent of students. School codes match for 87 percent of schools, with the rest being high schools and schools outside the enrollment extract.

A.2 Populations and Samples

Table A2 brings together the record flows, the populations used for each empirical object, and the four group counts. Enrollment counts in grades 1 to 7 include students who later left public school. The destination records cover kindergarten to grade 8 and require a recorded next public school, so the two flows are not nested. Comparisons of recorded destinations describe selected public school students who enrolled again. The choice model instead integrates population mass over block groups, rather than counting observed household choices.

Table A1. Variable sources, construction and use

	Source	Construction	Used in
Last operating year	Department's school file	Calendar year in which the school's last operating school year ends	Sections 2 to 8
School enrollment	Enrollment extract	Total enrollment, taken once per school year	Table 1, Figure 2, lists
Location	Department's school file	Coordinates by year	Distances, Figure 1
Rural designation	Department's school file	The Department's rural or urban designation of the school	Exit by area and Table A3
Rural share near a school	Census survey, block groups	Rural share weighted by population around the school (Table A3)	The four groups
Family poverty	Student records	The student's poverty flag, its most frequent value across years	The four groups and distance coefficients
Income poverty	Census microdata, 2013-17	Income below the Census poverty threshold	Attachment to public schools
Rural block group	Census survey, block groups	Log population density below -8.681	Families of the choice model
Grade	Student records and teacher links	Harmonized by source field. Production test grades are coded two above actual grade, with enrollment and test grades kept apart	Displacement and normalization of scores
Test score	Student records	Normalized within subject, grade and year	Score model and average scores
Average test score of a school	Student records	Mean normalized score of its students in the year before the closure year	Table 2, lists
Displaced student	Enrollment extract linked to school file	A student in grades 1 to 7 of a closed school in its last operating year	Table 1 and distance
Next public school	Student records	The student's school of enrollment the next year	Schools entered and Table 4
Km to the nearest surviving school	School file	Straight line times the road ratio of the school's class (1.52 rural, 1.42 urban), within the municipality, serving the grade	Table 2, Figure 3, lists
Km between schools, student records	Student records	Km from the school closed to the school entered, as recorded	Table 2 and rank shares
Km from a block group to a school	OpenStreetMap	Km along the road network, routed	Choice model
Exit	Student records, matched	Absent from the public records the next year, or enrolled in another municipality's public school	Nest parameter and Section 4.4
Teacher of a subject	Teacher links	One teacher per student, year and subject	Score model
Seats	Enrollment extract	Earlier maximum enrollment up to the closure year	Aggregate closure screen, seats removed and reach

Notes. A school year is labeled by the calendar year in which it ends. The Census survey is the Puerto Rico Community Survey, by block group. Tables 1 and 2 summarize the principal school and access measures. The aggregate closure screen differs from individual destination seat constraints, which the closure list model does not impose.

Table A2. Samples, model populations and empirical objects

Sample or population	Count
Panel A. Enrollment counts and observed destinations	
Enrollment at closed schools in their last operating year, grades 1 to 7	36,109
Excluded because the origin school lacks a rural or poor share	1,134
Enrollment population for raw access and origin school exposure, at 323 schools	34,975
Separate student records with a next public school, kindergarten to grade 8	32,514
Excluded for a missing family poverty record	11,548
Excluded because the closed school has no rural share	544
Records with group information	20,422
Excluded for a missing school value-added only at the school closed	343
Excluded for a missing school value-added only at the school entered	75
Excluded for missing effects at both schools	22
Records with effects at both schools	19,982
Excluded without a surviving school average for the municipality	55
Recorded school value-added comparison in Table 4	19,927
Of these, records matched to the enrollment origin for the distance comparison	19,925
Panel B. Choice records and populations for particular outcomes	
Choice extract with a recorded next public school	22,690
Entered a surviving school in the choice set and have a poverty record	15,573
Of these, outside the two poverty classes used for the four groups	548
Rank moment records, from 310 closed school and year pairs in 74 municipalities	15,025
Common sample with recorded school and teacher inputs at origin and destination	19,164
Students of the 315 closures selected again for counterfactual lists	34,037
Covered placed weight under the realized list	32,398
Model displaced mass in the 885 markets with a closure	28,024
Model mass in the 805 markets with recorded destinations, used for the access sensitivity	26,210

Panel C. Four group counts across the three populations

	Rural-poor	Rural nonpoor	Urban poor	Urban nonpoor	All
All realized closures	13,900	3,051	15,379	2,646	34,975
315 list closures	13,618	2,964	14,910	2,545	34,037
Choice model markets	14,026	2,510	9,882	1,606	28,024

Notes. Closures of 2013-17. The enrollment and recorded destination flows in Panel A are not nested. Enrollment counts include later public school exiters, whereas observed destination comparisons require a next public school. Conditions within each flow are applied sequentially. The populations in Panel B serve different outcomes rather than forming successive restrictions. The common sample decomposes the recorded destination minus origin change into school value-added and school-average teacher value-added. The 32,398 placed weight is enrollment weighted and differs from the integrated model mass.

The 938 students separating all realized closures from the list population attended nine of the 19 closures held in every list. All nine closed in 2015, 2016 or 2017. The 6,013 difference between the list population and model mass consists of 781 students in school and grade pairs without a model market or school and 5,232 by which the enrollment counts exceed the model's school data. The 885 market base includes 80 markets beyond the 805 with recorded destinations.

Group counts in the first two rows of Panel C are fractional sums using the origin school's rural and poor shares. The model uses the residence block group's rural indicator and the family poverty record. Rural-poor families account for 50.0 percent of model displacement, against 39.7 percent of the enrollment population. Rounded group counts need not add to the total.

The model classifies families by their residence block group's rural indicator, while the enrollment comparisons use the closed school's rural and poor shares. Rural-poor families constitute 50.0 percent of model displacement, against 39.7 percent in the enrollment counts. These definitions and populations differ, and the composition difference is not decomposed. Of the 34,037 students of the 315 closures selected again, 13,618 are rural poor and 20,419 belong to the other groups as fractional sums. The gap of 6,013 between this population and model mass comprises 781 students without a model market and 5,232 by which enrollment counts exceed the model's school data.

The choice record flow in Panel B supplies the nearest school choice shares. Of all 22,690 public school students who enrolled again, 19.5 percent entered the school outside the choice set most common among the closed school's other grades, and 11.9 percent another school outside it. These destinations differ from absence from public records. The 15,025 records used for the rank shares come from 310 closed school and year pairs in 74 municipalities.

On the 19,925 destination records that match their closed school in the enrollment extract, mean km to the nearest surviving school is 3.52, 3.49, 1.93 and 2.13 for the four groups, compared with 3.43, 3.37, 1.91 and 2.12 among all 34,975 displaced students. The footnote in Section 3.1 reconciles the 334, 339 and 323 school counts.

A.3 Schools, Enrollment and Closures by Year

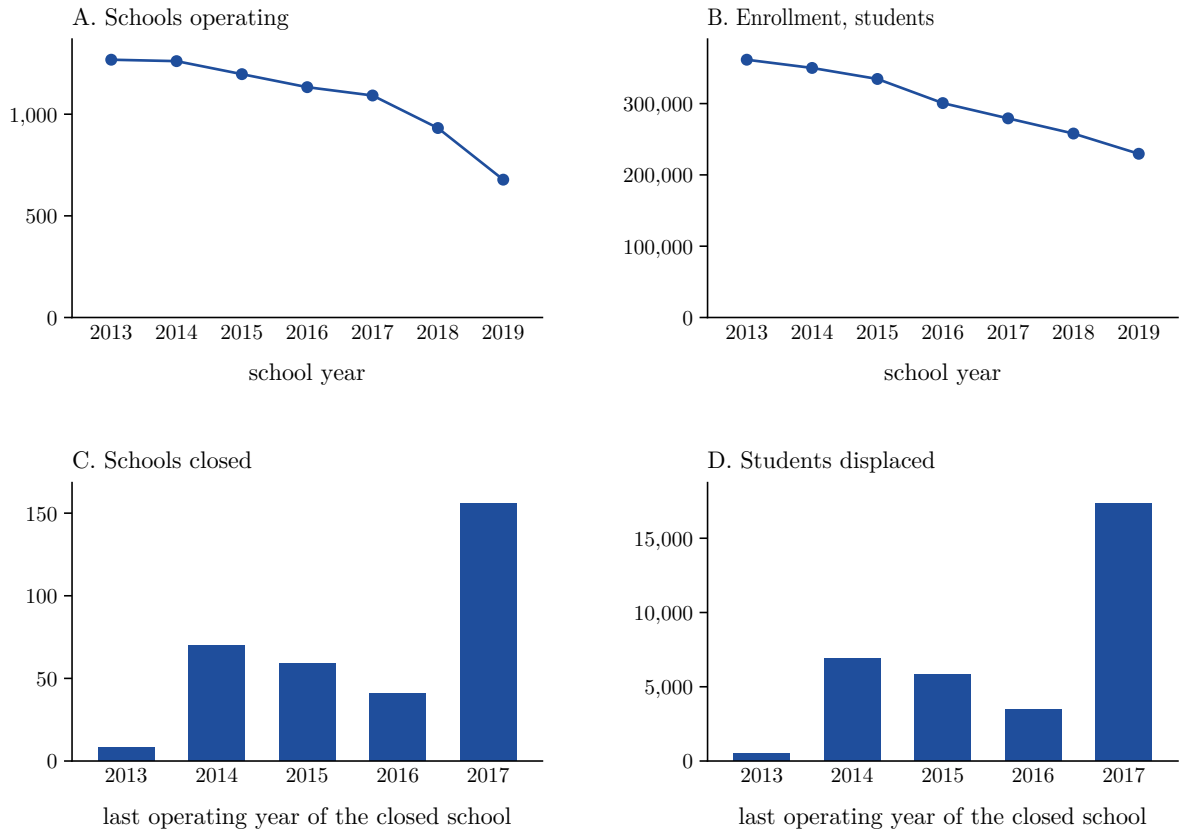
Figure A1 shows, by school year from 2012-13 to 2018-19, the number of schools that serve grades 1 to 8 and their enrollment in the Department's enrollment extract, with the closures of 2013-17 by last operating year. These series count schools in the enrollment extract, not all public schools, and count enrollment once for each school and year. They differ from the published totals of Section 2.1, which cover all grades and a longer period.

A.4 Distance

Km to the nearest surviving school (Section 3.3) multiplies the distance in a straight line between the closed school and each surviving school by the median ratio of road distance to that distance for the closed school's class. The ratio is 1.52 for rural schools and 1.42 for urban schools. A school is rural for this adjustment when at least half of its nearby population lives in rural block groups. The ratios come from the 279,759 routes from block groups to schools that the choice model uses, computed with the Open Source Routing Machine (OSRM) on the OpenStreetMap road network. The nearest school is the nearest surviving school of the closed school's municipality that serves the student's grade, and the share beyond 4.8 km uses the same construction.

Coordinates come from the school file for each year. Over the period, 129 schools had their coordinates revised by more than 10 m, 116 of them first in 2017. The 16 closed schools among the 129 all closed in 2017. Of the schools with revised coordinates, 21 moved by more than 1 km and 19 never closed. The

Figure A1. Schools, enrollment, and closures by year



Notes. Panels A and B show schools serving grades 1-8 and their enrollment in the administrative files. Panel C counts public schools closed, by last operating year, from the closure file. The paper studies the 334 closures of 2013-17. The bars count schools that served at least one grade with a standardized test (grades 3-8) in their last year. Counting every school with a last operating year, whatever its grades, gives 8, 72, 59, 42 and 158. Panel D counts students displaced by each year's closures, covering the 34,037 students of the 315 closures that Section 8 chooses again (the 938 students of the 19 held closures are left out). These are census counts, with no interval. Public schools fell from 1,509 in 2009-10 to 847 in 2019-20 and public enrollment from 473,735 in fall 2010 to 307,282 in fall 2018 (NCES, 2021b,a). Table 1 holds the counts of 2013-17.

choice of coordinates barely moves distances from closed schools. Weighted by enrollment, the distance in km along a straight line from a closed school to the nearest open school of its municipality is 1.25 with the coordinates used here, 1.29 with each school's first coordinates and 1.26 with its last, and 3 percent of closed schools move by more than 0.1 km.

The student records carry their own km between the school closed and the school entered. Section 4.4 uses it for the school entered and for the nearest surviving school on the same records, so that each comparison stays within one construction. The two constructions agree within 0.01 km on 1.0 percent of the 15,025 records of students who enrolled again, and the mean absolute difference between them is 0.47 km. I therefore never subtract a distance on one construction from a distance on the other, and I report no comparison of average test scores between the school entered and a nearest school found on the other construction.

Two other distances appear in the paper. Table 1 and Figure 2, Panel B measure km from a school to the

nearest other school serving a common grade, multiplying the distance in a straight line by the same ratio. In both, that school lies in the same municipality. Figure 2, Panel B takes, for each grade the school serves, the nearest other school of the municipality serving that grade, and weights these distances by the school's enrollment in each grade. The choice model measures distance from each block group to each school along the routes above. None of these distances measures fares, time on a school bus, or a student's trip from home.

A.5 The Groups and the Survey

Table A3 distinguishes rural exposure around the origin school for the descriptive comparisons, residence block group status for model families, and the Department's origin school designation for exit estimates. It reports the density threshold, population and distance weights, and calibration checks. The bandwidth comparison uses a target built from the same block group indicator, so it is not an outside validation of students' home locations. Figure 2, Panel B calls a school rural when the rural share near it is at least one half.

Table A3. Three constructions of rural

	Definition	Check	Used for
Rural share near a school	Share of the population around the school that lives in rural block groups, weighting block groups by population and by exp(−d/2) in their km d from the school.	Area under the curve against the rural status of the students attending is 0.866 at a 2 km bandwidth, 0.856 at 1 km and 0.792 at 5 km. The target uses the block group indicator.	The four groups in both flows. A share of at least one half also defines the school class for the road adjustment.
Block group indicator	A block group is rural when its log population density is below −8.681, the threshold that best matches the Department's designation of schools.	Accuracy 0.788 and area under the curve 0.849, with all 2,527 block groups of the choice model classified.	Families of the choice model, by block group of residence.
Department's designation	The Department's own designation of each school as rural or urban.	The target of the block group indicator's threshold.	Exit estimates by the closed school's area.

Notes. The Census urban share cannot serve in Puerto Rico because it equals 1.0 in the median block group. The area under the curve gives the probability that a school attended by rural students ranks above one attended by urban students. The bandwidth target uses the same block group indicator, so this check is not an outside validation of students' home locations.

For missing family poverty flags, I assign the student's most frequent recorded status across the Department's enrollment records from 2009-10 to 2022-23. Among the 292,691 students with a recorded poverty flag in at least two years, 80.7 percent never change recorded status. The assigned status agrees with the recorded flag 96.8 percent of the time where both exist. For students whose flag changes, it represents their modal status rather than necessarily their status in the current year. In the enrollment flow, a school's displaced students are split by the school's share of students with the flag, whereas each student in the student flow keeps the student's own record. A school's share is never used in place of a student's record.

The survey's microdata available for public use for 2013-17 describe students aged 8 to 14. Among them, 56.2 percent are income poor and 76.5 percent attend a public school. Among public school students,

67.1 percent (0.4) are income poor. Of income poor students, 6.9 percent attend a private school and 1.8 percent are not enrolled, 8.7 percent outside the public schools in all. Of students who are not income poor, 40.8 percent attend a private school and 1.8 percent are not enrolled, 42.6 percent in all. The rate of nonenrollment is thus the same in both groups, and private schooling makes the difference. In the counts by block group, rural block groups hold 45.5 percent of the population, 47.3 percent of students enrolled in public schools in grades 1 to 8, and 38.2 percent of students enrolled in private schools.

A.6 Exit around a Closure

The exit estimate of Section 4.4 comes from an event study that follows students around the closure of their school and compares them with matched students whose schools did not close. Treated students attended a school whose last operating year fell in 2013-16, were enrolled the year before in a grade from 3 to 7 that was not their school's highest, and lived in the school's municipality. Event time zero is the first year after the closure and event time -1 is the reference, with 7,138 treated students observed at event time zero. Matching uses coarsened exact matching. The estimator interacts cohort with event time and aggregates as in Sun and Abraham (2021), including effects for each student and matching cohort pair and each matching cohort and year pair. Standard errors are clustered by the Department's administrative districts. Identification requires that, given the matching and the fixed effects, treated students would have followed the comparison students' trend without the closure, and that the year before the closure was not already affected by it.

The two mutually exclusive outcomes are absence from the public enrollment records the next year and enrollment in a public school of another municipality. Their sum is leaving the municipality's public schools, which corresponds to the choice model's outside option. At event time zero the effects are 3.2 percentage points (0.9) on absence from the public records, 5.3 points (1.2) on enrollment in another municipality, and 8.5 points (1.5) on their sum. The estimated effect on leaving falls to 1.5 points (1.1) one year later and 0.9 points (1.5) two years later. By origin, the effect on leaving is 12.0 points (2.5) for students of schools the Department designates as rural, about two thirds of it enrollment in another municipality, and 3.2 points (1.2) for students of urban schools.

The leads at event times -4 to -2 are close to zero, at 0.0 points (0.7), 0.5 (0.5) and 0.3 (0.4), with a joint p value of 0.595. In the adopted resident sample, the raw rates of absence, enrollment in another municipality and their sum are 8.4, 6.9 and 15.3 percent. The lead estimates are consistent with the maintained parallel trend assumption but do not establish it.

Other samples give effects of similar size, with 7.6 points when the closures of 2017 are added, 6.8 points with all grades, and 10.6 points without the residence restriction, which describes a different population. The effects for grades 3 and 4 and for grades 5 to 7 are 8.6 and 8.5 points. Applying the pooled effect to the choice model's grades 1 to 7 assumes that it also holds in grades 1 and 2.

B Descriptive Evidence

The four exhibits in this appendix supplement Section 4 with predictors of closure, the distributions behind the group means of Figure 3, access allowing alternatives across municipal boundaries, and closure by area.

B.1 Predictors of Closure

Table B1 relates closure to the characteristics the Department held the year before. Both pooled specifications for 2013-17 show that smaller schools were more likely to close. Given enrollment, none of the other conditional logit coefficients is distinguishable from zero, which does not establish that those characteristics played no role. The enrollment trend coefficient is -0.41 (0.49) in the logit and -0.083 (0.027) in the linear probability model. The latter also indicates that schools with growing enrollment were less likely to close. Annual columns for 2014-17 provide supporting comparisons, while the paper's conclusions use the pooled specifications. The sample excludes the 19 held closures, leaving 5,930 of the 5,949 school and year observations.

B.2 The Distributions behind the Group Means

Figure B1 draws, for each group, the distribution across displaced students of the two quantities whose means Figure 3 and Table 2 report. Panel A is km from the closed school to the nearest surviving school. The rural distributions sit well to the right of the urban ones. For 81.6 percent of rural-poor and 81.0 percent of rural nonpoor students the nearest surviving school was more than 1.6 km (one mile) away, against 42.0 and 51.0 percent of urban poor and urban nonpoor students. Panel B is the average test score of the nearest surviving school minus that of the school closed, on the 34,191 students with both. These distributions are wide and overlap across groups. Across rural-poor students the standard deviation is 0.634 SD, and the nearest surviving school scored below the school closed for 52.9 percent of rural-poor students against 41.5 percent of urban poor students. Sampling noise, from the particular students with scores at each school, accounts for about 2.3 percent of the variance of this difference.

B.3 Alternatives across Municipal Boundaries

Table B2 holds the enrollment population, grade coverage and group weights of Table 2 fixed and allows the nearest surviving school to lie in any municipality. The rural-poor minus others gap falls by 0.16 km (0.05), from 1.29 to 1.12 km (0.18). Against urban nonpoor students alone, it falls by 0.19 km (0.06), from 1.31 to 1.12 km (0.20). A strictly nearer surviving school lies across the municipal boundary for 17.1 percent (2.5) of rural-poor students, against 7.6 percent (2.2) of urban nonpoor students. No additional students enter the comparison because every record has a surviving school serving the grade in the origin municipality. The measure describes proximity to an alternative from the closed school. It does not establish the student's commuting distance, a place at that alternative, or a loss of access valued by the model.

Table B1. Predictors of closure

Panel A. Conditional logit within municipality and year, log odds

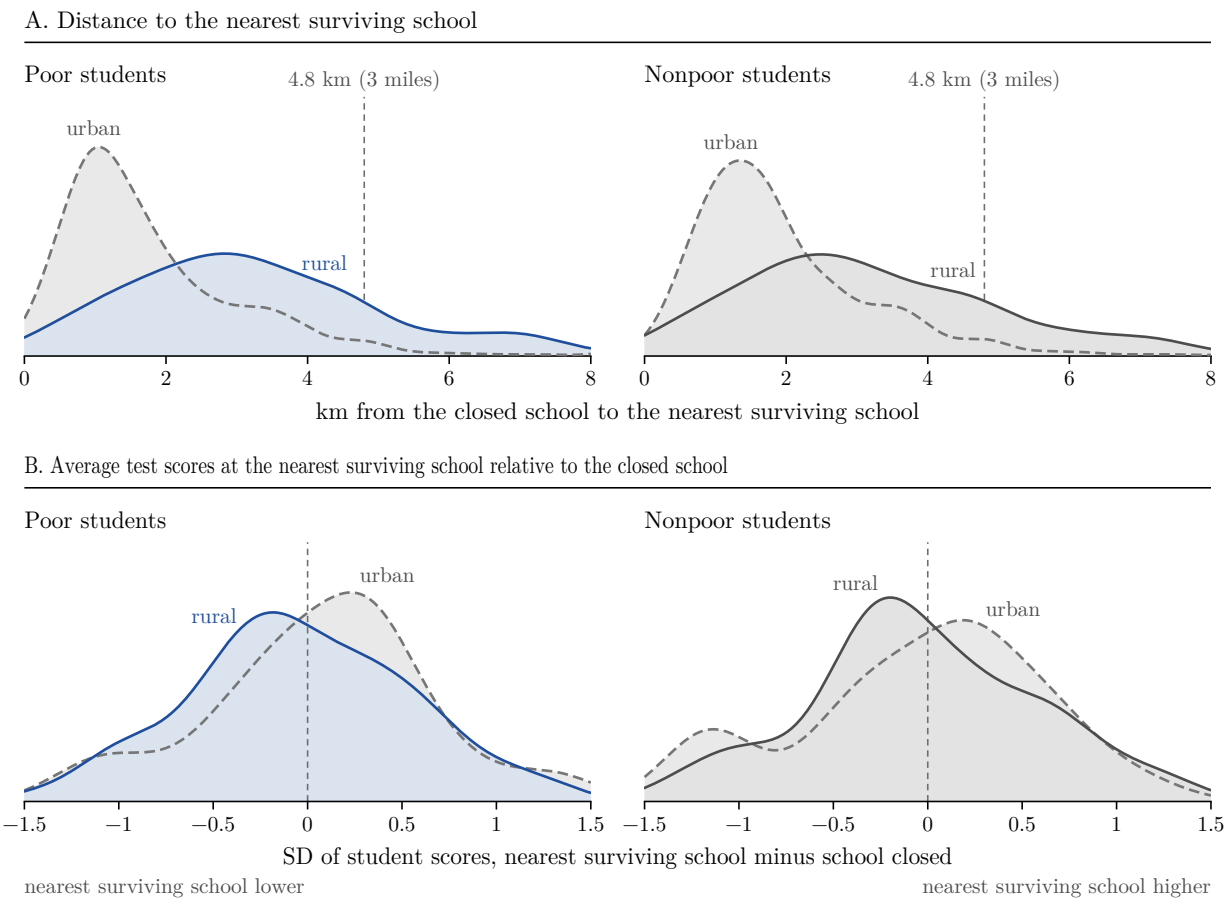
		By last operating year			
	2013-17 pooled	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)
Log enrollment the year before	−3.02	−3.41	−3.56	−2.60	−2.78
	(0.22)	(0.45)	(0.47)	(0.80)	(0.32)
Log km to the nearest other school	−0.09	−0.06	−0.20	0.30	−0.22
	(0.11)	(0.20)	(0.21)	(0.39)	(0.17)
Rural share near the school	−0.10	−0.12	−0.66	0.07	0.13
	(0.34)	(0.55)	(0.72)	(0.77)	(0.48)
Share with the family poverty record	0.37	0.93	1.32	0.08	0.43
	(0.77)	(1.62)	(1.44)	(2.63)	(0.78)
Closures	315	67	52	38	151
Schools in the choice sets	2,970	738	616	614	915

Panel B. Linear probability with the same fixed effects, share closed

		By last operating year			
	2013-17 pooled	2014	2015	2016	2017
	(6)	(7)	(8)	(9)	(10)
Log enrollment the year before	−0.143	−0.131	−0.109	−0.077	−0.274
	(0.011)	(0.017)	(0.018)	(0.018)	(0.029)
Log km to the nearest other school	−0.001	−0.002	−0.002	0.011	−0.014
	(0.004)	(0.010)	(0.006)	(0.008)	(0.015)
Rural share near the school	0.000	0.007	−0.025	0.011	0.011
	(0.014)	(0.030)	(0.029)	(0.026)	(0.049)
Share with the family poverty record	0.019	0.024	−0.003	0.019	0.049
	(0.029)	(0.070)	(0.062)	(0.049)	(0.066)
Closures	315	67	52	38	151
Schools in the choice sets	5,930	1,257	1,190	1,130	1,087

Notes. Closure of a public school on its characteristics the year before, among the schools open at the start of the year, closures of 2013-17. Panel A uses conditional logit within municipality and year, in log odds per unit of the regressor. Panel B uses linear probability with the same fixed effects. The pooled column and the years 2016 and 2017 also control for the change in log enrollment over the two years before, which the earlier years lack. Indicators for a missing regressor are included and not shown. Standard errors in parentheses are clustered on the municipality. The likelihood ratio test of the km row of the pooled logit gives $p = 0.388$, using 169 municipality years. 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).

Figure B1. The two margins across displaced students



Notes. Students displaced by the closures of 2013-17 (34,975, from 323 closed schools). A curve is a kernel density across a group's students. Panel A measures distance from the closed school, not from home, to the nearest surviving school of the municipality serving the grade (the straight line times the median road ratio of the school's class). Panel B gives that school's average test score minus the closed school's, each the year before the closure, in SD of student scores (34,191 students with an average score for both schools, with none for closures of 2013). Raw averages reflect a school's students as well as the school. Sampling noise is 2.3 percent of the variance of the difference. Off the axes lie 2 to 3 percent of students in Panel A and 2 to 4 in Panel B. The columns are alike because a closed school's poor and nonpoor students share its distance and schools. Panel B's means differ less than its peaks. Table 2 holds means and standard errors.

Table B2. Nearest Surviving School with and without Municipal Boundaries

	Mean distance (km)		Beyond 4.8 km (percent)	
	Own municipality	Across the island	Own municipality	Across the island
Rural poor	3.43 (0.16)	3.10 (0.15)	19.6 (3.0)	12.9 (3.1)
Rural nonpoor	3.37 (0.22)	3.03 (0.21)	20.4 (4.8)	14.0 (4.9)
Urban poor	1.91 (0.19)	1.77 (0.15)	5.1 (1.6)	2.3 (1.0)
Urban nonpoor	2.12 (0.18)	1.98 (0.15)	6.1 (1.9)	3.0 (1.4)
All others	2.15 (0.19)	1.98 (0.15)	7.5 (1.9)	4.1 (1.4)
Rural poor − all others	1.29 (0.20)	1.12 (0.18)	12.2 (2.5)	8.9 (2.4)
Rural poor − urban nonpoor	1.31 (0.22)	1.12 (0.20)	13.5 (3.1)	9.9 (3.0)

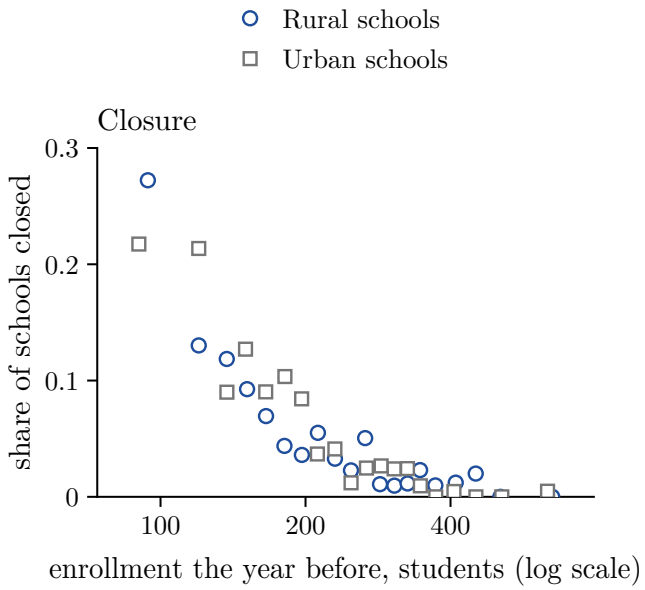
Notes. The same 34,975 students, represented by 1,619 school and grade records from 323 closed schools in 2013-17, enter both columns. Group weights use grade enrollment and the origin school's rural and poverty shares, as in Table 2. All others pools the three groups other than rural poor. Distance runs from the closed school to the nearest surviving school serving the grade, using km in a straight line multiplied by the same road factor for the origin school's class in both columns (Appendix A.4). The measure across the island removes only the municipal boundary restriction. Standard errors in parentheses are CR1, clustered by the closed school's municipality (76 clusters). As in Table 2, the standard errors for all others and its contrast with rural poor allow the mix of the pooled groups to vary. The final two rows are differences in means or percentage points. These distances measure geographic proximity rather than observed commuting or the choice model's kilometer equivalent access losses. The comparison does not impose limits on places at receiving schools.

B.4 Enrollment and Closure by Area

Figure B2 relates closure to enrollment separately for rural and urban schools. A school is rural when the rural share near it is at least one half. Closure falls with enrollment in both areas. In the smallest of the 20 bins, 27.2 percent of rural and 21.7 percent of urban school and year observations ended in closure, and almost none did in the largest.

The distance comparison in Figure 2, Panel B uses 5,861 school and year observations. The rural mean exceeds the urban mean in every enrollment bin, ranging from 3.45 to 4.49 km for rural schools and from 1.57 to 2.62 km for urban schools. Across all rural schools the mean is 3.89 km, against 1.93 km across urban schools. The slopes on log enrollment are −0.050 (0.183) and 0.291 (0.149), with closure year effects and standard errors clustered by municipality. These are associations rather than effects of school size.

Figure B2. Closure and enrollment in rural and urban schools



Notes. Public schools in 2013-17, with enrollment measured the year before. The 5,909 school and year observations are divided by area within the 20 enrollment bins of Figure 2. Each point gives the share ending in closure within a bin and area. Rural schools have a nearby rural share of at least one half, and urban denotes other schools. Appendix B.4 reports the companion distance comparison shown in Figure 2, Panel B.

C School and Teacher Value-Added

This appendix documents identification, estimation and robustness for the school and teacher value-added in Sections 6.1, 7.1 and 7.2. It also gives the uncertainty sensitivity for the recorded changes in school value-added and school effectiveness. The score model is estimated on the school years 2011-19, and the closures studied are those of 2013-17.

C.1 What Identifies What

Table C1 lists each ingredient of the two models, including the student effect, the teacher value-added, the school value-added, the distance coefficients by family type, the nest parameter, attachment to public schools and the responses to school effectiveness. For each it gives the fact of Section 4 that calls for it, the variation that identifies it and the check that bears on it, with the place where each appears.

C.2 The Connected Set

The production estimate inherits the input file's requirement for current and lagged scores in all three subjects, together with its filters that require a valid class and retain the largest class. This selection does not add lagged score controls to equation (1), which retains student effects. The input mainly covers grades 4 to 8 and contains almost no grade 3 observations. Applying estimated teacher value-added to grade 3 classrooms therefore also requires portability across grades. From this input, the estimation sample keeps the largest

Table C1. What identifies what

	Why the model needs it	Source of variation	Check
Student effect	An average test score mixes the students a school enrolls with what the school adds (Section 4.2).	A student's scores across years, teachers and schools.	Lagged scores in place of the student effect (Appendix C.5).
Teacher value-added	Teachers take their measured contributions to receiving schools (Section 5.1).	A student taught by different teachers with the student effect held fixed, and teachers who teach in more than one school.	Specification with lagged scores and bounds on drift and the match between teacher and school (Table 3).
School value-added	The gap in average test scores sits in the schools that closed (Section 4.2).	Students and teachers who change schools within the connected set they link. Closures add such moves (Table C2).	Residuals before closure (Figure 4), exclusion of each closure wave's last operating year and the year before (Table C5) or displaced students (Table C6), and two halves of the school years (Table C3).
Distance coefficients, by family type	Rural students were left farther from a surviving school, and most reenrolled students did not enter the nearest one (Section 4.3, Section 4.4).	Given the school mean utilities, the shares of reenrolled displaced students entering surviving schools at ranks from second to sixth or farther, by group.	Km to the school entered, not targeted (Table 5), and fit of the rank shares (Table D1).
Nest parameter	Students of closed schools left the municipality's public schools, more often from rural schools (Section 4.4).	Set so that the model's exit equals the exit effect of the event study, making it just identified.	Exit by the class of the closed school, not targeted (Table 5).
Attachment to public schools	Income poor and other families leave public schools at different rates.	The share of income poor students among public school students in the survey.	The fit of that share (Table D1).
Responses to school effectiveness, by family type	Poor and nonpoor displaced students entered schools of similar school effectiveness (Section 6.2).	Not separately, since the rural difference cannot be told apart from the rural difference in the distance coefficient. Both differences are set to zero, a restriction chosen after the results were seen.	The richer specification and a test that both differences are zero (Table D2).

Notes. The two models of Section 5 comprise student fixed effects, teacher value-added and school value-added in test scores, and a choice model of where displaced students enroll. Mobility provides the listed production variation. Separate identification of a reported component target also requires the estimability condition in Appendix C.2. The table adds no number to the text.

set of schools linked by a shared teacher and subject, and it requires at least two scored records for each student effect. I alternate the two restrictions until neither changes the sample, and I apply both again after removing observations with high estimated leverage.

Let Z contain the indicators for teachers by subject and for schools and let M_a absorb the student effects. For the retained design $\mathscr{Z} = M_a Z$, a linear target $c'\theta$, with $\theta = (\mu', \phi')'$, is identified only if $c'h = 0$ for every $h \in \ker(\mathscr{Z})$ remaining after the stated normalizations. A variance target or a difference between assignment objectives must likewise be invariant to these null space directions. Thus school graph connectedness supplies a sample restriction, not a certificate that every contrast separating school and teacher value-added is identified. A numerical null space check of the full sample normal matrix finds seven directions, comprising the two block constants and five additional directions in the teacher block. The latter involve 17 of the 19,484 teacher value-added coefficients by subject (0.09 percent), in five disconnected

components of the graph connecting students with teachers by subject, each at one school. They touch no school value-added, no teacher at a closer in its last operating year and no teacher at a destination in a move year. Contrasts of school value-added among the closers of 2013-17 and their destinations, school-average teacher value-added contrasts between a closer's last operating year and a destination's move year, and reassignment objective differences linear in those closer teachers' value-added are therefore invariant to these directions. Static school-average teacher value-added pooled over all years fails this check at fewer than ten destination schools, whereas closure comparisons use school-average teacher value-added for each school and year. The check covers the full sample solve, not designs based on split halves or other versions of the estimates, and does not certify the closure classes for 2018.

The date checks also find 435 scored rows after a school's recorded last operating year, at fewer than ten closers. Of these rows, 426 are two years later and nine are later still. These observations remain in the full sample estimates under their recorded school codes and years, so the estimates are conditional on those assignments despite the inconsistency with closure dates.

The computation absorbs the student effects by demeaning within student and solves the remaining normal equations with a ridge equal to 10^{-8} times their mean diagonal. Teacher value-added for each teacher and subject and school value-added for each school are centered separately at an unweighted mean of zero. The ridge stabilizes nearly singular directions, but at a finite value it also shrinks identified directions and is not merely a normalization. It cannot supply identifying variation for targets that depend on the null space. The smallest identified eigenvalue in the full sample check is 2.6×10^{-4} times the mean diagonal, implying shrinkage of at most 3.85×10^{-5} at the recorded ridge. Solving again at 0.01 and 100 times that ridge changes the four diagnostic linear contrasts by at most 8.5×10^{-6} and the uncorrected SDs by at most 5.44×10^{-5}, relative to the recorded solution at either multiplier. These contrasts use unweighted averages over schools or records of displaced students rather than the paper's weighted point estimates. A reassignment gain linear in the closer teachers' raw value-added, with absolute seat weights summing to at most one, changes by at most 2.8×10^{-5} at the smaller ridge and 2.8×10^{-3} at the larger one. These stability calculations apply to the diagnostic linear contrasts, uncorrected SDs and gains linear in raw teacher value-added. They do not establish stability across ridges for corrected variance components or reassignment gains evaluated at posterior means from empirical Bayes. The distinction between the ideal least squares identities and this implementation with a finite ridge is set out next.

Coverage is nearly complete (Table C2). School value-added is estimated for 97.5 percent of the 1,281 schools of 2013-17 in the enrollment extract and, in both halves of the school years, for 78.8 percent of them. Among the 334 closed schools the two shares are 97.3 and 72.8 percent. The schools with an estimate hold 98.2 percent of all students and 97.5 percent of the students of closed schools. On the two splits of the school years used in the paper, 1,018 and 1,037 of the 1,274 schools have an estimate in both halves, while a third split gives 1,013.

Table C2. The estimation sample and the connected set

Panel A. Effects estimated

	Schools (1)	Teacher by subject (2)
Estimation sample, full	1,274	19,484
Estimated in both halves, first split	1,018	8,722
Estimated in both halves, second split	1,037	8,912

Panel B. Schools of 2013-17 with estimated school value-added

	Schools (3)	With an effect, % (4)	In both halves, % (5)	Students covered, % (6)
All schools	1,281	97.5	78.8	98.2
Closed	334	97.3	72.8	97.5
Never closed	947	97.6	81.0	98.3
Rural	529	97.7	76.0	98.6
Urban	741	97.3	81.2	98.0
Closed, rural	165	98.2	73.3	98.2
Closed, urban	159	96.2	73.0	96.7

Notes. Panel A reports the model of test scores with student fixed effects, teacher value-added by subject and school value-added, school years 2011-19 (3,452,029 observations by student, subject and year for 395,030 students). Whole school years are assigned to two halves and the model is estimated on each. A unit counts when an estimate is available in both halves. Panel B covers schools of 2013-17. Covered students are those in grades with a standardized test whose school has estimated school value-added. School value-added is estimated on the set of schools connected by teachers and students who move. These counts describe coverage. The target rank check in Appendix C.2 applies to the full sample design rather than the designs for the two halves. The movers' graph has algebraic connectivity 0.0388, and 98.8 percent of the set stays connected when any one mover is removed.

C.3 Variance Components and the Two Splits

Write equation (1) in stacked form as $y = X\beta + e$, with $\beta = (a', \mu', \phi')'$, and let $H = (X'X)^{\dagger}X'$ under the normalization to minimum norm, $P = XH$ and $\hat{\beta} = Hy$. A variance component is a quadratic form $\theta_A = \beta' A \beta$, with its weights in the symmetric matrix A. The ideal least squares expression below requires this target to be estimable, which requires $Ah = 0$ for every remaining $h \in \ker(X)$, so that observationally equivalent effects give the same target. Given this condition, the design and an error with mean zero,

$$\mathbb{E}\big[\hat{\beta}' A \hat{\beta} \mid X\big] = \beta' A \beta + \operatorname{tr}\big(B_A \Sigma\big), \qquad B_A = H' A H, \quad \Sigma = \operatorname{Var}(e \mid X). \tag{C.1}$$

For the implemented regularized linear map, write $\hat{\beta}_\lambda = H_\lambda y$ and $S_\lambda = H_\lambda X$. At a finite ridge,

$$\mathbb{E}\big[\hat{\beta}_\lambda' A \hat{\beta}_\lambda \mid X\big] = \beta' S_\lambda' A S_\lambda \beta + \operatorname{tr}\big(H_\lambda' A H_\lambda \Sigma\big).$$

The first term need not equal θ_A. Correcting the trace term addresses estimation noise but does not by itself establish target estimability or remove shrinkage from a finite ridge. The ridge checks above measure stability of the diagnostic linear contrasts and uncorrected SDs without establishing stability of the corrected variance components in equation (C.2).

With independent errors, $\hat{\sigma}_r^2 = y_r \hat{e}_r/(1-P_{rr})$ estimates the error variance of record r (Kline et al., 2020). Here errors share shocks through $e_r = \nu_r + \zeta_{c(r)} + \eta_{j(r),t(r)}$, which includes a shock for each classroom and year and a shock for each school and year. For calibrated variances v_ζ and $v_{\eta,jt}$ of the two shocks, the corrected component is

$$\hat{\theta}_A = \hat{\beta}' A \hat{\beta} - \sum_r (B_A)_{rr} \hat{v}_{\nu,r} - v_\zeta \sum_c \mathbf{1}_c' B_A \mathbf{1}_c - \sum_{j,t} v_{\eta,jt}\, \mathbf{1}_{jt}' B_A \mathbf{1}_{jt}, \tag{C.2}$$

where $\hat{v}_{\nu,r}$ is the record's variance net of the two shocks and $\mathbf{1}_c$, $\mathbf{1}_{jt}$ select the observations for a classroom and year and for a school and year, respectively. Leverages and traces are approximated by random projections, so the calculation adapts the estimator that leaves out observations rather than deleting exactly every cluster defined by classroom and year or by school and year.

The variance of the classroom and year shock is set at $v_\zeta = 0.10018$. The variance of the school and year shock falls with school size, $\log v_{\eta,jt} = -0.9143 - 0.6072 \log n_j$, where n_j is the school's mean number of scored students per year, and is bounded above and below. Its reference value is 0.02218, from an empirical covariance adjusted for attenuation measured in simulated data. The unmasked variance targets for teacher value-added, school-average teacher value-added and school effectiveness depend on the five additional null directions. For the row corrected by leaving out observations, I therefore exclude the 17 affected teacher and subject pairs from the teacher variance and renormalize each school's teacher value-added weights over its remaining teachers. All 1,274 schools retain defined school-average teacher value-added, and the school value-added variance needs no exclusion. Recomputing the correction with the same random draws reproduces the original components before exclusion and changes the corrected SDs by $+0.00010$ for teacher value-added, $+0.00001$ for school-average teacher value-added and $+0.00003$ for school effectiveness. The masked targets give the same rounded SDs of 0.361 for school value-added, 0.225 for teacher value-added, 0.089 for school-average teacher value-added and 0.393 for school effectiveness. This agreement does not identify the original unmasked targets. Across the calibrations of the noise inputs previously examined, the first three run from 0.34 to 0.38, from 0.14 to 0.28 and from 0 to 0.15. These ranges describe the original unmasked targets rather than the masked central correction and are not confidence intervals (Table C3).

The second measure uses two halves of the school years. If $\hat{\phi}^A$ and $\hat{\phi}^B$ are estimated on separate halves, the school value-added is common to both and their estimation errors are uncorrelated, then $\mathrm{Cov}(\hat{\phi}^A, \hat{\phi}^B) = \mathrm{Var}(\phi)$. Splitting by school year keeps the same shock for each school and year out of both halves, although serially correlated shocks would still bias the covariance. The two splits give SDs of 0.303 and 0.307 for school value-added, 0.075 and 0.063 for school-average teacher value-added, and 0.203 and 0.191 for teacher value-added. The two measures differ in weights and in how they treat noise, so neither is a draw of the other. The covariance between school value-added and school-average teacher value-added is imprecise and changes sign across corrections, and I give it no sign.

Table C3. Variance components from the correction by leaving observations out and the two splits

Panel A. Standard deviation, correction by leaving observations out

	School value-added (1)	School-average teacher value-added (2)	Teacher value-added (3)
At the noise inputs used	0.361	0.089	0.225
Lowest across the noise inputs examined	0.34	0.00	0.14
Highest across the noise inputs examined	0.38	0.15	0.28

Panel B. Standard deviation, covariance across two halves of the school years

	School value-added (4)	School-average teacher value-added (5)	Teacher value-added (6)
First split	0.303	0.075	0.203
	(0.017)	(0.032)	(0.008)
Second split	0.307	0.063	0.191
	(0.017)	(0.036)	(0.010)

Notes. School value-added and teacher value-added of Table 3, SD of student scores. Panel A gives the correction by leaving observations out for estimation noise at the classroom by year and school year noise inputs used, with the school year variance falling with school size (log variance -0.9143 minus 0.6072 times log scored students). The central correction excludes 17 teacher value-added coefficients by subject in the unidentified components and renormalizes each school's teacher value-added weights over its remaining teachers, retaining all 1,274 schools. The calibration ranges describe the original unmasked targets, not confidence intervals. Panel B uses the covariance of the estimates on two halves of the school years, with two ways of splitting them and standard errors in parentheses. School effectiveness, the school value-added plus school-average teacher value-added, is 0.393 after the correction. The variance of school value-added is 2.6 times that of teacher value-added. The comparison excluding displaced students appears in Table C6, Panel A. The full sample rank check does not certify the split designs, and the split covariance rows retain their original target definitions.

C.4 Uncertainty for the Recorded Changes

A 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 nonpoor 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 applies to the larger sample for school value-added alone, while the common sample's school component, -0.101, is outside its scope. The calculation combines sampling and production estimation variances without their covariance. It provides a sensitivity calculation rather than established unconditional coverage or a guaranteed upper bound.

C.5 Check Using Lagged Scores

The lagged score specification replaces the student effects with controls for earlier scores as follows

$$Y_{ist} = X'_{ist}\gamma + \mu^{\mathrm{L}}_{k(i,s,t),s} + \phi^{\mathrm{L}}_{j(i,t)} + e^{\mathrm{L}}_{ist}, \tag{C.3}$$

where X_{ist} holds cubic terms interacted with grade in the previous year's mathematics, Spanish and English scores and the student's demographic characteristics. It uses grades 4 to 8 of the 2011-19 panel, since grade 3 has no earlier test score. Its assumption on assignment conditions on earlier scores instead of a permanent student effect (Chetty et al., 2014a), and because earlier scores reflect earlier schooling, its effects need not estimate the same object as equation (1). The teacher value-added estimates by subject of the two specifications correlate at 0.849, without correction for noise.

C.6 Out-of-Sample Forecast Tests

A student displaced at the end of year c left school o and entered school d in year $c+1$. Let ΔY_i be the student's score in $c+1$ minus the score in c, averaged over the subjects tested in both years, and $\Delta\hat{X}_i^h$ the change in an estimated input, d in $c+1$ minus o in c, on half h of the school years. Both halves exclude every displaced student's records from the move on. For each student, the clean half holds neither (o,c) nor $(d,c+1)$, so its estimation error is unrelated to the shocks in ΔY_i. The forecast coefficient is the two-stage least squares coefficient of ΔY_i on the other half's $\Delta\hat{X}_i$, instrumented by the clean half's, within closure year and grade. Standard errors are clustered by the closed school's municipality, and intervals invert a sign-flip Anderson–Rubin test over municipalities. The placebo uses the score change from $c-1$ to c at o, with a clean half that holds neither $(o,c-1)$ nor (o,c).

The instrument in columns (2) and (4) replaces the school entered with the menu the closure left: the enrollment-weighted mean of the clean half's estimates over the surviving public schools of the closed school's municipality that serve the student's grade, minus the clean half's estimate for the closed school. It does not use the school the family chose. Column (5) replaces the school-level change with the change in the student's own teacher's value-added in each subject, from the teacher at (o,c) to the teacher at $(d,c+1)$, within closure year, grade and subject.

C.7 Residuals before Closure, by Period

Figure C1 repeats the check of Figure 4 separately for the closures of 2013-16 and those of 2017 to assess variation across closure periods.

C.8 Without the Years of Closure Decisions

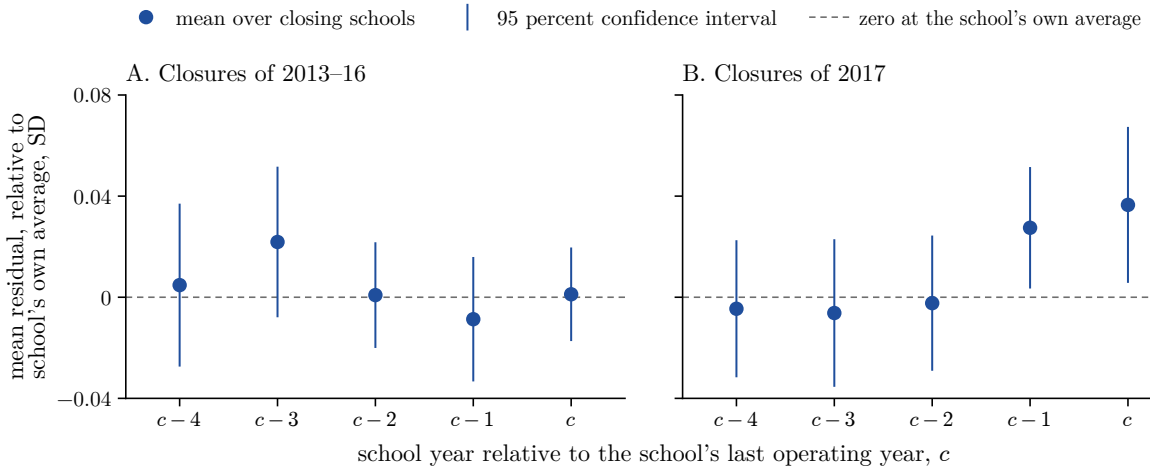
This version estimates the school value-added again separately for each wave of closures, leaving out the wave's last operating year and the year before. It applies the filters of the main estimates and admits a

Table C4. Out-of-sample forecast tests on displaced students

	School value-added		School effectiveness		Own teachers
Instrument	Clean half	Closure	Clean half	Closure	Clean half
	(1)	(2)	(3)	(4)	(5)
Panel A. Closures of 2013–17					
Forecast coefficient	0.894	0.662	0.718	0.576	1.32
	(0.124)	(0.184)	(0.088)	(0.102)	(0.46)
95% sign-flip interval	[0.64, 1.25]	[0.16, 1.15]	[0.52, 0.94]	[0.34, 0.79]	[0.55, 2.68]
First-stage F	28.8	14.7	61.7	40.4	29.9
Placebo coefficient	0.202	0.358^{w}	0.103	0.219	-1.02^{w}
	(0.210)	(0.357)	(0.152)	(0.205)	(0.84)
Observations	5,964	6,332	5,768	6,120	8,840
Closed schools	232	231	221	220	234
Municipalities	71	70	69	68	69
Panel B. Forecast coefficient by period					
Closures of 2013–16	0.936	0.605	0.758	0.558	0.94
	(0.148)	(0.190)	(0.109)	(0.126)	(0.40)
Closures of 2017	0.823	0.777^{w}	0.663	0.598	1.87^{w}
	(0.256)	(0.426)	(0.143)	(0.157)	(1.05)

Notes. Two-stage least squares of the change in a displaced student's test score across the move on the change in the estimated input, with the instrument named in each column; Online Appendix C.6 describes the design. Unbiased forecasts give a coefficient of one. Estimates use the two halves of the school years without displaced students' records from the move on. Columns (1)–(4) are within closure year and grade, and column (5) within closure year, grade and subject, with student–subject observations. Standard errors in parentheses are clustered by the closed school's municipality; intervals invert a sign-flip Anderson–Rubin test over municipalities. The placebo replaces the outcome with the score change in the year before the move. ^{w} First-stage F below 10; only the sign-flip test is read.

Figure C1. Checks of the school value-added, by closure period



Notes. Schools closed in 2013-17, by last operating year c (92 to 163 schools a year for the closures of 2013-16 and 144 to 153 for those of 2017). 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 model of test scores with 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 $p = 0.919$ for 2013-16 and $p = 0.012$ for 2017. Against the municipality's schools that never closed in the same year, $p = 0.834$ and $p = 0.040$. Figure 4 pools the periods. The estimates of the score model are in Table 3.

comparison only if at least 85 percent of its displaced records keep an estimate at both schools. The version covers 300 of the 312 closed schools in the comparison and 19,308 student records, 96.9 percent of the 19,927 of Table 4. Its school value-added estimates correlate with the full sample ones at 0.89 to 0.91. The 2013 wave falls below the floor. Table C5 sets each figure beside the full sample estimate on the same 19,308 records, with standard errors clustered by municipality.

Section 7.2 reports the contrast and the level for all displaced students, and the rest of the table agrees. The rural-poor level moves from −0.071 (0.033) with the full sample estimates to −0.130 (0.047) without the decision years, while the contrast with urban nonpoor students moves only from −0.091 (0.036) to −0.107 (0.054). The contrast with all other displaced students, −0.086 (0.042), has a 95 percent interval from −0.168 to −0.004 and a wild bootstrap p value over municipalities of 0.050. It sits in the term for the closed school, 0.120 (0.040) against 0.109 (0.029) with the full sample estimates, and not in the term for the entered school, 0.034 (0.021) against 0.026 (0.016).

Table C5. The change in school value-added without the last operating year and the year before

	Change, entered minus closed		School closed		School entered	
	Full sample (1)	Without decision years (2)	Full sample (3)	Without decision years (4)	Full sample (5)	Without decision years (6)
All displaced students	−0.021	−0.079	−0.019	0.023	−0.041	−0.056
	(0.023)	(0.033)	(0.025)	(0.036)	(0.015)	(0.019)
Rural poor	−0.071	−0.130	0.046	0.094	−0.025	−0.036
	(0.033)	(0.047)	(0.033)	(0.046)	(0.020)	(0.027)
Rural nonpoor	−0.023	−0.067	−0.007	0.020	−0.030	−0.047
	(0.035)	(0.054)	(0.038)	(0.054)	(0.026)	(0.036)
Urban poor	0.018	−0.043	−0.072	−0.030	−0.054	−0.073
	(0.025)	(0.035)	(0.030)	(0.041)	(0.016)	(0.018)
Urban nonpoor	0.021	−0.023	−0.079	−0.053	−0.059	−0.077
	(0.025)	(0.041)	(0.028)	(0.047)	(0.016)	(0.018)
Rural poor minus all other	−0.083	−0.086	0.109	0.120	0.026	0.034
	(0.029)	(0.042)	(0.029)	(0.040)	(0.016)	(0.021)
Rural poor minus urban nonpoor	−0.091	−0.107	0.125	0.148	0.034	0.041
	(0.036)	(0.054)	(0.035)	(0.052)	(0.021)	(0.027)
Student records	19,308					

Notes. Records of students displaced by closures of 2013-17 whose closed and entered schools have both estimates. There are 19,308 records, 96.9 percent of the weight of Table 4, from 300 of 312 closed schools. Without the decision years means the model is estimated again leaving out, for every school, that closure wave's last operating year and the year before. Both versions use the same records. Each school is measured relative to the municipality's mean school value-added among surviving schools, weighted by enrollment. Standard errors in parentheses are clustered on the closed school's municipality.

C.9 Without Displaced Students

This version leaves out displaced students' records from the year of their move on, so that their later test scores do not help estimate the school value-added of either the school closed or the school entered. Sections 7.1 and 7.2 report its main figures, and Table C6 gives the rest. The variance components change little (Panel A). This version covers 14,525 records, compared with 19,927 in Table 4. Its pooled standard errors hold group sizes fixed, as stated in Table C6. The contrast of −0.097 (0.041) between rural-poor and all other displaced students, with $p = 0.024$, sits in the term for the closed school, 0.103 (0.041), against 0.007 (0.021) in the term for the entered school (Panel B).

Table C6. School value-added estimated without displaced students' records

Panel A. Standard deviation and reliability

	School value-added (1)	School-average teacher value-added (2)	Teacher value-added (3)
SD across halves, full sample	0.303	0.075	0.203
SD across halves, without displaced students	0.311	0.078	0.198
Reliability, full sample (first split)	0.565	0.087	0.29
Reliability, without displaced students	0.549	0.084	0.26

Panel B. The change in school value-added, without displaced students' records, SD

	All (4)	Rural poor (5)	Rural nonpoor (6)	Urban poor (7)	Urban nonpoor (8)	Rural poor minus all other (9)	Rural poor minus urban nonpoor (10)
School closed	−0.006	0.055	0.026	−0.066	−0.032	0.103	0.087
	(0.034)	(0.046)	(0.054)	(0.038)	(0.047)	(0.041)	(0.056)
School entered	−0.066	−0.062	−0.055	−0.072	−0.065	0.007	0.003
	(0.019)	(0.024)	(0.031)	(0.023)	(0.025)	(0.021)	(0.028)
Change, entered minus closed	−0.059	−0.117	−0.081	−0.006	−0.033	−0.097	−0.084
	(0.033)	(0.045)	(0.044)	(0.036)	(0.055)	(0.041)	(0.063)

Notes. Panel A follows Table 3, with the model estimated on halves that leave out displaced students' records from the year of the move on. Panel B uses the student records of Table 4 (14,525 student records with both schools' estimates, 14,523 in column (10)), each school relative to the municipality's mean school value-added among surviving schools, weighted by enrollment. Standard errors in parentheses are clustered on the closed school's municipality. In the pooled columns they hold the groups' sizes fixed.

C.10 The Change in School Value-Added by Closure Year

Table C7 gives the change in school value-added by group and its two contrasts for each closure year, providing a comparison across waves alongside the pooled estimates of Section 7.2.

C.11 The Spread across Students, Net of Noise

The group means of Table 4 sit inside a wide spread across students (Table C8). Net of estimation noise, the SD of the change in school value-added across displaced students is 0.42, with a 95 percent interval from

Table C7. The change in school value-added by closure year

	Displaced students				Difference		
	Rural poor (1)	Rural nonpoor (2)	Urban poor (3)	Urban nonpoor (4)	Rural poor minus all other (5)	Rural poor minus urban nonpoor (6)	Student records (7)
2013	−0.099	0.028	−0.015	0.170	−0.147	−0.269	291
	(0.178)	(0.078)	(0.131)	(0.090)	(0.174)	(0.187)	
2014	−0.047	−0.013	−0.063	−0.024	0.006	−0.023	4,254
	(0.069)	(0.062)	(0.090)	(0.066)	(0.087)	(0.077)	
2015	−0.098	−0.007	0.179	0.199	−0.251	−0.297	3,181
	(0.089)	(0.091)	(0.049)	(0.063)	(0.088)	(0.103)	
2016	0.029	0.017	0.108	0.072	−0.061	−0.044	2,323
	(0.066)	(0.074)	(0.061)	(0.053)	(0.054)	(0.063)	
2017	−0.117	−0.060	−0.034	−0.042	−0.078	−0.075	9,933
	(0.048)	(0.052)	(0.033)	(0.032)	(0.045)	(0.053)	
2013-17, pooled	−0.079	−0.032	0.011	0.018	−0.084	−0.097	19,927
	(0.036)	(0.035)	(0.027)	(0.025)	(0.031)	(0.037)	

Notes. The change in school value-added, school entered minus school closed (full sample estimates), by the closed school's last operating year. Each school is measured relative to the municipality's mean school value-added among surviving schools, weighted by enrollment. Student records are the year's records of displaced students with both schools' estimates. Standard errors in parentheses are clustered on the closed school's municipality. Year rows count the 19,982 records with both schools' estimates, whereas the pooled row is Table 4's 19,927.

0.33 to 0.52. Relative to the average surviving school of the municipality, the SD of the school entered is 0.26 [0.19, 0.32] and that of the school closed 0.33 [0.21, 0.42]. The calculation keeps the schools that meet a coverage floor of 70 percent.

C.12 School Entered against School Closed

Figure C2 plots the term for the entered school against the term for the closed school for each closed school and wave, with the two terms estimated on separate halves of the school years so that they share no estimation noise. Across 233 of the 312 closed school and wave pairs, the slope is −0.04, with a 95 percent interval from −0.41 to 0.17. The school entered, relative to the municipality's average surviving school, does not rise detectably with the school closed.

Table C8. The spread of school value-added across displaced students, net of estimation noise

Panel A. Change, school entered minus school closed

	Raw SD (1)	Net SD (2)	Coverage, percent (3)
All displaced students	0.45	0.42 [0.33, 0.52]	73
Rural poor	0.50	0.45 [0.31, 0.58]	73
Rural nonpoor	0.45	0.44 [0.32, 0.56]	71
Urban poor	0.41	0.38 [0.30, 0.48]	74
Urban nonpoor	0.42	0.42 [0.31, 0.54]	74
Students		14,538	

Panel B. School closed, relative to the average surviving school

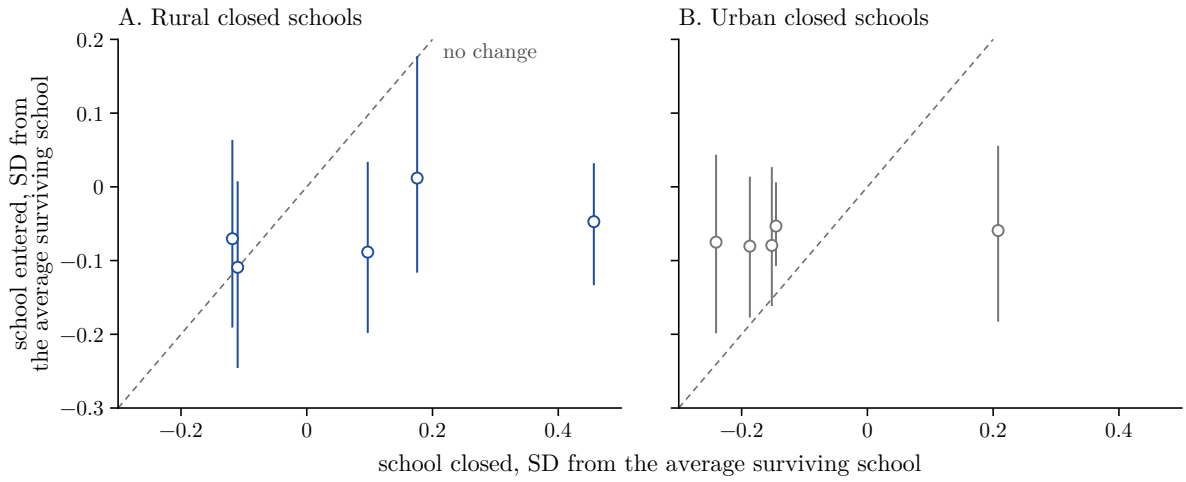
	Raw SD (4)	Net SD (5)	Coverage, percent (6)
All displaced students	0.40	0.33 [0.21, 0.42]	80
Rural poor	0.43	0.35 [0.18, 0.46]	79
Rural nonpoor	0.43	0.38 [0.22, 0.51]	77
Urban poor	0.35	0.28 [0.20, 0.37]	81
Urban nonpoor	0.34	0.31 [0.22, 0.40]	80
Students		15,855	

Panel C. School entered, relative to the same average

	Raw SD (7)	Net SD (8)	Coverage, percent (9)
All displaced students	0.32	0.26 [0.19, 0.32]	89
Rural poor	0.34	0.27 [0.17, 0.34]	90
Rural nonpoor	0.33	0.24 [0.15, 0.31]	90
Urban poor	0.31	0.26 [0.19, 0.34]	89
Urban nonpoor	0.28	0.24 [0.14, 0.31]	90
Students		17,788	

Notes. Students displaced by closures of 2013-17 with a recorded next school, in the weights of Table 4. Raw SD is the standard deviation across students of the full sample estimate. Net SD is the square root of the covariance between two estimates of the same quantity, each from one half of the school years with displaced students left out. Coverage gives the percent of the group's weight with both halves for every school involved. Brackets give 95 percent intervals from a bootstrap over municipalities.

Figure C2. The school entered against the school closed, split sample bins



Notes. Closed schools of 2013-17 with estimates on both halves of the school years (233 school waves, 75 percent). School value-added, in test score SD, is relative to the municipality's average surviving school, weighted by enrollment. Closed schools are put in five bins by their school value-added on one half of the school years. Each circle is the bin's mean for the school closed and for the school its students entered on the other half, with the two directions averaged. Lines give 95 percent intervals for the school entered, from a bootstrap over municipalities. The dashed line is the line at 45 degrees of no change. The split sample slope of the school entered on the school closed is −0.04 [−0.41, 0.17] for all closed schools, −0.02 [−0.50, 0.34] for rural schools and −0.10 [−0.68, 0.34] for urban schools. The comparison holds only across halves and only if closures were not chosen on the scores behind the estimates.

D The Choice Model

This appendix documents the moments, estimation, fit and sensitivity of the choice model in Sections 6.2 and 7.3, together with the check of its placements on the realized closures. The model is equation (2), with parameters $(\widetilde{\lambda}_0, \widetilde{\lambda}_R, \widetilde{\lambda}_P, \pi, \rho)$, and the school mean utilities are recovered from enrollment shares at each candidate value.

D.1 Moments, Estimation and Fit

The rank moments describe where the students of a closed school went. For a family of type z in market m, removing the closed school o alone gives

$$K_{moj}(z;\theta) = s_{mj}\big(z;\mathscr{J}_m \setminus \{o\},\theta\big) - s_{mj}\big(z;\mathscr{J}_m,\theta\big), \tag{D.1}$$

which, under Assumption 2, is the probability that the family attended o and, once o closes, enters j. Let $b(o,j)$ be the distance rank of the surviving school j from o, and $\mathscr{B}_{mo}$ the surviving schools eligible as destinations, which exclude other schools closing in the same market. For group g, with poverty status p_g, let $\gamma_g(o)$ be the rural share of the closed school's surroundings, $A^R(o)$, for the rural groups and $1-A^R(o)$ for the urban groups, and let $a_g(o,z) = \gamma_g(o)\,\mathbf{1}\{P = p_g\}$. With origin weights $\omega_{mo} = n_{mo}/S_{mo}$, the ratio of the

origin's displaced count to its enrollment share, the model's share of group g entering a school of rank b is

$$M_{gb}(\theta) = \frac{\sum_{m,o} \omega_{mo} \int a_g(o,z) \sum_{j \in \mathscr{B}_{mo}} \mathbf{1}\{b(o,j) = b\} K_{moj}(z;\theta)\, dF_m(z)}{\sum_{m,o} \omega_{mo} \int a_g(o,z) \sum_{j \in \mathscr{B}_{mo}} K_{moj}(z;\theta)\, dF_m(z)}. \tag{D.2}$$

Attendance at the closed school is itself a choice. In equation (D.1) a family's weight among a closed school's students is its probability of having attended that school, so each closed school's families are not simply the residents of its block groups. The ranks are 2, 3, 4 to 5, and 6 or more, in each of the four groups, with the share at rank one implied. The recorded shares apply the same restrictions to the students who enrolled again. The rank moments remove one closed school at a time, while the exit moment and the access loss remove all of a market's closed schools together. In markets with several closures the two need not give the same destinations, and the rank moments maintain the approximation that removes a single school.

The composition moment is the income poor share of public school students,

$$M^{\text{comp}}(\theta) = \frac{\sum_m M_m \int I \left(1 - s_{m0}(z;\theta)\right) dF_m(z)}{\sum_m M_m \int \left(1 - s_{m0}(z;\theta)\right) dF_m(z)}, \tag{D.3}$$

with M_m the market's population. Its target, from the survey's microdata for 2013-17, is 0.6706 (0.0036). Income poverty here differs from the family poverty record that defines the groups.

The exit moment sets the nest parameter. Let C_m be the closing schools of market m and ω_m^E its weight in the exit sample. The model's exit per student of a closing school is

$$A(\theta) = \frac{\sum_m \omega_m^E \int \left[s_{m0}(z;\mathscr{J}_m \setminus C_m) - s_{m0}(z;\mathscr{J}_m)\right] dF_m(z)}{\sum_m \omega_m^E \int \sum_{o \in C_m} s_{mo}(z;\mathscr{J}_m)\, dF_m(z)}. \tag{D.4}$$

Its target $\hat{\tau}$ is the exit effect in the year after a closure from the matched event study of Appendix A.6. It is 8.5 percentage points (1.5), of which 3.2 (0.9) come from leaving the public records and 5.3 (1.2) from entering another municipality's public school. The target is an effect on excess exit, not the exit rate. It is an estimating input to the choice model, distinct from the broader enrollment and achievement results of the companion study (Bobonis et al., 2026). A market whose public schools all close has a model exit of one whatever the parameters, so it carries no information on ρ.

Given ρ, the four other parameters minimize a weighted distance between the 17 moments and their targets. The rank discrepancies are weighted by the inverse of their bootstrap variances, clustered by municipality, and the composition discrepancy by 100 times its inverse variance. The nest parameter then solves $A(\hat{\vartheta}(\rho),\rho) = \hat{\tau}$, with the four parameters estimated again at each candidate ρ. Writing $g(\vartheta,\rho)$ for the 17 discrepancies and G_ϑ for their derivative, the standard errors come from the stacked equations

$$h(\theta) = \begin{pmatrix} G_\vartheta' W g(\vartheta,\rho) \\ A(\vartheta,\rho) - \hat{\tau} \end{pmatrix}, \tag{D.5}$$

with the full derivative $\partial h/\partial \theta'$, which keeps the terms that multiply nonzero discrepancies (Newey, 1984, Hall and Inoue, 2003). The calculation sets the covariance between the rank targets, the composition target and the exit target to zero. This is an approximation because the rank and event study samples are related. The population weights, the market sizes, the distances, the distance power and the weighting matrix are held fixed. The nest parameter is 0.847 (0.031), and inverting the exit equation gives a confidence set from 0.783 to 0.904.

Table D1 sets the 16 rank shares beside the model's, with the fit discussed in Section 7.3. The criterion is 26.90 for 17 moments and four parameters given ρ. The test based on the first order approximation of the overidentifying restrictions gives an approximate p value of 0.038 from its simulated null distribution of a weighted quadratic form. It is conditional on treating the estimated covariance of the targets as known. A change of one percentage point in any share entering the second nearest school moves the ratio of the rural-poor to the urban nonpoor access loss by less than 0.01 to first order.

Table D1. Data and model distance ranks of the school entered among surviving schools

	Rural poor		Rural nonpoor		Urban poor		Urban nonpoor	
	Data (1)	Model (2)	Data (3)	Model (4)	Data (5)	Model (6)	Data (7)	Model (8)
Nearest surviving school	44.6 (2.4)	49.7	37.2 (3.7)	46.9	38.7 (2.9)	42.2	37.9 (3.2)	40.4
Targeted								
Second nearest	26.9 (2.1)	21.4	28.4 (3.4)	20.5	21.8 (1.6)	19.5	22.1 (2.1)	19.1
Third	9.8 (1.0)	11.1	9.2 (1.5)	12.0	12.5 (1.6)	10.8	11.7 (1.5)	11.2
Fourth or fifth	11.7 (1.3)	10.9	16.3 (2.3)	12.3	12.1 (1.5)	12.3	11.5 (2.4)	12.7
Sixth or farther	7.0 (0.9)	6.9	8.8 (1.6)	8.3	14.9 (1.9)	15.2	16.7 (2.8)	16.5

Notes. Percent of reenrolled displaced students whose school entered has each rank in distance from the closed school among the municipality's surviving schools serving the grade. Data gives recorded shares with standard errors in parentheses, clustered by municipality, those of the estimation's targets (for the nearest school, Tables 2 and 5 print the student records' own, at most 0.1 point apart). Model gives the choice model's prediction at the estimates. The sixteen shares below the first row are targeted moments and the first row is their complement. A composition moment completes the seventeen. The first order test of the overidentifying restrictions, treating the target covariance as known, gives $p = 0.038$. Groups use the closed school's rural share and the student's poverty record.

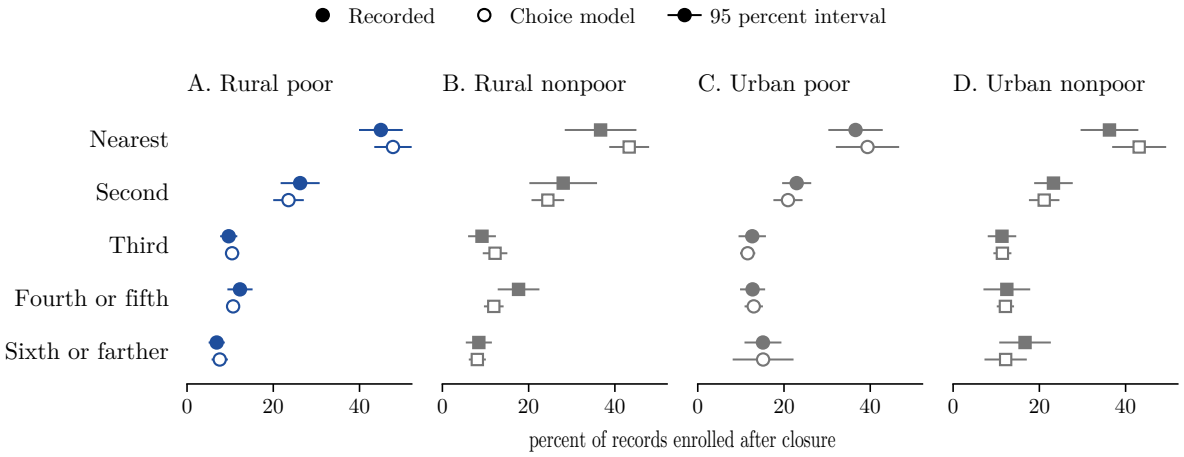
D.2 Rank of the School Entered

Figure D1 compares, by group, the recorded distance rank of the school a student who enrolled again entered with the rank the model predicts. The pattern of the moments is that in every group the model puts too many students in the nearest surviving school and too few in the second nearest.

Students of a closed school concentrate at one receiving school more than the model's probabilities imply. For each closed school, grade and year, I take the surviving school that the other grades of that closed

school in the same grade span enter most in excess of the model. Among the 14,034 displaced students who enrolled again inside the closed school's set of alternatives and for whom that school is defined (62 percent of the displaced students who enrolled again in the choice model's extract), 46 percent entered it, against a model probability of 21 percent, and the excess appears in all four groups. Assignment to a receiving school would produce this pattern. So would bus routes or siblings in other grades, an error in where the model places families, or an error in the distance coefficient, which the records do not separate. Estimating again with that school removed from both the recorded and the model's destinations, with the nest parameter held, leaves 57 percent of the sample and gives a ratio of the rural-poor to the urban nonpoor access loss of 2.04, against 1.90 on the same 805 markets (Appendix D.5). This concentration diagnostic and the masked estimation were added after the model's misfit was seen.

Figure D1. Recorded and predicted distance ranks of the school entered



Notes. The 13,058 records of students displaced by closures of 2013-17 who reenrolled in a surviving public school of their municipality and have a value in the choice model. Rows give the distance rank of the school entered among the municipality's surviving schools serving the grade, measured from the closed school. Filled marks show the recorded share and hollow marks the choice model's predicted share for the same records. Both weight by records, so they differ from the targets of Table D1, which weight by students. Lines give 95 percent intervals, with standard errors clustered by municipality. Circles identify poor students and squares nonpoor students.

D.3 Variants and the Responses to School Effectiveness

Write the response within the nest to school effectiveness as $\beta_0 + b_R R_i + b_P P_i$, comprising a response β_0 common to all families, and rural and poor differences b_R and b_P. Table D2 sets the coefficients and the access ratio of the specification of Table 5 beside two variants that free these responses.

The common response β_0 is not a separate parameter. It is part of the school mean utilities δ_{mj}, which are recovered from enrollment shares and also hold every other attribute that all families value alike. A common coefficient on school effectiveness could be separated from the school mean utilities only by a further restriction on how school effectiveness moves with those other attributes. A projection of the school mean utilities on lagged school effectiveness within markets bounds the common response from above. The

projection mixes the response with other attributes that all families value and with noise in the regressor. If, within a market, the attributes families value besides effectiveness are not negatively related to it, the slope divided by a lower bound on the within-market variance of effectiveness net of noise is an upper value for the response averaged over the four family types. On 15,393 school-grade-years in 77 municipalities this slope is 0.319 per unit of effectiveness, with a 95 percent upper limit of 1.03. At the distance coefficients of Table 5, measured from a school 3.9 km away, the upper limit equals 0.98 km of travel per SD of effectiveness for rural-poor families and 0.62 km for urban non-poor families. Because differences in estimated effectiveness forecast 58 to 72 percent of the corresponding differences in score changes (Section 6.1), the limit for rural-poor families is 1.4 to 1.7 km per SD of test scores. School-average teacher value-added alone gives no such bound, since the lower confidence limit of its within-market variance net of noise is not positive.

The two differences are estimated, and neither is informative. Estimated in a second step, given the first step's coefficients, the poor difference is $b_P = -0.063$ (0.398), with a 95 percent interval from -0.84 to 0.72. The rural difference is $b_R = -3.85$ (4.52), and its profile confidence set does not close at the lower end of the searched range, -10, and has an upper end of 3.0. A test that both differences are zero gives a score of 1.06 and $p = 0.59$. Estimated jointly with the distance and attachment coefficients, with b_R held at zero, the poor difference is -0.061 (0.345), with an interval from -0.74 to 0.62. Iterating the two steps to a common solution gives $b_R = -4.41$ and $b_P = -0.06$, with an access loss ratio of 1.78 for rural-poor relative to urban nonpoor students on the 28,024 students in 885 markets. The curvature check fails at that solution, so the ratio is reported as a point without an interval.

The restriction $b_R = b_P = 0$ was chosen after the results were seen. The response differences are weakly identified, the poor difference is imprecise and the test does not reject zero, without establishing that families do not respond to school effectiveness. Under the restriction no estimate of school value-added enters the access numbers, so noise in those estimates does not reach them. Appendix D.5 reports access sensitivity, and Appendix E.2 reports placements when the rural response difference is not zero.

D.4 Checks before Placing Students

Table D3 reports in full the check of Section 6.2. The error for each record 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, averaged over the model's probabilities. Both use estimates fitted on separate halves, so school value-added in the year compared comes from the half of the school years that does not contain that school year. The error is measured on 13,058 records of students who enrolled again in 73 municipalities and regressed, without weights, on the closed school's size band, its km to the nearest surviving school and the log number of surviving schools. For each list of Section 8, the predicted change in the error is the difference between that list's mean features and the realized closures', per displaced student, times the coefficients. Its standard error is the larger of the estimate clustered by municipality and the estimate from a municipality bootstrap, with the choice model estimates held fixed.

Table D2. The choice model under three treatments of the responses to school effectiveness

	Restricted (estimate) (1)	Response held at a first pass value (2)	Both steps iterated (3)
Coefficients			
Distance,	−2.785	−2.697	−3.053
urban nonpoor family	(0.577)	(0.505)	(1.454)
Distance,	1.219	1.146	1.251
rural family, difference	(0.637)	(0.569)	(1.835)
Distance,	−0.202	−0.201	−0.206
poor family, difference	(0.094)	(0.093)	(0.089)
Nest parameter	0.847	0.846	0.859
	(0.031)	(0.031)	(0.034)
Attachment of an income poor family	1.411	1.409	1.413
	(0.062)	(0.061)	(0.074)
Implied			
Rural over urban cost	0.56	0.58	0.59
of distance (nonpoor)	(0.15)	(0.15)	(0.42)
Access loss ratio,	1.84	1.81	1.78
rural poor over urban nonpoor	(0.16)		(0.35)

Notes. Nested logit of the school a displaced student entered (Table 5). Column (1) restricts responses to school effectiveness not to differ by family type and gives the estimates of the paper. Column (2) holds the responses at the first pass of estimation in two steps. Column (3) iterates both steps to a fixed point, where the rural and poor responses are −4.41 and −0.06. Standard errors in parentheses use first order approximations. In (3) the two first order formulas differ by more than a quarter for the distance coefficients and the ratios, and no interval is formed. The ratio in (2) is a point. The access loss ratio covers the 28,024 displaced students of Table 6.

The largest predicted change across the five lists, for all displaced students and for rural-poor students, is 0.007 SD. The thresholds were set after the model's error on the realized closures was known. With a largest change of at most 0.03 SD, differences between lists in where the model places students are read against a tolerance, and above 0.06 SD none would be read. Each list's tolerance is the larger of 0.02 SD and the largest predicted change plus two standard errors, over the features of the check and over the same features with the closed school's average test score and enrollment added. For rural-poor students' school value-added the tolerances run from 0.020 to 0.033 SD, while those for school effectiveness are 0.020 SD. These tolerances are sensitivity allowances informed by the regressions for the realized policy, not proven bounds on errors under alternative policies. In particular, the regressions do not bound changes operating through unmeasured features. The interpretation of the intervals and simultaneous bound in Appendix E.5 requires the allowance to cover the true differential model error for each list. The features do not jointly predict the error ($p = 0.70$ for school value-added and 0.89 for school effectiveness). The table also reports the error itself, for all displaced students and for rural-poor students. In school effectiveness it is −0.022 (0.015) and −0.031 (0.018). Two mechanical rules that place each student at the nearest surviving school or by the recorded rank shares give errors in school effectiveness of −0.053 (0.027) and −0.025 (0.014) for all displaced students. These are comparisons and play no part in the check.

Table D3. Check of the choice model's placements on the realized closures

Panel A. The model's error on the realized closures, SD

	School value-added		School effectiveness	
	All	Rural poor	All	Rural poor
	(1)	(2)	(3)	(4)
School entered, recorded minus model	−0.026	−0.041	−0.022	−0.031
	(0.020)	(0.024)	(0.015)	(0.018)

Panel B. Predicted change in the error under each list, SD

	School value-added		School effectiveness	
	All	Rural poor	All	Rural poor
	(5)	(6)	(7)	(8)
Smallest first	−0.002	−0.003	0.002	0.002
	(0.003)	(0.005)	(0.002)	(0.003)
Smallest first, alternative within 4.8 km	0.002	0.003	0.000	−0.001
	(0.002)	(0.003)	(0.001)	(0.002)
Lowest average test scores first †	−0.002	−0.002	0.000	0.000
	(0.003)	(0.004)	(0.002)	(0.002)
Lowest scores first, within 4.8 km †	0.001	0.003	−0.001	−0.001
	(0.003)	(0.003)	(0.002)	(0.002)
Nearest alternative first	0.006	0.007	−0.002	−0.001
	(0.008)	(0.009)	(0.005)	(0.006)

Panel C. Tolerance for the model's error, by list, SD

	School value-added		School effectiveness	
	All	Rural poor	All	Rural poor
	(9)	(10)	(11)	(12)
Smallest first	0.026	0.026	0.020	0.020
Smallest first, alternative within 4.8 km	0.027	0.027	0.020	0.020
Lowest average test scores first †	0.027	0.027	0.020	0.020
Lowest scores first, within 4.8 km †	0.020	0.020	0.020	0.020
Nearest alternative first	0.027	0.033	0.020	0.020

Notes. The 13,058 reenrolled records of students displaced by closures of 2013-17 with a model value. Panel A gives the effect of the school recorded less that of the school the model places a student in. In Panel B, the model's error is regressed on the closed school's size band, its km to the nearest surviving school and the number of surviving schools. Applied to each list's closures, the features predict the change shown. The largest change in absolute value is 0.007 SD, under the threshold of 0.03 set for this check, which was set after the model's error on the realized closures was known. Panel C takes the larger of 0.02 and the predicted change plus two standard errors over three versions of the regression. Standard errors in parentheses are the larger of clustered and bootstrap, by municipality. The † mark follows Table 7.

D.5 Access Loss under Other Assumptions

Table D4 repeats the access loss of Table 6 under other assumptions, on the same 28,024 students. Across the specification of Table 5 and four variants (the joint estimation with b_R held at zero or at either end of its interval, and the two steps iterated), the ratio of the rural-poor to the urban nonpoor loss runs from 1.78 to 1.85. Holding the first step fixed, a separate sensitivity exercise evaluates eight combinations of the response differences, using the rural range conditional on the assumptions $[-4.34, 2.35]$ and the recorded endpoints for the poor response. The ratio runs from 1.74 to 1.84 at these points. The rural sensitivity range is not the profile confidence set above. Over the 15 admissible points of the confidence sets for the rural difference in the distance coefficient, it runs from 1.4 to 2.1, and the difference between the two groups' losses is at least 0.47 kilometer equivalents. The ratio is above one at every admissible point evaluated. These are ranges over evaluated points, not confidence intervals.

Counting only the 26,210 displaced students in the 805 markets where displaced students' next school is recorded, the ratio is 1.90 (0.18), ranging from 1.83 to 1.91 across the variants, 1.79 to 1.90 at the ends of the intervals, and 1.4 to 2.2 over the confidence sets. The two counts agree exactly on the markets both include, while the other 80 markets raise urban students' loss per student. On these 805 markets the ratio in units of utility within the nest is 1.18 at the estimates and runs from 0.78 to 1.71 over the points of the confidence set for the rural difference evaluated on this base, so the ordering in utility is not determined, while the ratio in kilometer equivalents stays above one. Each displaced family's loss is at least one unit of utility within the nest whatever the map. At the estimates the closed schools' lower school mean utilities offset most of the part that does not depend on distance, so the loss in kilometer equivalents is mostly its distance part, at 1.86 kilometer equivalents for rural-poor students against a total of 1.83, and 0.90 against 0.96 for urban nonpoor students, a ratio of 2.05 in the distance parts.

The standard errors of Table 6 come from the delta method in the five coefficients. Along a direction of the coefficients' covariance, the ratio's response at one and at 1.96 standard errors stays within the tolerance of its linear approximation, so the local calculation is adequate at the main estimates.

D.6 Populations

Table A2 reconciles the enrollment counts, the students with recorded destinations and the model's displaced population. The choice model classifies families by the rural indicator of their block group and their family poverty record, whereas the enrollment comparisons use the closed school's rural and poor shares. Its group populations therefore differ from those of the raw distance and recorded school comparisons.

Table D4. Access loss under other assumptions

Panel A. As estimated

	Km equivalents per displaced student				Rural poor and urban nonpoor	
	Urban nonpoor (1)	Urban poor (2)	Rural nonpoor (3)	Rural poor (4)	Ratio (5)	Difference (6)
28,024 students of 885 markets	1.00	0.97	1.91	1.84	1.84	0.84
	(0.09)	(0.08)	(0.15)	(0.11)	(0.16)	(0.12)
26,210 students of 805 markets	0.96	0.93	1.90	1.83	1.90	0.87
	(0.09)	(0.08)	(0.15)	(0.11)	(0.18)	(0.12)

Panel B. Ranges over evaluated points

	Ratio, 28,024		Difference, 28,024		Ratio, 26,210	
	Lowest (7)	Highest (8)	Lowest (9)	Highest (10)	Lowest (11)	Highest (12)
The specification and four variants (5 points)	1.78	1.85	0.76	0.84	1.83	1.91
Ends of the intervals of the responses (8 points)	1.74	1.84	0.78	0.84	1.79	1.90
Confidence sets for the rural difference in the distance coefficient (15 points)	1.42	2.12	0.47	0.99	1.44	2.22

Notes. Closures of 2013-17. Access loss per displaced student of Table 6, by group, with standard errors in parentheses from the delta method in the five coefficients. The 805 markets are those where displaced students' next school is recorded. The two counts agree on the markets both include. Panel B holds the rural difference in the response to school effectiveness at zero or at either end of its interval. The ends set both differences in the response at the ends of their intervals with the first step fixed. These are ranges over evaluated points, not confidence intervals. The iterated solution and its curvature qualification appear in Table D2 and Appendix D.3. The ratio and difference compare rural poor with urban nonpoor.

E Definitions and Further Results for the Counterfactuals

This appendix supports Section 8. It defines the lists and what every comparison holds fixed (Appendix E.1), reports further results at the Department's counts (Appendix E.2), for closing fewer schools (Appendix E.3) and with the closures held fixed (Appendix E.4), and sets out how the standard errors and the statements of direction are computed (Appendix E.5).

E.1 Definitions, What Is Held Fixed, Seats and Coverage

Appendix D.6 describes the three populations of the comparisons. Distances use the 34,975 displaced students, including the 938 students of the held closures. The change in school value-added before students sort again uses the 34,037 students of the 315 closures chosen again, while the access calculation using model weights represents 28,024 displaced students. Placed school value-added comparisons instead use origins weighted by enrollment, with a covered weight of 32,398 under the realized closures. The account of savings uses each closed school's total enrollment in its last operating year, totaling 46,348 students in the 315 closures, or 147.1 per closure.

Table E1 gives the operational rules. The comparable pool excludes specialized schools, the closures

held fixed as background and protected earlier receiving schools. Schools must have eligible grade spans, belong to the estimation core and satisfy the year's cap on enrollment size. The priority for an alternative within 4.8 km is not a guarantee because other schools complete the count when too few qualify. About a tenth of displaced rural-poor students still have no surviving school within 4.8 km under these lists. The score rules rank the previous year's average on one half of the school years and evaluate it on the other. Schools with fewer than 10 scored students on the ranking half take their municipality, year and size band's median, with ties broken by enrollment. The first closure year uses size because no school has a score. The rule *nearest alternative first* recomputes student kilometers to the nearest surviving school after each closure.

Table E1. Rules, seats and students for closure lists at the Department's counts

		Counts		Beyond 4.8 km, percent	
	Rule	Seats removed	Students displaced	Rural poor	All other
	(1)	(2)	(3)	(4)	(5)
Realized list	The closures the Department chose.	35,455	34,975	19.6	7.5
Smallest first	The smallest comparable schools close first (enrollment the year before).	29,272	33,242	22.2	6.4
Smallest first, alternative within 4.8 km	*Smallest first* among schools that leave a school serving each of their grades open within 4.8 km. Where too few qualify, the count is completed from the other schools, so the condition is a priority, not a guarantee.	30,134	33,698	9.1	3.3
Smallest first, alternative within 3.2 km	The same with 3.2 km.	30,907	34,750	9.3	3.1
Smallest first, alternative within 4.8 km, growth condition †	The same with 4.8 km, excluding schools whose test score growth is in the top third of their municipality.	30,400	34,000	10.3	3.5
Lowest average test scores first †	The lowest average test score the year before closes first. A school with fewer than 10 scored students takes the median of its municipality, year and size band.	35,007	36,850	21.2	7.3
Lowest average test scores first, alternative within 4.8 km †	The same, with the 4.8 km priority.	34,855	37,339	10.1	3.4
Nearest alternative first	The school whose displaced students would travel the fewest student km to their nearest surviving school closes next, recomputing this after each closure.	32,979	35,217	10.7	3.0

Notes. Closures of 2013-17. Each list selects again 315 of the 334 closures, matching the realized number by municipality, year and size band (thirds of the size of that year's closed schools), one year at a time. The 19 others close under every list. Comparable schools are open that year and no larger than that year's largest closed school. The seat proxy is each closed school's earlier maximum enrollment in grades 3 to 8, not capacity for the grades 1 to 7 students counted beside it. Students displaced covers grades 1 to 7 and includes the held closures' students. Beyond 4.8 km means no surviving school serving the grade lies within 4.8 km. The † mark identifies lists ranked on one half of the school years and measured on the other, reporting the mean of the two directions. Summed over the rural-poor students each list displaces, km to the nearest surviving school changes against the realized list by −12.5 percent [−21.0, −2.5] under *smallest first with an alternative within 4.8 km* and by −32.4 percent [−41.2, −23.0] under *nearest alternative first* (95 percent bootstrap intervals).

Each year's list starts from the schools open that year under the realized closures of earlier years. A list

can therefore close a school the Department kept open, and it can close the same school in two years, as the second score list does for 29 and 35 schools in its two directions. Counts refer to closures per year, not to schools. Random lists draw, in every municipality, year and size band, as many comparable schools as closed there. The random orders of Section 8.3 instead permute the order in which the 315 realized closures are spared. Neither assigns students at random.

Besides what Section 8.1 holds fixed, the option of leaving the municipality's public schools keeps its estimated value. The model allocates students among the surviving schools rather than assigning every student to an administratively recommended receiving school.

A closure list must retain grade coverage and pass an aggregate screen for spare enrollment, with a relaxation where needed to admit the realized closures. The subsequent allocation by the choice model does not impose seat limits at individual destinations. These are distinct restrictions. For a descriptive check, I count each closed school's earlier maximum enrollment in grades 3 to 8 as a proxy for seats removed (Table 7, Panel A). Displaced students are counted in grades 1 to 7, so these totals do not establish that receiving schools can accommodate them. The seat proxy and displaced inflows use different grade bases, so the aggregate screen does not establish spare classroom capacity at each receiving school.

For rural-poor students, under every list, the share the choice model places is within 5 percentage points of the realized closures' share on the same estimates. Of the 19,927 student records of Table 4, the model covers 18,834. The change before students sort again has its own coverage rule under which a closed school without an estimate of school value-added, or a municipality without a surviving school that has one, contributes zero, while its students stay in the denominator. The comparisons are conditional on this rule and on the schools covered under each list.

The score lists, modeled placements under alternative lists, fewer closure exercises and expanded imputation sensitivity are exploratory additions following the initial results. Statements of direction use only the comparison families specified before each family's own results, with adjustment within those families. Other estimates remain descriptive. Appendix E.5 defines the families and uncertainty rules.

E.2 Further Results for Different Schools at the Department's Counts

Table E2 reports geographic access and changes in school value-added under 1,000 random lists. Its percentiles are spreads across possible lists at the fitted estimates, not sampling intervals. The school value-added gap uses either the students displaced by the realized closures or each list's own displaced students, giving random means of 0.102 and 0.093 SD respectively. These denominators are distinct. The low pooled rural distance percentiles of the realized closures are driven mainly by 2017 rather than every wave.

The school value-added levels in Panels B and D are descriptive because the realized and random lists use information from closure decision years differently, and levels move when those years are excluded from production estimation (Section 7.2). They do not establish a tested policy advantage.

Table E3 adds the *smallest first with an alternative within 3.2 km* rule and the *smallest first with an*

Table E2. Percentiles and spreads of the realized list among 1,000 random lists

Panel A. Access per student each list displaces

	Rural poor (1)	Rural nonpoor (2)	Urban poor (3)	Urban nonpoor (4)
Km to the nearest surviving school				
Realized list	3.43	3.37	1.91	2.12
Random lists, median	3.68	3.60	1.96	2.13
Realized list's percentile	1.2	2.2	28.2	47.9
Km equivalents, choice model				
Realized list	1.84	1.91	0.97	1.00
Random lists, median	2.00	2.07	1.06	1.09
Realized list's percentile	1.3	1.2	3.0	3.3

Panel B. Change in school value-added before students sort again, per student the realized list displaced, SD

	Rural poor (5)	Rural nonpoor (6)	Urban poor (7)	Urban nonpoor (8)
Realized list	−0.054	0.023	0.048	0.059
Random lists, median	−0.114	−0.069	−0.005	0.011
Realized minus random mean	0.061	0.092	0.053	0.049

Panel C. Gap in the change for all other minus rural poor, SD

	Full sample		Halves, per realized student	
	Per realized student (9)	Per own student (10)	First half (11)	Second half (12)
Realized list	0.099	0.099	0.073	0.130
Random lists, mean	0.102	0.093	0.122	0.120
2.5th percentile	0.048	0.041	0.054	0.047
97.5th percentile	0.154	0.141	0.197	0.187
Realized list's percentile	46.7	59.8	9.9	59.5
Realized minus random mean	−0.003	0.007	−0.049	0.010
2.5th percentile	−0.054	−0.042	−0.124	−0.057
97.5th percentile	0.051	0.058	0.019	0.083

Panel D. Realized minus random mean change per displaced student, SD

	Rural poor (13)	All other (14)
Realized minus random mean	0.051	0.057
2.5th percentile	0.001	0.005
97.5th percentile	0.099	0.108

Notes. Closures of 2013-17. Random lists close comparable schools at the realized counts in every municipality, year and size band (Table E1). Percentiles and ranges are spreads across the 1,000 lists at the full sample estimates, not sampling intervals. Panel A is per student each list displaces. Panels B and C compare the surviving schools with the school closed, with positive values more favorable. Panel B's level differences are descriptive. Information from decision years enters the realized and random lists differently, so no formal level test is reported. In Panel C, the gap measures how much less favorable the change is for rural-poor students than for all other displaced students. Halves use school value-added estimated on each half of the school years. Panel D is per student each list displaces.

alternative within 4.8 km and a condition on growth in test scores rule. It reports model access in kilometer equivalents per student each list displaces alongside geographic distance, and uses separate denominators for the school value-added comparisons.

Table E4 reports access contrasts per displaced student and the gap in placed school value-added on covered origins. These are different populations and units. Its intervals are descriptive and do not establish adjusted statements of direction. All five school value-added gap contrasts have intervals spanning zero. The simulation mark on one main access contrast retains its point, standard error and draw standard deviation without an interval.

Table E5 complements the school value-added contrasts in Table 7, Panel D, with school effectiveness, origin and destination components, and robustness checks. School effectiveness adds school-average teacher value-added. For the size and map lists, the closed schools account for nearly all of each rural-poor contrast. The score lists have larger destination components, about 0.04 to 0.06 SD, and origin components between -0.01 and $+0.06$ SD across directions.

Three checks concern the placed contrasts (Panel C). First, school value-added can absorb any effect of the move itself on displaced students' scores. Estimates leaving out observations, which read each school's value-added from the half of the school years that does not contain the closure year, give contrasts of the same sign for all five lists, although they cover only 70.5 to 80.0 percent of the students, against about 94 percent for the main estimates. Second, placing students with shares by family type or with shares pooled across types changes the contrasts by at most 0.007 SD. Third, letting rural families' response to school effectiveness differ, at five values spanning the range its estimates allow, moves the realized rural-poor level from -0.058 to $+0.002$, and every contrast keeps its sign at both ends. At a nonzero rural difference families sort on school-average teacher value-added, an estimate from the same test score data, so these rows check signs only. The panel also gives the contrasts under the school value-added estimated without the years of closure decisions.

The recorded and modeled comparison on 18,834 covered student records is the validation exercise in Section 6.2 and Table D3, rather than a policy contrast. Table E5 retains both figures on this matched sample. Expected exits extrapolated from the event study are 0.107 per displaced rural-poor student and 0.087 per displaced student under the realized closures.

E.3 Further Results for Closing Fewer Schools

Table E6 compares *farthest from a surviving school first* and *largest model access gain first* at 32, 47, 63 and 95 closures spared. Their common low cost schedule and random reference appear once. The order using model access spares larger schools and forgoes more savings at the high end. At 47, the closures remaining under the distance order remove 29,459 seats, against 35,455 under the realized closures.

The distance order nearly removes the tail beyond 4.8 km, while the model access order avoids more aggregate access loss and leaves a substantial tail. These are different policy objectives. Both orders avoid

Table E3. Access by closure list at the Department's counts

Panel A. Access, per student each list displaces

	Km equivalents		Km to the nearest surviving school	
	Rural poor (1)	All other (2)	Rural poor (3)	All other (4)
Realized list	1.84	1.14	3.43	2.15
	(0.11)	(0.07)		
Smallest first	2.20	1.28	3.79	2.25
	(0.09)	(0.06)		
Smallest first, alternative within 4.8 km	1.79	1.18	3.11	2.07
	(0.11)	(0.07)		
Smallest first, alternative within 3.2 km	1.68	1.12	2.75	1.83
	(0.12)	(0.07)		
Smallest first, 4.8 km, growth condition †, first half	1.81	1.20	3.19	2.10
	(0.11)	(0.07)		
Smallest first, 4.8 km, growth condition †, second half	1.83	1.19	3.25	2.11
	(0.11)	(0.07)		
Lowest average test scores first †, first half	1.96	1.17	3.72	2.19
	(0.10)	(0.07)		
Lowest average test scores first †, second half	1.93	1.22	3.63	2.18
	(0.11)	(0.07)		
Lowest average test scores first, 4.8 km †, first half	1.71	1.12	3.08	1.99
	(0.12)	(0.07)		
Lowest average test scores first, 4.8 km †, second half	1.74	1.17	3.14	2.02
	(0.12)	(0.07)		
Nearest alternative first	1.65	1.01	2.67	1.50
	(0.12)	(0.08)		

Panel B. Change in school value-added, list minus realized list, per student the realized list displaced, SD

	Before sorting again		Placed by the model	
	Rural poor (5)	All other (6)	Rural poor (7)	All other (8)
Smallest first	−0.098	−0.089	−0.092	−0.057
	(0.039)	(0.030)	(0.034)	(0.031)
Smallest first, alternative within 4.8 km	−0.081	−0.084	−0.076	−0.061
	(0.035)	(0.029)	(0.032)	(0.031)
Smallest first, alternative within 3.2 km	−0.073	−0.083	−0.071	−0.074
	(0.040)	(0.028)	(0.037)	(0.029)
Smallest first, 4.8 km, growth condition †	−0.071	−0.062	−0.044	−0.020
	(0.049)	(0.040)	(0.043)	(0.044)
Lowest average test scores first †	0.048	0.094	0.061	0.119
	(0.042)	(0.032)	(0.045)	(0.038)
Lowest average test scores first, 4.8 km †	0.057	0.104	0.067	0.129
	(0.036)	(0.034)	(0.039)	(0.041)
Nearest alternative first	−0.054	−0.028	−0.049	−0.006
	(0.035)	(0.030)	(0.034)	(0.033)

Notes. Closures of 2013-17, with lists as in Table E1. Panel A reports the choice model's access loss (Table 6) with standard errors from the delta method, alongside km from the closed school to the nearest surviving school serving the grade, without a standard error. The † lists are chosen on one half of the school years, showing each direction. In Panel B, the change before students sort again follows Table 7, Panel C, using the largest standard error across four imputation rules. Placement by the model covers all the list's placed students. The † lists average the two directions and their standard errors. The part due to the number of students placed is below 0.006 SD in every entry.

Table E4. Model access contrasts and the placed school value-added gap across closure lists

Panel A. List minus realized access, km equivalents per own displaced student

	Rural poor	All other	Gap (RP minus other)
Smallest first	0.364 (0.025)	0.143 (0.010)	0.221 (0.026)
	[0.315, 0.413]	[0.124, 0.162]	[0.170, 0.272]
Smallest first, 4.8 km	−0.047 (0.004)	0.039 (0.009)	−0.087 (0.007)
	[−0.054, −0.040]	[0.021, 0.058]	[−0.099, −0.074]
Lowest scores first	0.110 (0.007)	0.056 (0.005)	0.054 (0.009)
	[0.096, 0.125]	[0.047, 0.065]	[0.037, 0.072]
Lowest scores first, 4.8 km	−0.110 (0.007)	0.004 (0.0033) (S)	−0.114 (0.009)
	[−0.125, −0.096]	Draw SD 0.0042	[−0.131, −0.097]
Nearest alternative first	−0.187 (0.011)	−0.126 (0.017)	−0.061 (0.025)
	[−0.208, −0.166]	[−0.159, −0.093]	[−0.109, −0.012]

Panel B. Placed school value-added gap, all other minus rural poor, SD

	Gap level school value-added	Gap contrast school value-added	Gap contrast school effectiveness
Realized list	0.045 (0.037)	-	-
Smallest first	0.070 (0.033)	0.025 (0.035)	−0.002 (0.021)
		[−0.043, 0.094]	
Smallest first, 4.8 km	0.062 (0.033)	0.017 (0.033)	−0.027 (0.019)
		[−0.047, 0.081]	
Lowest scores first	0.111 (0.052)	0.068 (0.053)	0.063 (0.026)
		[−0.035, 0.172]	
Lowest scores first, 4.8 km	0.106 (0.052)	0.064 (0.048)	0.068 (0.024)
		[−0.031, 0.158]	
Nearest alternative first	0.099 (0.042)	0.055 (0.035)	0.046 (0.019)
		[−0.014, 0.123]	

Notes. Standard errors appear in parentheses and 95 percent intervals in brackets, without adjustment for multiple comparisons or statements of direction. Score lists average the two cross fitting directions, with the mean of the two directions' school value-added standard errors. Panel A uses each list's own displaced students and model weights for km equivalent access loss. Realized levels are 1.837 (0.111) for rural poor and 1.140 (0.073) for others. Rural-poor percent contrasts are 19.8, −2.6, 6.0, −6.0 and −10.2 percent in row order. An (S) mark means draw SD differs from the SE from the delta method by more than 25 percent in the completed check with 200 draws, so no interval is printed for that figure. Panel B gives expected destination minus closed school value-added on covered enrollment weights. The realized covered weight is 32,398, distinct from the observed incidence student sample. Each score list contrast subtracts the realized gap measured on the same scoring half before averaging the directions, so its baseline differs from the displayed full sample realized gap. Other gap contrasts use the full sample realized gap. School effectiveness adds school-average teacher value-added. These intervals are sampling intervals under the stated propagation and split half noise construction. They do not add the differential model error allowance used in the main family tests. Shared perturbations preserve covariance between groups. None of these placed figures is marked by the simulation check.

Table E5. Further results for students placed by the choice model

Panel A. List minus realized list for rural-poor students, SD

	Smallest first (1)	*Smallest first, 4.8 km* (2)	*Lowest scores* † (3)	*Lowest scores, 4.8 km* † (4)	*Nearest alternative* (5)
School effectiveness	−0.055	−0.023	0.087	0.077	−0.052
	(0.020)	(0.018)	(0.027)	(0.026)	(0.018)
School value-added, from the schools closed	−0.110	−0.097	0.024	0.030	−0.077
	(0.034)	(0.032)	(0.042)	(0.038)	(0.033)
School value-added, from the schools entered	0.017	0.012	0.048	0.050	0.015
	(0.014)	(0.015)	(0.022)	(0.023)	(0.018)
School value-added, leave out estimates	−0.144	−0.068	0.060	0.109	−0.052
	(0.067)	(0.068)	(0.057)	(0.053)	(0.063)

Panel B. List minus realized list for all other students, SD

	Smallest first (6)	*Smallest first, 4.8 km* (7)	*Lowest scores* † (8)	*Lowest scores, 4.8 km* † (9)	*Nearest alternative* (10)
School effectiveness	−0.058	−0.050	0.150	0.145	−0.006
	(0.018)	(0.018)	(0.026)	(0.026)	(0.016)

Panel C. Checks of list minus realized school value-added for rural-poor students

	Smallest first (11)	*Smallest first, 4.8 km* (12)	*Lowest scores* † (13)	*Lowest scores, 4.8 km* † (14)	*Nearest alternative* (15)
Students with leave out estimates, percent	70.5	71.4	80.0	78.8	73.9
Shares pooled across family types	−0.087	−0.081	0.075	0.084	−0.058
Without the last operating year and the year before, closures of 2014-17	−0.054	−0.047	n.a.	n.a.	−0.035
Lowest rural response to school effectiveness	−0.105	−0.095	0.025	0.073	−0.066
Highest rural response to school effectiveness	−0.082	−0.079	0.117	0.089	−0.059
Smallest difference read alone	0.118	0.116	0.160	0.141	0.137
Expected exits per displaced rural-poor student	0.111	0.109	0.110	0.108	0.105

Notes. Closures of 2013-17, with lists as in Table E1. Students are placed by the choice model given reenrollment, with school value-added fixed. School effectiveness adds school-average teacher value-added. The school value-added contrasts are in Table 7, Panel D. Panels A and B here add school effectiveness and components per student each list places on covered origins, with common enrollment weights within a row. Standard errors combine choice coefficient propagation and school value-added noise from the two halves. The † lists report the mean of the two directions and the mean of their standard errors. Panel C gives numbers only, evaluating the response to school effectiveness at five values spanning its estimates (lowest and highest contrast, both directions for †). The † designs use evaluation on separate halves rather than the decision year exclusion check. Under realized closures, expected exits are 0.107 per rural-poor student. On 18,834 student records, simulated rural poor minus all other is −0.056 (0.036) and recorded is −0.074 (0.031), not differenced. No list's difference exceeds 0.233 SD (95 percent, simultaneous, allowing for the model's error, Table D3).

Table E6. Closing fewer schools at 32, 47, 63 and 95 closures spared

	Farthest first				*Largest model access gain first*			
	32	47	63	95	32	47	63	95
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Savings forgone, thousand dollars a year								
High end	2,008.4	2,925.0	3,833.9	5,755.1	2,242.6	3,226.8	4,247.3	6,349.6
Access loss avoided, km equivalent student years								
Rural poor	5,394.7	7,835.7	9,693.5	12,742.9	8,692.3	11,136.8	13,241.3	16,283.7
	(123.7)	(183.8)	(280.3)	(405.4)	(221.8)	(356.4)	(480.4)	(732.3)
All other	2,075.8	2,656.6	3,068.8	4,466.8	3,434.0	4,406.3	5,392.9	7,474.2
	(76.0)	(99.8)	(127.9)	(201.2)	(76.8)	(121.1)	(176.0)	(310.1)
All students	7,470.5	10,492.4	12,762.2	17,209.7	12,126.3	15,543.2	18,634.2	23,757.9
	(180.9)	(269.6)	(392.8)	(596.4)	(295.4)	(460.6)	(617.6)	(937.1)
Access loss avoided, percent of the group's realized loss								
Rural poor	20.9	30.4	37.6	49.5	33.7	43.2	51.4	63.2
	(1.0)	(1.4)	(1.6)	(1.8)	(1.4)	(1.5)	(1.6)	(1.4)
All other	13.0	16.6	19.2	28.0	21.5	27.6	33.8	46.8
	(1.0)	(1.2)	(1.4)	(1.9)	(1.4)	(1.4)	(1.5)	(1.5)
All students	17.9	25.1	30.6	41.2	29.1	37.3	44.7	56.9
	(0.6)	(0.8)	(0.9)	(1.0)	(1.0)	(1.2)	(1.2)	(1.2)
Break even value, all students, dollars a year per km equivalent student year								
Low end	52	55	60	67	32	37	41	49
	[50, 55]	[52, 58]	[57, 64]	[63, 72]	[31, 34]	[35, 39]	[39, 44]	[45, 53]
High end	269	279	300	334	185	208	228	267
	[257, 282]	[265, 294]	[283, 320]	[313, 359]	[177, 194]	[196, 220]	[214, 244]	[248, 290]
Change in school value-added before students sort again, SD								
Rural poor	−0.013	−0.024	−0.015	−0.005	−0.020	−0.021	−0.004	0.011
	(0.009)	(0.022)	(0.023)	(0.034)	(0.030)	(0.031)	(0.034)	(0.034)
All other	−0.006	−0.010	−0.011	−0.010	−0.008	−0.005	0.007	0.005
	(0.004)	(0.009)	(0.009)	(0.016)	(0.015)	(0.016)	(0.018)	(0.019)
Change in school value-added, students placed by the choice model, SD								
Rural poor	−0.010	−0.016	−0.009	−0.005	−0.018	−0.014	−0.002	0.013
	(0.011)	(0.020)	(0.022)	(0.034)	(0.028)	(0.030)	(0.034)	(0.035)
All other	−0.004	−0.006	−0.009	−0.007	−0.001	0.004	0.025	0.029
	(0.004)	(0.008)	(0.008)	(0.016)	(0.015)	(0.016)	(0.021)	(0.022)
Students spared and the far tail								
Rural-poor students spared	2,002	2,964	3,884	5,651	2,780	3,711	4,741	6,424
All other students spared	1,057	1,570	1,934	3,260	2,244	3,310	4,453	7,034
Rural poor beyond 4.8 km, %	6.43	0.14	0.16	0.19	10.81	10.42	9.61	9.43

Common cost schedule and random reference for both orders

Closures kept open	32	47	63	95
Low savings forgone, thousand dollars a year	390.3	573.3	768.4	1,158.7
1,000 random orders at the same count				
Median access, all	4,401.4	6,521.7	8,734.8	13,095.6
5th percentile	3,534.8	5,512.0	7,566.1	11,812.8
95th percentile	5,401.2	7,608.0	9,982.5	14,416.0
Median school value-added, rural poor	0.006	0.009	0.013	0.018
Median placed, rural poor	0.003	0.004	0.005	0.008

Notes. Both orders spare closures from the 315 realized closures of 2013-17 (Figure 8). *Farthest first* abbreviates *farthest from a surviving school first*. Savings use Table F1's two ends. Access uses the choice model's measure, 28,024 students, against the realized loss of 41,722.8 (25,764.8 rural poor), with delta method standard errors in parentheses. The break even value divides savings by access avoided, and its interval divides savings by the ends of the access interval. School value-added comparisons compare remaining with realized closures per student displaced under realized closures (34,037), using the largest standard error across four imputation rules. Modeled placement is conditional on reenrollment. The far tail covers displaced rural-poor students (19.6 percent under realized closures). Random orders give a spread across orders rather than sampling error. Brackets give 95 percent delta method intervals.

more access loss for all students than the 95th percentile of random orders at each count. The distance order's change in school value-added before students sort again lies below the random median at all four counts, with both halves agreeing, and at the 0.4th percentile at 47. Random spreads are not sampling intervals, and the model access order's school value-added differences remain descriptive.

The break even value divides annual savings forgone by access loss avoided for all students. Its interval holds each cost assumption fixed and divides that amount by the ends of the 95 percent access interval. The measure counts access for the school year of displacement while savings recur annually. No price is assigned to access or school value-added, and the school value-added differences do not establish improvements at the adjusted level.

Figure 8 uses simultaneous 95 percent bands over the 31 displayed counts under the maintained parameter and noise construction. Access uses the delta method in the five choice coefficients. School value-added uses the largest standard error and critical value across the four imputation rules, and every school value-added band includes zero.[5] Figure E1 preserves the bands of both orders for the school value-added comparison.

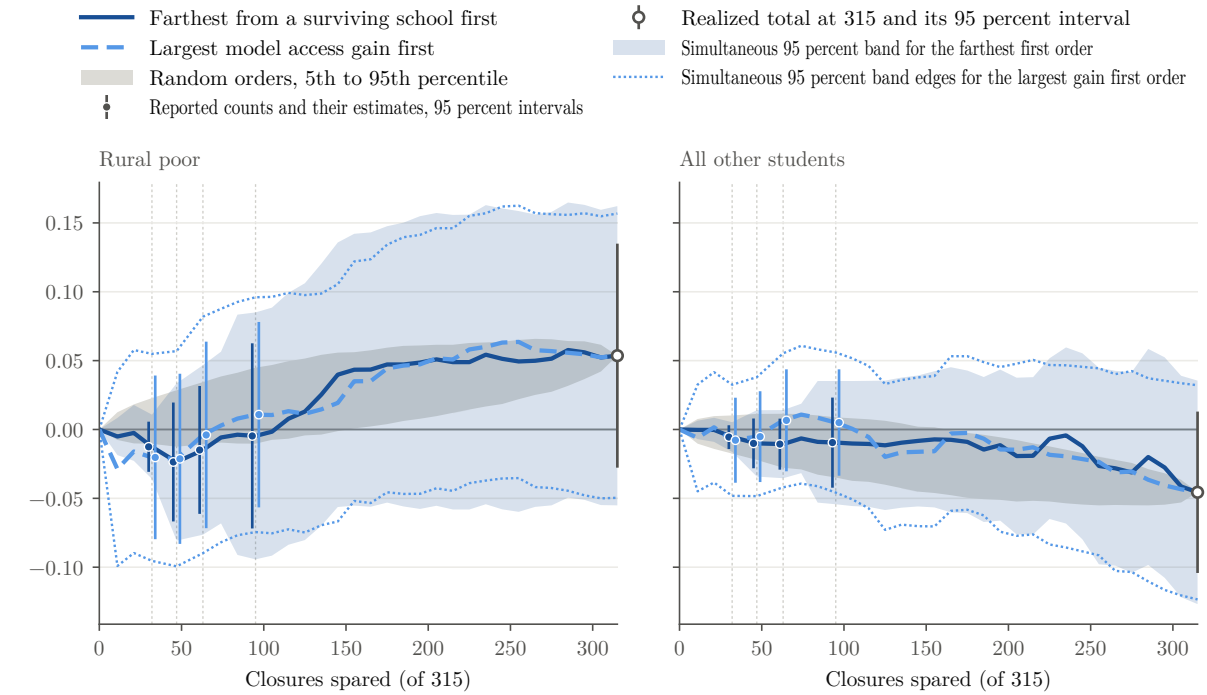
A third order uses estimates the Department did not compute (Table E7). It spares first the closures whose estimated contribution to the change in school value-added before students sort again is least favorable on one half of the school years, measures the result on the other half, and averages the two directions, placing closures without estimates on both halves last. For rural-poor students its differences from the realized closures run from 0.049 (0.023) at 32 to 0.098 (0.028) at 95, where positive is more favorable. Each standard error is the mean of the two directions' standard errors, a bound. Its access loss avoided is 73 to 81 percent of that of *farthest from a surviving school first* for all students and 60 to 70 percent for rural-poor students. These figures carry no statement of direction.

Two further orders use what the Department held, past test scores and the map, and were added after the results were seen (Table E8). *Highest average test scores first (cross fitted)* spares first the closures whose average test score the year before most exceeds the average weighted by enrollment of the surviving schools serving the same grades in the municipality, weighted by the students displaced. It ranks the 142 of 315 closures that have scores and estimates of school value-added on both halves of the school years, so that each can be measured on the half not used to rank it. The other 173 hold 55.4 percent of the displaced rural-poor students and come after all 142. *4.8 km, then highest average test scores (cross fitted)* spares the 47 closures that *farthest from a surviving school first* spares up to its 4.8 km point, those that can be ranked first, and then continues by the same score. Each order is chosen on one half of the school years and measured on the other.

Against *farthest from a surviving school first* at the same count, the closures that remain under the first score order leave a higher access loss at all four counts. That order avoids 72 to 78 percent as much access

[5] For access, critical values are 2.071 and 2.078 for rural-poor and other students under the distance order and 2.187 and 2.172 under the model access order. For school value-added they are 2.653, 2.764, 2.520 and 2.646 in the same order.

Figure E1. Changes in school value-added with fewer closures and both orders' bands



Notes. Panel C of Figure 8 with the simultaneous 95 percent bands of both orders. Values compare the closures that remain with the realized closures, before students sort again, in SD per student the realized closures displaced, with values above zero more favorable. Bars give pointwise 95 percent intervals at 32, 47, 63 and 95. Standard errors are the largest of four imputation rules (Table E11). Gray gives the 5th to 95th percentile of 1,000 random orders.

Table E7. Closing fewer schools in the order of school value-added, cross fitted

	Change before students sort again		Minus the farthest first order	
	Rural poor (1)	All other (2)	Rural poor (3)	All other (4)
32 closures kept open	0.049	0.042	0.054	0.046
	(0.023)	(0.015)	(0.024)	(0.015)
47 closures kept open	0.067	0.047	0.089	0.059
	(0.025)	(0.017)	(0.034)	(0.019)
63 closures kept open	0.079	0.048	0.096	0.061
	(0.026)	(0.019)	(0.035)	(0.021)
95 closures kept open	0.098	0.054	0.102	0.063
	(0.028)	(0.021)	(0.038)	(0.024)

Notes. Closures of 2013-17 kept open in the order of the closed schools' estimated school value-added, chosen on one half of the estimates and measured on the other, with the two directions averaged. This order uses estimates the Department did not have. The change before students sort again gives the closures that remain minus the realized closures, per student the realized closures displaced, with positive values more favorable. Standard errors in parentheses are clustered by municipality. For the average of the two directions, they are bounded by the average of their standard errors.

Table E8. Closing fewer schools in orders that use test scores and the map

	Highest average test scores first				*4.8 km, then highest average test scores*			
	32	47	63	95	32	47	63	95
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Savings forgone, thousand dollars a year								
Low end	390.3	573.3	768.4	1,158.7	390.3	573.3	768.4	1,158.7
High end	2,083.1	2,964.2	3,983.5	5,942.7	2,093.6	2,925.0	3,995.2	5,938.4
Access loss avoided, km equivalent student years								
Rural poor	3,512.0	4,651.6	5,630.7	8,225.6	6,174.6	7,835.7	9,338.9	11,575.8
	(194.9)	(274.0)	(352.9)	(545.4)	(152.1)	(183.8)	(294.0)	(430.6)
All other	2,262.0	2,949.0	3,685.7	5,147.5	2,263.2	2,656.6	3,941.8	5,732.1
	(152.5)	(187.3)	(236.5)	(346.9)	(86.0)	(99.8)	(134.8)	(241.0)
All students	5,774.0	7,600.6	9,316.4	13,373.1	8,437.8	10,492.4	13,280.7	17,307.8
	(275.1)	(374.0)	(491.7)	(740.2)	(219.6)	(269.6)	(419.7)	(630.5)
All students,	13.8	18.2	22.3	32.1	20.2	25.1	31.8	41.5
percent of the realized loss	(0.2)	(0.2)	(0.1)	(0.1)	(0.7)	(0.8)	(0.8)	(0.9)
All students,	−1,696.5	−2,891.8	−3,445.9	−3,836.6	967.3	0.0	518.5	98.1
minus farthest first	(145.7)	(149.3)	(155.8)	(229.9)	(40.5)	-	(63.1)	(139.2)
Break even value, all students, dollars a year per km equivalent student year								
Low end	68	75	82	87	46	55	58	67
	[62, 75]	[69, 83]	[75, 92]	[78, 97]	[44, 49]	[52, 58]	[54, 62]	[62, 72]
High end	361	390	428	444	248	279	301	343
	[330, 398]	[356, 432]	[387, 477]	[401, 498]	[236, 261]	[265, 294]	[283, 321]	[320, 369]
Change in school value-added before students sort again, SD								
Rural poor	0.036	0.050	0.049	0.045	−0.032	−0.023	0.024	0.043
	(0.026)	(0.027)	(0.027)	(0.029)	(0.027)	(0.028)	(0.028)	(0.030)
All other	0.033	0.042	0.041	0.027	−0.011	−0.012	0.018	0.037
	(0.012)	(0.014)	(0.016)	(0.018)	(0.010)	(0.010)	(0.013)	(0.017)
Rural poor,	0.040	0.072	0.065	0.048	−0.027	0.000	0.040	0.047
minus farthest first	(0.028)	(0.020)	(0.018)	(0.027)	(0.025)	-	(0.015)	(0.025)
All other,	0.037	0.055	0.053	0.036	−0.006	0.000	0.031	0.046
minus farthest first	(0.013)	(0.014)	(0.015)	(0.019)	(0.009)	-	(0.009)	(0.017)

Notes. Closures of 2013-17 kept open, of 315. Both orders were added after the results were seen. The first spares first the closures whose average test score the year before most exceeds that of the municipality's surviving schools, among the 142 with scores and school value-added on both halves of the school years. The other 173 (55.4 percent of displaced rural-poor students) follow. The second spares farthest first's 47 (ranked ones first), then continues by score, so the two coincide at 47. Each order is chosen on one half and measured on the other. The two directions are averaged, with standard errors bounded by their mean. Measures follow Table E6, with differences from farthest first on the same half. Brackets give 95 percent intervals from the delta method.

loss for all students and 58 to 65 percent as much for rural-poor students. Its remaining closures give a more favorable change in school value-added before students sort again for rural-poor students at 47 and 63 closures spared, by 0.072 (0.020) and 0.065 (0.018) SD per student the realized closures displaced, and for other students at 32, 47 and 63. The second score order spares the same closures as the first distance order at 47. At 63, it avoids 518.5 (63.1) more kilometer equivalent student years for all students, and its remaining closures give a more favorable change for both groups, by 0.040 (0.015) SD for rural-poor students and 0.031 (0.009) for others. Its access loss avoided for rural-poor students is 3.7 percent below the first distance order's, a point estimate with no test. At 95, neither its access difference for all students nor its difference in school value-added for rural-poor students can be distinguished from zero, its remaining closures give other students a change more favorable by 0.046 SD (0.017), and its access loss avoided for rural-poor students

is 9.2 percent below the first distance order's, again a point estimate. At 32, both directions of the second score order spare the same closures, so the set there may be fixed by which closures can be ranked, not by their scores. These statements are adjusted for multiple comparisons within six sets of comparisons fixed before the results were seen, and each requires both directions to agree in sign. The two directions differ in size, and I do not interpret that difference.

E.4 Further Results Holding the Closures Fixed

Table 8 consolidates the reach results for rural-poor and urban nonpoor students. Each student's most effective surviving school must serve the grade and satisfy the stated distance restriction. Distances use recorded km between schools where available and straight line distance times the school's road factor otherwise. A seat is open below the school's maximum recorded enrollment up to closure, with a floor admitting the realized allocation. This is an enrollment accounting rule rather than measured classroom capacity. Selection on one half of the school years is evaluated on the other, with roles reversed and averaged. Every other student's school stays fixed, so the benchmark is individual rather than a jointly feasible cohort assignment. The sample comprises 12,564 student records, including rural-poor weight 5,369. Brackets identify 95 percent bootstrap intervals over municipalities, while entries without brackets are point estimates only. Separate results cover the rural-poor and urban nonpoor groups.

The exercise reassigning teachers instead keeps every student's school and classroom fixed (Table E9). Its pool requires scored students at the teacher's closed school, a post in a tested subject at exactly one surviving school in the next year, and an estimate of teacher value-added for each profile subject. Starting from 1,178 teacher and wave pairs with scored origin students yields the 626 posts used here. About 170 otherwise eligible destination posts fail the requirement for scored students at the origin. The broader roster contains 2,253 displaced teacher and wave pairs (2,151 distinct teachers), a different definition from earlier teacher counts. Under restrictions by municipality, year, subject profile and grade band, 323 posts belong to blocks with more than one teacher and 303 are singletons. The pool of 626 posts teaches 20.6 percent of all displaced students' linked observations in tested subjects and 21.5 percent of rural-poor students'. The exercises do not reassign incumbents at receiving schools.

The original objective weights each post's scored caseload by $1+$ its school's model rural-poor share, using teacher value-added adjusted for sampling noise. It is not an objective directed specifically at displaced rural-poor students' classes. Its outcome based on classrooms averages changes in linked teacher value-added over each student's tested subjects with a link to a destination teacher. Teachers outside the pool contribute zero change, as do students without eligible destination links. The denominator is the 22,475 displaced students enrolled again in a public school in the same municipality in 2013-17, with rural-poor weight 8,647.4, including these zeros. This is a measure of classroom exposure, distinct from school-average teacher value-added used in the decomposition of school effectiveness.

Table E9 reports local and island exchanges under the original and displaced student objectives, including

the version relaxing grade bands and results through 2016. The original objective weights scored caseload and the receiving school's rural-poor share, whereas the targeted objective weights the linked classes of displaced rural-poor students. These are different assignments on the same selected posts, students and classrooms, rather than estimates of a single policy.

The 95 percent composite intervals combine municipality resampling and draws of teacher value-added, with each optimized assignment held fixed. The intervals apply separately to each design, without adjustment across designs or inference on their differences. The calculation does not include uncertainty about the prior or production specification. All designs use observed classes in the next year and omit teachers' residences, preferences, priority rules and implementation costs, so the wider exchanges are optimistic assignment benchmarks rather than demonstrated feasible policies. They hold teacher value-added summed over posts fixed but can alter average exposure because posts serve different numbers of students. Under the targeted assignment across the island, other students' mean exposure changes by -0.0082 SD $[-0.0192, 0.0030]$ per seat in a tested subject at a pool post, a different denominator from the outcome for displaced students above. The targeted benchmark therefore does not establish a Pareto improvement.

Table E9. Classroom exposure under reassignment of displaced teachers among observed posts

Assignment design	Rural poor 2013-17	Rural poor through 2016	Posts changed
Municipality, original objective	0.00033	−0.00029	201
	[−0.00262, 0.00383]	[−0.00474, 0.00410]	
Municipality, displaced student objective	0.00250	0.00244	189
	[−0.00094, 0.00674]	[−0.00319, 0.00902]	
Across the island, original objective	0.00773	0.00235	585
	[−0.00311, 0.01864]	[−0.00925, 0.01345]	
Across the island, displaced student objective	0.01830	0.01395	585
	[0.00775, 0.03004]	[0.00233, 0.02755]	
Across the island, any grade band, original objective	0.00886	0.00339	607
	[−0.00147, 0.02012]	[−0.00827, 0.01557]	
Across the island, any grade band, displaced student objective	0.01946	0.01522	607
	[0.00904, 0.03160]	[0.00330, 0.02822]	

Notes. Changes in linked teacher value-added, SD per displaced rural-poor student. Every design uses the same selected pool of 626 posts and their teachers, students, schools and classrooms, with year and subject profiles fixed. Grade bands remain fixed except in the last two rows. The original objective weights scored caseload by one plus the school's model rural-poor share. The displaced student objective targets linked classes of displaced rural-poor students. The 2013-17 denominator is 22,475 students reenrolled in public schools of the same municipality, with rural-poor weight 8,647.4. Each linked student's change averages across tested subjects with a link to a destination teacher. Teachers outside the pool and students without eligible links contribute zero. Only 323 posts are in nonsingleton municipality blocks. The pool reaches 21.5 percent of displaced rural-poor students' linked observations in tested subjects. Brackets give 95 percent composite intervals for each design from 999 replicates combining resampling of municipalities with draws of teacher value-added, with assignments fixed and no adjustment across designs or intervals for differences between designs. The urban nonpoor gain in the first row is 0.00059 [−0.00546, 0.00685]. The wider exchanges use classes observed the following year and omit teacher residences, preferences, priority rules and implementation costs. They are conditional assignment benchmarks.

In the realized placement, within municipality, year, subject and grade band, teachers with higher estimated teacher value-added went to posts serving fewer rural-poor students (permutation $p = 0.005$). The difference is about 0.0005 SD, smaller than the 0.00066 SD that the test detects with 80 percent power in simulation. This diagnostic describes the realized assignment rather than the gain from reassigning teachers.

The information scenario adds to the choice model a response to a report card on each surviving school's estimated school effectiveness. Families' current choices bound the weight they place on school effectiveness, not the weight they would place if informed, so the weight is an input of the scenario, not an estimate. I report four weights: 0.30 and 0.98 km per SD for rural-poor families, at which the added weight equals the estimate and the 95 percent upper limit from the school mean utilities (Online Appendix D.3); about 3 km per SD, the value Bau (2022) estimates for richer families in rural Pakistan; and about 13 km per SD, implied by Harris and Larsen (2022) for New Orleans, where school ratings are published. The added weight is the same in utility for all families. One half of the school years supplies the card and the other evaluates the resulting allocation, with the roles reversed and averaged. The scenario compares two allocations from the model and assumes full participation, no seat limit, and exits unchanged. At 0.98 km per SD, the school value-added of the school entered rises by 0.041 SD [0.029, 0.053] for rural-poor students and by 0.039 [0.022, 0.055] for all other displaced students, a difference of 0.002 [−0.013, 0.017]. School effectiveness rises by 0.050 [0.040, 0.061] and 0.045 [0.027, 0.063], and the distance to the school entered changes by less than 0.01 km. At 0.30, 3 and 13 km per SD, the gain for rural-poor students is 0.013, 0.106 and 0.282 SD, and at no weight can it be distinguished from the gain for all other displaced students. If instead every family values the card at the same distance in km, all other displaced students gain more than rural-poor students. At 13 km per SD, 96 percent of the added inflow goes to receiving schools where it exceeds 5 percent of enrollment, so the larger weights rest on the absence of a seat limit.

Table E10. Information on school effectiveness under an assumed response

	Rural poor	Urban nonpoor
Gain in school effectiveness, SD	0.047	0.062
	[0.037, 0.058]	[0.041, 0.087]
Change in km to the school entered	0.004	—
	[−0.025, 0.036]	
Placements above earlier maximum enrollment, percent	78	70

Notes. Sensitivity scenario for information on surviving schools, with an assumed response of 1.1 km per SD of school effectiveness rather than an estimated intervention effect. The scenario uses an earlier choice specification with 805 markets and 98,069 families, on 12,564 of 15,025 reenrolled student records carrying 84.6 percent of rural-poor weight. One half of the school years supplies the information and the other evaluates school effectiveness, with directions reversed and averaged. Participation is full, receiving schools have no seat limit, and other families' choices stay fixed. School effectiveness combines school value-added and school-average teacher value-added. Distance change is reported for rural-poor students. Brackets give 95 percent bootstrap intervals over municipalities, while excess enrollment shares are point estimates. These placements need not fit receiving schools' historical enrollment or represent a feasible joint allocation.

E.5 Inference

Standard errors of access come from the delta method in the five coefficients of the choice model, clustered by municipality. The derivatives are taken numerically at two step sizes, and a figure is reported only when the two give standard errors within 1 percent of each other. Enrollment shares, the set of closed schools, population weights, the reference distance and the distance power are held fixed.

Standard errors of the change in school value-added before students sort again come from the split of the school years into halves, clustered by municipality. The half estimates leave out displaced students' records from their move year on, whereas the estimates using the full sample keep them. Among the schools that carry weight in these comparisons, 960 have estimates on both halves, 209 on one half (174 of them with scored years in both halves) and 19 on neither (Table E11). Schools without an estimate in both halves have no difference between halves, and they carry 13 to 19.5 percent of the absolute weight, so their noise variance is imputed. Intervals use the largest standard error across four imputation rules. The initial donor rule uses schools with exactly two scored years and both halves, fewer than 10 schools. Sensitivity rules use all schools with both halves or make noise variance inversely proportional to scored years or scored student records in the available half, calibrated within enrollment terciles. The expanded sensitivity set was adopted after the initial results because the original donor base was small. Among schools with both halves, the squared difference between halves falls with a school's scored student records, but less than in proportion, and within enrollment terciles it varies little with scored years.

The rule binds in two places. In Figure 8, the largest standard error comes from the rule on scored student records at 15 of the 16 estimates, and more than half of the variance is imputed at 95 closures spared under *farthest from a surviving school first* (51.0 percent for rural-poor students and 78.8 percent for others) and at 7 of the 8 estimates for *largest model access gain first*. No statement about these comparisons depends on the choice of rule. In Table 7, Panel C, the rule changed one statement. Under the larger standard error, 0.039 against 0.035, the difference between *smallest first* and the realized closures for rural-poor students can no longer be distinguished from zero after adjustment.

Lists chosen on one half of the school years are measured on the other, so their noise comes from one half only and its variance is doubled. The reported standard error of the mean of two directions is the average of their standard errors, a conservative bound for any correlation between directions.

For students placed by the choice model, a standard error combines the uncertainty in the model's coefficients, from the delta method with the school mean utilities solved again at each step, and the noise in the school value-added, from the split halves as above. The calculation treats the two parts as independent under the main specification, which does not use estimates of school value-added to estimate the choice coefficients, and the coefficient part is negligible, at most 0.002 of the variance for the rural-poor contrasts. Covariance between enrollment choices and test score shocks is not estimated. A contrast D with the realized closures is tested with $p = 2[1 - \Phi((|D| - a)/\mathrm{SE})]$, and $p = 1$ when $|D| \leq a$, where a is the list's tolerance for the model's error (Table D3). A contrast that passed would also have needed the same sign on school effectiveness, on the estimates leaving out observations (for the three lists not chosen on one half) and across the range of the rural difference in the response to school effectiveness, the same sign in both directions for the score lists, and a placed share within 5 percentage points of the realized closures'. No contrast passed, so these conditions decided nothing.

Holm adjustment controls the familywise error rate at 5 percent within each specified family, not across families. The five lists in Table 7 form separate rural-poor families for the change before sorting and for

Table E11. Imputed noise and the standard errors of the change in school value-added

Panel A. Schools carrying weight, and their share of the weight

	Schools (1)	Rural poor, weight share (2)	All other, weight share (3)
An estimate in both halves	960	0.842	0.859
In one half only	209	0.142	0.112
In neither half	19	0.015	0.029

Panel B. Standard errors with fewer closures under each imputation rule, SD

	Two scored years (4)	All with both halves (5)	Scored years (6)	Scored student records (7)	Largest (8)
Farthest from a surviving school first					
32 kept open, rural poor	0.0070	0.0079	0.0088	0.0086	0.0088
32 kept open, all other	0.0035	0.0036	0.0038	0.0039	0.0039
47 kept open, rural poor	0.0198	0.0203	0.0210	0.0215	0.0215
47 kept open, all other	0.0069	0.0071	0.0073	0.0087	0.0087
63 kept open, rural poor	0.0211	0.0218	0.0228	0.0232	0.0232
63 kept open, all other	0.0071	0.0073	0.0075	0.0089	0.0089
95 kept open, rural poor	0.0237	0.0246	0.0264	0.0338	0.0338
95 kept open, all other	0.0075	0.0079	0.0089	0.0162	0.0162
Largest model access gain first					
32 kept open, rural poor	0.0205	0.0211	0.0227	0.0298	0.0298
32 kept open, all other	0.0078	0.0080	0.0086	0.0152	0.0152
47 kept open, rural poor	0.0218	0.0226	0.0243	0.0310	0.0310
47 kept open, all other	0.0094	0.0099	0.0105	0.0163	0.0163
63 kept open, rural poor	0.0244	0.0256	0.0276	0.0341	0.0341
63 kept open, all other	0.0105	0.0113	0.0126	0.0184	0.0184
95 kept open, rural poor	0.0238	0.0251	0.0273	0.0338	0.0338
95 kept open, all other	0.0110	0.0123	0.0141	0.0192	0.0192

Notes. Schools that carry weight in comparisons of the closure lists and of closing fewer schools, with weight shares at 47 closures kept open, farthest first. A school without an estimate in both halves has no difference between halves, so its noise variance is imputed. Panel B gives the standard error of the change in school value-added before students sort again, per student the realized closures displaced. The four noise variance rules take estimates from schools with exactly two scored years, apply the same rule over all schools with both halves, or make variance inversely proportional to scored years or scored student records (the last two added after the results were seen). The largest is used here and in Table 7.

model placements. For fewer closures, the four counts form a family for each group and outcome. The score orders have six comparison families, with both evaluation directions required to agree in sign. Each family was specified before its own results, but the broader exploratory selection of exercises is not covered by the adjustment. Results outside these families, including all other students, school effectiveness, access by list and contrasts between the two orders, are descriptive estimates rather than adjusted statements of direction.

The intervals for students placed by the choice model are conditional on the delta method approximation, the noise calculation from the two halves and the independence assumption above. The allowance for differential placement error is a sensitivity assumption informed by the validation regressions for the realized policy, which do not bound error operating through unmeasured features. The 0.233 SD simultaneous bound is conditional both on the stated uncertainty calculation and on the allowance covering the true differential model error for each list. It is not evidence that the lists have equivalent school value-added. For a single contrast, the conditional reading thresholds range from 0.116 to 0.160 SD in absolute value, depending on the list (Table E5). These thresholds incorporate the model error allowance and sampling error, rather than a generic power calculation. The list setting the 0.233 SD simultaneous bound is *lowest average test scores first*.

A completed numerical simulation check draws the five choice coefficients 200 times from their estimated distribution, solves again the school mean utilities at each draw, and pairs each draw with random signs on the difference between halves of school value-added. There is one sign per municipality and one per school estimated on one half only, scaled by its imputed variance. The check reproduces the point estimates and checks arithmetic and curvature under this coefficient and noise construction from the two halves without validating the noise model or the counterfactual assumptions. No placed figure for school value-added fails the rule that compares its draw standard deviation with its governing standard error, so their reported intervals, including the simultaneous bound, are retained. The reported direction statements are unchanged by this numerical check.

For model access levels, draw standard deviations are 1.04 to 1.20 times standard errors from the delta method and draw means lie 0.3 to 0.4 standard errors above point estimates. The reported standard errors and intervals continue to use the delta method. Nine of the 85 newly computed access figures have draw standard deviations differing from their standard errors by more than 25 percent. These are marked (S) and print both values without an interval. Among the five main lists' mean contrasts, the only marked figure is other students' access contrast under *lowest scores first with an alternative within 4.8 km*, at $+0.004$ (SE 0.0033 and draw SD 0.0042). The other marked figures include that list's contrast for other students and its percent change in the first direction, the access gap contrast for *lowest scores first* in the second direction, and five rural-poor contrasts or percent changes for the *smallest first, 4.8 km variant with a growth condition*. None of the five main contrasts of access gaps is marked.

F Fiscal Accounting

This appendix sets out the account of savings used in Section 8.3, the only exercise in the paper that uses dollars. It gives the components of a closure's savings, the prices behind them, the totals, and what the account leaves out.

F.1 Scope of the Component Account

The account measures facilities spending and staff posts potentially avoided under stated price and cost assumptions. It excludes changes in transport, transition spending, spending associated with students who leave public schools and the capital value of closed buildings. Spending on a transition belongs to its year, while buildings belong to a capital account rather than recurring savings. An accounting saving from students leaving the system would not itself measure a social benefit. The component totals therefore differ from a full comparison of budgets.

F.2 Components and Prices of Savings per Closure

Facilities spending comes from a regression on the spending reported for 127 surviving schools in the 2019-20 school year. The fitted spending is \$18,657 (5,200) plus \$121.16 (9.64) per enrolled student, standard errors in parentheses, with an R^2 of 0.558. No school closed in 2013-17 appears among these schools, so applying the regression to the closures is an extrapolation. The low end keeps the intercept and subtracts an assumed deduction for a closed site of \$6,460 a year, giving \$12,197 per closure. The savings ledger derives the deduction by applying a 73 percent account share to reported utility payments of about \$8,850 per unused school. It is a calibration rather than a measured avoidable cost for a school in this sample. The high end adds the slope times the school's enrollment in its last operating year, \$121.16 per student. Whether a closure avoids this part of facilities spending is not known, and that is the reason for the range.

The high end also adds \$31,544 a year per closure for staff posts. In the Department's staff records, the staff of 467 closed schools show 496 more exits in the year after closure than the exit rate of staff in schools that stayed open implies, or 1.06 posts per closure. Each post is priced at \$2,475 a month, the median monthly wage of staff in closing schools, times twelve, or \$29,700 a year, which gives \$31,544 per closure. The count comes from the closure waves the staff records observe, which include closures after 2017, and the records cannot tell whether a released post was later refilled, so the high end assumes it was not. The wage comes from a payroll file that holds the workforce of 2024 back to 2010. It omits everyone who left before 2024, at least 28.6 percent of the 2013 workforce, so it prices posts at the wages of staff who stayed. The file supplies prices rather than payroll totals.

F.3 Totals and What the Account Leaves Out

For the 315 closures chosen again in Section 8.2, the amount at the low end sums to $3,842,055 a year, the enrollment component to $5,615,524 and posts to $9,936,360, so the range runs from $3,842,055 to $19,393,939 a year (Table F1). At the high end a closure saves $43,741 plus $121.16 per student, giving $60,582 at the median closed school of 139 students and $61,568 at the mean of 147.1 students. Because the high end grows with enrollment, equal counts need not forgo equal savings. Keeping open the first 47 closures of *farthest from a surviving school first* forgoes $573,259 to $2,925,029 a year, and the first 47 of *largest model access gain first*, which are larger schools, forgo the same low end and $3,226,838 at the high end.

Table F1. Components and totals of yearly savings from a closure

	Per closure	All 315 closures
Low end		
Facilities intercept	12,197	3,842,055
less assumed deduction for a closed site		
Added at the high end		
Facilities at 121.16 dollars a student	17,827	5,615,524
Staff posts	31,544	9,936,360
High end	61,568	19,393,939

Notes. Dollars a year. The low end takes the facilities intercept ($18,657) less an assumed deduction for a closed site ($6,460), from a regression on 127 surviving schools in 2019-20 applied to earlier closures. The high end adds the enrollment term at each closed school's enrollment in its last year and 1.06 posts per closure. The column per closure uses the mean closed school (147.1 students, 46,348 in all). Posts savings assume released positions are not refilled. The account excludes transport, transition spending, costs per student that follow the student, and capital. The two ends are a range of assumptions, not a confidence interval.

The account leaves out five items. It assumes that spending per student outside facilities follows the student to the receiving school, so no saving from that component is credited. Spending that stops when students leave the public schools is also excluded from this component account. Transport is not identified in the records, so the savings are gross of any added transport cost, and such a cost would lower the break even values of Section 8.3. Spending on the transition is not counted. The value of closed buildings belongs to a capital account and is never added as a recurring saving. The range is a range of assumptions, not a confidence interval and not a bound on what the closures saved, because the spending regression does not identify the cost each closure avoided.

Valuing the closures would require more than this account supplies. It would need how long the access loss lasts and a price for it, and an interpretation and a price for school value-added. Comparing lists at a common budget would also need the seats and costs of receiving schools, transport and feasible assignments. The paper supplies none of these, and it compares lists at the Department's counts, not at equal savings.

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