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The effect of school construction on test scores, school enrollment, and home prices☆

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ABSTRACT

This paper provides new evidence on the effect of elementary and middle school construction projects on home prices, academic achievement, and school enrollment. Combining the staggered implementation of a comprehensive school construction project in a poor urban district with panel data on student test scores and neighborhoods of residence, we find that, by six years after building occupancy, school construction increases reading scores by 0.15 standard deviations relative to the year before building occupancy. We do not observe similar effects for math scores. School construction raised home prices in affected neighborhoods by roughly 10%, and led to increased public school enrollment.

1. Motivation

1.1. Motivation and summary

Investment in school infrastructure is one of the principal ways in which federal, state, and local governments develop physical capital in U.S. communities. In 2008, public expenditures on school construction, land, and building acquisition totaled more than $58 billion.[1] This represents not just a large share of total education-related expenditures – roughly 10% – but a large share of overall infrastructure expenditure: in 2004, public investment in school infrastructure was $75.9 billion, nearly as much as the $99.7 billion public investment in all forms of transportation infrastructure, including roads, mass transit, and aviation.[2]

In this paper, we present new evidence on the causal effect of school infrastructure investment on student test scores, neighborhood-specific public school enrollment, and housing prices. We take advantage of a unique natural experiment in which a poor, urban school district embarked upon a comprehensive 15-year, $1.4 billion school construction program (believed to be the largest per-capita construction program in the nation over the period) to produce estimates that are unbiased by the endogeneity of school construction to school characteristics. Our empirical strategy uses the fact that occupancy dates varied widely across schools, with the first school completed in 1998 and the last slated to be completed in the mid-2010s. Specifically, we use a difference-in-differences comparison of test scores, home prices, and public school attendance in neighborhoods on different construction schedules. For our test score analysis, we combine this approach with panel data methods such as student fixed effects to address concerns about student selection into newly built schools.

We find strong evidence that the school construction program led to sustained gains in reading scores for elementary and middle school students. Trends in reading scores are flat in the years leading up to construction, but turn upwards in the year of construction and continue to increase for at least the next six years. By the sixth year following building occupancy, student scores rise by 0.15 standard deviations above their levels in the year prior to building occupancy. These gains are large, but not implausibly so; roughly speaking, they are of similar magnitude to those experienced by students who enroll in high-performing charter schools for one year.

We use three complementary empirical specifications to ensure that our findings are not the result of student selection into newly-built schools. The first focuses on students present in neighborhoods the year prior to occupancy of new facilities. The second and third control, respectively, for individual fixed effects and lagged score outcomes. Combined with the absence of pre-occupancy score effects, the consistency of our findings across these specifications suggests that our estimates capture the causal effect of school construction on scores.

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[1] See Filardo et al. (2010) and Abramson (2008).

[2] Source: CBO 2008.

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Math scores also appear to increase following school construction, but estimated effects are small and we are generally unable to reject the null hypothesis that school construction does not affect math scores.

Housing prices and neighborhood public school enrollment also respond positively to school construction. Elementary and middle school construction raised home values by 10.3%, and the number of school zone residents attending public school by up to 17.3%. The impact of school construction on public school enrollment at the district level is reduced by negative spillovers across neighborhoods; we find no evidence of similar spillovers for reading scores or home prices. As with the estimated test score effects, the timing of these changes generally coincides with the occupancy of completed buildings, though we do see some indications that housing markets price in infrastructure effects prior to building occupancy in a way that corresponds to the release of information about project plans. Taken together, our student outcome, home price, and enrollment results indicate that families, and in particular families with children, place a high value on school infrastructure investment. If families only valued infrastructure insofar as it improved education production, this would imply that raising school value added by 0.1 standard deviations would raise neighborhood home prices by 6.7%, and enrollment of neighborhood residents in public schools by 11.3%. Since school construction also changes neighborhood amenities in other ways, these values should be interpreted as upper bounds on the true elasticities.

Our findings beg the question of *why* school construction has the observed effects. Possible pathways through which school construction could improve educational production include the direct effects of new facilities on pedagogy, effects on student and teacher motivation during school hours, and effects on student and parent motivation to invest in academic production at home. Though our empirical work does not allow us to distinguish between these channels, a survey of district principals indicates that student and teacher motivation were at least as important as direct pedagogical effects for improving academic outcomes. We also conduct empirical tests of the hypothesis that the effects of school construction spill over into close substitute neighborhoods, perhaps due to changes in sorting patterns of students and their families. We find no indication that cross-neighborhood spillovers affected home prices or reading scores.

1.2. Contributions to the literature

We build upon and link two distinct strands of literature. The first considers the effects of school infrastructure investment on student and neighborhood outcomes in the context of the U.S. and other developed countries.[3] There are few compelling estimates of how infrastructure expenditures affect student performance. In a review of literature on the education production function, Hanushek (1997) reports that of 91 correlational studies examining the relationship between facility quality and student performance, only 9% found a statistically significant positive relationship, while 5% found a statistically significant negative relationship.[4] Recent studies have returned similarly mixed results; see, e.g., Bowers and Urick (2011). Since facility quality is closely associated with other inputs into education production, the absence of consistent findings is difficult to interpret.

Cellini et al. (2010), henceforth CFR, provide estimates with a clearer causal interpretation. CFR employ a regression discontinuity around the outcomes of school district-level votes on the bond issues used to finance school construction projects to estimate the effects of school construction spending in California on home prices and test scores. They find that home prices rise in response to bond passage, increasing by 4% in the year following the vote and by between 7 and 10% six years later. They find weaker evidence of test score effects, which remain close to zero and statistically insignificant for five years following bond passage before rising somewhat in the sixth year, and then falling back to zero again thereafter.

CFR's research design has two important limitations. First, CFR use district-level third and fourth grade test scores to measure score effects. Since district-level expenditures are a noisy measure of the expenditures we would expect to improve outcomes for third and fourth graders (e.g., expenditures on elementary schools as opposed to high schools), it is likely that their estimates understate the role of school construction in educational production. Second, the cost of identification via regression discontinuity is that estimates cannot necessarily be extrapolated to districts that are not on the electoral margin of bond passage. If electoral outcomes are a function of residents' beliefs about the benefits of school construction, effects in marginal districts will likely differ systematically from effects for districts in which bonds pass or fail by a comfortable amount. In sum, CFR show convincingly that the residents of electorally marginal districts value school construction, but questions remain about test score effects generally, and about home price and test score effects for different types of inframarginal districts. Of particular interest are poor urban districts, because these districts are frequent targets of policy interventions aimed at improving school quality and also tend to have low-quality existing facilities.[5] Using student microdata, we present evidence of a plausibly causal relationship between school construction and test scores, and do so in the context of a poor urban district with baseline facility quality similar to that in other urban districts in the state.

The second strand of literature examines the way housing markets price the quality of local schools. Black (1999) uses discontinuities in the prices of homes on the borders between school districts to estimate the price effects of differences between school-average test scores. Bayer et al. (2007) nests this identification strategy within a model of housing demand and makes the observation that much of the observed price gap is attributable to endogenous socioeconomic segregation along district boundaries, not to test scores per se. One implication of the finding that school quality has an independent effect on the housing market is that changes in school quality should set off a process of residential sorting and changes in home prices. This is how the socioeconomic stratification along zone boundary lines reported in Bayer et al. comes into existence. Because both Black and Bayer et al. estimate static models of housing demand, they cannot observe this process as it unfolds. We use panel data on home prices, public school enrollment, and test scores to document dynamic changes in education production, home prices, and residency patterns in response to the school construction intervention. Our results indicate that changes in school enrollment among neighborhood residents and education production begin at the time of occupancy. Home prices also jump at occupancy, although there is some evidence that housing markets price in part of the construction effect around the time project plans are filed.

We also add to this literature by examining price responses to changes in education production as opposed to aggregate school scores. One limitation of Black and Bayer et al. is that both papers use average scores within school attendance zones as their measure of school quality. Zone-level averages represent a mix of the causal effect of zoned schools (i.e., education production) on scores for students living in the

[3] Duflo (2001) uses a difference-in-differences approach to obtain plausibly causal estimates of the effects of a large Indonesian school construction program on educational attainment and labor market outcomes. Because her paper focuses on the construction of schools where none had existed before, there is little reason to think that these results would apply in developed countries, where the main challenges are those of renovation and rebuilding.

[4] 23% reported statistically insignificant positive relationships, 19% reported statistically significant negative relationships, and 44% reported relationships of indeterminate sign.

[5] Filardo (2006) found that rates of investment in new infrastructure were twice as high in rich urban districts as in poor urban districts between 1995 and 2004. Evidence of differential investment can be seen in the heterogeneity of infrastructure quality across schools: 43% of schools in which 75 or more percent of students are eligible for free lunch use portable buildings as classrooms, in contrast with 27% of schools in which less than 35% of students are free-lunch eligible (Source: NCES Digest of Education Statistics (2009): Table 101.) If the marginal returns to investment in infrastructure are decreasing in the quality of existing infrastructure, then poor districts will benefit disproportionately from school construction.

neighborhood and the underlying test score determinants for those students. Parents trying to optimize over education production will be interested in the causal portion of the average but not the portion that is tied to selection. We use the school construction natural experiment to identify changes in education production at the school zone level, and link these changes to increases in home prices. Because school construction may make neighborhoods more desirable in other ways, our estimates should be interpreted as upper bounds on the true elasticities of home prices with respect to education production. We believe they nevertheless constitute an important first attempt at pricing educational production.

The paper proceeds as follows. In Section 2, we describe the school district and the school construction program. In Section 3, we discuss an economic framework that we use to guide and interpret our empirical specifications. In Section 4, we describe the student and home price data we use to conduct our analysis. Section 5 presents our empirical findings on the home price, residency, and test score effects of school construction. Section 6 discusses possible mechanisms and describes the results of our survey of school principals. Section 7 concludes.

2. The natural experiment

2.1. The school district

Our project focuses on the public school system in New Haven, Connecticut, which we will refer to as NHPS or the District. New Haven is one of the largest districts in Connecticut and is similar to many urban school districts in the United States. The students mostly come from poor families and overwhelmingly belong to minority groups that have traditionally lagged in educational outcomes such as graduation rates and test scores. The District has an enrollment of approximately 20,000 students, of whom more than 80% are eligible for free lunch and just under 90% are either black or Hispanic. More than one out of four students speak a language other than English at home. High school dropout rates are triple the state average and test scores are substantially lower than those in the rest of the state. Between 2001 and 2009 (during which the bulk of school construction took place; see below) the proportion of black students in the District fell while the proportion of Hispanic students rose and the proportion of white students remained steady. District enrollment fell from 20,201 to 19,607, mirroring a slight decline in enrollment across Connecticut. More detail is provided in Table A-1 of the Online Appendix.

In Connecticut, poor urban districts typically have infrastructure of lower quality than other districts. Early in the school construction program, NHPS' facilities were roughly comparable to those of other in-state urban districts. In 2001, public schools in New Haven and Hartford, another urban district in Connecticut, were on average well over 50 years old. In both cities, schools reported problems with more than half of basic service systems, such as heating, air conditioning, plumbing, and lighting. In Connecticut as a whole, schools reported problems with less than one third of these systems.[6]

2.2. The school construction project

In contrast to many urban districts, NHPS has had the political and financial backing to enact an ambitious infrastructure investment program. An important contributor to the successful execution of this project was the availability of federal and state financing: the District paid for only 23% of the total cost of buildings completed by 2010.[7]

The School Construction Project (SCP) had a dramatic effect on primary- and secondary-school infrastructure across the city. The first SCP school was completed in 1998, and the last is scheduled to be completed by 2014. Projected total spending is $1.4 billion, with $1.1 billion spent on projects that had been completed by 2010. Of 42 school buildings,[8] 12 had been rebuilt completely by 2010, and 18 had been significantly renovated. An additional seven were under construction or under design. The remaining five buildings, all of which house inter-district magnet or small K-1 schools, will not be rebuilt or renovated. School renovations were generally substantial, incurring costs similar to those of new construction: mean expenditure on renovated schools was $33 million, compared to a mean expenditure of $38 million on rebuilt schools (all dollar values refer to 2005 dollars). The project served students at all educational levels: of nine high schools in the district, five had been rebuilt or renovated and occupied by 2010, with an additional high school in the construction stage. Similarly, of 33 total elementary or middle schools, 25 had been rebuilt or renovated and occupied by 2010, with work on an additional six in the planning or construction phase. The top two panels of Table A.2 describe the scope of the SCP.

Though the changes made to schools varied depending on the condition of the existing school, SCP administrators targeted a number of areas for improvement at all schools. One priority was heating and air conditioning. Prior to the SCP, many schools did not have air conditioning, and some had inadequate heating. A second was classroom technology. Classrooms in new and renovated schools were designed to facilitate the use of computers, and science and media facilities for school-wide use were also improved. A third was community access. SCP administrators designed gyms, playgrounds, and meeting spaces to allow for use by community members as well as students. A fourth was to decrease energy and maintenance costs. A fifth and slightly more abstract goal of the SCP was to make schools more 'livable' through subtler changes in design. The design of new school buildings often allowed for more natural light than in the old buildings, and a portion of the budget for each school was allocated to public artwork. For a more detailed description of several of the school construction projects, see Online Appendix B.

School expansion was not among the primary goals of the SCP, which took place in a time of declining overall demand for classroom space in the District.[9] The SCP did not seek to change the allocation of students across the District: there were no major changes in school zone boundaries over the period.[10] One consequence of the neutrality of the SCP with respect to enrollment was that new and renovated buildings typically did not offer much more classroom space than the facilities they replaced. Among the sixteen projects for which we were able to recover pre- and post-completion classroom square footage, the median change was less than 7%. Classroom space fell by 6.5% in the District's best-performing school.

The SCP had a pronounced effect on the quality of the school environment in the District. One way to see this is to track changes in the quality of District schools and compare them to changes at the state level and in other urban districts. Between 2001 and 2009, the percentage of low quality basic service systems (for example, air conditioning or lighting) fell from 32 to 18% at the state level, and from 54 to 30% in Hartford, another poor district in Connecticut. The percentage of service failures in NHPS schools fell from 53 to 14%. The SCP pushed the quality of NHPS infrastructure from far below the state average to somewhat above it.[11]

[6] The Connecticut Department of Education collects information on school infrastructure by surveying school principles. Table A.1 shows the frequency with which principals rated service items such as heating, plumbing and air conditioning as either fair or poor in New Haven, Hartford, and Connecticut as a whole in 2001 and 2009.

[7] The majority of funding for the District share of school construction (about $300 million) was drawn from New Haven's general fund, financed by city revenues and bond issues. Roughly $70 million came from a dedicated trust fund funded by sales of assets such as delinquent tax liens and old school buildings.

[8] This count omits charter schools and transitional schools for at-risk youth, and counts each address separately for schools with multiple addresses.

[9] See Table A.1 of the Online Appendix.

[10] There were a handful of small changes to zone boundaries. For example, in one case, school construction allowed students in a PK-4 school to be merged with a nearby K-8 school. We hold school zone boundaries fixed in their 2007 positions throughout our analysis.

[11] See Table A.1.

The construction process led to some movement of student populations across buildings prior to facility completion. Though students in schools that were completely rebuilt often remained in their original school building until the new building became ready for occupancy, students at schools that underwent major renovations were typically moved to swing spaces during the construction process. Students were moved as a group, so that construction did not mechanically affect school composition.

2.3. Selection of schools in the SCP

Each student in the NHPS system is assigned to a zoned elementary, middle, and high school based on his or her address. School assignment triplets partition the District into 25 geographic areas, which we term ‘neighborhoods.’ These neighborhoods form the basis for the assignment of the school construction treatment to homes and resident students in our analysis. We focus our analysis on construction of elementary and middle schools because most District high schools are magnet schools not associated with neighborhood zones.

Our goal is to identify the effects of school construction on neighborhood and student outcomes using a difference-in-differences approach. This approach will yield unbiased estimates even if the selection of schools for renovation or the timing of renovation conditional on selection is systematically related to school performance levels. Quasi-random assignment of school construction is not required. Further, combining the difference-in-differences approach with student panel data allows our test score analysis to account directly for any changes in student selection into neighborhoods that accompanies school construction. However, our estimates will be biased if the timing of school construction is related to neighborhood-specific, time-varying shocks to the outcomes of interest. For instance, our results would be compromised if the SCP only placed schools in neighborhoods following a sudden drop in crime.

Discussions with SCP administrators and empirical investigation indicate that the process by which schools were chosen was largely exogenous to community and school characteristics. The comprehensive nature of the SCP rendered the question of *which* schools should be renovated irrelevant; instead, the key question administrators faced was how to choose the order of construction. SCP administrators have stated to the authors that, with the exception of the first few schools, the determinants of construction order were primarily logistical and design hurdles, not community or student characteristics. Further, construction projects did not coincide with other school or community-related interventions. This claim is consistent with what we observe in the data. Schools built or renovated in the early phases of the SCP do not differ from schools constructed in the project's later phases in terms of student demographics or the characteristics of surrounding neighborhoods. Table 1 compares schools constructed prior to 2006 (the approximate midpoint of the project in terms of completed buildings) to schools constructed after 2006. There are no statistically significant differences between the characteristics of schools constructed in the first half of the project and the second.[12]

Table 1
Characteristics of treated and untreated schools in 2006.

	Untreated	Treated	p-Value
Count	21	18	
Male	0.51	0.50	0.622
Black	0.53	0.54	0.886
Hispanic	0.32	0.32	0.949
English Language Learner (ELL)	0.14	0.10	0.380
Special education	0.08	0.08	0.506
Free lunch	0.80	0.78	0.623
Income	17.7	16.3	0.437
Rent	647	627	0.132
Family with kids	0.30	0.31	0.840

Comparison of characteristics of treated and untreated elementary, middle, and high schools at the project midpoint in terms of occupied buildings (2006). The upper panel describes the characteristics of student bodies in the two groups of schools. The lower panel describes the characteristics of surrounding neighborhoods, using averages across the closest three tracts from the 2000 Census. Income is per capita income for the total population in 1999 dollars. Rent is median gross rent in renter occupied housing. The p-value is from the t-test of equality across the two groups. The joint test fails to reject the null hypothesis of no relationship between the two groups at conventional levels.

A direct way to assess the relationship between school construction and community characteristics is to look for effects of school construction that begin prior to the occupancy of the new building. The presence of pre-occupancy effects might indicate a relationship between the construction ‘treatment’ and changes in the student body or surrounding neighborhood. Such effects could also reflect a forward-looking response to the construction project itself; we discuss this possibility in more detail below. In any case, our empirical analysis yields no evidence of pre-occupancy changes in neighborhood school enrollment or test scores, and the limited evidence of pre-occupancy changes in home-prices we do observe corresponds to the release of information about pre-occupancy phases of the construction projects. The timing of school construction does not appear to have been endogenous to student and community characteristics.

It is also important to ask why district officials chose to pursue such an ambitious infrastructure project. One might imagine that the district embarked upon the project to compensate for particularly decrepit pre-SCP facilities in the district as a whole. If this were the case, it would compromise the generalizability of our results to districts with better baseline levels of infrastructure. However, as discussed above, school buildings in the district were not in observably worse condition than school buildings in similar cities. That said, we caution against applying our findings to schools in wealthier districts with average or above-average levels of existing infrastructure.

3. Economic framework

3.1. Conceptual model

We estimate the effects of school construction on home prices, school enrollment, and test scores using a difference-in-differences strategy that exploits the fact that students and homes in different neighborhoods receive the school construction treatment at different times. Underlying our estimates are a set of economic agents facing choices about where to live and how much to invest in educational production. These choices take neighborhood amenities (including school quality) and neighborhood home prices as inputs; prices, residential patterns, and test score outcomes are jointly determined in equilibrium. In this section, we highlight key implications of the education production and residential choice problems, then present our empirical specifications. We focus on our home price and test score specifications, leaving aside discussion of our very similar school enrollment specifications for the sake of brevity.

First consider educational production. Students' test score performance depends on a combination of current and prior investments at home and at school, as well as stochastic shocks (Todd and Wolpin 2007). School construction can affect test scores directly by facilitating more efficient in-school investment. It can also affect test scores indirectly by encouraging investment outside of school.

Changes in school facilities can affect preferences for neighborhoods either through improved education production or through improved

[12] Informative cross-group comparisons of test scores are not feasible because we do not have test score data prior to 2004, and many schools in the treated category had already been treated by then. Comparisons of 2004 or 2006 scores would reflect the effects of treatment whether or not initial assignment was balanced.

neighborhood amenities. Some of these amenities, like playing fields or meeting spaces, may be available to non-student community members. Although families making residential choices may be forward-looking, moving is costly, and information about proposed policy changes may spread slowly, so changes in neighborhood-specific policies will not be immediately reflected by changes in home prices or neighborhood composition.

Four points are important to keep in mind:

1. School construction projects that raise neighborhood amenities, including school quality, will tend to raise home prices.
2. Families with school-aged children may value school quality more than families without children, since they value access not just to playing fields and meeting rooms but also to the education production process.
3. The degree to which school construction changes home prices and affects cross-neighborhood sorting depends on the value of the associated amenities to different types of families and the size of moving frictions that may hinder readjustment. The timing of these changes depend on the spread of information about the projects and in principle may precede project completion.
4. If sorting driven by school construction is related to determinants of test scores, school construction will have compositional effects that raise test performance in treated neighborhoods even if there is no increase education production.

3.2. Home price specifications

Home prices are a function of neighborhood amenities, school quality, and home characteristics. Because homebuyers are likely partially (if not fully) forward-looking, we allow markets to price in school construction gradually as information about the project becomes available. Our core difference-in-difference specifications are presented in Eq. 1.

$$p_{zht} = X_t^h\beta + \alpha_t + \alpha_z + \gamma_z \cdot t + \delta_f D_{ftz} + \delta_c D_{ctz} + \delta_o D_{otz} + \epsilon_{zth} \tag{1}$$

p_{zht} is the log price of home h in neighborhood z at time t. The X_t^h are the characteristics of home h at time t, while the α_z and γ_z are neighborhood-specific intercept and slope terms. These capture persistent gaps in school quality and other neighborhood amenities across neighborhoods. D_{ftz}, D_{ctz}, and D_{otz} are dummy variables equal to one if time t is after the filing date, construction date, or building occupancy date, respectively, in neighborhood z. Since filing precedes construction and construction precedes occupancy, these variables ‘stack’, so that homes sold post-occupancy receive a price bump of $\delta_f + \delta_c + \delta_o$. We choose the filing–construction–occupancy form because it parsimoniously but precisely captures changes in the information about the current and expected future state of school construction projects available to market participants. We present results from event-study specifications that allow for separate effects by year relative to building occupancy in section C of the Online Appendix.

For this model to yield unbiased estimates of construction effects, a) treatment dummies must not be correlated with changes in the unobservable price determinants of transacted homes, and b) treatments cannot coincide with other discontinuities in neighborhood-specific trends. Assumption a) will be violated if families with the resources and tastes to select into neighborhoods with new schools prefer homes that are unobservably more expensive than other families. As discussed in more detail in Section 5.1, we use assessor estimates of ‘unobserved’ home quality to address this issue. As mentioned above, discussions with district officials do not indicate that assumption b) is a major concern.

3.3. Test score specifications

Estimating the role of school construction in educational production is challenging because students can sort across neighborhoods. If students in families with preferences for new buildings differ from other students in terms of levels or trends in academic inputs, we may confound residential sorting with educational production. Results from Section 5 suggest that this kind of selection may not be an issue. Still, we take the problem of sorting seriously. Here, we describe three separate specifications, each of which provides unbiased estimates under different assumptions about student sorting and the structure of the error term in the educational production function.

The three specifications are:

$$T_{igzt} = \tau_{zg} + \tau_t + \sum_l \Delta_l D_{lzt} + X_i\beta + e_{igzt} \tag{2}$$

$$T_{igzt} = \tau_{zg} + \tau_t + \sum_l \Delta_l D_{lzt} + \tau_i + e_{igzt} \tag{3}$$

$$T_{igzt} = \tau_{zg} + \tau_t + \sum_l \Delta_l D_{lzt} + \pi T_{i,t-1} + X_i\beta + e_{igzt}. \tag{4}$$

Here, T_{igzt} denote test scores for student i in grade g and neighborhood z in year t. The τ_{zg} are neighborhood by grade fixed effects, and the τ_t are year fixed effects. The X_i are a set of student covariates, and e_{igzt} is a mean-zero error term. The D_{lzt} are dummy variables equal to one if neighborhood z is l years post-occupancy in year t, and the Δ_l are the coefficients of interest.

Eq. 2 is the Baseline OLS estimator. It is a standard difference-in-differences specification: the test score effects of school construction are identified off of within-neighborhood changes in scores around a common time path. To address the possibility that students select into treated neighborhoods, we restrict the sample to students who we observe in treated neighborhoods the year prior to building occupancy. We then assign treatment variables D_{lzt} and neighborhood effects τ_{zg} on the basis of time relative to building occupancy in the baseline neighborhood regardless of whether students are present in that neighborhood in year t. Holding neighborhood assignment fixed for the analysis sample means that our results will not be biased by time-invariant individual heterogeneity in test score determinants. The key identifying assumption is that time-varying individual-specific test score determinants are not correlated with the construction schedule. The main drawback of this specification is that it can only draw upon data for the subset of students whom we observe in a neighborhood the year prior to construction. This eliminates all pre-2004 projects from the analysis, since our student residency data begins in 2003 (see below).

Eq. 3 is the Fixed Effect estimator, which includes the student fixed effect term τ_i. Unlike the Baseline OLS estimator, we can estimate (3) using data that includes students who are present in schools only after or before building occupancy, and assign treatment variables based on students' year t neighborhood of residence. This allows us to look at longer-run effects of school construction and to assess the effects of construction in a way that includes students who enter zoned neighborhoods post-occupancy. The τ_i ensure that our results will not be biased by time-invariant individual heterogeneity. As with the Baseline OLS, unbiased estimation requires the assumption that time-varying individual-specific test score determinants are not correlated with time relative to treatment.

Eq. 4 is the Value Added estimator. It is similar to the Fixed Effect estimator, but includes a control for prior-year scores rather than a student fixed effect. This will return unbiased estimates in the presence of time-varying student-specific heterogeneity under two assumptions.

First, the effects of all test score inputs – including lagged school construction treatments – must decay geometrically year to year. Second, contemporaneous investments must be orthogonal to the treatment dummies conditional on other controls. Prior research casts doubt on the validity of the geometric decay assumption (Todd and Wolpin (2007), Rothstein (2010)). Even so, we view this as a complement to the OLS and FE specifications because it includes a time-varying control for heterogeneity across students.

These three specifications differ in terms of sample selection, treatment definition, and method of controlling for student heterogeneity. If they yield similar effect estimates, and the timing of observed effects corresponds to the timing of school construction, we will interpret our findings as evidence of a causal effect of school construction on student scores. Note that the coefficients we estimate reflect the effect of school construction on test scores through increased educational production at school, at home, and in the neighborhood. Note also that we do not attempt to distinguish between the effects of having a new school this year on this year's score from the effects of having a new school last year on this year's score.

3.4. Neighborhoods with multiple projects

Eight out of 25 neighborhoods were home to separate school construction projects for their elementary and middle schools. To account for multiple construction projects in a single neighborhood, we modify our specifications in a way that allows us to recover the average effect of an individual construction project. We do this by summing over projects within a neighborhood, so that, e.g., our Baseline OLS specification becomes

$$T_{igzt} = \tau_{zg} + \tau_t + \sum_p \left(\sum_l \Delta_l D_{lzpt} \right) + X_i \beta + e_{igzt}. \tag{5}$$

Here, p indexes projects and takes a value of either one or two. The dummy variables D_{lzpt} are equal to one if year t is l years prior to occupancy of project p in neighborhood z.

4. Data

4.1. Home sales data

For our home sales analysis, we use administrative records of residential property sales that took place in the school district between January 1st, 1995 and January 31st, 2010. These records are maintained by the Office of the City Clerk. The data include sale prices as well as a variety of property and home characteristics. These characteristics include property address and acreage, home square footage, the number of bedrooms, bathrooms, and total rooms, and the 'style' of the property (e.g., 'Georgian,' or 'multi-family'). The data also include a subjective evaluation of each home made by the town tax assessor. These evaluations are categorical and range from 'poor' to 'excellent.' The assessor's evaluations have substantial explanatory power even after conditioning on observable home characteristics, and therefore can be interpreted as a measure of what would in most cases be deemed 'unobservable' home quality.

Table 2 summarizes the home sales data. Between the beginning of 1995 and January 2010, there were 14,266 residential properties sold in the district. The pace of sales was relatively slow between 1995 and 1999, during which time 2817 homes were sold, and picked up thereafter to a rate of over 5000 homes per five year period. We were able to match nearly all of the sales records to school zone-defined neighborhoods. Non-matches were due to incomplete address records in the sales data or omissions from the school assignment list. The average price of a home sold in the district (expressed throughout in real 2005 dollars) rose from \$120,301 between 1995 and 1999 to \$164,345 between 2000 and 2004 to \$245,909 between 2005 and 2010. This occurred even though characteristics of the transacted homes did not change very much: square footage, acreage, and number of rooms all remained relatively constant between 1995 and 2010. About 40% of homes sold in each period were deemed by the assessor to be of high quality, a constructed binary designation that includes good to excellent ratings. On average, transacted homes were located in neighborhoods that received just over one and a quarter new or renovated zoned schools.

Table 2
Fifteen years of district home sales.

	1995–2010	1995–1999	2000–2004	2005–2010
Number of homes sold	14,266	2817	5784	5665
Matched to schools	14,081	2772	5718	5591
Mean price ($1000s)	188	120	164	246
Median price ($1000s)	156	101	140	213
Square feet	1956	2026	1948	1929
Acreage	0.12	0.14	0.12	0.11
Bedrooms	3.64	3.60	3.62	3.67
Bathrooms	1.88	1.87	1.89	1.89
Rooms	7.98	7.92	7.98	8.01
High quality	0.39	0.45	0.38	0.37
N projects	1.26	1.27	1.26	1.26

Data describe home sales in New Haven over the 1995–2010 period. Sales are counted as matched to schools if we can locate the address on the map of school zones and assign it elementary, middle, and high schools. Prices are in 2005 dollars and rounded to nearest 1000. High quality is equal to one if a home is described as 'good,' 'above average,' or 'excellent' in assessor's records.

4.2. Student data

We use administrative student microdata to examine the impact of the SCP on residential choices and academic outcomes. For our residential choice analysis, we use data on the addresses of enrolled students for the academic years 2002–2003 through 2009–2010.[13] As with the home sales data, we map student addresses to zone-defined neighborhoods based on address. Descriptive statistics on neighborhood school enrollment levels and flows are available in Table A.3. The overall picture is one of a school district that is shrinking in size and in substantial residential flux, as students enter and leave the district and move within it.

For our analysis of academic outcomes, we use data for the academic years 2004 through 2010. Key variables include student race, English Language Learner (ELL) status, special education status, free or reduced-price lunch status, and student scores on state-mandated assessment tests (the Connecticut Mastery Test, or CMT), which we standardize using state-level means and standard deviations within grade-year cells.[14] Table 3 shows summary statistics for the students in our data. We have data on 152,151 student-years over the seven-year window, reflecting a district size of about 22,000 students.[15] Black students make up roughly half of all students, and Hispanic students account for another 35%. Because the proportion of free lunch students is so high, all district students receive free lunch at school. Each year, the district sends home a survey requesting income data so that they can renew district-level free lunch eligibility, and our data reflects the results of this survey. Generally about 75% of students report being free or reduced price eligible.

[13] We refer to academic years using the spring year from this point forward.

[14] We have access to state-level means and standard deviations for the years 2006 through 2010. For 2004 and 2005, we extrapolate means and standard deviations from later years using linear time trends within grade-subject-statistic cells. Observed means and standard deviations for NHPS students are consistent across years under this standardization.

[15] Comparing this statistic to enrollment data from Table A.1 in the Online Appendix, we see that District's internal counts of students exceed state-provided enrollment counts by about 10%.

Table 3
School district demographic profile.

	Total	Matched	In-district matched	FE sample	VA sample	BL sample
N	152,151	136,883	123,285	38,214	20,592	16,557
Black	0.52	0.52	0.53	0.51	0.51	0.49
Hisp.	0.35	0.35	0.36	0.38	0.39	0.41
ELL	0.11	0.12	0.13	0.12	0.11	0.14
Spec. ed.	0.11	0.1	0.1	0.07	0.06	0.07
F/R lunch	0.74	0.76	0.77	0.85	0.92	0.85
Reading	−0.66	−0.65	−0.69	−0.65	−0.63	−0.67
Math	−0.63	−0.62	−0.66	−0.59	−0.57	−0.61
PK-2	0.31	0.26	0.27	0	0	0
Gr. 3–8	0.41	0.45	0.46	1	1	1
Gr. 9–12	0.27	0.29	0.27	0	0	0
N projects	1.17	1.19	1.21	1.21	1.21	1.25

Characteristics of student population observed in microdata. Unit of observation is the student-year. 'Total' column includes all students in district. 'Matched' column includes student-years with matched addresses. 'In-district matched' includes student years with matched addresses for in-district students only (i.e., not students from neighboring towns). 'FE sample' column describes student-year obs. with current-year scores for students who are never enrolled in transitional schools, and have test scores less than three standard deviations above or below district mean. 'VA sample' column introduces lag-score requirement. 'BL sample' restricts FE sample to student-year observations within three years of occupancy of the reference project, as described in the text. Reading scores and math scores are standardized using state-level means and standard deviations.

Mean reading and math scores in the district were approximately two thirds of a student-level standard deviation below state means throughout the period. Eight of the 25 district neighborhoods had separate projects in their zoned elementary and middle schools; the student-weighted average number of projects was roughly 1.2.

When conducting our analysis of test scores, we restrict our student sample in a number of ways. Since treatments take place at the neighborhood level, we eliminate enrollment records that cannot be matched to addresses. As shown in the second column of Table 3 matched students tend to be older than the student body as a whole but are otherwise demographically indistinguishable. We also eliminate out-of-district students who enroll in district schools, because these students cannot be matched to neighborhood-level treatments.[16] The third column of Table 3 describes these students, who again resemble the broader student population.

To construct our analysis sample from the sample of in-district students with matched addresses, we make several further sample trims. We eliminate students who attend 'transitional' schools – schools specifically for struggling students – in any of our data years. We eliminate these students because transitional schools are not tied to specific school zones, and because we are interested in the effects of school construction on students in standard academic programs. We also drop student-year observations with test scores more than three standard deviations above or below the mean. The goal of this cut is to limit the impact of score outliers on our analysis. Relaxing these restrictions leads to results very similar to those presented here.

In our main analysis sample, used for fixed effect estimation, we include all remaining student-year observations with valid scores. This requirement eliminates students in non-tested grades: the CMT was administered in grades three through eight between 2006 and 2010, and in grades four, six, and eight prior to 2006. Students in other grades are dropped. This sample is described in the fourth column of Table 3. In our value added analysis, we include only students with non missing current- and prior-year scores. This sample is described in the fifth column of Table 3. The prior-year score requirement eliminates all students in academic years 2004 and 2005, third and fourth graders in 2006, and third graders between 2007 and 2010. Though requiring the presence of baseline scores reduces our sample size from 38,214 to 20,592, students in the value added sample do not differ substantially from students in the fixed effects sample in terms of their observable characteristics. We construct a Baseline OLS sample that includes only students who lived in a treated neighborhood one year prior to building occupancy. We do so by taking the FE sample, dropping all observations from student-neighborhood spells that do not span a baseline year, and also dropping all student-year observations that are more than three years before or after occupancy of the reference building. This restriction eliminates all data on projects completed prior to 2004, because we cannot identify baseline neighborhoods in years prior to 2003. It also eliminates, e.g., all post-2007 data on neighborhoods with projects completed in 2004, and all pre-2007 data for projects completed in 2010. Sample size falls from 38,214 in the FE specification to 16,557 in the BL sample, but students in the two samples are again similar in terms of their observable characteristics.

[16] These students enroll in District schools through regional school choice programs.

5. Results

5.1. Effects on home prices

Table 4 reports our estimates of four versions of Eq. 1. We report results for elementary school and middle school construction only, since high school assignment is generally not neighborhood based. The first two columns include year effects, seasonal effects, observable home covariates, neighborhood intercepts and slopes, and high school construction treatment variables as controls. Column I makes the restriction $\delta_f = \delta_c = 0$; i.e., home prices in affected neighborhoods are permitted to rise discontinuously only at the time of occupancy. The result is a binary difference-in-differences specification that yields an estimated 4.6% rise in home prices at the time of occupancy. This effect is significant at the 10% level. Column II allows for separate effects at the time of filing and the time of construction. In this specification, sale prices rise by 2.9% at the time of filing, 1.7% at construction start and a further 5.0% at the time of occupancy. The price changes at filing and construction start are not significantly different from zero, but we can reject the hypothesis that the change at occupancy is zero at the 5% level. The estimated total effect of construction – the sum of the score gains at each project phase – is 9.6%, and is also significantly different from zero at the 5% level.

In column III, we add controls for assessor-measured 'unobservables' to the regression. This causes our estimated effects to rise slightly: prices increase by 5.4% upon occupancy, and by 10.3% in total. The time-of-

Table 4
Elementary and middle school construction and home prices.

	I	II	III	IV
Filing (δ_f)		0.0285	0.0324	0.0288
		(0.0201)	(0.0203)	(0.0198)
Construction (δ_c)		0.0168	0.0161	0.0096
		(0.0121)	(0.0123)	(0.0200)
Occupancy (δ_o)	0.0455*	0.0503**	0.0543**	0.0555**
	(0.0235)	(0.0241)	(0.0231)	(0.0223)
Years post occupancy				−0.0060
				(0.0183)
Filing plus construction		0.0453	0.0485	0.0383
		(0.0285)	(0.0289)	(0.0349)
Total		0.0956**	0.1028**	0.0938*
		(0.0376)	(0.0371)	(0.0458)
Assessor quality	No	No	Yes	Yes
N	13,559	13,559	13,551	13,551

Results from a regression of log home sale price on time relative to filing, occupancy, and construction of neighborhood elementary and middle schools. All regressions control for year effects, season effects, house covariates, high school construction status, neighborhood dummies and slopes. Regressions control for assessor home quality as indicated. 'Years post occupancy' row reports coefficients on an interaction term between the post-occupancy dummy and the count of years post occupancy. The 'Filing plus construction' row reports $\delta_f + \delta_c$, and the 'Total' row reports $\delta_f + \delta_c + \delta_o$. HS construction treatment variables are included but not reported. Standard errors are clustered at the neighborhood level.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

filing coefficient rises slightly to 3.2%; the p-value associated with this estimate is 0.124. In column IV, we add an interaction term between the post-occupancy dummy, and years post occupancy. This allows the post-occupancy effect to deteriorate or increase over time. The estimated effect is small and statistically insignificant, and other estimates do not change much. It appears that school construction has a significant and large effect on home prices, and that our results are not driven by neighborhood-specific trends or by changes in the unobservable characteristics of homes sold, and that post-occupancy effects do not decay as time passes. A component of the total effect may be priced in at the time of filing, but the bulk of the price increase appears to coincide with occupancy of the new building.[17]

Our estimates of price effects are similar to the estimated treatment-on-the-treated effects of bond passage reported in CFR in terms of both levels and time paths. We find some evidence of small increases in home prices (on the order of 3%) after project filing, which occurs about six years prior to building occupancy. This is followed by larger gains at the time of occupancy. Estimates of home price effects in CFR grow from roughly 4% in the year following bond approval to between 7 and 10% six years after passage, by which time the spending effects of the bonds have faded. One way to think about the similarity between the two sets of estimates is as follows. Districts have a list of capital improvement projects they would like to undertake, with items ranked by the percentage change in home prices they would induce. Given that the per-pupil funding associated with bond approval in the CFR sample is about $6300, compared to about $70,000 in the New Haven SCP, it is likely that most marginal California districts did not fund projects as far down their lists as did New Haven. That the estimated home price effects are similar in the two environments suggests that the average project in New Haven has a price effect similar to the most desirable projects in the CFR sample. Given that the CFR study focuses on relatively high-income, high test-score districts (see Table II in CFR) and that school facilities in New Haven were in poor condition prior to the school construction project, this may not be surprising.

One concern about the specifications estimated here is that, although they correspond well to the revelation of information about construction projects, they do not give as clear a picture of pre- and post-construction trends as an event study analysis. We present a year-by-year event study in section C of the Online Appendix. Though the standard errors are large, the pattern of point estimates suggests that the more parsimonious filing/construction/occupancy specification is apt: we observe a price jump five years prior to occupancy, around the time of filing, a small rise in the year prior to occupancy, around the time of construction, and a larger and sustained increase beginning in the year of occupancy.

5.2. Effects on school enrollment

At least two distinct stories are consistent with our finding that school construction increases home prices. The first is that school infrastructure is a selling point for homeowners regardless of whether they have children eligible to enroll in a neighborhood school. For example, homeowners may value local amenities like swimming pools or playing fields. The second is that price increases are driven by the desire of homeowners to enroll their children in the rebuilt schools. These stories are not mutually exclusive, but have different implications for the effects of infrastructure investment on schooling demand and community demographics. In this section, we examine the relationship between schooling demand and the residency patterns for district students, and find support for the second story, though we cannot rule out the first using the data at hand.

Fig. 1 displays estimated enrollment effects computed using a regression of log public school enrollment by neighborhood on dummies for year relative to project completion, year fixed effects, neighborhood fixed effects, and neighborhood-specific trends. Coefficient estimates and standard errors are reported in Table 5. The coefficient on the treatment dummy is restricted to be zero in the year prior to building occupancy, and coefficients are restricted to be the same six or more years after building occupancy and six or more years before building occupancy. There is no observable pre-trend in the effect of per-capita construction expenditure. Enrollment effects begin to rise in the year of occupancy, and continue to do so through the end of our time

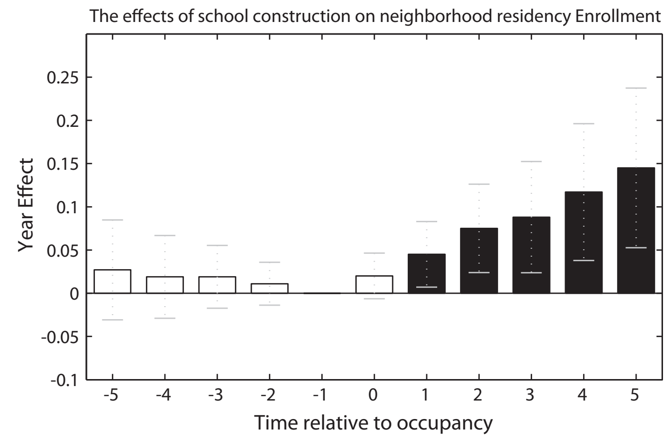

Fig. 1. The effects of school construction on neighborhood residency. The figure shows the effect of school construction treatment on log neighborhood enrollment by year relative to occupancy. See Table 5 . Shaded bars represent coefficients that differ from the effect in year −1 at the 10% level. Dashed lines show 90% confidence intervals. Standard errors are clustered at the neighborhood level.

Table 5
Selection on indices of observable test score determinants.

	Log enrollment	Reading index	Math index	Black	Hisp.	Male	F/R lunch
<−5	0.037	−0.001	0.009	−0.017	0.008	0.014	0.034
	(0.049)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.024)
−5	0.027	−0.006	−0.005	0.014	−0.008	0.014	−0.006
	(0.035)	(0.013)	(0.012)	(0.014)	(0.014)	(0.011)	(0.023)
−4	0.019	−0.007	−0.004	−0.003	0.011	−0.005	0.002
	(0.029)	(0.010)	(0.009)	(0.012)	(0.012)	(0.009)	(0.016)
−3	0.019	−0.003	0.001	−0.016*	0.020**	0.004	−0.022
	(0.022)	(0.007)	(0.006)	(0.009)	(0.009)	(0.010)	(0.024)
−2	0.011	−0.003	−0.003	0.013	−0.010	0.003	0.005
	(0.015)	(0.006)	(0.005)	(0.010)	(0.012)	(0.008)	(0.011)
−1	0	0	0	0	0	0	0
0	0.020	0.005	−0.002	0.002	−0.001	−0.001	−0.043
	(0.016)	(0.009)	(0.006)	(0.011)	(0.009)	(0.009)	(0.034)
1	0.045*	0.010	−0.001	0.009	−0.011	−0.014	−0.041
	(0.023)	(0.011)	(0.010)	(0.015)	(0.009)	(0.015)	(0.025)
2	0.075**	0.011	−0.003	0.006	−0.007	−0.017	−0.055
	(0.031)	(0.015)	(0.014)	(0.020)	(0.012)	(0.016)	(0.032)
3	0.088**	0.011	−0.010	0.002	0.005	−0.032	−0.088*
	(0.039)	(0.021)	(0.017)	(0.025)	(0.015)	(0.021)	(0.047)
4	0.117**	0.000	−0.020	0.003	0.016	−0.035	−0.083
	(0.048)	(0.026)	(0.021)	(0.030)	(0.023)	(0.023)	(0.050)
5	0.145**	0.002	−0.025	0.017	0.004	−0.037	−0.094
	(0.056)	(0.030)	(0.026)	(0.034)	(0.024)	(0.028)	(0.056)
>5	0.173**	0.004	−0.032	0.021	0.005	−0.049	−0.128
	(0.068)	(0.038)	(0.034)	(0.041)	(0.026)	(0.032)	(0.075)
N	196	38,190	38,989	38,190	38,190	38,214	38,214

Log enrollment results use enrollment-weighted data at the neighborhood-year level and include controls for neighborhood FEs, year FEs, and neighborhood-specific trends. Remaining columns show results from regressions of observable score determinants on year FEs, neighborhood/grade FEs and treatment indicators. Linear indices are based on race dummies, sex dummies, and free lunch status. Weights are determined by a regression of test scores on these variables. Standard errors clustered at the neighborhood level.
* Significant at the 10% level.
** Significant at 5% level.
*** Significant at 1% level.

[17] Note that the comparison between filing and occupancy effects is based on point estimates; we cannot in general reject the hypothesis that the two coefficients are equal at conventional levels.

window. The enrollment effects of school construction reach 17.3% six or more years post-construction.

There are three important things to note here. First, because these findings reflect changes in where students live, they cannot be a mechanical result of changes in school capacity, even if such changes had taken place.[18] Second, estimates reported in Table B.1 suggest that the increase in enrollment is the result of post-occupancy increases in student 'churn': both inflows and outflows increase post-construction, but inflow effects begin earlier than outflow effects. The standard errors associated with these estimates are large, however. Third, the large positive magnitude of estimated effects may be surprising given that the district decreased in size over the period studied here (see above). As we discuss in Section 5.4, the positive effects of school construction on neighborhood enrollment are at least partially offset at the district level by negative effects on enrollment in nearby neighborhoods.

Who are the new arrivals to neighborhoods with rebuilt schools? To answer this question we construct indices of observable test score determinants by regressing reading and math scores on race dummies, a sex dummy, and free lunch status and computing predicted test score values for each student. We do not include ELL and special education status in the indices because these outcomes may be endogenous to education quality. Table 5 reports results from a regression of math and reading score indices on dummies for time relative to construction, controlling for year and neighborhood/grade fixed effects. Though a small positive shift is observable in the reading score index post-occupancy, it is uniformly insignificant, and the math score index appears to fall slightly. Covariate-specific regressions in columns four through seven of Table 5 return results that are for the most part statistically insignificant. The point estimates provide some evidence of a shift towards female students and students who do not receive free or reduced price lunch following building occupancy. Overall, the impression here is that selection into neighborhoods following building occupancy is uncorrelated with observable determinants of test scores. Recall that our strategies for estimating test score effects are designed to deal with any selection on unobservable score determinants that may occur despite the lack of evidence of selection on observables.

The finding that neighborhood-specific school enrollment begins to rise at the time of school occupancy is consistent with the finding of rising home prices at that juncture. It suggests that migration rates are high enough or fixed costs low enough that readjustment in response to school construction is feasible; families willing to pay for school infrastructure move in, while families not willing to pay move out. One possible reason for this is that families with children benefit directly from any test score gains associated with construction, while other families do not. The next section assesses the size of the test score gains caused by school construction.

5.3. Effects on test scores

Table 6 presents results from estimates of Eqs. 2 (Baseline OLS), 3 (Fixed Effects), and 4 (Value Added) for reading and math scores. Recall that in each of these specifications we take steps to limit the effects of student selection into newly-built schools, either by restricting our sample to students present in affected neighborhoods before treatment (Baseline OLS), or by controlling directly for individual heterogeneity (VA and FE specifications). For each subject area, the first column presents the Baseline OLS specification, the second the Value Added specification, and the third column the Fixed Effect specification. We restrict effects to be zero in the year prior to building occupancy in all specifications. In the Baseline OLS specification, our sample is limited to student-year observations within three years on either side of the occupancy year for the reference project. Due to the difficulty of separately identifying year effects and treatment effects in the smaller Baseline OLS sample, we restrict treatment effects to be constant between one and three years after building occupancy. In the VA and FE samples we do not impose the three year restriction. We restrict effects to be the same six or more years prior to occupancy and six or more years after occupancy.

First consider reading scores. We find no evidence of pre-occupancy trends in any specification. In the Baseline OLS specification, estimated effects are slightly negative prior to occupancy, jump to positive values in the year immediately following occupancy, and reach 0.072 standard deviations for years one through three post-occupancy. This pooled effect is significant at the 5% level. The value-added and fixed effect specifications also show trend breaks at the occupancy date, rising nearly in parallel to 0.121 standard deviations (VA) and 0.153 standard deviations (FE) six or more years post-occupancy. Effects in the FE specification are significant at at least the 10% level in all post-occupancy years, while VA effects become significant at the 10% level four years after occupancy. In years one through three post-occupancy, estimated effects sizes in the VA and FE specifications range between 0.038 and 0.077 standard deviations, broadly consistent with evidence from the Baseline OLS specification. Fig. 2 plots the estimated year-by-year effects of school construction on reading scores in the fixed effects specification.

We interpret the consistent finding across specifications and the trend break in estimated effects at the time of building occupancy as strong evidence that school construction caused reading scores to rise in affected neighborhoods. That we observe this pattern even when

Table 6
Effect of school construction on test scores.

	Reading			Math		
	BL	VA	FE	BL	VA	FE
<−5		−0.021	−0.049*		−0.015	−0.036
		(0.031)	(0.024)		(0.049)	(0.039)
−5		−0.023	−0.027		−0.007	−0.006
		(0.033)	(0.019)		(0.028)	(0.027)
−4		−0.023	−0.028*		−0.027	−0.023
		(0.014)	(0.014)		(0.021)	(0.019)
−3	−0.002	0.008	−0.002	0.014	−0.007	−0.006
	(0.027)	(0.014)	(0.015)	(0.030)	(0.021)	(0.015)
−2	−0.015	−0.025	−0.019	−0.002	−0.019	−0.011
	(0.016)	(0.018)	(0.016)	(0.021)	(0.018)	(0.016)
−1	0	0	0	0	0	0
0	0.019	0.042	0.035**	0.025	0.041*	0.024
	(0.021)	(0.029)	(0.013)	(0.016)	(0.022)	(0.016)
1 (1–3 for BL)	0.072**	0.038	0.046**	0.012	−0.022	−0.027
	(0.026)	(0.033)	(0.020)	(0.027)	(0.030)	(0.022)
2		0.053	0.057**		0.005	−0.039
		(0.036)	(0.027)		(0.038)	(0.033)
3		0.075	0.077*		0.035	−0.014
		(0.052)	(0.039)		(0.050)	(0.043)
4		0.109*	0.108**		0.052	0.010
		(0.054)	(0.049)		(0.060)	(0.057)
5		0.108*	0.124**		0.043	0.021
		(0.060)	(0.059)		(0.069)	(0.067)
>5		0.121*	0.153**		0.055	0.031
		(0.069)	(0.061)		(0.086)	(0.074)
Observations	16,557	20,592	38,214	16,726	21,033	39,016
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Nbd/Grade FEs	Yes	Yes	Yes	Yes	Yes	Yes
Lag scores	No	Yes	No	No	Yes	No
Student FEs	No	No	Yes	No	No	Yes

The omitted category for year relative to treatment is year −1, the year immediately prior to building occupancy. Column 'BL' displays estimates of effects of school construction by year relative to treatment using treatment status in the baseline district, as shown in Eq. 2. Column 'FE' displays estimates of effects of school construction expenditure by year relative to treatment obtained using Eq. 3 in the FE analysis sample. Column 'VA' displays estimates of score gains obtained using Eq. 4 in the VA analysis sample. Controls include year FEs and neighborhood/grade FEs. Lagged scores and student FEs are included as indicated. Standard errors allow for clustering at the neighborhood level.

* Significant at the 10% level.
** Significant at 5% level.
*** Significant at 1% level.

[18] Recall from the discussion in Section 2 that there is no evidence to suggest that this occurred.

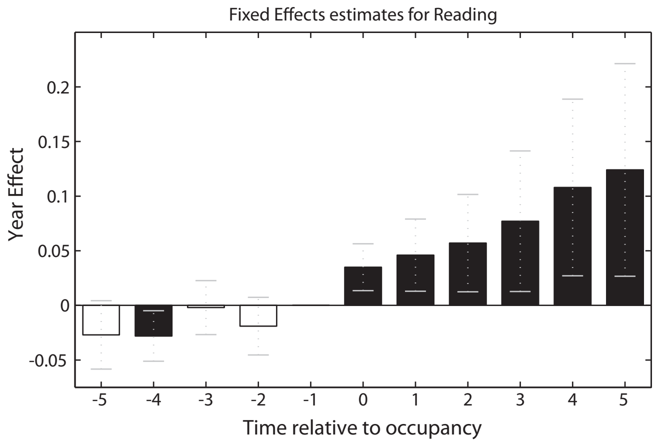

Fig. 2. Fixed effects estimates for reading. Y-axis shows estimates of the effects of school construction on student score levels by year relative to treatment, as described in Eq. 3, (FE estimation). Score results are measured in standard deviations. Shaded bars represent coefficients that differ from the effect in year −1 at the 10% level, allowing for clustered errors at the neighborhood level. Dashed lines show 90% confidence intervals. Controls include student fixed effects, year effects, and school-grade fixed effects. Estimates reported in Table 6.

controlling for individual-specific heterogeneity in multiple ways indicates that the estimated effects are appropriately viewed as the causal impact of school construction on the education production function, not as a consequence of selection into treated neighborhoods.

Now consider estimates of math score effects. As was the case for reading scores, we find little evidence of pre-occupancy effects. In the Baseline OLS specification, estimated effects are 0.025 and 0.012 in year zero and years one through three post-occupancy, respectively. Neither effect is significant at the 10% level. In the VA specification, we see a significant and positive year zero effect, followed by a steady increase to 0.055 standard deviations between years one and years six or more post-occupancy. After year zero, the estimated effects are insignificant. Estimates from the FE specification follow a similar pattern, with a positive year zero effect followed by a rise to a 0.031 standard deviation effect six or more years post-occupancy. Though these findings are consistent with the idea that school construction may have a small, positive effect on math scores, we cannot rule out the null hypothesis that school construction does not affect math scores at all.

Findings of heterogeneous effects across test subjects are common in evaluations of educational treatments. For instance, Angrist et al. (2010) and Abdulkadiroğlu et al. (2011) conduct lottery-based evaluations of the effects of attending high-performing charter schools and find much larger impacts on math than on reading scores. Dobbie and Fryer (2011) find that children attending a Harlem Children's Zone (HCZ) school also realize larger math gains than reading gains. The effect heterogeneity that we observe takes the opposite form (i.e., larger reading than math effects). This may be because the charter school intervention involves changes in teachers, peers, and curriculum, while the school construction intervention holds these inputs fixed while changing the characteristics of the physical plant.

In terms of magnitudes, the effects we observe are relatively similar to annual gains from the charter school literature. Angrist et al. (2010) report annual score gains of 0.12 standard deviations for reading and 0.35 standard deviations for math per year of enrollment. Dobbie and Fryer (2011) report math score gains of 0.2 standard deviations per year for HCZ students.

It is also useful to relate these results to changes in home prices and school enrollment by computing the implied sensitivity of home prices and school enrollment with respect to changes in test scores. If school construction only altered home prices through its effects on student test scores, we could reasonably compute the derivative of home prices with respect to school quality by dividing the percentage change in home prices post-construction by the change in test scores post-construction. Of course, school construction may affect home prices through other channels, like neighborhood aesthetics or access to public facilities. We conduct the exercise in spite of this limitation and interpret our results as upper bounds on the true effects. We further assume that long-term test score effects are immediately capitalized into home prices.

From Table 4, we know that school construction raised home prices by 10.3% on average. From Table 5, we know that the estimated effect on neighborhood enrollment counts six or more years post-occupancy was 17.3%. The estimated effect of school construction on reading scores six or more years post-occupancy in the fixed effects specification was 0.153 standard deviations. These values imply that a 0.1 standard deviation increase in a school's effect on reading scores would raise home prices by 6.7% and public school enrollment among neighborhood children by 11.3%. These estimates should not be compared directly to the elasticities presented in Black (1999) or Bayer et al. (2007), because both the numerator and denominator differ in critical ways. In the denominator, we use student-level test score standard deviations while Bayer et al. and Black use percent changes in school average scores. In the numerator, we use changes in the causal effect of schools on test score production, while they use school average scores which incorporate both school causal effects and student selection into schools.

5.4. Spillovers

Results from the previous three sections indicate that school construction raised home prices, school enrollment, and reading scores in treated neighborhoods relative to other neighborhoods in the District. One concern about these findings is that they might overstate the positive effects of school construction on the district as a whole if they are driven by negative cross-neighborhood spillover effects. For instance, it could be the case that buyers purchasing homes in one neighborhood following the completion of school construction there would otherwise have purchased homes in another District neighborhood, and that the observed positive effect in the neighborhoods with new schools masks a zero-sum price effect in the district as a whole. Similarly, if peer effects drive changes in educational production, then construction-driven movement of students from one neighborhood to another could have zero-sum effects in the district but a positive effect on treated neighborhoods.

We address these concerns using the intuition that spillover effects should be larger for pairs of neighborhoods that are close substitutes from the perspective of prospective homebuyers or students and their parents. That is, home price spillovers should be largest in neighborhoods where buyers otherwise would have bought homes, and peer effect-based test score spillovers should be largest in neighborhoods where students otherwise would have lived. Specifically, for each neighborhood in each year, we construct indices of construction project status in close-substitute neighborhoods. We then estimate the effect of construction in close-substitute neighborhoods on outcomes in the reference neighborhood. This exercise cannot rule out the presence of district-level spillovers that are uncorrelated with within-district sorting, but in our view it is difficult to come up with a story that predicts district-level spillovers but not within-district spillovers.

We consider three indices. Index 1 is equal to the number of completed projects in the neighborhood with elementary and middle schools located closest (on average) to those in the reference neighborhood. Index 2 is a weighted average of the number of completed projects in all other neighborhoods, with weights given by the inverse of squared neighborhood-to-neighborhood distance, normalized to sum to one. Index 3 is also a weighted average, but with weights given by observed transition probabilities from the reference neighborhood to other neighborhoods in the district, again normalized to sum to one. When estimating the effect of close substitute neighborhood construction indices on reference neighborhood outcomes, we use the same sets of difference-in-difference controls as in our analyses of reference neighborhood treatments above.

Results from this exercise are reported in Table 7. Panel 1 shows how mean values for the three indices rise in parallel over time, from zero in 1995 to nearly 1 in 2010. Panel 2 shows that changes in construction status for substitute neighborhoods do not reduce home prices in the reference neighborhood. Raising the value of index 2 in fact appears to increase reference neighborhood prices. Panel 3 shows that changes in construction status for substitute neighborhoods do reduce reference neighborhood school enrollment. This helps reconcile the large, positive enrollment effects shown in Fig. 2 with the overall decline in district size over the period in question (Table 1). A back of the envelope calculation based on estimates of reference and substitute neighborhood construction effects and changes in construction status over time suggests that between 2003 and 2010, own-neighborhood school construction raised district enrollment by about 6.8%, while substitute-neighborhood construction reduced enrollment by about 4.7%, for a net gain of 2.1% attributable to the construction program in the district as a whole.

The fourth and fifth panels of Table 7 show the test score effects of construction in substitute neighborhoods for reading and math scores, respectively. There is no evidence of negative spillover effects on reading scores: estimated spillovers are positive in six of nine specifications and generally statistically insignificant. We see some evidence of negative spillovers on math scores: estimated effects are negative in seven out of nine specifications and statistically significant at the 10% level in four out of nine.

This exercise leaves us with two key points. First, we do observe cross-neighborhood negative spillover effects on student enrollment, which helps reconcile our finding of large enrollment gains with broader district trends. Second, there is little evidence of negative spillover effects on home prices or reading scores. This suggests that our estimates of neighborhood-specific reading score and home price effects reflect gains at the district level and not zero-sum within-district shifts.

Table 7
Effects of school construction in other neighborhoods.

	Index 1	Index 2	Index 3
Means			
1995	0	0	0
2000	0	0.034	0.028
2005	0.364	0.544	0.536
2010	0.767	0.957	0.949
Regressions			
Home prices	0.015	0.081*	0.068
	(0.024)	(0.047)	(0.093)
Enrollment	−0.043***	−0.061**	−0.077
	(0.012)	(0.030)	(0.073)
Reading scores			
BL	0.023*	0.041	0.033
	(0.013)	(0.061)	(0.113)
VA	−0.010	−0.086*	−0.259
	(0.014)	(0.050)	(0.168)
FE	0.012	0.028	0.028
	(0.013)	(0.051)	(0.109)
Math scores			
BL	−0.029*	−0.158**	−0.194
	(0.017)	(0.070)	(0.129)
VA	0.005	−0.108*	−0.324*
	(0.020)	(0.059)	(0.185)
FE	0.001	−0.059	−0.170*
	(0.016)	(0.049)	(0.099)

Indices reflect occupancy status of projects in nearby neighborhoods. Index 1: single closest neighborhood. Index 2: Distance-weighted average over all neighborhoods. Index 3: Neighborhoods weighted by observed transition probabilities. SEs clustered and neighborhood level. All regressions control for neighborhood and year effects. Home prices and enrollment regressions also control for neighborhood specific slopes, as in our main specifications for those outcomes.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

6. Possible mechanisms

Having documented the test score gains that accompany the construction of new school buildings, it is natural to ask why this might occur. Thus far, we have remained agnostic about whether school construction affects test scores through the direct pedagogical effect of improved facilities (e.g., new science labs that permit a more sophisticated curriculum), through improved in-school motivation for students and teachers (e.g., teachers who develop better lesson plans because they are excited to teach in a room with natural light), or through raised rates of out-of-school educational investment (e.g., more emphasis on schoolwork from parents or peers). In practice, it may be difficult to distinguish between these pathways. If a student's access to a computer within the classroom encourages him to read news online when at home and this improves his reading score, it is unclear whether to attribute the gain to the specific feature of the facilities or to the change in home investment. Still, some pathways can be clearly categorized, and if one plays a dominant role it would be valuable to know this.

A related question with important implications for policymakers is *which* building features are associated with score gains. Even if the pedagogical impacts of a given feature could not be distinguished from the motivational effects, future construction programs might like to design buildings with features that have large total effects. Unfortunately, we do not have consistent data on the characteristics of the newly-constructed buildings, and therefore cannot examine the heterogeneity of construction effects across building features in a quantitative way.

In lieu of a quantitative analysis, we address the motivation versus pedagogy issue and the specific building features question using a survey of district principals. We surveyed principals at 22 district schools about their experiences before, during, and after school construction. We chose to interview school principals rather than teachers or students because we believe principals' experiences are likely to be the most representative of school climate as a whole. Of the 22 principals we surveyed, ten were in office at the time of school construction; we restrict our discussion to the responses of these ten individuals.

Our questionnaire asked principals to rate the contribution of the SCP to student, parent, and teacher motivation, and the timing of any observed changes. We also asked about the improvements they observed in different facility attributes, like libraries, classrooms, and ventilation, and about whether or how much they believed each improvement type contributed to academic performance. We then asked principals to weigh the relative importance of indirect motivational effects and direct 'new facilities' effects in improving students' scores. The survey questionnaire is presented in section D of the Online Appendix.

Principals agreed that the school construction project raised motivation at home and at school. All of the surveyed principals reported moderate to large effects of school construction on parent involvement, and nine of the ten reported large effects on student motivation. All principals reported moderate or large effects on teacher motivation. When asked to compare the importance of motivational effects to the importance of direct infrastructure effects for raising test scores, nine out of ten principals believed that the motivational effects of the SCP were at least as important as the direct effects of improved infrastructure on pedagogy. Though principals likely faced some of the same difficulties we do when trying to separate motivational from pedagogical effects, the surveys indicate that, at minimum, observed school construction effects are not entirely due to direct pedagogical changes. This is consistent with the emphasis placed on community, student, and teacher involvement in the construction process, and with a growing economic literature on the importance of intrinsic motivation in determining student outcomes (see, e.g., Heckman et al. (2006), Hastings et al. (2012)).

When asked to identify specific building features that were important to student success, principals pointed to library improvements and heating, air conditioning and ventilation. Particularly interesting in this part of the survey were the responses principals gave to an open-ended question in which they were asked to identify important pathways through which school construction affected student outcomes that had not been identified elsewhere on the survey. Several responses focused on ‘student and teacher pride,’ while others identified important but subtle building features, such as a glass wall which allowed teachers to observe student activities in hallways while standing in a central courtyard location. An implication is that some of the infrastructure features that determine student achievement may be relatively inexpensive, but difficult to measure or categorize. This presents both an opportunity and a challenge to designers of future infrastructure improvements.

7. Discussion

This paper describes the effects of a comprehensive school construction program in a poor urban district on student and community outcomes. We find that school construction had substantial positive effects on home prices in affected neighborhoods, and led to increases in the population of families with children attending public schools. These effects coincided with increases in student reading scores on the order of those experienced by students who attend high-performing charter schools for a year. Given the pressing need for large-scale investment in school infrastructure at the national level, and in poor, urban areas in particular, our findings are important for assessing the costs and benefits of potential infrastructure policies.

The evidence presented here also links prior work on the home price effects of school construction to a broader literature on the way that housing markets capture school quality. We document for the first time the way that dynamic changes in school quality (and other amenities associated with school construction) impact home prices and patterns of public school enrollment, and in doing so help explain how the social stratification along school boundaries described in Bayer et al. (2007) could arise over time due to local changes in education policy. We innovate further with respect to the housing market literature by focusing on the price effects of changes in the causal effect of schools on student scores, not on school average scores that mix differences in education production with student selection. This distinction is important if we wish to separate the value families place on school quality and the value they place on attending schools that students with high levels of observable and unobservable test score determinants also attend.

Our work has number of limitations. We cannot determine whether school construction affects test scores through specific changes to the built environment that enhance pedagogy, or through more generalized changes in student, parent, and teacher motivation that accompany the project both inside and outside of school. A survey of school principals suggests that both physical and motivational changes play an important role. A corollary is that we cannot identify specific building features that are particularly important for improved educational outcomes. Our attempts to compute the elasticities of home prices with respect to changes in school value added are hampered by the fact that we cannot determine the extent to which home buyers value other amenities associated with school construction. We therefore interpret the estimated elasticities as upper bounds on the true effects of test scores on prices.

We conclude with a broader discussion of the relationship between school construction and other policy interventions aimed at helping students in poor urban districts. The basic challenge in these districts is to help students from low-SES backgrounds succeed in school despite limits on local resources. Many current policies aim to help students who have the wherewithal to seek out educational opportunity leave troubled schools or districts. In at least some instances – notably a subset of high-achieving charter schools – students who win admissions lotteries realize large score gains. What is unclear is the extent to which these policies are scalable: straightforward models of economic behavior suggest that students who do not opt in to high-achieving charters would benefit less from attendance than those who do. Further, there may be negative spillovers from choice-based policies if the students who exercise choice no longer positively influence those who do not, though empirical evidence suggests that these effects are not large (Altonji et al. 2010).

School construction differs from choice-based policies because students do not have to opt in. With this in mind, the observed score gains may be even more impressive, because they are not limited to students who express an interest in improving their academic outcomes. The sticker price

Appendix A. Descriptive statistics

Table A.1
State of service systems in Connecticut schools: principals' survey.

Proportion less than good	2001	2009
Hartford	0.54	0.30
New Haven	0.53	0.14
Connecticut average	0.32	0.18

Percentage of school systems deemed ‘fair’ or ‘poor’ in a survey of public school principals. System categories are internal communications ,, interior lighting, technology infrastructure, exterior lighting, air conditioning, roadways and walks, heating, and plumbing/lavatories. School principals ranked each system ‘excellent’, ‘good’, ‘fair’, or ‘poor.’

Table A.2
School construction project summary.Source: NHPS.

Number of schools	Elem/MS	HS
Total schools	33	9
Planned	31	6
Constructed	27	6
Occupied	25	5
Expenditures (millions of 2005 dollars)		
Mean	34.0	50.1
Median	37.6	48.3
75th percentile	40.2	64.2
25th percentile	29.8	35.1
Duration (in years)		
Filing to occupancy	6.1	6.9
Construction to occupancy	1.7	2.6

Counts exclude transitional schools and schools that house only kindergarten or pre-kindergarten grades. Each school address is counted as a separate school.

Table A.3
Mean and standard deviation of school enrollment by neighborhood.

	Total	2004	2007	2010
Enrollment	755	827	712	693
	(383)	(398)	(383)	(382)
Inflows	218	242	209	199
	(113)	(116)	(116)	(109)
Inflows: inter.	128	144	114	122
	(66)	(67)	(61)	(68)
Inflows: intra.	90	98	96	77
	(53)	(53)	(58)	(46)
Outflows	234	239	272	199
	(121)	(124)	(126)	(104)
Outflows: inter.	143	141	173	122
	(75)	(70)	(80)	(66)
Outflows: intra.	91	98	100	77
	(53)	(61)	(55)	(43)
N	196	24	25	25

Student enrollment in district public schools by neighborhood-year. Within each row, the upper number is the variable mean and the lower number is the standard deviation. Inflows represent students new to a neighborhood between the current year and the previous year. Inter-district inflows represent students new to a neighborhood who were not enrolled in a district public school the previous year, while intra-district inflows represent students who moved from one district neighborhood to another. Inter- and intra-district outflows are defined analogously.

of school construction projects is much higher than the price of choice-based reforms in almost every case. But, given the poor state of infrastructure in poor urban districts, some school construction costs are fixed in the sense that they must surely be undertaken at some point in the relatively near future. At minimum, the results we present here indicate that when this construction occurs, it should be viewed not as an unfortunate necessity but as a part of the broader school reform toolkit.

Appendix B. Results — residential choices

This appendix presents estimates of the effects of school construction on neighborhood (log) public school enrollment, inflows, and outflows. We decompose inflows (outflows) into two categories: students entering (leaving) the district, and students entering (leaving) one neighborhood in the district for (from) another. Inflows count the number of students living in a neighborhood in a given year who were not enrolled in the district the previous year, or who were enrolled but lived in a different neighborhood. Outflows count the number of students who lived in a given neighborhood and were enrolled in a district school last year but this year either were not enrolled or remained enrolled but moved to a different neighborhood. Both inflows and outflows capture a wide variety of student movements, including district residents entering a public school for the first time, students arriving from out of town, students leaving town, and students graduating or dropping out. Each regression controls for neighborhood fixed effects, district-wide year effects, and neighborhood-specific slopes, and weights observations by neighborhood population.

Table B.1 presents results from these specifications. We find that school construction has large effects on enrollment. These reach 17.3% by six or more years following graduation. The increase in enrollment is the result of rising student 'churn': both inflows and outflows increase post-construction, but inflow effects begin earlier than outflow effects. The standard errors associated with these estimates are large.

Appendix C. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2014.08.002.

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Table B.1
School enrollment by neighborhood.

	Enrolled	Inflows	Outflows	New dist.	New nbd.	Leave dist.	Leave nbd.
<−5	0.0370	−0.193	−0.167	−0.101	−0.244	0.00713	−0.448
	(0.0489)	(0.167)	(0.177)	(0.204)	(0.319)	(0.202)	(0.358)
−5	0.0266	−0.0974	−0.146	−0.0326	−0.104	−0.0455	−0.278
	(0.0352)	(0.125)	(0.133)	(0.152)	(0.239)	(0.151)	(0.268)
−4	0.0193	−0.0834	−0.110	−0.0880	−0.0389	−0.0138	−0.283
	(0.0289)	(0.0997)	(0.106)	(0.121)	(0.190)	(0.121)	(0.214)
−3	0.0190	−0.00131	−0.0660	0.0141	0.0262	0.00826	−0.186
	(0.0220)	(0.0730)	(0.0773)	(0.0888)	(0.139)	(0.0883)	(0.156)
−2	0.0106	0.00717	−0.0210	0.0103	0.0674	0.00810	−0.0469
	(0.0153)	(0.0462)	(0.0489)	(0.0562)	(0.0880)	(0.0558)	(0.0990)
0	0.0204	0.0737	−0.00518	0.0721	0.120	−0.0136	0.0488
	(0.0159)	(0.0451)	(0.0478)	(0.0549)	(0.0860)	(0.0546)	(0.0967)
1	0.0452*	0.0683	0.0108	0.0416	0.172	−0.0354	0.113
	(0.0231)	(0.0712)	(0.0754)	(0.0866)	(0.136)	(0.0861)	(0.153)
2	0.0746**	0.107	0.0521	0.0932	0.150	−0.0433	0.214
	(0.0310)	(0.0967)	(0.102)	(0.118)	(0.184)	(0.117)	(0.207)
3	0.0885**	0.0863	0.151	0.0815	0.182	0.0464	0.338
	(0.0391)	(0.123)	(0.130)	(0.149)	(0.234)	(0.149)	(0.263)
4	0.117**	0.108	0.152	0.128	0.150	0.0519	0.328
	(0.0478)	(0.149)	(0.158)	(0.181)	(0.284)	(0.180)	(0.319)
5	0.145**	0.0706	0.161	0.0368	0.208	0.0497	0.380
	(0.0559)	(0.171)	(0.182)	(0.208)	(0.327)	(0.207)	(0.367)
>5	0.173**	0.0707	0.174	0.0929	0.0961	0.0849	0.358
	(0.0677)	(0.197)	(0.209)	(0.240)	(0.376)	(0.239)	(0.423)
Observations	196	171	168	170	169	168	168

Effects of school construction by year relative to treatment on neighborhood-level log enrollment flows. Observations are at the neighborhood by year level. Standard errors are clustered at the neighborhood level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

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