Journal of Urban Economics 71 (2012) 189–203

Contents lists available at SciVerse ScienceDirect

Journal of Urban Economics

www.elsevier.com/locate/jue

Closing schools in a shrinking district: Do student outcomes depend on which schools are closed?

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ARTICLE INFO

Article history:
Received 11 March 2011
Available online 10 October 2011

JEL classification:
I22
I23
I24
I28

Keywords:
School closures
Value-added
Low-performing schools

ABSTRACT

In the last decade, many cities around the country have needed to close schools due to declining enrollments and low achievement. School closings raise concerns about the possible negative impacts on student achievement, neighborhoods, families, and teaching staff. This study examines an anonymous urban district that, faced with declining enrollment, chose to make student achievement a major criterion in determining which schools would be closed. The district targeted low-performing schools in its closure plan, and sought to move their students to higher-performing schools. We estimate the impact of school closures on student test scores and attendance rates by comparing the growth of these measures among students differentially affected by the closures. We use residential assignment to school as an instrument to address non-random sorting of students into new schools. We also statistically control for the contemporaneous effects of other reforms within the district. Results show that students displaced by school closures can experience adverse effects on test scores and attendance, but these effects can be minimized when students move to schools that are higher-performing (in value-added terms). Moreover, the negative effect on attendance disappears after the first year in the new school. Meanwhile, we find no adverse effects on students in the schools that are receiving the transferring students.

1. Introduction

Closing schools is one of the most controversial actions a district can take. Nevertheless, urban districts across the nation are closing schools due to declining population, competition from charter schools, and accountability regimes (such as the federal No Child Left Behind Act) that target schools with chronically low achievement (see Sunderman and Payne, 2009). In total, 70 urban school districts shuttered schools between 2000 and 2010, closing on average 11 schools per district.[1] These districts are in over half the states, and they include not only rust belt cities such as Buffalo, Cincinnati, Detroit, Kansas City, and Pittsburgh, but also southern and western cities such as Birmingham, Little Rock, Richmond, San Antonio, Portland, Provo, and Seattle. The upsurge in school closings has given rise to concerns about their impacts on students, neighborhoods, families, and teaching staff (Steiner, 2009). With charter-school enrollments continuing to grow, and accountability regimes ratcheting up expectations, school closures will remain an important policy issue in cities across the country for the foreseeable future.

In this paper we examine a mid-sized urban district (unnamed at the request of the district) that recast the necessity of school closures as an opportunity to improve student achievement. The district's enrollment had been declining for many years, and it needed to close a substantial number of schools to reduce costs. If schools had to be closed, the district wanted to close the low-performing ones—and it commissioned a "value-added" analysis to identify them. Much like other districts (e.g., Chicago, Detroit, Kansas City, New Orleans), this district is using school closures in hopes of addressing the dual problems of low achievement and financial distress, consistent with the demands of No Child Left Behind (NCLB) and the Obama administration's "Race to the Top" initiative.

Our analysis evaluates the impact of school closures on student test scores and absenteeism. We examine these outcomes before and after the closures, using a longitudinal, student-level database. We examine absenteeism not only because it is among the

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[1] Information gathered from National Center for Educational Statistics Common Core data available at http://nces.ed.gov/ccd/bat/index.asp. Using these data, we examined school districts that are either in large or mid-size cities by the common core and have at least 10 schools in the 2009–2010 school year. In total there were 260 such school districts in 2009–2010 school year. Also, we should note that we assumed any district that had fewer schools in 2010 than 2000 had closed schools.

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doi:10.1016/j.jue.2011.10.001

academic outcome measures many schools have to meet as part of NCLB, but also because it may be related to important long-term outcomes, including graduation rates (Neild and Balfanz, 2006; Allensworth and Easton, 2007).

The analysis takes into consideration non-random student sorting into schools by using school assignments based on residential location as an instrument in an instrumental variable approach. We also statistically control for the contemporaneous effects of the changes in grade configuration and school reconstitution. We find that students displaced from closed schools can experience negative effects on achievement and attendance, but the effect on achievement can be offset when students move to schools with higher performance. Moreover, the initial effect on absenteeism disappears after the first year following school closure. The analysis also shows no adverse effects (as measured by test scores or absenteeism) on students in the schools receiving the transfer students. These results suggest that school closures can be implemented in ways that not only save money, but minimize the adverse effects on students.

2. Literature review

While there has been a fair amount of research in urban reforms more generally (Buddin and Zamarro, 2009; Kodel, 2008; Booker et al., 2008; Zimmer and Buddin, 2006; Lankford and Wyckoff, 2001), Sunderman and Payne (2009) note a general lack of research on the effect school closures have on student outcomes. However, there is a related literature on the effects of student mobility that consistently finds adverse effects of mobility on student outcomes (Hanushek et al., 2004; Booker et al., 2007; Xu et al., 2009; Ozek, 2009). A few studies have directly addressed the consequences of closing schools and transferring students. A study by the Consortium on Chicago School Research examined outcomes for 5445 Chicago students from 18 schools, of which half were closed due to underutilization and the other half due to poor academic performance (de la Torre and Gwynne, 2009). The authors compared the expected learning trajectories (as measured by math and reading test scores) to the students' actual learning trajectories. The authors found a negative effect on student achievement in the year the closings were announced, but found neither positive nor negative effects once students were enrolled in their new schools. However, the researchers also found that only 6% of students transferred to schools that had test scores in the top quartile of the district and students generally transferred to schools that were academically weak—suggesting that the school closure policy failed to place students in substantially better schools. A second Chicago study by SRI International examined the Renaissance 2010 initiative which had the goal of closing 60–70 schools and opening 100 new smaller schools by 2010. This study used a matching strategy to examine two cohorts of students from closed schools attending 23 newly-created schools and found that students generally performed at the same levels as matched comparison students (Young et al., 2009). Complementing this quantitative research is qualitative analysis of one high school closure in a western city (Kirshner et al., 2009), which suggested that transferring students to new schools disrupted their relationships with teachers. Students also reported differing academic norms, routines, and expectations in the new schools, which could create adverse learning effects.

Overall, this research suggests that students transferring out of closed schools do no better in their new schools. While this research is informative, to date it has not addressed the non-random sorting of students out of closed schools into new schools. Moreover, prior research has not yet had an opportunity to examine a school closure plan that explicitly sought to move students from low-value-added schools, which would be closed, to high-value-added schools.

3. Background on the district's reform efforts

In initiating the reform, the district planned to move from a system of small schools with significant variation in educational quality, equity and cost effectiveness to a system of quality schools that promote high student achievement in the most equitable and cost-effective manner. Overcapacity was great enough that the district entered the process with the expectation that it would need to close approximately 20 schools—about one-fourth of the schools operating at the time—at the conclusion of the 2005–2006 school year.

In selecting schools to be closed, academic performance was not the only criterion, but it was the first priority. There were four ways that the focus on academic performance was operationalized in the district's restructuring plan. First, high-performing schools were to be kept open unless doing so would create serious inequities in resource use. Second, any students who had to be moved because their school was closed should be moved to a school at least as high-performing as, and preferably higher performing than, the one they left. Third, all schools with value-added results that placed them in the lowest of four classifications would be closed or reconstituted with extended school hours, enhanced professional development, and a comprehensive Whole School Reform (WSR) model.[2] Fourth, where possible, closed middle schools would be replaced by expanding existing elementary schools from K-5 to K-8; the shift in grade configurations was intended to reduce the disruption often associated with switching schools, and was supported in part by analysis indicating that student achievement trajectories from grades 5 to 8 were more favorable in the district's existing K-8 schools than in its comprehensive (non-magnet) middle schools.[3]

The district ultimately identified 22 schools that would be closed, eight schools that would be reconstituted as accelerated academies through a WSR effort, and 13 schools that would be expanded from K-5 to K-8—with all of these changes occurring in the summer of 2006. In instituting its restructuring plan, the district recognized that moving students to new schools could create short-term social and academic disruptions for transferring students. But district leadership believed that placing students in higher-performing schools would ultimately lead to improved academic outcomes, even while operational costs were reduced by closing buildings.

The district's theory of action implicitly assumed that exposing students and teachers from low-performing schools to students and teachers from higher performing schools would improve performance (Lefgren, 2004; Hanushek et al., 2003[4]; Zimmer and Toma, 2000). We should note that teachers in closed schools did not, in general, lose their jobs, and instead had opportunities to transfer elsewhere (though some retired or left the district). While

[2] Evidence on the effects of comprehensive reform models is mixed. In a meta-analysis of comprehensive school reform models, Borman et al. (2002) found some promising results, but noted that results are affected by the quality of the reform designs.

[3] The district's decision to promote K-8 schools over middle schools is supported by evidence from other locations that shows that transitions into middle schools from elementary schools can have adverse academic, behavioral, or self-esteem effects (Alspaugh, 1998; Weiss and Kipnes, 2006; Byrnes and Ruby, 2007; Cook et al., 2008; Rockoff and Lockwoood, 2010).

[4] In addition to the positive externality high ability students can create, Hanushek et al. (2003) explored the possibility of externalities created by exposure to different racial and socioeconomic mixes. While their research found positive peer effects from students with strong test scores, they found no effect, either positive or negative from exposure to different racial and socioeconomic mix of students.

the district was assuming that teachers' effectiveness would improve through their transfer to higher performing schools, one could argue that teachers' effectiveness could be unaffected by their new surroundings and that closures would merely produce a game of musical chairs, failing to improve outcomes for students. Furthermore, it may actually create adverse effects for students because of student disruption from the moves (Hanushek et al., 2004; Booker et al., 2007; Xu et al., 2009; Ozek, 2009). Our aim is to examine the extent to which the district's approach succeeded in producing better results for students forced to move—without undermining the achievement of students who did not move.

4. School closures

The district used a School Performance Score (SPS) to guide school closing and reconstitution decisions. The SPS is a continuous scale between 0 and 5, with 5 representing the highest performance. It is the metric that was used by the district to identify low-performing schools and is based on several value-added measures, aggregated to the school level, from the school years 2001–2002 through 2004–2005. The value-added measures include average achievement gains, average achievement levels adjusted for student characteristics, and the coefficients on school dummies from a regression of achievement on student fixed-effects.

Fig. 1 shows the school-level histograms of the school performance score for those schools that closed, for those that remained opened, and for those that were reconstituted. These figures clearly indicate that the closed and reconstituted schools had lower school performance scores than other schools. The histogram for the closed schools also shows, however, that a few closed schools were relatively high performing. This reflects the decision of the district to close some schools for reasons other than performance, such as being too small to allow for cost-effective instruction.

These school closures affected a significant proportion of students. Nearly 25% of students in grades K through 8 were in schools that closed. With these closures came a large influx of students to the remaining schools. However, as shown in Fig. 2, the number of students arriving from closed schools varied across the remaining schools.

Fig. 3 indicates that most students from closed schools were assigned to schools with higher performance. However, a significant proportion of students from closed schools—around 21%—did not attend their newly assigned schools. Students could opt for a magnet program. All students assigned to the WSR schools were permitted to opt out of these extended-time schools. Under NCLB, students could opt out of Title I schools that were "identified for improvement" based on previous results that fell short of state standards. And students could opt out to attend charter schools, private schools, or (via residential mobility) schools in other districts.

Fig. 4 shows the difference in school performance scores between the assigned school and the school attended, for those not attending their assigned schools. As indicated by this figure, a significant proportion of students chose to attend schools with lower performance than the one assigned. If these school choices are correlated with unobserved factors that affect student outcomes, this will pose a problem for estimating the effect of school closures. The next section explains how we use information about school assignments to correct for the possible endogeneity of our measures of policy variables due to schooling choices made by families.

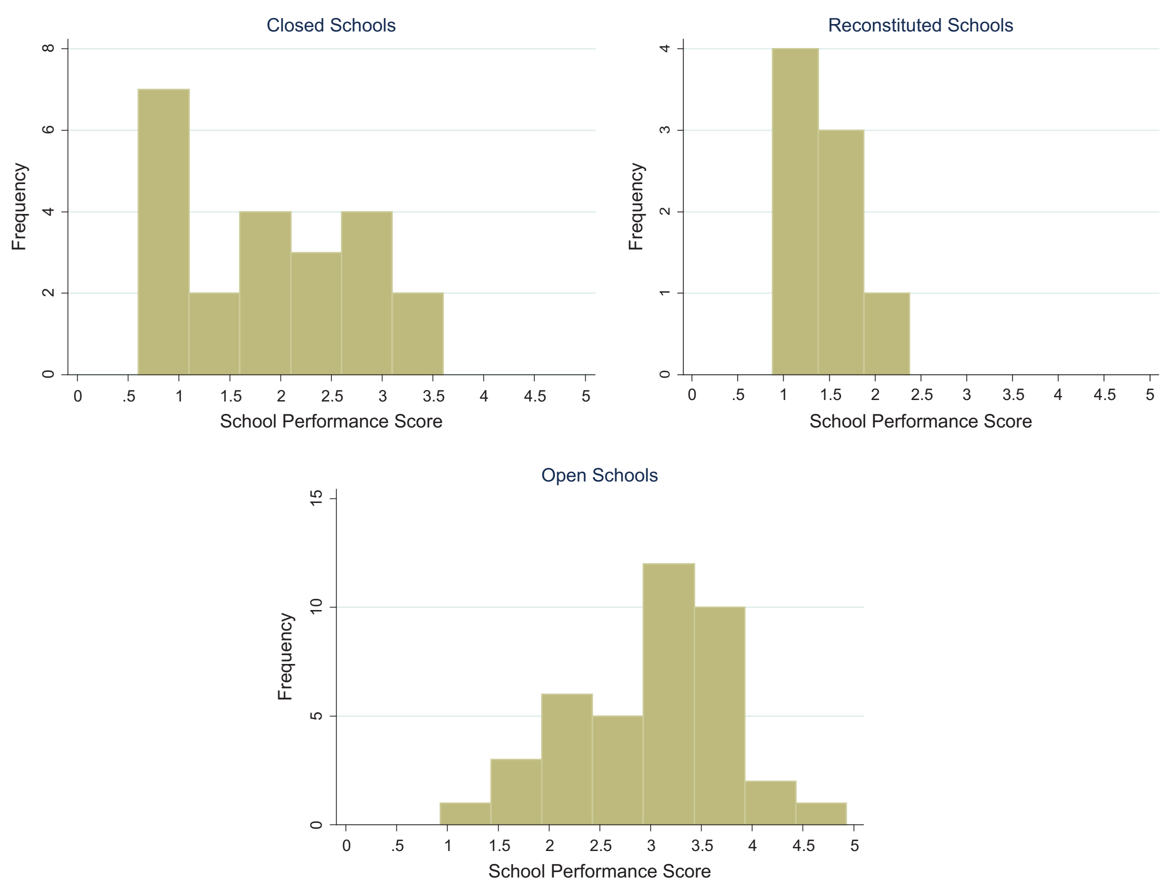

Fig. 1. School-level histograms of the school performance score.

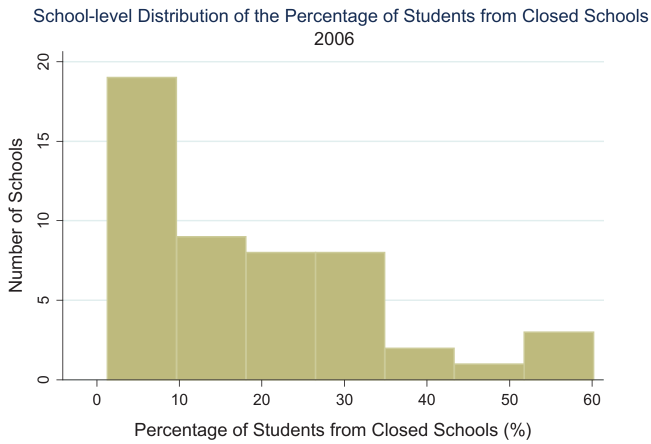

Fig. 2. Distribution of the percentage of students from closed schools.

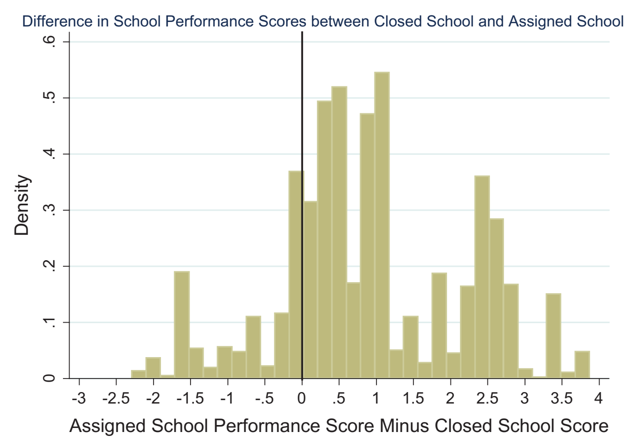

Fig. 3. Distribution of the difference in school performance scores between closed school and assigned school.

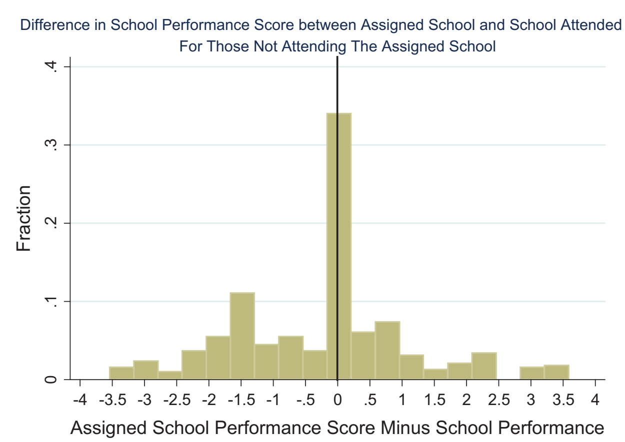

Fig. 4. Distribution of the difference in school performance scores between assigned school and school attended for those not attending their assigned school.

5. Analytical approach

Ideally, if school closings were exogenous and unexpected events, identifying their effect would be straightforward using standard OLS regression models. In practice, parents may try to engage in actions (such as relocating their children into better schools) that may endogenously affect the outcomes that are also affected by school closures. Standard OLS models can account for observed differences between groups, but they cannot account for additional differences that are unobserved but may affect school choice. Our strategy to address the unobserved differences is an instrumental variables approach. For the instruments, we use information about the student's residence and school catchment areas to construct variables based on students' new school assignments in the year following closures. Although the ultimate school attended is affected by family choices, the imposition by the district of new attendance zones in the wake of closures is arguably exogenous. In our data section below, we present some evidence regarding the exogeneity of residence with respect to the new school attendance zones.

We first examine absenteeism in a model that controls for pre-closure absences.

The formal model is defined by Eq. (1):

$$\log(Absences_{it}+1) = \alpha_0 + \alpha_1 \log(Absences_{i0}+1) + \alpha_2 X'_{it} + \alpha_{3t} P'_i + \varepsilon_{it},\ t = 1,2,3 \qquad (1)$$

For both the dependent and pre-closure independent variable measuring absences, we use the log of the number of annualized absences, defined as:

$$Absences = (\#absences)/\#Days\ in\ District) * 180$$

By using logs of absences variable,[5] we avoid issues of heteroskedasicity and extreme skew. The model also includes a vector of student characteristics, X_{it}, and a vector of policy variables, P_i. We estimate a single model that uses absences for three years following closures (t = 1–3) as outcomes and we allow α_{3t}, the effect of the policy variables, to change over time.

Similarly, in the model of achievement we use a value-added approach with controls for pre-closure achievement:

$$Y_{it} = \beta_0 + \beta_1 Y_{i0} + \beta_2 X'_{it} + \beta_{3t} P'_i + \varepsilon_{it}, \quad t = 1,2,3 \qquad (2)$$

In Eq. (2), the dependent variable (Y_{it}) is either the math or reading achievement score (run in separate models) for student i in each of the first three years after school closures.

For each model, we include the respective outcome measure immediately prior to school closures, Y_{i0} or $log(Absences_{i0} + 1)$, to control for student ability and past educational inputs. Therefore, the coefficients on the remaining variables can be interpreted as the effect on the difference in the outcome between year *0* and year *t*. In both models, X_{it}, is a vector of student characteristics that include demographic variables such as gender and race; indicators for the student receiving free- or reduced-price lunch; indicators for the student ever being classified as gifted, as a special needs student or as an English language learner; and a set of grade and year dummies. In addition, we include an indicator for students who voluntarily change schools for reasons other than closures (e.g., residential mobility) and a measure of the percentage of students in the school that are new to the school for similar reasons.

For both model (1) and (2), closures and reforms are represented by a vector of policy variables (P_i). First, we estimate the overall impact of school closings on students' outcomes via a dummy variable indicating if the student *i* was in a closed school. We also examine whether changes in quality between the closed school and the school the student attended after closures are related to student outcomes. We do this by including the difference in the school performance score between the closed school and the school the student attends immediately after the closings (year t = 1), both measured in t = 0.

[5] As an alternative transformation we also considered taking the squared root of the number of annualized absences. This, however, did not change the main results that are presented below.

Table 1
Residential mobility in years surrounding school closures.

School years	Proportion with any residential move	Proportion with residential move causing change in elementary feeder	Proportion with residential move causing change in middle feeder
2004–2005 to 2005–2006	0.190	0.144	0.095
2005–2006 to 2006–2007	0.180	School closures occurred in summer of 2006	
2006–2007 to 2007–2008	0.156	0.112	0.099
2007–2008 to 2008–2009	0.160	0.119	0.103
2008–2009 to 2009–2010	0.167	0.123	0.109

Note: "Feeder" is the school assigned to the student based on residential location.

Moreover, it is important to recognize that students in schools that remained open could nonetheless experience changes in their schooling due to the arrival of displaced students. School size of course increased in many receiving schools, and peer composition changed. To control for the possible effects of closures on these non-displaced students, we include the fraction of the school that student *i* attends that is comprised of students from closed schools in both model (1) and model (2). The set of policy variables also includes the following reform measures: a dummy indicating if student *i* attends a reconstituted school in year *t*, and dummies to distinguish K-8 from K-5 and middle schools. The effects of all policy variables are allowed to vary over time.

An indicator of voluntary mobility is also included as control. Changing schools can occur either due to a change in residential location or because a student takes advantage of choice options within the district. This voluntary move variable is included because prior research has shown that mobility is an indicator of disadvantage and because mobility itself can be disruptive to the learning process (Hanushek et al., 2004; Booker et al., 2007; Xu et al., 2009; Ozek, 2009). Furthermore, many of the reforms were related to student reassignment, so it is necessary to control for the ongoing voluntary mobility that is correlated with student characteristics and varies among schools. Additionally, we included controls for the proportion of students in each open school that voluntarily moved to the school in a particular year, excluding the flow of students arriving from closed schools. This variable is analogous to our measure of the proportion of students from closed schools, except that it measures *voluntary* influx. Finally, for each model, we use robust standard errors that account for the clustering of students within schools.

As noted previously, estimating the effects of school closings faces several challenges stemming from the non-random choice of schools by students. For example, students who are assigned to schools that remain open and who anticipate the arrival of a large proportion of displaced students may try to move to other schools. Similarly, students from closed schools may try to relocate from their newly assigned schools into other schools they think are most suitable. In addition, some determinants of student outcomes are measured poorly or omitted entirely. Under these circumstances, variables included in P_i, which use information on actual school attended, could be correlated with measurement error or other omitted variables, leading to spuriously significant effects.

Therefore, school variables in P_i are instrumented with information on assigned school. This approach follows Hoxby and Weingarth (2006) who used information about the student's residence and school catchment areas to construct variables based on a student's "simulated cohort." A student's "simulated cohort" is the group of students who are assigned to a specific school. We then constructed a variable denoting the proportion of new students that were expected to move to the school the student is assigned to, given the school assignments. This variable serves as an instrument for the proportion of students arriving from closed schools. Similarly we created a variable indicating the difference in quality between the closed school and the school to which the student was assigned immediately after the reform ($t = 1$). This variable is then used as instrument for the difference in quality between the closed school and the school which the student subsequently attended.

This choice of instruments leads to a just identified model which limits our possibilities for diagnosis of the validity of our instruments. In this respect, we provide results for the first stage regressions (see Appendix A Tables A4 and A5), as well as tests for weak instruments.[6] An important assumption, however, when using these instruments, is that residential location is exogenous, at least in the short run. We investigate this by examining the fraction of students that make any residential move between two school years and the fraction that make a move sufficiently large that it causes them to be assigned to an elementary or middle school that is different from the previous year. We study this for the year immediately prior to the closures and for the four years following the closures. However, data limitations prevent us from examining moves that result in a new school assignment in the year following the closures. Table 1 shows that total residential moves dropped following the school closures. Relocations resulting in changed elementary attendance zones dropped 3% points following closures and those resulting in changed middle school attendance zones increased approximately 1% point. We take this as evidence that residential location is exogenous with respect to the new school boundaries during the 3 years following the school closures that are included in our analysis.

6. Data

This study was made possible due to access to the anonymous district's longitudinal student data warehouse. The district has maintained student-level data on enrollment, demographics, residential location, student attendance, and state achievement test performance since the late 1990s. We were able to construct an analytic data set that associated each student with his/her achievement test scores and attendance rates, with the school s/he was attending, and with the assigned neighborhood school, which was not always the same as the one attended. In addition, we were

[6] Bound et al. (1995) argue that an analysis may be better off using an OLS model as opposed to instrumental variable (IV) approach if there is a weak correlation between the instrument and the endogenous variable(s). If there is a weak correlation, then any relationship between the instrument and error term "can be magnified and make the IV worse than OLS, even if we restrict attention to bias" (Wooldrige, 2009, p. 515). To test the possibility of a weak correlation between the instrument and the endogenous variables, we employed a test for weak instruments suggested by Staiger and Stock (1997) by modeling the correlation between the instrument and the endogenous variables as a function of sample size. Using F statistics generated from the model, we examined whether we had a strong correlation. Overall, the analysis suggested that we reject the hypothesis that all instruments are not relevant. Second, nearly all correlations between the instruments and endogenous variables were statistically significant. In particular the correlations between the endogenous policy variables of interest (i.e., the difference in school quality between the new and closed schools as well as the percentage of students from closed schools) and the instruments were highly significant.

Table 2
Distribution of student observations by school year, grade and type of analysis.

Grade	School year			
	2005–2006 (baseline)	2006–2007	2007–2008	2008–2009
Annualized absences analysis				
1	1925	110	0	0
2	1888	1864	157	1
3	1783	1862	1639	168
4	1868	1785	1615	1531
5	1903	1859	1593	1489
6	1994	1900	1642	1421
7	1951	1994	1667	1495
8	20	1958	1754	1539
Achievement analysis-math				
3	1717	24	0	0
4	1790	1718	58	0
5	1822	1777	1590	57
6	1877	1811	1625	1408
7	1496	1862	1659	1474
8	13	1488	1678	1496
Achievement analysis-reading				
3	1707	23	0	0
4	1780	1705	57	0
5	1817	1764	1582	56
6	1868	1799	1616	1394
7	1490	1852	1659	1464
8	14	1482	1667	1493

able to link each student to his/her demographic characteristics and special needs status.

This study uses panel data for students for four consecutive school years from 2005–2006 to 2008–2009. To be considered in our analysis the student must be present in the district immediately before the closures (2005–2006) and immediately after the closures (2006–2007). In math, the final sample consists of 8680 students who contributed to 19,725 observations (the reading sample has similar number of students and observations). Table 2 shows the distribution of observations by school year, grade and type of analysis. In this respect, we should note that because students are only tested in a subset of grades, we are able to observe attendance numbers for substantially more students and years than we are able to observe students with test scores.

Table 3 provides descriptive statistics for the information that we use in our analysis of reform impacts, as well as for the subset that were dislocated due to a school closing. The dependent variables for the models, described in the first three rows are math and reading achievement scores as well as annualized absences, calculated as the number of days absent divided by days in the district multiplied by the standard number of days within a year (180 days).[7] For math and reading test scores, each student's score is standardized by subtracting the statewide mean and dividing by the statewide standard deviation for the specific grade and year. This standardization allows the variable to be interpreted relative to statewide performance.

The next several rows describe demographic characteristics. The district divides the student population into self-reported ‘‘African-American'' and ‘‘Other'' groups for purposes of tracking racial integration. ‘‘Other'' consists almost entirely of non-Hispanic white students. We interact this racial grouping with student gender, creating four race-by-sex groups. We also include information on whether a child was ever designated as gifted, an English language learner (ELL) or had other special needs. Finally, we included the student's eligibility for free or reduced-price lunch (FRL), which is the only available student-specific measure of family economic status. Dislocated students are much more likely to be African American (40% male and 37% female) and to receive free or reduced price lunch (84%) and much less likely to be gifted (3%) than the average student in the district (28% are African American, 69% receive free or reduced lunch, and 11% are gifted students on average).

To construct the indicator of voluntary mobility, a student is coded as having changed schools if s/he is in a different school at the time of testing than s/he was at the time of testing in the previous year. Structural movements to 6th grade for those attending K-5 schools are not considered voluntary moves. Around 10% of students in the sample move voluntarily. We also constructed a variable measuring the percentage of students in each open school that voluntarily moved to the school in a particular year, excluding the flow of students arriving from closed schools. This is analogous to our policy variable measuring the proportion of students from closed schools, except that it measures *voluntary* influx.

The policy variables capture various aspects of the reforms. We include a dummy variable indicating whether a student was forced to move as a result of school closure (which identifies students enrolled in schools in the final year prior to closure, except the students in the schools' terminal grades, because they would have aged out independently of closure). By this definition, 18% of students in the district are classified as coming from closed schools. We also allow for closures to have differential effects on achievement depending on the quality of the school to which the student ended up going after the reform. The School Performance Score (SPS) variable measures the difference between the school the student attends in the year immediately after the reform and the school that s/he was attending before the closings.[8]

The flip side of students leaving closed schools was a substantial influx of students into some of the schools that remained open, which could create disruption and negative peer effects (Lefgren, 2004; Hoxby, 2003; Zimmer and Toma, 2000). We calculate the proportion of students in each open school in 2006–2007 (the first year after the reconfiguration) that came from schools which were closed before the start of the school year as a measure of this influx (PC, which stands for Proportion from Closed). The value of PC is set equal to zero for relocated students. Therefore, the coefficient on PC only captures the impact on the students who would have been in those schools even if there had been no closings. The average value of 13% for this variable in the first column of Table 3 reflects the weighted average of the zeros from the 18% of students who are from closed schools and the non-zero values from the 82% of students from schools that remained open. Therefore, the average value of PC for students from schools that remained open is 16%.

Another aspect of the reform was the reconfiguring of many schools from K-5 to K-8, in part to absorb the students from closed middle schools. We constructed indicator variables for K-5 and middle school (i.e. grades 6 through 8) configurations, with K-8 being the omitted category. A final aspect of the reforms was the reconstitution of eight very low performing schools. As explained above, these are schools that would have been closed based on their performance, but the district needed the facilities to remain open in order to serve the need in particular neighborhoods where higher-performing schools were not available. Therefore, they were chosen for WSR programs. We construct an indicator variable for these schools.

[7] We standardized the annualized absences because some students exit or enter the school district in the middle of the year. In addition, we dropped anomalous records that suggested a student had been absent more days than existed in the district's calendar. A total of 578 observations were deleted. We also only included students who were enrolled in the district at least 100 days during the year.

[8] We use values of School Performance Score (SPS) based on the school attended immediately after the reform, rather than the value of SPS associated with the current school if students move again in the following years. This is intended to capture the ''treatment'' effect of the relocation from the closed school to the new school in 2006–2007, The medium-term effect of this treatment is then captured during years two and three following relocation, regardless of where the student is during those years.

Table 3
Descriptive statistics.

Variable	District-wide		Dislocated students	
	Mean	Standard deviation	Mean	Standard deviation
Standardized math score	−0.30	0.97	−0.63	0.89
Standardized reading score	−0.33	0.99	−0.71	0.89
Annualized Absences	12.6	11.6	14.9	13.1
African-American male	0.28	0.45	0.40	0.49
African-American female	0.28	0.45	0.37	0.48
Other male	0.22	0.42	0.12	0.32
Other female (omitted)	0.21	0.41	0.11	0.31
Gifted	0.11	0.31	0.03	0.18
English language learner	0.02	0.16	0.003	0.006
Other special needs	0.31	0.46	0.34	0.47
Free or reduced lunch	0.69	0.46	0.84	0.37
Voluntary mobility	0.10	0.30	0.09	0.29
Proportion of students moving from other open schools*	0.16	0.11	0.17	0.12
Policy measures				
Student from closed school (PC)	0.18	0.39	1	0
School Performance Score difference due to moving from a closed school (SD)*	n/a	n/a	0.88	1.3
Proportion of students from closed schools (CL)*	0.13	0.13	n/a	n/a
K-5*	0.33	0.47	0.33	0.27
Middle school*	0.27	0.44	0.15	0.36
K-8*	0.40	0.49	0.52	0.50
Whole School Reform (WSR)*	0.18	0.38	0.26	0.44
Instruments (values reflect school assignment based on place of residence rather than actual attendance based on school choice options)				
School performance score difference relative to previously assigned school*	n/a	n/a	0.82	1.3
Proportion of students from closed schools*	0.14	0.12	n/a	n/a
K-5*	0.33	0.47	0.37	0.48
Middle school*	0.20	0.40	0.14	0.35
Whole school reform*	0.29	0.45	0.28	0.45
Number of students (*N*)	8680 (in math)	1806 (in math)		
	8625 (in reading)	1794 (in reading)		
	13,332 (in absences)	2618 (in absences)		
Number of student × year observations ($N \times T_i$)	19,725 (in math)	3525 (in math)		
	19,613 (in reading)	3498 (in reading)		
	31,043 (in absences)	5746 (in absences)		

Note: Scores are standardized based on statewide mean and standard deviation.
* Indicates calculated using records for the school year immediately after the reform, 2006–2007.

In addition to these explanatory variables that we use in the regression, we constructed several instruments for the endogenous explanatory variables as described in previous section. The movement of students following the reforms reflected changing opportunities as well as decisions by students and their families. Thus, we take the change in school options following the school closures as exogenous and use the new school assignments as instruments for the explanatory variables that reflect choices among these new options. Therefore the instruments for each of the reform variables are the same variables calculated using the residence-based school assignment for each student rather than the school actually attended, which is based on student choices as well as the exogenous options.

7. Results

We first display the results for attendance patterns in Table 4.[9] The first column indicates the number of years after students were transferred out of the closed school. The second column indicates the effect on students from the closed schools in the first, second, and third year after they were transferred out. The third column focuses on students in ongoing schools and indicates the effect of the proportion of students within the schools that transferred in from closed schools in the first, second, and third years.

The results suggest that there is an initial spike of 13% in absenteeism on students who transferred out of closed schools. However, this spike drops and is not significant by the second year.

The results also suggest no relationship between the percentage of students coming in from closed schools and student absenteeism in ongoing schools. The point estimates for the PC variable reflect the estimated effect if 100% of the students in a school were new; scaling them down to values appropriate for actual changes suggests that effects, if any, are small. For example, the first year estimate indicates that a school with 10% of its population made up of students coming from closed schools would have an average achievement that is 0.012 standard deviations less than a school with no students coming from closed schools, with a confidence interval from −0.03 to 0.06.[10]

Table 5 shows the estimated coefficients of closure variables included in model (2) for both math and reading.[11] In the table, all coefficients are reported in standardized effect sizes reflecting the use of a z-score transformation of test scores. The coefficients for the relocation variable (CL) are negative and statistically significant in the first year. For a student who moves to a school with an

[9] See Appendix A for complete tables including the coefficients of the rest of variables included in Eq. (1). These variables include demographic controls, an indicator for voluntary mobility, a measure of the percentage of students in the school that are new to the school, pre-closure absences, and pre-closure absences interacted with grade and year dummies.

[10] Results still hold when restricting the sample to the same grades available in the student achievement analysis, as well as when including measures of the school performance score difference between the school the student attends in the year immediately after the reform and the school that s/he was attending before the closings as we did for the estimation of achievement effects.

[11] See Appendix A for complete tables including the coefficients of the rest of variables included in Eq. (2). These variables include demographic controls, an indicator for voluntary mobility, a measure of the percentage of students in the school that are new to the school, pre-closure test scores, and pre-closure test scores interacted with grade and year dummies.

Table 4
Estimated effects (IV) of school closures on absences.

Years since closure	Relocation (CL)	In receiving school, percent of tudents who are from closed schools (PC)
1	0.13*	0.12
	(0.05)	(0.22)
2	0.06	−0.06
	(0.05)	(0.17)
3	0.07	0.25
	(0.04)	(0.15)

Notes: (i) Just identified model, N. Obs: 31,043. (ii) Cluster by school robust-standard errors in parentheses.
* Significance at the 5% level.

Table 5
Estimated effects (IV) of school closures on cumulative reading and math achievement.

Years since closure	Relocation (CL)	Difference in school performance score relative to prior school (SD)	In receiving school, percent of students who are from closed schools (PC)
Math			
1	−0.19*	0.09*	−0.18
	(0.05)	(0.02)	(0.25)
2	−0.17	0.07*	−0.44
	(0.10)	(0.02)	(0.40)
3	−0.14	0.06*	−0.17
	(0.08)	(0.03)	(0.31)
Reading			
1	−0.20*	0.07*	−0.42
	(0.05)	(0.02)	(0.22)
2	−0.07	0.06*	0.03
	(0.05)	(0.02)	(0.26)
3	−0.03	0.04	0.19
	(0.05)	(0.03)	(0.19)

Notes: (i) Just identified model, N. Obs: 19,725 (math) and 19,613 (reading) (ii) Cluster by school robust-standard errors in parentheses.
* Significance at the 5% level.

equivalent school performance score, these coefficients imply effect sizes are −0.19 in math and −0.20 in reading. Overall, this suggests that there are non-trivial adverse transitional effects for students from closed schools. Although the second and third year effects are statistically insignificant, the coefficients suggest that the negative effect in math may persist over time.

As previously noted, however, the district constructed its closure and restructuring plan with the aim of moving students to higher-performing schools, so the effect for students moving to schools with merely equal performance should not have been typical. The coefficients on the difference in the School Performance Score (SD) capture the differing effect of school closure for students attending schools with various levels of performance must be interpreted in conjunction with the coefficient on CL. In Table 5, the effect of SD is positive and statistically significant in the first and second year and nearly statistically significant in the third year. This suggests that students from closed schools who move to high performing schools will increase their test score more (or decrease less) than students who move to lower performing schools. When the effects estimated by the coefficients on CL and SD are combined, we find effect sizes of −0.13 in reading and −0.11 in math for a student with the average change in school performance score of 0.88.[12]

[12] Results were similar when excluding variables on the difference in SPS as controls. In this case the effect sizes were −0.10 for math and −0.13 for reading, which is nearly identical to the results reported in the text based on the average change in SPS.

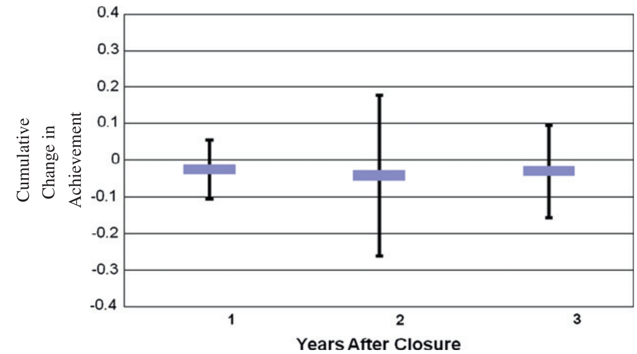

Fig. 5. Math achievement effect of relocation, relocation to higher quality schools: 75th percentile.

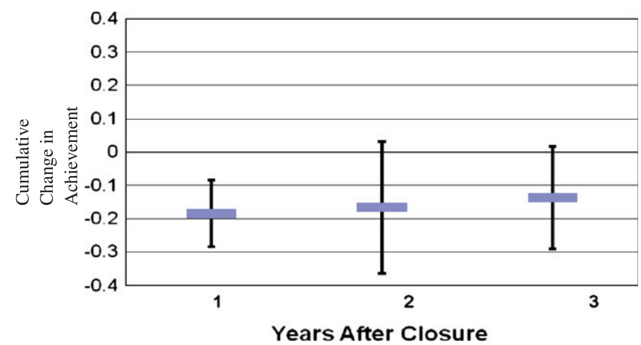

Fig. 6. Math achievement effect of relocation, relocation to same quality schools: 25th percentile.

The effect for students in receiving schools (PC) does not achieve statistical significance in any of the models or years. The first year math estimate indicates that a school with 10% of its population made up of students coming from closed schools would have an average achievement that is 0.018 standard deviations less than a school with no students coming from closed schools, with a confidence interval from −0.07 to 0.03.

From a policy perspective, these results suggest that if a district has to close schools, the district could offset adverse effects by having students move to higher-performing schools. Whether these adverse effects can be completely offset by the sending students to higher quality schools depends upon the relative performance of the closed schools and the receiving schools. Figs. 5 and 6 illustrate this point using math test scores. In these figures, we show both the cumulative point estimate (as displayed by the bars in the graph) and the confidence interval of the estimates (lines in the graph) for two scenarios. For each figure, we display the estimated effect for the first, second, and third years. Fig. 5 displays the effect for students moving to a school whose SPS is sufficiently higher than the SPS of the closed school so that the "SPS gain" for those students is in the 75th percentile of all SPS gains for students forced to move because of closure. The 75th percentile of all SPS gains is approximately equivalent to moving to a school with higher SPS than that of the closed school by about 1.0 SPS units. Fig. 6 displays the effect for a move at the 25th percentile of change in school quality, which corresponds with a move to a school with an SPS level that is comparable to the SPS of the closed school. Students relocated to schools of substantially higher quality (75th percentile change in SPS) experienced no significant drop in achievement. On the other hand, students relocated to schools of similar quality (25th percentile change in SPS) experienced a large, initially significant and apparently persistent drop in

Table 6
Estimated effects (IV) of school closures on cumulative reading and math achievement by race.

Years since closure	Relocation		Difference in school performance score relative to prior school		In receiving school, percent of students who are from closed schools	
	Black	White	Black	White	Black	White
Math						
1	−0.28*	−0.06	0.08*	0.06	−0.71	0.36
	(0.09)	(0.06)	(0.02)	(0.04)	(0.49)	(0.26)
2	−0.20	−0.06	0.08*	0.07	−0.56	−0.27
	(0.10)	(0.14)	(0.02)	(0.07)	(0.44)	(0.39)
3	−0.12	−0.04	0.07	0.05	0.08	−0.01
	(0.11)	(0.11)	(0.04)	(0.05)	(0.42)	(0.27)
Reading						
1	−0.25*	−0.06	0.08*	0.02	−0.55	−0.27
	(0.09)	(0.06)	(0.02)	(0.04)	(0.55)	(0.24)
2	−0.14	0.08	0.06*	0.02	−0.26	0.06
	(0.08)	(0.11)	(0.02)	(0.06)	(0.47)	(0.25)
3	−0.06	0.15	−0.03	0.03	0.22	0.13
	(0.08)	(0.13)	(0.08)	(0.06)	(0.54)	(0.27)

Notes: N. Obs for math analysis: 11,158 (Black students) and 7255 (White students); N. Obs for reading analysis: 11,113 (Black students) and 7212 (White students); cluster by school robust-standard errors in parentheses.
* Significance at the 5% level.

Table 7
Estimated effects (IV) of school closures on cumulative reading and math achievement by free or reduced lunch status.

Years since closure	Relocation		Difference in school performance score relative to prior school		In receiving school, percent of students who are from closed schools	
	FRL	Non-FRL	FRL	Non-FRL	FRL	Non-FRL
Math						
1	−0.21*	−0.13*	0.09*	0.08*	−0.32	0.30
	(0.05)	(0.08)	(0.02)	(0.03)	(0.23)	(0.49)
2	−0.16	−0.18	0.06*	0.10	−0.38	−0.57
	(0.10)	(0.20)	(0.02)	(0.05)	(0.39)	(0.54)
3	−0.12	−0.14	0.05	0.10*	−0.09	−0.24
	(0.08)	(0.16)	(0.03)	(0.05)	(0.29)	(0.49)
Reading						
1	−0.20*	−0.21*	0.06*	0.09*	−0.41*	−0.36
	(0.04)	(0.08)	(0.02)	(0.03)	(0.20)	(0.50)
2	−0.04	−0.15	0.06*	0.05	0.10	−0.09
	(0.06)	(0.14)	(0.02)	(0.05)	(0.25)	(0.41)
3	−0.01	0.14	0.04	0.02	0.41	−0.23
	(0.05)	(0.11)	(0.03)	(0.06)	(0.22)	(0.35)

Notes: N. Obs for math analysis: 13,320 (FRL students) and 6405 (non-FRL students); N. Obs for reading analysis: 13,258 (FRL students) and 6355 (non-FRL students); cluster by school robust-standard errors in parentheses.
* Significance at the 5% level.

achievement. We should note, while we only show math results here, the reading results are similar.

To examine the robustness of our achievement results, we conducted a number of sensitivity analyses and specification checks. First, we conducted a value-added analysis in which we used a lagged test score on the right hand side. While our main achievement analysis as specified in Eq. (2) controls for achievement before the closures, the alternatively specified value-added model with lagged test score controls for the previous year's achievement. Therefore, the coefficient on the policy variables in our main model measures cumulative effects of the reform whereas the policy coefficients in the alternative model measure the annual effects of the reform. The alternative model has the advantage that it uses up-to-date information about student achievement as an explanatory variable. However, there is a concern that post-closure test scores would be endogenous if closings affected test scores. As a solution to this issue we use pre-closure test scores (Y_{i0}) as instruments for prior year test scores ($Y_{i,t-1}$). This sensitivity analysis leads to very similar results with an initial dip in performance for students in the first year after the school closure. In years two and three, there were neither significant gains nor losses in the performance of students. In a separate sensitivity analysis, we examined the sensitivity of our decision to pool three years of outcome data together. We ran each model separately by year, thereby allowing the coefficients of all the control variables to differ for years 1, 2 and 3. We found no appreciable difference in the results.[13]

We also examine the assumption that the choice of school prior to school closings is exogenous. In the analyses we have reported, we instrument for the changing school choices due to school closures but not for the choice of school prior to the reforms. The instruments are based on assumption that residential location is exogenous in the short run, so the change in school options due to closures and resulting changes in feeder patterns are exogenous, whereas a change in the school that a student attends will also reflect choices among these options. However, it should be acknowledged that the school attended prior to the reforms reflects long term residential choices as well as a choice among schools given

[13] Results from the sensitivity analyses and specification checks are available upon request.

Table 8
Estimated effects (IV) of school closures on cumulative reading and math achievement by special education status.

Years since closure	Relocation		Difference in school performance score relative to prior school		In receiving school, percent of students who are from closed schools	
	Special education	Non-special education	Special education	Non-special education	Special education	Non-special education
Math						
1	−0.18*	−0.18*	0.10*	0.08*	−0.04	−0.24
	(0.07)	(0.05)	(0.02)	(0.02)	(0.24)	(0.30)
2	−0.17	−0.16	0.10*	0.06*	−0.32	−0.46
	(0.09)	(0.11)	(0.03)	(0.03)	(0.33)	(0.46)
3	−0.21	−0.10	0.12	0.03	−0.03	−0.27
	(0.11)	(0.08)	(0.07)	(0.03)	(0.31)	(0.34)
Reading						
1	−0.22*	−0.19*	0.09*	0.06*	−0.18	−0.56
	(0.09)	(0.04)	(0.04)	(0.02)	(0.25)	(0.29)
2	−0.21*	0.01	0.07*	0.05	−0.32	0.18
	(0.10)	(0.06)	(0.03)	(0.03)	(0.34)	(0.30)
3	−0.18	0.05	0.20	0.002	0.04	0.34
	(0.22)	(0.05)	(0.39)	(0.03)	(0.63)	(0.22)

Notes: N. Obs for math analysis: 5933 (Special education students) and 13,792 (non-special education students); N. Obs for reading analysis: 5883 (special education students) and 13,730 (non-special education students); cluster by school robust-standard errors in parentheses.
* Significance at the 5% level.

residential location. Unfortunately, we do not have an instrument for residential location choice, so we can only instrument for the choice of schools prior to reforms given the residential location. When we do so using a similar strategy of instrumenting for whether a student attended a school that was subsequently closed using feeder pattern information, we obtain results that are very similar to the reported results, only somewhat less precise.

We also were concerned that our findings might be driven by omitted variables. If the students from the closed schools were primed for low achievement growth due to factors associated with their attendance at these schools but not observed by us, they might have had lower achievement growth even if they had not been dislocated by the school closings. To test this, we considered model 2 and incorporated additional dummies for the achievement growth one, two and three years after the reform for fifth graders that were in closed K-5 schools at the time of the reform. These students were not dislocated because they would have made a move to a new school even without the school closings, but presumably they share the same unobserved characteristics as the dislocated students. We also incorporated controls for structural changes in other years. We found that the achievement growth of the fifth graders from the closed K-5 schools was indistinguishable from that of fifth graders in K-5 schools elsewhere, leading us to conclude that we had indeed found a true effect of school closure.

We also examined results from our analysis in Eq. (2) by race, free-and-reduced lunch (FRL) status, and special needs status, as shown in Tables 6–8. We find that the overall patterns of magnitude and significance displayed in Table 5 are repeated for each of the subgroups except for the subgroup of White students. As shown in Table 2, 88% of the dislocated students were African American. Therefore, the number of White students in closed schools was rather small. The point estimates from the regression using White students suggests a similar advantageous effect of relocating to a better school, although the White students did not appear to suffer a relocation penalty *per se*.

8. Conclusions and Implications

Traditionally, districts would close schools only in response to facility obsolescence, falling enrollments, or fiscal shortfalls. In recent years, however, policymakers have begun to think of school closures as means of improving student achievement. For instance, beginning in 2002, NCLB included school closure as a possible sanction for schools that chronically fail to meet academic targets. The Obama administration has brought additional energy to this effort by providing large federal grants for the closing of low-performing schools and reopening new schools.

In this analysis, we examine the rate of absences and achievement of students after the reassignment, controlling for the reforms in the reconstituted and reconfigured schools and the non-random choices by students of new schools. The results show that the transition to new schools can have an adverse effect on attendance and achievement gains for students from closed schools, but these effects can be minimized when students move to higher-performing schools. The negative effect on attendance vanishes after the first year following closure, but the negative effect on achievement appears to persist – unless the students are transferred to substantially higher-performing schools. The analysis also shows no detectable adverse effects on either the attendance or the achievement of students in schools that receive the influx of students from the closed schools.

From a policy standpoint, this suggests that if a district needs to close schools because of fiscal challenges or overcapacity, then closing low performing schools and transferring students to higher performing schools can minimize adverse effects. However, our analysis does not necessarily support school closures as a means for improving student achievement. The evidence from this school district suggests that producing higher levels of achievement would require moving students to schools that are dramatically higher achieving than the schools they left.

Given that this is only one district, caution is warranted in interpreting these results too broadly. However, the results make a strong case for further research in a broader set of districts and circumstances to examine whether school closures can be an effective policy for improving student achievement. In addition, further research is needed to examine whether the effects we observe in this study change over a longer time horizon. It may be the case that the negative transitional effects we observe in this study are driven by disruptions created by attending a new school and would be offset in the long-term by attending a high quality school. Moreover, achievement benefits may be larger for subsequent cohorts of students who never experience the disruption of the move, and begin their education in the higher-performing schools; further examination of the long-term effects for such students is merited, after sufficient time has passed to examine new cohorts.

Acknowledgements

Support for this research was provided by the Institute of Education Sciences. We would like to thank Drs. Dennis Epple and Holger Sieg for their helpful comments throughout the research project. In addition, we would like to thank discussants and conference participants for helpful comments at American Education Finance and Policy, American Education Research Association, and Society for Research on Educational Effectiveness.

Appendix A

See Tables A1–A5.

Table A1
Annualized absences: controlling for pre-closure absences (OLS and IV).

	OLS	IV		OLS	IV
Pre-closure Absences_Gr 1_y1	0.43*	0.44*	English Language Learner	−0.15*	−0.15*
	(0.1)	(0.1)		(0.04)	(0.04)
Pre-closure Absences Gr 2_y1	0.59*	0.59*	Gifted	−0.15*	−0.17*
	(0.02)	(0.02)		(0.02)	(0.03)
Pre-closure Absences_Gr 3_y1	0.56*	0.56*	Special Needs student	0.05*	0.06*
	(0.02)	(0.02)		(0.01)	(0.01)
Pre-closure Absences _Gr 4_y1	0.58*	0.58*	Voluntary Mover	0.13*	0.14*
	(0.02)	(0.02)		(0.02)	(0.02)
Pre-closure Absences _Gr 5_y1	0.57*	0.57*	**Student from Closed School-y1**	0.16*	**0.13***
	(0.03)	(0.02)		(0.04)	**(0.05)**
Pre-closure Absences _Gr 6_y1	0.58*	0.59*	**Student from Closed School-y2**	0.01	**0.06**
	(0.03)	(0.03)		(0.03)	**(0.05)**
Pre-closure Absences _Gr 7_y1	0.63*	0.63*	**Student from Closed School-y3**	0.09*	**0.07**
	(0.02)	(0.02)		(0.04)	**(0.04)**
Pre-closure Absences _Gr 8_y1	0.63*	0.63*	**Prop from Closed_y1**	0.35*	**0.12**
	(0.04)	(0.04)		(0.13)	**(0.22)**
Pre-closure Absences _Gr 2_y2	0.51*	0.51*	**Prop from Closed_y2**	−0.02	**−0.06**
	(0.06)	(0.06)		(0.13)	**(0.17)**
Pre-closure Absences_Gr 3_y2	0.48*	0.48*	**Prop from Closed_y3**	0.36*	**0.25**
	(0.03)	(0.03)		(0.11)	(0.15)
Pre-closure Absences _Gr 4_y2	0.46*	0.46*	Prop from other_open_y1	−0.12	−0.20
	(0.03)	(0.03)		(0.21)	(0.27)
Pre-closure Absences Gr 5_y2	0.48*	0.49*	Prop new to school_y2	0.34	−0.83
	(0.02)	(0.02)		(0.34)	(0.65)
Pre-closure Absences Gr 6_y2	0.51*	0.52*	Prop new to school_y3	0.12	0.04
	(0.03)	(0.03)		(0.29)	(0.41)
Pre-closure Absences _Gr 7_y2	0.49	0.50*	Reconstituted School_y1	0.15*	0.13*
	(0.03)	(0.03)		(0.04)	(0.05)
Pre-closure Absences _Gr 8_y2	0.57*	0.59*	Reconstituted School_y2	0.1	0.05
	(0.02)	(0.02)		(0.05)	(0.07)
Pre-closure Absences _Gr 2_y3	0.40*	0.40*	Reconstituted School_y3	0.20*	0.20*
	(0.10)	(0.10)		(0.04)	(0.07)
Pre-closure Absences_Gr 3_y3	0.39*	0.39*	k5_y1	−0.06	−0.10*
	(0.10)	(0.10)		(0.04)	(0.04)
Pre-closure Absences_Gr 4_y3	0.46*	0.46*	k5_y2	−0.05	−0.12*
	(0.03)	(0.03)		(0.04)	(0.06)
Pre-closure Absences Gr 5_y3	0.47*	0.47*	k5_y3	−0.05	−0.10
	(0.02)	(0.02)		(0.04)	(0.07)
Pre-closure Absences Gr 6_y3	0.48*	0.48*	ms_2006	−0.005	0.05
	(0.02)	(0.02)		(0.06)	(0.09)
Pre-closure Absences _Gr 7_y3	0.46*	0.46*	ms_2007	−0.03	0.39
	(0.03)	(0.03)		(0.09)	(0.20)
Pre-closure Absences _Gr 8_y3	0.38*	0.38*	ms_2008	0.06	0.09
	(0.04)	(0.04)		(0.07)	(0.11)
African American-Female	−0.01	0.0007	Constant	1.22*	1.14*
	(0.02)	(0.02)		(0.28)	(0.19)
Other-Male	0.03*	0.03*	Adj. R-squared	0.34	0.34
	(0.01)	(0.01)	Obs	31,043	31,043
African American-Male	0.02	0.04			
	(0.02)	(0.02)			
Free-Reduced lunch	0.16*	0.17*			
	(0.01)	(0.01)			

Notes: School cluster robust standard errors in parenthesis; * indicates significance at the 5% level; year dummies, grade dummies and interaction of year and grade dummies also included. Bold text indicates the estimates that are presented in Table 4 in the body of the paper.

Table A2
Achievement analysis: controlling for pre-closure scores (OLS and IV) – math.

	OLS	IV		OLS	IV
Pre-closure Scores_Grade3_y1	0.81*	0.79*	English Language Learner	0.04	0.04
	(0.14)	(0.14)		(0.06)	(0.06)
Pre-closure Scores _Grade4_y1	0.73*	0.73*	Gifted	0.31*	0.31*
	(0.03)	(0.03)		(0.03)	(0.03)
Pre-closure Scores_Grade5_y1	0.71*	0.71*	Special Needs student	−0.16*	−0.16*
	(0.02)	(0.02)		(0.02)	(0.02)
Pre-closure Scores _Grade6_y1	0.70*	0.70*	Voluntary Mover	−0.04	−0.02
	(0.02)	(0.02)		(0.02)	(0.03)
Pre-closure Scores _Grade7_y1	0.74*	0.74*	**Student from Closed School-y1**	−0.13*	**−0.19***
	(0.01)	(0.02)		(0.05)	**(0.05)**
Pre-closure Scores _Grade8_y1	0.81*	0.81*	**Student from Closed School-y2**	−0.07	**−0.17**
	(0.01)	(0.01)		(0.06)	**(0.10)**
Pre-closure Scores _Grade4_y2	0.84*	0.83*	**Student from Closed School-y3**	−0.04	**−0.14**
	(0.16)	(0.18)		(0.04)	**(0.08)**
Pre-closure Scores_Grade5_y2	0.69*	0.67*	**Dif in SPI _y1**	0.08*	**0.09***
	(0.02)	(0.03)		(0.02)	**(0.02)**
Pre-closure Scores _Grade6_y2	0.69*	0.68*	**Dif in SPI _y2**	0.05*	**0.07***
	(0.02)	(0.02)		(0.02)	**(0.02)**
Pre-closure Scores _Grade7_y2	0.67*	0.66*	**Dif in SPI _y3**	0.01	**0.06***
	(0.02)	(0.02)		(0.02)	**(0.03)**
Pre-closure Scores_Grade8_y2	0.75*	0.75*	**Prop from Closed_y1**	0.02	**−0.18**
	(0.02)	(0.03)		(0.17)	**(0.25)**
Pre-closure Scores_Grade5_y3	0.64*	0.69*	**Prop from Closed_y2**	0.01	**−0.44**
	(0.10)	(0.13)		(0.18)	**(0.40)**
Pre-closure Scores_Grade6_y3	0.65*	0.66*	**Prop from Closed_y3**	0.08	**−0.17**
	(0.02)	(0.02)		(0.13)	**(0.31)**
Pre-closure Scores_Grade7_y3	0.65*	0.65*	Prop from other_open_y1	−0.007	0.28
	(0.02)	(0.02)		(0.29)	(0.35)
Pre-closure Scores_Grade8_y3	0.67*	0.68*	Prop new to school_y2	−0.21	0.11
	(0.02)	(0.02)		(0.25)	(0.81)
African American-Female	−0.12*	−0.11*	Prop new to school_y3	−0.45	0.06
	(0.03)	(0.03)		(0.26)	(0.56)
Other-Male	−0.02	−0.02	Reconstituted School_y1	0.001	−0.03
	(0.02)	(0.02)		(0.05)	(0.07)
African American-Male	−0.14*	−0.13*	Reconstituted School_y2	−0.07	−0.05
	(0.03)	(0.03)		(0.06)	(0.09)
Free-Reduced lunch	−0.04*	−0.04*	Reconstituted School_y3	−0.07	0.07
	(0.01)	(0.01)		(0.07)	(0.13)
k5_y1	0.08	0.15*	ms_2006	−0.01	−0.09
	(0.05)	(0.07)		(0.06)	(0.11)
k5_y2	0.10	0.38*	ms_2007	−0.03	−0.08
	(0.08)	(0.14)		(0.07)	(0.26)
k5_y3	−0.05	−0.53	ms_2008	0.04	−0.10
	(0.17)	(1.26)		(0.04)	(0.15)
Constant	0.79*	0.51	Adj. R-squared	0.70	0.70
	(0.33)	(0.79)	Obs	19,725	19,725

Notes: School cluster robust standard errors in parenthesis; * indicates significance at the 5% level; year dummies, grade dummies and interaction of year and grade dummies also included. Bold text indicates the estimates that are presented in Table 5 in the body of the paper.

Table A3
Achievement analysis: controlling for pre-closure scores (OLS and IV) – reading.

	OLS	IV		OLS	IV
Pre-closure Scores_Grade3_y1	0.43*	0.41*	English Language Learner	0.03	0.03
	(0.14)	(0.14)		(0.03)	(0.03)
Pre-closure Scores _Grade4_y1	0.67*	0.66*	Gifted	0.40*	0.40*
	(0.02)	(0.02)		(0.02)	(0.02)
Pre-closure Scores_Grade5_y1	0.63*	0.62*	Special Needs student	−0.17*	−0.17*
	(0.02)	(0.02)		(0.01)	(0.02)
Pre-closure Scores _Grade6_y1	0.67*	0.66*	Voluntary Mover	−0.03	−0.02
	(0.02)	(0.02)		(0.02)	(0.03)
Pre-closure Scores _Grade7_y1	0.64*	0.64*	**Student from Closed School-y1**	−0.14*	**−0.20***
	(0.02)	(0.02)		(0.03)	**(0.05)**
Pre-closure Scores _Grade8_y1	0.69*	0.69*	**Student from Closed School-y2**	−0.04	**−0.07**
	(0.02)	(0.02)		(0.04)	**(0.05)**
Pre-closure Scores _Grade4_y2	0.52*	0.52*	**Student from Closed School-y3**	−0.03	**−0.03**
	(0.17)	(0.17)		(0.04)	**(0.05)**
Pre-closure Scores_Grade5_y2	0.64*	0.64*	**Dif in SPI _y1**	0.07*	**0.07***
	(0.02)	(0.02)		(0.02)	**(0.02)**
Pre-closure Scores _Grade6_y2	0.67*	0.67*	**Dif in SPI _y2**	0.05*	**0.06***
	(0.01)	(0.02)		(0.01)	**(0.02)**
Pre-closure Scores _Grade7_y2	0.64*	0.64*	**Dif in SPI _y3**	0.02	**0.04**

Table A3 (*continued*)

	OLS	IV		OLS	IV
	(0.02)	(0.02)		(0.02)	**(0.03)**
Pre-closure Scores_Grade8_y2	0.64*	0.64*	**Prop from Closed_y1**	−0.10	**−0.42**
	(0.02)	(0.02)		(0.10)	**(0.22)**
Pre-closure Scores_Grade5_y3	0.42*	0.38	**Prop from Closed_y2**	0.14	**0.03**
	(0.17)	(0.22)		(0.13)	**(0.26)**
Pre-closure Scores_Grade6_y3	0.64*	0.64*	**Prop from Closed_y3**	0.14	**0.19**
	(0.02)	(0.02)		(0.10)	(0.19)
Pre-closure Scores_Grade7_y3	0.61*	0.62*	Prop from other_open_y1	−0.47*	−0.16
	(0.02)	(0.01)		(0.21)	(0.31)
Pre-closure Scores_Grade8_y3	0.68*	0.69*	Prop new to school_y2	−0.80*	−0.75
	(0.03)	(0.03)		(0.28)	(0.47)
African American-female	−0.15*	−0.14*	Prop new to school_y3	−0.92*	−0.89*
	(0.02)	(0.02)		(0.26)	(0.45)
Other-Male	−0.12*	−0.12*	Reconstituted School_y1	−0.02	−0.03
	(0.02)	(0.01)		(0.04)	(0.05)
African American-Male	−0.23*	−0.23*	Reconstituted School_y2	0.01	0.06
	(0.02)	(0.02)		(0.05)	(0.06)
Free-Reduced lunch	−0.08*	−0.08*	Reconstituted School_y3	−0.01	−0.003
	(0.01)	(0.01)		(0.05)	(0.08)
k5_y1	0.07*	0.14*	ms_2006	0.07	−0.003
	(0.04)	(0.06)		(0.05)	(0.09)
k5_y2	0.10	0.18	ms_2007	0.20*	0.21
	(0.06)	(0.11)		(0.08)	(0.15)
k5_y3	−0.14	0.43	ms_2008	0.17*	0.17
	(0.21)	(0.92)		(0.05)	(0.12)
Constant	0.20	0.32	Adj. *R*-squared	0.68	0.68
	(0.18)	(0.67)	Obs	19,613	19,613

Notes: School cluster robust standard errors in parenthesis; * indicates significance at the 5% level; year dummies, grade dummies and interaction of year and grade dummies also included. Bold text indicates the estimates that are presented in Table 5 in the body of the paper.

Table A4
Summary of first stage results for IV estimates of absences.

External instruments	Endogenous variable											
	Prop from Closed_yl	Prop from Closed_y2	Prop from Closed_y3	Reconstituted School_yl	Reconstituted School_y2	Reconstituted School_y3	k5_yl	k5_y2	k5_y6	ms_yl	ms_y2	ms_y3
Assigned Prop from Closed_yl	**0.56***	−0.01*	−0.01*	0.04*	−0.04*	−0.03*	0.02	0.02*	0.01*	0.24*	0.04*	0.04*
	(0.01)	(0.001)	(0.001)	(0.02)	(0.004)	(0.003)	(0.02)	(0.004)	(0.003)	(0.03)	(0.003)	(0.003)
Assigned Prop from Closed_y2	−0.02*	**0.59***	−0.01*	−0.04*	0.12*	−0.03*	0.02*	0.07*	0.01*	0.03*	−0.20*	0.04*
	(0.002)	**(0.01)**	(0.001)	(0.004)	(0.02)	(0.003)	(0.004)	(0.03)	(0.003)	(0.004)	(0.03)	(0.003)
Assigned Prop from Closed_y3	−0.02*	−0.02*	**0.69***	−0.05*	−0.04*	0.15*	0.02*	0.02*	0.05	0.04*	0.04*	−0.41*
	(0.002)	(0.001)	**(0.02)**	(0.005)	(0.004)	(0.03)	(0.004)	(0.004)	(0.03)	(0.004)	(0.003)	(0.04)
Assigned Reconstituted School_yl	0.005*	−0.001*	−0.0009*	**0.52***	-0.003*	−0.002*	0.04*	0.002*	0.0009*	0.05*	0.003*	0.003*
	(0.002)	(0.0001)	(0.0001)	**(0.008)**	(0.0004)	(0.0003)	(0.005)	(0.0003)	(0.0003)	(0.005)	(0.0003)	(0.0003)
Assigned Reconstituted School_y2	−0.001*	−0.006*	−0.0008*	−0.002*	**0.49***	−0.002*	0.001*	0.06*	0.0007*	0.002*	0.02*	0.002*
	(0.0002)	(0.002)	(0.0001)	(0.0004)	**(0.009)**	(0.0003)	(0.0003)	(0.006)	(0.0003)	(0.0005)	(0.005)	(0.0003)
Assigned Reconstituted School_y3	−0.001*	−0.0007*	−0.012*	−0.002*	−0.002*	**0.47***	0.0008*	0.007*	0.06*	0.001*	0.002*	0.05*
	(0.0002)	(0.0002)	(0.003)	(0.0004)	(0.0003)	**(0.01)**	(0.0003)	(0.0003)	(0.006)	(0.0006)	(0.0004)	(0.007)
Assigned k5_yl	0.004*	−0.0002	−0.0002	−0.01*	0.0004	0.0002	**0.58***	0.0002	0.0003	0.05*	0.0002	0.00009
	(0.002)	(0.0001)	(0.0001)	(0.008)	(0.0003)	(0.0002)	**(0.009)**	(0.0003)	(0.0002)	(0.003)	(0.0004)	(0.0003)
Assigned k5_y2	−0.0003	−0.008*	−0.00009	0.005	−0.013	0.0004	−0.0001	**0.57***	0.0003	0.0007	0.02*	−0.0003
	(0.0002)	(0.002)	(0.0001)	(0.0004)	(0.009)	(0.0003)	(0.0003)	**(0.011)**	(0.0003)	(0.0006)	(0.004)	(0.0004)
Assigned k5_y3	0.0005	−0.0002	−0.012*	0.001	0.0006	−0.015	0.0001	0.0005	**0.54***	0.001	0.0003	0.026*
	(0.0003)	(0.0002)	(0.003)	(0.0005)	(0.0005)	(0.01)	(0.0003)	(0.0004)	**(0.01)**	(0.0008)	(0.0005)	(0.005)
Assigned ms_yl	−0.007*	0.001*	0.001*	0.03*	0.002*	0.002*	0.08*	−0.003*	−0.002*	**0.39***	−0.004*	−0.003*
	(0.003)	(0.0002)	(0.0001)	(0.008)	(0.0005)	(0.0004)	(0.004)	(0.0004)	(0.0003)	**(0.01)**	(0.0005)	(0.0004)
Assigned ms_y2	0.003*	−0.015*	0.001*	0.005*	0.06*	0.003*	−0.003*	0.07*	−0.003*	−0.007*	**0.21***	−0.004*
	(0.0003)	(0.004)	(0.0002)	(0.0006)	(0.009)	(0.0005)	(0.0005)	(0.004)	(0.0004)	(0.0007)	**(0.009)**	(0.0005)
Assigned ms_y3	0.003*	0.002*	−0.01*	0.005*	0.003*	0.05*	−0.003*	−0.004*	0.06*	−0.008*	−0.005*	**0.36***
	(0.003)	(0.0002)	(0.004)	(0.0007)	(0.0006)	(0.009)	(0.0005)	(0.0006)	(0.004)	(0.0008)	(0.0006)	**(0.01)**

Notes: (i) N. Obs: 31,043; (ii) cluster-robust standard errors in parenthesis, * indicates significance at the 5% level; (iii) Also included internal instruments: grade, year, grade year interaction dummies, etnicity, free or reduced lunch, English language learner, special needs student, gifted, voluntary mover, student from closed school, proportion movers from other open schools.

Table A5
Summary of first stage results for IV estimates of math.

External Instruments	Endogenous variable														
	Dif in SPIyl	Dif inSPI_y2	Dif in SPI_y3	Prop from Closed yl	Prop from Closed y2	Prop from Closed y3	Reconstituted School yl	Reconstituted School y2	Reconstituted School y3	k5_yl	k5_y2	k5_y6	ms yl	ms y2	ms y3
Assigned Dif in SPIyl	**0.86*** **(0.01)**	0.0001 (0.0001)	0.0001 (0.0001)	0.002 (0.001)	−0.0003* (0.001)	−0.0002* (0.0001)	−0.05* (0.005)	−0.0003* (0.0002)	−0.0002 (0.0001)	0.02* (0.005)	0.0003* (0.0001)	0.00003 (0.00002)	0.003 (0.006)	0.001* (0.0002)	0.0004 (0.0002)
Assigned Dif in SPI_y2	0.0001 (0.0001)	**0.86*** **(0.02)**	0.0002 (0.0001)	0.0001 (0.0003)	−0.001 (0.001)	−0.0001* (0.0001)	−0.0005 (0.0003)	−0.04* (0.01)	0.0002 (0.0002)	−0.0004 (0.0003)	0.01* (0.01)	−0.00003 (0.00003)	−0.0004 (0.001)	−0.0005 (0.008)	−0.00005 (0.00003)
Assigned Dif in SPI_y3	0.00005 (0.0002)	−0.0001 (0.002)	**0.85*** **(0.02)**	0.0002 (0.0003)	−0.0000 (0.0001)	0.0003 (0.001)	0.001* (0.0004)	0.001* (0.0003)	−0.05* (0.01)	−0.001* (0.0003)	0.0005 (0.0003)	0.007* (0.003)	−0.0005 (0.001)	−0.0002 (0.0004)	0.03* (0.01)
Assigned Prop from Closedyl	0.02* (0.01)	0.003 (0.003)	0.004 (0.002)	**0.48*** **(0.01)**	−0.01* (0.002)	−0.004* (0.001)	0.05* (0.02)	−0.03* (0.004)	−0.02* (0.003)	0.03 (0.03)	0.01* (0.003)	0.001 (0.001)	0.37* (0.04)	0.02* (0.003)	0.04* (0.004)
Assigned Prop from Closed_y2	0.01* (0.003)	0.04* (0.01)	0.003 (0.002)	−0.01* (0.002)	**0.55*** **(0.02)**	−0.005* (0.001)	−0.03* (0.005)	0.15* (0.03)	−0.02* (0.003)	0.02* (0.004)	0.04 (0.02)	0.001 (0.001)	0.02* (0.005)	−0.17* (0.03)	0.04* (0.004)
Assigned Prop from Closed_y3	0.01* (0.003)	0.005 (0.003)	0.05* (0.02)	−0.01* (0.002)	−0.01* (0.002)	**0.68*** **(0.02)**	−0.03* (0.005)	−0.03* (0.004)	0.20* (0.04)	0.02* (0.004)	0.01* (0.003)	0.01 (0.005)	0.02* (0.01)	0.02* (0.003)	−0.52* (0.05)
Assigned Reconstituted School yl	0.02* (0.01)	0.0005 (0.0003)	0.0005* (0.0002)	0.01* (0.002)	−0.001* (0.0002)	−0.001* (0.0001)	**0.44*** **(0.01)**	−0.002* (0.0004)	−0.001* (0.003)	0.03* (0.006)	0.001* (0.0003)	0.0001 (0.00005)	0.08* (0.01)	0.002* (0.0004)	0.003* (0.0005)
Assigned Reconstituted School_y2	0.001* (0.0003)	0.01 (0.01)	0.0004* (0.0002)	−0.001* (0.0003)	−0.005 (0.003)	−0.0005* (0.0001)	−0.002* (0.0004)	**0.45*** **(0.01)**	−0.001* (0.0003)	0.001* (0.0004)	0.04* (0.006)	0.00003 (0.00004)	0.002* (0.001)	0.02* (0.01)	0.002* (0.0005)
Assigned Reconstituted School_y3	0.0003 (0.0002)	0.0002 (0.002)	−0.01 (0.01)	−0.0005 (0.0004)	−0.0004* (0.0002)	−0.01* (0.004)	−0.001 (0.0005)	−0.001* (0.0003)	**0.44*** **(0.01)**	0.0003 (0.0004)	0.0003 (0.0004)	0.006* (0.002)	0.001 (0.001)	0.001* (0.0005)	0.04* (0.01)
Assigned k5 yl	0.01 (0.01)	0.0005 (0.0003)	−0.00005 (0.002)	0.01* (0.003)	−0.0005* (0.0002)	−0.0004 (0.0002)	−0.02* (0.01)	−0.0001 (0.0004)	0.0002 (0.0003)	**0.54*** **(0.01)**	0.0009* (0.0004)	0.0001 (0.00005)	0.05* (0.01)	0.001 (0.0005)	−0.001 (0.0005)
Assigned k5_y2	−0.0003 (0.0003)	0.01* (0.01)	−0.0003 (0.003)	−0.001* (0.001)	−0.001 (0.004)	−0.0004 (0.0002)	−0.0002 (0.001)	0.01 (0.02)	0.001 (0.0004)	0.001 (0.001)	**0.50*** **(0.02)**	0.0001 (0.0001)	0.004* (0.001)	−0.02* (0.01)	−0.002* (0.001)
Assigned k5_y3	−0.0003 (0.0003)	0.005 (0.003)	0.17 (0.12)	−0.01* (0.003)	−0.01* (0.002)	−0.02 (0.02)	−0.01* (0.004)	−0.01 (0.003)	−0.03 (0.1)	0.01* (0.004)	0.01* (0.004)	**0.22** **(0.13)**	0.03* (0.01)	0.01* (0.004)	−0.22* (0.07)
Assigned msyl	−0.001 (0.01)	−0.0012* (0.0005)	−0.00001* (0.0003)	−0.004 (0.003)	0.001* (0.0002)	0.001* (0.0002)	0.04* (0.01)	0.002* (0.0005)	0.001* (0.0004)	0.06* (0.004)	−0.003* (0.0004)	−0.0002* (0.0001)	**0.35*** **(0.01)**	−0.003* (0.0005)	−0.002* (0.0005)
Assigned ms y2	−0.0002 (0.0003)	0.02* (0.01)	0.0002 (0.0004)	0.003* (0.0004)	−0.01 (0.004)	0.001* (0.0002)	0.004* (0.001)	0.10* (0.01)	0.002* (0.0005)	−0.004* (0.001)	0.05* (0.003)	−0.0003* (0.0001)	−0.01* (0.001)	**0.17*** **(0.01)**	−0.002* (0.0006)
Assigned ms y3	−0.0002* (0.0003)	−0.001 (0.0005)	0.03* (0.01)	0.002* (0.0004)	0.001* (0.0002)	−0.01* (0.004)	0.003* (0.0006)	0.002* (0.0005)	0.07* (0.01)	−0.003* (0.0005)	−0.002* (0.0005)	0.005* (0.001)	−0.01* (0.001)	−0.003* (0.0006)	**0.29*** **(0.01)**

Notes: (i) N. obs: 19,725; (ii) cluster-robust standard errors in parenthesis, * indicates significance at the 5% level; (iii) also included internal instruments: grade, year, grade year interaction dummies, etnicity, free or reduced lunch, English language learner, special needs student, gifted, voluntary mover, student from closed school, proportion movers from other open schools.

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