Measuring the Efficacy and Efficiency of School Facility Expenditures

Julien Lafortune & David Schönholzer*

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Abstract

We offer new evidence on the effects and efficiency of school facility investment on student and neighborhood outcomes, linking data on new facility openings to administrative student and real estate records in Los Angeles Unified School District (LAUSD). Since 1997, LAUSD has built and renovated hundreds of schools as a part of the largest public school construction program in US history. Using an event study design that exploits quasi-random variation in the timing of new facilities and a residential assignment instrument, we find strong positive impacts on math, English, and attendance. Effects are not driven by changes in class size, peers, teachers, or principals, but rather by increased facility quality and, to a lesser extent, reductions in overcrowding. House prices increase by 6% in neighborhoods that receive new schools. Using a residential choice model, we then estimate that a dollar spent on school facilities raises the sum of housing values and adult earnings by 1.62 dollars, with only 22% of this valuation due to academic benefits of the program. The housing market valuation of academic benefits captures most but not all of the implied future earnings gains.

*Lafortune (corresponding author): Public Policy Institute of California, 500 Washington Street, Suite 600, San Francisco, CA 94111 (email: lafortune@ppic.org). Schönholzer: Institute for International Economic Studies, Stockholm University (email: david.schonholzer@iies.su.se). We thank, in particular, Jesse Rothstein, David Card, and Patrick Kline for invaluable guidance and support on this project. We thank Jack Bragg for outstanding research assistance. We also thank Bruce Fuller and Jeff Vincent for many helpful conversations and data assistance. This work has benefited from helpful comments and conversations with Steven English, Hilary Hoynes, Tomas Monnarez, Waldo Ojeda, Tom Rubin, Emmanuel Saez, Raffaele Saggio, Jon Schellenberg, Chris Walters, Danny Yagan, and seminar participants at UC Berkeley, UCSB, Yale, PPIC, ASSA, SOLE, AEFP, Michigan, and UCSC. We are also grateful to the Los Angeles Unified School District for data access and support. This work was supported by a National Academy of Education/Spencer Foundation Dissertation Fellowship. All findings and conclusions are solely those of the authors and do not necessarily represent the views of the Los Angeles Unified School District or any other institution.

1 Introduction

Investing in public infrastructure, including around 84,000 public school facilities, is a primary responsibility of federal, state, and local governments in the United States. Each year, around fifty billion dollars is spent on constructing and renovating these facilities, making up a significant share of public expenditures on K-12 education (McFarland et al., 2017). Despite the magnitude of this spending, many facilities have fallen into disrepair, with millions of students attending schools in poor conditions (Alexander and Lewis, 2014). In light of these conditions, policymakers and analysts have repeatedly called for immediate action to increase investments into school facilities, with estimates of required funding ranging in the hundreds of billions nationally (Crampton et al., 2001; Arsen and Davis, 2006; Filardo, 2016).

There is substantial disagreement in economics over the need for these investments. The debate revolves around two questions. First, are facility investments an effective tool to improve student outcomes? And second, are they an efficient use of public funds? A growing literature studies the achievement effects of facility investments, but so far has failed to arrive at a consensus on whether students attending higher-quality facilities are better off. This is largely due to the difficulty in finding convincing research designs and large policy changes (Jackson, 2018), which has bolstered the view that resource effects may be negligible (Hanushek, 1997).

Similarly, evidence on the efficiency of these investments is scarce, not least because of the disagreement over whether better facilities improve human capital and later life earnings. Even if these benefits were known, it is unclear whether they are fully capitalized in housing markets of affected neighborhoods. As a consequence, aggregating benefits from student achievement and real estate capitalization of education infrastructure programs remains challenging.

We contribute new evidence on these questions in the context of the largest school construction program in U.S. history. Like many other districts, Los Angeles Unified (LAUSD) saw declining capital investment throughout the 1970s and 80s, hovering around the median in terms of per-pupil real capital expenditures. However, after a series of voter-approved bonds starting in 1997, LAUSD was at the forefront of districts investing in school infrastructure, constructing nearly 150 new schools by 2012 (Figure 1, panel a). As a result, the average LAUSD facility age fell from 57 years to 53 years, moving from the 80th percentile closer to the median of the national distribution (Figure 1, panel b). While LAUSD facility quality improved considerably, about one in five schools are in inadequate condition nationwide (National Center for Education Statistics, 2014).

We use administrative records on more than two million students to provide precise and comprehensive estimates of the causal impact and the underlying mechanisms of these expenditures on student outcomes. We then combine the implied gains in later life earnings with changes in house prices in affected neighborhoods to evaluate the efficiency of the program in a way that avoids double-counting benefits. To this end, we develop a residential sorting model that allows for program benefits to arise from both academic achievement and valuation of non-academic aspects of new schools.

We find robust evidence that students attending newly constructed schools in LAUSD experience large, significant gains in cognitive and non-cognitive outcomes. Relying on within-student variation in the timing of exposure to new facilities, we estimate that spending four years in a new school facility leads to a 0.1 standard deviation increase in standardized math scores and a 0.05 standard deviation increase in English-language arts (ELA) scores. In addition, students who attend newly constructed schools attend on average four additional days per academic year, and score slightly higher on teacher-reported measures of student effort. Results are nearly identical when using exogenous residential assignment due to the creation of school attendance zones of new schools as an instrument for the timing of attending a new school.

Examining the mechanisms through which these effects are mediated, we conclude that the majority of the effects were driven by improved facility quality. We also find some evidence that reduced overcrowding contributed to these positive effects: students at existing facilities, who experienced reductions in overcrowding but no facility improvements, saw some test score and attendance gains. We find no evidence that student sorting, changes in teacher quality, principal quality, peer quality, or changes in teacher-pupil ratios were positive contributing factors.

We establish that the benefits generated by the program in terms of housing market capitalization and later life earnings gains substantially outweighed its cost, suggesting it increased welfare but remained below efficient scale. We arrive at this result by developing a model in the spirit of Tiebout (1956), Brueckner (1979) and Barrow and Rouse (2004) that clarifies the relationship between neighborhood investment in school amenities, household valuation of school amenities in the real estate market, and future earnings valuation of academic achievement. Using this model, we derive a hybrid expression for the marginal value of public funds (Hendren, 2016), combining the housing capitalization approach and the later life earnings approach commonly found in the literature. The model identifies the share of valuation due to academic achievement as a key parameter, which we estimate via the covariance of achievement effects and house price effects across neighborhoods receiving new schools. We show that this parameter is also closely related to the literature on housing valuation of school performance surveyed by Black and Machin (2011).

We estimate the model using administrative records on property sales, finding that house prices increase by 6% in neighborhoods that receive new school facilities. Using only the benefits implied by these house price effects, we arrive at a total gross benefit of more than $14 billion against a program cost of $9 billion, or a marginal value of public funds of 1.53. Relying on the relationship between test scores and future earnings (Chetty et al., 2011), we estimate that the achievement gains induced by the program increase future earnings by roughly $4 billion. We then estimate that around 22 cents out of each dollar capitalized in the housing market is due to achievement, which accounts for about 76% of the estimated gains in later life earnings. Using a hybrid approach that additionally includes future earnings gains that are not capitalized in the real estate market yields a marginal value of public funds of around 1.62. This is only slightly larger than only using real estate capitalization, but substantially larger than if we were to only consider gains in future earnings.

Our study contributes new evidence to several related literatures. First, we provide robust estimates of student-level effects from facility improvements using variation induced by the largest school capital construction program in the United States. Most prior studies examine effects of capital expenditure programs on district-level average outcomes, often finding mixed and imprecise estimates of effects on student outcomes (Cellini et al., 2010; Martorell et al., 2016; Hong and Zimmer, 2016; Conlin and Thompson, 2017; Goncalves, 2015). These studies do not measure effects on directly treated students, and are generally underpowered to detect modest but meaningful effects. A notable exception is the work by Neilson and Zimmerman (2014), who examine a smaller-scale construction boom in New Haven, Connecticut, and find evidence of positive effects on reading but not math scores several years after school construction.[1] We build on this prior work by leveraging the scale of the entire LAUSD school construction program, allowing us to decompose effects and examine specific mechanisms.

Second, we contribute to the literature estimating the capitalization of school quality in the real estate market. We provide some of the first large-sample evidence of localized house price capitalization of dynamic changes in school quality. Much of the work in this literature has estimated the capitalization of *static differences* in school quality, and thus does not provide direct estimates of how *changes* in school quality are valued in the real estate market.[2] A handful of recent papers provide estimates of capitalization of changes in school quality using variation induced by capital expenditure policies, generally finding positive effects after several years (see Cellini et al., 2010; Goncalves, 2015; Conlin and Thompson, 2017; Neilson and Zimmerman, 2014). We build upon these prior studies by more precisely examining the dynamics of these changes, over both time and space. Moreover, we study a (mostly) locally funded program that was inherently redistributive: local property taxes were raised districtwide to fund new schools in only one-third of neighborhoods.

Finally, we contribute to the broad literature and debate over the efficacy and efficiency of resource-based education policies. Economists have long been skeptical of the productivity of such investments (e.g. Hanushek, 1997), although recent studies of state-level school finance reforms have provided evidence that broad based expenditure programs can improve educational outcomes (e.g. Jackson, 2018; Jackson et al., 2016; Lafortune et al., 2018; Candelaria and Shores, 2015; Hyman, Forthcoming), labor market outcomes (Jackson et al., 2016), and intergenerational mobility (Biasi, 2017). Our study of the LAUSD school construction program provides additional evidence that: (1) school expenditures – even those dedicated to capital costs – can improve student cognitive and non-cognitive outcomes; (2) such programs can induce increases in aggregate real estate prices in excess of program cost; and (3) most of these increases reflect non-academic benefits of the

[1] Another notable exception is Hashim et al. (2018) who focus on subset of new school openings in LAUSD in 2010/2011. They study the effects of the two initial cohorts of "strategic" new school openings. These openings were a subset of the schools constructed after 2010 as a part of the district's Public School Choice Initiative.

[2] Several papers, most notably Black (1999) and Bayer et al. (2007) exploit boundary discontinuities within narrowly defined neighborhoods to estimate the market valuation of school quality. Other papers have used variation across district boundaries (e.g. Barrow, 2002; Barrow and Rouse, 2004), within-district boundary changes (e.g. Ries and Somerville, 2010; Collins and Kaplan, 2017), school "report-card" grades (Figlio and Lucas, 2004), and public reporting of teacher value-added scores (Imberman and Lovenheim, 2016). For a comprehensive overview, see the review of the capitalization literature in Black and Machin (2011).

program, although much of the academic benefits are also capitalized in the housing market.

There are two important caveats to these conclusions. First, as our study focuses on the outcomes of one large district investing heavily in facilities, our results may not generalize to all other districts in the country. However, many large, urban districts as well as smaller districts serving disadvantaged students face consistently underfunded and low-quality facilities (e.g. Filardo et al. (2006), Alexander and Lewis (2014)). While its program was large, LAUSD was not the only large district investing heavily in facilities after decades of neglect (Figure A1, panel a). Its experience may be broadly representative of many other large urban districts that undertook major investments over this period or consider doing so in the future.

Second, an important feature of the LAUSD program was the reduction of overcrowding and the expansion of available school facilities. Most of the bond funds invested in LAUSD in the period we study took the form of new school openings, expanding the seat capacity of schools, as opposed to major renovations of existing facilities. But many aspects of pupil experience at new schools resemble large renovation projects: students switch to modern, spacious facilities in mostly the same neighborhoods as before with similar teachers and staff, as they would in renovation projects. New school openings also capture many aspects of school capital expenditures more broadly, as they typically involve not only new classrooms but also administrative buildings, sports facilities, land acquisitions, and equipment purchases.[3]

The paper proceeds as follows. In Section 2 we detail the context for our study and discuss specific details of the LAUSD program. In Section 3 we examine the effects of the new facilities on student outcomes, first detailing the data and empirical results, and then examining student effects and mechanisms. In we Section 4 present the data, empirical approach, and results for neighborhood house price effects. Section 5 presents the residential choice model, linking the results in the previous section to interpret the valuation and efficiency of the program. Finally, in Section 6, we conclude with a brief summary of results and their implications.

[3]Additionally, overcrowded school facilities are not unique to LAUSD; over 25% of California public schools were recently designated as overcrowded (Rogers et al., 2009), and thus our results are relevant to many school districts facing similar constraints.

2 Context: School Facility Investment in LAUSD

LAUSD is the second largest school district in the United States, serving 747,009 students at its peak in the 2003-2004 school year. Like many large urban school district in the US, it is majority-minority, and serves students who are much more disadvantaged than the typical US public school student. Relative to the rest of California, students in LAUSD are underachieving.[4] As of the early 2000s, LAUSD's capital stock had fallen well below current needs. As shown in Figure 2, no new schools were opened between 1975 and 1996, and the average student attended a school that was around 60 years old in 2000. Many were in extremely poor condition. Classrooms were often non-functional, with broken and missing equipment, and school facilities sometimes lacked adequate restrooms.[5] Inadequate climate control was additionally a major source of distraction; classroom temperatures upwards of 90 degrees fahrenheit were not uncommon.

The schools were also severely overcrowded, as the district's enrollment had increased roughly 10% since 1975 (Figure 2). Nearly 25,000 students were bused daily to faraway schools to relieve capacity constraints, and roughly half of students attended schools that operated on "multi-track" calendars that staggered the school year to use the facility year-round and thereby accommodate as many as 50% more students than could be served at any single time. Even with these measures, many schools relied on lower-quality portable classrooms, and even converted gymnasiums, libraries, and computer labs into classroom space. This also severely limited access to extra-curricular opportunities. Rapid depreciation of facilities due to continued overuse compounded these issues.

Between 1997 and 2008, voters in Los Angeles approved a series of bonds dedicating around $20 billion in local and state funding to the construction, expansion, and renovation of hundreds of schools. This was the largest public infrastructure program in the U.S. since the interstate highway

[4] In 2002 the average student scored roughly 28% of a standard deviation below the state mean in English-Language Arts (ELA) and roughly 21% of a standard deviation below the state average in math. Scores from the CST ELA exam in grades 2-11, and the CST math exam in grades 2-7.

[5] In a 1999 review of the facilities practices of LAUSD and other California districts, the California "Little Hoover Commission", an independent oversight body, reprimanded the district for gross mismanagement and noted in particular that LAUSD school facilities were "overcrowded, uninspiring and unhealthy", and that "Researchers have attempted to gauge the link between the quality of school buildings and the quality of learning. In Los Angeles, however, this link is obvious. In some classrooms, there are twice as many children as there are desks" (Terzian, 1999). One high school of nearly 2000 students had only one functioning bathroom. One teacher noted that "... we had roaches, ants, an air conditioner that barely worked, no sink [...] and barely any storage for classroom materials" (Fuller et al., 2009).

system (Fuller et al., 2009). While the program was unprecedented in absolute dollars, it was less remarkable in per-pupil terms: the program pushed LAUSD from just above the national median in per-pupil capital spending, to nearly the 90th percentile at its peak in 2007 (Figure 2). The per-pupil construction cost was also similar to new school construction programs in many other large districts (Figure A1, panel a) and smaller urban districts across the country (e.g. Neilson and Zimmerman (2014), see Figure A2).

As a result of these investments, facility quality improved considerably in LAUSD. This is in line with a broader trend across the country of shifting resources to poorer districts: after decades of neglect, districts with low capital investment during the 70s and 80s caught up considerably during the 2000s, nearly equalizing capital expenditures across districts (Figure A1, panel b). Despite this increased investment, between 1999 and 2012 the average facility age nationally increased from 40 to 43 years; for low-income urban districts the average age increased from 45 to 50 years (Greene, 2003; National Center for Education Statistics, 2014).

The first new school was completed in 2002, and over the next decade 144 new school facilities were constructed in LAUSD as part of the facilities program. Nearly \$11 billion was spent over this period, about 86% of which went to new school openings, while the rest went to additions, renovations, and equipment delivery at existing schools.[6] By 2012, over 75,000 students attended a newly constructed school (see Figure A4).[7] To identify suitable sites for new schools, designated search areas were defined near the most overcrowded schools, and construction sites were selected

[6]In total, the projects we study in our data cost \$9.17 billion (roughly \$6,000 per household or \$15,000 per pupil), the majority being funded from the various local bonds that were passed in and after 1997. We focus on new school facilities completed between 2002 and 2012, for which we have detailed project data matched to administrative student data. A database of capital projects in LAUSD, including measures of project cost, size, completion timeline, and location, was constructed from records listed publicly by the LAUSD Facilities Services Division (FSD). The data cover all major projects and new school constructions with a preferred site designated between 1997 and 2011 (Projects not yet constructed by the end of 2011, but that were already in the planning phase, are included) and include over 500 capital projects totaling nearly \$11 billion in planned or realized spending. We restrict attention only to large new school construction projects, defined as those that created over 100 new seats and/or cost at least \$10 million. We do not examine effects for the small number of projects for school campuses that already existed in the first year of the student sample (e.g. major additions). These restrictions exclude roughly 14% of the spending in our database.

[7]By 2012, less than 1% of students remained on a multi-track calendar (see Figure A5), overcrowding had been effectively eliminated, and there was very little busing of students to distant schools. This period was marked primarily by an increase in capital and not instructional expenditures. Districtwide, per-pupil instructional expenditures increased by roughly 15% in the early 2000s, but fell following the Great Recession. Capital expenditures per-pupil increased nearly 400% in the same period. Other LA county districts saw similar changes in instructional but not capital expenditures (Figure A3).

from within these areas primarily based on site feasibility (e.g. size, location, accessibility), cost of acquiring land, environmental concerns, and local community engagement.

By 2001, nearly all new school sites had been identified, although the process of acquiring land, securing adequate funding, negotiating with local stakeholders, meeting environmental regulations, and designing and constructing schools resulted in a staggered delivery of new facilities over the next decade. It is this plausibly random variation in the timing of openings, induced primarily through idiosyncrasies in the construction process, which we exploit to estimate the effect of new schools on student and neighborhood outcomes. Summary statistics for the new school projects are presented in Table 1. In total, there were 144 new schools built as a part of 114 new school campuses.[8] The median project cost $57 million and created about 800 new student seats, with several projects costing in the hundreds of millions of dollars. Projects typically took two years to construct, and were complete roughly 5 years after the site had been designated by the district. Figure 3 shows the attendance zones for new and existing school facilities in 2012.[9]

New schools were filled quickly, typically reaching close to steady state enrollment within 2 years after construction. Students from nearby schools were reassigned based on redrawn school assignment zones to the newly constructed schools. Switching students experienced drastic changes in facility quality: they switched from schools that were on average 70 years old and had substantial physical deficiencies. New facilities enabled the district to reduce overcrowding and eliminate multi-track calendar schedules at both new and nearby existing schools.

3 Student Impacts

3.1 Student data

To study the effects of improved school facilities on student outcomes, we use administrative records from LAUSD from the 2002-2003 school year to the 2012-2013 school year. Every student who attended LAUSD during this time period is included, and the data allow for longitudinal links

[8] In some cases, a new school campus comprised several new schools, either because the site was combined to house both elementary and middle (or middle and high school students), or because magnet or alternative schools serving the same grades were housed on the same campus.

[9] As can be seen in the figure, new schools were concentrated in East Los Angeles, where students are predominantly low-income and Hispanic/Latino and schools were previously the most overcrowded and in need of repair.

across years for students who remain in the district.[10] To ensure comparability of scores across students, we focus only on California Standards Test (CST) math scores for grades 2-7 and CST ELA scores for grades 2-11.[11] Test scores are normalized relative to the California-wide mean and standard deviation.[12] Total annual attendance, measured in days, is recorded for each student. For elementary school students, report card data contain teacher-reported measures of both achievement and effort in different classroom subjects. These are reported on an ordinal scale from 1 to 4 for several subjects.[13]

Data on teacher education, experience, age, and gender are available in all years, except 2009 and 2011. Teacher identifiers are also available for all years in the student data, and teachers can be linked longitudinally using unique teacher IDs. In the student data, each elementary record contains a single teacher identifier. Teacher-student links for secondary school are constructed using student-level course data.[14] Class size is constructed for elementary school students by measuring the total number of students associated with a particular teacher ID in a given year. For students in secondary school we do not compute class size as direct classroom identifiers are unavailable.

Summary statistics for students are presented in Table 2. Column 1 shows the average demographic characteristics for all student-year observations in the sample. Column 2 reports means for students who never attend a newly constructed school during the sample period (i.e. "never treated"). Column 3 reports means for "always treated" students, that is, those whose first year in the data sample is at a newly constructed school. In practice, these are almost always kindergarten

[10] These data provide one record per student-year with information on grades, test scores, demographics, attendance, addresses, residential assignments, and teacher assignments. For some years and grade levels, data are included from both the fall and spring semesters; we collapse these data to the annual level for comparability. Demographics include gender, race, language spoken at home, parental education, and eligibility for free or reduced price lunch.

[11] In each of grades 2-7, students take the same grade-level math exam; however, beginning in grade 8 the particular test depends on the student's particular math course enrollment. For the CST ELA exam, exams do not depend on a student's enrollment. Some students with limited English proficiency and/or individual education programs take alternative exams. These students are excluded from all test score analyses.

[12] Means and standard deviations are reported in the California Standardized Testing and Reporting (STAR) documentation provided by the California Department of Education.

[13] Scores pertaining to student effort are averaged within each student-year record to construct a "effort" index. Scores pertaining to student achievement or proficiency are averaged within each student-year record to construct a teacher-reported "marks" index. These indices are then normalized to have mean zero and a standard deviation of one within each grade-year cell.

[14] Principal names are available for 85% of student-year observations, allowing us to construct within-district measures of principal experience. Principal names are available for all but two years of our data. For years with missing names, we assign a school its principal from the prior year or following year (giving preference to the prior year where there are conflicts).

students, although this also includes students who show up in LAUSD for the first time in other grades. Columns 4 and 5 show means for switchers and "stayers", respectively. The former are students who switch to a newly constructed school at some point during the sample period, while the latter are defined as students at schools where more than 10% of grade-year cohort switches to a newly constructed school in the following year.

Over 85% of students in LAUSD are black or Hispanic, and most students speak a language other than English at home with their parents. Students in LAUSD are also much more socioeconomically disadvantaged than the typical California school district: over three-quarters of students are eligible for free and reduced price lunch and do not have a parent who attended any level of postsecondary education. Importantly, treated students who attend newly constructed schools are even more likely to be black or hispanic, low-income, and speak a language other than English at home. Comparing students who switch to new schools against their peers who stay behind at old schools, the same pattern of selection emerges: student switchers are slightly more likely to be low income and score more than 10% of a standard deviation lower in both math and ELA than those students who stay behind at old schools. This pattern was a deliberate feature of the construction program: new school facilities were targeted toward neighborhoods with the most overcrowded and depreciated schools, and these school zones were overwhelming located in the most underprivileged areas of the district. Comparing the stayers and switchers shows that even within disadvantaged neighborhoods, new schools were located in areas with slightly lower performing and more disadvantaged students.

3.2 Econometric Design

3.2.1 Generalized difference-in-differences

We estimate the effect of attending a newly constructed school on student outcomes using a generalized difference-in-differences strategy that relies on variation in the year a student begins at a new facility. To deal with the concern that attending a new facility may be driven by selection, we rely only on *within-student* changes in outcomes over time, controlling for student fixed-effects to eliminate any biases due to time-invariant differences between students who matriculate at different schools. The key identification assumption is that the timing of student switching to newly constructed school facilities is as good as random, after accounting for fixed differences between students, grades, and years. This leads to a flexible event-study specification that allows for differential effects of attending a new school for each year a student outcome is observed:

$$y_{it} = \alpha_i + \gamma_{g(i,t)} + \delta_{t,d(i)} + \sum_{k=\underline{K}}^{\overline{K}} \beta_k \mathbb{1}(t = t_i^* + k) + \epsilon_{it} \tag{1}$$

for an outcome y_{it}, for student i in year t and grade $g(i,t)$. We include fixed effects for student (α_i), grade ($\gamma_{g(i,t)}$), and year-by-local district ($\delta_{t,d(i)}$).[15] Here, the coefficient β_k captures the effect of attending a newly constructed facility k years after the first year t_i^* a student attends a new school. k is zero in a student's first year attending a school, and thus β_k estimates the effect of $k+1$ years of exposure to a new facility. Effects are measured relative to year $k=-1$, which is excluded in estimation. Endpoints are binned at $\underline{K}=-3$ and $\overline{K}=3$,[16] which represent the average of student outcome y_{it} three or more years prior to attending a new school, or after four or more years of exposure to a new facility, respectively. Standard errors are two-way clustered by both school and student, to account for any serial correlation within school and/or within student outcomes over time. This design builds in placebo tests that identify violations of the identification assumption that the timing of student switching is as good a random: for $k<0$, nonzero coefficients would be an indication of non-randomness in the timing of student switching.

Equation (1) estimates the effects of attending a new school separately by year. Following Lafortune et al. (2018), we can approximate the dynamics of these effects by estimating a more parametric version of (1) where we allow for a new school to have an immediate effect, and for effects to phase in gradually over time. Imposing linearity in the growth rate of student outcomes and defining $\tilde{t_i} \equiv t - t_i^*$, we can estimate the following generalized difference-in-differences specification:

$$y_{it} = \alpha_i + \gamma_{g(i,t)} + \delta_{t,d(i)} + \beta_1 \mathbb{1}(\tilde{t_i} \geq 0) + \beta_2 \mathbb{1}(\tilde{t_i} \geq 0) * \tilde{t_i} + \beta_3 \tilde{t_i} + \epsilon_{it} \tag{2}$$

Here β_1 captures the immediate effect of a new school facility in the first year t_i^* a student attends

[15] LAUSD is comprised of six administrative local districts: Northwest, Northeast, West, Central, East, and South. We include separate year effects for each local district to more flexibly account for regional shocks and trends.

[16] We choose $\overline{K}=3$ as few students attend a new school facility for more than 4 years in the data.

a new school. β_2 reflects effects of the new school that accrue gradually over the time a student is exposed to a new school.[17] As a student is repeatedly exposed to improved facilities in each year she attends a new school, we would expect effects to cumulate and increase over time with continued exposure: $\beta_2 > 0$. We also include a linear trend in "event time", $\tilde{t_i}$, to test for any selection on prior trends. β_3 captures this selection, and also provides a useful placebo test of the assumption that the timing of student switching is as good a random.

3.2.2 Instrumental variables

While the majority of students attend their residentially assigned school, nearly 25% do not. This share is lowest for elementary school students, and greater for middle and high school students, as there are additional alternative school options (e.g. magnet schools) in later grades. If residential non-compliance is correlated with time-invariant student characteristics, then the student fixed effects models in equations (1) and (2) will still recover unbiased estimates of the new school effects. However, if student sorting into new schools outside of their residential assignment zone is correlated with *changes* in outcomes (e.g. Roy selection), treatment effect estimates in equations (1) and (2) may be biased.

To account for this potential source of bias, we estimate instrumental variables versions of (2) via two stage least squares (2SLS), where we instrument for the new school effects $\mathbb{1}(\tilde{t_i} \geq 0)$ and $\mathbb{1}(\tilde{t_i} \geq 0) * \tilde{t_i}$ using the residential school assignment based on students' home addresses. Specifically, we instrument for a student's matriculation at a new school, $\mathbb{1}(\tilde{t_i} \geq 0)$, with $\mathbb{1}(\hat{t_i} \geq 0)$ (where $\hat{t_i} \equiv t - \hat{t_i^*}$ is year relative the first year a student was assigned to a newly constructed school, $\hat{t_i^*}$), an indicator for whether a student was assigned to attend a new school given her home residence. Analogously, we instrument the linear phase-in, $\mathbb{1}(\tilde{t_i} \geq 0) * \tilde{t_i}$, with $\mathbb{1}(\hat{t_i} \geq 0) * \hat{t_i}$, the number of years since a student's residential assignment switched to a newly constructed school.

To the extent that families systematically sort between neighborhoods in anticipation of new school openings, 2SLS estimates from (2) may still suffer from bias. For the years in our sample, we can directly observe student moves between residences, which enables us to assess the extent to

[17]We can directly interpret β_2 as an impact on the gain score, often an outcome of interest in many studies of educational interventions.

which such "Tiebout" moves may affect our estimates. In Appendix Table A7, we present estimates where we split the sample of new school switchers based on whether or not they moved into the new school zone in the years immediately prior to attending the new school, or whether they had lived there prior to construction. Results are nearly identical for movers and non-movers, with the exception of attendance, in which the estimated treatment effects are actually larger for non-movers. Thus, while we cannot definitively account for this source of bias, we view it as empirically negligible to our estimated effects.

3.3 Effects on switching students

In our baseline estimation we use all student-year observations in the relevant grades for a given outcome.[18] Students who never attend new school facilities are included in the regressions as controls, as are students who we observe at newly constructed schools in their first year in the data. In Appendix Table A8 we compare estimates where "stayers", never treated, and always treated students are excluded; reassuringly, results are very robust to the inclusion or exclusion of these students.

3.3.1 Student achievement

We begin our empirical analysis by examining effects on student achievement. Figure 4 reports estimates of the event study coefficients, β_k, from equation (1) for both math and ELA test scores. Standard errors are two-way clustered by both school and student. Time $k = -1$ is excluded; all effects are relative to the year before a student begins attending a new school facility. Panel A reports estimated coefficients on standardized math scores. There is no indication that students who switch to new schools have rising (or falling) scores relative to other students prior to the switch. Then, in the first year at a new school, there is a small but significant decrease of 4.1% of a standard deviation. This decline is short-lived, however: scores increase nearly linearly with each successive year a student attends school in a newly constructed facility, relative to other students who did not switch to a new school. After four or more years of attending a newly constructed

[18] The sole exception are those students who attend multiple new facilities, who are excluded to avoid any confounds in the dynamics of estimated treatment effects.

school, students score 9.7% (SE 2.8%) of a standard deviation higher.

Estimates for standardized ELA tests, reported in panel B, show a similar pattern. Students who attend a new facility for 4 or more years score 5.0% (SE 1.5%) of a standard deviation higher in ELA. For both subjects, the event-study figures indicate that the parametric specification in equation (2) fits the data quite well: after an initial decline in the year a student transitions to a new facility, test scores gradually increase, roughly linearly in years of exposure.

Table 3 reports estimates of equation (2) for math (columns 1-6) and ELA (columns 6-12) standardized test scores. Columns 1-3 and 7-9 report OLS estimates, whereas Columns 4-6 and 10-12 report 2SLS estimates. In columns 1 and 7, a simple one-parameter OLS specification is reported where only the change in the slope of student growth is included. For each additional year a student attends a newly constructed school facility her test score increases by 2.7% (SE 0.7%) and 1.7% (SE 0.4%) of a standard deviation in math and ELA, respectively. The implied effect for a student who attends a new school for four years is 8.1% (SE 2.2%) of a standard deviation for math and 5.0% (SE 1.2%) of a standard deviation for ELA. Columns 2 and 8 add indicators for attending a newly constructed school. Student achievement declines in the first year of attending a new school, although these coefficients are small and insignificant for both math and ELA test scores. Notably, the coefficient on the slope of student growth (β_2) and the implied 4-year test score effect are very similar to the one-parameter models in columns 1 and 7. Columns 3 and 9 add in a linear trend in student event time. The trend coefficients are small and insignificant for both math and ELA: less than one-tenth of one percent of a standard deviation per year. Importantly, the inclusion of the linear trend in the specification also does little to affect the magnitude or statistical significance of the coefficient on the change in trend, or the implied cumulative 4-year effect.

Columns 4-6 and 10-12 report estimates where matriculation at a newly constructed school is instrumented using a student's residential assignment. The 2SLS results indicate that this sorting channel has only a minimal effect on estimated treatment effects; in fact, while estimates for math are nearly identical, estimates for ELA are nearly 50% larger. This provides suggestive evidence that students who violate residential assignments and select into the new schools experience smaller gains from the new schools, a pattern of "reverse-Roy" selection that has been documented in other settings (e.g. Walters 2018). As in the OLS models, the linear trends included in columns 6 and 12 are small and insignificant, and have no effect on the magnitude of the estimated treatment effects.

Both event study and parametric difference-in-differences specifications provide consistent evidence of test score improvements upon switching to a new school. Effects are larger for girls than boys, but are similar for students with different levels of parental education (Appendix Table A7).[19] Both specifications also show that student test score gains accumulate gradually, after a slight decline in student performance in the year of the switch.

This pattern of gradual improvement is different from many other educational interventions considered in the literature, where effects tend to fade out over time.[20] Improvements in school facility quality are not a one-time intervention, however: students are continuously exposed to improved facility conditions for every year in which they attend a given school. We would therefore expect that achievement gains accumulate over time with additional years of exposure, even in the absence of initial disruption effects due to student-level switching costs[21] or school-level inefficiencies in the first few years post-construction.

3.3.2 Student non-cognitive effects

Figure 5 reports event study estimates for student attendance and teacher-reported student effort. Panel A shows the change in annual days attended for students who switch to new schools. Upon switching to a new school, students attend an additional 2.9 (SE 0.6) days per year. In the second year a student attends a new school facility, this jumps to 7.1 (SE 0.7) days. The effect tapers off somewhat in subsequent years, although after four or more years of attending a new school facility, students attend on average more than four additional days per academic year. Again, as with the student cognitive test score effects, there is little indication of any meaningful prior trend in attendance in the years before switching to a new school facility.

[19] We use parental education as a proxy for socioeconomic status in lieu of free/reduced price lunch eligibility, as over 90% of students who switched to new schools are eligible for free/reduced price lunch.

[20] Effects persist when examining a wider window in "event-time", although sample sizes are significantly smaller. See Figure A10.

[21] "Placebo" event study estimates for non-facility related student switches are reported in Figure A7. These estimates suggest that "normal" switches are associated with disruption effects of similar magnitudes, which fade out over time. Importantly, these switches are not associated with any short or long run test score improvements. These findings are consistent with results in Hanushek et al. (2004), who find evidence of short-run disruption effects with no-long run gains for students who switch schools within-district. Similarly, in a study of school closures in Michigan, Brummet (2014) finds short-run disruptions but not persistent effects.

Panel B shows the effect of switching to a new school facility on teacher-reported student effort for elementary students. Upon matriculation into a new school facility, student effort increases by roughly 3% (SE 1.7%) of standard deviation. As with attendance effects, the estimated increase in effort occurs immediately upon switch with no indication of an increasing trend in effort in the years prior to switching. This effect remains similar and marginally significant in the second year of a student's tenure at a new school, but fades out after 3 or more years of exposure to the new school. As was the case for the other outcomes, there is no indication of any meaningful pre-trend in student effort prior to a student's switch to a new school, providing additional justification for the identification assumption that the timing of student switching is as good as random.

Table 4 reports analogous estimates for attendance (columns 1-6) and effort (columns 7-12).[22] For student attendance, estimates in columns 1-6 imply that most of the effect occurs immediately upon switching to a new school. In column 1, the estimate of β_1 is 4.74 (S.E. 0.55), meaning that student attendance increases by nearly 5 days per year at newly constructed schools. 2SLS effects are slightly larger, at 5.2 (SE 0.57) days, again suggestive of (small) negative selection on gains among students students who attend new schools outside of their residential assignment. Adding the phase-in coefficient in columns 2 and 5 picks up some of this effect, reducing the coefficient on β_1 slightly. Columns 3 and 6 add in a linear trend in event-time, which slightly attenuates the estimates of β_1 and β_2. The estimated trend is small but statistically significant in the OLS model (column 3), but is 40% smaller and insignificant in the 2SLS model, indicating that the instrumental variables strategy is able to account for the (small) estimated pre-trend in attendance.

Estimates in columns 7-12 show a similar pattern for teacher-reported student effort, with small increases immediately upon a student's switch to a new school. OLS and 2SLS effects are similar and both insignificant in the one-parameter models in columns 7 and 10. The new school effect increases slightly with the addition of the post-trend coefficient, which picks up the fade-out of the effort effect that was apparent in panel B of Figure 5. The linear trends included in columns 9 and 12 are small and insignificant, and as was the case for the other student outcomes, has a negligible

[22]Unlike in Table 3, we begin columns 1, 4, 7, and 10 with one-parameter specifications where only the coefficient for mean difference in the outcome post matriculation at a new facility included. Columns 2, 5, 8 and 11 add a phase-in coefficient, and columns 3, 6, 9, and 12 include a linear trend in student event-time. In contrast to test score outcomes, which measure a stock of accumulated knowledge, student effort and attendance are flows, and thus a priori we might expect effects to occur immediately rather than accrue over time with continued exposure.

effect on the new school treatment effect estimates. Overall, these estimates provide some evidence of small initial increases in student effort upon switching to a new school.

3.4 Mechanisms

The pattern of student effects provides consistent evidence that student outcomes improved at new school facilities. What mechanisms underly these improvements? In this section, we summarize five set of results that speak to this question. First, to focus on the role of reduced overcrowding associated with the construction program, we examine outcomes of students at schools experiencing a significant outflow of peers to new schools. These students attend the same low-quality facilities as before but with a student body size closer to the intended capacity. They experience modest improvements in ELA and attendance but not in math or attendance. We interpret these findings as pointing to a moderate role of overcrowding, explaining up to a third of treatment effects. These findings are described in greater detail in Appendix B.1.

Second, we find little evidence that changes in calendars, class size, or peer quality at newly constructed schools confound student improvements. In fact, we find that moving to a new school is associated with slightly larger class sizes and slightly lower peer quality. We do find that switching to a new school is associated with large reductions in multi-track calendars, and that this mediates some of the attendance effects, but not test score effects. We describe these results in more detail in Appendix C.1.[23]

Third, we examine the role of teacher quality and principal quality. Along both observable and unobservable (using value added estimates) characteristics of teachers, we conclude that differences in teacher quality cannot account for observed student test score gains. If anything, somewhat lower-quality teachers attend new schools, although point estimates are small. Similarly, principals at new schools are somewhat less experienced. We provide more details in Appendix C.2.

Fourth, we study how much of the treatment effects are explained through these mechanisms by controlling for these changes both individually and jointly. To this end, we conduct an exercise similar to Card and Giuliano (2016) by controlling for predicted peer characteristics, teacher

[23]Changes typically associated with new school facilities (Appendix Tables A2, A3, and A5). We examine heterogeneity in the results by prior school conditions to test whether these changes are systematically related to the observed student gains (Table 5).

and principal fixed effects, multi-track calendar use, and facility congestion. We find that only attendance is moderately attenuated, entirely due to the elimination of multi-track calendars, while treatment effects on other outcomes remain largely robust. We describe this exercise in more detail in Appendix C.3.

Finally, we attempt to use available data to test whether school facility characteristics are important for these effects, as would be expected if they are indeed driven by improvements in the quality of school facilities. Unfortunately, we only have data on the characteristics of school facilities from a single point in time (2008),[24] meaning that we lack continuous variation in these measures.[25] Thus, in Table 5 we examine heterogeneity in effect estimates by the characteristics of the prior school attended by a student. The table reports estimates from models where only student switchers are included, excluding always and never treated students.

The first panel reports baseline one-parameter effect estimates. The second panel splits this effect by whether the student previously attended a school that was above or below the district median share of permanent classrooms (as opposed to portable classrooms). Portable facilities are also often of much worse quality, and have less functionality than traditional classroom space. Estimates for test score and effort effects are considerably larger for those coming from schools with less permanent classroom space (equivalently, a higher share of portable classrooms), and are small and statistically insignificant for students switching from schools with above-median shares. However, we can only reject equality of the coefficients for student effort ($p < 0.01$), differences in test score and attendance effects are not significant.

The third panel reports effects by whether the prior school was above or below the median building age, and shows a similar pattern. Students switching from older schools see consistently larger effects, although only the difference in student effort is statistically significant. The final panel reports effects split by prior building physical condition, or "FCI".[26] Again test score and effort

[24] Data on facility condition of LAUSD structures are available as a snapshot from maintenance records collected by the Facilities Service Division, and contain information on the age, condition, size, replacement value, and classification (e.g. permanent or portable) for each structure on every LAUSD school campus in 2008.

[25] We also lack comparable data on the relative quality of new school facilities. While there is variation across new facilities in terms of cost, this was often driven by land acquisition and remediation costs, and not necessarily the physical quality of the new construction.

[26] The "Facility Condition Index" (FCI) is the ratio of deficiencies to current replacement value. We calculate school FCI by taking a weighted average of the FCIs across all classroom structures at a school, weighted by the total square feet of each structure. An FCI close to zero indicates a facility is in excellent physical condition, whereas an

effects follow a mostly similar pattern, with slightly larger effects overall for students coming from schools in worse condition, although these differences are smaller than for age and share permanent, and none are significant. Notably, the attendance effect is nearly 50% larger for students coming from schools in *better* prior condition, and this difference is significant ($p = 0.02$). We hypothesize that this is related to the mechanical changes in the total number of instructional days at some schools, as was discussed earlier.

The results in Table 5 provide suggestive evidence that effects were larger among students who experienced larger improvements in facility quality upon switching to a new school. Without time varying-data on facility quality of both existing and new schools, it is difficult to provide more definitive evidence. Nevertheless, these results – in combination with the relative lack of importance of any other contemporaneous changes such as peers, class size, teachers, and principals – provide evidence that facility quality is the primary mechanism explaining student gains at new facilities.

4 Neighborhood Impacts

The evidence on student impacts of the school construction program suggests it was effective. But since we seek to provide a comprehensive assessment of the efficiency of the program, we now turn towards the housing market impacts of the program. As we show in Section 5, these impacts capture a much greater share of program benefits than do student impacts alone.

4.1 Real estate data

To analyze the effects of increased capital expenditures on the real estate market, we use administrative records from the Los Angeles County Assessor's Office. Records contain information for each property in Los Angeles county, and includes data on the three most recent sales, as well as information on property characteristics from the most recent assessment. Properties are matched to the assigned school district, school attendance assignment (for elementary, middle, and high school) in each year, city, and tax rate area (TRA).[27]

FCI of greater than one indicates that a facility has deteriorated to the point where the total sum of deficiencies is greater than the total replacement cost of the facility.

[27]The TRA is defined as the specific geographical area within a county wherein each parcel is subject to the same combination of taxing entities; the tax rate is therefore uniform for all properties in a given TRA. Our database

We focus only on sales of residential properties with non-missing sales prices. We limit attention to single-family residences and exclude large parcels with greater than 1 acre of usable area. We then drop properties with missing information on property characteristics (<1%). Data on property characteristics is available only for the most recent assessment; we therefore drop to-be rebuilt properties (i.e. those sales with a "negative" building age) to avoid biases arising from incorrect valuation of property characteristics. This final restriction is non-trivial; roughly 2.8% of sales are excluded. Finally, we exclude the top 1% and bottom 1% of property sales in each year to avoid results being affected by outliers or non-market-rate transactions.[28]

Table 6 summarizes these data. Column 1 reports means for all property sales in the sample within LAUSD district boundaries. Column 2 restrict to only those properties that ever reside in a new school attendance zone, while column 3 reports means for those properties that never receive a newly constructed school facility during the sample period. The average single-family residence in the district was $565,801 (in 2015$) during the sample period. Comparisons of columns 2 and 3 show that new school neighborhoods are generally negatively selected in terms of house prices: houses in new school zones sold for over $200,000 less than those in areas that did not receive new schools. Overall, after sample restrictions, the assessor dataset covers 505,835 property sales for 350,299 unique properties, roughly one-third of which are located in neighborhoods that received new schools during the construction program.

4.2 Econometric design

Given the haphazard rollout of the program across sites, the timing of construction is plausibly exogenous relative to any underlying neighborhood characteristics or trends. Thus, parallel to our estimation of student effects, we estimate house price effects in a dynamic setting by examining changes in school quality induced by new constructions, relying on variation in the exact timing of completion. Specifically, we compare changes in house prices over time in neighborhoods that received new schools, relying on across-neighborhood variation in the exact year of school con-

of LAUSD school assignment zones is only comprehensive up to 2012; moreover, our project database of post-2012 school constructions is also incomplete. For this reason, we limit attention only to the 1995-2012 period, although results are robust to including later years. See Appendix Table A11, where we compare results using all years to pre-2013 years.

[28]Results are robust to relaxing these sample restrictions. See Appendix Table A12.

struction, and controlling for neighborhood effects to account for any time invariant neighborhood characteristics. Changes in prices reflect the present discounted value of current and future benefits of new schools to households. Thus, we estimate the mean difference in house prices before and after construction with following difference-in-differences specification:

$$ln(P_{it}) = \alpha_{j(i)} + \delta_{t,h(j(i))} + \theta N_{j(i),t} + X'_{it}\Gamma + \epsilon_{it} \tag{3}$$

where $N_{j(i),t} = \mathbb{1}[NewSchoolZone_{j(i),t} = 1]$ is an indicator for a property sale occurring in a new school attendance zone, after the date of the new school opening, for a given property i in neighborhood $j(i)$ that is sold at time t. X'_{it} is a vector of property characteristics that includes the number of bathrooms, the number of bedrooms, building square footage, square footage squared, building age, age squared, effective age, effective age squared, usable lot area, usable lot area squared, an indicator for the specific tax rate area, and an indicator for number of sales observed in the data for specific parcel. $\alpha_{j(i)}$ and $\delta_{t,h(j(i))}$ are fixed effects for neighborhood and year-by-high school zone, respectively.[29]

In all house price specifications, standard errors are clustered by neighborhood. Baseline specifications include all parcels in the district, including those that are never assigned to the attendance zone of a newly constructed school. As long as the exact timing of school construction within the set of receiving neighborhoods is uncorrelated with time-varying neighborhood trends, estimation of equation (3) will yield an unbiased estimation of θ. In addition, we estimate specifications that also exclude "never-treated" properties as controls, as well as specifications that only included "never-treated" properties within 1km of a new school attendance zone.

If capitalization occurs prior to construction due to anticipatory effects, neighborhood house prices may diverge prior to construction between those soon to receive new schools and those

[29] Here we use the high school zones from the 2004 school year, the year before the first new high school construction, to flexibly account for differential trends in house prices between local areas. We also report specifications that instead use uniform year effects, δ_t (Table 7, columns 4 and 6). We define neighborhoods as the elementary-middle-high school assignment triplet in the 2000-2001 academic year, prior to the construction of any new facilities. See Figure A11 for a map of these neighborhoods. We define school zones using pre construction boundaries from 2000, to eliminate concerns over endogenous new school attendance boundary formation. Reassuringly, this distinction makes no quantitative difference, as results are nearly identical when post construction boundaries are used instead (Table A11 panel B).

receiving new schools in later years. Conversely, initial uncertainty by parents as to the quality of a new school could lead to house price effects that gradually cumulate post-completion. Thus, we also estimate more flexible event-study models, akin to equation (1), that estimate the difference in house prices relative to the year prior to building occupancy:

$$ln(P_{it}) = \alpha_{j(i)} + \delta_{t,h(j(i))} + \sum_{k=\underline{K}}^{\overline{K}} \theta_k \mathbb{1}(t = t_i^* + k) + X_{it}'\Gamma + \epsilon_{it} \tag{4}$$

In these non-parametric event study models, θ_k measures the effect of receiving a new school in year t_i^* k years after construction (or prior, where $k < 0$). Effects are measured relative to year $k = -1$, which is excluded in estimation. We focus on a ten-year window, binning endpoints at $\underline{K} = -6$ and $\overline{K} = 3$, which represent average house prices six or more years prior to construction or four or more years post- construction, respectively.

In equations (3) and (4), identification of θ assumes that trends in house prices are uncorrelated with the exact timing of school construction, conditional on property-specific controls and controls for time-invariant differences between neighborhoods. This assumption could be potentially violated if unobserved differences in the characteristics of those properties sold in a given year are correlated with the timing of switching.[30] To account for this potential source of bias, we also estimate equation (3) with property fixed effects, controlling for time-invariant unobserved differences between individual properties:

$$ln(P_{it}) = \alpha_i + \delta_{t,h(j(i))} + \theta N_{j(i),t} + \epsilon_{it} \tag{5}$$

In equation (5), estimation of θ relies only on properties with repeat sales in the sample window. Repeat sales indices are commonly used when estimating dynamic capitalization in real estate prices (e.g. Figlio and Lucas 2004) to account for unobserved differences in property and neighborhood characteristics. In practice, estimates of θ are very similar in both equations (3) and (5), implying that differences in unobserved property characteristics are uncorrelated with timing of construction

[30] This would be the case, for example, if houses with positive unobserved characteristics are more likely to be sold within a given neighborhood post-construction than pre-construction. Regressions using predicted prices based on observable property characteristics suggest there are no major differences in the observable composition of properties sold post-construction. See Table A15.

and do not drive the estimated results. Moreover, we find little evidence of differential trends prior to school construction, and effects accrue quickly within 2-3 years post-construction. Therefore, we emphasize the simple linear differences-in-differences estimate of β from equation (3).

4.3 Neighborhood Results

Table 7 reports estimates of the effect of new school constructions on house prices. Columns 1-4 report estimates using fixed effects for school zone and property-specific control variables as in equation (3). Columns 5 and 6 report estimates using property fixed effects as in equation (5). Column 1 reports estimates from the baseline specification using all properties in LAUSD. House prices rise 6.0% (SE 1.8%) post construction in neighborhoods that receive new schools, relative to nearby property sales in the same year within the same initial high school attendance area. To account for any potential biases from including far away "never treated" properties, in column 2 we drop "never-treated" properties further than one kilometer from a new school zone, and in column 3 we further restrict the sample to only those properties that ever receive a new school. Results in columns 1 and 2 are nearly identical, and the estimated coefficient drops slightly to 4.4% (SE 1.1%) in column 3. Column 4 substitutes year effects for the year-by-high school zone effects – now unnecessary as we have limited the control group to properties near the new schools – and the point estimate increases slightly to 5.5% (SE 1.5%).

Columns 5 and 6 report estimates analogous to columns 3 and 4 using property fixed effects in lieu of property controls and neighborhood fixed effects. In column 5, estimation includes year-by-high school zone effects, while column 6 shows estimates where only year-specific effects are included. Estimated effects are very similar to analogous neighborhood fixed effects estimates in columns 3 and 4. Overall, estimates are consistent in magnitude and show that house prices increase by roughly 4-6% post-construction in new school attendance areas.[31]

In Figure 6 we report event study estimates of the effects of new school constructions, corresponding to the specification in equation (4). Estimation includes only those properties ever within any new school zone and year-by-high school fixed effects, as in the specification in column 3 of

[31] We find little evidence that effects vary by distance from the attendance area boundary or by mean neighborhood price level. See Appendix E.

Table 7. Effects are estimated relative to the year before school occupancy, which is omitted from the regression. Results in both panels of Figure 6 show little sign of pre-existing trends or dynamic anticipatory effects pre-construction. Capitalization occurs somewhat gradually upon completion, with nearly all of the effect coming in the first two years after school completion, before stabilizing after three or more years. Three or more years after the new school construction, house prices in the new school attendance areas were 7% higher, slightly larger than the point estimates presented in Table 7.

5 Program Efficiency

In previous sections, we established that the school construction program increased both academic achievement and housing valuation in the neighborhoods receiving new schools. We now turn to the question of how these benefits compare to the costs of the program. There are two distinct approaches in the literature to valuing the impacts of educational programs: first, in terms of capitalization in the real estate market (Barrow and Rouse, 2004; Cellini et al., 2010); and second, in terms of later life earnings (Heckman et al., 2010; Chetty et al., 2011; Heckman et al., 2013; Chetty et al., 2014; Kline and Walters, 2016).

Both of these approaches have important advantages and disadvantages. The capitalization approach captures program benefits beyond academic achievement, such as safety, health, or recreational opportunities; however, it relies on revealed preferences that may only imperfectly capture achievement and later life earnings, as shown by a literature documenting that parental valuation of school effectiveness conditional on peer characteristics is limited (Rothstein, 2006; Abdulkadiroğlu et al., 2017). In contrast, the later life earnings approach sidesteps the limited effectiveness valuation issue by directly estimating the earnings gains induced by the program, but it cannot speak to other benefits outside of the academic realm.

To the extent that parental valuation in the housing market already captures academic benefits, simply adding up benefits from both of these approaches would amount to double-counting. Thus, we now develop a model that integrates both of these approaches in a way that isolates preferences for academic valuation revealed in the housing market. To do so, we extend a class of residential choice models in the tradition of Tiebout (1956) and Brueckner (1979) by allowing households to value new schools both through their academic benefits on student achievement as well as their non-academic benefits on affected neighborhoods. In this way, we can study what share of benefits derives from later life earnings gains due to achievement effects of the program, and how the magnitude of these achievement benefits depends on the choice of valuation approach. We focus on the fiscal externalities that arise due to the concentration of funds in a subset of treated neighborhoods but abstract from fiscal externalities that could arise from ad valorem taxation on properties or taxes on later life earnings gains.[32]

5.1 Household valuation model

5.1.1 Setup and program costs

Consider a single school district with N one-child households and neighborhoods indexed by $j = 0, 1, ..., J$. Each neighborhood has fixed housing supply N_j with $\sum_j N_j = N$, school amenities A_j, and endogenously determined house prices P_j. The district launches a redistributive school construction program that imposes a head tax τ on every household in the district and spends all proceeds on the subset of treated neighborhoods with index $j \geq 1$. For convenience, we normalize school amenities to increase by one unit due to the program in these treated neighborhoods, such that neighborhood investment per household R_j increases by $\frac{\partial R_j}{\partial A_j}$ due to a program of size τ. The school district is required to balance its budget: $\tau N = \sum_{j \geq 1} N_j \frac{\partial R_j}{\partial A_j}$. It then spends the same per-household amount on each neighborhood that receives funds such that per-household investment in schools is given by

$$\frac{\partial R_j}{\partial A_j} = \frac{\tau N}{\sum_{k \geq 1} N_k} \equiv \tau n \tag{6}$$

for $j \geq 1$, while no program funds are spent in control neighborhood $j = 0$.[33] The inverse share of treated households $n = N / \sum_{j \geq k} N_k$ also represents how many dollars are spent on treated

[32] We are evaluating the welfare impact of the program from the perspective of the district, which does not directly benefit from taxes on later life earnings gains. If one were to take the perspective of the state or federal government, these taxes would lower the cost of the program through this positive fiscal externality, further increasing the benefit-cost ratio.

[33] Because we allow the effect of this one-unit increase due to the program to be heterogeneous across neighborhoods, assuming that per-household spending is constant is equivalent to assuming that spending differs by neighborhood and that each per-household dollar of spending translates into the same achievement effect.

neighborhoods for each dollar raised in taxes, and so τn is the cost of the program per household in treated neighborhoods (which we can observe in the data). Because we normalize units of school amenities to the program size and marginal utility of income is assumed to be one across all households, the marginal cost of the program equals the marginal rate of transformation between school amenities and income, MRT. This will be useful to show that, under efficiency, the marginal rate of substitution equals the marginal rate of transformation.

Households derive utility from non-academic aspects of neighborhood amenities, school achievement, and private consumption $U(A_j, Y_j, c)$ and choose a neighborhood subject to the budget constraint $w \geq c + \tau + P_j$, where w is household income. Thus, households value school amenities both directly as well as indirectly through their effect on student achievement. Indirect utility is then given by $V_j \equiv V(A_j, Y_j, w - \tau - P_j)$, which households maximize by trading off the benefits of school amenities against the housing cost in the neighborhood. With homogeneous households, the equilibrium market price of housing equalizes utility in all neighborhoods.

5.1.2 Program willingness to pay

We now characterize our two key empirical parameters – the achievement effect and the house price effect – in the context of this model, which will allow us to interpret both of them in terms of (a) the implied household preferences for both the direct (i.e. neighborhood) and the indirect (i.e. achievement) effects of the program as well as (b) the marginal value of a dollar of public expenditures. It turns out that the covariance between the house price effect and the achievement effect is key to distinguish between preferences for neighborhood improvement and student achievement. Thus, we now express our empirical parameters as neighborhood-specific random coefficients.

Define the estimated achievement effect in neighborhood j as $\beta_j = \frac{\partial Y_j}{\partial A_j} + \nu_j$. The component $\frac{\partial Y_j}{\partial A_j}$ is the average test score gains due to program investment into school amenities for households in j as estimated in Section 3, and ν_j is an error term with $E[\nu_j] = 0$. Similarly, let $\theta_j = \frac{\partial P_j}{\partial A_j} + \xi_j$ be the estimated house price effect in neighborhood j relative to the control neighborhood. Here, $\frac{\partial P_j}{\partial A_j}$ is the increase in household willingness to pay for housing in j due to additional school amenities net of any tax changes, as estimated in Section 4, and ξ_j is an error term with $E[\xi_j] = 0$. We can now show that willingness to pay for housing and student achievement in a given neighborhood are tightly linked through direct and indirect preferences for school amenities:

$$\theta_j = \left[\frac{\partial V_j}{\partial c}\right]^{-1}\left(\frac{\partial V_j}{\partial A_j} + \frac{\partial V_j}{\partial Y_i}\frac{\partial Y_j}{\partial A_j}\right) + \xi_j = \mathrm{MRS}^A + \mathrm{MRS}^Y \beta_j + \varepsilon_j \tag{7}$$

where the first equality follows from the Implicit Function Theorem; $\mathrm{MRS}^A \equiv \frac{\partial V_j}{\partial A_j} / \frac{\partial V_j}{\partial c}$ and $\mathrm{MRS}^Y \equiv \frac{\partial V_j}{\partial Y_i} / \frac{\partial V_j}{\partial c}$ are the marginal rates of substitution between neighborhood amenities or school achievement and income, respectively; and $\varepsilon_j = \xi_j - \mathrm{MRS}^Y \nu_j$. We can see that the program is capitalized in the housing market through both valuation of academic benefits MRS^Y scaled by the magnitude of achievement gains in the neighborhood β_j as well as valuation of a non-academic benefits MRS^A of the program. We refer to these components as the value of school effectiveness and neighborhood attractiveness, respectively.

The capitalization approach aims to capture both of these sources of program benefits, but recent evidence points towards little parental valuation of school effectiveness (Abdulkadiroğlu et al., 2017). The later life earnings approach avoids this issue by replacing household valuation of achievement revealed in the housing market MRS^Y with (usually external) estimates of the labor market value of human capital W, which may be superior to household forecasts of earnings gains due to academic achievement. Program benefits in neighborhood j are then simply $W\beta_j$. However, the later life earnings approach omits neighborhood attractiveness MRS^A, which may make up an important part of program benefits. The hybrid approach we develop below combines the strength of both of these approaches by estimating benefits in j as $\mathrm{MRS}^A + W\beta_j$: the housing market value of neighborhood attractiveness plus the labor market value of school effectiveness.

To isolate the value of neighborhood attractiveness, consider that achievement is largely uncorrelated with other factors driving heterogeneous program responses, that is $Cov(\beta_j, \varepsilon_j) = 0$.[34] Then, notice that the population regression coefficient of the house price effect on the achievement effect in equation (7) corresponds to household valuation of achievement:

$$\frac{Cov(\theta_j, \beta_j)}{Var(\beta_j)} = \mathrm{MRS}^Y \tag{8}$$

[34] See Table A7 that achievement effects do not vary significantly with parental education or residential mobility. Thus, in our case, OLS estimation of (7) may yield a reasonable approximation of household preferences. We also show that our estimate is similar those in the literature using a variety of identification strategies.

This is the object of interest in the vast literature on housing valuation of school performance surveyed in (Black and Machin, 2011), which studies house price responses to variation in student achievement.[35] Using this relationship, we can quantify the share of housing valuation due to the student achievement benefits of the program as

$$\gamma \equiv \frac{\text{MRS}^Y E\left[\beta_j\right]}{\text{MRS}^A + \text{MRS}^Y E\left[\beta_j\right]} = \frac{E\left[\beta_j\right] Cov\left(\theta_j, \beta_j\right)}{E\left[\theta_j\right] Var\left(\beta_j\right)}, \tag{9}$$

which says that the share of household valuation due to achievement equals the regression coefficient in (8) rescaled by the ratio of mean effects. This is a general result that can, in principle, be applied to all studies of educational programs for which mean effects on housing and test scores as well as their covariance are available.

5.1.3 Marginal value of public funds and efficiency

Now that we have characterized the willingness to pay for the program, we are ready to define the marginal value of public funds (Hendren, 2016). It is defined as the ratio of the willingness to pay and the net cost of the program. In each case, we express the MVPF first as a function of preferences and technology and then as a function of program effects and observables. From the perspective of the capitalization approach, this is

$$\text{MVPF}^C \equiv E\left[\frac{\partial P_j/\partial A_j}{\partial R_j/\partial A_j}\right] = \frac{\text{MRS}^A + \text{MRS}^Y E\left[\beta_j\right]}{\text{MRT}} = \frac{E\left[\theta_j\right]}{n\tau},$$

which shows we can estimate it as the ratio of the average house price effect (in dollars) and the per-treated-household cost of the program. Similarly, from the perspective of the later life earnings approach, it is

$$\text{MVPF}^E \equiv E\left[\frac{\left(\partial Y_j/\partial A_j\right) W}{\partial R_j/\partial A_j}\right] = \frac{W \cdot E\left[\beta_j\right]}{\text{MRT}} = \frac{W \cdot E\left[\beta_j\right]}{n\tau},$$

so that it can be estimated as the achievement effect scaled by the labor market price of human capital divided by the per-treated-household cost. Finally, we define the MVPF for the hybrid

[35] To see this, integrate both sides of (7) over A_j to arrive at $P_j = c + Y_j \text{MRS}^Y + u_j$, which corresponds to equation (10.1) in (Black and Machin, 2011).

approach as

$$\begin{aligned}\text{MVPF} &\equiv (1-\gamma)\,\text{MVPF}^C + \text{MVPF}^E \\ &= \frac{\text{MRS}^A + W \cdot E\left[\beta_j\right]}{\text{MRT}} = \frac{(1-\gamma)\,E\left[\theta_j\right] + W \cdot E\left[\beta_j\right]}{\tau n}.\end{aligned} \tag{10}$$

We summarize these definitions by making two observations about this hybrid MVPF. First, if household valuation of academic achievement revealed in the housing market MRS^Y equals the labor market valuation of human capital W, then the hybrid approach equals the capitalization approach (and vice versa). And second, if, in addition, households value a program entirely due to its academic impacts such that $\text{MRS}^A = 0$, then the hybrid approach also equals the later life earnings approach (and vice versa).

The MVPF serves as the key statistic to assess program efficiency: if it is greater than one, a dollar raised in taxes is worth more than one dollar in terms of household valuation of neighborhood improvements and later life earnings. This would suggest that school amenities were underprovided relative to the efficient level. Conversely, if the MVPF is smaller than one, the costs outweigh the benefits, and we would infer that the program was inefficiently large. The program is at efficient scale when the MVPF is exactly one, in which case the marginal rate of substitution equals the marginal rate of transformation as in the Samuelson's condition (1954) for fully congested public goods.

5.2 Cost-benefit analysis

With these relationships in hand, we can now quantify the costs and benefits and decompose the latter into valuation for school effectiveness and school attractiveness. The results of this exercise can be seen in Table 8. We begin with program costs. According to the 2005-2009 American Community Survey (ACS), there were 1.52 million non-vacant housing units in LAUSD. The total cost of the program was \$9.17 billion, meaning that the average cost to a treated household of the program (τ) is approximately \$6,045 in present value. Given that just under one in three households lives in treated neighborhoods, the cost per treated household is around \$18,430.

Moving to program benefits, we begin with the benefits reflected in the real estate market. The average sale price (within-sample) of properties in zones that received new schools was $494,650. Using the estimates in Table 7, the median house price change in treated neighborhoods is 5.7%. This implies houses in treated neighborhoods gained $28,195 in value, with a resulting gross capitalization benefit of $14.06 billion. The ratio of these benefits to costs, which corresponds to MVPF^C, yields a value of 1.53. Thus, housing capitalization suggests the program was inefficiently small.

We now turn to the benefits in the form of later life earnings. Using the estimates presented in Chetty et al. (2011), we can project forward the gain in future earnings from the observed test score gains. Chetty et al. (2011) use experimental variation in classroom quality to estimate that a 0.1 standard deviation increase in test scores[36] leads to a 1.3% increase in earnings at age 27.[37]To extrapolate our estimates forward, we first compute the present discounted value of future earnings for future cohorts:

$$\text{PDV}_{\text{cohort}} = \sum_{j\geq 1} N_j \sum_{t=16}^{56} \frac{E_t}{(1+\delta)^t}$$

where E_t = earnings gain at each age, which we compute under the assumption of a constant percentage gain of 1.3% per 0.1 SD increase in test scores, using age-earnings profiles from the March CPS.[38] The average elementary school student is 11 years old, therefore we discount forward 16 years to age 27, and count benefits until retirement at age 67. From our data, roughly 16% of students entering elementary school, 13% of students entering middle school, and 25% of students entering high school in LAUSD were in a newly constructed school facility. Plugging this in and using the estimated effects on math test scores, assuming a 3% discount rate, yields a present discounted value of future earnings per cohort of $177 million. From our facilities data, we estimate that a brand new facility would take roughly 35 years to depreciate to the mean condition of existing facilities in LAUSD. Assuming the effects are constant for this 35 year horizon and discounting the

[36]Notably, this is for kindergarten scores. However, non-experimental estimates in the same paper show that the correlation between test scores and earnings grows with age, suggesting that these effects may underestimate the effects of improvements in later grades.

[37]The effects estimated in Chetty et al. (2011) are in the middle of the range of estimates in the literature estimating the relationship between test scores and future earnings. See Table A.IV in Kline and Walters (2016) for a comparison of effect size estimates.

[38]We compute the age-earnings profiles using data from 2012-2016, and use the average earnings, including those with zero earnings. This follows the procedure in Chetty et al. (2011), but may overstate impacts if earnings of LAUSD students are below average over the life cycle.

earnings of future cohorts implies a gain in later life earnings of \$3.9 billion in present discounted value.[39] The total program cost was \$9.17 billion, implying that the gain in later life earnings from test score improvements covers roughly 42% of the total program cost, which corresponds to a MVPF^E of around 0.42. If we were to consider only later life earnings, we would conclude that the program was inefficiently large.[40]

Having separately demonstrated the implied benefits and marginal values of public funds using the two approaches, we now combine them by isolating the non-academic share of housing valuation. Regressing house price effects by neighborhood on achievement effects by neighborhood and rescaling by the ratio of mean effects, we estimate that around 22% of real estate valuation is due to the academic benefits of the program, while around 78% is associated with non-academic benefits.

Specifically, we proceed as follows to arrive at this estimate. First, we compute neighborhood specific treatment effects by interacting our baseline treatment effect coefficients with neighborhood fixed effects.[41] Second, we regress the resulting 65 neighborhood-specific house price effects on their corresponding average achievement effects, weighted by the number of households per neighborhood. This gives us the coefficient in equation (8), which we estimate to be 0.168, or \$83,101 after rescaling using average house prices in the district – that is, a one standard-deviation better new school sees house prices rise by 16.8% more (see Figure A14). This is similar to the median estimate in the literature surveyed by Black and Machin (2011), whose median effect across 15 papers is about 0.14 per student-level standard deviation in achievement.[42] Third, we rescale this coefficient by the ratio of mean effects, as given by equation (9), which results in $\gamma = 0.22$.[43]

This implies a housing valuation of academic benefits of around \$2.96 billion, which is about 76% of estimated later life earnings. Unlike recent work finding that parental preferences for schools

[39] If we instead assume that effects decay geometrically at a 3% rate over a 70-year horizon (the average age of buildings students switched from), the cumulative earnings gains are 24% smaller, or \$3 billion.

[40] Here we are not counting any indirect improvements for students who stayed behind at existing schools. Including these would slightly increase aggregate future earnings gains, but would not change the qualitative conclusion that future earnings gains from test score improvements do not cover total program costs.

[41] We shrink test score estimates using Empirical Bayes. Results are very similar using raw estimates.

[42] We refer to 15 papers using U.S. data mentioned in Black and Machin (2011) and scale them to student-level standard deviations using the ratio in Kane et al. (2003), which is the only paper in the review that reports both school and student-level standard deviations.

[43] The numerator of this ratio, $E[\beta_j]$, is 0.075, which is the average achievement effect of new schools across math and ELA. The denominator, $E[\theta_j]$ is $0.057 \cdot E[P_j]$, the median estimate of the log house price effect of new schools scaled by average house prices. Thus, $\gamma = 0.075/(0.057 \cdot E[P_j]) \times 0.168 \cdot E[P_j] = 0.22$.

are almost entirely determined by peers instead of school effectiveness (Abdulkadiroğlu et al., 2017), this result suggests that households value academic benefits reasonably well. Perhaps, some of the non-academic benefits of educational programs considered in residential choice steer households towards schools in a way that more closely matches the academic value of the program, unlike school choice conditional on residential location.

Finally, we combine these findings to estimate program benefits and the marginal value of public funds using expression (10). We find that total program benefits using the hybrid approach are around \$14.85 billion, with a marginal value of public funds of around 1.62. Unsurprisingly, given that the majority of benefits derive from non-academic program benefits and housing capitalization of academic benefits is fairly close to later life earnings, these quantities are quite similar to real estate capitalization alone, as captured in MVPF^C. We conclude from this finding that, while both capitalization and later life earnings are important, using only benefits arising from later life earnings may severely underestimate program benefits.

6 Conclusion

In this paper we provide robust and comprehensive estimates of the effects of educational capital investments on student outcomes and neighborhood house prices. To date, the literature on the effects of school capital investments has been mixed and inconclusive; many prior studies are underpowered to detect modest effects, often relying on district-level average outcomes to study the impacts of programs that impact only a subset of students (Figure A2). Studying the largest school construction program in US history, we provide robust new evidence that school facility investments lead to modest, gradual improvements in student test scores, large immediate improvements in student attendance, and marginal improvements in student effort. We provide evidence that these improvements stem from exposure to higher-quality facilities.

New school constructions induced large increases in neighborhood house prices upon completion, implying significant parental valuation of improvements in school quality. House prices increased substantially in areas that received new schools, implying that the total real estate capitalization exceeded program cost. We derive a marginal value of public funds of around 1.5 for one dollar of per-household school capital investment. This implies that prior capital spending had been inefficiently low in the distinct, and that the targeted program to improve facilities generated aggregate welfare increases in the district.

Parents, however, may not fully or correctly internalize the future benefits of academic improvements (Rothstein, 2006; Abdulkadiroğlu et al., 2017). To date, the literature evaluating the efficiency of educational investments has only separately considered capitalization and future earnings. To integrate these two approaches, we extend a standard residential choice model to isolate preferences for the academic valuation revealed through real estate capitalization. We show that 76% of future earnings benefits are capitalized into the housing market, but that only 22% of the total valuation is due to the direct test score benefits of the new facilities. Taking into account the partial capitalization of school effects using our newly developed hybrid method of benefit accounting, we arrive at a marginal value of public funds of around 1.6.

These substantial positive impacts of new school facilities on achievement and house prices raise the question what aspects of school facilities generate these benefits. There are two theories that may be able to account for them. According to the Broken Windows theory (Zimbardo, 1969, named and popularized by Wilson and Kelling, 1982), neglect in public spaces signals the absence of binding social norms and opens the door to disorderly and destructive behavior. Branham (2004) argues that this theory holds especially true in a school infrastructure context: students perceive school as a place where effort goes unrewarded when the learning environment is dilapidated. In contrast, school facility effects may have nothing to do with social norms but may run primarily through physiological effects such as the temperature of the learning environment (Goodman et al., 2018). With more precise data on changes in facility conditions in the course of a facility program, future research may be able to distinguish between these two theories and provide guidance on which facility components have the highest return in terms of learning and real estate capitalization.

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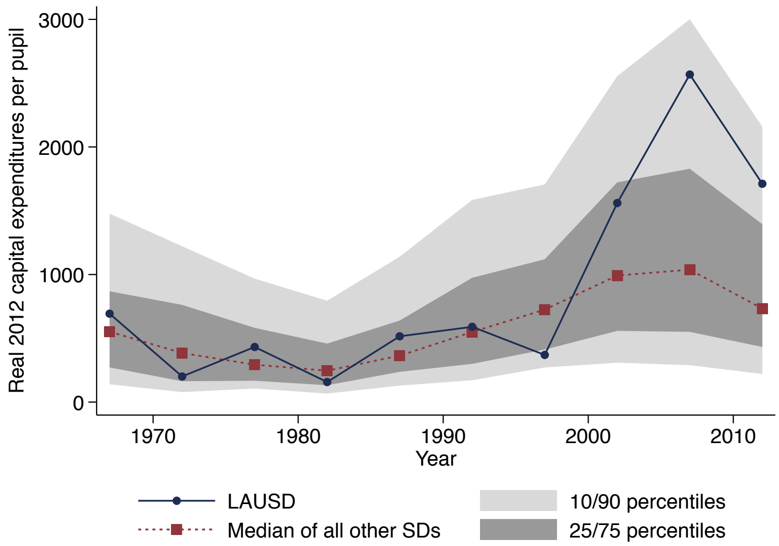
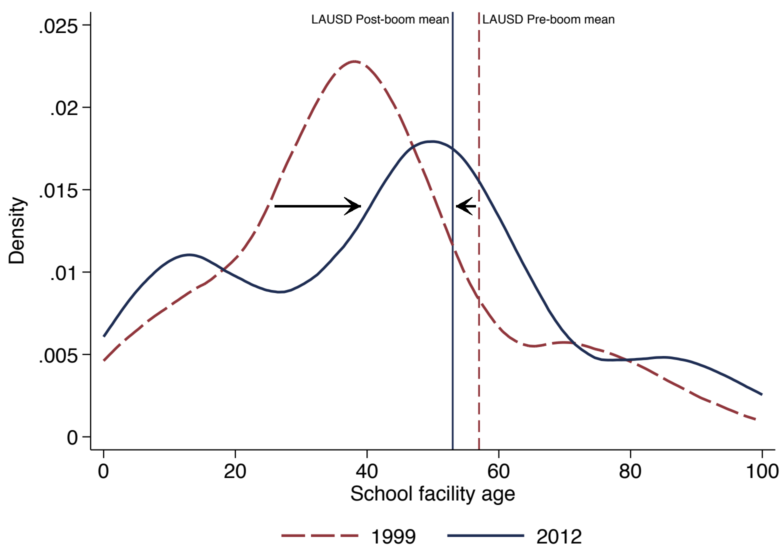
Figures

Figure 1: LAUSD School Capital Spending in Context



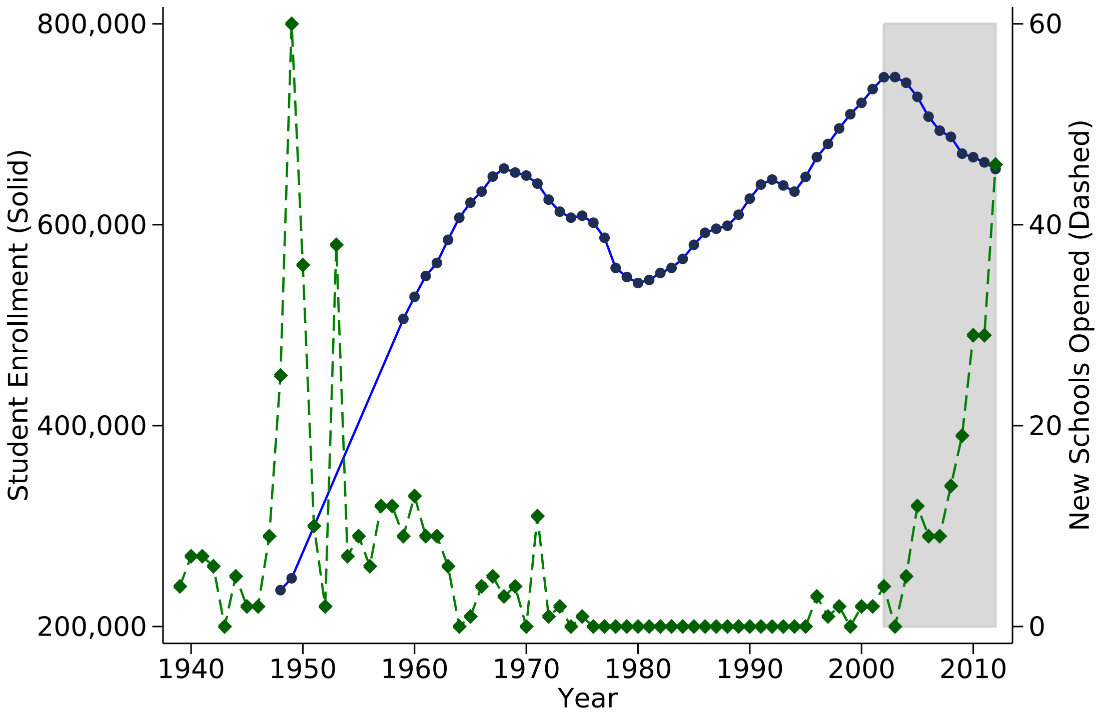
(a) U.S. and LAUSD School Capital Spending, 1967-2012



(b) National distribution of school facility age, 1999 and 2012

Notes: Panel (a) reports per-pupil capital spending, in five year-intervals using data from the Census of Governments. Only districts with enrollment above 1,000 students are included. Panel (b) reports the distribution of school age in the United States, using data from 1999 and 2012 surveys conducted by the National Center for Education Statistics.

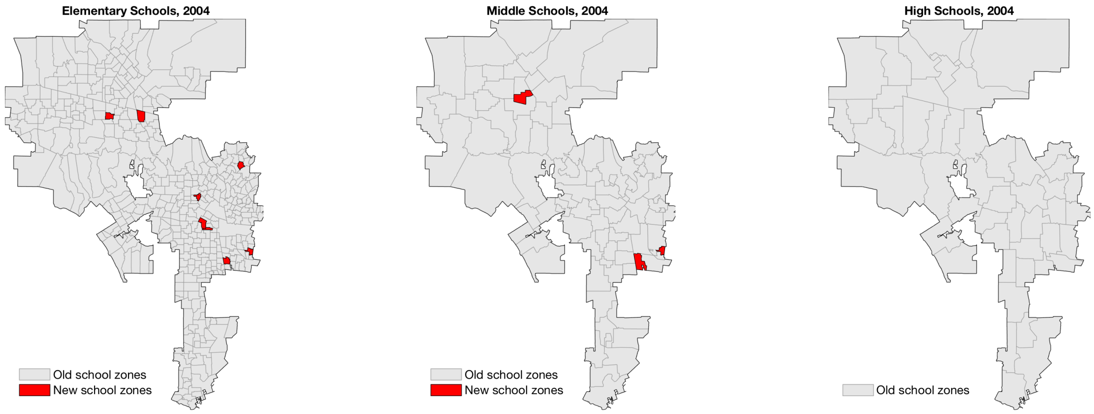
Figure 2: School construction and enrollment, LAUSD 1940-2012



Notes: Figure shows annual student enrollment and the number of new facilities opened by year for LAUSD. The solid blue line shows enrollment (left axis) and dashed green line shows the number of new schools opened in a given year (right axis). Shaded area from 2002-2012 shows the treatment period covered in the paper. The number of new schools only includes facilities still operational in 2008, and is computed as the minimum age over all buildings that comprise a given school facility. Historical enrollment data were obtained from the California Department of Education.

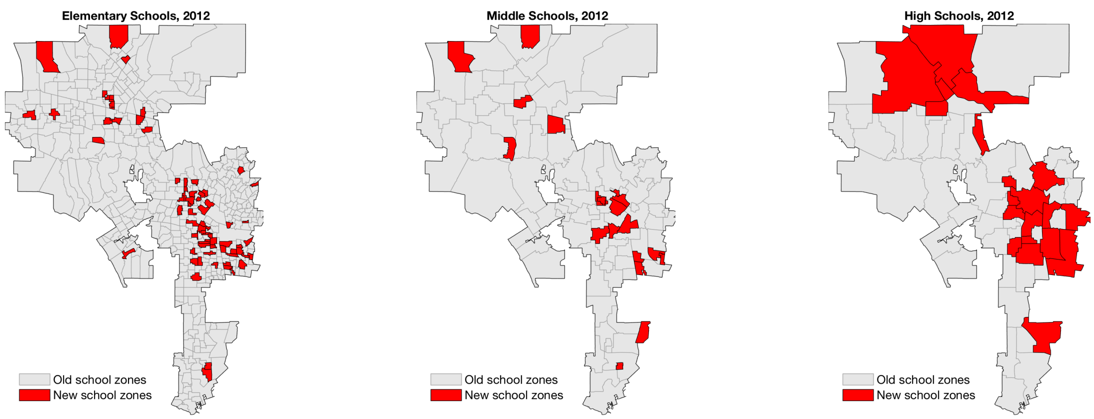
Figure 3: LAUSD school attendance zones, 2004 vs 2012

2004:



(a) Elementary (b) Middle (c) High

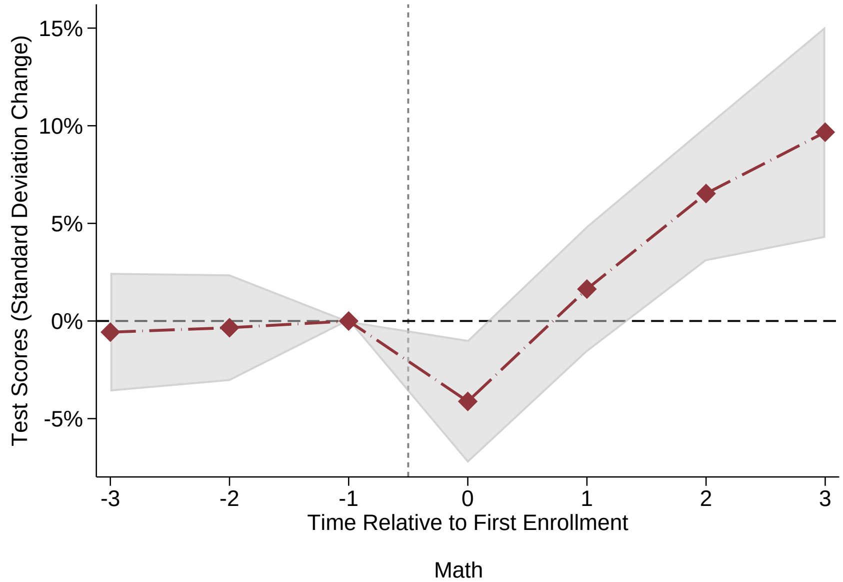
2012:



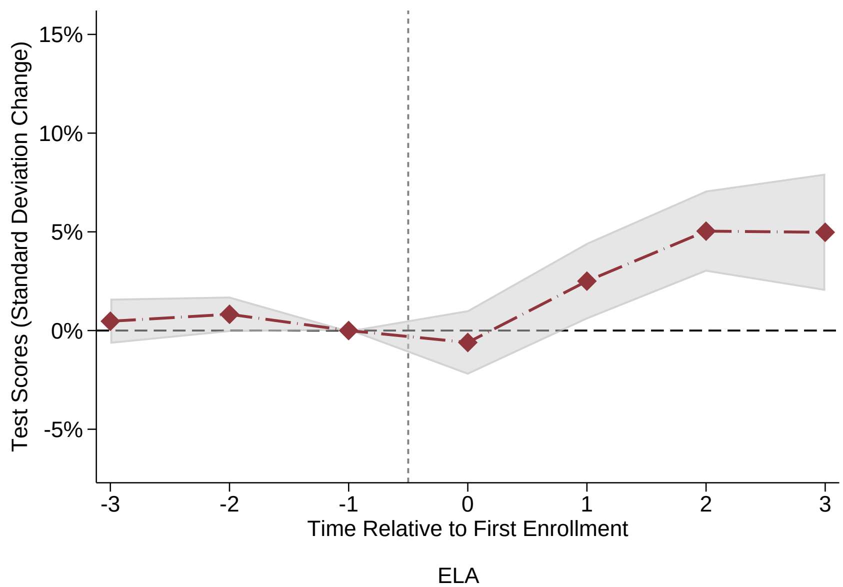
(d) Elementary (e) Middle (f) High

Notes: Figure displays school attendance boundaries for elementary schools (panels a,d), middle schools (panels b,e), and high schools (panels c,f) in LAUSD in 2004 and 2012. Panels (a)-(c) show 2004 attendance boundaries; panels (d)-(f) show 2012. Shaded areas in red denote attendance zones that correspond to schools newly constructed during the sample period from 2002-2012.

Figure 4: Test score effects



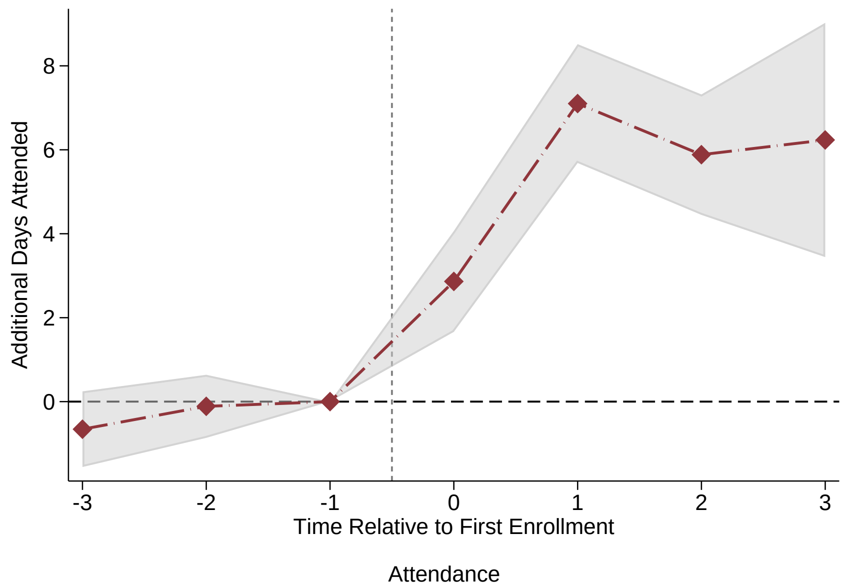
(a) Math



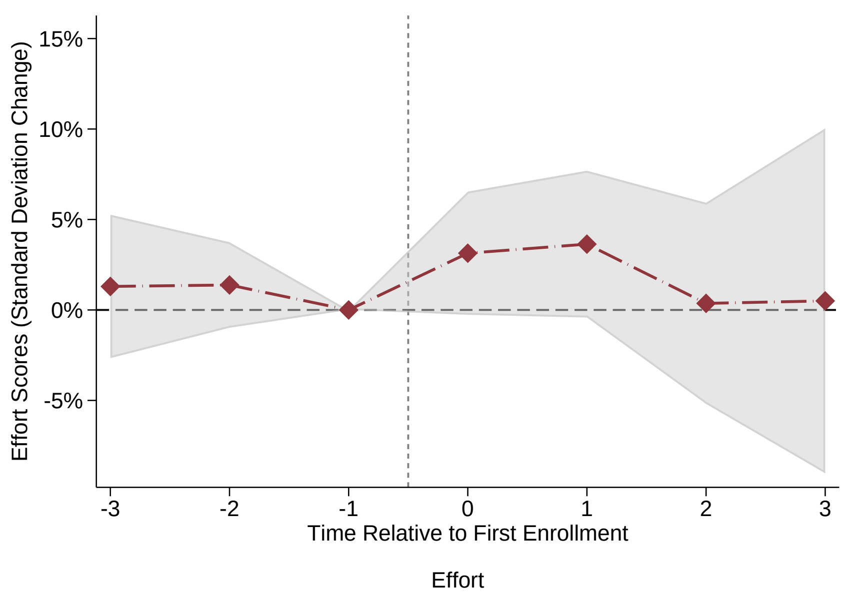
(b) ELA

Notes: Figures shows estimated coefficients from event study regressions following equation (1). Note that total years of exposure is equal to time relative to enrollment, plus one (where non-negative). Dependent variables are standardized math test scores for students in grades 2-7 (panel a) and standardized english-language arts test scores for students in grades 2-11 (panel b). Test scores are standardized relative to the statewide mean and standard deviation for each year-grade-subject exam. The shaded areas denote 95% confidence intervals for the estimated coefficients. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Figure 5: Non-cognitive effects



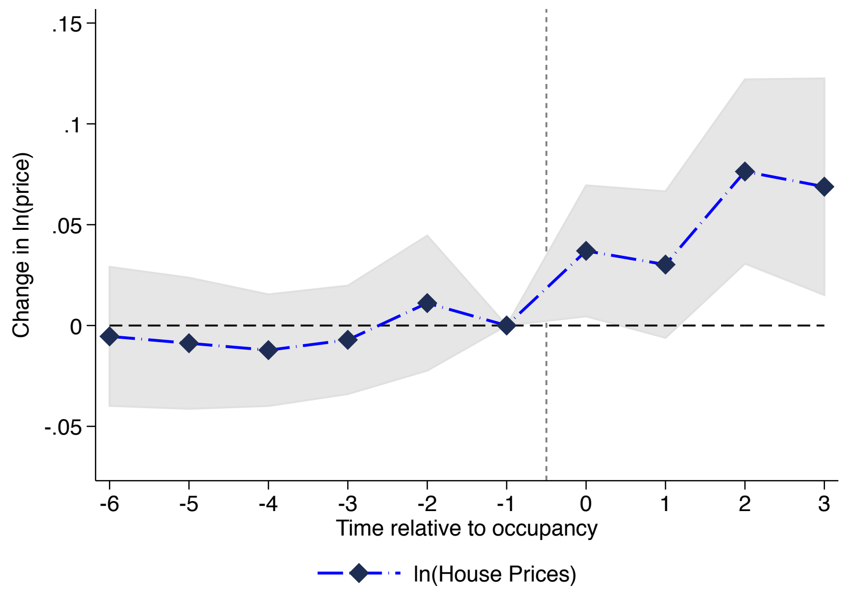
(a) Attended days



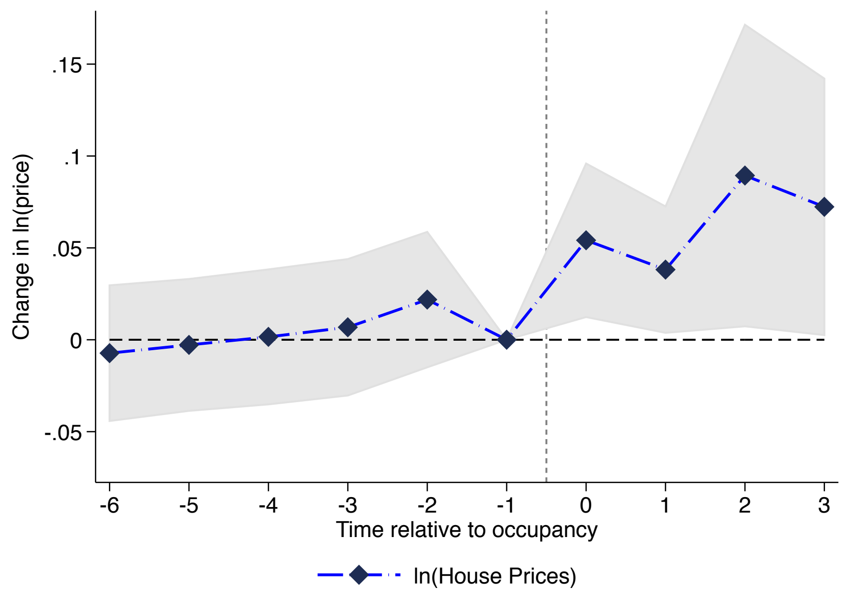
(b) Teacher-reported student effort

Notes: Figures show estimated coefficients from event study regressions following equation (1). Note that total years of exposure is equal to time relative to enrollment, plus one (where non-negative). Dependent variables are annual days attended (panel a) and standardized teacher-reported effort scores for students in grades K-5 (panel b). The shaded areas denote 95% confidence intervals for the estimated coefficients. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Figure 6: House price effects



(a) House prices: Only treated



(b) House prices: All LAUSD

Notes: Figures show estimated coefficients from event-study regressions following equation (4). Dependent variable in both panels is the ln(sale price). In panel (a), only properties that are ever in a new school attendance zone are included in the estimation, corresponding to baseline estimates presented in column (4) of Table 7. In panel (b), all properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column (2) of Table 7. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Tables

Table 1: Summary statistics, new school projects

	Mean	Median	Min	Max
Total cost (million USD)	81.9	56.5	11.1	578.7
New student seats	1,050	800	162	3,440
New classrooms	40.3	32	6	130
Building SQFT	100,585	70,115	12,507	391,840
Completion year	2,008	2,008	2,002	2,012
Site designation to completion (yrs)	5.18	5	2	9
Construction to completion (yrs)	2.12	2	1	5
New School Codes	1.26	1	1	5
Total New School Campuses	114			
Total New School Codes	144			

Notes: Table reports summary statistics for new school project data, at the project level.

Table 2: Summary statistics, LAUSD student data

	All LAUSD	Never Treated	Always Treated	Switchers	Stayers
Free/reduced-price lunch	0.80	0.78	0.79	0.94	0.89
Hispanic	0.73	0.71	0.85	0.89	0.82
Black	0.11	0.12	0.05	0.06	0.08
White	0.09	0.10	0.03	0.03	0.05
Asian	0.04	0.04	0.04	0.01	0.03
Parent: any college	0.27	0.28	0.24	0.16	0.20
English spoken at home	0.33	0.35	0.27	0.18	0.22
Predicted test score	-0.25	-0.23	-0.27	-0.38	-0.33
Math score ($t = -1$)				-0.34	-0.16
ELA score ($t = -1$)				-0.52	-0.37
Days attended ($t = -1$)				153.15	150.56
N student-years	7,317,019	6,495,040	122,045	699,934	1,353,762

Notes: Table reports summary statistics for LAUSD student data, at the student-year level.

Table 3: Student effects, cognitive

	Math						English Language Arts					
	OLS			2SLS			OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New School * Trend	0.027***	0.033***	0.033***	0.026**	0.031**	0.031**	0.017***	0.018***	0.018***	0.024***	0.026***	0.027***
	(0.007)	(0.009)	(0.009)	(0.012)	(0.012)	(0.014)	(0.004)	(0.004)	(0.004)	(0.008)	(0.009)	(0.010)
New School		-0.028	-0.028*		-0.017	-0.016		-0.003	-0.004		-0.006	-0.004
		(0.017)	(0.017)		(0.020)	(0.021)		(0.009)	(0.009)		(0.010)	(0.012)
Trend			0.000			-0.001			0.000			-0.001
			(0.002)			(0.003)			(0.001)			(0.002)
Cumul. Effect	0.081	0.072	0.071	0.079	0.074	0.076	0.050	0.050	0.048	0.072	0.071	0.075
	(0.022)	(0.023)	(.024)	(0.035)	(0.036)	(0.043)	(0.013)	(0.013)	(.014)	(0.025)	(0.025)	(0.031)
Grade FEs	X	X	X	X	X	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X	X	X	X	X
N student-years	2,851,853	2,851,853	2,851,853	2,851,853	2,851,853	2,851,853	4,397,777	4,397,777	4,397,777	4,397,777	4,397,777	4,397,777
N students	724,087	724,087	724,087	724,087	724,087	724,087	945,740	945,740	945,740	945,740	945,740	945,740
N treated students	86,501	86,501	86,501	86,501	86,501	86,501	95,928	95,928	95,928	95,928	95,928	95,928
N treated schools	77	77	77	77	77	77	124	124	124	124	124	124

Notes: Table reports estimates of parametric event study models corresponding to equation (2). Columns 1 and 4 include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Columns 2 and 5 include coefficients for both the immediate effect β_1 and the change in growth β_2; β_3 is constrained to be zero. Columns 3 and 6 include all coefficients, corresponding exactly to the specification in equation (2). Dependent variable is the standardized math test score (grades 2-7) in columns 1-3. In columns 4-6 the dependent variable is the standardized ELA test score (grades 2-11). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table 4: Student effects, non-cognitive

	Days Attended						Effort					
	OLS			2SLS			OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New School	4.736***	3.954***	3.344***	5.150***	4.414***	4.076***	0.025	0.030*	0.032*	0.031	0.036*	0.038*
	(0.547)	(0.651)	(0.673)	(0.568)	(0.685)	(0.768)	(0.015)	(0.016)	(0.017)	(0.019)	(0.019)	(0.021)
New School * Trend		1.197***	0.959**		1.240**	1.099*		-0.009	-0.007		-0.011	-0.009
		(0.445)	(0.449)		(0.579)	(0.620)		(0.013)	(0.013)		(0.018)	(0.021)
Trend			0.280***			0.162			-0.002			-0.002
			(0.096)			(0.142)			(0.004)			(0.006)
Grade FEs	X	X	X	X	X	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X	X	X	X	X
N student-years	5,572,957	5,572,957	5,572,957	5,572,957	5,572,957	5,572,957	2,761,809	2,761,809	2,761,809	2,761,809	2,761,809	2,761,809
N students	1,170,739	1,170,739	1,170,739	1,170,739	1,170,739	1,170,739	692,490	692,490	692,490	692,490	692,490	692,490
N treated students	119,104	119,104	119,104	119,104	119,104	119,104	90,992	90,992	90,992	90,992	90,992	90,992
N treated schools	143	143	143	143	143	143	80	80	80	80	80	80

Notes: Table reports estimates of parametric event study models corresponding to equation (2). Columns 1 and 4 include only the coefficient for the immediate new school effect β_1; β_2 and β_3 are constrained to be zero. Columns 2 and 5 include coefficients for both the immediate effect β_1 and the change in growth β_2; β_3 is constrained to be zero. Columns 3 and 6 include all coefficients, corresponding exactly to the specification in equation (2). Dependent variable is the annual days attended in columns 1-3. In columns 4-6 the dependent variable is the standardized average teacher-reported effort score (grades K-5). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table 5: Student effects, by prior facility conditions

	Math	ELA	Attendance	Effort
Pooled (switchers only)	0.035***	0.014***	3.692***	0.031*
	(0.012)	(0.005)	(0.765)	(0.019)
By share permanent classrooms:				
Low share permanent	0.037***	0.015***	4.505***	0.059***
	(0.013)	(0.005)	(0.799)	(0.018)
High share permanent	0.020	0.006	4.434***	-0.045
	(0.017)	(0.007)	(0.835)	(0.029)
p-value	0.34	0.22	0.93	0.00
By prior building age:				
Below median age	0.025**	0.012**	4.754***	-0.000
	(0.012)	(0.005)	(0.804)	(0.020)
Above median age	0.047***	0.015**	5.296***	0.056**
	(0.017)	(0.006)	(0.811)	(0.025)
p-value	0.19	0.62	0.50	0.03
By prior building FCI:				
Low FCI	0.033*	0.010*	6.085***	-0.002
	(0.018)	(0.005)	(0.930)	(0.031)
High FCI	0.034***	0.015**	4.103***	0.038*
	(0.013)	(0.006)	(0.696)	(0.020)
p-value	0.96	0.52	0.02	0.22

Notes: Table reports estimates of one-parameter event study models corresponding to equation (2), where only the sample of students observed at an old and a new school (i.e. "switchers") are included. Columns 1 and 2 include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Columns 3 and 4 include coefficients only the coefficient for the immediate effect β_1; β_2 and β_3 are constrained to be zero.Dependent variables are standardized math test scores (column 1), standardized english-language arts test scores (column 2), annual days attended (column 3), and standardized average teacher-reported effort scores (column 4). Panel (a) reports baseline estimates for those students who are observed switching from an existing to a new school facility. The remaining panels show coefficients on the interactions for being below or above the median in terms of prior school share permanent classrooms (panel b), prior school age (panel c), and prior school FCI (panel d). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table 6: Summary statistics, LA County assessor data

	All LAUSD	New School Zones	Existing School Zones
Sale price (2015$)	565,801	416,509	636,010
Building SQFT	1,664	1,539	1,722
Number of bedrooms	2.9	2.9	2.8
Number of bathrooms	2.2	2.1	2.3
Building age	44	45	44
Effective age	39	40	39
Useable lot SQFT	5,238	5,704	5,018
N property sales	505,835	161,795	344,040
N properties	350,299	115,247	235,052

Notes: Table reports summary statistics for LA County Assessor data, at the property sale level.

Table 7: House price effects

	Neighborhood Fixed Effects				Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
New School	0.060***	0.059***	0.044***	0.054***	0.045***	0.059***
	(0.018)	(0.016)	(0.011)	(0.015)	(0.013)	(0.016)
Month FEs	X	X	X	X	X	X
Yr-HSZ FEs	X	X	X		X	
Yr FEs				X		X
Sch Zone FEs	X	X	X	X		
Prop Controls	X	X	X	X		
Prop FEs					X	X
New Sch Zones	X	X	X	X	X	X
All LAUSD	X					
w/in 1km		X				
Number of sales	505,715	255,457	161,766	161,792	87,516	87,557
R2	.82	.79	.78	.75	.91	.9

Notes: Table reports estimated coefficients from difference-in-difference regressions following equations (3) and (5). Dependent variable is the ln(sale price). Columns 1-4 report estimates from equation (3), including neighborhood fixed effects and property specific controls. Columns 5 and 6 report estimates from equation (5), including property fixed effects. Columns 4 and 6 report estimates using year fixed effects; the remaining columns include year-by-high school zone fixed effects in estimation. In column 1, all properties in LAUSD in the sample are included. Column 2 restricts the sample to include only properties within a new school zone or within a 1km of a new school zone (by 2012). Columns 3-6 include only properties within a new school zone by 2012: "never-treated" properties are excluded from estimation. Standard errors are clustered by neighborhood.

Table 8: Cost-benefit analysis

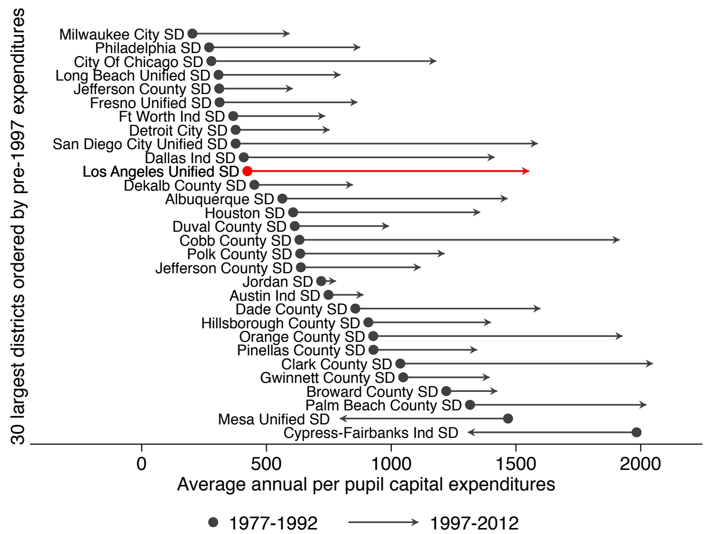
Program component	Parameter	Value
Program cost		
Households in LAUSD	N	1.52 million
Share treated households	$1/n$	0.328
Per treated household cost	τn	\$18,430
Total program cost	τN	**\$9.17 billion**
Program benefit		
1. Housing capitalization approach		
Estimated house price in treated areas	$E[\theta_j]$	\$28,201
Total real estate valuation	$N \cdot E[\theta_j]$.	**\$14.06 billion**
Marginal value of public funds (capitalization)	MVPF^C	**1.53**
2. Later life earnings approach		
Implied later life earnings per treated household	$E[\beta_j]W$	\$7,782
Total earnings valuation	$N \cdot E[\beta_j]W$	**\$3.88 billion**
Marginal value of public funds (earnings)	MVPF^E	**0.42**
3. Hybrid approach		
Share housing valuation due to academic achievement	γ	0.22
Share future earnings captured in academic valuation	MRS^Y/W	0.76
Program benefit per treated household	$(1-\gamma)E[\theta_j] + E[\beta_j]W$	\$29,786
Total benefits	$N\left[(1-\gamma)E[\theta_j] + E[\beta_j]W\right]$	**\$14.85 billion**
Marginal value of public funds	MVPF	**1.62**

Notes: Table reports values and estimates of model parameters introduced in section 5.

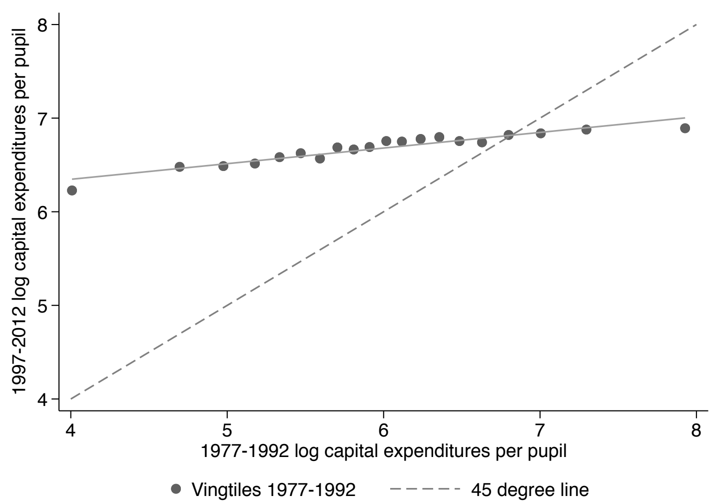
A Appendix Figures and Tables

A.1 Appendix Figures:

Figure A1: LAUSD Capital Spending in Context



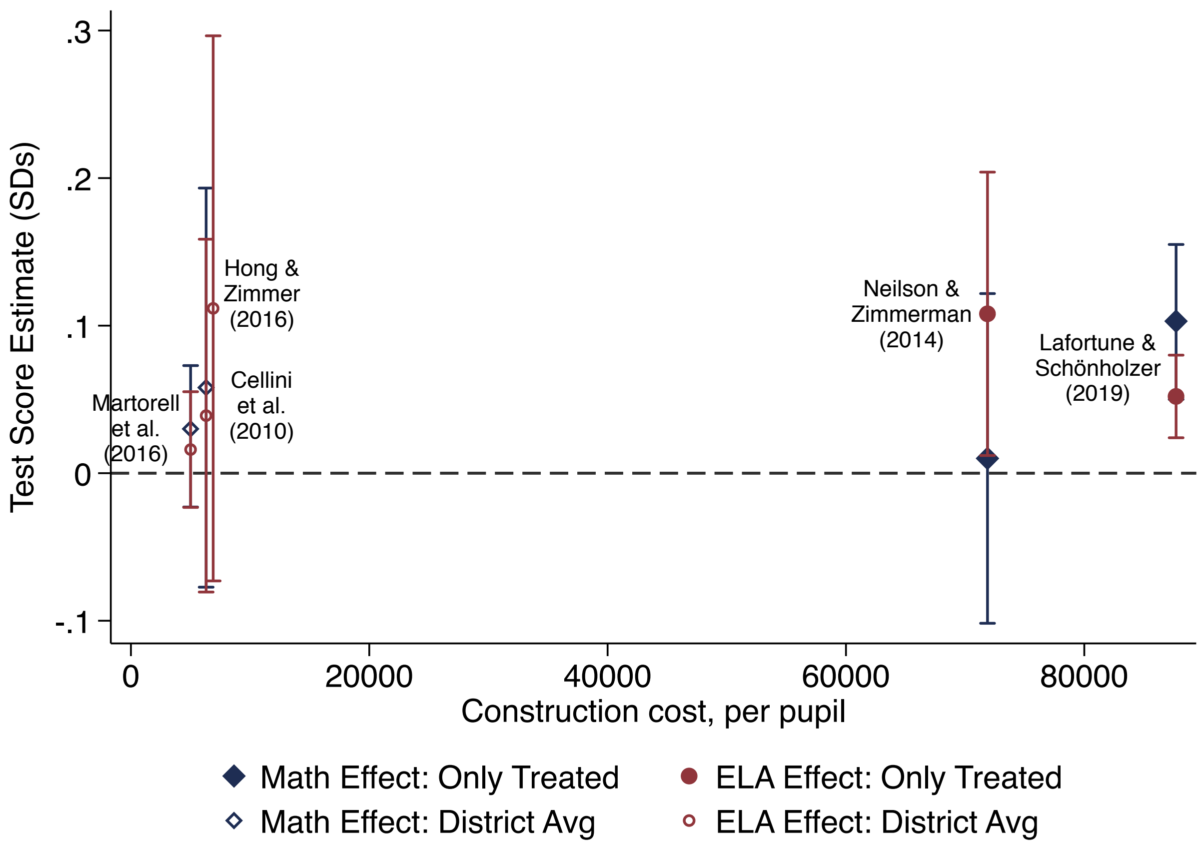
(a) School capital spending for 30 largest districts



(b) Binned means of capital spending for 1977-1992 against 1997-2012

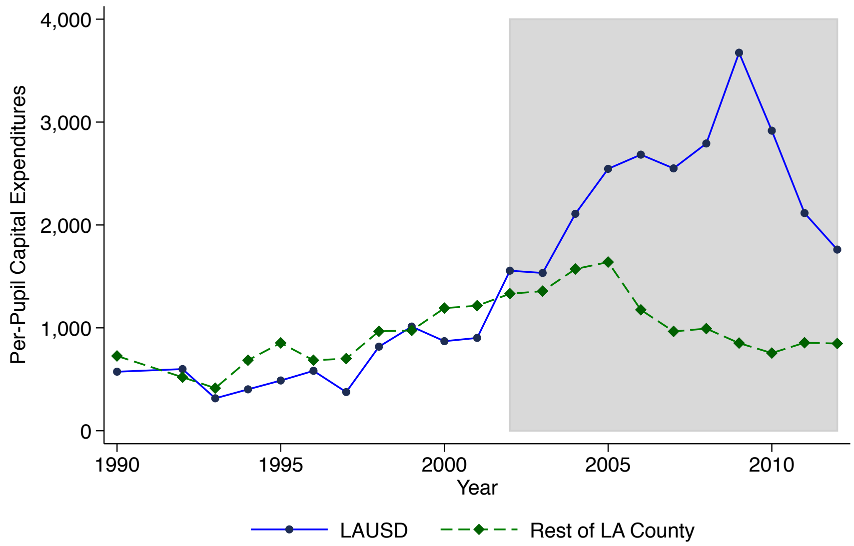
Notes: Top panel shows average per-pupil capital spending for the thirty largest school districts in the U.S. (other than the New York district, which is a dependent agency of the City of New York) over the period 1977-1992 and 1997-2012. Bottom panel shows means in 5% percentile bins (vingtiles) of 1977-1992 log capital expenditures per pupil across all districts against their 1997-2012 log capital expenditures per pupil.

Figure A2: Student effects comparison from capital expenditure literature

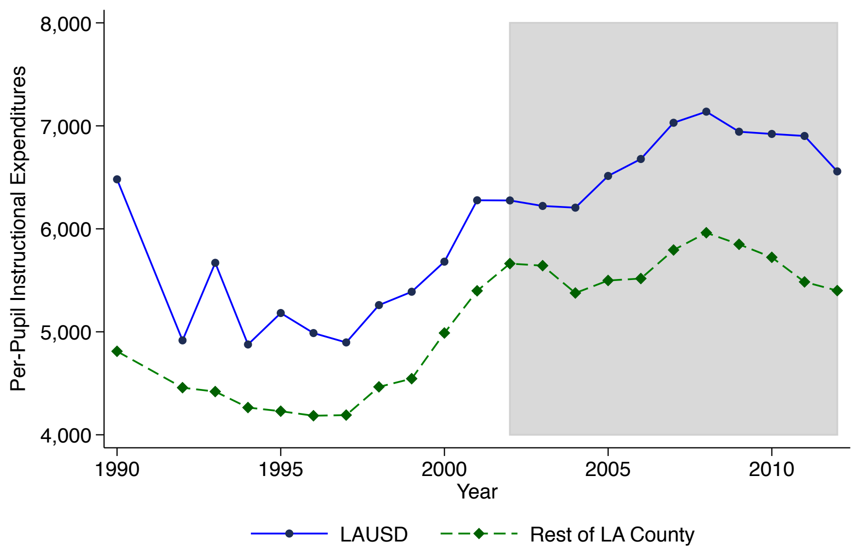


Notes: Figure plots estimated coefficients from related papers in economics evaluating the effects of school capital expenditures (y axis) against per-pupil expenditures in each study (x axis). Blue diamond shaped markers denote math test score estimates whereas red circular markers denote English / Language Arts test score estimates (both in standard deviation units). Solid markers denote estimates on directly treated students from Neilson and Zimmerman (2015) and Lafortune and Schönholzer (2019), 4 years after school construction or student occupancy, respectively. For these studies, construction cost is calculated per treated pupil. Hollow markers denote estimates from studies examining district average test scores after passage of a capital construction bond, where construction cost per pupil is the average over all students in the district. For these studies, estimates 6 years after bond passage are reported.

Figure A3: Spending per pupil, LAUSD vs LA County



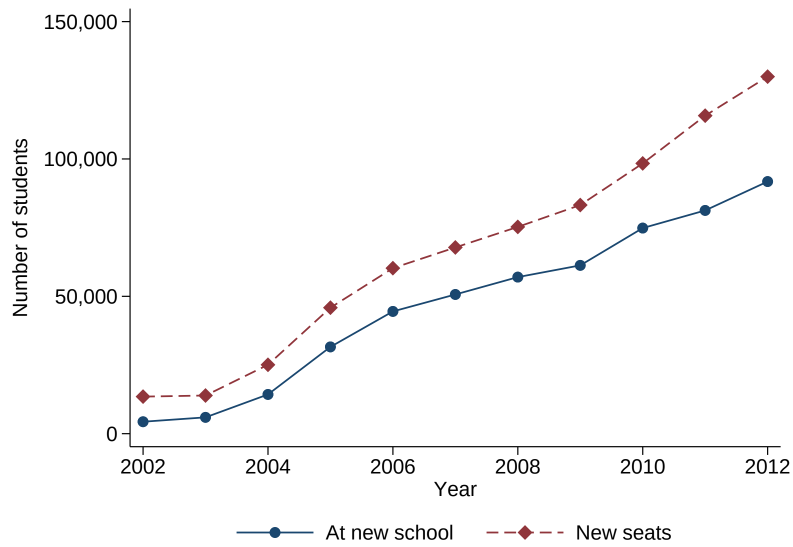
(a) Capital



(b) Instructional

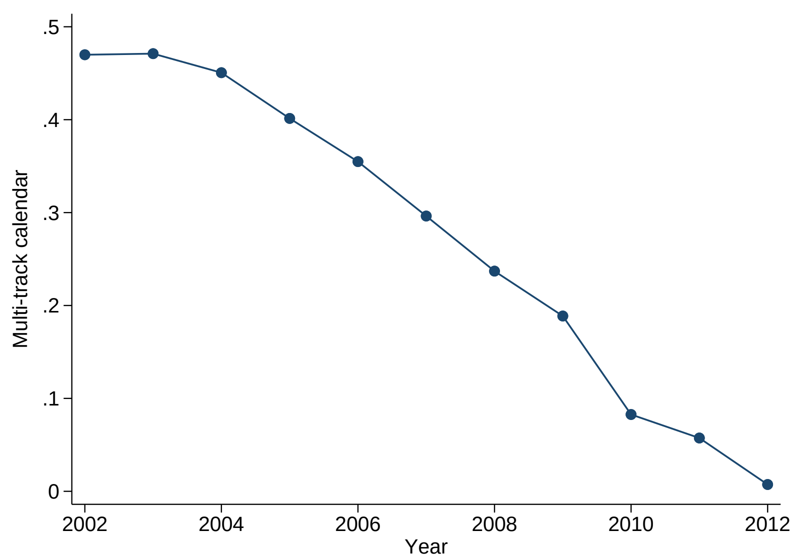
Notes: Panel (a) shows per-pupil capital expenditures and panel (b) shows per-pupil instructional expenditures. Expenditures are expressed in real 2013 dollars. In both panels, the expenditures for LAUSD (solid blue line) and the student-weighted average of all other LA County public school districts (dashed green line) are shown. The shaded area from 2002-2012 shows the treatment period covered in the main analysis. Expenditure data were from the National Center for Education Statistics (NCES) annual census of school districts and from the Census of Governments.

Figure A4: Students at newly constructed schools



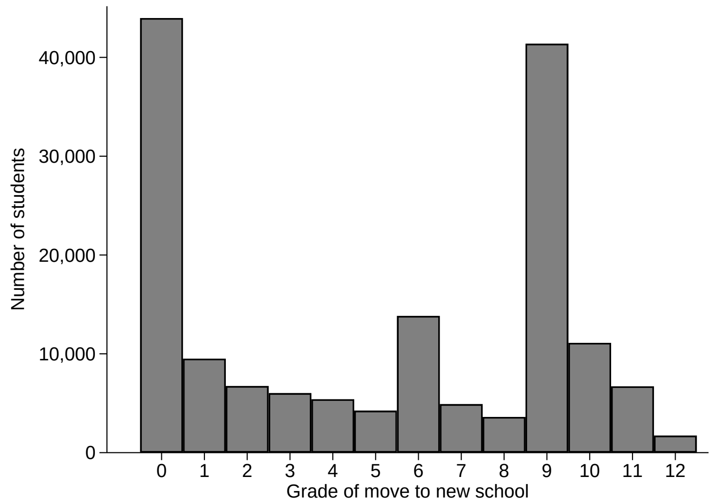
Notes: Figure shows time series of total new seats (from new construction project database) and the number of students attending newly constructed school facilities (from the student microdata).

Figure A5: School age and multi-track calendars in LAUSD, by year and student race



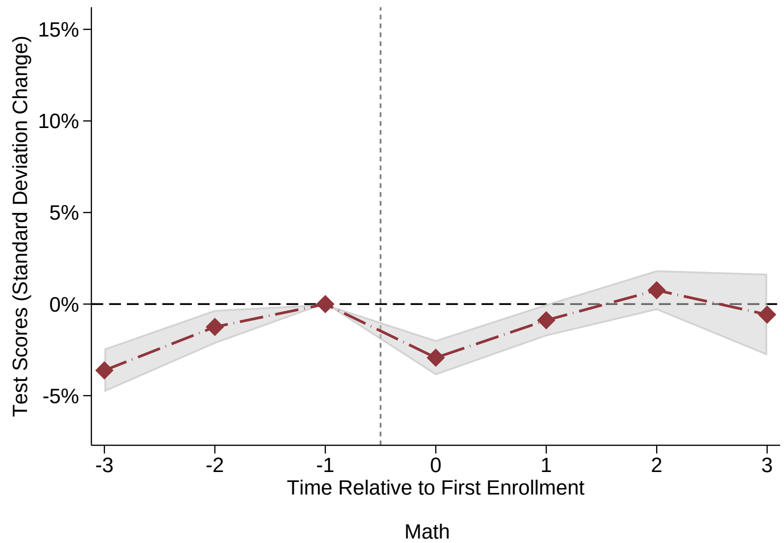
Notes: Figure reports proportion of students attending a school on a multi-track calendar, by year.

Figure A6: Grade of switch to new school facilities

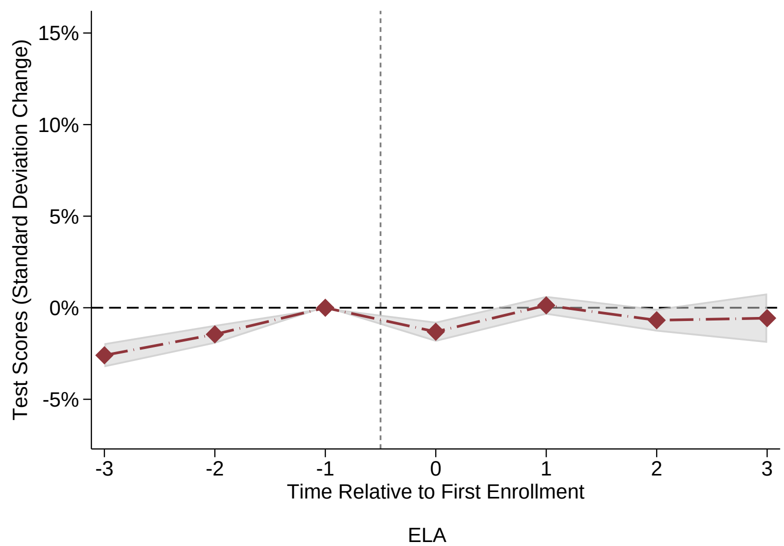


Notes: Grade of student switch to new school facility. Y-axis reports number of student observations.

Figure A7: Student switching, non-new facility related



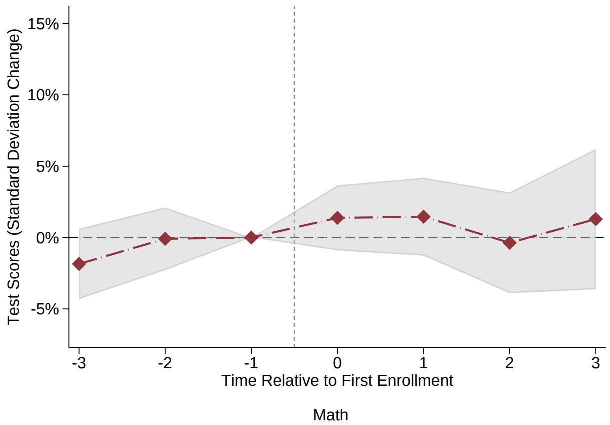
(a) Math



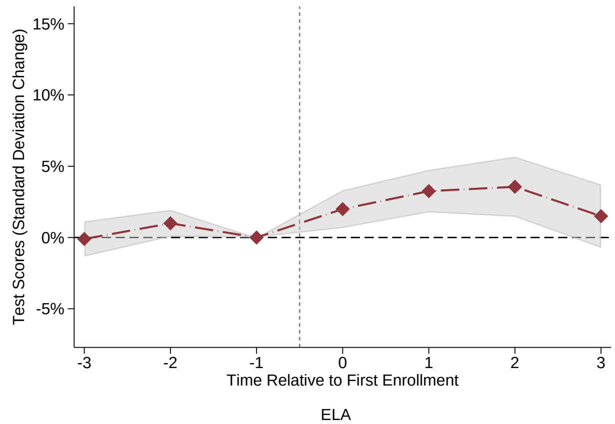
(b) ELA

Notes: Figures show estimated coefficients from event study regressions following equation (1), for students who switch schools for reasons unrelated to new school facilities. Dependent variables are standardized math test scores for students in grades 2-7 (panel a) and standardized english-language arts test scores for students in grades 2-11 (panel b). Test scores are standardized relative to the statewide mean and standard deviation for each year-grade-subject exam. The shaded areas denote 95% confidence intervals for the estimated coefficients. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

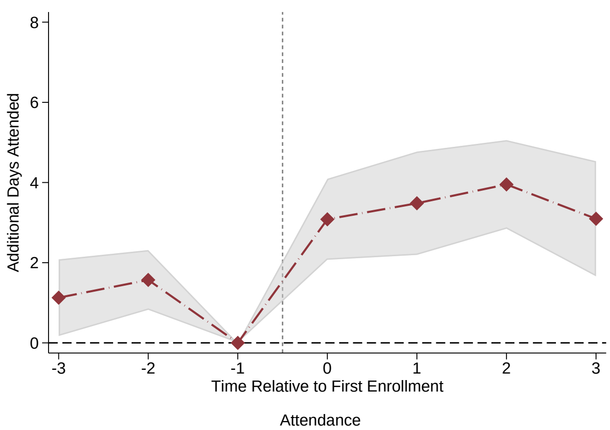
Figure A8: Student effects: Stayers



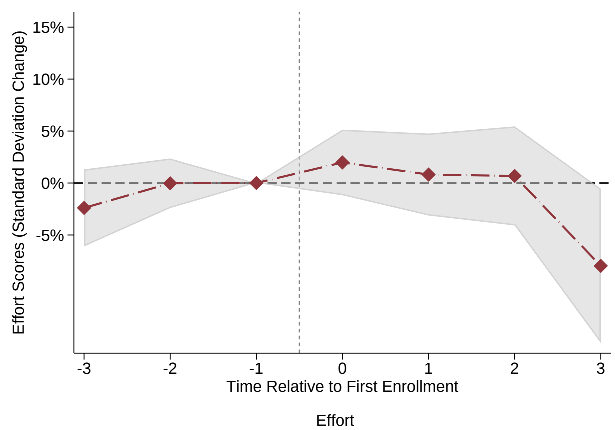
(a) Test scores: Math



(b) Test scores: ELA



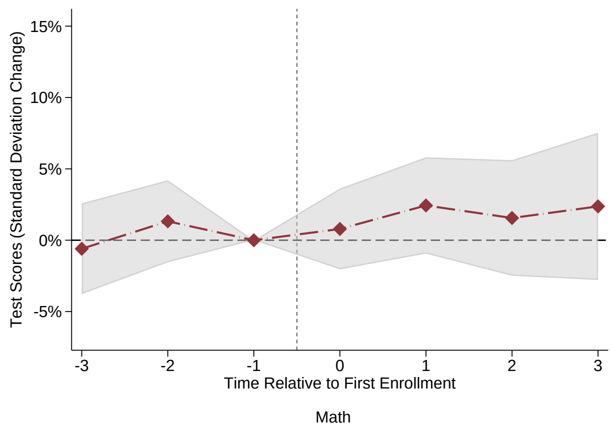
(c) Attended days



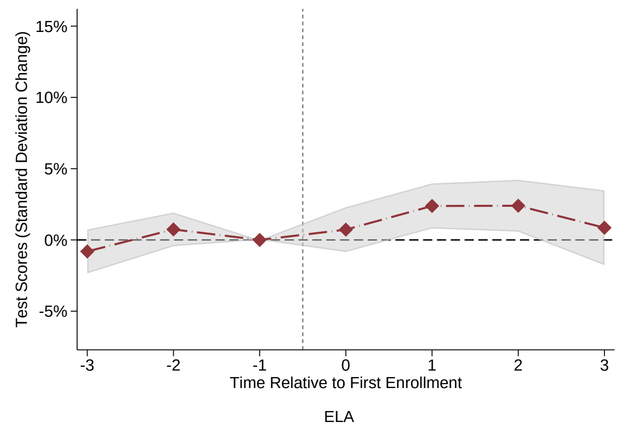
(d) Teacher-reported student effort

Notes: Figures show estimated coefficients from event study regressions following equation (1) for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Event time is centered relative to the year of the peer outflow. Dependent variables are standardized math test scores for students in grades 2-7 (panel a), standardized english-language arts test scores for students in grades 2-11 (panel b), annual days attended (panel c), and standardized teacher-reported effort scores for students in grades K-5 (panel d). The shaded areas denote 95% confidence intervals for the estimated coefficients. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

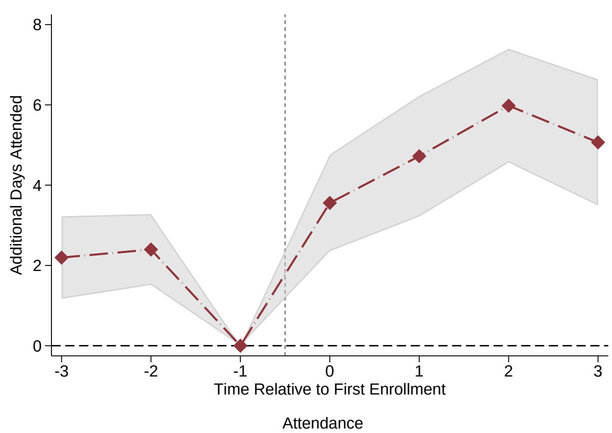
Figure A9: Student effects: Stayers, 20% cohort exit threshold



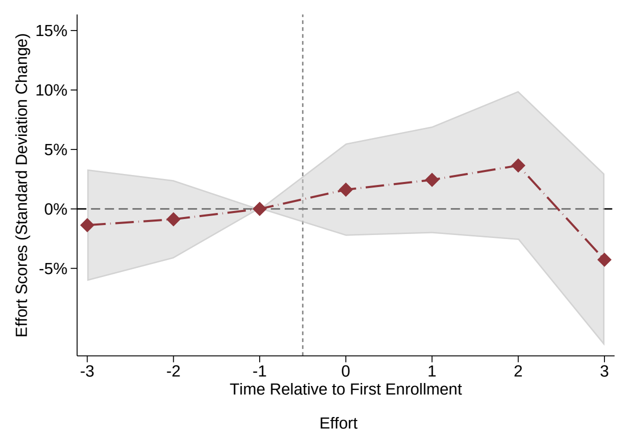
(a) Test scores: Math



(b) Test scores: ELA



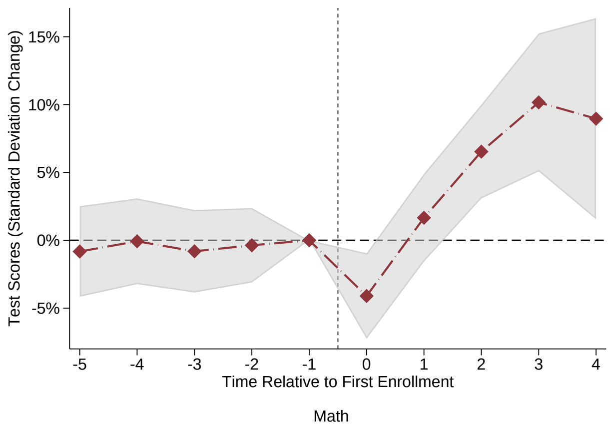
(c) Attended days



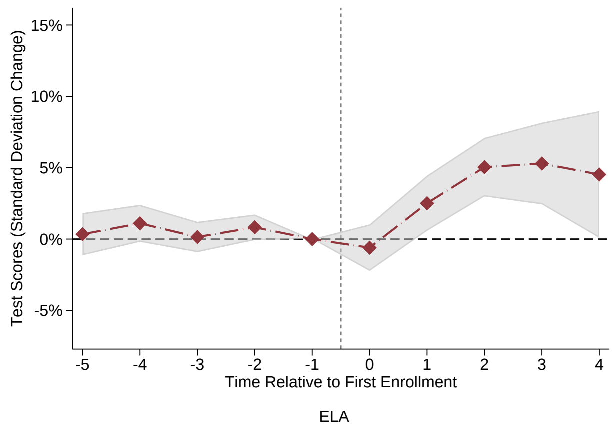
(d) Teacher-reported student effort

Notes: Figures show estimated coefficients from event study regressions following equation (1), for students that had 20% or more of their school-grade cohort exit to a newly constructed school. Figures are analogous to Figure A8, with the threshold for "stayers" raised from 10% to 20% of a student's cohort. Event time is centered relative to the year of the peer outflow. Dependent variables are standardized math test scores for students in grades 2-7 (panel a), standardized english-language arts test scores for students in grades 2-11 (panel b), annual days attended (panel c), and standardized teacher-reported effort scores for students in grades K-5 (panel d). The shaded areas denote 95% confidence intervals for the estimated coefficients. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

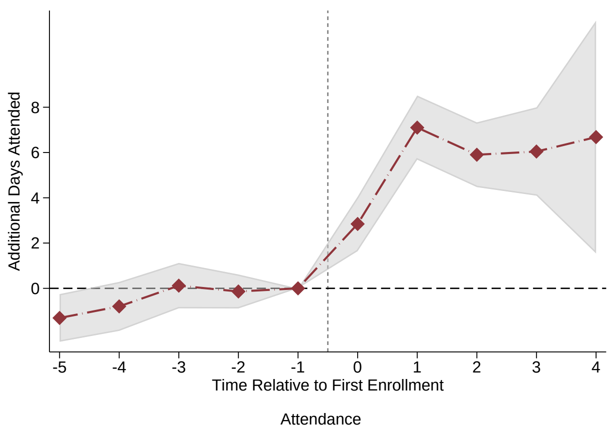
Figure A10: Student effects: Extended event-time window



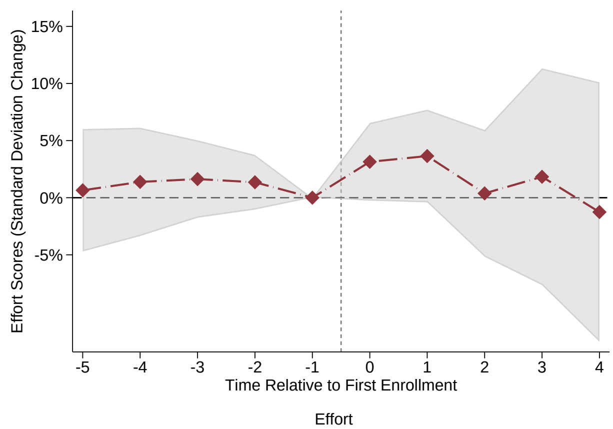
(a) Test scores: Math



(b) Test scores: ELA



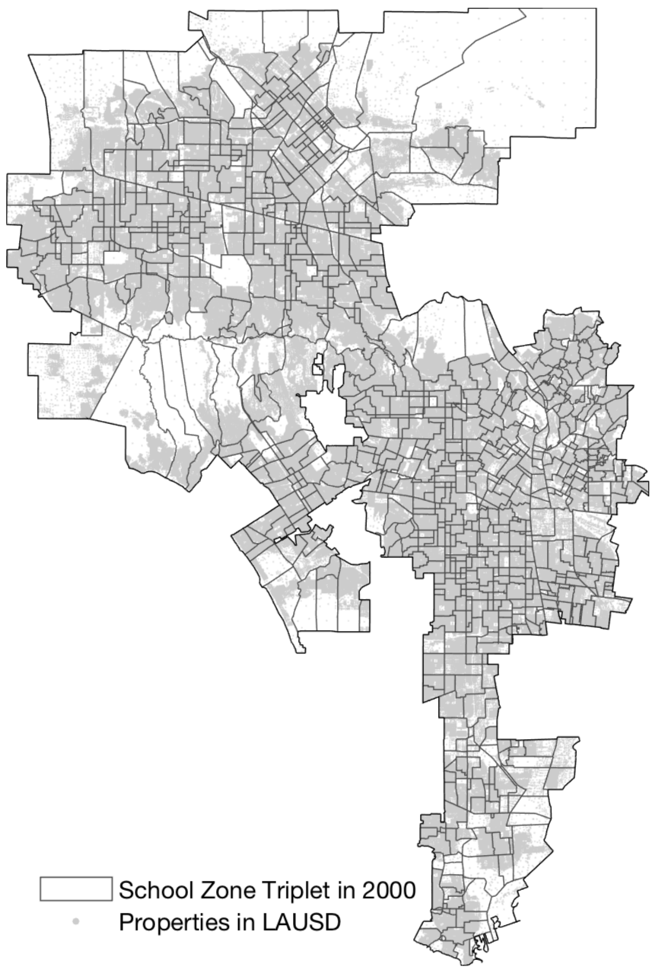
(c) Attended days



(d) Teacher-reported student effort

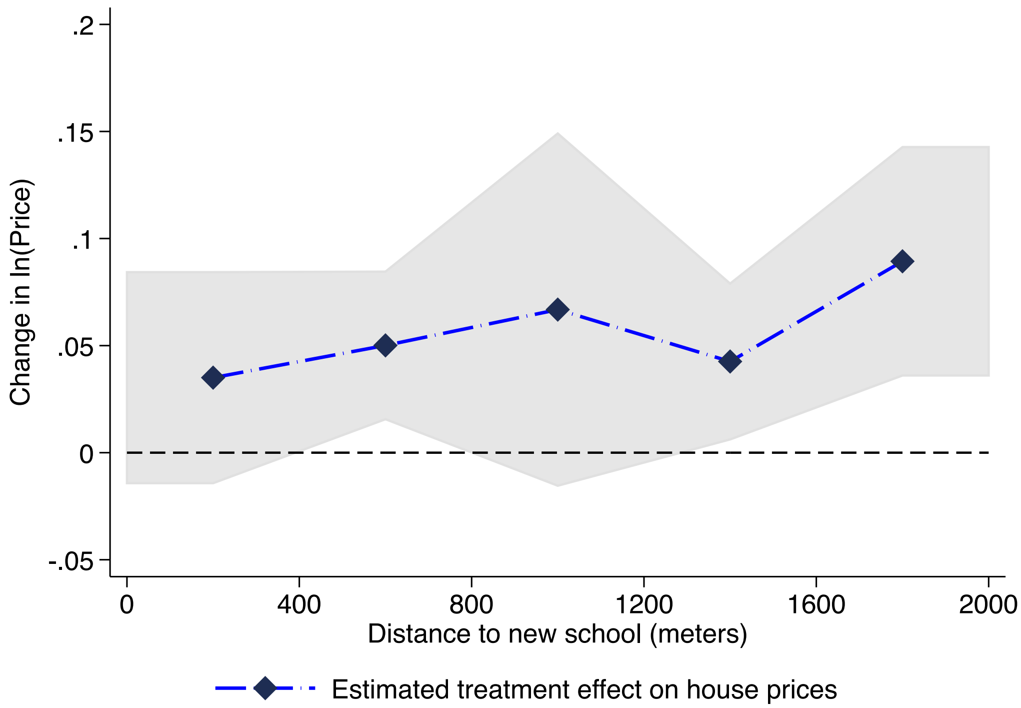
Notes: Figures show estimated coefficients from event study regressions following equation (1). Figures are analogous to Figures 4 and 5, except that event-time endpoints are extended to include additional years of data. Note that total years of exposure is equal to time relative to enrollment, plus one (where non-negative). Dependent variables are standardized math test scores for students in grades 2-7 (panel a), standardized english-language arts test scores for students in grades 2-11 (panel b), annual days attended (panel c), and standardized teacher-reported effort scores for students in grades K-5 (panel d). The shaded areas denote 95% confidence intervals for the estimated coefficients. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Figure A11: Neighborhood boundaries in LAUSD, based on 2000 school zones



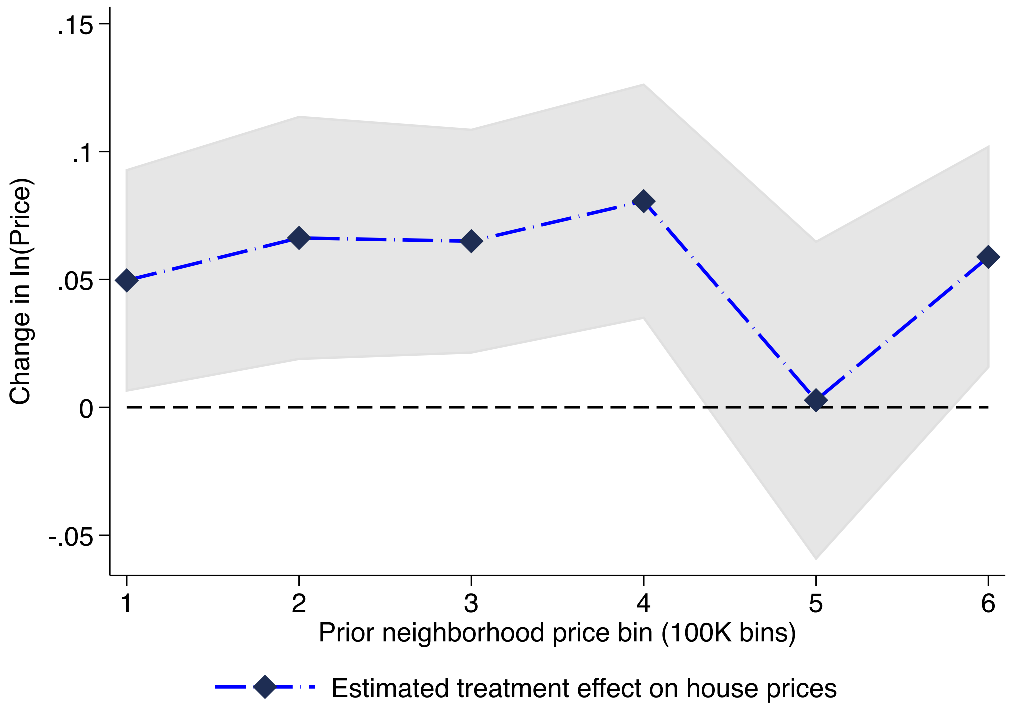
Notes: Figure shows school assignment zone triplets in LAUSD using 2000 assignment boundaries, which are used to define neighborhoods in the estimation of real estate effects. Solid lines denote neighborhood boundaries. Each gray dot represents one property from the LA County Assessor data.

Figure A12: Spillovers: Effects by distance to new school



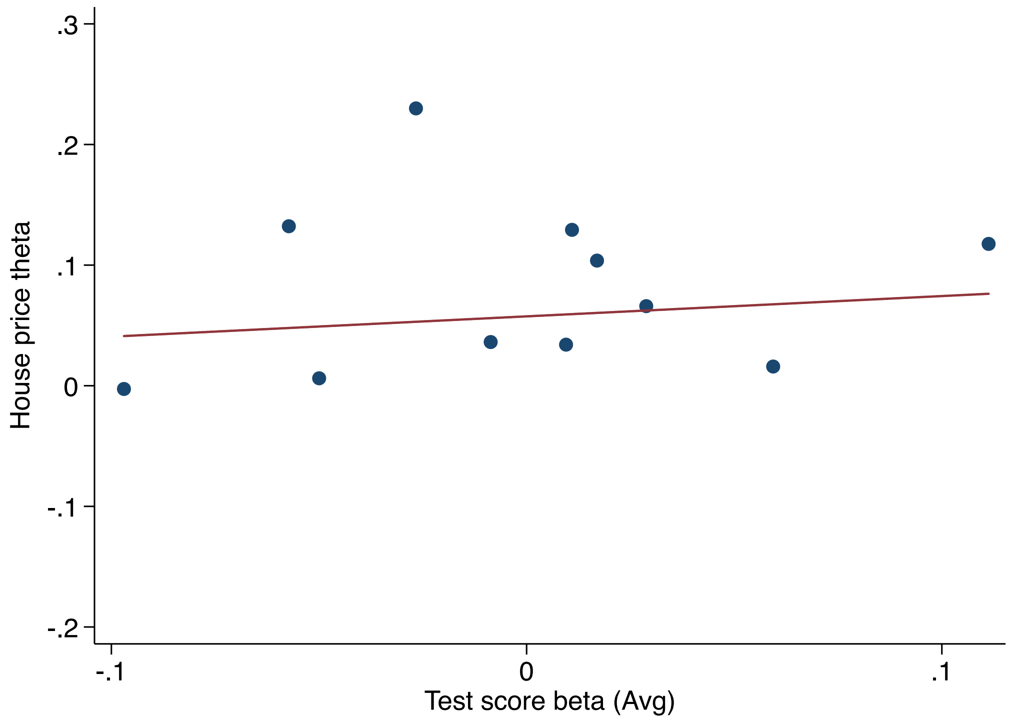
Notes: Figure shows estimated coefficients from a difference-in-difference regression based on equation (3), where the treatment indicator is interacted with indicators for 400 meter bins of distance to the new school in 2012. Each point reports the estimated coefficient for the treatment indicator interacted with the corresponding distance bin. Points are located at the midpoint of each distance bin (i.e. the estimate at 200m corresponds to the 0-400m distance bin). All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 2 of Table 7. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure A13: Heterogeneity: By neighborhood mean prior house prices



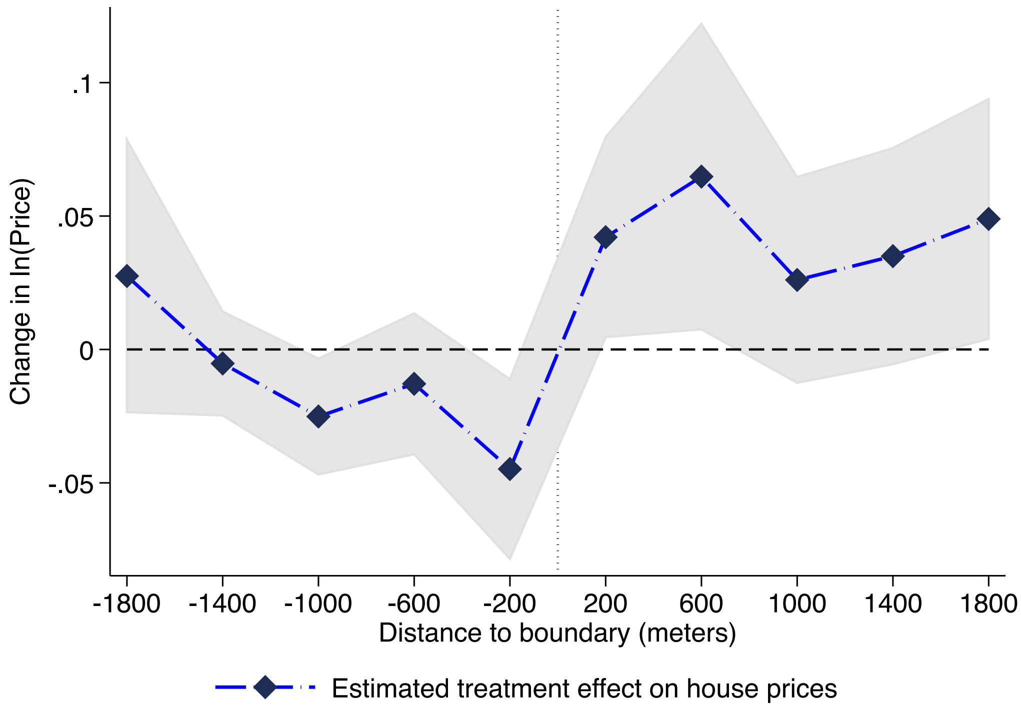
Notes: Figure shows estimated coefficients from a difference-in-difference regression based on equation (3), where the treatment indicator is interacted with indicators for $100,000 bins of prior neighborhood average prices. Bin 1 also includes average neighborhood house prices less than $100K, while bin 6 includes all neighborhoods with average house prices above $600K; all other bins only include a $100K range. Prior neighborhood average house prices are calculated using data from pre-construction property sales from 1995-2001. All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 2 of Table 7. All specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure A14: Correlation between house price and test score effects



Notes: Figure shows binned scatterplot of average estimated school-level test score gains against estimated house price effects in the corresponding school attendance zone. Points and regression lines are weighted by the number of properties in each attendance zone. School-specific test score effect estimates are shrunken towards the mean overall effect via Empirical Bayes. The point estimate on the regression line is 0.17 (SE 0.30).

Figure A15: Spillovers: Effects by distance to school attendance boundary



Notes: Figure shows estimated coefficients from a difference-in-difference regression based on equation (4), where the treatment indicator is interacted with indicators for 400 meter bins of distance to the new school attendance zone in 2012. Properties with positive (negative) distance are inside (outside) the new school attendance zones. Properties outside the attendance zone and within 2 km of a new school attendance zone are assigned the construction date corresponding to the nearest new school attendance zone boundary. Each point reports the estimated coefficient for the treatment indicator interacted with the corresponding distance bin. Points are located at the midpoint of each distance bin (i.e. the estimate at 200m corresponds to the 0-400m distance bin). All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column (2) of Table 7. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Appendix Tables

Table A1: Student effects, "staying" students

	Math Score		ELA Score		Days Attended		Effort Score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post * Trend: Stayers	-0.002	-0.009*	0.005*	0.000		0.078		-0.030***
	(0.005)	(0.005)	(0.003)	(0.003)		(0.192)		(0.010)
Post: Stayers		0.008		0.019***	3.089***	2.340***	0.013	0.014
		(0.011)		(0.007)	(0.433)	(0.543)	(0.017)	(0.016)
Trend: Stayers		0.006***		0.001		0.220*		0.008**
		(0.002)		(0.001)		(0.116)		(0.004)
Grade FEs	X	X	X	X	X	X	X	X
PLD-Yr FEs	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X
N student-years	2,475,534	2,475,534	3,864,574	3,864,574	4,898,159	4,898,159	2,379,587	2,379,587
N students	640,752	640,752	855,498	855,498	1,060,585	1,060,585	604,310	604,310
N treated students	146,255	146,255	166,614	166,614	180,403	180,403	121,221	121,221
N treated cohort	24,064	24,064	30,765	30,765	38,713	38,713	29,062	29,062

Notes: Table reports estimates of parametric event study models corresponding to equation (2), for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Event time is centered relative to the year of the peer outflow. Columns 1 and 2 include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Columns 5 and 7 include coefficients only the coefficient for the immediate effect β_1; β_2 and β_3 are constrained to be zero. Columns 2, 4, 6, and 8 include all coefficients, corresponding exactly to the specification in equation (2). Dependent variable is the standardized math test score (grades 2-7) in columns 1-2, the standardized ELA test score (grades 2-11) in columns 3-4, annual days attended in columns 5-6, and the standardized average teacher-reported effort score in columns 7-8. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A2: School-level changes

(a) Switching students

	Calendar		School		Peers	
	(1) Multi-track	(2) Max days	(3) Age	(4) Stu/tch	(5) Peers: Bl/Hisp	(6) Peers: predicted
New School	-0.275***	0.996***	-71.389***	0.243***	0.021***	-0.013**
	(0.027)	(0.209)	(1.294)	(0.089)	(0.003)	(0.005)
Grade FEs	X	X	X	X	X	X
PLD-Yr FEs						
Stu FEs	X	X	X	X	X	X
N student-years	6,414,594	5,847,450	6,313,348	2,891,902	6,416,430	4,252,570
N students	1,244,591	1,201,715	1,233,194	724,529	1,244,899	903,868
N treated students	127,389	126,196	127,327	93,929	127,477	96,144
N treated schools	143	141	143	80	143	124
R2	0.72	0.44	0.80	0.74	0.89	0.87

(b) Staying students

	Calendar		School		Peers	
	(1) Multiple	(2) Max days	(3) Age	(4) Stu/tch	(5) Peers: Bl/Hisp	(6) Peers: predicted
Post: Stayers	-0.212***	0.944***	1.602**	-0.151	-0.013***	0.019***
	(0.024)	(0.263)	(0.803)	(0.125)	(0.003)	(0.004)
Grade FEs	X	X	X	X	X	X
PLD-Yr FEs	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X
N student-years	5,656,017	5,143,417	5,558,391	2,497,392	5,657,185	3,732,943
N students	1,133,137	1,090,312	1,121,035	633,945	1,133,391	817,334
N treated students	184,744	183,023	184,286	126,343	184,755	161,503
N treated schools	804	794	755	503	804	786
R2	0.73	0.46	0.70	0.75	0.89	0.88

Notes: Table reports estimates of models corresponding to one-parameter versions of equation (2), where only the coefficient for the immediate new school effect β_1 is included; β_2 and β_3 are constrained to be zero. Dependent variables are multi-track status (column 1), total instructional days (column 2), school age (column 3), class size (i.e. pupils per teacher) for students in grades K-5 (column 4), school leave-out mean proportion black and/or hispanic (column 5), and school leave-out mean predicted test scores (column 6). Panel (a) reports estimates for students attending new school facilities. Panel (b) reports analogous estimates for staying students: here β_1 is an indicator for having experienced a 10% or greater school-grade cohort exit to a newly constructed school. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A3: Teacher changes at new schools

	Demographics			VA Average (pre-switch)		VA Gap (new-veteran)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Experience	MA+	Pr(New)	Math	ELA	Math	ELA
New School	-3.018***	0.052***	0.054***	-0.002	-0.025*	-0.014	-0.024***
	(0.270)	(0.016)	(0.006)	(0.006)	(0.015)	(0.012)	(0.008)
Grade FEs	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X
N student-years	5,565,255	5,565,883	5,710,079	3,032,969	4,629,725	1,987,918	2,403,367
N students	1,179,506	1,179,539	1,156,796	767,092	1,005,599	582,502	659,759
N treated students	125,229	125,242	126,949	92,269	103,796	78,464	85,590
N treated schools	137	137	143	75	116	63	62
R2	0.47	0.33	0.29	0.58	0.41	0.34	0.32

Notes: Table reports estimates corresponding to one-parameter versions of equation (2), where only the coefficient for the immediate new school effect (β_1) is included; β_2 and β_3 are constrained to be zero. Dependent variables are teacher age (column 1), teacher years experience (column 2), an indicator for having a masters degree or higher (column 3), and an indicator for having a new teacher in either math or ELA (column 4). Columns 4-7 reports estimates where dependent variables are school-year averages of teacher value added: in columns 4 and 5 dependent variables are average value-added scores based on prior-year observations at existing school facilities in math and ELA, respectively. In columns 6 and 7 dependent variables are the school year gap in mean value-added between novice and experienced teachers in math and ELA, respectively. See Appendix C.2.1 for further detail on computation of teacher and school-level value-added variables. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A4: Teacher changes at existing schools

	Demographics			VA Average (pre-switch)		VA Gap (new-veteran)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Experience	MA+	Pr(New)	Math	ELA	Math	ELA
Post: Stayers	0.915***	0.000	-0.009**	-0.012*	-0.003	-0.005	-0.012
	(0.203)	(0.013)	(0.004)	(0.007)	(0.008)	(0.021)	(0.011)
Grade FEs	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X
N student-years	5,565,255	5,565,883	5,710,079	2,363,804	2,968,727	1,987,918	2,403,367
N students	1,179,506	1,179,539	1,156,796	653,847	739,855	582,502	659,759
N treated students	182,911	182,917	182,705	137,737	151,288	130,449	144,667
N treated schools	772	772	804	616	727	602	614
R2	0.47	0.33	0.29	0.30	0.33	0.34	0.32

Notes: Table reports estimates corresponding to one-parameter versions of equation (2), for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Only the coefficient for having experienced a 10% or greater school-grade cohort exit is included (β_1); β_2 and β_3 are constrained to be zero. Dependent variables are teacher age (column 1), teacher years experience (column 2), an indicator for having a masters degree or higher (column 3), and an indicator for having a new teacher in either math or ELA (column 4). Columns 4-7 report estimates where dependent variables are school-year averages of teacher value added: in columns 4 and 5 dependent variables are average value-added scores based on prior-year observations at existing school facilities in math and ELA, respectively. In columns 6 and 7 dependent variables are the school year gap in mean value-added between novice and experienced teachers in math and ELA, respectively. See Appendix C.2.1 for further detail on computation of teacher and school-level value-added variables. All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A5: Principal experience

	(1) Exper (Dist)	(2) Exper (Sch)	(3) New (Dist)	(4) New (Sch)
New School	-0.867***	-1.104***	0.148***	0.208***
	(0.150)	(0.103)	(0.024)	(0.021)
Grade FEs	X	X	X	X
PLD-Yr FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	5,319,931	5,319,931	5,319,931	5,319,931
N students	1,119,114	1,119,114	1,119,114	1,119,114
N treated students	131,098	131,098	131,098	131,098
N treated schools	134	134	134	134
R2	0.64	0.54	0.52	0.45

Notes: Table reports estimates corresponding to one-parameter versions of equation (2), where only the coefficient for the immediate new school effect β_1 is included; β_2 and β_3 are constrained to be zero. Dependent variables are within-district principal experience (column 1), within-school principal experience (column 2), an indicator for having a new principal (new to the district) in a given year (column 3), and an indicator for having a new principal (new to the school) in a given year (column 4). Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table A6: Student effects, adjusted for changes in school characteristics

Controls	(1) Math	(2) ELA	(3) Attendance	(4) Effort
None	0.029***	0.015***	5.464***	0.018
	(0.008)	(0.004)	(0.602)	(0.018)
Predicted peer characteristics	0.028***	0.015***	5.438***	0.017
	(0.008)	(0.004)	(0.509)	(0.018)
Teacher fixed effects	0.021***	0.014***	6.547***	0.004
	(0.004)	(0.003)	(0.493)	(0.014)
Principal fixed effects	0.031***	0.014***	3.355***	0.099***
	(0.009)	(0.004)	(0.936)	(0.028)
School calendar	0.028***	0.014***	2.354***	0.016
	(0.007)	(0.004)	(0.475)	(0.019)
Congestion	0.029***	0.015***	5.465***	-0.004
	(0.008)	(0.004)	(0.604)	(0.020)
All mediators	0.026***	0.013***	3.712***	0.011
	(0.005)	(0.003)	(0.894)	(0.022)
Observations	2,227,008	3,401,126	3,412,142	1,523,709

Notes: Table reports estimates of one-parameter event study models corresponding to equation (2). Columns 1 and 2 include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Columns 3 and 4 include coefficients only the coefficient for the immediate effect β_1; β_2 and β_3 are constrained to be zero. Dependent variables are standardized math test scores (column 1), standardized english-language arts test scores (column 2), annual days attended (column 3), and standardized average teacher-reported effort scores (column 4). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A7: Student effects, heterogeneity

	Math	ELA	Attendance	Effort
Pooled	0.027***	0.017***	4.736***	0.025*
	(0.007)	(0.004)	(0.547)	(0.015)
By Sex:				
Female	0.035***	0.024***	4.775***	0.038**
	(0.008)	(0.004)	(0.539)	(0.016)
Male	0.019***	0.009*	4.698***	0.013
	(0.007)	(0.005)	(0.574)	(0.017)
p-value	0.00	0.00	0.71	0.05
By parental education:				
No college	0.028***	0.018***	4.945***	0.019
	(0.007)	(0.004)	(0.575)	(0.016)
Any college	0.022**	0.013***	3.780***	0.056***
	(0.009)	(0.004)	(0.541)	(0.018)
p-value	0.42	0.13	0.00	0.01
By residential mobility				
Mover	0.025***	0.018***	3.566***	0.025
	(0.008)	(0.004)	(0.524)	(0.016)
Non-mover	0.028***	0.015***	5.781***	0.027
	(0.007)	(0.005)	(0.630)	(0.019)
p-value	0.45	0.23	0.00	0.86
By school level:				
Elementary	0.027***	0.016***	2.704***	0.025
	(0.008)	(0.005)	(0.489)	(0.015)
Middle	0.026	-0.005	3.503***	
	(0.023)	(0.007)	(0.824)	
High		0.028***	7.123***	
		(0.008)	(1.065)	
p-value	0.98	0.00	0.00	
By grade of switch:				
Reg (KG,G6,G9)	0.018**	0.015***	5.894***	0.051*
	(0.009)	(0.005)	(0.651)	(0.030)
Irregular	0.036***	0.019***	3.207***	0.009
	(0.009)	(0.005)	(0.560)	(0.015)
p-value	0.08	0.42	0.00	0.20

Notes: Table reports estimates of parametric event study models corresponding one-parameter versions of equation (2). Columns 1 and 2 include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Columns 3 and 4 include only the coefficient for the immediate new school effect β_1; β_2 and β_3 are constrained to be zero. Dependent variables are standardized english-language arts test scores (column 1), standardized math test scores (column 2), annual days attended (column 3), and standardized average teacher-reported effort scores (column 4). Panel a repeats baseline one-parameter estimates from columns 1 and 4 of Tables 3 and 4. The remaining panels report estimates of coefficients interacted with student gender (panel a), parental education (panel b), residential mobility (panel c), school level (panel d), and whether a student switched in a typical (KG, G6, G9) or atypical grade (panel e). P-values for the test of equality of the coefficient(s) are reported in the third row of each panel. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table A8: Student effects, robustness

	Baseline	No Stayers	Only Treated	Only Switchers	Balanced
ELA Score					
New School * Trend	0.017***	0.018***	0.016***	0.014***	0.039**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.015)
Math Score					
New School * Trend	0.027***	0.027***	0.034***	0.035***	0.045
	(0.007)	(0.007)	(0.012)	(0.012)	(0.034)
Days Attended					
New School	4.74***	5.06***	3.65***	3.69***	5.58***
	(0.55)	(0.55)	(0.74)	(0.76)	(1.93)
Effort Score					
New School	0.025*	0.025*	0.024	0.031*	0.069
	(0.015)	(0.015)	(0.017)	(0.019)	(0.048)

Notes: Table reports estimates of parametric event study models corresponding one-parameter versions of equation (2). Panels (a) and (b) include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Panels (c) and (d) include only the coefficient for the immediate new school effect (β_1); β_2 and β_3 are constrained to be zero. Dependent variables are standardized english-language arts test scores (panel a), standardized math test scores (panel b), annual days attended (panel c), and standardized average teacher-reported effort scores (panel d). Estimates in column 1 repeat baseline one-parameter estimates from columns 1 and 4 of Tables 3 and 4. Column 2 excludes "staying" students that had 10% or more of their school-grade cohort exit to a newly constructed school. Column 3 excludes never-treated students. Column 4 restricts estimation only to those students observed at an existing school prior to attending a school at a new facility. Column 5 restricts to a balanced sample with 5 years of data in panels (a) and (c), or 3 years of data in panels (b) and (d). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A9: Stayer effects, adjusted for changes in school characteristics

Controls	(1) Math	(2) ELA	(3) Attendance	(4) Effort
None	0.022*	0.020**	3.376***	0.015
	(0.013)	(0.008)	(0.544)	(0.023)
Predicted peer characteristics	0.024*	0.021***	2.953***	0.016
	(0.014)	(0.008)	(0.512)	(0.023)
Teacher fixed effects	0.027***	0.016***	2.165***	0.004
	(0.008)	(0.005)	(0.502)	(0.011)
Principal fixed effects	0.030***	0.021***	1.691***	0.005
	(0.011)	(0.006)	(0.536)	(0.024)
School calendar	0.015	0.015*	1.343***	0.015
	(0.013)	(0.008)	(0.405)	(0.023)
Congestion	0.011	0.020**	3.375***	0.004
	(0.013)	(0.008)	(0.547)	(0.024)
All mediators	0.019***	0.013***	0.180	-0.012
	(0.007)	(0.004)	(0.407)	(0.012)
Observations	1,913,604	2,952,227	2,961,414	1,309,752

Notes: Table reports estimates of one-parameter event study models corresponding to equation (2), for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Columns 1 and 2 include only the coefficient for the change in growth β_2; β_1 and β_3 are constrained to be zero. Columns 3 and 4 include coefficients only the coefficient for the immediate effect β_1; β_2 and β_3 are constrained to be zero. Dependent variables are standardized math test scores (column 1), standardized english-language arts test scores (column 2), annual days attended (column 3), and standardized average teacher-reported effort scores (column 4). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student. Notes: All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

Table A10: House price effects, by school level

	(1)	(2)	(3)	(4)	(5)
New Elementary	0.051***			0.026*	
	(0.015)			(0.014)	
New Middle		0.031		0.003	
		(0.023)		(0.016)	
New High			0.071**	0.065**	
			(0.030)	(0.029)	
Only New Elementary					0.065***
					(0.021)
Only New Middle					0.008
					(0.018)
Only New High					0.072**
					(0.034)
p, Elem effects =0	.00063			.064	.0027
p, Mid effects =0		.18		.87	.66
p, HS effects =0			.019	.027	.034
p, All effects =0				.04	.0036
p, All effects equal				.17	.024
Yr-HSZ FEs	X	X	X	X	X
Month FEs	X	X	X	X	X
Sch Zone FEs	X	X	X	X	X
Prop Controls	X	X	X	X	X
All LAUSD	X	X	X	X	X
Number of sales	381,407	374,915	480,967	505,781	471,528
R2	.83	.83	.82	.82	.83

Notes: Table reports estimated coefficients from difference-in-difference regressions by school level, based off of equation (3). Columns 1, 2, and 3 report estimates of the effects of new elementary, new middle, and new high schools, respectively. Properties in new school zones for schools at the other two levels are excluded from the control group in estimation in columns 1- 3 (i.e. column 1 excludes properties that received new middle and/or new high school zones but not elementary schools from the control group). Column 4 includes coefficients for all three school levels. Column 5 restricts estimation to include only those properties in the attendance area of a single new school level. P-values for the tests that the effect at each level equals zero are included, as are p-values for the hypothesis tests that effects for all levels are equal to zero and that effects for all levels are equal. All specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Table A11: House price effects, including post-2012 data or 2012 neighborhood definitions

(a) Including post-2012 data

	Neighborhood Fixed Effects				Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
New School	0.053***	0.049***	0.034***	0.049***	0.042***	0.046***
	(0.015)	(0.013)	(0.011)	(0.013)	(0.012)	(0.014)
Month FEs	X	X	X	X	X	X
Yr-HSZ FEs	X	X	X		X	
Yr FEs				X		X
Sch Zone FEs	X	X	X	X		
Prop Controls	X	X	X	X		
Prop FEs					X	X
New Sch Zones	X	X	X	X	X	X
All LAUSD	X					
w/in 1km		X				
Number of sales	593,296	298,463	188,197	188,240	114,503	114,549
R2	.82	.79	.77	.74	.91	.9

(b) Neighborhoods based on 2012 post-construction boundaries

	Neighborhood Fixed Effects				Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
New School	0.068***	0.067***	0.046***	0.055***	0.045***	0.059***
	(0.019)	(0.017)	(0.013)	(0.016)	(0.014)	(0.017)
Month FEs	X	X	X	X	X	X
Yr-HSZ FEs	X	X	X		X	
Yr FEs				X		X
Sch Zone FEs	X	X	X	X		
Prop Controls	X	X	X	X		
Prop FEs					X	X
New Sch Zones	X	X	X	X	X	X
All LAUSD	X					
w/in 1km		X				
Number of sales	505,713	255,458	161,762	161,769	87,516	87,544
R2	.82	.8	.78	.75	.91	.9

Notes: Table reports estimated coefficients from difference-in-difference regressions following equations (3) and (5). Panel (a) including additional data from 2013-2015, while panel (b) uses neighborhood fixed effects based on 2012 school assignment zones in lieu of 2000 school zones. Dependent variable is the ln(sale price). Columns 1-5 report estimates from equation (3), including neighborhood fixed effects and property specific controls. Columns 6 and 7 report estimates from equation (5), including property fixed effects. Columns 1, 5, and 7 report estimates using year fixed effects; the remaining columns include year-by-high school zone fixed effects in estimation. In columns 1 and 2, all properties in LAUSD in the data sample are included. Column 3 restricts the sample to include only properties within a new school zone or within a 1km of a new school zone (by 2012). Columns 4-7 include only properties within a new school zone by 2012: "never-treated" properties are excluded from estimation. All specifications also include month effects. Standard errors are clustered by neighborhood.

Table A12: House price effects, robustness to sample restrictions

		Relaxing sample restrictions for:			
	(1) Baseline	(2) Price outliers	(3) Renovated/torn-down	(4) Large/multi-unit	(5) Non-residential
New School	0.060***	0.088***	0.056***	0.058***	0.048***
	(0.018)	(0.028)	(0.020)	(0.018)	(0.014)
Yr FEs					
Yr-HSZ FEs	X	X	X	X	X
Month FEs	X	X	X	X	X
Sch Zone FEs	X	X	X	X	X
Prop Controls	X	X	X	X	X
Baseline sample	X	X	X	X	X
Price outliers		X			
Renovated			X		
Large/multi-unit				X	X
Non-residential					X
Number of sales	505,780	512,577	525,469	513,039	625,632
R2	.82	.75	.75	.8	.72

Notes: Table reports estimated coefficients from difference-in-difference regressions corresponding to estimates of equation (3). Dependent variable is the ln(sale price). Column 1 repeats baseline estimates presented in Table 7 panel (a) column 2. Column 2 makes no restriction on sale price, including the top and bottom 1% of sales based on price. Column 3 relaxes the restriction on renovated and/or torn-down properties, including these properties with an additional indicator variable for having been renovated and/or torn-down in the controls. Column 4 includes large properties, with greater than one acre of space. Column 5 includes non-residential properties. All specifications include neighborhood fixed effects, property specific controls, and month fixed effects. Standard errors are clustered by neighborhood.

Table A13: House price effects, by distance to school assignment zone boundary

	(1)	(2)	(3)	(4)
New School: inside zone	0.060***	0.061***	0.054***	0.051***
	(0.018)	(0.018)	(0.017)	(0.018)
Distance to boundary		0.019		0.017
		(0.014)		(0.013)
Inside zone * dist to boundary		-0.005		-0.004
		(0.010)		(0.010)
New School: outside w/in 2km			-0.013	-0.046***
			(0.010)	(0.017)
Outside w/in 2km * dist to boundary				0.035**
				(0.014)
Yr-HSZ FEs	X	X	X	X
Month FEs	X	X	X	X
Sch Zone FEs	X	X	X	X
Prop Controls	X	X	X	X
New Sch Zones	X	X	X	X
All LAUSD	X	X	X	X
Number of sales	505,781	505,781	505,781	505,781
R2	.82	.82	.82	.82

Notes: Table reports estimated coefficients from difference-in-difference regressions based off of equation (3). Dependent variable is the ln(sale price). Column 1 repeats baseline estimates reported in column 2 of Table 7. Column 2 adds coefficients for property-level distance to the school assignment boundary and the interaction between distance to the boundary and the new school zone treatment variable. Column 3 includes an additional treatment variable for properties outside but within 2km of the new school attendance zone, where the completion date assigned to these properties corresponds to that of the nearest new school attendance zone. Column 4 combines columns 2 and 3, and adds an interaction with distance to the boundary for properties outside but within 2km of the new school zone. All properties in LAUSD in the data sample are included in estimation. All specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Table A14: "Stayers" school zones

	Neighborhood Fixed Effects			Repeat Sales	
	(1)	(2)	(3)	(4)	(5)
Post: Stayer School	-0.009	0.023	-0.009	-0.010	-0.014
	(0.017)	(0.018)	(0.019)	(0.031)	(0.025)
Month FEs	X	X	X	X	X
Yr-HSZ FEs	X	X		X	
Yr FEs			X		X
Sch Zone FEs	X	X	X		
Prop Controls	X	X	X		
Prop FEs				X	X
All LAUSD	X				
Number of sales	343,939	180,469	180,469	107,450	107,450
R2	.83	.82	.81	.93	.93

Notes: Table reports estimated coefficients from difference-in-difference regressions following equations (3) and (5). Dependent variable is the ln(sale price). Properties in new school zones are excluded from estimation; columns 1-3 report estimates corresponding to equation (3), with neighborhood fixed effects and property specific controls included. Columns 4 and 5 show estimates where property fixed effects are included, corresponding to equation (5). Columns 4 and 6 of panel (b) include year fixed effects, while the remaining columns include year-by-high school zone fixed effects in estimation. In column 1, all properties in LAUSD in the sample are included, while columns 2-5 further restrict the estimation sample to only include those properties in school zones affected by student outflows. Standard errors are clustered by neighborhood.

Table A15: Predicted house prices

	(1)	(2)	(3)	(4)
New School	0.001	0.010	-0.004	-0.009
	(0.010)	(0.012)	(0.006)	(0.009)
Month FEs	X	X	X	X
Yr-HSZ FEs	X	X	X	
Yr FEs				X
Sch Zone FEs	X	X	X	X
New Sch Zones	X	X	X	X
All LAUSD	X			
w/in 1km		X		
Number of sales	505,715	255,457	161,766	161,792
R2	.39	.4	.39	.38

Notes: Table reports estimated coefficients from difference-in-difference regressions following equations (3), excluding property-specific controls. Dependent variable is a predicted sales price, constructed via a hedonic regression of prices on property characteristics. Column 4 report estimates using year fixed effects; the remaining columns include year-by-high school zone fixed effects in estimation. In column 1, all properties in LAUSD in the sample are included. Column 2 restricts the sample to include only properties within a new school zone or within a 1km of a new school zone (by 2012). Columns 3-4 include only properties within a new school zone by 2012: "never-treated" properties are excluded from estimation. Standard errors are clustered by neighborhood.

B Effects on Staying Students and Sending Neighborhoods

B.1 Effects on Staying Students

Students who switched to new school facilities were not the only students to experience significant school-level changes: nearby new school constructions induced cohort-level outflows from existing facilities. Those students who stayed behind experienced reductions in overcrowding, conversion from year-round multi-track calendars back to traditional two-semester calendars, and changes in peer composition, but not improvements in facility quality. Examining the effects of new facility openings on the outcomes of students who stayed behind at nearby existing facilities therefore allows us to determine the relative importance of reduced crowding vs direct facility quality improvements in the new school treatment effects.

New schools were typically populated with students from several nearby school catchment areas. To identify existing schools which are most affected, we focus on those which saw large student outflows to new schools. We define "stayers" to be students for whom 10% or more of their school-grade cohort switched to a newly constructed school facility.[44] We then define event-time analogously for these students: year "0" is the year in which a school cohort experienced a large outflow induced by a nearby new school construction. We estimate effects for these students using the same event study methodology for the main student effects presented in equations (1) and (2); because these cohort outflows were induced by new facilities, estimates rely on the same variation in the timing of construction between different students.[45]

Figure A8 shows event-study estimates of cognitive and non-cognitive outcomes for stayers. Stayers see small but significant increases in ELA scores (panel B) following the cohort outflow to the new facility. For math (panel A), effects are smaller and insignificant. The increase in days attended (panel C) is immediate and significant - students attend 3.1 (SE 0.5) more days relative to the year prior to the cohort outflow. Panel D shows estimates for standardized effort scores. Point

[44] Appendix Figure A9 reports analogous event study estimates using a 20% threshold. This reduces the sample considerably, but results are robust to alternative thresholds.

[45] Here, students who switched to new schools are excluded from estimation; estimates are relative to a control group of students in the same grade and year who have yet to experience a cohort outflow shock, and never-treated students who experienced no significant peer outflow.

estimates are all very close to zero and insignificant, with the exception of the binned endpoint for 4 or more years of exposure, which is negative and significant.

Parametric versions of the estimates corresponding to equation (2) are reported in Table A1. For each outcome, both one- and three-parameter estimates are shown. Columns 1 and 2 report estimates for math test scores. Estimates in column 1 show no change in test score growth in the years following the cohort outflow, while estimates in column 2 show that once pre-existing trends are included, there is a small insignificant immediate effect that fades out within the following year. For ELA (columns 3 and 4), the pattern is different, and the parametric estimates more closely align with the event study estimates in Figure A8 . Column 3 shows a 0.5% (SE 0.3%) of a standard deviation increase in ELA test score growth in the years following the cohort outflow. However, once the post indicator and trend variable are included in column 4, all of the effect loads onto the post coefficient, with no ensuing growth or fade-out of effects. This pattern of cognitive effects differs from that of students attending new schools: effects accrue immediately, and either fade out (math), or remain roughly constant (ELA).

Columns 5 and 6 report estimates for days attended. Stayers see a 3.1 (SE 0.4) day increase in days attended; this effect attenuates to 2.3 (SE 0.5) days with the inclusion of trend variables. Columns 7 and 8 show no immediate effects on teacher-reported effort, with evidence of small negative effects after several years. As shown in Panel D of Figure Figure A8, this is driven by negative but imprecise effects several years post-outflow. Taken together, these results indicate positive indirect effects induced by peer outflows to new school facilities, but only for ELA test scores[46] and attendance.

Besides reductions in overcrowding due to peer outflows to newly constructed schools, what sort of changes in the school environment were experienced at these existing schools? Panel B of Appendix Table A2 presents estimates of the changes staying students experienced after they experienced a cohort outflow. Results indicate that stayers experienced a significant decline in multi-track calendar usage (21pp) and a significant increase in the total number of instructional

[46] McMullen and Rouse (2012) also find that reading, but not math test scores are adversely affected by school facility overcrowding and congestion.

days (0.94) per year. Both staying and switching students experienced a similar decline in multi-track calendars, and increase in the total number of instructional days per year. This is driven by the fact that in LAUSD, students at multi-track schools often had fewer instructional days per academic year.[47] Class sizes saw a negligible and insignificant decline for students who stay behind. Comparing these estimates to the estimated 3 day increase in total attended days in Table A1 implies that roughly one-third of the attendance effect is mechanically driven by increased number of days. Columns 5 and 6 report changes in the average peer group. Consistent with the fact that switching students were slightly more disadvantaged and lower-scoring than staying students, stayers see reductions in peer minority shares and increases in predicted scores of peers due to cohort outflows to new facilities.

Overall, indirect effects appear to be driven by reductions in overcrowding and the switch from multi-track calendars to traditional schedules.[48] Attendance effects are roughly half the size as for switching students, once the increase in the number of instructional days is factored in. Test score effects are only significant in ELA and not math, and are much smaller than those estimated for students who switch into new schools. Taken together, these results indicate that overcrowding reductions are not a primary mechanism driving effects at newly constructed schools. In the next section we examine the mechanisms underlying the new school effects in greater detail.

B.2 Effects on Sending Neighborhoods

As discussed in Section B.1, schools that experienced large student outflows to new schools saw significant reductions in overcrowding and multi-track calendar utilization, and small but significant increases in the share of more advantaged students. Students at these schools also experienced gains in ELA scores and attendance. To what extent were these gains at existing "sending" schools capitalized into local house prices? In Appendix Table A14 we report difference-in-differences esti-

[47] Many of the year-round district schools operated on a multi-track calendar known as "Concept 6", which increased school capacity by up to 50% but at the cost of 17 instructional days (out of 180). The loss in instructional days was made up by increased instructional time per day.

[48] "Horse-race" style regressions that include these mechanisms as controls are reported in Appendix Table A9, and indicate that school calendar changes are important, but changes in peers, teachers, and principals do not mediate the effects. Additional evidence on teacher effects is included in Appendix Table A4, and shows that there were no meaningful changes in teacher demographics or value-added at stayer schools.

mates where treatment is similarly defined for existing "sending" schools that experienced student outflows to newly constructed facilities.[49] Specifications in columns 1 and 2 correspond to those in columns 1 and 2 of panel A; specifications in columns 2-5 correspond to those in columns 3-6 in panel A. Estimates provide little indication that house prices increased in the sending school neighborhoods. These results suggest that (a) parental valuation of new schools is driven by non-test score/amenity improvements at new schools, independent of the school calendar or level of overcrowding, and/or (b) improvements in school quality due to reductions in overcrowding and multi-track calendar utilization are less salient to prospective homebuyers, who may instead rely on school facility condition as a signal for underlying school quality. Later, in Section 5 we will use a residential choice model to interpret the valuation and efficiency of the program; our findings imply that most of the valuation of the new schools is driven by non-test score and/or amenity improvements.

C Further Evidence on Mechanisms

C.1 Contemporaneous changes: peers, class sizes, and school environment

Contemporaneous changes in peers, class sizes, and the school environment are documented in Appendix Table A2. One of the stated goals of the LAUSD school construction program was to eliminate the use of multi-track academic calendars that required schools to continuously operate year-round. Before the construction program, half of LAUSD students attended multi-track schools. By reducing overcrowding in neighborhood schools, district officials were able to begin new schools on traditional two-semester calendars, as well as convert existing schools from multi-track back to traditional calendars.

Column 1, panel A of Appendix Table A2 report difference-in-differences and event study estimates of the likelihood of being exposed to a multi-track calendar. Switching to a new school was accompanied by a 28 percentage point reduction in the likelihood that a student was exposed to a

[49] "Sending" schools are defined as schools that have a non-trivial share (greater than 10%) of student enrollment that experienced a substantial cohort outflow to a newly constructed school. The treatment year for sending schools is analogously defined as the treatment year for stayers; i.e. the year in which the peer outflow occurred.

multi-track calendar. This conversion also meant that many students in new schools experienced additional instructional days: as reported, students switching to a new school had on average nearly one additional instructional day per year, relative to the prior year at an existing school (column 2, Appendix Table A2). Taking the 2SLS estimate of 5 additional days attended per year from Table 4, this implies that roughly one-fifth of the observed attendance effect is mechanically due to a change in school calendar.

At new schools, class sizes were actually somewhat larger: on average, teachers at new schools taught classes with 0.24 more students per teacher (Column 4 of Appendix Table A2). The magnitude of this difference, however, is quite small; roughly speaking, the district was approximately able to maintain similar pupil-teacher ratios at new school facilities by transferring teachers to new facilities in roughly equal proportion to students. Thus, changes in class size do not contribute to the estimated new school effects.

If students who switch to newly constructed school facilities are exposed to higher quality peers, changes in peer quality could explain some of the observed effects. As discussed earlier and shown in Table 2, students who attend newly constructed schools are more disadvantaged relative to students in the rest of LAUSD. However, new schools could offer better peer groups than do other schools in nearby neighborhoods. This could occur if new school boundaries were drawn in a such a way as to increase the concentration of more advantaged students, or if nearby higher-SES parents were less likely to comply with school residential assignments. Empirically, this does not appear to be the case: average peer predicted scores fall significantly upon switching to a new school, and new school peers are more likely to be black and/or Hispanic (Columns 5-6, Appendix Table A2).[50]

C.2 Teachers and principal quality

Student gains at new schools could be attributable to systematic differences in teacher and/or principal quality between new and existing schools. New facilities provide improved working environments for staff, and these amenities could attract better quality staff from either within or

[50] Predicted scores are generated from a regression of contemporaneous ELA test scores on a vector of demographic characteristics. Leave-out mean school-year predicted scores are then computed for each student-year observation.

outside the district.[51] In this section we examine the changes in teacher and principal demographics, and teacher value-added associated with a student's switch to a new school facility.

Systematic teacher resorting would imply that student gains at new schools came at the expense of students at existing schools; any within-district resorting of existing teachers would be zero-sum in aggregate. To empirically assess whether there was differential sorting of higher quality teachers into new school facilities, we compare differences in teacher observables and test score value-added (Appendix Table A3). Results indicate that students who switch to new schools are exposed to teachers that are less experienced, slightly more likely to have a master's degree, and are more likely to be new to the district.

Observable teacher characteristics, however, generally explain little of the variation in test-score based measures of teacher quality. However, estimates of changes in value-added (columns 4-5) indicate that students who switched to new schools experienced teachers with *lower* test-score value-added scores than prior to switching. The point estimates are for both math and ELA are small, although the estimate is more negative and marginally significant for ELA.[52]

While we find little evidence of positive resorting of existing teachers into new schools, it could still be the case that the new teachers hired into new schools were of differential quality. We cannot directly compare contemporaneous value-added scores of new teachers at new and existing schools, as this would confound student gains due to school-level facility improvements with improvements in new teacher quality. However, under the assumption that new facilities affect novice and experienced teachers identically, we can assess the quality of new teachers by testing whether the school-level gap in value-added scores between new and existing teachers is larger or smaller at new facilities.[53] These estimates are reported in columns 6 and 7 of Appendix Table A3, and provide little evidence that newly hired teachers were of higher quality at new schools. [54] Overall, sys-

[51] Complementarities between facility quality and teacher effort and/or performance could also result in improved teacher productivity at new schools. Unfortunately, we cannot directly assess this using our data, as any such improvements could not be separately distinguished from general school- or student-level improvements.

[52] We focus on switching teachers, for whom we have value-added estimates from their prior (existing) school. See Appendix B for an explanation of how teacher value-added scores are calculated.

[53] In Appendix B we explain how these gaps are calculated, and the assumptions under which they identify the relative quality of novice teachers.

[54] Given that we find evidence of negative sorting of existing teachers on value-added the difference in point estimates between columns 4 and 6 and columns 5 and 7 would need to be positive to support an interpretation that newly

tematic differences in teacher quality cannot account for observed student test score gains. In the longer-run, it is still possible that higher-quality facilities could attract and retain better teachers, although further research is necessary to determine if this channel to improve teacher quality is empirically relevant.[55]

Principals and school administration are also important inputs in education production, and recent work has shown that improved managerial skills among principals can have positive effects on student achievement (Fryer, 2017). While we lack direct measures of principal quality, we examine principal experience as a proxy. Using data on principal names, we constructed measures of within-district principal experience to test whether new schools were more likely to have more experienced principals. On average, however, the opposite is true: new schools employ principals with less experience, and which are more likely to be new to the school and district (Appendix Table A5). We view this as compelling evidence that principal quality does not mediate the positive effects we find, and that if anything, principal quality may have been lower at the newly constructed schools.

C.2.1 Estimating value-added

To estimate teacher value-added scores, we use a subsample of students for which the following criteria are met: (1) Student-year observations have non-missing test scores and are currently in grades 3-7 in math, and 3-11 in ELA; value-added scores are not computed for grade 2 teachers so as to have at least one prior score for a student; (2) Student-year observations have non-missing teacher assignment;[56] (3) Student-year observations are in classrooms with at least 7 students. Consider the following data-generating process for test scores, closely following Kane and Staiger

hired teachers were of higher quality at new facilities.

[55]Priority for intra-district teacher transfers within LAUSD was allocated using a tenure-based point system, which may not be systematically correlated with teacher quality. It is possible that school facility improvements have a larger impact on teacher quality in settings where within-district mobility is less restricted.

[56]Nearly every student in K-5 has a non-missing assignment; teacher IDs in later grades were assigned to a student-subject pair based on the teacher associated with a student's math and/or ELA class

(2008) and Chetty et al. (2014):

$$y_{i,t} = \alpha_{t,g(i,t)} + X'_{it}\beta + \nu_{it} \tag{11}$$

$$\nu_{it} = \mu_{j(i,t),t} + \epsilon_{it} \tag{12}$$

where $y_{i,t}$ is student i's test score in a given subject in year t, $g(i,t)$ denotes a student's grade in a given year, $j(i,t)$ denotes a student's teacher in a given year, and X'_{it} is a vector of controls. Here, $\mu_{j(i,t),t}$ is a teacher's effect on student test scores in year t and $\epsilon_{j(i,t),t}$ captures unobserved error in test scores unrelated to teacher quality.

To compute value-added for a given teacher-year, we estimate equation (11), and then compute the average residual within each teacher-year cell: $\text{VA}_{jt} \equiv \overline{\nu}_{jt}$. Unlike many prior studies, we do not use an Empirical Bayes or similar procedure to shrink these noisy estimates of value-added: here we only use value-added estimates as dependent variables, and using posterior means as left-hand side variables can introduce bias.[57]

In estimation, X'_{it} includes third-degree polynomials in lagged student test scores (for both subjects), demographics (race, gender, parental education, free/reduced-price lunch status, limited English status), class size (only available for elementary students), and school-level variables (school leave-out means of the share black/hispanic, share with any parental postsecondary education, share who speak English at home, and the share eligible for free or reduced-price lunch). We do not include school fixed effects in estimation, meaning estimated teacher effects are relative to all other teachers within LAUSD.

C.2.2 Estimating changes in value-added at new schools

Standard value-added models can confound school and teacher effects. For example, new school facilities could generate improvements in student attentiveness and/or teacher productivity, both of which would result in gains in estimated teacher valued-added. However, student gains resulting from school improvements would reflect improvements resulting from the new facility itself, and

[57]See Jacob and Rothstein (2016) for a more detailed discussion of potential problems using estimated posterior means of student test scores as dependent variables in regression models.

not from variation in underlying (prior) teacher quality. Thus, to directly assess whether teacher resorting explains any of the student gains, we focus specifically on switching teachers, for whom we have an estimate of value-added based on student test score observations from their prior, existing school facilities.

For these switching teachers, we compute the student-weighted average of prior value-added scores, using only data from years a teacher taught at an existing school facility. Specifically, we define $\text{VA}_j^{prior} \equiv \sum \frac{n_{jt}}{n_j} \text{VA}_{jt}$, where VA_{jt} is the estimated value-added for teacher j in year t, n_{jt} is the number of student observations for contributing to teacher$j$'s value-added score in year t, and n_j is the total number of students taught by teacher j (prior to switching to a newly constructed facility). For each student-year observation, we assign the mean prior value-added score, averaged over all teachers in a given school-year.[58]

While we find little evidence of positive restoring of existing teachers into new schools, it could still be the case that the new teachers hired into new schools were of differential quality. We cannot directly compare contemporaneous value-added scores of new teachers at new and existing schools, as this would confound student gains due to school-level facility improvements with improvements in new teacher quality. However, under the assumption that new facilities affect novice and experienced teachers identically, we can assess the quality of new teachers by testing whether the school-level gap in value-added scores between new and existing teachers is larger or smaller at new facilities. We can decompose the estimated teacher effect to include the true teacher effect, a new-school specific shock, and an unobserved error term:[59]

$$\text{VA}_{jst} = \mu_{jt} + \theta_{st} + \eta_{jst}$$

Insofar as the effect of a new school in a given year, θ_{st}, is constant for all teachers, we can use the gap between experienced and novice teachers at new schools to difference out the any differential

[58] Results are nearly identical if we instead assign a student the prior value-added score of her specific teacher in a given year.

[59] Recall teacher-year value-added is defined as the average residual from a regression of student test scores on polynomials in lagged test scores, demographic variables, and school variables: $\text{VA}_{jt} \equiv \overline{\nu}_{j(i,t)t}$ where $\nu_{it} = y_{i,t} - \alpha_{t,g(i,t)} - X'_{it}\beta$.

new school effects at the school by year level:

$$\begin{aligned}\overline{\text{VA}}_{st}^{GAP} &\equiv \overline{\text{VA}}_{st}^{New} - \overline{\text{VA}}_{st}^{Old} \\ &= \overline{\mu}_{jt}^{New} - \overline{\mu}_{jt}^{Old} + \tilde{\eta}_{st}\end{aligned}$$

We therefore assign each student the difference between the school-year average value-added of new teachers and existing teachers. A positive school-level gap between new and existing teachers would indicate that the new teachers at a school have higher value-added than the existing teachers, and vice-versa. Thus, holding existing teacher quality constant, if new teachers hired into new facilities are of higher quality, we would expect a positive coefficient on the gap.

C.3 Adjusting for changes in the school environment

How do these changes in the school environment mediate the positive effects found for students attending new schools? For a classroom or school characteristic to explain any part of the new school effect, it must be the case that (1) there is a change in the characteristic between the new and existing schools attended by switching students, and (2), the characteristic must have a (causal) effect on the student outcomes we study.[60] Contemporaneous changes in teacher, principal, peer, and other school characteristics have been previously documented (Appendix Tables A2, A3, and A5). Prior research has shown that many of these mechanisms may be important determinants of student outcomes.

In Table A6 we examine how the main effect estimates vary with the inclusion of time varying controls for changes in the school and classroom environment at new schools. The first row of Table A6 reports baseline one-parameter effect estimates from (2), for the subsample of students with non-missing values for school characteristics.[61] The second row includes an index of peer quality, based on a prediction of test scores using demographic characteristics. The third and fourth rows

[60] This discussion and approach borrows from the method used by Card and Giuliano (2016) to examine how effect estimates are explained by changes in classroom characteristics.

[61] This restriction primarily excludes students for whom teacher and principal assignments are missing, as row 3, 4, and 7 of the table include teacher and/or principal fixed effects in the model.

include teacher and principal fixed effects, respectively. The fifth row includes a control for whether a multi-track school calendar was used in that school-year, while the sixth row includes a control for school congestion, based on the ratio of current enrollment in a facility to 2013 enrollment, after nearly all of the overcrowding in the district had been eliminated. Finally, the seventh row jointly includes all aforementioned variables in the estimation.

Including controls for peer quality has no impact on any of the effect estimates. Teacher fixed effects slightly attenuate the coefficient on math test scores, but not ELA. On the other hand, attendance estimates actually increase with the inclusion of teacher effects. Taken together with previous evidence that teacher at new schools had, if anything, lower value-added scores, we take this as evidence that teacher quality is not a first-order mechanism mediating the new school effects we document. Similarly, the inclusion of principal fixed effects has little impact on test score estimates, although they attenuate the attendance effect and strongly increase the effort effect. Principals often have discretion over school-wide attendance policies, and may also affect school culture more broadly. However, given that there are relatively few principal switchers between existing and new facilities in the data, we are wary of over-interpreting these results.

Controlling for whether a school is currently operating on a year-round multi-track calendar has no impact on test score outcomes, but does mediate over half of the effect on student attendance. Multi-track calendar schools sometimes had fewer total instructional days, and required that students attend school during the summer (when absences may be more likely). These results imply that multi-track calendars may be detrimental to student attendance, and that roughly half of the attendance increase at new schools is due to the elimination of these non-standard school schedules. On the other hand, while we found some evidence of positive effects for students who stayed behind at existing schools and experienced reductions in overcrowding as a result of peer outflows to new schools, we find little evidence that facility congestion is an important mechanism for switching students; coefficient estimates are changed little with the inclusion of this control.

Including all mediators at once shows that, collectively, these variables explain very little of the positive test score effects at new schools. Attendance effects are somewhat attenuated, which is entirely driven by the multi-track calendar elimination. Effort effects are small and insignificant, although this was true for the baseline estimation on this subsample of students. Along with previously presented evidence, we conclude that the results presented in Table A6 provide further evidence that changes in peer quality, teacher quality, and principal quality at new schools are quantitatively unimportant mechanisms for the new school effects.

D Robustness to Treatment Sample

Baseline estimates from one-parameter models for cognitive and non-cognitive outcomes in Tables 3 and 4 (columns 1 and 4) are reported in Table A8 for different sample definitions, varying the set of students used as the control group for students switching to new schools. As test score effects reflect the cumulative impact of multiple years of exposure to new schools, we compare one-parameter estimates of the phase-in coefficients (β_2) from models where we constrain $\beta_1 = \beta_3 = 0$. Reassuringly, implied cumulative 4-year effects from parametric estimates in columns 1 and 4 of Table 3 are indeed very similar to point estimates reported in Figure 4 for students who attended new schools for four or more years. On the other hand, as we expect the flow of student effort and attendance to increase immediately upon matriculation to a new school, we report one-parameter estimates of the mean difference post-new school matriculation (β_1) from models where we constrain $\beta_2 = \beta_3 = 0$.

Column 1 repeats baseline estimates reported in Tables 3 and 4. Column 2 excludes students who stay behind at existing schools when 10% or more of their cohort switches to a new school. Estimated coefficients for ELA and days attended are only slightly larger, while estimates for math and effort standardized scores are essentially identical.[62] In column 3, we drop all students who never attend new schools, using only "ever-treated" students. If students who switch to new schools are systematically different from those who do not, inclusion of never-treated students as controls may induce bias (though our inclusion of student fixed effects would absorb differences in outcome levels). However, this does not appear to be the case, as estimates are nearly identical for all

[62]In Section B.1 we specifically examine indirect effects on these students, finding evidence of small positive effects on ELA scores and attendance. Since these students make up only a small fraction of the overall "never treated" group in baseline regressions, we would therefore expect the magnitude of differences between columns 1 and 2 to be very small in the presence of small indirect effects.

outcomes. Column 4 further excludes students who appear in the data sample in their first year at a new school. Inclusion of these "always treated" students could be problematic if new school constructions systematically induce students of different ability to enter LAUSD, perhaps from private schools or from outside the district. As shown in column 4, estimated treatment effects are, if anything, slightly larger when only switching students are included in the estimation sample, implying effects are not generated by a resorting of students entering LAUSD to attend newly constructed school facilities.

In column 5, we restrict the sample to include a balanced panel of students in event time. As discussed in Section 3.1, ELA test scores are recorded for students in grades 2-11, and attendance is measured for all grades. Math test scores are only included for grades 2-7, and effort marks are only measured in elementary school (grades KG-5). Thus, for math and effort we include students who have outcome data both one year before and one year after switching to a new school facility. For ELA and attendance we need not be as restrictive, and use a balanced panel of students with non-missing outcomes both 2 years before and after switching to a new facility. Estimated treatment effects in column 5 are less precise, as expected given the reduction in sample size, but point estimates are if anything slightly larger than those in columns 1-4. Results are robust to these sample permutations, and we therefore conclude that baseline estimates including all students are not biased by differential sample selection in event time.

E Treatment Effect Heterogeneity

Student effects: Heterogeneity

Heterogeneity in estimated student effects is presented in Table A7. Row 1 reports pooled estimates using the entire sample, which correspond to baseline estimates presented in column 1 of Table 3. In the remaining rows, the one-parameter treatment effect coefficients are interacted with student demographic and other characteristics.[63] Estimated cognitive effects are nearly twice as large for girls than boys, and the differences are statistically significant ($p < 0.01$) for both math and ELA.

[63]Note that this constrains grade and year effects to be equivalent for each group, as opposed to running separate regressions or also interacting fixed effects with demographic indicators.

Effects on student effort are also larger for girls, although the magnitude of the difference is smaller and not significant. The pattern is the opposite for attendance, as effects on the number of days attended are larger for boys than girls, although the magnitude of the difference is small. These differences suggest that substandard classroom facilities may inhibit girls' learning more than boys, although the mechanisms underlying this difference are unclear.

When results are split by level of parental education, a mixed picture emerges. Estimated effects on math scores, ELA scores, and attendance are larger for students with parents who did not attend any level of postsecondary education, although the difference in math scores is small and insignificant. For student effort, estimated effects are over twice as large for students with parents who have any level of postsecondary education than for those whose parents have a high school education or less. Overall, the results provide little evidence that improvements in school facilities systematically benefit students from lower socio-economic backgrounds.[64] Recall however, as shown in Table 2, that there is little variation in socio-economic status in LAUSD: nearly 90% of treated students are eligible for free or reduced-price lunch and less than one-fifth have parents with any level of post-secondary education.

Table A7 also shows estimates split by school level. Cognitive effects are insignificant for students who attend new middle schools, although for math, the difference between elementary and middle school effects is insignificant. For ELA, effects are large and significant in both elementary and high school, and are essentially zero for students who switch to a new middle school. For attendance, a clear pattern emerges: effects increase monotonically with school level, and are the largest for students in new high schools. Insofar as student motivation is impacted by new facilities and drives changes in student attendance, we would expect effects to grow with grade level as older students have greater autonomy over attendance decisions than younger students, whose daily attendance is more directly dictated by parental influence.

Finally, estimated effects are also split by whether a student switches schools during a "regular" grade transition (KG, G6, G9) or switches to a new school in another grade. "Irregular" grade transitions in off-grades occurred immediately following school construction, when students were

[64] Analogous breakdowns by race and free lunch status (not reported) show only small and insignificant differences.

transferred between schools to fill enrollment at the new school. Overall, effects are similar for both types of switching students, with only a large and significant difference in estimated attendance gains. Estimated effects on cognitive outcomes and student effort are somewhat larger for initial switchers who switch during an irregular grade transition, although only the difference in math scores is statistically significant at the 10% level. For student attendance, effects are significantly larger (5.2 days vs 1.9 days) for regular grade switchers. Students switching at a typical grade transition are mostly switching in grades 6 and 9, which explains most of the difference in days attended, as attendance gains are larger for middle and high school students than elementary school switchers.[65]

E.1 Real estate effects: By school level

Estimates reported in Table 7 and Figure 6 include properties that received multiple new schools. The average treated property in the sample was in the school attendance area of 1.1 new school constructions, implying the the effect of receiving a single school (elementary, middle or high) would be 9% lower than the baseline estimates, roughly a 5.5% increase in house prices per new school construction using baseline estimates from column 2. In Table A10 we report house price effects separately by school level. Results indicate that effects are largest for new elementary and high schools, although we cannot statistically reject differences in estimated coefficients in all specifications. Qualitatively, results are consistent with student effect heterogeneity reported in Table A7, which provided evidence that test score effects were larger and more significant for newly constructed elementary and high school than for new middle schools. As middle schools represent the shortest duration of student attendance (3 years, vs 4 for high school and up to 6 for elementary), it is unsurprising that the effects may be smaller.

[65] See Figure A6 for the distribution of student switching grades to new schools.

E.2 Real estate effects: By neighborhood price level

While new school quality was similar across treated neighborhoods,[66] the tax price of the new facilities faced by district residents was greater in areas with higher property values.[67] In Section 5, we use the estimated house price effect for a welfare calculation, applying the coefficient to the mean home value in LAUSD. But insofar as home prices capitalize local investment, one might expect larger percentage effects on prices in low-price neighborhoods than in high-price neighborhoods. If so, applying the average percentage treatment effect to the average house price could overstate the aggregate impact. Empirically this does not appear to be the case. In Figure A13 we report heterogeneity in estimated treatment effects by neighborhood prior mean house prices. We define neighborhood prior mean house prices as the average house price in a neighborhood over all pre-treatment years in the sample, 1995-2001. Estimates of β from equation (3) are shown interacted with \$100,000 bins of neighborhood prior mean house prices.[68] With the exception of the \$500,000 -\$600,000 bin, all effects are similar and statistically significant, providing little evidence of smaller estimated treatment effects in areas with higher property values.

E.3 Real estate effects: Local boundary and spillover effects

School assignment boundaries do not stay constant in perpetuity, and due to uncertainty over future boundary locations, capitalization effects may be smaller near the boundaries within new school zones. In addition, if home buyers substitute housing in existing school zones for housing purchases in new school zones, prices could decline in other LAUSD neighborhoods. On the other hand, new school constructions and changing neighborhood composition could lead to spillovers that increase house prices both within and near new school zones. Prices in nearby neighborhoods that did not receive new schools could increase due to positive externalities from neighborhood upgrading (e.g. Hornbeck and Keniston (2017)). Moreover, new schools could act as a direct amenity that

[66] In conversations with district officials, it was stated that much of the variation in project cost was due to site-specific acquisition expenses, and not systematic differences in new facility quality.

[67] Unlike in the model presented in Section 5.1, which assumed a constant lump sum tax for all households, property owners in higher-priced areas contributed a greater dollar amount towards district bond revenues.

[68] Note: the \$100K bin includes a small number of properties in neighborhoods with mean house prices below \$100K; the \$600K bin includes properties in all neighborhoods with mean house prices greater than \$600K in 1995-2001.

generates positive benefits (e.g. increased park/playground space) both within and outside the actual attendance areas. Estimates in Figure A15 and Appendix Table A13 assess the extent to which the effect of new school constructions varies by distance to the attendance boundary, and whether new schools generate spillover effects beyond the attendance zone.

Figure A15 reports how price effects vary by the distance to the school attendance zone boundary. Each point represents a difference-in-differences estimate interacted with distance to the new school attendance zone boundary, in 400 meter bins. Properties with positive distance are located within new school boundaries, while those with negative distance are in school zones where the residential assignment is to an existing school. Results indicate that within the new school zones, capitalization is roughly constant at approximately 5% for all distance bins. We find no evidence of smaller effects closer to the boundary. Properties within 400 meters but outside of the boundary see statistically significant declines in house prices of 4.9% (SE 1.7%) post-construction, providing evidence of negative spillovers for properties that are just outside the new school zone. These negative spillover effects quickly diminish, however; point estimates for distances greater than 1.2 km are positive, though insignificant. Table A13 reports analogous estimates of treatment effects by distance to the school attendance boundary, using linear interaction terms in lieu of separately estimating effects in distance bins. Results are consistent between the two approaches. Overall, this pattern of results is consistent with cross-neighborhood substitution within very narrowly defined markets, wherein demand for properties located marginally outside the new school zones decreases for prospective homebuyers searching within the vicinity a new school.