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The Impact of Chicago's Small High School Initiative

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Abstract

This project examines the effects of the introduction of new small high schools on student performance in the Chicago Public School (CPS) district. Specifically, the researchers investigate whether students attending small high schools have better graduation/enrollment rates and achievement than similar students who attend regular CPS high schools. They show that students who choose to attend a small school are more disadvantaged on average. To address the selection problem, they use an instrumental variables strategy and compare students who live in the same neighborhoods but differ in their residential proximity to a small school. In this approach, one student is more likely to sign up for a small school than another statistically identical student because the small school is located closer to the student's house and therefore the "cost" of attending the school is lower. They find that small schools students are substantially more likely to persist in school and eventually graduate. Nonetheless, there is no positive impact on student achievement as measured by test scores. The finding of no test score improvement but a strong improvement in school attainment is consistent with a growing literature suggesting that interventions aimed at older children are more effective at improving their non-cognitive skills than their cognitive skills.

I. Introduction

There is a building consensus among policy makers, educators, parents and future employers that American high schools are in need of significant reform. Nationwide, only about 75 percent of high school freshmen graduate from high school within 4 years (Snyder and Dillow, 2011). Students from poor families and students of color are more likely to drop out than more advantaged youth. Improvements that have recently been seen in lower grades (possibly because of the introduction of accountability reforms like *No Child Left Behind*) have failed to carry over to high school performance. According to the National Assessment of Educational Progress (NAEP), 74 percent of 12th graders have math skills below the proficiency level, and 90-95 percent of African-American and Hispanic students fail to meet the bar.[1] Further, over 60 percent of employers complain that high school graduates do not have good math and writing skills (U.S. Department of Education, 2003).

The organization of schools has a potentially large impact on the performance of students (Barker and Gump, 1964; Chubb and Moe, 1990). In the recent past, high schools have been accused of being rather large, impersonal educational "factories" where teachers know little about the students in their charge, and the learning environment is not very supportive (Sizer, 1984; Sizer 1997). In response, reform efforts known as the "Small Schools Movement" have been mounted to reduce the size of high school learning communities by breaking up existing large schools and creating new schools that are small by design. The Bill & Melinda Gates Foundation was a major supporter of this reform, making over $2 billion in grants to invest in small schools (Gates Foundation, 2009). The Annenberg Foundation, Carnegie Foundation, and Department of Education also contributed substantial resources to small schools (Shear and

[1] Cited statistics are 2009 NAEP test score results for 12th grade students reported in the Digest of Education Statistics, 2012.

Smerdon, 2003).

Despite the substantial financial investment in small school reforms, there have been few experimental or quasi-experimental evaluations of their impacts on student outcomes. This project attempts to isolate the causal impact of the 22 new small high schools created in Chicago between 2002 and 2006 under the Chicago High School Redesign Initiative (CHSRI). We use individual-level longitudinal data from the Chicago Public Schools (CPS) and employ an instrumental variables design based on a student's residential proximity to a small high school to measure their impacts on enrollment and graduation up to 5 years after a student began high school.

We document substantial negative selection into small high schools in Chicago. When we control for background characteristics, the correlation between small school attendance and enrollment indicates that small school students are somewhat less likely to drop out and more likely to progress on time and graduate. The instrumental variables results are substantially larger than the OLS results and suggest that small schools increase the likelihood that a student graduates from high school on time by 9 percentage points on a base of 48 percent. We find no impacts on test scores. These findings are consistent with the broader literature that finds that better high schools – such as Catholic high schools (Evans and Schwab 1995; Altonji et al. 2005) or small schools in New York City (Bloom and Unterman 2012; Schwartz et al. 2013) – improve educational attainment but not test scores.

II. Background on the Small Schools Movement

The small schools movement grew out of the observation that that poor, urban students who already have lower levels of academic performance are more likely to drop out of large high schools (Toch, 1991; Bryk and Thum, 1989; Maeroff, 1992). There are several theories about why small schools can be more effective, largely involving improved relationships between teachers and students in small schools (Rossi and Montgomery, 2004). In smaller schools, teachers may be able to get to know their students better and tailor their teaching approaches to students' interests and strengths; students may feel more connected to a small school community which leads to reduction in violence and dropping out; and expectations may be raised for the high achievement of all students. In addition, teachers are thought to be more collaborative, creative and effective in small schools.

Policies to expand the availability of small schools in urban environments were motivated by mostly correlational research from an earlier generation of small school interventions that showed positive outcomes (Cotton, 1996; Haller, 1993; Howley, 1989). Small schools had been shown to have lower dropout rates, smaller achievement gaps, and better access to challenging coursework (Bryk et al. 1990; Darling-Hammond et al., 2002; Holland, 2002; Pittman and Haughwout, 1987). However, the research was not universally positive; one-half of the studies reviewed in Cotton (1996) showed no impact of small schools.

Fueled by this theory and empirical evidence, over 1600 new, mostly urban small schools were founded in the early 2000's (Toch, 2010). While the guideline for enrollment was no more than 600 – and ideally closer to 400 students – it is important to note that the intervention of the small schools movement was intended to be about more than just the number in the student body. The small schools were expected to have an additional set of attributes including common focus, high expectations, a culture of respect and responsibility, performance standards, and effective use of technology.

Despite much previous research on small schools, our knowledge of the potential impact of policies encouraging the formation of new small high schools in urban districts is limited. The most credible evidence comes from two recent studies of new small high schools in New York City. Bloom and Unterman (2012) use lotteries for admission to over-subscribed small high schools in order to compare outcomes for lottery winners who go on to attend one of the new small high schools to lottery losers who attend one of the other types of public high schools available in New York City. Because lottery winners were randomly chosen, on average the two groups should have identical observable and unobservable characteristics. The authors find that in 9th grade the lottery winners were 10 percentage points more likely to be on track to graduate, and that they were 8.6 percentage points more likely to graduate four years after entering 9th grade. They also find that lottery winners are more likely to score at or above 75 on the English Regents exam, the level at which the City University of New York exempts students from taking remedial English classes.

In work most closely related to our paper, Schwartz, Stiefel, and Wiswall (2013) also study the effect of new small high schools on student outcomes in New York City using distance from student zip codes to the nearest schools by size and age as instrumental variables for attending a new small school, a new large school, an old small school, or an old large school. They find that students who attend one of the new small high schools are 17.5 percentage points more likely to graduate from high school than students who attend a large high school. Further, new small high school students are more likely to attempt a Regents math or English test by around 16 percentage points. In contrast to the findings of Bloom and Unterman (2012), however, Schwarz et al. (2013) find that new small high school students perform less well on the English Regents' exam than their large high school counterparts although they are also more likely to have taken the exam.

Early studies the introduction of small schools in Chicago found positive impacts on measures of student engagement, but no impact on gross measures of achievement (Kahne et al., 2005; Wasley et al. 2000; Hess and Cytrynbaum, 2002). The lack of findings on achievement may be due to evaluating the schools "too early" after their opening while schools were still struggling with basic start-up organizational challenges or because selection into the new schools was not properly accounted for. Additionally, the first small high schools to open in Chicago differ from later-opening small schools in potentially important ways. Namely, the first schools were so-called "conversion" schools that divided a large high school into a number of small schools in the same building. Later-opening schools were more typically new-start schools. Using longer run data, Sporte and de la Torre (2010) find that small school students in Chicago have better attendance and persistence than a demographically similar control group, but perform no better on test scores. They find similar impacts for both conversion and new-start schools. Our paper is the first to use a quasi-experimental design to evaluate the performance of small schools in Chicago.

III. Data

The data used in this project come from the Consortium on Chicago School Research's longitudinal dataset on student enrollment patterns and test scores. These data have been a fruitful source for many recent research projects on a variety of topics (e.g., Roderick et al., 2002; Cullen et al., 2005; Jacob, 2005; Jacob and Levitt, 2003; Neal and Schanzenbach 2009). These data allow us to address some of the problems that have plagued earlier studies of high school reform. Because of the availability of prior test scores and other demographic characteristics, we can account for selection on observables into new high schools. We include controls for a student's age, race, gender, neighborhood characteristics, whether she is old for her cohort (a proxy for grade retention), and whether the student is eligible for free or reduced price lunch or participates in a special education program. We have pre-test scores from the 8th grade math and reading components of the state standardized test, the Illinois Standards Achievement Test (ISAT). Because the Consortium has access to student address data they were able to construct for us our instrumental variable, the distance from the student's home to the closest small school.

The Chicago Public School District (CPS) is the third-largest district in the United States, with large numbers from several racial/ethnic groups. CPS overall is 40 percent African-American, with another 45 percent Hispanic, 3 percent Asian and the remainder Caucasian. Most students in the district are disadvantaged – 85 percent are from low-income families who qualify for free or reduced-price lunch – and dropout rates are high (35-43 percent in recent cohorts).[2] Chicago's introduction of new small high schools occurred against a backdrop of considerable existing school choice (over half of the 100,000 Chicago high school students attend a high school outside of their attendance area), several charter high schools, and improving test scores as a result of its 1997 NCLB-style accountability reforms (Jacob 2005).

Our primary outcome measures use fall administrative enrollment records to construct indicators of whether a student is still enrolled, is progressing from grade to grade on time, and whether they graduated from high school. We use five cohorts of students who enter 9th grade between fall 2002 and fall 2006 at one of 22 new small high schools. We have data to follow all students through 5 years after entering high school, long enough to capture most high school completion information even for students who are delayed. We also have standardized test scores from ACT's Educational Planning and Assessment System (EPAS) given to students in the fall

[2] These are five-year cohort dropout rates reported by CPS (2012).

of 9th and 10th grades, and spring of 11th grades. The 11th grade test includes a full-length ACT test that can be sent to colleges for admissions purposes.

The primary challenge of evaluating the effectiveness of new small schools is to isolate causality – that is, how would a student have scored this year if she had attended a "regular" school, and how does that compare to how she scored at the small school she actually attended? In order to begin to describe the difficulties of isolating causality, we first document the extent of the selection problem by presenting 8th grade characteristics of students who do and do not choose to attend a small school in 9th grade.[3] These are presented in Table 1. The first column shows mean characteristics of students who enroll in a small school. Because the schools are located in particular neighborhoods, we do not compare these students to the overall CPS population. Instead, we form the comparison group for 9th grade small school students using their former 8th grade classmates. Small school students were drawn from about 400 different 8th grade "sending" schools (out of almost 500 8th grade schools in the CPS system). Mean characteristics of the 8th grade classmates of small school students are in column (2). Because sending schools have varying rates of treatment (that is, one school might only send one or two students to a small high school, while another might send half of their enrollment or more to a small school), we test whether these characteristics are different conditional on sending school fixed effects. In other words, we examine how students who go to small schools compare to their own 8th grade classmates. P-values associated with tests for differences in means between columns (1) and (2) after conditioning on sending school fixed effects in an OLS regression are shown in column (3). Most characteristics are measured as binary variables, with a value of one indicating that the student has the characteristic described (e.g. female, receive free or reduced price lunch).

[3] Our sample is limited to students who are in 8th grade in the spring of year t-1 and in 9th grade in the fall of year t. We omit approximately 5 percent of the control group who enrolled in a selective high school in 9th grade; this has no significant impact on the results.

About 80 percent of the small school students are Black or African-American, 20 percent are Hispanic, and nearly 90 percent are eligible for free or reduced price lunch. Roughly one-third of the small school students are old for their grade, and almost one-quarter has some type of disability. Small school students live 1.2 miles away from the closest small school (whether or not they attend that particular small school). While their 8th grade classmates are equally likely to be low-income as measured by school-lunch eligibility, they are less likely to be African American, somewhat more likely to be Hispanic, more likely to be old for their grade, and less likely to have any disability. Small school students are more likely to have unstable enrollment in 8th grade, which is measured as whether a student ends the school year attending a different school than he or she began the year.

We also observe ISAT test scores from when the students were enrolled in grade 8. The ISAT was re-normed in 2005 (when our final cohort was in 8th grade), so we standardize math and reading scores by the mean and standard deviation across all CPS test takers in the same grade level and year in order to produce comparable statistics over time. The average 8th grade math score among small schools enrollees is -0.45, or 0.45 standard deviations below the district average, and the average reading score is -0.34. While the 8th grade classmates of small high school students also score below the district average on the 8th grade ISAT tests, their average test scores are significantly higher than the small school enrollees by roughly 0.2 standard deviations.

Finally, we also include mean characteristics for the Census block groups in which the students reside based on data from the 2000 Census. Specifically we look at poverty concentration, socioeconomic status (SES), and the average number of years household heads have lived in their residence.[4] Students enrolling in small high schools have very similar neighborhood characteristics to their 8th grade classmates.

Overall, we conclude that small school students are negatively selected in terms of expected educational outcomes compared to their prior classmates: they are more likely to be disabled and be old for their grade (a proxy for whether they have been held back in a prior year), more likely to have changed schools during the 8th grade school year, and their test scores are markedly worse in both math and reading and in 8th grade. Based on these differences we would expect small school students to have worse high school outcomes than their peers, all else equal.

The raw outcome means are presented at the bottom of Table 1. About 10 percent of students drop out or leave the Chicago Public Schools after each year of high school. That is, in the control group 10.8 percent of students are no longer enrolled in CPS in the fall of what would be their 10th grade year if they had progressed on time, denoted here as t+1 for one year after starting 9th grade. Twenty percent are no longer enrolled in the fall 2 years after starting 9th grade (i.e. what would be their 11th grade year), and thirty percent are no longer enrolled in the third fall after starting high school. Forty percent have dropped out or left CPS as of the fall 4 years after starting high school. A related measure of high school attainment is whether a student is still enrolled and is accumulating course credits progressing up the grade levels on time. Approximately three-quarters of the 9th graders in our sample are enrolled as 10th graders in CPS the subsequent year, and just under half of them graduate from high school on time. Note that

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[4] All three measures are constructed by CCSR. Poverty concentration is constructed using percent of adult males employed and percent of families with incomes above the poverty line. The measure is standardized such that the mean value for all census block groups in Chicago equals zero and one-half of the Census blocks will have above average poverty concentration (a positive value) and one-half will have below average poverty concentration. The SES measure is constructed using data on mean level of adult education and the percentage of employed persons who work as managers or professionals. The measure is similarly standardized so that mean Census block in Chicago equals zero, high SES block groups have positive values, and low SES block groups have negative values.

despite the fact that small school 9th graders are negatively selected along observable characteristics, their average high school outcomes are the same as their prior classmates.

Cohort-by-cohort summary statistics are presented in Appendix Table 1. Over time, the cohorts attending small schools become slightly less negatively selected on test scores: each year the pooled mean test scores among the small schools treatment group improve by approximately 0.04 standard deviation in math (from -0.54 for 2002 9th graders to -0.39 for 2006 9th graders) and 0.025 standard deviation in reading (from -0.38 to -0.32). In the empirical work that follows, we always condition on cohort fixed effects.

IV. Empirical Approach

As shown above, small school students differ from their prior classmates along observable characteristics. One approach to measure the relationship between small school attendance and student outcomes would be to condition on these observable characteristics such as special education status, race and gender. We model this approach as follows:

(1) $$Y_{itys} = \alpha_0 + X_i \beta + \alpha_1 SM_{i9} + \gamma_y + \varepsilon_{itys}$$

where Y is an outcome measure, such as standardized test score or dropout status, for student i at time t in cohort y in school s. X is a vector of student characteristics such as race, gender and free-lunch status, SM is an indicator variable for whether a student is enrolled in a small school in grade 9, γ is a cohort fixed effect (that is, a dummy variable for the year in which the cohort enters 9th grade), and ε is an individual error term that includes a component that allows for correlations across students in the same school. In some specifications, we augment the equation to include fixed effects η for 8th grade school units, or fixed effects φ for a student's home ZIP code, or both. This approach adjusts for selection into small schools as reflected by demographic characteristics.

However, equation (1) ignores potentially important unobserved characteristics that may be correlated with both the outcome and the decision to enroll in a small school. Failure to control for these characteristics would bias the measured impact of small schools. A potential method to address this problem is to control for additional observable characteristics a baseline test score *T*, that is:

(2) $$Y_{itys} = \alpha_0 + X_i\beta + \alpha_1 SM_{i9} + T_i\delta + \gamma_y + \varepsilon_{itys}.$$

This strategy works under the (likely untenable) assumption that the baseline test score adequately captures all of the other unobserved characteristics that affect both the student outcome and whether a student enrolls in a small school. In effect, equation (2) compares two children who have the same prior test score and share the same demographic characteristics, but one is enrolled in a small school and the other is enrolled in a regular school. A positive coefficient on α_1 (for an outcome such as a test score) would indicate that the test score gain (or value-added) is larger for a student who attends a small school.

While the approach described in equation (2) is an improvement over the approach in equation (1), there are still potentially serious shortcomings. For example, there is considerable year-to-year fluctuation in test score performance. If due to chance a student has an unusually bad test performance in 8th grade, her parents may react to this low score by enrolling her in a new school. The next year, we would expect her score to rebound to its previous higher level no matter whether she enrolls in a small or a regular high school. But failure to account for her previous test score trend will result in this “rebound” effect being attributed to the new school (Ashenfelter, 1978). If on the other hand an 8th grader has an unusually high score – again, just due to chance – his parents will likely judge that the current school regime is serving him well and may be less likely to enroll him in a different school. One can imagine situations in which this type of bias cuts in favor of small schools and other situations in which it cuts against them. In any case, the estimated effect will be biased.

Ideally, we would be able to evaluate the effectiveness of small schools by utilizing some sort of random assignment mechanism. Some recent studies of school reforms – including the Bloom and Unterman (2012) paper on small schools in New York – have used variation induced by randomized lotteries that are often used to allocate school admissions when there are more students who want to participate in a program than can be accommodated. In a classic lottery-style setup, students would be randomly assigned by a lottery to attend the new school or not from a school's application pool, and then the students who were assigned to attend the new school would be compared to those who lost the lottery. The students who signed up for the lottery likely share some similar characteristics – they may have highly motivated parents who are looking for the best available educational opportunity, or they may be students who feel they were not served well by the old school, or they may be students who faced academic or disciplinary problems at their prior school. The key feature for evaluation is that once the students identified themselves as being interested in changing schools, no characteristics predict whether they were selected from the list of applicants to attend the new school. As a result, the lottery "winners" and "losers" share the same distribution of prior achievement, family characteristics, etc. Since the groups are on average the same at the beginning of the year, any average difference at the end of the year would be due to the impact of the new school. Unfortunately, in this case there are no such lotteries available to use to help isolate the treatment effect of attending a small school.

In the absence of a truly randomized experiment, we turn to an instrumental variables strategy to isolate the causal impact, similar to the approach in recent papers in the economics literature that use proximity to college (Card 1995; Kling 2001; Currie and Moretti 2003) or selective high schools and career academies (Cullen et al. 2005) as an instrument for attendance. In this approach, the distance between a student's home and the nearest small school is used as a proxy variable for the time cost of attending a small school. The maintained assumption is that residential location is given, and proximity to a small school is not correlated with other determinants of attending a small school. If living closer to a small school increases the likelihood of enrolling in a small school but does not directly impact (or proxy for) student outcomes, then distance to the nearest small school can be used as an instrument for small school enrollment.

In other words, there is some (partially unobserved) selection process into small schools. Conditional on observable characteristics, those who choose small schools could have the most highly motivated parents, or they could be the most likely to drop out of a regular high school, or something else. The instrument is based on the intuition that students who live 1.0 vs. 1.4 miles away from a small school have the same underlying propensity to have motivated parents, a high likelihood of dropping out, etc. The difference in proximity to a small school induces one student to have a lower daily costs (in time and money) in terms of commuting to this particular school instead of a regular high school. To be a credible instrument, distance from small school must be a strong predictor of small school attendance but must not belong in the outcome equation directly nor proxy for other unmeasured characteristics that are omitted from the outcome equation. Preliminary evidence on the validity of the instrument is presented in Table 2.

When we condition on relatively small geographic units such as ZIP code, 8^{th} grade neighborhood school, or both, the difference in proximity to a small school is relatively small with standard deviation ranging from 0.55 to 0.76 miles.[5] Nonetheless, proximity to the nearest small school is a strong predictor of small school attendance as shown in the row marked "First stage estimates." Conditional on background characteristics and 8th grade neighborhood school fixed effects (column 2), living one mile closer to a small school increases the probability that a student attends a small school by 5 percentage points, with an F statistic of 54. Results are similar if we condition on ZIP code fixed effects (column 3) or saturate the model with both types of fixed effects (column 4).[6] To further assess the validity of the instrument, we investigate whether distance from a new school is correlated with pre-existing characteristics such as a student's prior test scores that might proxy for other, unobservable characteristics. When we control for ZIP code fixed effects, the instrument does not predict 8th grade math scores, student gender, whether they had unstable enrollment in 8th grade, or disability status. It is, however, correlated with 8th grade reading scores, free lunch status and student race. The estimated coefficients are not large, and we control for these characteristics directly in all subsequent regressions.

Specifically, the first stage equation is:

(3) $$SM_{iyn} = \alpha_0 + X_i\beta_1 + N_n\beta_2 + \alpha_1 MinDist_i + \gamma_y + \delta_n + \varepsilon_{iyn}$$

where an individual i in cohort year y living in neighborhood n decides to enroll in a small school based on distance to the nearest small school, a vector *X* of other student-level characteristics including race, gender, disability status and prior achievement, a vector *N* of neighborhood characteristics measured at the Census block level such as SES and poverty concentration, cohort-specific dummy variables, neighborhood-specific dummy variables

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[5] The average (standard deviation) of students per cohort in a ZIP code is 292 (326), and in an 8th grade neighborhood school zone is 43 (53).
[6] Results are very similar if only geographic fixed effects are included and individual and neighborhood characteristics are omitted.

(measured as fixed effects for 8th grade neighborhood school, ZIP code, or both) and an error term. The instrumental variable is the minimum distance between a student's home address and the closest small school location. In the data, a student who attends a small school attends the unit that is closest to her home about three quarters of the time.

V. Results

To construct the analysis sample, we identify all students in each school year T (spanning fall 2002-fall 2006) who are enrolled in 9th grade in either the fall or spring semester at a small school and who were enrolled in 8th grade in a CPS school in the spring of the previous school year, T-1. We construct a control group consisting of the small school enrollees' 8th grade classmates who also went on to enroll in 9th grade in a non-selective enrollment, CPS high school in school year T.

We construct several outcome measures for students in school years T through T+5. If the student progresses at an expected rate they will be in grade 10 in year T+1, grade 11 in year T+2, grade 12 in year T+3, and will have graduated by year T+4. Our primary outcomes of interest are measures of persistence in school. We calculate these measures using the district's fall master enrollment file, which includes information on a student's school attended, grade level, if they are currently an active student. If the student is not currently active, a code is included indicating the reason that a student exited the system, for example, whether they graduated, dropped out, transferred to a private school or a school out of the area, and so on. Using these data, we construct an indicator for whether in the current year a student is enrolled, graduated, or has dropped out or otherwise left the Chicago Public School system. In theory, this allows us to separate those who drop out from those who otherwise exit the system for parochial or suburban schools. In practice, we are both concerned about the quality of the drop out reason variable in general (because schools may have an incentive to erroneously code a student as a transfer instead of a dropout), and that the quality of this variable may be systematically different in small schools. For example, small schools might systematically do a better job keeping records on the whereabouts of their exiting students because there are fewer of them and would be more likely to know whether a student enrolled in a non-CPS school. As a result, we aggregate leavers and dropouts in our main specifications.[7] We also construct indicator variables for whether a student is in the grade level that would be expected if they were progressing at a normal rate of one grade level per year.

In addition, we have access to test score outcomes. CPS requires all high schools to administer the EXPLORE and PLAN tests from ACT's Educational Planning and Assessment System (EPAS). These test score outcomes affect high schools' probation status in the CPS Performance, Remediation and Probation Policy. In addition, Illinois requires all students to take the Prairie State Achievement Examination (PSAE) in order to receive a regular high school diploma. One component of the PSAE is a full-length ACT that can be used for college admission. As a result, we generally observe EXPLORE math and reading scores from the fall of 9^{th} grade, PLAN math and reading test scores from the fall of 10^{th} grade, and ACT math, reading, English, and science test scores from the spring of 11^{th} grade.

Of course, test scores are not available for all students in part due to the fact that some students drop out of school before reaching the grade in which the exam is administered and in part because test scores are missing for some enrolled students. Not surprisingly we observe test scores for the largest share of students on the 9^{th} grade exam. Here we observe math scores for 87 percent of the sample of students for whom we also have baseline 8^{th} grade test scores. In

[7] Results separating dropouts and leavers are substantially similar, and are available upon request.

contrast, we only observe 10th grade test scores for 69 percent of the sample and ACT scores for roughly 42 percent of the sample. If attrition due to dropout, for example, differs between small high schools and all other CPS high schools then examining test score differences between these school types will likely produce biased results. In particular, if we think that students who are most likely to dropout also have the lowest test scores and that small high schools reduce the dropout rate, then small high schools are likely to have lower average ACT test scores.

One simple way to try to correct for the potentially differential selection across the two groups of students is to impute test scores for all students missing test scores. In order to do this, we assume that a student's percentile rankings on the 8th grade tests equal the percentile rankings they would have received on the later tests had they taken the tests. We then set the missing test scores equal to the scores associated with their percentile rankings. For the ACT science test we assume a student's ranking is equal to her 8th grade math percentile ranking, and for the ACT English test we assume a student's ranking is equal to her 8th grade reading percentile ranking.[8]

A. Descriptive Results

In the first columns of Table 3 we present OLS estimates of the relationship between small school enrollment in 9th grade and persistence and graduation as described in equation (2). Standard errors are clustered by cohort-by-8th grade school groupings. Each row represents a separate outcome variable. Column (1) presents control group means for the outcome variables, and columns (2) through (4) represent particular specifications in terms of included geographic

[8] Because the EXPLORE, PLAN, and ACT tests are based on scales of only 25 to 36 points, we average test scores within percentile ranks and interpolate scores across gaps in percentile rankings. For example, an ACT math score of 18 equals the 77th percentile in the CPS while a score of 19 is at the 82nd percentile. In order to assign scores to the intervening percentile ranks we set the 78th percentile equal to 18.2, the 79th percentile equal to 18.4, and so on.

dummy variables. All estimates include controls for individual demographic characteristics measured in 8th grade including indicators for female, black, Hispanic, eligibility for free or reduced price lunch, whether the student was over age-for-grade, had unstable school enrollment, was disabled or had a learning disability, residential neighborhood characteristics measured at the Census block level, and cohort dummy variables. Since small school students are observably more disadvantaged on many of these characteristics, their inclusion in the regression pushes the coefficients toward more positive estimates (i.e. less likely to drop out and more likely to progress or graduate on time). Each cell in columns (2) through (4) reports the estimate and standard error on the small school indicator from a separate regression. By the time we would expect students to be enrolled in 10th grade (year T+1), approximately 10 percent of students have dropped out of school or otherwise left CPS (see column 1). After conditioning on background characteristics and ZIP code fixed effects, students who attend small schools are 0.5 percentage points less likely to drop out or leave, but this relationship is not statistically different from zero. The coefficient estimates on dropout rates hover around zero in the first 3 years of high school, and emerge negative and statistically significant by the beginning of what would be a student's senior year if he or she progressed on time. Small school students are slightly more likely to be progressing on time in grade level in grades 10 through 12. They are 3 percentage points more likely to graduate from high school on time, and 2 percentage points more likely to graduate within 5 years. In column (3) we replace ZIP code fixed effect with a fixed effect for residential neighborhood measured as the student's assigned neighborhood school in 8th grade (whether or not the student attended this school). In column (4) we saturate the model with both ZIP code and neighborhood school fixed effects. The estimates are very similar across different specifications.

B. Instrumental variables approach

In order to isolate the causal impact of small school attendance on student outcomes we turn to using distance to the closest small school as an instrumental variable for small school attendance as described in equation (3). We present results using this approach in columns (5) through (7) of Table 3. As with the OLS results, the treatment effect is relatively stable across specifications that control for different geographic units.

The results show consistent, strong and positive results of attending a small high school that are uniformly larger than the corresponding OLS results. This suggests that small school students are negatively selected on unobservable characteristics just as they are negatively selected on observable characteristics. In the fall one year after starting 9th grade, small schools improve the likelihood that a student is still enrolled in CPS by 9 percentage points in the fully saturated model (column 7). Three years after enrolling in 9th grade they are 14 percentage points less likely to have dropped out or left CPS.

Small school attendance also increases the likelihood that a student is still enrolled and progressing through the grade levels on time. Small school students are 13 percentage points more likely to be on time in 10th grade, and 18 percentage points more likely to be on time in 11th grade. They are 9 percentage points more likely to have graduated on time, although the standard errors in this instrumental variables regression are large so the result is not statistically significant.

One lingering concern about the instrument is whether distance to school attended belongs in the equation directly. After all, if the cost of attending school is lower because a student lives closer to the school, it might directly impact their likelihood of dropping out regardless of whether the high school is small or large. To address this, we can additionally control for distance from a student's residence to his or her assigned high school. Distance to high school generally is a significant predictor of dropout in the expected direction, that is, living farther away from high school slightly increases the likelihood of dropping out. Nonetheless, directly controlling for distance does not alter the IV relationship between small school attendance and dropout or persistence outcomes.[9]

C. Heterogeneous Impacts across Students

In Table 4 we present OLS and instrumental variables estimates by subgroups for the fully saturated model with neighborhood school and ZIP code fixed effects (i.e. columns 4 and 7 from Table 3). In each case we present the control group mean in the first column, the OLS relationship between small school attendance and the outcome in the second column, and the IV coefficient and standard error estimates in the third columns. We also show that the first stage relationship between distance to school and small school attendance is strong for each subgroup.

Comparing the first two sets of columns, the impact of small schools on African American and Hispanic students are quite different. According to the IV results, the small school impact on African American students is strongest in years T+1 to T+3, but declines sharply thereafter. Note that among African American students the OLS results are consistently zero, suggesting that failing to account for unobservable determinants of small school enrollment paints a particularly misleading picture for this subgroup. Among Hispanics, the pattern is reversed with the estimated impact on the dropout rate and persistence approximately zero in the first two years, but a stronger impact in years T+3, T+4, and T+5. This finding is especially interesting because the year-to-year dropout rates appear quite similar between African

[9] Results available upon request.

American and Hispanic students. More research is needed to understand whether this finding reflects a school effect (which would be hard to disentangle, since most small schools are predominantly Hispanic or African American), or something else specific to the education production function for these groups.

Comparing across gender, the small school impacts are relatively similar for the first year after high school entry, but by year T+2 the impacts on boys become larger. Small school attendance reduces boys' dropout rate in T+3 by 19 percentage points compared to a (statistically insignificant) 8 percentage point reduction for girls. Small schools improve the likelihood of graduating on time by 15 percentage points for boys compared to a statistically insignificant impact of 1 percentage point for girls. Note that the base dropout rate for boys is substantially higher than it is for girls in each year as well. While all of the corresponding impact estimates for girls are positive, all are smaller than the estimates for boys, and they are generally not statistically different from zero.

Next we look at the impact by the level of the student's 8th grade test scores. We define a student (somewhat arbitrarily) as having "high" prior test scores if his math and reading z-scores were greater than 0.5, and as having "low" prior scores if both math and reading z-scores were less than -0.5 in 8th grade. Even among students with high 8th grade test scores, 30 percent of students fail to graduate from a CPS school. Although the standard errors are large, the point estimates suggest that small school attendance seems somewhat more important for improving outcomes among the higher performing students, especially on measures of staying on track to graduate and graduating on time. In particular, the point estimates suggest that small schools reduce dropout rates for both high and low-performing students and that the magnitudes are larger for high performing students that low-performing students. However, none of the estimates are statistically different from zero at conventional levels. Similarly, the estimated impacts of small school attendance on grade progression and graduation are all positive and generally larger (relative to the control group means) for high-performing students, but once again, very few are statistically significant. Finally, we see that the point estimates of small school impacts are generally largest in magnitude for students who were categorized as learning disabled in grade 8. Three years after 9th grade enrollment, small schools reduce dropout/leave rates for students with disabilities by 32 percentage points (from a base of 34 percent), and five years after high school enrollment small schools reduce their dropout/leave rates by 16 percentage points from a base of 50 percent (although this latter estimate is no longer statistically different from zero). This translates into increases in four- and five-year graduation rates of over 50 percent. In summary, we find that small school attendance improves outcomes for all types of students with larger impacts for boys and students with an identified disability.

D. Test scores

Test score outcomes are even more problematic than other outcomes because, at a minimum, they are only available for students who are still enrolled in school. Even among students who are still enrolled in CPS we only observe test scores for a sub-sample. The fact that we find impacts of small school attendance on dropout probabilities and the likelihood of progressing on time through the grades suggests that analysis of the small school impact on test score outcomes will yield biased results. With that in mind, however, we present OLS and instrumental variable estimates of the effect of small school attendance on test scores in 9th grade, 10th grade, and ACT test score outcomes. In order to have some sense of the effect of sample selection on test score estimates, we include one set of estimates based on observed test scores and a second set in which we impute missing EPAS and ACT test scores in 9^{th}, 10^{th}, and 11^{th} grade with a student's 8^{th} grade ISAT test scores. We present both OLS and IV estimates for each. The top panel of the table presents results for the math and science tests, while the bottom presents results for the reading and English tests. Note that these scores are measured in score points; the average score is approximately 14 and the standard deviation of scores ranges between 3 and 4.

Comparing the OLS estimates in columns (2) and (5) for 9^{th} and 10^{th} grade math we see, indeed, that the estimates from the imputed sample are larger than the estimates from the select sample, consistent with small schools reducing dropout/increasing persistence among lower performing students. However, we do not see a similar increase in estimated coefficients on the ACT math and science tests which is puzzling. Once we instrument for small school attendance using distance to the nearest small school, we find negative but not statistically significant impacts on math and science test scores for the imputed test score sample. In the select sample, we estimate a negative impact of small school attendance on ACT math scores that is significant at the 5 percent level. Overall, we find no evidence of a positive impact of small school attendance on student math and science test scores.

Results from the reading and English test score outcomes are more puzzling. Once again, the OLS estimates from the select and imputed samples for the 9^{th} and 10^{th} grade reading test scores are consistent with small high schools reducing dropout and increasing persistence among lower performing students but no similar observation from the ACT reading and English test scores. Our instrumental variables estimates indicate strong negative impacts of small school attendance on both 9^{th} grade reading test scores and the ACT reading and English test scores. The statistically significant, negative 9^{th} grade reading score estimates are particularly hard to understand because the tests are taken in the fall before we would expect the schools to have a large impact on the students. Further research is needed to fully understand these test score implications, but we conclude, once again, that there is no evidence of a positive impact of small school attendance on student test scores.

VI. Discussion and Conclusions

This paper has examined the effects of the introduction of small schools in the Chicago Public School district on student performance. As in any exercise in evaluating a policy intervention, the strength of the results rests on how well one can define the counter-factual – i.e., what would have happened to the small school students if they had not been granted access to these new schools? We show definitively that students who attend small high schools look different from even their own 8th grade classmates along several observable characteristics. They have a higher probability of having been retained in grade, a history of substantially lower test scores, and are more likely to have a disability. If these characteristics are not properly accounted for, the estimated "impact" of attending a small school will be biased.

We use an instrumental variables strategy to address the selection problem and compare students who attended the same schools for 8th grade and live in neighborhoods with similar characteristics. In this approach, one student was more likely to sign up for a small school than another similar student because the small school was located closer to the student's house and therefore the "cost" of attending the school as measured by commuting time is lower. Distance to the nearest small school has strong predictive power to identify who attends a small school. Using this strategy, we find that small school students are substantially more likely to persist in school and eventually graduate. Nonetheless, there is no consistent measurable impact on student achievement as measured by test scores. The finding of no test score improvement but a strong improvement along dimensions of persistence and attainment is similar to the literature on the impacts of Catholic high schools (e.g. Evans and Schwab 1995, Altonji et al. 2005) and to the findings of evaluations of the New York City small schools intervention (Bloom and Unterman 2012; Schwartz et al. 2013). Further, it is consistent with a growing literature that suggests that interventions aimed at older children can improve their non-cognitive skills but do little to improve their cognitive skills (Cunha et al. 2006).

Our empirical strategy provides the means to identify the causal impact of enrollment in a small school on student outcomes. An important remaining question, then, is what is the likely mechanism for the improvements? While limiting the enrollment of the student body was an important cornerstone of the small schools movement, it also encouraged differences in personnel and culture compared to a typical, large, urban high school. Unfortunately, while we can say that the impact of the introduction of small schools in Chicago has been positive – especially for students who were already relatively disadvantaged – we cannot at this point disentangle what exactly it is about these small schools that generated the improvements in student outcomes.

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Table 1: Mean characteristics of small high school students and their 8th grade schoolmates

Characteristic	Small school 9th graders	Former classmates	p-value of difference
	(1)	(2)	(3)
8th grade year demographics			
Female	0.503	0.505	0.653
Black	0.806	0.696	0.012
Hispanic	0.177	0.262	0.057
Free and reduced price lunch	0.885	0.885	0.687
Over age-for-grade	0.328	0.287	0.000
Unstable enrollment 8th grade	0.063	0.051	0.001
Disability: any	0.225	0.187	0.000
Diability: learning disabled	0.161	0.128	0.000
Minimum distance to a small high school	1.21	2.47	0.000
Prior test scores			
8th grade math z-score	-0.453	-0.238	0.000
8th grade reading z-score	-0.343	-0.180	0.000
2000 Census block group characteristics			
Poverty concentration	0.604	0.502	0.113
Socioeconomic status	-0.395	-0.392	0.198
Tenancy	11.8	11.7	0.961
High school outcomes			
Dropout/left year t+1	0.106	0.108	0.302
Dropout/left year t+2	0.214	0.205	0.094
Dropout/left year t+3	0.307	0.299	0.226
Dropout/left year t+4	0.410	0.411	0.807
Dropout/left year t+5	0.438	0.434	0.775
On time 10th grade	0.765	0.737	0.591
On time 11th grade	0.635	0.608	0.381
On time 12th grade	0.554	0.546	0.893
Graduated on time	0.491	0.481	0.449
Graduated within 5 years	0.529	0.527	0.731
N	7368	57568	

Notes: This table presents summary statistics for the analysis sample. Column (1) presents average characteristics among students who attended a small high school in 9th grade. Column (2) presents average characteristics of the 8th grade schoolmates of the students in column (1). Students who attended a selective enrollment high school are omitted from column (2). Column (3) presents the p-value of a test for equality across columns (1) and (2) after conditioning on 8th grade school fixed effects. 5th and 8th grade test scores are normalized by the district-wide mean and standard deviation in the year of the test. High school outcomes are measured in the fall.

Table 2: Relationship between Distance to Nearest Small School and Selected Variables

Characteristic	Control group mean	OLS relationship between instrument and dependent variable		
	(1)	(2)	(3)	(4)
Panel A: First stage regressions				
Attends small school		-0.046	-0.054	-0.045
		(0.006)	(0.007)	(0.006)
F statistic		54.1	63.7	51.9
Panel B: Correlation between distance and 8th grade characteristics				
8th grade math z-score	-0.238	0.009	0.015	0.009
	(0.831)	(0.008)	(0.008)	(0.008)
8th grade reading z-score	-0.18	0.021	0.025	0.021
	(0.900)	(0.008)	(0.007)	(0.008)
Female	0.505	-0.002	-0.001	-0.001
	(0.500)	(0.003)	(0.002)	(0.003)
Black	0.696	0.004	0.011	0.005
	(0.460)	(0.002)	(0.006)	(0.002)
Hispanic	0.262	-0.010	-0.018	-0.010
	(0.440)	(0.002)	(0.006)	(0.002)
Free or reduced price lunch	0.885	-0.005	-0.009	-0.005
	(0.319)	(0.002)	(0.002)	(0.002)
Over age-for-grade	0.287	-0.010	-0.012	-0.010
	(0.452)	(0.003)	(0.003)	(0.003)
Unstable enrollment 8th grade	0.051	-0.001	0.000	0.000
	(0.219)	(0.002)	(0.001)	(0.002)
Disability: any	0.187	-0.001	-0.001	-0.001
	(0.390)	(0.002)	(0.002)	(0.002)
Diability: learning disabled	0.128	0.000	-0.002	0.000
	(0.334)	(0.002)	(0.002)	(0.002)
ZIP code fixed effects		yes		yes
8th grade neighborhood school fixed effects			yes	yes

Note: Sample size is 64,936. The first column presents control group means (standard deviations). In columns (2) through (4) each cell presents the coefficient and standard error on a variable measuring the distance between a student's residence and the closest small high school. The columns differ by what geographic fixed effects are included. Standard errors are clustered by cohort and 8th grade school. Panel A reports the first stage regression and includes the following control variables in addition to the geographic fixed effects: indicators for whether a student is female, black, Hispanic, over age-for-grade, learning disabled, received free or reduced-price lunch or had unstable 8th grade enrollment, and Census tract information on concentration of poverty, socioeconomic status and tenancy. Panel B regresses the dependent variable listed in the row on the distance measure, cohort fixed effects, and geographic fixed effects only.

Table 3: Small school effects on high school persistence and completion

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Ordinary Least Squares			Instrumental Variables		
Dropout							
Dropout/left year T+1	0.108	-0.005	-0.003	-0.002	-0.077	-0.090	-0.094
	(0.310)	(0.006)	(0.006)	(0.005)	(0.037)	(0.043)	(0.083)
Dropout/left year T+2	0.205	0.000	0.004	0.004	-0.097	-0.100	-0.105
	(0.403)	(0.006)	(0.006)	(0.006)	(0.047)	(0.059)	(0.066)
Dropout/left year T+3	0.299	-0.002	-0.001	-0.001	-0.088	-0.140	-0.144
	(0.458)	(0.007)	(0.007)	(0.007)	(0.050)	(0.066)	(0.068)
Dropout/left year T+4	0.411	-0.017	-0.017	-0.018	-0.008	-0.037	-0.032
	(0.492)	(0.007)	(0.008)	(0.008)	(0.051)	(0.066)	(0.078)
Dropout/left year T+5	0.434	-0.015	-0.014	-0.015	-0.032	-0.060	-0.062
	(0.496)	(0.007)	(0.008)	(0.008)	(0.050)	(0.067)	(0.076)
Persistence							
On time 10th grade	0.737	0.030	0.027	0.026	0.095	0.132	0.133
	(0.440)	(0.009)	(0.008)	(0.008)	(0.063)	(0.071)	(0.086)
On time 11th grade	0.608	0.033	0.029	0.028	0.134	0.174	0.178
	(0.488)	(0.008)	(0.008)	(0.008)	(0.062)	(0.073)	(0.080)
On time 12th grade	0.546	0.023	0.021	0.021	0.058	0.102	0.098
	(0.498)	(0.008)	(0.008)	(0.008)	(0.056)	(0.069)	(0.076)
Graduated on time	0.481	0.031	0.027	0.028	0.067	0.092	0.089
	(0.500)	(0.008)	(0.008)	(0.008)	(0.051)	(0.065)	(0.069)
Graduated within 5 years	0.527	0.022	0.021	0.022	0.026	0.048	0.051
	(0.499)	(0.007)	(0.008)	(0.008)	(0.049)	(0.064)	(0.071)
ZIP code fixed effects		yes		yes	yes		yes
8th grade neighborhood school fixed effects			yes	yes		yes	yes

Note: Sample size is 64,936. The column (1) presents control group means (standard deviations). In columns (2) through (4) each cell presents the coefficient and standard error on an indicator for whether a student attended a small school in 9th grade in a regression where the dependent variable is listed in the row and geographic fixed effects specificied in the column. Standard errors are clustered by cohort and 8th grade school. Baseline controls include In columns (5) through (7) each cell presents the coefficient and standard error of an instrumental variables regression where enrollment in a small high school is predicted by the minimum distance between a student's home address and the closest small high school. All regressions in columns (2) through (7) have standard errors clustered by cohort and 8th grade school, and control for cohort fixed effects and the following characteristics: indicators for whether a student is female, black, Hispanic, over age-for-grade, learning disabled, received free or reduced-price lunch or had unstable 8th grade enrollment, and Census tract information on concentration of poverty, socioeconomic status and tenancy.

Table 4: Small school effects on high school persistence and completion: Subgroup analysis

	African American			Hispanic			Female			Male		
	control mean	OLS	IV	control mean	OLS	IV	control mean	OLS	IV	control mean	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(10)	(12)
First stage												
Attend small school			-0.039			-0.070			-0.043			-0.048
			(0.006)			(0.014)			(0.007)			(0.006)
Dropout												
Dropout/left year t+1	0.102	0.002	-0.128	0.121	-0.021	-0.004	0.098	0.002	-0.115	0.118	-0.005	-0.061
	0.302	(0.009)	(0.054)	0.326	(0.020)	(0.077)	0.297	(0.011)	(0.059)	0.323	(0.010)	(0.056)
Dropout/left year t+2	0.201	0.014	-0.187	0.208	-0.036	0.054	0.179	0.004	-0.044	0.230	0.007	-0.144
	(0.401)	(0.010)	(0.078)	(0.406)	(0.019)	(0.108)	(0.384)	(0.011)	(0.075)	(0.421)	(0.010)	(0.086)
Dropout/left year t+3	0.300	0.004	-0.157	0.291	-0.025	-0.141	0.257	-0.007	-0.082	0.342	0.007	-0.186
	(0.458)	(0.011)	(0.081)	(0.454)	(0.019)	(0.115)	(0.437)	(0.011)	(0.082)	(0.474)	(0.012)	(0.093)
Dropout/left year t+4	0.421	-0.011	0.037	0.386	-0.051	-0.159	0.353	-0.026	-0.025	0.471	-0.008	-0.023
	(0.494)	(0.012)	(0.086)	(0.487)	(0.024)	(0.112)	(0.478)	(0.013)	(0.090)	(0.499)	(0.014)	(0.091)
Dropout/left year t+5	0.446	-0.009	-0.005	0.402	-0.039	-0.226	0.367	-0.027	-0.049	0.503	-0.002	-0.063
	(0.497)	(0.012)	(0.089)	(0.490)	(0.025)	(0.112)	(0.482)	(0.013)	(0.094)	(0.500)	(0.014)	(0.092)
Persistence												
On time 10th grade	0.744	0.018	0.169	0.717	0.077	0.055	0.784	0.023	0.094	0.688	0.027	0.161
	(0.436)	(0.018)	(0.092)	(0.450)	(0.028)	(0.112)	(0.412)	(0.016)	(0.083)	(0.463)	(0.020)	(0.099)
On time 11th grade	0.610	0.019	0.233	0.604	0.085	0.085	0.670	0.033	-0.005	0.546	0.022	0.325
	(0.488)	(0.016)	(0.096)	(0.489)	(0.026)	(0.120)	(0.470)	(0.016)	(0.092)	(0.498)	(0.016)	(0.103)
On time 12th grade	0.541	0.013	0.044	0.557	0.057	0.236	0.613	0.025	-0.006	0.479	0.015	0.184
	(0.498)	(0.014)	(0.087)	(0.497)	(0.027)	(0.129)	(0.487)	(0.015)	(0.094)	(0.500)	(0.015)	(0.092)
Graduated on time	0.469	0.022	0.017	0.507	0.054	0.255	0.556	0.040	0.014	0.404	0.013	0.148
	(0.499)	(0.013)	(0.086)	(0.500)	(0.025)	(0.110)	(0.497)	(0.014)	(0.095)	(0.491)	(0.014)	(0.091)
Graduated within 5 years	0.510	0.018	0.008	0.567	0.041	0.161	0.599	0.033	0.007	0.454	0.009	0.077
	(0.500)	(0.013)	(0.087)	(0.495)	(0.025)	(0.100)	(0.490)	(0.014)	(0.094)	(0.498)	(0.014)	(0.088)

Notes: This table presents heterogeneous impacts across different subgroups. Each set of columns is limited to the subgroup named at the top of the column. The first column in each set presents control group means (standard deviations). The second column reports the OLS relationship between small school attendance and the outcome denoted in the row title, and uses the same specification as column (4) of Table 3. The third column reports the IV estimate of the impact of small school attendance on each outcome, and uses the same specification as column (7) of Table 3. Standard errors are clustered by cohort and 8th grade school. All regressions include fixed effects for cohort, 8th grade neighborhood school, and ZIP code. Where appropriate, additional controls include indicators for whether a student is female, black, Hispanic, over age-for-grade, learning disabled, received free or reduced-price lunch or had unstable 8th grade enrollment, and Census tract information on concentration of poverty, socioeconomic status and tenancy.

Table 4: Small school effects on high school persistence and completion: Subgroup analysis (Part 2)

	Prior Low Score			Prior High Score			Learning Disabled		
	control mean	OLS	IV	control mean	OLS	IV	control mean	OLS	IV
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20	(21)
First stage									
Attend small school			-0.051			-0.040			-0.061
			(0.007)			(0.007)			(0.008)
Dropout									
Dropout/left year t+1	0.122	0.000	-0.078	0.082	0.007	-0.143	0.121	-0.006	-0.221
	0.327	(0.009)	(0.053)	0.274	(0.015)	(0.091)	0.326	(0.013)	(0.091)
Dropout/left year t+2	0.246	0.003	-0.067	0.138	0.021	-0.110	0.225	0.014	-0.321
	(0.431)	(0.010)	(0.072)	(0.345)	(0.018)	(0.118)	(0.418)	(0.017)	(0.137)
Dropout/left year t+3	0.366	-0.011	-0.127	0.194	0.037	-0.186	0.343	0.018	-0.321
	(0.482)	(0.012)	(0.081)	(0.395)	(0.019)	(0.134)	(0.475)	(0.019)	(0.144)
Dropout/left year t+4	0.492	-0.025	-0.020	0.278	0.013	-0.178	0.462	0.004	-0.150
	(0.500)	(0.013)	(0.083)	(0.448)	(0.023)	(0.154)	(0.499)	(0.019)	(0.135)
Dropout/left year t+5	0.523	-0.019	-0.050	0.286	0.003	-0.235	0.496	0.007	-0.163
	(0.499)	(0.013)	(0.081)	(0.452)	(0.023)	(0.150)	(0.500)	(0.020)	(0.133)
Persistence									
On time 10th grade	0.676	0.024	0.067	0.838	0.015	0.211	0.683	0.011	0.084
	(0.468)	(0.020)	(0.087)	(0.368)	(0.021)	(0.132)	(0.465)	(0.027)	(0.123)
On time 11th grade	0.520	0.033	0.183	0.754	0.017	0.147	0.547	0.019	0.368
	(0.500)	(0.017)	(0.084)	(0.431)	(0.022)	(0.153)	(0.498)	(0.024)	(0.146)
On time 12th grade	0.446	0.027	0.084	0.712	-0.006	0.194	0.477	0.005	0.222
	(0.497)	(0.015)	(0.083)	(0.453)	(0.023)	(0.157)	(0.499)	(0.022)	(0.138)
Graduated on time	0.375	0.031	0.099	0.660	0.001	0.182	0.409	-0.012	0.290
	(0.484)	(0.013)	(0.075)	(0.474)	(0.025)	(0.162)	(0.492)	(0.021)	(0.132)
Graduated within 5 years	0.426	0.028	0.066	0.698	0.002	0.154	0.458	-0.007	0.264
	(0.494)	(0.013)	(0.077)	(0.459)	(0.023)	(0.151)	(0.498)	(0.020)	(0.130)

Notes: This table presents heterogeneous impacts across different subgroups. Each set of columns is limited to the subgroup named at the top of the column. The first column in each set presents control group means (standard deviations). The second column reports the OLS relationship between small school attendance and the outcome denoted in the row title, and uses the same specification as column (4) of Table 3. The third column reports the IV estimate of the impact of small school attendance on each outcome, and uses the same specification as column (7) of Table 3. Standard errors are clustered by cohort and 8th grade school. All regressions include fixed effects for cohort, 8th grade neighborhood school, and ZIP code. Where appropriate, additional controls include indicators for whether a student is female, black, Hispanic, over age-for-grade, learning disabled, received free or reduced-price lunch or had unstable 8th grade enrollment, and Census tract information on concentration of poverty, socioeconomic status and tenancy.

Table 5: Small school effects on high school test scores

	Test scores			Test scores with missing scores imputed		
	Mean of control	OLS	IV	Mean of control	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Mathematics/science test scores						
Math fall 9th grade	13.0	-0.69	-0.83	12.9	-0.59	-0.69
	(3.6)	(0.17)	(0.72)	(3.7)	(0.16)	(0.70)
Math fall 10th grade	14.2	-0.54	-0.10	14.1	-0.46	-0.60
	(3.1)	(0.12)	(0.59)	(3.2)	(0.11)	(0.58)
Math ACT score (spring 11th grade)	16.1	-0.61	-1.28	16.7	-0.61	-0.75
	(2.8)	(0.11)	(0.61)	(3.0)	(0.11)	(0.59)
Science ACT score (spring 11th grade)	16.4	-0.49	-0.44	16.9	-0.79	-0.87
	(3.6)	(0.14)	(0.73)	(3.6)	(0.15)	(0.73)
Reading/English test scores						
Reading fall 9th grade	12.7	-0.59	-1.14	12.6	-0.50	-1.24
	(2.8)	(0.14)	(0.55)	(2.8)	(0.13)	(0.52)
Reading fall 10th grade	14.3	-0.51	-0.46	14.1	-0.44	-1.09
	(3.4)	(0.14)	(0.66)	(3.4)	(0.13)	(0.63)
Reading ACT score (spring 11th grade)	15.7	-0.74	-1.79	16.2	-0.78	-2.25
	(4.1)	(0.16)	(0.92)	(4.1)	(0.17)	(0.89)
English ACT score (spring 11th grade)	15.1	-0.99	-2.12	15.6	-0.90	-2.56
	(4.5)	(0.21)	(1.00)	(4.7)	(0.20)	(1.02)

Notes: This table presents impacts of small schools on high school test score outcomes. The first set of columns uses all available test scores, and the second set imputes missing values for students who were no longer enrolled or did not take the test for some other reason. The first column in each set presents control group means (standard deviations). The second column reports the OLS relationship between small school attendance and the outcome denoted in the row title, and uses the same specification as column (4) of Table 3. The third column reports the IV estimate of the impact of small school attendance on each outcome, and uses the same specification as column (7) of Table 3. Standard errors are clustered by cohort and 8th grade school. All regressions include fixed effects for cohort, 8th grade neighborhood school, and ZIP code, and indicators for whether a student is female, black, Hispanic, over age-for-grade, learning disabled, received free or reduced-price lunch or had unstable 8th grade enrollment, and Census tract information on concentration of poverty, socioeconomic status and tenancy.

Appendix Table 1: Mean characteristics of small high school students and their 8th grade schoolmates, by 9th grade cohort year

	9th grade in 2002			9th grade in 2003			9th grade in 2004		
Characteristic	**Small school 9th graders**	**Former classmates**	**p-value of difference**	**Small school 9th graders**	**Former classmates**	**p-value of difference**	**Small school 9th graders**	**Former classmates**	**p-value of difference**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
8th grade year demographics									
Female	0.530	0.519	0.719	0.470	0.518	0.000	0.497	0.511	0.096
Black	0.820	0.857	0.276	0.858	0.752	0.678	0.863	0.693	0.000
Hispanic	0.175	0.122	0.223	0.133	0.213	0.930	0.116	0.261	0.012
Free and reduced price lunch	0.879	0.895	0.762	0.881	0.879	0.161	0.863	0.875	0.251
Over age-for-grade	0.269	0.237	0.253	0.347	0.291	0.000	0.358	0.284	0.000
Unstable enrollment 8th grade	0.042	0.045	0.347	0.079	0.053	0.074	0.075	0.050	0.002
Disability: any	0.241	0.156	0.000	0.221	0.185	0.000	0.257	0.196	0.000
Diability: learning disabled	0.168	0.113	0.003	0.160	0.127	0.001	0.189	0.133	0.000
Minimum distance to a small high school	1.09	2.88	0.000	1.15	2.58	0.000	1.28	2.48	0.000
Prior test scores									
8th grade math z-score	-0.534	-0.237	0.000	-0.489	-0.257	0.000	-0.516	-0.221	0.000
8th grade reading z-score	-0.376	-0.117	0.000	-0.384	-0.181	0.000	-0.391	-0.168	0.000
2000 Census block group characteristics									
Poverty concentration	0.628	0.618	0.928	0.657	0.581	0.040	0.605	0.502	0.674
Socioeconomic status	-0.217	-0.271	0.249	-0.287	-0.399	0.075	-0.341	-0.382	0.896
Tenancy	12.0	12.4	0.856	11.7	11.8	0.036	11.9	11.6	0.639
High school outcomes									
Dropout/left year t+1	0.133	0.110	0.743	0.135	0.095	0.002	0.116	0.100	0.055
Dropout/left year t+2	0.252	0.197	0.142	0.261	0.207	0.011	0.225	0.223	0.720
Dropout/left year t+3	0.339	0.297	0.295	0.350	0.329	0.174	0.344	0.286	0.000
Dropout/left year t+4	0.486	0.408	0.040	0.484	0.447	0.117	0.463	0.418	0.018
Dropout/left year t+5	0.516	0.442	0.036	0.507	0.458	0.043	0.477	0.442	0.029
On time 10th grade	0.797	0.758	0.084	0.734	0.757	0.038	0.694	0.738	0.016
On time 11th grade	0.603	0.641	0.062	0.569	0.602	0.085	0.595	0.575	0.306
On time 12th grade	0.479	0.543	0.051	0.502	0.518	0.374	0.528	0.552	0.146
Graduated on time	0.409	0.470	0.034	0.422	0.438	0.262	0.464	0.481	0.326
Graduated within 5 years	0.451	0.500	0.135	0.458	0.492	0.106	0.493	0.520	0.097
N	428	4419		1011	10794		1478	13196	

Notes: This table presents summary statistics for the analysis sample, separately by 9th grade cohort year. The first column in each group presents average characteristics among students who attended a small high school in 9th grade. The second column presents average characteristics of the 8th grade schoolmates of the students in column (1). The third column presents the p-value of a test for equality across the first two columns after conditioning on 8th grade school fixed effects. 5th and 8th grade test scores are normalized by the district-wide mean and standard deviation in the year of the test. High school outcomes are measured in the fall.

Appendix Table 1: Mean characteristics of small high school students and their 8th grade schoolmates, by 9th grade cohort year

Characteristic	9th grade in 2005			9th grade in 2006		
	Small school 9th graders	**Former classmates**	**p-value of difference**	**Small school 9th graders**	**Former classmates**	**p-value of difference**
	(10)	(11)	(12)	(13)	(14)	(15)
8th grade year demographics						
Female	0.515	0.499	0.264	0.506	0.491	0.067
Black	0.784	0.671	0.016	0.765	0.636	0.001
Hispanic	0.201	0.289	0.051	0.213	0.312	0.006
Free and reduced price lunch	0.883	0.889	0.649	0.905	0.891	0.307
Over age-for-grade	0.326	0.306	0.352	0.313	0.285	0.741
Unstable enrollment 8th grade	0.060	0.051	0.373	0.055	0.051	0.292
Disability: any	0.216	0.195	0.003	0.213	0.183	0.007
Diability: learning disabled	0.150	0.129	0.003	0.150	0.127	0.024
Minimum distance to a small high school	1.17	2.35	0.000	1.25	2.39	0.000
Prior test scores						
8th grade math z-score	-0.442	-0.261	0.000	-0.391	-0.221	0.000
8th grade reading z-score	-0.314	-0.201	0.001	-0.315	-0.188	0.003
2000 Census block group characteristics						
Poverty concentration	0.598	0.485	0.514	0.579	0.429	0.030
Socioeconomic status	-0.472	-0.416	0.279	-0.437	-0.408	0.796
Tenancy	11.8	11.5	0.470	11.8	11.5	0.259
High school outcomes						
Dropout/left year t+1	0.089	0.137	0.028	0.097	0.096	0.198
Dropout/left year t+2	0.206	0.199	0.246	0.187	0.194	0.628
Dropout/left year t+3	0.299	0.311	0.814	0.264	0.278	0.298
Dropout/left year t+4	0.379	0.411	0.409	0.359	0.382	0.031
Dropout/left year t+5	0.415	0.438	0.678	0.388	0.405	0.146
On time 10th grade	0.768	0.680	0.004	0.820	0.766	0.019
On time 11th grade	0.655	0.598	0.146	0.677	0.641	0.156
On time 12th grade	0.573	0.539	0.669	0.591	0.570	0.116
Graduated on time	0.512	0.485	0.482	0.536	0.510	0.041
Graduated within 5 years	0.557	0.532	0.396	0.573	0.562	0.306
N	2264	13747		2187	15412	

Notes: This table presents summary statistics for the analysis sample, separately by 9th grade cohort year. The first column in each group presents average characteristics among students who attended a small high school in 9th grade. The second column presents average characteristics of the 8th grade schoolmates of the students in column (1). The third column presents the p-value of a test for equality across the first two columns after conditioning on 8th grade school fixed effects. 5th and 8th grade test scores are normalized by the district-wide mean and standard deviation in the year of the test. High school outcomes are measured in the fall.