The Impact of School Facility Investments on Students and Homeowners: Evidence from Los Angeles

By Julien Lafortune & David Schönholzer*

We study school facility investments using administrative records from Los Angeles. Exploiting quasi-random variation in the timing of new facility openings and using a residential assignment instrument, we find positive impacts on test scores, attendance, and house prices. Effects are not driven by changes in class size, peers, teachers, or principals, but some evidence points towards increased facility quality. We evaluate program efficiency using implied future earnings and housing capitalization. For each dollar spent, the program generated 1.62 dollars in household value, with about 24% coming directly through test score gains and 76% from capitalization of non-test score amenities.

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, Thompson and Hagey, 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

* Lafortune: Public Policy Institute of California, 500 Washington Street, Suite 600, San Francisco, CA 94111, lafortune@ppic.org. Schönholzer: Institute for International Economic Studies, Stockholm University, david.schonholzer@iies.su.se.
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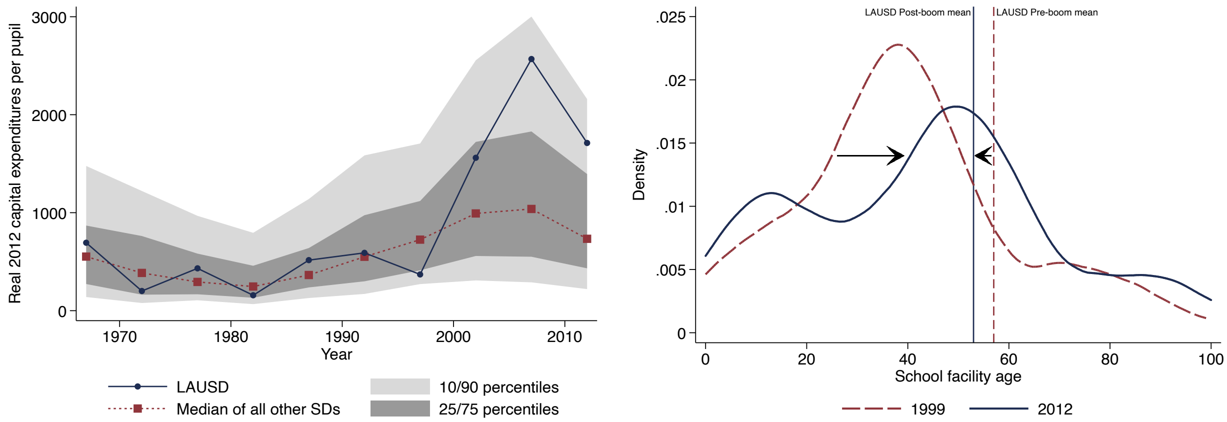
Figure 1. : LAUSD School Capital Spending in Context



(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.

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 (Greene, 2003; 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 method 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 test score and other 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, evidence suggests 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 and was more efficient than a hypothetical direct transfer to households. We arrive at this result by enhancing the traditional housing capitalization approach with the later life earnings approach commonly found in the literature. To do so, we identify 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. This parameter is also closely related to the literature on housing valuation of school performance surveyed by Black and Machin (2011). More generally, our hybrid valuation approach combining capitalization and later life earnings is broadly applicable to any program that impacts both the housing market and the labor market.

We estimate program efficiency 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 use the covariance between house price and test score effects to estimate that only 22 cents out of each dollar capitalized (about $3 billion out of $14 billion) is due to test score improvements. This implies that roughly three-quarters of the increase in students' future earnings is capitalized into house prices ($3 billion out of $4 billion). We then incorporate future earnings gains that are not capitalized in the real estate market using the hybrid approach, which yields a marginal value of public funds of 1.62 ($15 billion in total valuation). This is slightly larger than only using real estate capitalization, but substantially larger than if we were to only consider gains in future earnings. The overall ratio of future earnings valuation to total valuation is 0.24, suggesting that around 24% of total program value is due to test score improvements with the rest due to the capitalization of non-test score improvements.

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, Ferreira and Rothstein, 2010; Martorell, Stange and McFarlin, 2016; Hong and Zimmer, 2016; Conlin and Thompson, 2017; Goncalves, 2015; Baron, Forthcoming). 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 small-scale construction program 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 detect precise and robust effects, as well as decompose effects to 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

[1] Another exception is Hashim, Strunk and Marsh (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.

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, Ferreira and Rothstein, 2010; Neilson and Zimmerman, 2014; Goncalves, 2015; Conlin and Thompson, 2017). 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. Candelaria and Shores, 2015; Jackson, Johnson and Persico, 2016; Hyman, 2017; Jackson, 2018; Lafortune, Rothstein and Schanzenbach, 2018), labor market outcomes (Jackson, Johnson and Persico, 2016), and intergenerational mobility (Biasi, 2019). Our study of the LAUSD school construction program provides additional evidence that: (1) school expenditures – even those dedicated to capital costs – can improve student test score and other 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 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 (Online Appendix 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 the 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. 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 I we detail the context for our study and discuss specific details of the LAUSD program. In Section II we examine the effects of the new facilities on student outcomes, first detailing the data and empirical results, and then examining student

[2]Several papers, most notably Black (1999) and Bayer, Ferreira and McMillan (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).

[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.

effects and mechanisms. In Section III we present the data, empirical approach, and results for neighborhood house prices. Section IV presents the residential choice model, linking the results in the previous section to interpret program valuation and efficiency. Finally, Section V concludes with a brief summary of results and their implications.

I. 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: 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.[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 1989, 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.

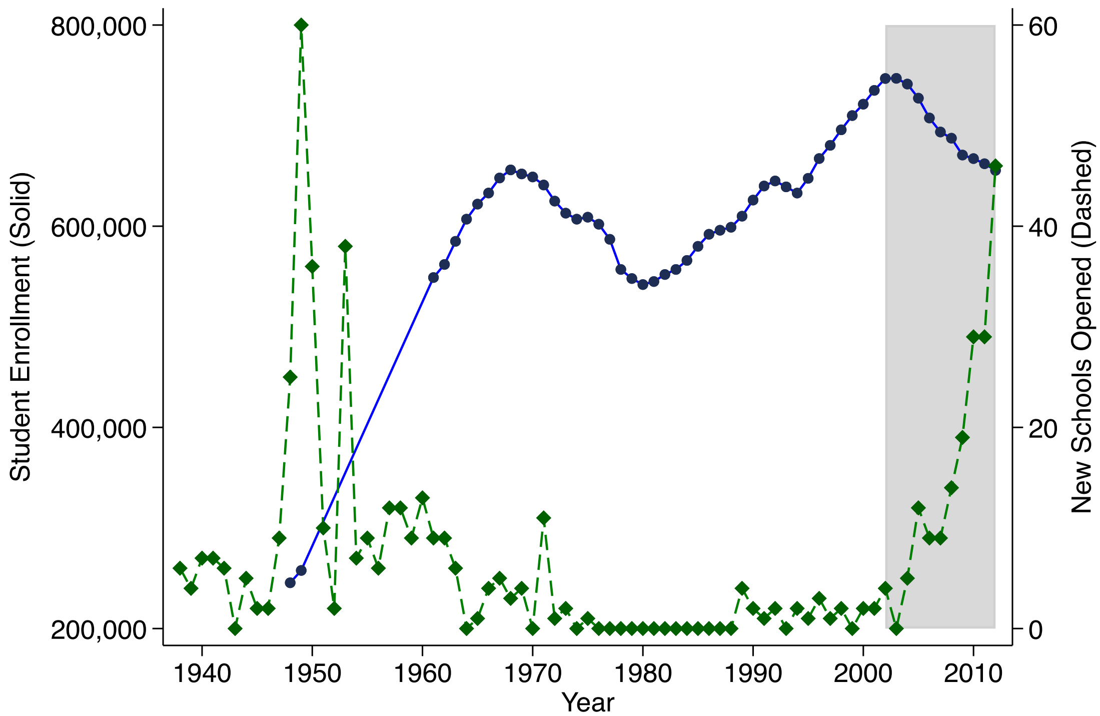
Between 1997 and 2008, voters in Los Angeles approved a series of bonds dedicating around $20 billion in local and state funding to the construction, expansion, and renovation of hundreds of schools. This was the largest public infrastructure program in the U.S. since the interstate highway system (Fuller 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 (Online Appendix Figure A1, panel A) and smaller urban districts across the country (e.g. Neilson and Zimmerman (2014), see Online Appendix 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 (U.S. Census Bureau Government Division, 2015; National Center for Education Statistics, 2020): 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 (Online Appendix Figure A1, panel B). Despite this increased investment, between 1999 and 2012 the average facility age

[4]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).

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.

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.[7]

[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 local bonds that were passed in and after 1997. We focus on new 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 schools 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 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, 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 (Online Appendix Figure A3).

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	2008	2008	2002	2012
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.

The construction program proceeded in phases, beginning in 2001 with the establishment of program budgets and schedules for nearly all of the projects in "Phase I" of the program. Phase I consisted of higher-priority projects to the districts, which were selected based on the extent of overcrowding in a given area. Phases II and III began when new local bonds were passed in 2002 and 2004, and covered projects projected to be completed by 2012.[8] To identify suitable sites for new schools, designated search areas were defined near the most overcrowded schools, and construction sites were selected from within these areas primarily based on site feasibility (e.g. size, location, accessibility), cost of acquiring land, environmental concerns, and local community engagement.

While nearly all new school sites had been identified by 2001, the process of internally approving and commencing projects, 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. From the time of site identification, 90% of projects were completed and ready for occupancy within 3 to 7 years. It is this plausibly random variation in the exact timing of openings – induced through the order in which projects commenced and by idiosyncrasies in the construction process – which we leverage to estimate the effect of new schools on student and neighborhood outcomes. Residential catchment areas dictated student assignment to new schools, and were determined in the months prior to occupancy by the district using census demographic and districtwide enrollment data to balance enrollment across new and existing facilities.[9]

We present summary statistics for the new school projects in Table 1. In total, there were 144 schools built as a part of 114 new school campuses.[10] The median project cost $57 million and

[8] There were an additional two phases of the program, but our study primarily consists of projects in these first three phases that were completed by 2012.

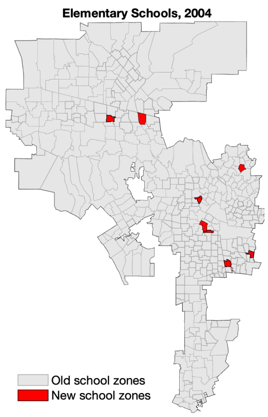
[9] Conversations with staff in the LAUSD demographics unit that oversaw the creation of school boundaries indicated that boundaries were selected to (1) balance enrollment and program needs relative to capacity across facilities, while (2) simultaneously trying to minimize travel distance for students and their families. Based on these conversations, we believe the potential scope for political and/or parental influence in the creation of boundaries was very limited.

[10] In some cases, a new school campus comprised several new schools, either because the site was combined to house both

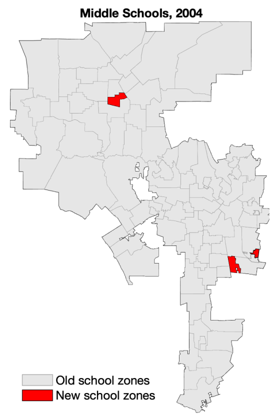
created about 800 new student seats, with several projects costing in the hundreds of millions of dollars. Projects typically took two years to construct, and were complete roughly 5 years after the site had been designated by the district. Figure 3 shows the attendance zones for new and existing school facilities in 2012.[11]

Figure 3. : LAUSD school attendance zones, 2004 vs 2012

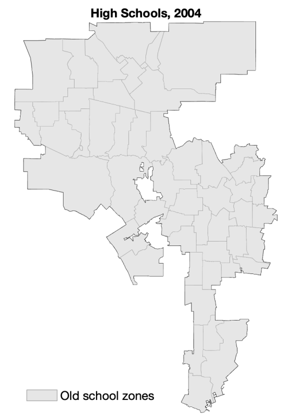
2004:



(a) Elementary

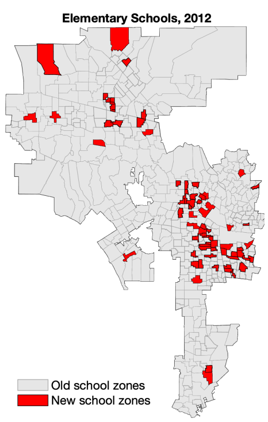


(b) Middle

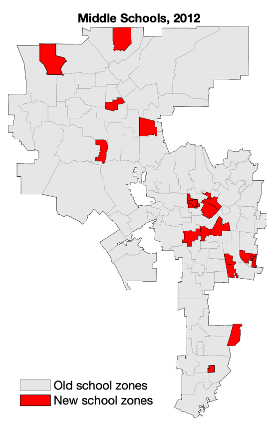


(c) High

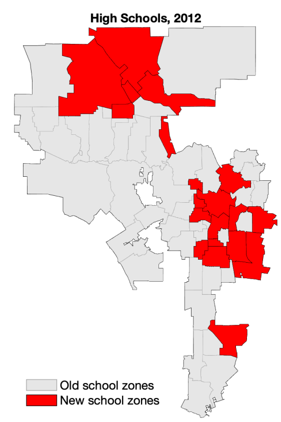
2012:



(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.

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.

[11] As shown in the figure, new schools were concentrated in East Los Angeles, where students are predominantly low-income and Hispanic/Latino and schools were the most overcrowded and dilapidated.

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.

II. Student Impacts

A. 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 (Los Angeles Unified School District, n.d.). Every student who attended LAUSD during this time period is included, and the data allow for longitudinal links across years for students who remain in the district.[12] 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.[13] Test scores are normalized relative to the California-wide mean and standard deviation.[14] 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.[15]

Data on teacher education, experience, age, and gender are available in all years, except 2009 and 2011. Teachers can be linked longitudinally in the student data using unique IDs.[16] Each elementary record contains a single teacher identifier. Teacher-student links for secondary school are constructed using student-level course data. Principal names are available for 85% of student-year observations, allowing us to construct within-district measures of principal experience.[17] 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

[12]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.

[13]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 (roughly 4.5% of grade 7 students in LAUSD take Algebra 1 during the sample period; these test scores are excluded). 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.

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

[15]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.

[16]Teacher demographic data cannot be linked to teacher identifiers in our student data, however. For teacher demographics we instead rely on school-year averages of each variable.

[17]Principal names are available in all but two years of our data. For missing years we assign a school its principal from the prior year or following year (giving preference to the prior year where there are conflicts).

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.

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.

B. Econometric Design

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 ($\delta_{t,d(i)}$).[18] 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 $\underline{K} = -6$ and $\overline{K} = 3$,[19] 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, Rothstein and Schanzenbach (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 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.[20] 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.

[18]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.

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

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

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 which such "Tiebout" moves may affect our estimates. In Online 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. We also present estimates where assignment is instrumented with a student's residence two years prior to attending a new school (Online Appendix Table A12). Estimates are quantitatively and qualitatively similar, implying that endogenous mobility in anticipation of new school openings is not an empirically meaningful source of bias in our estimated effects.

C. *Effects on switching students*

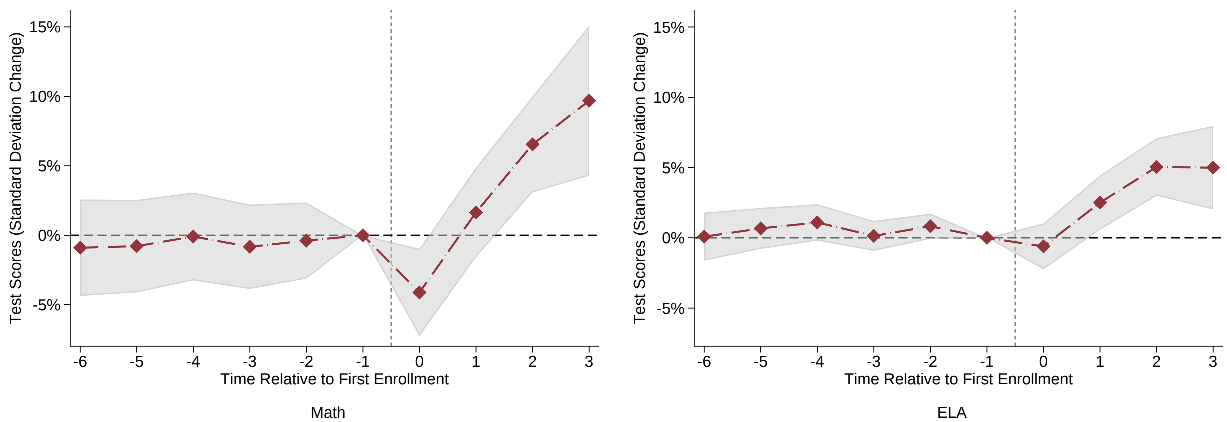
We now turn towards the discussion of our student results. In our baseline estimation we use all student-year observations in the relevant grades for a given outcome.[21] Students who never attend new school facilities are included in the regressions as controls, as are students who we observe at newly constructed schools in their first year in the data. In our baseline specifications we include students who stay behind at schools that see a substantial outflow of peers to new schools, although most students who attended old schools saw little to no reduction in crowding. This means that the counterfactual to new school attendance is attending an old but potentially slightly less crowded school. In Online Appendix Tables A9 and A10 we present analogous estimates where these "stayers" are excluded, and results are nearly identical. We also consider specifications where we exclude always treated, exclude never treated, and strongly balance the panel in event time in Online Appendix Table A12; reassuringly, results are very robust to these sample definitions. Later in Section II.D we directly evaluate the effect of experiencing a substantial outflow of peers to new schools.[22]

[21] 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.

[22] We discuss this in greater detail in Online Appendix B; students that stay behind do experience some gains from the outflow of treated students. However, since stayers make up a small part of the sample and their effects are small, effects on treated students are almost identical when stayers are included in the sample.

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

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.

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 (Panel A) and ELA (Panel B) standardized test scores. Columns 1-3 in each panel report OLS estimates, whereas Columns 4-6 report 2SLS estimates. In column 1, 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. Column 2 adds indicators for attending a newly constructed school.

Table 3—: Student effects, test scores

Panel A: Math	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
New School * Trend	0.027	0.033	0.033	0.026	0.031	0.031
	(0.007)	(0.009)	(0.009)	(0.012)	(0.012)	(0.014)
New School		-0.028	-0.028		-0.017	-0.016
		(0.017)	(0.017)		(0.020)	(0.021)
Trend			0.000			-0.001
			(0.002)			(0.003)
Cumul. Effect	0.081	0.072	0.071	0.079	0.074	0.076
	(0.022)	(0.023)	(0.024)	(0.035)	(0.036)	(0.043)
N student-years	2,851,853	2,851,853	2,851,853	2,851,853	2,851,853	2,851,853
N students	724,086	724,086	724,086	724,086	724,086	724,086
N treated students	86,373	86,373	86,373	86,373	86,373	86,373
N treated schools	77	77	77	77	77	77
Panel B: ELA	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
New School * Trend	0.017	0.018	0.018	0.024	0.026	0.027
	(0.004)	(0.004)	(0.004)	(0.008)	(0.009)	(0.010)
New School		-0.003	-0.004		-0.006	-0.004
		(0.009)	(0.009)		(0.010)	(0.012)
Trend			0.000			-0.001
			(0.001)			(0.002)
Cumul. Effect	0.050	0.050	0.048	0.072	0.071	0.075
	(0.013)	(0.013)	(0.014)	(0.025)	(0.025)	(0.031)
N student-years	4,397,778	4,397,778	4,397,778	4,397,778	4,397,778	4,397,778
N students	945,740	945,740	945,740	945,740	945,740	945,740
N treated students	95,757	95,757	95,757	95,757	95,757	95,757
N treated schools	124	124	124	124	124	124

Notes: Table reports estimates of parametric event study models corresponding to equation (2) using OLS (columns 1-3) and 2SLS (columns 4-6) using the timing of school assignment changes as instruments. 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 Panel A. In Panel B 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.

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 column 1. Column 3 adds 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 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 the program was fairly well-targeted at students who were most likely to benefit from the improvements. As in the OLS models, the linear trends included in column 6 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. Both specifications also show that student test score gains accumulate gradually, after a slight decline in student performance in the switching year. This pattern of gradual improvement is different from many other educational interventions considered in the literature, where effects tend to fade out over time. 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[23] or school-level inefficiencies in the first few years post-construction.

Other outcomes. — 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 test score effects, there is little indication of any meaningful prior trend in attendance in the years before switching to a new school facility. The binned endpoint for 6 or more years prior to switching to a new school is statistically significant; across all of the baseline event study estimates of student effects at new schools this is the only statistically significant pre-treatment coefficient.

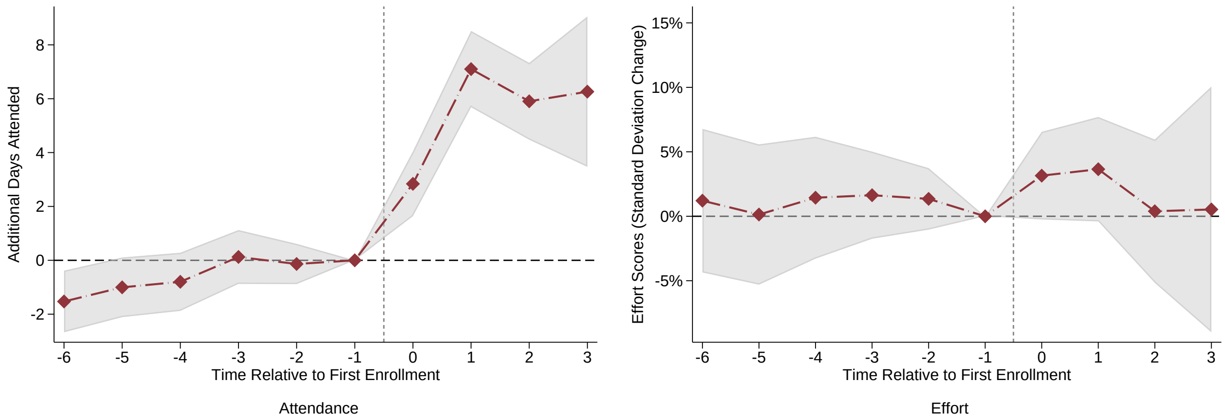
Panel B shows the effect of switching to a new school facility on teacher-reported student effort for elementary students. Upon matriculation into a new school facility, student effort increases by roughly 3% (SE 1.7%) of standard deviation. As with attendance effects, the estimated increase in effort 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 (Panel A) and effort (Panel B).[24] 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

[23] "Placebo" event study estimates for non-facility related student switches are reported in Online Appendix Figure A4. 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, Kain and Rivkin (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.

[24] Unlike in Table 3, we begin columns 1 and 4 with one-parameter specifications where only the coefficient for mean difference in the outcome post matriculation at a new facility included. Columns 2 and 5 add a phase-in coefficient, and columns 3 and 6 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.

Figure 5. : Other 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.

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 Panel B 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 1 and 4. 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 3 and 6 are small and insignificant, and as was the case for the other student outcomes, has a negligible 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.

Student effect heterogeneity. — We present student effect heterogeneity in Online Appendix Tables A7 and A8. Table A7 reports one-parameter treatment effect coefficients interacted with gender, parental education, and prior achievement.[25] Effects are larger for girls along both test score dimensions and for effort. Effects on test scores and attendance are slightly larger for students of parents with no postsecondary education – although only the difference in attendance is significant – while effort effects show the opposite pattern. Notably, test score and attendance effects are concentrated among students with below-median prior achievement, with large and statistically

[25] As roughly 90% of students who switch to new schools are Hispanic/Latino and/or receive free or reduced-price lunch, we do not report heterogeneity along these dimensions.

Table 4—: Student effects, other outcomes

Panel A: Days Attended	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
New School	4.736	3.953	3.344	5.150	4.414	4.075
	(0.547)	(0.651)	(0.673)	(0.568)	(0.685)	(0.768)
New School * Trend		1.197	0.959		1.240	1.099
		(0.445)	(0.449)		(0.579)	(0.620)
Trend			0.280			0.162
			(0.096)			(0.142)
N student-years	5,572,952	5,572,952	5,572,952	5,572,952	5,572,952	5,572,952
N students	1,170,738	1,170,738	1,170,738	1,170,738	1,170,738	1,170,738
N treated students	119,049	119,049	119,049	119,049	119,049	119,049
N treated schools	143	143	143	143	143	143
Panel B: Effort	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
New School	0.025	0.030	0.032	0.031	0.036	0.038
	(0.015)	(0.016)	(0.017)	(0.019)	(0.019)	(0.021)
New School * Trend		-0.009	-0.007		-0.011	-0.009
		(0.013)	(0.013)		(0.018)	(0.021)
Trend			-0.002			-0.002
			(0.004)			(0.006)
N student-years	2,761,804	2,761,804	2,761,804	2,761,804	2,761,804	2,761,804
N students	692,487	692,487	692,487	692,487	692,487	692,487
N treated students	91,056	91,056	91,056	91,056	91,056	91,056
N treated schools	80	80	80	80	80	80

Notes: Table reports estimates of parametric event study models corresponding to equation (2) using OLS (columns 1-3) and 2SLS (columns 4-6) using the timing of school assignment changes as instruments. 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 Panel A. In Panel B 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.

significant differences.[26] Effort effects show a bifurcated pattern: effects are large and significant for students with below-median effort prior to their switch to a new school, while effects are negative, significant, and of a similar absolute magnitude for students with high prior effort. The overall pattern of results suggests that new school facility benefits may be greater for lower-achieving and lower-SES students, although some of this may also reflect the fact that prior facility conditions were slightly worse for such students. Gender differences suggest that substandard facilities may inhibit girls' learning more than boys', but the mechanisms behind these differences are unclear and worthy of future research.

Table A8 examines differences by the context of a student's switch to a new school, reporting interactions with residential mobility (whether a student moved into the school zone at some point in the 3 years prior to matriculation), school level, and whether the grade of switch was "typical" (i.e. kindergarten, grade 6, grade 9) or atypical. Effects are nearly identical between movers and

[26] Prior achievement is defined based on the within-LAUSD distribution of the same subject-year-grade test/effort score (or grade-year number of days attended) in the year prior to switching to a new school.

Table 5—: Changes in other inputs at new schools

	Calendar		School		Peers	
	(1) Multi-track	(2) Max days	(3) Age	(4) Stu/tch	(5) Bl/Hisp	(6) 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)
N student-years	6,414,589	5,847,446	6,313,341	2,891,897	6,416,425	4,252,572
N students	1,244,589	1,201,713	1,233,191	724,526	1,244,897	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

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 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). All specifications include fixed effects for student, grade, and year-by-physical location district. Standard errors are two-way clustered by school and student.

non-mover students, with the exception of attendance, where effects are larger by two days among students who did not move prior to switching. Differences by school level show that test score effects are large and significant for elementary and high school, and are small and insignificant for middle school.[27] Attendance effects increase monotonically with school level; insofar as student motivation drives changes in attendance, we would expect effects to grow with grade level as older students have greater autonomy over attendance decisions than younger students, whose attendance is more directly dictated by parental influence. Differences by whether the grade of switch was typical or atypical are inconsistent: effects are slightly larger for test scores (the difference is marginally significant for math but not ELA) but smaller for attendance.[28]

D. 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 provide evidence that facility conditions and overcrowding may have mediated some of these effects, while changes in school calendars, class size, and characteristics of peers, teachers, and principals are less likely to have done so.

We begin by showing what changes students were exposed to when they switched to a new school. To this end, we regress school and peer characteristics on a new school indicator as in equation (2). As shown in Table 5, students at new schools were much more likely to be off a multi-track calendar, with about one additional instructional day per year. The average age of the school facility they attended fell by more than 71 years. The student-teacher ratio increased slightly, peers were slightly more likely to be black or hispanic, and peers' predicted test performance based on observable characteristics was slightly lower. In sum, attending a new school led to substantial changes along several dimensions, all of which could be potentially important for the achievement effects we document.

We now 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

[27]Notably, this pattern is mirrored in the real estate capitalization effects by school level. See Online Appendix Table A13.
[28]Differences in effort are not reported for school level and grade of switch, as this outcome is only available for elementary students.

Table 6—: 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	4.504	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.

facilities. Unfortunately, we only have data on the characteristics of school facilities from a single point in time (2008),[29] meaning that we lack continuous variation in these measures.[30] Thus, in Table 6 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.

[29]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.

[30]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.

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".[31] Again test score and effort 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.

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 presented in Online Appendix Figure A5 and Table A1, and we describe them in greater detail in Online Appendix B.

In contrast to facility quality and overcrowding, we find little evidence for other potential channels mediating the achievement gains of students at new schools. We examine the role of teacher quality and principal quality in Online Appendix Tables A3 and A5. 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 Online Appendix C.2.

Finally, 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 and principal fixed effects, multi-track calendar use, and facility congestion. As shown in Online Appendix Table A6, 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 Online Appendix C.3.

III. 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 IV, these impacts capture a much greater share of program benefits than do student impacts alone.

[31]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 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.

A. *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 (Los Angeles Office of the Assessor, 2017). 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).[32]

We focus only on sales of residential properties with non-missing sales prices.[33] 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.[34]

Table 7—: Summary statistics, LA County assessor data

	All LAUSD	New School Zones	Existing School Zones
Sale price (2015$)	565,801	416,507	636,017
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	161,805	344,030
N properties	350,299	115,254	235,045

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

Table 7 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

[32]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 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.

[33]We use CPI data from Bureau of Labor Statistics (2017) to correct for inflation.

[34]Results are robust to relaxing these sample restrictions. See Online Appendix Table A14.

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.

B. 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 construction, 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.[35] 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.[36]

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 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

[35] 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 8, columns 4 and 6).

[36] See Online Appendix Figure A7 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.

in house prices relative to the year prior to building occupancy:

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

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 school construction.[37] 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:

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

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 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).

C. *Neighborhood Results*

Table 8 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%).

[37]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 Online Appendix Table A13.

Table 8—: 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)
Sch Zone FEs	X	X	X	X		
Prop Controls	X	X	X	X		
Prop FEs					X	X
Yr-HSZ FEs	X	X	X		X	
Yr FEs				X		X
All LAUSD	X					
Within 1km		X				
Number of sales	505,715	255,457	161,766	161,792	87,516	87,557
R2	0.82	0.79	0.78	0.75	0.91	0.9

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.

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 with year-specific effects instead. Estimated effects are very similar to analogous neighborhood fixed effects models 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.[38]

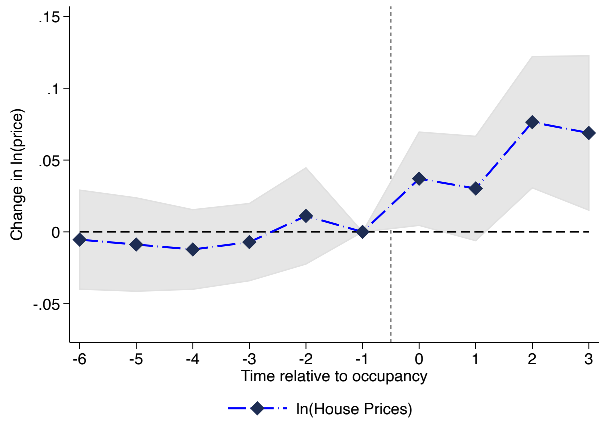
In Figure 6 we report event study estimates of the effects of new school constructions, corresponding to the specification in equation (4). Estimation includes only those properties ever within any new school zone and year-by-high school fixed effects, as in the specification in column 3 of Table 8. 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 8.

D. Enrollment effects

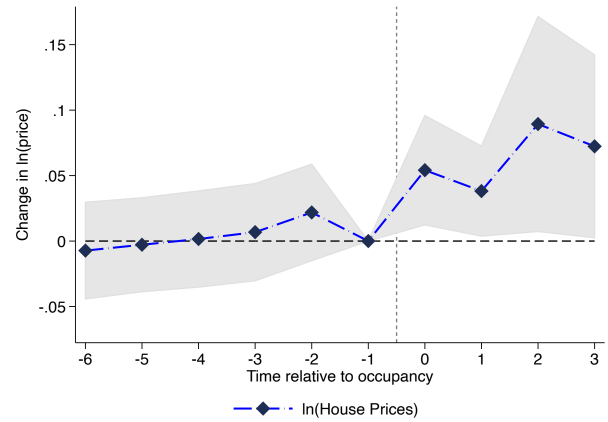
New school constructions may increase local demand for these new schools, both relative to existing schools in LAUSD, and relative to charter schools, private schools, and other schools outside of LAUSD. The evidence presented in prior sections shows that new schools led to improvements in student performance and that these and other benefits were capitalized into local real estate prices, which suggests the potential for increased student enrollment in these new school neighborhoods.

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

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 (3) of Table 8. In panel (b), all properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column (1) of Table 8. 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.

We report event-study estimates of log student enrollment in new school neighborhoods in Figure A6, estimated via equation 4, excluding controls for property characteristics and tax-rate area.[39] Unlike in prior within-district research on school constructions (Neilson and Zimmerman, 2014) we find no change in enrollment within new school neighborhoods either before or after the construction of a new school. While price effects indicate increases in demand for new schools in general, the lack of increased enrollment in a neighborhood indicates that these increases did not translate into a greater number of children attending LAUSD in these neighborhoods.

IV. 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. To do so, we rely primarily on the capitalization approach pioneered by Brueckner (1979) and further developed by Barrow and Rouse (2004) and Cellini, Ferreira and Rothstein (2010).

The basic intuition of the approach is that the increase in house prices relative to the increase in public expenditures reflects the extent to which households value the program relative to the higher taxes to fund it. A ratio below one means that a dollar invested into the program returned less than a dollar of value in the housing market, while a ratio above one indicates the willingness to pay for the program's benefits were higher than the cost. This ratio is also closely related to the marginal value of public funds (MVPF, see Hendren 2016) when the costs in the denominator take into account any (positive or negative) fiscal externalities of the program.

[39]Here we use neighborhood definitions based on the end-of-sample (2012) attendance boundaries, so that there is no within-neighborhood variation in assignment to a new school in the same year.

Table 9—: Cost-benefit analysis

Program component	Value
Program cost	
Households in LAUSD	1.52 million
Share treated households	0.328
Per treated household cost	$18,430
Total program cost	**$9.17 billion**
Program benefit	
1. Housing capitalization approach	
Estimated house price effect in treated areas	$28,201
Total real estate valuation	**$14.06 billion**
Benefit-cost ratio using only capitalization	**1.53**
2. Later life earnings approach	
Implied later life earnings per treated household	$7,782
Total earnings valuation	**$3.88 billion**
Benefit-cost ratio using only later life earnings	**0.42**
3. Hybrid approach	
Share housing valuation due to academic achievement	0.22
Share future earnings captured in academic valuation	0.76
Program benefit per treated household	$29,786
Total benefits	**$14.85 billion**
Marginal value of public funds	**1.62**

Notes: Table reports estimates of costs and benefits discussed in section IV.

A. *Housing Capitalization*

Estimates of the benefit-cost ratio can be seen in Table 9. We begin with the denominator: 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 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 the program benefits in terms of house price appreciation, the average sale price (within-sample) of properties in zones that received new schools was $494,650. Using the estimates in Table 8, 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 yields a value of 1.53. Using the confidence interval in the house price effect, which ranges from 2.7% to 8.7%, our estimates of the ratio vary from 0.73 to 2.34. Thus, while we cannot reject values below one, the evidence leans towards a return of each dollar invested in the program of more than one dollar in the housing market, and hence the program may have been inefficiently small.

The capitalization approach captures both academic benefits as well as non-academic benefits such as enhanced health and safety, better extracurricular activities, and improved community spaces. But it has a number of drawbacks. It assumes that individuals are identical and that there are no mobility costs. There is growing evidence that mobility costs are substantial (see e.g. Busso, Gregory and Kline 2013). Hence, this approach captures only the valuation of marginal movers induced by the program, not of all affected households. Another important assumption is that households have perfect information about the returns of the program. But recent research finds that the ability of households to value school effectiveness seems limited (Rothstein, 2006; Abdulkadiroğlu et al., 2020).

B. *Later Life Earnings*

In light of these shortcomings, we also consider another approach to program efficiency: direct valuation of implied later life earnings due to the program impacts on student achievement (Heckman et al., 2010; Chetty et al., 2011; Heckman, Pinto and Savelyev, 2013; Chetty, Friedman and Rockoff, 2014; Kline and Walters, 2016). In this approach, we replace the house price gains as a measure of willingness to pay for the program (i.e. the numerator of the benefit-cost ratio) with student achievement gains scaled by external estimates of the labor market value of these gains in terms of future earnings. In this way, we can sidestep issues related to potential biases in households' assessment of program benefits.

We present estimates of this approach in the second panel of Table 9. 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 kindergarten classroom quality to estimate that a 0.1 standard deviation increase in test scores leads to a 1.3% increase in earnings at age 27.[40] Extrapolating these gains for all affected future cohorts suggests a gain in later life earnings of $3.9 billion in present discounted value. We present further details on this calculation in Online Appendix F. Compared to the total program cost of $9.17 billion, the gains in later life earnings from test score improvements covers roughly 42% of the total program cost, a ratio far below one. If we were to consider only later life earnings, we would conclude that the program was inefficient.

C. *Combined Benefits*

Both capitalization and later life earnings capture relevant dimensions of program benefits. In the traditional view, benefits from later life earnings should be entirely captured in capitalization; specifically, households should perfectly anticipate both the magnitude of achievement effects of new schools as well as their future labor market returns. But in light of recent skepticism about households' ability to properly evaluate these benefits, it is possible that only a share of later life earnings are capitalized in the housing market. In Online Appendix E, we develop a model that integrates both approaches in a way that isolates preferences for academic valuation revealed in the housing market.

The basic idea of this hybrid approach is as follows. The strength of the relationship between neighborhood-specific housing price effects and achievement effects provides information about the extent to which achievement benefits get capitalized in housing prices: if this relationship is weak, this suggests that household valuation of test score effects is small relative to other program benefits. We can directly estimate this relationship in the data, although it relies on the strong assumption that sources of heterogeneity in house price effects are uncorrelated to those in achievement effects. Alternatively, we can use estimates from the vast literature on housing valuation of school performance surveyed in (Black and Machin, 2011), which studies house price responses to variation in

[40]Other estimates in the literature are generally similar: Kline and Walters (2016) present estimates of test score and earnings impacts from various studies (Table A.IV), with the Chetty et al. (2011) estimate slightly above the median.

student achievement. As we show in Online Appendix F, estimates from our own data fall squarely within the distribution of estimates in the literature.

Using this hybrid approach, 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. 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 are almost entirely determined by peers instead of school effectiveness (Abdulkadiroğlu et al., 2020), this result suggests that households may 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. However, it is also possible that households primarily value the physical appearance of school facilities along the lines of the Broken Windows theory (Zimbardo, 1969) with little regard for their academic value.

We find that total program benefits using the hybrid approach are around $14.85 billion, with a benefit-cost ratio of around 1.62. Notably, the ratio of the implied future earnings valuation to the total program benefits is 0.24, suggesting that 24% of the total program value is directly attributable to test score improvements. The remaining 76% results from the capitalization of non-test score improvements, which may include other school improvements as well as the valuation of school and neighborhood amenities.

Unsurprisingly, given that the majority of benefits derive from non-academic program benefits and the housing capitalization of academic benefits is fairly close to later life earnings, these quantities are quite similar to real estate capitalization alone. 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. Relative to recent estimates by Hendren and Sprung-Keyser (2020), our estimates of the marginal value of public funds are somewhat more modest than the most effective health and education programs targeted at low-income children but substantially larger than many other policies.[41]

V. 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 (Online Appendix 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. Using only neighborhood price appreciation, we derive a marginal value of public funds of around 1.5 for one dollar of per-household school capital investment. Parents, however, may not fully or correctly internalize the future benefits of academic improvements (Rothstein, 2006; Abdulkadiroğlu et al., 2020). 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

[41] It is also worth noting that estimating direct program costs is relatively straightforward, while many potential benefits, such as higher future taxes paid due to the increase in later life earnings, are not included.

residential choice model to isolate preferences for the academic valuation revealed through real estate capitalization. This approach is broadly applicable to any setting or program that affects both neighborhood quality and labor market outcomes, such as lead abatement and other forms of environmental remediation.

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. 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. One important reason why our estimate for the program value is so high is that supermajority requirements for bonds in California permit only the highest-return programs to pass. Ultimately, the magnitude of 1.6 is broadly in line with estimates of the impact of education policies more generally (Hendren and Sprung-Keyser, 2020).

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 household valuation.

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