Do School Facilities Matter? Measuring the Effects of Capital Expenditures on Student and Neighborhood Outcomes

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Abstract

We offer new evidence on the effects of school facilities spending on student and neighborhood outcomes, linking data on new facility openings to administrative student and real estate records in Los Angeles Unified School District (LAUSD). Since 1997, LAUSD has built and renovated hundreds of schools as a part of the largest public school construction project in US history. Using an event-study design that exploits variation in the timing of new school openings, we find that spending 4 years in a new school increases test scores by 10% of a standard deviation in math, and 5% in English-language arts. This in part reflects non-cognitive improvements: Treated students attend four additional days per school year and teachers report greater effort. Effects do not appear to be driven by changes in class size, teacher composition, or peer composition, but reduced overcrowding plays a role. House prices increase by 6% in neighborhoods that receive new schools. Real estate capitalization is greater than program cost, implying a willingness-to-pay in the range of 1.2 to 1.6 per dollar spent.

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1 Introduction

There has been a longstanding debate among educational policymakers and researchers over the productivity of school spending, with little consensus as to whether and under what circumstances increased expenditures improve student outcomes (e.g. Hanushek 2006). Much of the empirical literature has focused on instructional inputs, with considerably less attention paid to the role of capital expenditures. However, capital expenditures comprise an important component of US public school spending: in the 2013-2014 school year roughly 8% of total expenditures went towards direct capital outlays, and an additional 9% was spent on operation and maintenance of existing facilities and equipment (McFarland et al., 2017). Despite the magnitude of this spending, one-quarter of U.S. public schools are in fair or poor condition[1] (Alexander and Lewis, 2014), and estimates of the funding required to address substandard facilities conditions range in the hundreds of billions nationally (Crampton et al., 2001; Arsen and Davis, 2006; Filardo, 2016)). Substandard facilities are thought to be a particular problem in low-income districts, which have schools that are more likely to be in fair or poor condition and/or rely on temporary rather than permanent buildings (Alexander and Lewis, 2014), and on average spend 15% less on capital investments than do high-income districts.

In this paper, we address three fundamental unanswered questions. First, do increases in school capital expenditures improve student outcomes? Second, if they do, what are the mechanisms through which capital expenditures improve outcomes? And third, how are these capital expenditures valued in the real estate market and what are their welfare implications? We investigate these questions in the context of the largest public school capital construction program in U.S. history. From 2002 to 2017, Los Angeles Unified School District (LAUSD) constructed over 150 new schools and renovated hundreds more. Using administrative student and property sale records, we provide precise and comprehensive estimates of the causal impact of school facility expenditures on student outcomes and neighborhood house prices. Finally, we use these estimates to evaluate the welfare consequences of the construction program for LAUSD residents.

The empirical literature on capital expenditures offers little guidance with regard to these questions. Several studies find no or imprecise effects of capital expenditures on student achievement (see Cellini et al., 2010; Bowers and Urick, 2011; Goncalves, 2015; Martorell et al., 2016), while others find some evidence of positive impacts on student achievement, often only in reading and English-language arts (Welsh et al., 2012; Neilson and Zimmerman, 2014; Hong and Zimmer, 2016; Conlin and Thompson, 2017; Hashim et al., 2018). Other studies have looked at longer-run impacts

[1] "Fair" condition means that the facility meets minimum needs, but requires frequent maintenance and has other limitations. "Poor" means that the facility does not meet minimal requirements for normal school operation.

of school construction programs in other countries that expand access to education (e.g. Duflo, 2001, 2004), measuring the effects of more general increases in human capital accumulation. Despite inconclusive evidence in the literature and general skepticism among economists, resource-based capital expenditure programs continue to be used by policymakers at the state and local level as tools to improve schools and reduce achievement gaps.

We find robust evidence that attending newly constructed schools in LAUSD leads to large, significant gains in cognitive and non-cognitive student 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 0.06 standard deviations higher on teacher-reported measures of student effort. We provide additional evidence of smaller indirect test score and attendance gains for students at existing facilities who experienced reductions in overcrowding induced by peer outflows to newly constructed schools. These indirect effects allow us to decompose the relative contribution of overcrowding reductions to the observed student gains at newly constructed schools. Examining the mechanisms through which these effects are mediated, we conclude that the majority of the effects were due to improved facility quality, while reduced overcrowding was also an important factor. 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 find significant valuation of school quality improvements in the real estate market. Using administrative records on property sales, we find that house prices increase by 6% in neighborhoods that receive new school facilities. Effects accumulate in the first three years following construction, with little evidence of anticipatory house price increases. House prices in nearby neighborhoods are mostly unaffected, although we find some evidence of negative house price spillovers for properties very close to, but outside new school catchment areas. We use a simple model to assess the household valuation of a redistributive public education capital expenditure program. From this model, we derive an expression with a direct difference-in-differences analogue to assess the valuation of the spending program using relative price changes between neighborhoods. Reduced-form estimates of the change in relative house prices imply a household willingness to pay ratio in the range of 1.2 to 1.6 per dollar spent, providing evidence that the total real estate capitalization resulting from the program exceeds the total program cost, and that educational capital had been under-provided.[2]

[2]The efficient choice of local public expenditures is typically defined by the "Samuelson condition" (Samuelson, 1954): spending levels will equate the marginal rate of transformation of the public good and the sum of the marginal rates of substitution between numeraire consumption and the public good. Here, a WTP ratio greater (less) than one suggests under-provision (over-provision) of local educational capital expenditures. It is worth noting, however, that

Our study contributes new evidence to a few related literatures. First, we provide robust estimates of both direct and indirect student-level effects from facility improvements using variation induced by the largest school capital construction program in the United States. We estimate direct effects on treated students, as well as indirect effects on students who are affected by cohort-peer outflows from existing to new school facilities. Most prior studies examine effects of capital expenditure programs on district-level average outcomes, often finding mixed and imprecise estimates of effects on student outcomes (Cellini et al., 2010; Martorell et al., 2016; Hong and Zimmer, 2016; Conlin and Thompson, 2017; Goncalves, 2015). These studies do not measure effects on directly treated students, and are generally underpowered to detect modest but meaningful effects. Most school districts consist of at least several school campuses, and thus programs to construct new schools or renovate existing ones often only affect a subset of students.[3]

There are two notable exceptions in the literature that examine direct effects on students exposed to new school facilities. Hashim et al. (2018) focus on subset of new school openings in LAUSD in 2010/2011,[4] and find evidence of gradual positive effects on both math and ELA scores. Neilson and Zimmerman (2014) examine a similar construction boom in New Haven, Connecticut, and find evidence of positive effects on reading but not math scores several years after school construction. In both cases, many fewer students and schools were impacted. We build on this prior work by leveraging the scale of the entire LAUSD school construction program, allowing us to carefully decompose effects and examine specific mechanisms. New school openings do not exist in a policy vacuum, and involve many other contemporaneous changes in peers, teachers, and administration, all of which we are able to examine directly. In addition, we examine outcomes of students who experience cohort-level peer outflows induced by new school openings, providing new evidence of indirect spillover effects of new facilities through reduced overcrowding at nearby existing schools.

Second, we contribute to the literature estimating the capitalization of school quality in the real estate market. We provide some of the first large-sample evidence of localized house price capitalization of dynamic changes in school quality. Much of the work in this literature has estimated the capitalization of *static differences* in school quality, and thus does not provide direct estimates of how *changes* in school quality are valued in the real estate market. Several papers,

educational inputs are not pure public goods; schooling is both excludable and subject to congestion. In Sections 3 and 8 we provide a more detailed discussion of program efficiency and welfare implications.

[3]See Figure A1 for a comparison of the estimated test score effects by per-pupil spending change for prior studies of school capital expenditures.

[4]Specifically, they study the effects of the initial two cohorts of "strategic" new school openings, a subset of the new schools constructed in LAUSD after 2010 as a part of the district's Public School Choice Initiative. The operation of these schools was more autonomous than traditional district schools, and involved other non-standard redesign features.

most notably Black (1999) and Bayer et al. (2007) exploit boundary discontinuities within narrowly defined neighborhoods to estimate the market valuation of school quality. Other papers have used variation across district boundaries (e.g. Barrow, 2002; Barrow and Rouse, 2004), within-district boundary changes (e.g. Ries and Somerville, 2010; Collins and Kaplan, 2017), school "report-card" grades (Figlio and Lucas, 2004), and public reporting of teacher value-added scores (Imberman and Lovenheim, 2016). Static differences in house prices between school zones include parental preferences for school quality, peer quality, and racial composition; these estimates are less informative for understanding the dynamic effects from policy changes.

A handful of recent papers provide estimates of real estate capitalization of changes in school quality using variation induced by capital expenditure policies, generally finding positive effects after several years (see Cellini et al., 2010; Goncalves, 2015; Conlin and Thompson, 2017; Neilson and Zimmerman, 2014). We build upon these prior studies by more precisely examining the dynamics of these changes, over both time and space. Moreover, we study a (mostly) locally funded program that was inherently redistributive: local property taxes were raised districtwide to fund new schools in only one-third of neighborhoods. We directly relate our estimates of within-district relative price changes within a simple spatial equilibrium model, allowing us to directly assess the efficiency of program spending.

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

There are two important caveats to these conclusions. First, as our study focuses on the outcomes of one large district, our results may not generalize to other districts or states. However, many large, urban districts as well as smaller districts serving disadvantaged students face consistently underfunded and worse quality facilities relative to more affluent districts (e.g. Filardo et al., 2006). Our study is directly applicable to these contexts. Second, an important feature of the LAUSD program was the reduction of overcrowding and the expansion of available school facilities. We find some evidence of larger gains for students coming from previously overcrowded schools. Our estimates are therefore likely to represent upper bounds on possible effects in districts with stable or declining enrollment seeking to replace, rather than expand the school capital stock. Importantly, overcrowded school facilities are not unique to LAUSD; over 25% of California public schools are designated as overcrowded (Rogers et al., 2009), and thus our results are relevant to many school districts facing similar constraints.

The paper proceeds as follows. In Section 2 we detail the context for our study and discuss specific details of the LAUSD program. In Section 3 we outline a simple theoretical framework to motivate our analysis and interpretation of house price changes. In Section 4 we briefly describe each of the data sources we use. Section 5 outlines the empirical specifications and quasi-experimental setup we use to estimate program effects. In Section 6 we present the student-level results, and discuss mechanisms and indirect effects. In Section 7 we present house price results, and examine potential spatial spillover effects. Section 8 provides a discussion of results, and an assessment of the benefits, costs, and welfare implications of the program. Finally, in Section 9, we conclude with a brief summary of results, their implications, and their generalizability.

2 Context of Study

LAUSD is the second largest school district in the United States, serving 747,009 students at its peak in the 2003-2004 school year. It enrolls roughly 10% of all public K-12 students in California. Like nearly every 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. The district itself encompasses 26 cities in total, including the City of Los Angeles, as well as other nearby "gateway" cities and some unincorporated areas within Los Angeles County. Some of the more affluent areas in LA County, including Beverly Hills and Santa Monica, operate separate school districts for their residents. 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.[5]

As of the early 2000s, LAUSD's capital stock had fallen well below current needs. As shown in Figure 1, no new schools were opened between 1975 and 1996, and the average student attended a school that was over 60 years old in 2000. Many were in extremely poor condition. 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

[5]Scores from the CST ELA exam in grades 2-11, and the CST math exam in grades 2-7.

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) Classrooms were often non-functional, with broken and missing equipment, and school facilities sometimes lacked adequate restrooms.[6] Inadequate climate control was additionally a major source of distraction; classroom temperatures upwards of 90 degrees fahrenheit were not uncommon. 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)

The schools were also severely overcrowded, as the district's enrollment had increased roughly 10% since 1975 (Figure 1). Nearly 25,000 students were bused daily to faraway schools to relieve capacity constraints, and roughly half of students attended schools that operated on Òmulti-trackÓ 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 student access to extra-curricular opportunities. Rapid depreciation of facility condition due to continued overuse compounded these issues.

Between 1997 and 2007, voters in Los Angeles approved a series of bonds dedicating over $27 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). The first new school was completed in 2002, and over the next 15 years nearly 150 new school facilities were constructed in LAUSD, totaling over $10 billion in capital expenditures. Many more schools were renovated, modernized, or received additions that increased school capacity. By 2012, over 75,000 students attended a newly constructed school (see Figure A2), less than 1% of students remained on a multi-track calendar (see Figure A3), overcrowding had been effectively eliminated, and there was no longer widespread busing of students to distant schools.

After the first bond authorization in 1997, the district began by identifying overcrowded schools and attendance areas. Designated search areas were defined for each of these locations, 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. By 2001, nearly all new school sites had been identified, although the process of acquiring land, securing adequate funding, negotiating with local stakeholders, meeting environmental regu-

[6] For example, one high school of nearly 2000 students had only one functioning bathroom.

lations, and designing and constructing schools resulted in a staggered delivery of new facilities over the next decade. It is this plausibly random variation in the timing of openings, induced primarily through idiosyncrasies in the construction process, which we exploit to estimate the effect of new schools on student and neighborhood outcomes. We will provide a comprehensive discussion of our empirical approach and identification assumptions in Section 5.

In this paper we focus on new school facilities completed between 2002 and 2012, for which we have detailed project data matched to administrative student data. A database of capital projects in LAUSD, including measures of project cost, size, completion timeline, and location, was constructed from records listed publicly by the LAUSD Facilities Services Division (FSD). The data cover all major projects and new school constructions with a preferred site designated between 1997 and 2011,[7] and include over 500 capital projects totaling nearly $17 billion in planned or realized spending. We restrict attention only to large new school construction projects, defined as those that created over 100 new seats and/or cost at least $10 million.[8]

Summary statistics for the new school projects are presented in Table 1. In total, there were 143 new schools built as a part of 114 new school campuses. In some cases, a new school campus comprised several new schools, either because the site was combined to house both elementary and middle (or middle and high school students), or because magnet or alternative schools serving the same grade levels were housed on the same campus. The median project cost $57 million and created about 800 new student seats, with several projects costing in the hundreds of millions of dollars.[9] Projects typically took two years to construct, and were complete roughly 5 years after the site had been designated by the district. In total, the projects we study in our data cost $9.17 billion (roughly $6,000 per household or $15,000 per pupil), the majority being funded from the various local bonds that were passed in and after 1997.

Figure 2 shows the time series of educational spending in LAUSD relative to the other nearby districts in LA County. Panel A shows per-pupil capital expenditures, while panel B shows per-pupil instructional expenditures. Capital expenditures in LAUSD and in other LA County school districts increased similarly during the 1990s, and prior to the passage of the first school construction bond in 1997, capital expenditures were slightly lower in LAUSD (roughly $500 per pupil) than in the rest of LA County (roughly $750 per pupil). The magnitude of the program is clearly seen in panel A:

[7] Projects not yet constructed by the end of 2011, but that were already in the planning phase, are included.

[8] 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). In a few instances, students show up at a particular new school in either the year before or after the listed completion year; we adjust the completion year to correspond to the student administrative records in these cases.

[9] One controversial high school project, the Robert F. Kennedy Community Schools, cost nearly $600 million to construct, becoming the most expensive public school ever built.

expenditures rose rapidly in LAUSD during the construction boom, to a peak of nearly $4000 per pupil in 2009. Capital expenditures increased much less dramatically in other LA County districts until 2005, before declining to roughly the same level in 2012 as in 1990. Conversely, instructional expenditures saw much smaller increases during the new construction boom from 2002-2012, and the relative difference between LAUSD and other LA County schools was essentially unchanged during this period. Overall, the sample period from 2002-2012 was marked by a large increase in capital expenditures, without a meaningful increase in instructional educational expenditures in absolute or relative terms.

Figure 3 shows the attendance zones for new and existing school facilities in 2012. As can be seen in the figure, new schools at all levels were concentrated in East Los Angeles, where students are predominantly Hispanic and schools were previously the most overcrowded and in need of repair. Schools in East LA serve students who are socioeconomically disadvantaged; for example, the median school in the areas most heavily affected by new school construction serves a student population where fewer than one-fifth of students have a parent with any level of postsecondary attainment.

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.[10] These student outflows also generated substantial changes in school environments for those students who "stayed behind" at existing facilities. New school facilities enabled the district to reduce overcrowding and eliminate multi-track calendar schedules at both new and nearby existing schools. Our main analyses will focus on the students who switched to new facilities, as induced by the change in school assignment in the year of construction completion. Here, effects are estimated relative to a control group consisting of all other students in LAUSD, most of whom were unaffected by peer outflows to new facilities. Later, in Section 6.5, we will use an analogous identification strategy to examine changes in outcomes for the indirectly treated students who stayed behind at existing schools.

3 Theoretical Framework

The LAUSD school construction program induced dramatic changes in the physical and educational environment of district schools. By the end of 2012, roughly one-third of residential properties

[10] Among switching students, the average ratio of the cost of facility deficiencies to current replacement value of prior schools was 0.4.

within district boundaries were assigned to a school zone for a newly constructed school facility at least one level (i.e. elementary, middle, or high school). Improved school facilities can affect students in a number of ways. Reduced overcrowding and improvements in the physical school environment can have direct effects on student learning through reduced distraction[11] and improved health. Better facilities may improve student motivation and effort, leading to indirect improvements in student learning. Improved facilities may also improve teacher motivation and health, as well as help to attract and retain higher quality teachers (e.g. Buckley et al., 2004; Uline and Tschannen-Moran, 2008). Beyond direct and indirect effects on student learning, educational capital investments affect dimensions of the school environment that improve the amenity value of a school to both students and parents. Insofar as parents value improvements in educational quality and the physical amenities of a school campus, local real estate prices will respond to these changes. To fully evaluate the many potential impacts induced by the construction program, we use local changes in equilibrium housing prices to identify revealed preferences for educational spending changes.

The school construction program was funded primarily through local bond initiatives, which increased property taxes throughout the district to fund new school constructions only in a subset of district neighborhoods. Typical analysis of the valuation of local public goods relates changes in real estate prices to changes in the provision and tax-price of public goods. When educational spending increases are valued more than associated changes in taxes, real estate prices will rise, and vice versa. Importantly, however, only a subset of households receive additional school spending (in the form of capital spending on new school construction), meaning that the implied valuation will necessarily be asymmetric: neighborhoods that pay increased taxes but do not receive new school facilities should see prices fall, whereas prices will rise in neighborhoods in areas that receive new facilities to the extent that the additional spending in valued greater than the loss in consumption induced by the tax increase. We formalize this notion by examining the comparative statics of a such a tax and expenditure change within a simple hedonic equilibrium model, borrowing heavily from the models presented in Brueckner (1979), Barrow and Rouse (2004) and Cellini et al. (2010).

We begin by assuming there are N households, who derive utility from school amenities A_j, and consumption c: $U_j(A_j, c)$. Households can live in one of two neighborhoods: $j \in \{0, 1\}$. Households in neighborhood 1 receive new school facility spending, while households in neighborhood 0 do not. Denote the number of households in each neighborhood as N_j. New schools are funded by a tax τ on households, and the local government faces the budget constraint $R = \tau N$. The local government spends all of the revenues in neighborhood 1, and thus the per capita change in school funding (denoted R_j) is:

[11] Student distraction due to externalities from disruptive peers has been proposed as a motivation for class size effects (Lazear, 2001). Poor facility condition could cause similar disruptions that impede classroom learning.

$$R_1(\tau) = \frac{N}{N_1}\tau$$
$$R_0(\tau) = 0$$

To understand how the spending policy affects the level of school amenities, it is helpful to write the school amenity value as a function of tax expenditures: $A_j = A(R_j(\tau))$. Households receive income y and face the budget constraint $c = y - \tau - p_j$ where p_j is the rental price of housing. We can therefore write the household's indirect utility function as $V(A_j(R_j(\tau)), y - \tau - p_j)$.

When neighborhood j provides higher utility than alternatives, willingness to pay for housing there will be higher, prices will therefore be bid up. With homogeneous households, the equilibrium market price of housing will equalize utility in all neighborhoods.[12] A household's willingness to pay, or "bid", for a given neighborhood is therefore implicitly defined by function $P_j = P(\tau)$. Using the implicit function theorem, we can derive the change in neighborhood house prices, for a marginal increase in τ:

$$\frac{\partial P_j}{\partial \tau} = \frac{\frac{\partial V_j}{\partial A_j}}{\frac{\partial V_j}{\partial c}} \frac{\partial A_j}{\partial R_j} \frac{\partial R_j}{\partial \tau} - 1 \tag{1}$$

Equation (1) shows that the change in prices for a tax increase is a function of the marginal rate of substitution between the educational amenity and consumption, the marginal product of educational amenities with respect to educational expenditures, and the concentration of total tax receipts spent in a given neighborhood. In neighborhood 0, where $R_0 = 0$, $\frac{\partial R_0}{\partial \tau} = 0$. In neighborhood 1, $R_1 = \frac{N}{N_1}\tau$, so $\frac{\partial R_1}{\partial \tau} = \frac{N}{N_1}$. Therefore we have:

$$\frac{\partial P_0}{\partial \tau} = -1$$
$$\frac{\partial P_1}{\partial \tau} = \frac{N}{N_1} \left[\frac{\frac{\partial V_j}{\partial A_j}}{\frac{\partial V_j}{\partial c}} \frac{\partial A_j}{\partial R_j} \right] - 1$$

[12] This is true in equilibrium because if a household would achieve higher utility elsewhere, it would move. More generally, if we were to allow heterogeneity in preferences and/or income, the market price of a neighborhood would be equal to the bid of the marginal consumer, and marginal households with the same preferences and income would achieve identical utility.

Intuitively, as households in neighborhood $j = 0$ receive no additional educational expenditures, their marginal willingness to pay is exactly equal to the negative of the tax increase. For neighborhoods that receive the additional spending, their willingness to pay is equal to the product of the MRS and the marginal product of educational amenities with respect to expenditures, multiplied by the per-capita increase in expenditures, minus 1. Taking the difference in the two price changes yields:

$$\left(\frac{\partial P_1}{\partial \tau} - \frac{\partial P_0}{\partial \tau}\right) = \frac{N}{N_1}\left[\frac{\frac{\partial V_j}{\partial A_j}}{\frac{\partial V_j}{\partial c}}\frac{\partial A_j}{\partial R_j}\right] \tag{2}$$

Equation (2) shows that for a one unit increase in τ, relative prices will rise by the concentration of spending multiplied by the marginal valuation of the additional educational expenditures. For example, if households are evenly split between the two neighborhood types, relative prices will rise by two times the marginal valuation of the additional expenditures for households. When spending is at the efficient level, i.e. when the "Samuelson condition" holds, the aggregate marginal rates of substitution over all households will equal the marginal rate of transformation, and relative prices in neighborhoods that get public investments will rise by the concentration of spending per tax dollar: $\left(\frac{\partial P_1}{\partial \tau} - \frac{\partial P_0}{\partial \tau}\right) = \frac{N}{N_1}$. If prior spending levels were inefficiently low (i.e., if the marginal rate of transformation of funding into amenity value was higher than the marginal rate of substitution between amenities and consumption) and educational facilities had been under-provided, $(\frac{\partial V_j}{\partial A_j}/\frac{\partial V_j}{\partial c})\frac{\partial A_j}{\partial R_j}$ will be greater than one and prices will rise by greater than $\frac{N}{N_1}$, as marginal households value the increase in expenditures more than the forgone consumption. Alternatively, a relative price change of less than $\frac{N}{N_1}$ implies that the additional spending is inefficiently high, and that there had been over-provision of educational facilities.[13] Equation (2) therefore motivates an evaluation of the efficiency of the construction program using *relative price changes* between neighborhoods that received new schools and those that did not. Difference-in-differences estimates of price changes in response to school constructions will approximate (2) and provide a useful benchmark for evaluation of the program, which we will return to in Section 8.

Changes in real estate prices are informative about the product of the MRS between educational amenities and consumption, and the marginal product of additional capital expenditures. Examining the direct impacts of capital expenditures on student outcomes allows us to further understand $\partial A_j/\partial R_j$, the productivity of additional school resources. Assuming all of the amenity

[13] Over- or under-provision of educational facilities may result from allocative inefficiencies, where the district provides an inefficient level of facilities, or from productive inefficiencies, where the district does not minimize costs. In this paper we will abstract from this distinction when evaluating the overall efficiency of the expenditure program.

value of new schools comes through test score improvements, estimates of the treatment effect of attending new schools on test scores can be directly interpreted as a (non-marginal) approximation of this marginal product. Under this assumption, a direct comparison of difference-in-differences estimates of (2) and estimates of $\partial A_j/\partial R_j$ using student data allow us to recover plausible estimates the MRS for improvements in school quality for marginal parents. However, as discussed earlier, test scores likely only capture a portion of the amenity value associated with new school facilities; any such estimates will therefore represent upper bounds on the parental valuation of test score improvements.[14]

4 Data

4.1 Student data

To study the effects of increased capital expenditures on student outcomes, we use administrative records from LAUSD from the 2002-2003 school year to the 2012-2013 school year. Every student who attended LAUSD during this time period is included, and the data allow for longitudinal links across years for students who remain in the district. These data provide one record per student-year with information on student grades, test scores, demographics, attendance records, school assignment, and teacher assignments.[15] Demographics include gender, race, language spoken at home, parental education, and eligibility for free or reduced price lunch. Students in grades 2-11 are administered the California Standards Test (CST) annually in math and English-language arts (ELA). In each of grades 2-7, students take the same grade-level math exam; however, beginning in grade 8 the particular test depends on the student's particular math course enrollment. For the CST ELA exam, exams do not depend on a student's enrollment.[16] To ensure comparability of scores across students, we focus only on CST math scores for grades 2-7 and CST ELA scores for grades 2-11. Test scores are normalized relative to the California-wide mean and standard deviation reported in the California Standardized Testing and Reporting (STAR) documentation provided by the California Department of Education.

Total annual attendance, measured in days, is recorded for each student.For elementary school

[14] Recent work using revealed preferences from school choice applications suggests no relationship between parental school preferences and school productivity (measured by school test score value-added), once peer quality is taken into account (Abdulkadiroğlu et al., 2017). This need not be inconsistent with our findings: to the extent that parents value non-test score school improvements and/or new schools are a more salient signal of school treatment effects, increased local education expenditures would generate positive relative price changes in equation (2).

[15] 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.

[16] Some students with limited English proficiency and/or individual education programs take alternative exams. These students are excluded from all test score analyses.

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 over one dozen subjects. 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.

Data on teacher education, experience, age, and gender are available in all years, except 2009 and 2011. Teacher identifiers are also available for all years in the student data, and teachers can be linked longitudinally using unique teacher IDs. However, teacher IDs are scrambled between the secondary student and teacher demographic datasets, meaning that secondary school students are less reliably linked to teacher demographic variables. In the student data, each elementary record contains a single teacher identifier. Teacher-student links for secondary school are constructed using student-level course data. Principal names are available for 85% of student-year observations,[17] allowing us to construct within-district measures of principal experience. 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 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

[17] Principal names are listed at the school-year level for all but two years of our data. For these school-years with missing principal names, we assign a school its principal from the prior year if the school has the same principal in both the prior year and the following year.

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 selection 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 slightly worse areas and their catchment areas encompassed slightly lower performing and slightly more disadvantaged students.

4.2 Real estate data

To analyze the effects of increased capital expenditures on the real estate market in Los Angeles, we use administrative records from the Los Angeles County Assessor's Office. Records contain information for each property in Los Angeles county, and includes data on the three most recent sales,[18] as well 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). 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. We limit attention only to the years 1995 to 2012. 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 exclude the years 2013-2016 from the baseline real estate analyses, although results are robust to including these later years.[19]

We focus only on sales of residential properties with non-missing sales prices. We limit attention to single-family residences. We exclude large parcels with greater than 1 acre of usable area. We then drop the less than 1% of properties with missing information on property characteristics. 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.[20]

[18] As of April 2017, when we retrieved the data.
[19] See Appendix Table A6, where we compare results using all years to pre-2013 years.
[20] See Appendix Table A7 for a comparison of estimates with relaxed sample restrictions.

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

5 Empirical Strategy

5.1 Student Outcomes

To estimate the effect of attending a newly constructed school on student outcomes we use a generalized difference-in-differences strategy that relies on variation in the year a student begins at a new facility. Importantly, we only observe the school a student attends and not her actual neighborhood school assignment. Moreover, families may systematically sort between neighborhoods based on differences in preferences for educational quality and/or school amenities. If residential sorting or school assignment non-compliance are correlated with underlying student-level characteristics, estimates of the effect of attending a newly constructed school facility may suffer from selection bias. To address this, we rely only on *within-student* changes in outcomes over time, controlling for a student fixed-effect 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:

$$y_{it} = \alpha_i + \gamma_{g(i,t)} + \delta_t + \sum_{k=\underline{K}}^{\overline{K}} \beta_k \mathbb{1}(t = t_i^* + k) + \epsilon_{it} \tag{3}$$

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 (δ_t). Here, the coefficient β_k captures the effect of attending a newly constructed facility k years after the first year a student attends, t_i^*. $k = 0$ in a student's first year attending a school, and thus β_k estimates the effect of $k+1$ years of exposure to a new facility. Effects are measured relative to year $k = -1$, which is excluded in estimation. Endpoints are binned at $\underline{K} = -3$ and $\overline{K} = 3$,[21] which represent the average of student outcome y_{it} three or more years prior to attending a new school, or three or more years after first attending (i.e. 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 (3) estimates the effects of attending a new school separately by year. We can approximate the dynamics of these effects by estimating a more parametric version of (3) 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:

$$y_{it} = \alpha_i + \gamma_{g(i,t)} + \delta_t + \beta_1 \mathbb{1}(\tilde{t_i} \geq 0) + \beta_2 \mathbb{1}(\tilde{t_i} \geq 0) * \tilde{t_i} + \beta_3 \tilde{t_i} + \epsilon_{it} \tag{4}$$

Here β_1 captures the immediate effect of a new school facility in the first year a student attends, t_i^*. We include a linear trend in "event time", $\tilde{t_i}$, to control for any selection on trends into schools opening in a particular year. β_3 captures this selection, while β_2 reflects effects of the new school that accrue gradually over the time a student is exposed to a new school.[22] 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: $\beta_2 > 0$.

Estimates from equations (3) and (4) are presented in Section 6.1. Event-study estimates from equation (3) indicate that the parametric specification in (4) does a good job of capturing the dynamics of the effects on various student outcomes. Later, to more parsimoniously examine heterogeneity, mechanisms, and robustness, we will focus on the estimates from equation (4), and on even simpler versions that constrain $\beta_1 = \beta_3 = 0$. In our baseline estimation we use all student-year observations in the relevant grades for a given outcome, with the sole exception of those students

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

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

who attend multiple new facilities, who are excluded to avoid any confounds in the dynamics of estimated treatment effects. Students who never attend new school facilities are included in the regressions as controls,[23] as are students who we observe at newly constructed schools in their first year in the data (e.g. students who begin elementary at a newly constructed school, or transfer from another school district). Inclusion of the latter group of students may induce bias if students on different trajectories in outcome y_{it} sort into LAUSD to attend school at a newly constructed facility. Furthermore, students who "stay behind" at existing school facilities and see significant changes in their school and peer environments are also included as controls. In Section 6.3 we compare estimates where "stayers", never treated, and always treated students are excluded; reassuringly, results are very robust to the inclusion or exclusion of these students.

5.2 Real Estate Capitalization

As expected due to the design of the construction program detailed in Section 2, the location of the new schools is negatively selected: areas that received new schools had lower house prices, lower average incomes and educational attainment, and lower student test scores. However, conditional on a neighborhood receiving a new school, the timing of new school constructions is plausibly exogenous relative to any underlying neighborhood characteristics or trends. Thus, parallel to our estimation of student effects, we estimate house price effects of the program 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 variation in the exact year of school construction between these neighborhoods, 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:

$$ln(P_{it}) = \alpha_{j(i)} + \delta_t + \beta N_{j(i),t} + X_{it}'\Gamma + \epsilon_{it} \tag{5}$$

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

[23] Event-time indicators are set to zero for these students, who contribute only to the estimation of the year (δ_t) and grade ($\gamma_{g(i,t)}$) effects in the regressions.

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 δ_t are fixed effects for neighborhood and year, respectively.

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.[24] In all house price specifications, standard errors are clustered by neighborhood. Baseline specifications include only those parcels that are ever 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 (5) will yield an unbiased estimation of β. In addition, we estimate specifications that also include "never-treated" properties as controls, and specifications that control for year-by-high school zone fixed effects,[25] to flexibly account for differential trends in house prices between local areas.

If capitalization occurs prior to construction due to anticipatory effects,[26] 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 (3), that estimate the difference in house prices relative to the year prior to building occupancy:

$$ln(P_{it}) = \alpha_{j(i)} + \delta_t + \sum_{k=\underline{K}}^{\overline{K}} \beta_k \mathbb{1}(t = t_i^* + k) + X'_{it}\Gamma + \epsilon_{it} \tag{6}$$

In these non-parametric event study models, β_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 three or more years after the year of construction, respectively.

In equations (5) and (6), identification of β assumes that trends in house prices are uncorrelated with the exact timing of school construction, conditional on property-specific controls and controls

[24] See Figure A8 for a map of these neighborhoods.
[25] Here we use the high school zones from the 2004 school year, the year before the first new high school construction.
[26] Recall: new school locations were announced on average 5 years prior to school completion.

for time-invariant differences between neighborhoods. This assumption could be potentially violated if unobserved differences in the characteristics of those properties sold in a given year are correlated with the timing of switching; for example, if houses with positive unobserved characteristics are more likely to be sold within a given neighborhood post-construction than pre-construction. To account for this potential source of bias, we can estimate equation (5) with property fixed effects, controlling for time-invariant unobserved differences between individual properties:

$$ln(P_{it}) = \alpha_t + \alpha_i + \beta N_{j(i),t} + \epsilon_{it} \tag{7}$$

In equation (7), estimation of β relies only on properties with repeat sales in the sample window. Repeat sales indices are commonly used in papers 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 β are very similar in both equations (5) and (7), implying that differences unobserved property characteristics are uncorrelated with timing of construction and do not drive the estimated results. We find little evidence of differential house price trends in the years prior to school construction. Moreover, effects accrue quickly, typically within 2 or 3 years following construction. Therefore, we emphasize the simple linear differences-in-differences estimate of β from equation (5).

6 Student Results

6.1 Student achievement

Table 4 reports estimates of equation (4) for math (columns 1-3) and ELA (columns 4-6) standardized test scores. Cumulative four-year test score effects estimates are reported in row 4. In columns 1 and 4, a simple one-parameter specification is reported where only the change in the slope of student growth is included ($\mathbb{1}(\tilde{t_i} \geq 0) * \tilde{t_i}$). Here, the estimate on β_2 is 0.029 (SE 0.007) for math and 0.019 (SE 0.004) for ELA, implying that for each additional year a student attends a newly constructed school facility her test score increases by 3% and 2% of a standard deviation in math and ELA, respectively. The implied test score effect for a student who attends a new school for four years is 0.086 (SE 0.021) for math and 0.058 (SE 0.011) for ELA. Columns 2 and 4 add in indicators for attending a newly constructed school ($\mathbb{1}(\tilde{t_i} \geq 0)$). 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 essentially unchanged. Columns 3 and 6 add in a linear trend in student event time. The coefficient on the linear trend is marginally significant for math, and statistically significant for ELA. However, these coefficients are both minuscule: less than one-half of one percent of a standard deviation per year in both math and ELA. More importantly, the inclusion of the linear trend in the specification does little to affect the magnitude or statistical significance of the coefficient on the change in trend, while the total implied 4-year effect declines somewhat due to initial effects (β_1) that are slightly more negative.

Figure 4 reports estimates of the event study coefficients, β_k, from equation (3) 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 little 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 3.8% 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 10% (SE 2.6%) of a standard deviation higher. Estimates for standardized ELA tests, reported in panel B, are quite similar. Students who attend a new facility for 4 or more years score 5.2% (SE 1.4%) of a standard deviation higher in ELA. For both math and ELA scores, the event-study figures indicate that the parametric specification in equation (4) 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.

Both event study and linear difference-in-differences specifications show that student test score gains accumulate gradually, after a slight decline in student performance in the year of the switch. This pattern of gradual improvement is different from many other educational interventions considered in the literature, where effects tend to fade out over time. 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,[27] even in the absence of initial disruption effects due to student-level switching costs[28] or school-level inefficiencies

[27]The closest analogue is perhaps the STAR class size experiment, in which treated students were assigned to small classes for up to four consecutive years. In STAR, the treatment effects grew after the first year, like here, but at a slower rate. We see no sign here that the treatment effect is concentrated in the first year.

[28]Event study estimates for non-facility related student switches are reported in Figure A6. 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 student test score improvements. These findings are consistent with results in Hanushek et al. (2004), who find evidence of short-run disruption effects with no-long run

in the first few years post-construction.

6.2 Student non-cognitive effects

Table 5 reports analogous estimates for attendance (columns 1-3) and effort (columns 4-6). Unlike test score outcomes, which measure a stock of accumulated knowledge, student effort is a flow, and thus we would expect effects to occur immediately rather than accrue over time with continued exposure. For this reason, in columns 1 and 4 we begin with one-parameter specifications where only the coefficient for mean difference in the outcome post matriculation at a new facility ($\mathbb{1}(\tilde{t_i} \geq 0)$) included. Columns 2 and 5 add a phase-in coefficient ($\mathbb{1}(\tilde{t_i} \geq 0) * \tilde{t_i}$), and columns 3 and 6 include a linear trend in student event-time. For student attendance, estimates in columns 1-3 imply that most of the effect occurs immediately upon switching to a new school. In column 1, the estimate of β_1 is 3.97 (S.E. 0.55), meaning that student attendance increases by 4 days per year at newly constructed schools. Adding the phase-in coefficient in column 2 picks up some of this effect, reducing the coefficient on β_1 slightly. Column 3 adds in a linear trend in event-time, which does little to affect the estimates of β_1 and β_2. Estimates in columns 4-6 show a similar pattern for teacher-reported student effort, which increases immediately upon a student's switch to a new school. In column 1, the point estimate is 0.061 (SE 0.017), implying a 6% of a standard deviation increase in student effort at new schools. In columns 5 and 6 estimates of the pre- and post-trends (β_2 and β_3) are both small and insignificant, and the inclusion of these coefficients has little impact on estimates of β_1.

Figure 5 reports event study estimates for student attendance and teacher-reported 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 three days per year. In the second year a student attends a new school facility, this jumps to seven days. The effect tapers off somewhat in subsequent years, although after four or more years of attending a new school facility, students attend on average more than four additional days per academic year. Again, as with the student cognitive test score effects, there is no indication of a prior trend in student attendance in the years prior to switching to a new school facility; if anything, attendance appears to be declining slightly, although this trend is minuscule and insignificant - a decline in annual attendance of less than one-third of a day per year in the three years prior to switching.

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

gains for students who switch schools within-district.

by greater than 6% of standard deviation.[29] 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 roughly constant with additional years of exposure, and is statistically significant for the first three years a student attends a newly constructed facility. After 4 or more years of exposure to new elementary school facilities, the estimated effect on effort is slightly smaller, around 5% of a standard deviation, and no longer statistically significant. Notably, two years before attending a new facility, effort marks are roughly 3% of a standard deviation higher than in the year prior to attending a new facility, which is significant at the 10% level. Of all baseline event-study estimates, this is the only estimated pre-effect that is marginally significant, providing additional justification for the identification assumption that the timing of student switching is as good as random.

6.3 Robustness to sample treatment

Baseline estimates from one-parameter models for cognitive and non-cognitive outcomes in Tables 4 and 5 (columns 1 and 4) are reported in Table 6 for different sample definitions, varying the set of students used as the control group for students switching to new schools. As test score effects reflect the cumulative impact of multiple years of exposure to new schools, we compare one-parameter estimates of the phase-in coefficients (β_2) from models where we constrain $\beta_1 = \beta_3 = 0$. Reassuringly, implied cumulative 4-year effects from parametric estimates in columns 1 and 4 of Table 4 are indeed very similar to point estimates reported in Figure 4 for students who attended new schools for four or more years. On the other hand, as we expect the flow of student effort and attendance to increase immediately upon matriculation to a new school, we report one-parameter estimates of the mean difference post-new school matriculation (β_1) from models where we constrain $\beta_2 = \beta_3 = 0$.

Column 1 repeats baseline estimates reported in Tables 4 and 5. Column 2 excludes students who stay behind at existing schools when 10% or more of their cohort switches to a new school. Estimated coefficients for ELA and days attended are only slightly larger, while estimates for math and effort standardized scores are essentially identical.[30] In column 3, we drop all students who never attend new schools, using only "ever-treated" students. If students who switch to new schools

[29]Figure A5 also reports similar event-study estimates for teacher-reported student grades (in elementary school). Effects are noisier and insignificant, but suggest improvements of similar magnitude in report card grades only after the first year at a new school, which is qualitatively consistent with the observed patterns for test score effects.

[30]In Section 6.5 we specifically examine indirect effects on these students, finding evidence of small positive effects on ELA scores and attendance. Since these students make up only a small fraction of the overall "never treated" group in baseline regressions, we would therefore expect the magnitude of differences between columns 1 and 2 to be very small in the presence of small indirect effects.

are systematically different from those who do not, inclusion of never-treated students as controls may induce bias (though our inclusion of student fixed effects would absorb differences in outcome levels). However, this does not appear to be the case, as estimates are nearly identical for all outcomes. Column 4 further excludes students who appear in the data sample in their first year at a new school. Inclusion of these "always treated" students could be problematic if new school constructions systematically induce students of different ability to enter LAUSD, perhaps from private schools or from outside the district. As shown in column 4, estimated treatment effects are, if anything, slightly larger when only switching students are included in the estimation sample, implying effects are not generated by a resorting of students entering LAUSD to attend newly constructed school facilities.

In column 5, we restrict the sample to include a balanced panel of students in event time. As discussed in Section 4.1, ELA test scores are recorded for students in grades 2-11, and attendance is measured for all grades. Math test scores are only included for grades 2-7, and effort marks are only measured in elementary school (grades KG-5). Thus, for math and effort we include students who have outcome data both one year before and one year after switching to a new school facility. For ELA and attendance we need not be as restrictive, and use a balanced panel of students with non-missing outcomes both 2 years before and after switching to a new facility. Estimated treatment effects in column 5 are less precise, as expected given the reduction in sample size, but point estimates are if anything slightly larger than those in columns 1-4. Results are robust to these sample permutations, and we therefore conclude that baseline estimates including all students are not biased by differential sample selection in event time.

6.4 Mechanisms

The pattern of estimated student effects provides consistent evidence that cognitive and non-cognitive student outcomes improved at new school facilities. Are these improvements due to the increased facility quality itself, or due to other changes in the school environment associated with new school constructions? A thorough understanding of the mechanisms underlying student gains is important if the LAUSD construction program is to inform school capital expenditure decisions in other districts and institutional contexts. In this section we detail several facility and non-facility related changes associated with new school facilities (Figure 6 and Table A1). We examine heterogeneity in the results by prior school conditions to test whether these changes are systematically related to the observed student gains (Table 7).[31]

[31]Note: results in Table 7 are estimated using only switching students; baseline estimates correspond to those presented in column 3 of Table 6

We find little evidence that changes in class size, peer quality, or teacher quality at newly constructed schools can explain student improvements. In fact, we find that moving to a new school is associated with slightly larger class sizes and slightly lower teacher and peer quality. We do find that switching to a new school is associated with large reductions in overcrowding and increases in total instructional days. We find suggestive evidence that student gains are larger for students who switched from schools that were on multi-track calendars, for students who switched from more overcrowded schools, and for students who came from schools with a high share of portable classroom buildings. We find inconclusive evidence that students gains are larger for those coming from older or more deteriorated facilities, although the magnitude of these prior differences is small relative to the total change in facility quality for switching students.

6.4.1 Peer Composition

If students who switch to newly constructed school facilities are exposed to higher quality peers, changes in peer quality could explain some of the observed effects. As discussed earlier and shown in Table 2, students who attend newly constructed schools are more disadvantaged relative to students in the rest of LAUSD. However, new schools could offer better peer groups than do other schools in nearby neighborhoods. This could occur if new school boundaries were drawn within receiving neighborhoods in a such a way as to increase the concentration of more advantaged students at new school facilities. In addition, insofar as parents have some discretion to override school residential assignment, one might expect that higher-SES parents from outside redrawn boundaries would be more likely to petition to enroll their children in at schools with new and improved facilities. However, empirically, this does not appear to be the case. Panel A of Figure 6 shows event study estimates of peer quality, measured as the school (leave-out) mean predicted test score.[32] Average peer predicted scores fall significantly upon switching to a new school, and after 4 years at a new school average peer predicted scores are (insignificantly) below their level prior to switching. Columns 5 and 6 of Table A1 report estimates of the average peer differences associated with switching to a new school. Column 5 shows the change in school proportion black and hispanic, while column 6 reports the mean difference in peer predicted scores. Estimates show that students who switch to new facilities attend more segregated schools, with a 2.7 percentage point higher share of black and/or hispanic students. Consistent with Figure 6 panel A, peer predicted scores are on average 2.4% of a standard deviation lower.

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

6.4.2 Class size

Panel B of Figure 6 reports event study estimates for elementary school students where class size is the dependent variable. At new schools, class sizes were actually somewhat larger: class sizes increased by less than one-half of a student per teacher.[33] Column 4 of Table A1 reports analogous difference-in-differences estimates of the change in class size at new schools. On average, teachers at new schools taught classes with 0.31 more students per teacher. The magnitude of this difference, however, is quite small; roughly speaking, the district was approximately able to maintain similar pupil-teacher ratios at new school facilities by transferring teachers to new facilities in roughly equal proportion to students.

6.4.3 Multi-track calendar

One of the stated goals of the LAUSD school construction program was to eliminate the use of multi-track academic calendars that required schools to continuously operate year-round. Schools on multi-track calendars operate year-round and divide the students and staff into separate tracks, which are staggered throughout the school year in an effort to increase overall facility capacity. Moreover, in LAUSD, students at multi-track schools often had fewer instructional days per academic year.[34]

Before the construction program, half of LAUSD students attended multi-track schools. By reducing overcrowding in neighborhood schools, district officials were able to begin new schools on traditional two-semester calendars, as well as convert existing schools from multi-track back to traditional calendars. Column 1, panel A of Table A1 and panel C of Figure 6 report difference-in-differences and event study estimates of the likelihood of being exposed to a multi-track calendar. Switching to a new school was accompanied by a 27 percentage point reduction in the likelihood that a student was exposed to a multi-track calendar. This conversion also meant that many students in new schools experienced additional instructional days: as reported in panel D of Figure 6 (and column 2 of Table A1) students switching to a new school had on average nearly 2 additional instructional days per year, relative to the prior year at an existing school. Taking the baseline estimate of 4 additional days attended per year from Table 5, this implies that almost half of the observed attendance effect is mechanically due to a change in school calendar.

Student gains at new schools may be driven by increased instructional days and the conversion

[33] In fact, due to budget cuts in California during the Great Recession, LAUSD laid off roughly 25% of teachers between the 2008 and 2010 school years, increasing class sizes across the district, particularly in grades K-3.

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

back to traditional two-semester calendars. To examine this, we estimate separate treatment effects by prior school calendar schedule (multi- or single-track) for each main outcome variable. Rows 2 and 3 (panel 2) of Table 7 report estimated effects for students who switched from a school on a multi-track calendar or a single-track calendar, respectively. Results show that student test scores and effort effects are larger for students who switched from multi-tracked schools, yet none of these differences are statistically significant. For attendance, gains are larger for those students who came from multi-track schools, and the difference in highly significant ($p < 0.01$). As previously mentioned, this is driven largely by mechanical changes in the total number of instructional days.[35] However, the overall attendance effect is not entirely due to the calendar change: even those students who switched from single-track schools and saw no increase in total instructional days attended 2.7 additional days of school per year after switching to a newly constructed school. It is worth noting that students switching from schools on multi-track calendars also faced greater overcrowding and worse facility conditions on average than those coming from single track schools; these differences are therefore likely to represent an upper bound on the effect of converting from a year-round multi-track calendar school back to a traditional two-semester calendar.

6.4.4 Overcrowding

Students who switched to new schools also experienced reduced overcrowding, which was another primary motivation of the district construction program. Panels 3 and 4 of Table 7 present heterogeneity in estimated effects by two measures of overcrowding, where treatment effects are split by whether a student is above or below the median among treated students on either measure in the year prior to switching to a new school facility. The first measure, the number of students per square foot of classroom space, gives a direct measure of the physical classroom capacity of a school. The second, the share of permanent classrooms, measures the extent to which portable classrooms are used to accommodate a school's student population. Portable facilities are also often of much worse quality, and have less functionality than traditional classroom space. The share of classrooms in permanent vs portable structures therefore relates both to the level of overcrowding of a school, and the underlying facility quality.

Results split by prior SQFT per pupil indicate mixed and generally insignificant differences: gains are larger for ELA and attendance when coming from more crowded prior schools (i.e. a low SQFT per pupil), but smaller for math and effort scores. Only the difference in attendance is statistically significant; gains are large and significant for students coming from either above

[35]For students switching from multi- to single-track calendar schools, the average gain in total attended days was approximately 4 days per year.

or below median overcrowded schools for all but ELA scores. Estimates split by the share of permanent classrooms show a more consistent pattern: effects for students coming from schools with a low share of permanent classroom structures are larger for all outcome measures, and the differences in ELA and effort scores are statistically significant. In fact, estimates for math, ELA, and effort scores are statistically insignificant for students who came from schools with relatively more permanent classroom space. This pattern of results suggests that reductions in overcrowding are important but alone do not fully explain observed test score gains: there are few systematic differences by prior SQFT per pupil, while differences by the prior share of permanent classrooms additionally reflect fundamental improvements in facility quality beyond overcrowding.

6.4.5 Facility Condition

New school constructions induced drastic changes in the facility quality for students who switched. Students who switched to new schools came from a school that was, on average, 70 years old (panel A, Table A1) and had substantial deficiencies. Results split by the share of permanent classrooms provided indirect evidence that observed student gains were larger for students switching from schools that were of poor quality. In panels 5 and 6 of Table 7 we examine heterogeneity in treatment effects by the age and physical condition (measured by FCI[36]) of a student's prior school. Results indicate that all student effects are larger for students switching from older schools, although these differences are insignificant. Student test score and effort effects are very similar between students switching from schools in above or below median condition, and the differences are insignificant. Only for student attendance is the difference significant; students switching from schools in relatively better condition (low FCI) actually saw larger attendance gains.

With the exception of estimates by the share of permanent classrooms, results presented in Table 7 provide inconclusive evidence of heterogeneity in student effects by prior facility quality. This does not necessarily imply that facility quality improvements themselves were not important: the variation in facility improvements within treated students is small relative to the change experienced for any student switching to a new school. Moreover, these variables are imperfect proxies for "true" facility quality, which we cannot directly quantify.

Overall, estimates in Table 7 suggest reductions in overcrowding and multi-track calendars may explain up to half of observed student effects. Later, in Section 6.5 we will examine students who stayed behind at existing school facilities and experienced significant peer outflows. These students experience very similar reductions in overcrowding and multi-track calendars, yet for these students

[36]Recall: the FCI is the ratio of deficiencies to current replacement value. An FCI close to zero indicates a facility is in good physical condition, whereas an FCI of greater than one indicates that a facility has deteriorated to the point where the total cost of deficiencies is greater than the total replacement cost of the facility.

we find much smaller effects, and only for ELA and attendance. Thus, taken together with results presented here, we argue that at least half of the observed test score effects for switching students are therefore attributable to the direct improvement in the physical school environment.

6.4.6 Teacher quality

Student gains at new schools could in part be due to systematic differences in teacher quality. New school facilities provide improved working environments for teachers, and these amenities could attract better quality teachers to these schools from either within or outside the district.[37] Due to budget cuts following the Great Recession, the district effectively stopped hiring new teachers: prior to 2009, roughly 10% of the teachers in LAUSD in any given year were new entrants, while afterwards this decreased to 4% or less. Even for those new facilities that opened before this reduction in teacher hiring, the teaching staff was composed of greater than 80% existing teachers who switched from elsewhere in the district. Following the reduction in teacher hiring, schools opening in 2009 or later this proportion increased to over 90%. Thus, any differences in the quality of new teachers is unlikely to explain a large share of the observed effects. However, new facilities may have attracted relatively better teachers from within the district. Improved non-wage amenities at new school facilities could have led to sorting of higher quality teachers into new schools. On the other hand, priority for intra-district teacher transfers within LAUSD was allocated using a tenure-based point system, which may not be systematically correlated with underlying teacher quality (broadly defined).

Systematic teacher resorting would imply that student gains at new schools came at the expense of students at existing schools; any within-district resorting of existing teachers would be zero-sum in aggregate. To empirically assess whether differential sorting of higher quality teachers into new school facilities explains any of the observed student gains, we compare differences in teacher observables and test score value-added in Table 8. Panel A reports differences in teacher observables at new schools. Students who switch to new school facilities have teachers who are, on average, less experienced, younger, and slightly more likely to have a masters degree. Students at new school facilities are also 5.4 percentage points more likely to have a new teacher in either math or ELA. Observable teacher characteristics, however, are generally not highly correlated with test-score based measures of quality. Thus, in panel B, we examine differences in test score value-added for teachers at new schools.[38]

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

[38] See Appendix B for an explanation of how teacher value-added scores are calculated at the teacher-year level.

Standard value-added models can confound school and teacher effects. For example, new school facilities could generate improvements in student attentiveness and/or teacher productivity, both of which would result in gains in estimated teacher valued-added. However, student gains resulting from school improvements would reflect improvements resulting from the new facility itself, and not from variation in underlying (prior) teacher quality. Thus, to directly assess whether teacher resorting explains any of the student gains, we focus specifically on switching teachers, for whom we have an estimate of value-added based on student test score observations from their prior, existing school facilities.

For these switching teachers, we compute the student-weighted average of prior value-added scores, using only data from years a teacher taught at an existing school facility. Specifically, we define $\text{VA}_j^{prior} \equiv \sum \frac{n_{jt}}{n_j} \text{VA}_{jt}$, where VA_{jt} is the estimated value-added for teacher j in year t, n_{jt} is the number of student observations for contributing to teacher$j$'s value-added score in year t, and n_j is the total number of students taught by teacher j (prior to switching to a newly constructed facility). For each student-year observation, we assign the mean prior value-added score, averaged over all teachers in a given school-year.[39] Columns 1 and 2 of panel B report difference-in-differences estimates of the change in mean prior value-added for students attending newly constructed school facilities. Results indicate that students who switched to new schools experienced teachers with *lower* test-score value-added scores than prior to switching. The point estimates are for both math and ELA are small, although the estimate is more negative and statistically significant for ELA.

We find little evidence of positive restoring of existing teachers into new schools, but it could still be the case that the new teachers hired into new schools were of differential quality. We cannot directly compare contemporaneous value-added scores of new teachers at new and existing schools, as this would confound student gains due to school-level facility improvements with improvements in new teacher quality. However, under the assumption that new facilities affect novice and experienced teachers identically, we can assess the quality of new teachers by testing whether the school-level gap in value-added scores between new and existing teachers is larger or smaller at new facilities. We can decompose the estimated teacher effect to include the true teacher effect, a new-school specific shock, and an unobserved error term:[40]

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

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

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

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

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

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

Columns 3 and 4 report these estimates, where the dependent variable is the school-year mean gap in value-added between novice and experienced teachers, $\overline{\text{VA}}_{st}^{GAP}$. The point estimate for math is small, negative, and insignificant. For ELA, the point estimate is positive and of larger magnitude, but insignificant. Given that we find evidence of negative sorting of existing teachers on value-added ($\overline{\mu}_{jt}^{Old} \leq 0$ in columns 1-2), the difference in point estimates between columns 1 and 3 and columns 2 and 4 would need to be positive to support an interpretation that newly hired teachers were of higher quality at new facilities. As the estimated coefficients in columns 3 and 4 are small and noisily estimated we do not report a formal test of these differences. Results from Table 8 panel B therefore provide little evidence that newly hired teachers were of higher quality at new schools.

Overall, the evidence presented in Table 8 reveals that systematic differences in teacher quality cannot account for observed student test score gains. As the overwhelming majority of students at new schools were taught by existing teachers, point estimates from columns 1 and 2 of panel B imply that student test score gains at new schools would have been roughly 15% larger in math and 50% larger in ELA had teacher quality remained constant. The upper bound of the 95% confidence interval for the math effect can rule out positive teacher sorting explaining more than 30% of the total effect from column 1 of Table 4. In the longer-run, it is still possible that higher-quality facilities could attract and retain better teachers, although further research is necessary to determine if this channel to improve teacher quality is empirically relevant.

6.4.7 Principal quality

Improvements at new schools could be attributable to principal sorting and differential principal quality between new and existing schools. Principals and school administration are important inputs in education production, and recent work has shown that improved managerial skills among principals can have positive effects on student achievement (Fryer, 2017). While we lack direct measures of principal quality, we examine principal experience as a proxy. Using data on principal names, we construct measures of within-district principal experience to test whether new schools were more likely to have more experienced principals. On average, however, the opposite is true: new schools employ principals who have roughly 0.5 years less experience in any given year. 65% of new schools begin with a new principal that has no prior within-district experience; similarly, of existing schools that switch principals during our sample, 64% hire a principal with no prior experience. To more directly examine the change in exposure to principal experience for treated students, we provide within-student difference-in-differences estimates in Table A3. Students who switch to a new school are exposed to principals that are 14 pp (SE 2.1) more likely to be new as a principal in LAUSD, and have 0.83 years (SE 0.146) less experience as a principal in the district. While these are not direct measures of principal quality, we view this as compelling evidence that principal quality does not mediate the positive effects we find, and that if anything, principal quality may have been lower at the newly constructed schools.

6.5 Effects on staying students

Students who switched to new school facilities were not the only students to experience significant school-level changes: student switches to new facilities induced cohort-level outflows from existing facilities. Those students who stayed behind experienced reductions in overcrowding, conversion from year-round multi-track calendars back to traditional two-semester calendars, and changes in peer composition, but not improvements in facility quality. Thus, examining the effects of new facility openings on the outcomes of students who stayed behind at existing facilities can shed light on the relative importance of crowding vs direct facility quality effects in producing the aforementioned estimated impacts on students at new schools.

We define "stayers" to be students for whom 10% or more of their school-grade cohort switched to a newly constructed school facility.[41] We then define event-time analogously for these students: year "0" is the year in which a school cohort experienced a large outflow induced by a nearby new school construction. We estimate effects for these students using the same event study methodology

[41]Appendix Figure A7 reports analogous event study estimates using a 20% threshold. This reduces the sample considerably, but results are robust to alternative thresholds.

for the main student effects presented in equations (3) and (4); because these cohort outflows were induced by new facilities, estimates rely on the same variation in the timing of construction between different students.

Panel B of Table A1 presents estimates of the changes staying students experienced after they experienced a cohort outflow, analogous to estimates for switching students presented in panel A. Students who switched to new schools are excluded from estimation; estimates are relative to a control group of students in the same grade and year who have yet to experience a cohort outflow shock, and never-treated students who experienced no significant peer outflow. Results indicate that stayers experienced a significant decline in multi-track calendar usage and a significant increase in the total number of instructional days per year. Both staying and switching students experienced a roughly equivalent decline in multi-track calendars, while staying students actually experienced a slightly larger increase in the total number of instructional days than switching students (2.3 days vs 1.7 days). Class sizes decreased slightly for students who stay behind, by about one-third of a student per teacher. Though significant, the magnitude of the effect is negligible.[42] Columns 5 and 6 report changes in the average peer group. Consistent with the fact that switching students were slightly more disadvantaged and lower-scoring than staying students, stayers see reductions in peer minority shares and increases in predicted scores of peers due to cohort outflows to new facilities. Taken together, these results suggest that small indirect effects would be likely, even in the absence of facility improvements, due to the reductions in overcrowding, increase in instructional days, improved peer quality, and slightly decreased class size.

Figure 7 shows event-study estimates of cognitive and non-cognitive outcomes for stayers. Stayers see small increases in math (panel A) and ELA (panel B) test scores, although the math effects show some indication of a pre-trend prior to the year of the cohort outflow to a new facility. The increase in days attended (panel C) is immediate and significant - students attend roughly 4 more days relative to the year prior to the cohort outflow. As was the case for switching students, much of this increase derives from the reduced use of multi-track schedules in stayers' schools. Panel D shows estimates for standardized effort scores, for which the point estimates are all very close to zero and insignificant.

Parametric versions of the estimates corresponding to equation (4) are reported in Table 10. For each outcome, both one- and three-parameter estimates are shown. Columns 1 and 2 report estimates for math test scores. Estimates in column 1 show no change in test score growth in the years following the cohort outflow, while estimates in column 2 show that once pre-existing trends are included, there is a small effect immediate effect that fades out within the following year. For

[42]Note that the effect is similar in (absolute) magnitude to the increased class size documented for switching students in panel A.

ELA (columns 3 and 4), the pattern is different, and the parametric estimates more closely align with the event study estimates. Column 3 shows an 0.01 standard deviation increase in ELA test score growth in the years following the cohort outflow. However, once the post indicator and trend variable are included in column 4, all of the effect loads onto the post coefficient, with no ensuing growth or fade-out of effects. This pattern of cognitive effects differs from that of students attending new schools: effects accrue immediately, and either fade out (math), or remain constant (ELA). Columns 5 and 6 report estimates for days attended. Stayers see a roughly 3.5 day increase in days attended, which is robust to the inclusion of trend variables. Comparing these estimates to the estimated 2.3 day increase in total instructional days from column 2 of Table A1 Panel B implies that roughly two-thirds of the attendance effect is mechanically driven by increased number of days. Columns 7 and 8 show no effects on teacher-reported effort.

Taken together, these results are suggestive of positive indirect effects induced by peer outflows to new school facilities, but only for ELA test scores[43] and total days attended.[44] Attendance effects are mostly driven by an increase in the total number of instructional days, and the residual non-mechanical effect is roughly half the size as for switching students (1.3 vs 2.3 additional days). These indirect effects are likely driven by reductions in overcrowding, improved peer quality, and the switch from multi-track calendars to traditional schedules.

The small magnitude of effects relative to baseline effects on switching students implies that reductions in multi-track calendars and overcrowding alone cannot explain the bulk of baseline effects, as these changes were similar for students who stayed behind at existing schools. Moreover, other notable changes in the school environment (peers, class sizes, and teachers) all went *against* finding positive test score effects. This supports the conclusion that direct facility quality effects – e.g. Increased concentration due to reduced distractions from inadequate heating, cooling, or other aspects of the physical environment – account for a substantial portion of the new school effect seen earlier.

7 Real estate capitalization

Next, we turn to the analysis of the impact of new school openings on local housing prices. In Table 10 we present difference-in-differences estimates corresponding to equations (5) and (7), while in

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

[44]Note that this is consistent with the evidence reported in Table 6, which showed only small increases in the ELA and attendance estimates when the stayers were excluded from the control group (column (2)). Given that stayers represent a small fraction of the control group, we would expect that the inclusion of stayers in baseline regressions only produces a small downward bias in the presence of small positive indirect effects.

Figure 8 we report event study estimates corresponding to equation (6). Panel A of Table 10 reports estimates of the effect of new school constructions on house prices. Columns 1-5 report estimates using fixed effects for school zone and property-specific control variables, which corresponds to the specification in equation (5). Columns 6 and 7 report estimates of equation (7) using property fixed effects.

Column 1 reports estimates using all properties in LAUSD and basic year and neighborhood effects. The point estimate is negative and insignificant, which indicates that neighborhoods in areas that did not receive new schools saw, if anything, larger increases in house prices during the sample period. However, using uniform year effects for all of LAUSD may confound the effects of differential price trends and shocks in different areas of the city and surrounding areas. Recall that the new schools are concentrated in East LA, where baseline prices were low and poverty rates high relative to the rest of the district. For example, if house prices in more affluent areas were already growing at a higher rate than those in less affluent areas (where the new schools were mainly built), difference-in-differences estimates of the effects of new schools could be biased downwards. Rather than impose parametric trends for each neighborhood, in column 2 we substitute year effects for year-by-high school zone[45] effects to more flexibly account for any differential local house price trends or changes. The point estimate flips sign and is statistically significant, implying that 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.

Properties that are very far from new school zones are included as "never-treated" controls in columns 1 and 2, and even with the inclusion of year-by high school zone effects we may still be worried about bias from the inclusion of these properties. To account for this, in column 3 we drop "never-treated" properties further than one kilometer from a new school zone, and in column 4 we further restrict the sample to only those properties that ever receive a new school.[46] Results in columns 2 and 3 are nearly identical, and the estimated coefficient drops slightly to 4.4% (SE 1.1%) in column 4. Column 5 substitutes year effects for the year-by-high school zone effects introduced in column 2 – 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%).

To address additional concerns that within-neighborhood difference-in-differences results may be biased by fixed unobserved property-level differences, we rely on repeat sales and estimate effects *within-property*, using property fixed effects to account for any such differences. Columns 6 and 7

[45]Recall, here we define school zones using pre construction boundaries from 2000, to eliminate concerns over endogenous new school attendance boundary formation. Reassuringly, this distinction makes no quantitative difference, as results are nearly identical when post construction boundaries are used instead (Table A6 panel B).

[46]By restricting to these properties, identification is coming solely off variation in the timing of when a specific neighborhood receives a new school facility.

report estimates analogous to columns 4 and 5 using property fixed effects; property controls and neighborhood fixed effects are excluded. Here, variation comes only from properties sold multiple times during the sample window, resulting in a sample size reduction of nearly half. In column 6, estimation includes year-by-high school zone effects, while column 7 shows estimates where only year-specific effects are included. Estimated effects are very similar to analogous neighborhood fixed effects estimates in columns 4 and 5. Overall, estimates imply that house prices increase by roughly 4-6% post-construction in new school attendance areas.

Difference-in-differences coefficients correspond only to the mean difference in house prices pre vs post construction. Pre-existing differential trends between neighborhoods in the same initial high school zone could still induce bias, even with the inclusion of flexible year-by-high school zone effects. More importantly, difference-in-differences estimates obscure the dynamics of effects, which could result in downward bias if capitalization occurs gradually, and/or in anticipation of construction. New school locations were announced on average 5 years before completion: real estate capitalization may occur in advance of school completion insofar as parents and other homebuyers are forward-looking and are able to anticipate whether a given property falls within the school assignment zone for the new school. On the other hand, initial uncertainty by parents in the actual improvements generated by a new school may lead to more gradual capitalization post-construction, as the quality of the new school is revealed.

To account for flexibly for any dynamics in the timing of capitalization effects, in Figure 8 we report event study estimates of the effects of new school constructions, corresponding to the specification in equation (6). In panel A, estimation includes only those properties ever within any new school zone and year-by-high school fixed effects, corresponding to the specification in column 4 of panel A of Table 10. In panel B, we include all never-treated properties in LAUSD as controls, corresponding to column 2. Effects are estimated relative to the year before school occupancy, which is omitted from the regression. Results in both panels of Figure 8 show little sign of pre-existing trends or dynamic anticipatory effects pre-construction: all estimated pre-construction effects are practically zero. 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 10.

As discussed in Section 6.5, schools that experienced large student outflows to new schools saw significant reductions in overcrowding and multi-track calendar utilization, and small but significant increases in the share of more advantaged students. Students at these schools also experienced gains in ELA scores and attendance. To what extent were these gains at existing "sending" schools capitalized into local house prices? In panel B of Table 10 we report difference-in-differences estimates where treatment is similarly defined for existing "sending" schools that experienced student outflows to newly constructed facilities.[47] Specifications in columns 1 and 2 correspond to those in columns 1 and 2 of panel A; specifications in columns 3-6 correspond to those in columns 4-7 in panel A. Overall, results provide little indication that house prices increased in the sending school neighborhoods. In column 1 the coefficient is positive and significant, but this result is not robust to the inclusion of year-by-high school zone effects in column 2, nor the exclusion of never-treated properties in columns 3-6. These results suggest that (a) parental valuation of new schools is driven by non-test score/amenity improvements at new schools, independent of the school calendar or level of overcrowding, and/or (b) improvements in school quality due to reductions in overcrowding and multi-track calendar utilization are less salient to prospective homebuyers, who may instead rely on school facility condition as a signal for underlying school quality.

7.1 By neighborhood price

While new school quality was similar across treated neighborhoods,[48] the tax price of the new facilities faced by district residents was greater in areas with higher property values.[49] Later, in Section 8, we use the estimated house price effect for a welfare calculation, applying the coefficient to the mean home value in LAUSD. But insofar as home prices capitalize local investment, one might expect larger percentage effects on prices in low-price neighborhoods than in high-price neighborhoods. If so, applying the average percentage treatment effect to the average house price could overstate the aggregate impact. Empirically this does not appear to be the case. In Figure 9 we report heterogeneity in estimated treatment effects by neighborhood prior mean house prices. We define neighborhood prior mean house prices as the average house price in a neighborhood over all pre-treatment years in the sample, 1995-2001. Estimates of β from equation (5) are shown interacted with $100,000 bins of neighborhood prior mean house prices.[50] With the exception of the $500,000 -$600,000 bin, all effects are similar and statistically significant, providing little evidence of smaller estimated treatment effects in areas with higher property values.

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

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

[49] Unlike in the model presented in Section 3, which assumed a constant lump sum tax for all households, property owners in higher-priced areas contributed a greater dollar amount towards district bond revenues.

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

7.2 Local boundary and spillover effects

Increased demand for neighborhoods receiving new schools could have differential effects on house prices near the boundaries of new school neighborhoods. School assignment boundaries do not stay constant in perpetuity, and due to uncertainty over future boundary locations, capitalization effects may be smaller near the boundaries within new school zones. In addition, if home buyers substitute housing in existing school zones for housing purchases in new school zones, prices could decline in other LAUSD neighborhoods. On the other hand, new school constructions and changing neighborhood composition could lead to spillovers that increase house prices both within and near new school zones. Prices in nearby neighborhoods that did not receive new schools could increase due to positive externalities from neighborhood upgrading (e.g. Hornbeck and Keniston (2017)). Moreover, new schools could act as a direct amenity that generates positive benefits (e.g. increased park/playground space) both within and outside the actual attendance areas. Estimates in Figure 10 and Table 11 assess the extent to which the effect of new school constructions varies by distance to the attendance boundary, and whether new schools generate spillover effects beyond the attendance zone.

Table 11 reports estimates of treatment effects by distance to the school attendance boundary. Column 1 repeats baseline estimates from column 2 of Table 10 panel A. In column 2, we add a coefficient for distance from the boundary (in kilometers) and distance to the boundary interacted the treatment dummy. Both added coefficients are small and insignificant, and do little to affect the point estimate on the treatment indicator in the first row. Column 3 adds an indicator for being within 2 kilometers of a new school zone, after completion. For these properties, we assign the treatment date of the nearest new school construction. Effects are estimated relative to properties greater than 2 kilometers from any new school boundary. The estimated effect on properties just outside the new school's attendance zone is -1.3% (SE 1.0%) and insignificant. These estimates provide little evidence of substitution patterns that indicate decreased demand for housing in existing school attendance zones within 2 kilometers of a new school zone, nor that new school zones generate positive spillovers in nearby neighborhoods, as would be expected if new schools induced general neighborhood amenity upgrading. Column 4 adds in controls for distance and the interactions with the treatment indicators. Here the treatment dummy for being outside the zone is highly negative and significant, while the interaction with distance is positive and significant, implying a large negative effect on house prices immediately after crossing a new school attendance boundary that fades out within 1 kilometer outside the boundary.

Figure 10 provides a non-linear visualization of the pattern reported in column 4. Each point represents a difference-in-differences treatment effect estimate interacted with distance to the new school attendance zone boundary, in 400 meter bins. Properties with positive distance are located within new school boundaries, while those with negative distance are in school zones where the residential assignment is to an existing school. Results indicate that within the new school zones, capitalization is roughly constant at approximately 5% for all distance bins. We find no evidence of smaller effects closer to the boundary. Properties within 400 meters but outside of the boundary actually see statistically significant declines in house prices of 4.9% (SE 1.7%) post-construction, providing suggestive evidence of negative spillovers for properties that are "unlucky" enough to fall just outside the new school zone. These negative spillover effects quickly diminish however; point estimates for distances greater than 1.2 km are positive, though insignificant, consistent with the findings from Table 11. This pattern is consistent with cross-neighborhood substitution within very narrowly defined markets, wherein demand for properties located marginally outside the new school zones decreases for prospective homebuyers searching within the vicinity a new school.

8 Welfare Analysis

Thus far we have shown that new school constructions in LAUSD generated large student cognitive and non-cognitive gains. These improvements in school quality - physical and educational - were capitalized into the real estate market, as properties in new school attendance areas saw large and significant increases in prices post construction. In this section we use our estimated price effects to compute the implied willingness-to-pay for residents who received new schools. As outlined in Section 3, the magnitude of difference-in-differences estimates of the relative price change induced by new school constructions provides a benchmark to assess the economic efficiency of the spending program. One-third of households in LAUSD reside in a new school attendance zone. Thus, if the estimated relative price change is less than the per household cost of the program, multiplied by three, then we can infer that homebuyers value the new schools less than the cost of building them, and therefore that using taxpayer money to build new schools reduced welfare. Conversely, if the estimated price change exceeds this we can infer that the additional expenditures were valued in excess of the total program cost by homebuyers.

This computation relies on strong assumptions. Most notably, we assume that the observed price change affects all household units in LAUSD, although we only estimate on the subsample of single-unit properties that sold during the sample window. According to the 2005-2009 American Community Survey (ACS), there are 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 housing unit of the program is approximately $6,045 in present value. During the treatment period from 2002-2012, the average sale price (within-sample) of properties in zones that received new schools was $494,650. Using the estimates in Table 10 panel A, this implies a price change in the range of $21,765 to $29,679, where the preferred estimates from column 2 using all properties in LAUSD are the upper bound of that range. Comparing this to the program cost per housing unit in a new school attendance zone, $3 * \$6,045 = \$18,135$, implies a willingness-to-pay ratio in the range of 1.2 to 1.64.[51] Put differently, each additional dollar of capital expenditures by the district generated 1.2 to 1.64 additional dollars in the real estate market. These results suggest that the value to families of the school capital expenditure program was greater than the program cost, implying the program raised welfare.

The real estate valuation of the program incorporates the market valuation of all potential benefits generated by the new school program, beyond simply the effects related to increased academic performance of students. However, many studies of educational interventions rely on extrapolations of test score effects to assess a program's efficiency. 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. use experimental variation in classroom quality to estimate that a 0.1 standard deviation increase in test scores[52] leads to a 1.3% increase in earnings at age 27. To extrapolate our estimates forward, we first compute the present discounted value of future earnings for future cohorts:

$$PDV_{cohort} = N_c \sum_{t=16}^{56} \frac{E_t}{(1+\delta)^t}$$

where E_t = earnings gain at each age, which we compute under the assumption of a constant percentage gain of 1.3% per 0.1 SD increase in test scores, using age-earnings profiles from the March CPS.[53] The average elementary school student is 11 years old, therefore we discount forward 16 years to age 27, and count benefits until retirement at age 67. From our data, roughly 16% of students entering elementary school, 13% of students entering middle school, and 25% of students entering high school in LAUSD were in a newly constructed school facility. Plugging this in and using the estimated effects on math test scores, assuming a 3% discount rate, yields a present discounted value of future earnings per cohort of $150 million. From our facilities data, we estimate

[51] Using instead the average price over all properties in the treatment period, as suggested by the model in Section 3, would increase this WTP ratio by roughly 33%, to a range of 1.62 to 2.21.

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

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

that a brand new facility would take roughly 35 years to depreciate to the mean condition of existing facilities in LAUSD. Assuming the effects are constant for this 35 year horizon and discounting the earnings of future cohorts implies a gain in future earnings of \$3.8 billion in present discounted value. The total program cost was \$9.17 billion, implying that the gain in future earnings from test score improvements covers roughly 40% of the total program cost.[54]

Real estate capitalization greatly exceeds the estimated increases in future earnings from test score improvements, providing strong evidence that parental valuation of educational expenditures exceeds benefits captured by test scores alone.[55] New schools also generated improvements in student non-cognitive outcomes, improvements in school safety and health, and allowed for increased access to extra-curricular opportunities, among many other benefits. While test score improvements provide a useful benchmark for interpreting the efficiency of educational interventions, they are likely to severely understate the true benefits of capital infrastructure investments.

9 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 capital expenditure programs that impact only a subset of students (Figure A1). 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 significant improvements in student effort. New facilities also generated indirect improvements for students elsewhere in the district who did not attend new facilities, but nonetheless saw improvements in their school environments due to peer outflows to new facilities. Reductions in overcrowding and the elimination of "multi-track" academic calendars only account for some of the observed gains, implying that capital improvements themselves were responsible for student gains beyond reductions in congestion.

New school constructions induced large increases in neighborhood house prices upon completion, implying significant parental valuation of improvements in school quality, generally defined. House prices increased substantially in areas that received new schools, although we find no evidence of

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

[55]Appendix Figure A10 plots school-level test score treatment effects against school-level house price effects, showing little systematic relationship between the two in both math and ELA.

similar price increases in existing school zones that sent students to new schools and experienced corresponding reductions in overcrowding. Overall, house price estimates imply that the total real estate capitalization exceeded program cost, and suggest an implied willingness-to-pay on behalf of district residents of 1.2 to 1.6 for one dollar of per-household school capital investment. Willingness-to-pay estimates provide evidence that prior capital spending had been inefficiently low in the distinct, and that the targeted program to improve facilities for the most disadvantaged students in the district generated aggregate welfare increases in the district. These findings are especially relevant for large, urban districts and other districts serving low-income students with a history of facilities underinvestment, and imply that policies to improve school capital can be productive and efficient uses of public funding.

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). The extent to which each of these two theories can explain the effects may also explain why we the impact on achievement and house prices is only weakly correlated (see Figure A10). With more precise data on changes in facility conditions in the course of a facility program, future research may be able to distinguish between these two theories and provide guidance on which facility components have the highest return in terms of learning and real estate capitalization.

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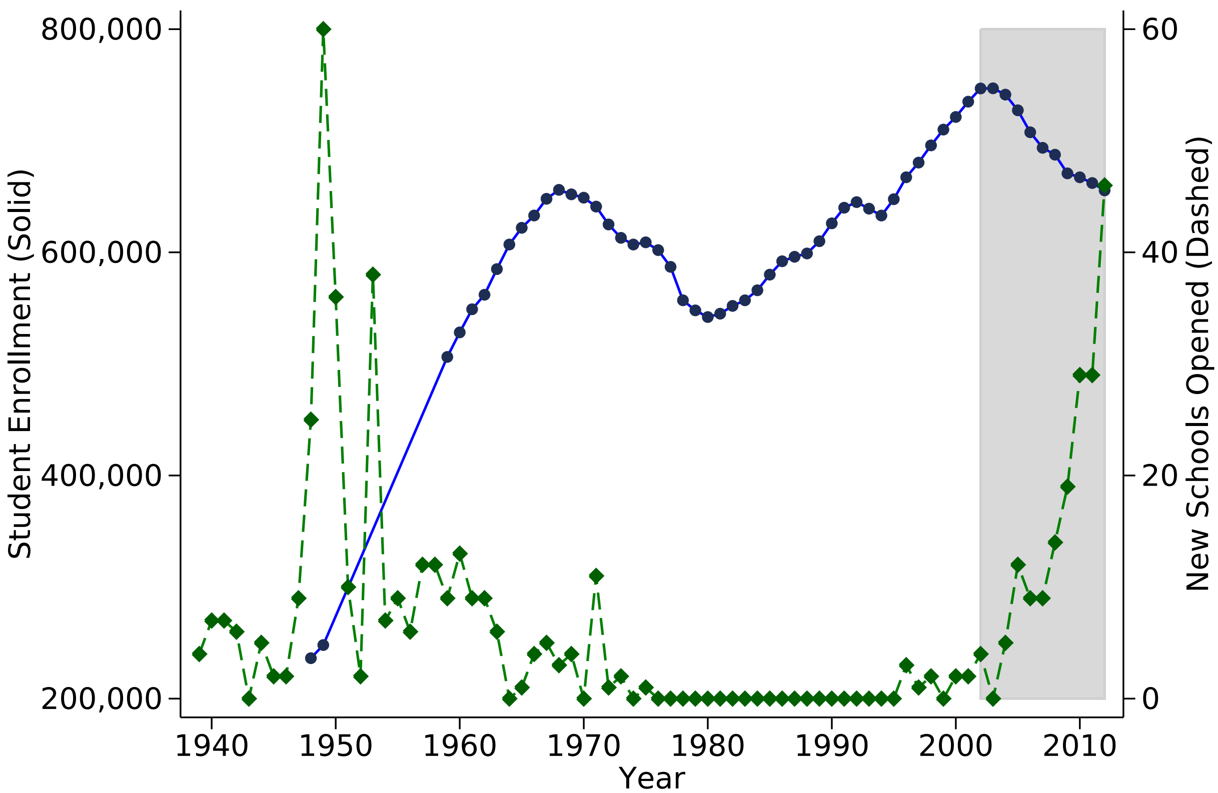
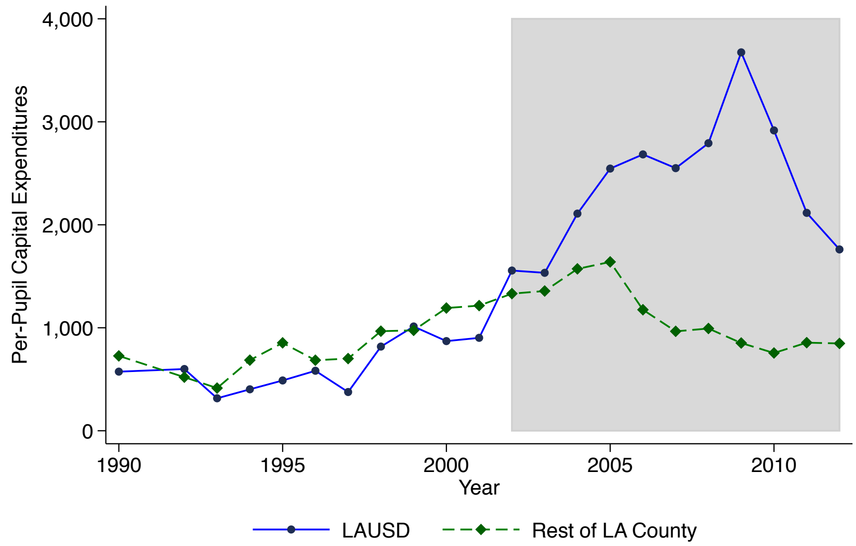
Figures

Figure 1: School construction and enrollment, LAUSD 1940-2012

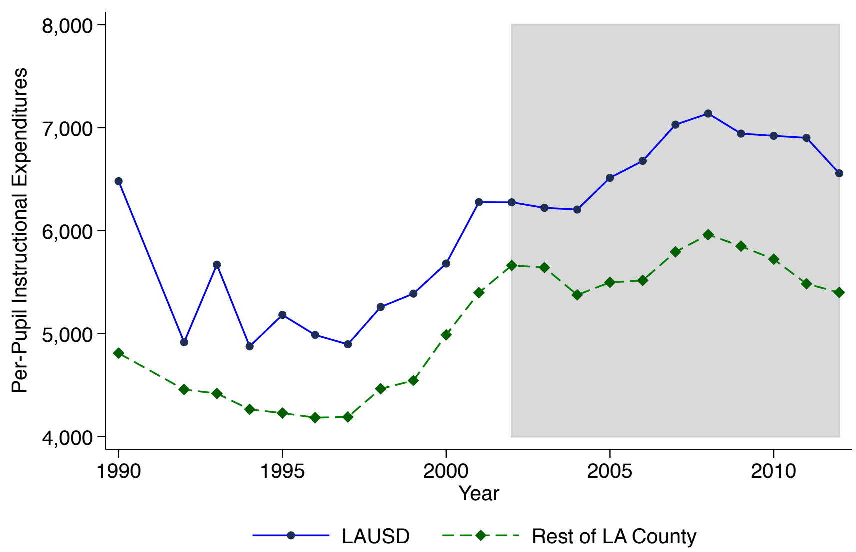


Notes: Solid blue line depicts student enrollment by year (left axis) and dashed green line depicts the number of new school facilities opened in a given year (right axis). Shaded area from 2002-2012 shows the treatment period covered in the main analysis. The number of new school openings only includes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

Figure 2: Spending per pupil, LAUSD vs LA County



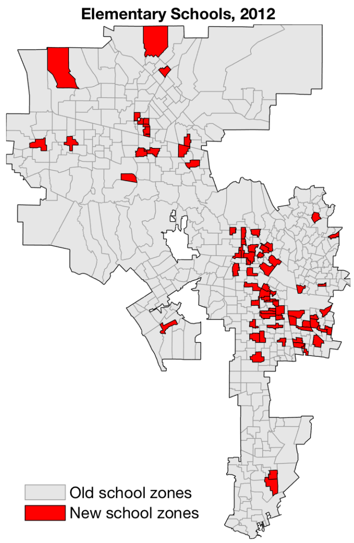
(a) Capital



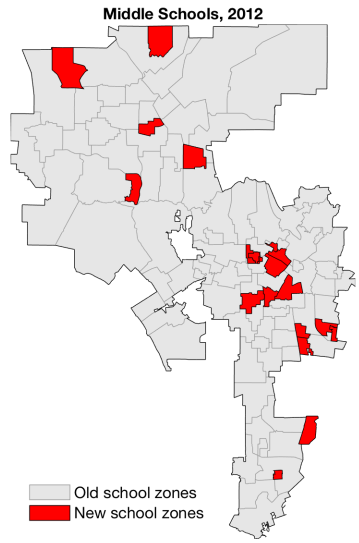
(b) Instructional

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

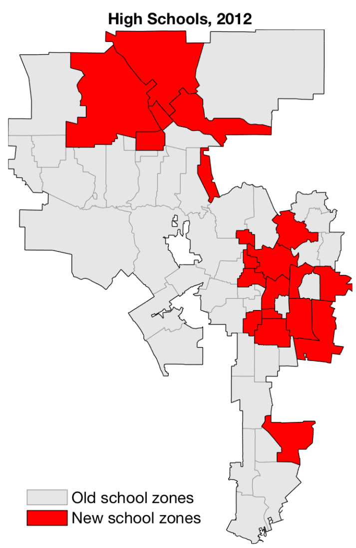
Figure 3: LAUSD school attendance zones, 2012



(a) Elementary



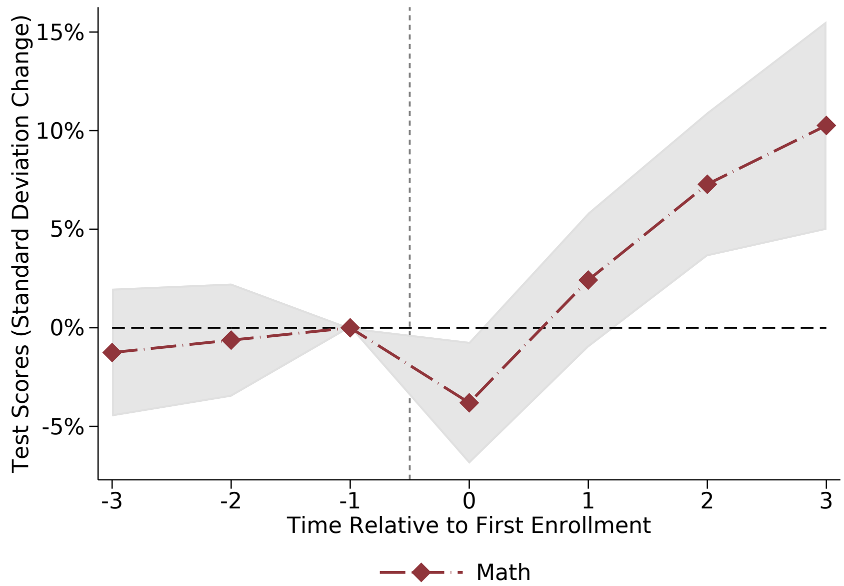
(b) Middle



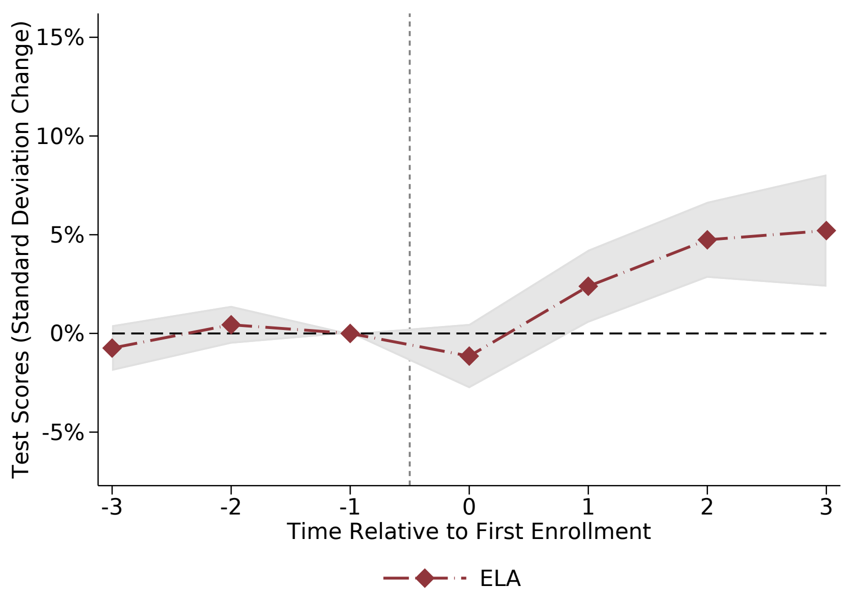
(c) High

Notes: Figure displays school attendance boundaries for elementary schools (panel A), middle schools (panel B), and high schools (panel C) in LAUSD in 2012. Shaded areas in red denote attendance zones that correspond to schools newly constructed during the sample period from 2002-2012.

Figure 4: Test score effects



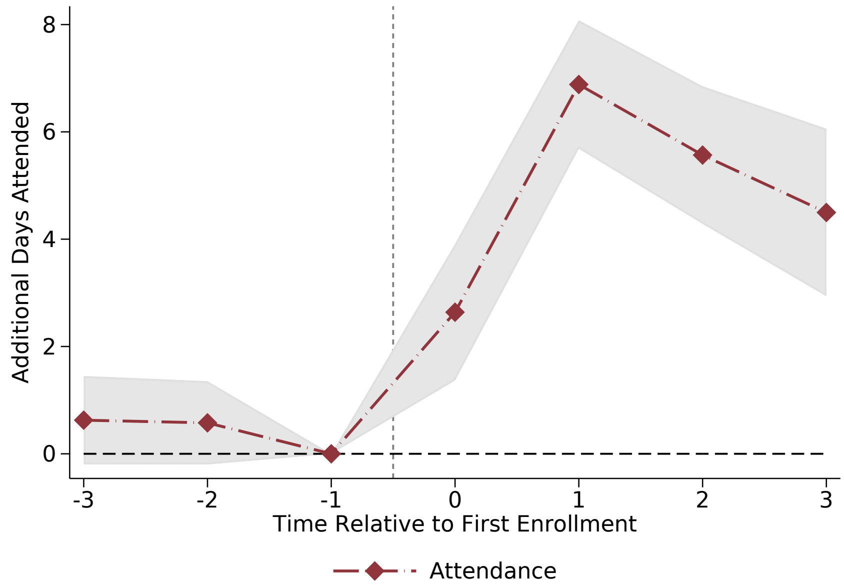
(a) Math



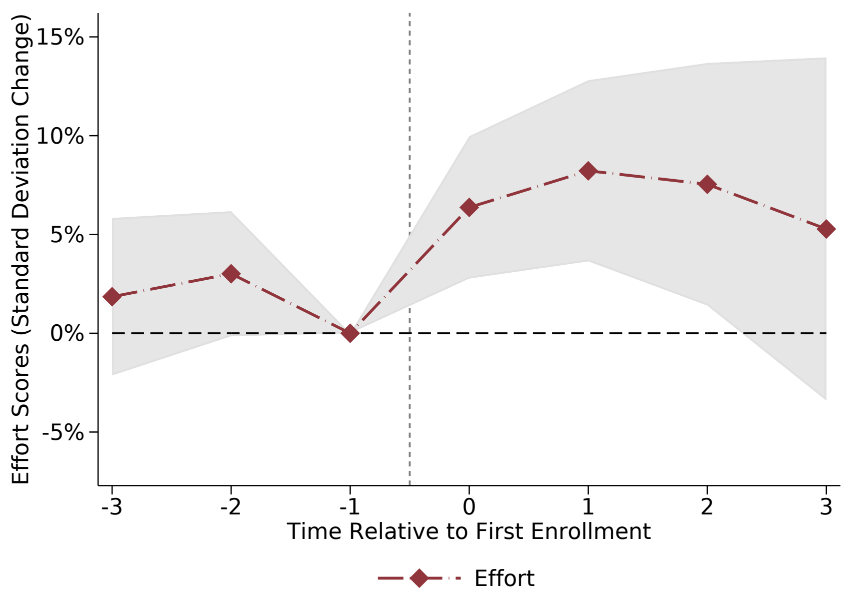
(b) ELA

Notes: Figures shows estimated coefficients from event study regressions following equation (3). 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. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Figure 5: Non-cognitive effects



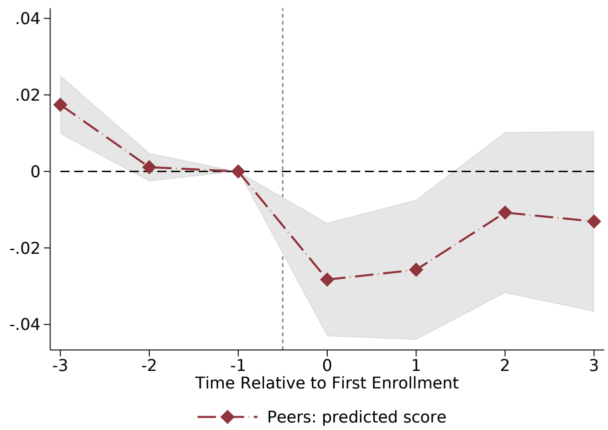
(a) Attended days



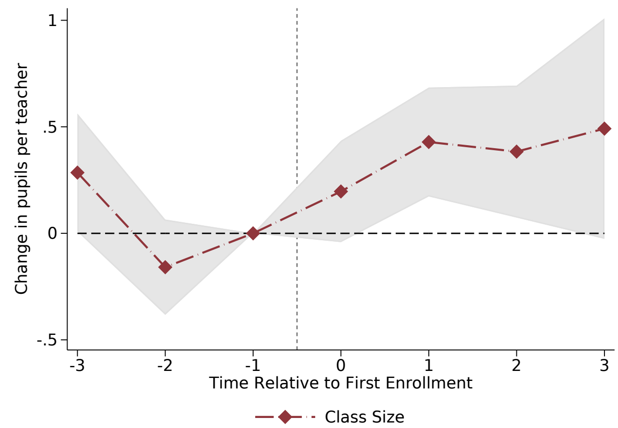
(b) Teacher-reported student effort

Notes: Figures show estimated coefficients from event study regressions following equation (3). 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. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

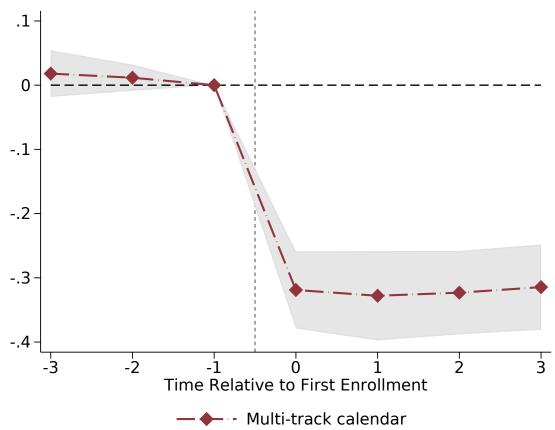
Figure 6: School effects



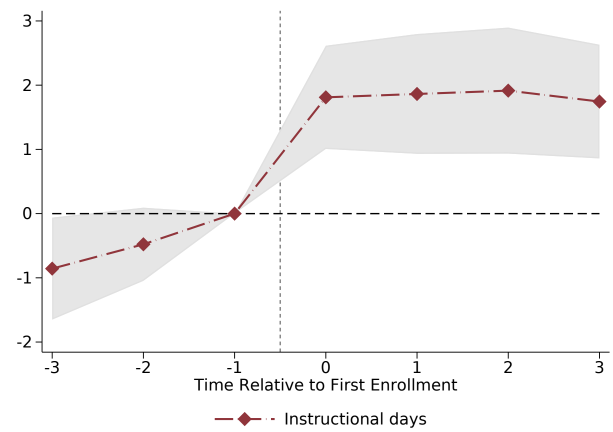
(a) Peers: predicted score



(b) Class size



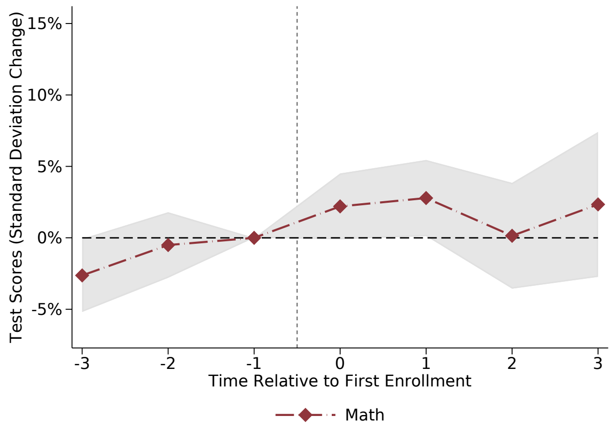
(c) Multi-track calendar



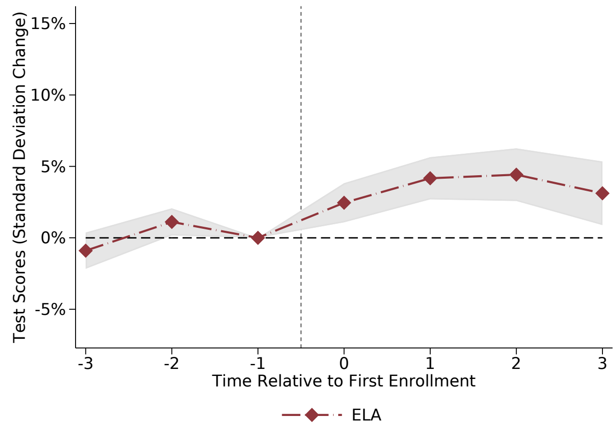
(d) Total instructional days

Notes: Figures show estimated coefficients from event study regressions following equation (3). Dependent variables are leave-out school mean predicted test scores (panel A), class size for students in grades K-5 (panel B), multi-track calendar status (panel C), and total instructional days for a given school-year (panel D). The shaded areas denote 95% confidence intervals for the estimated coefficients. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

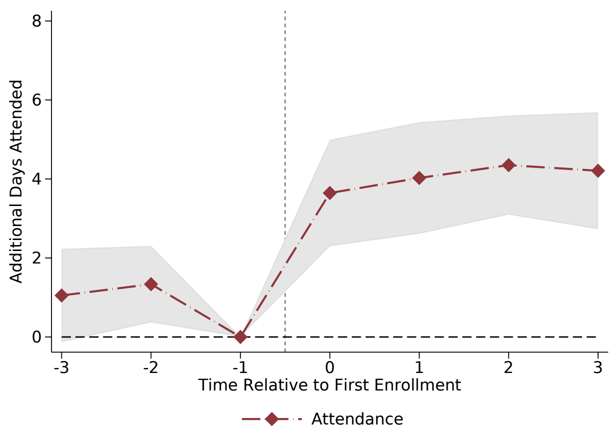
Figure 7: Student effects: Stayers



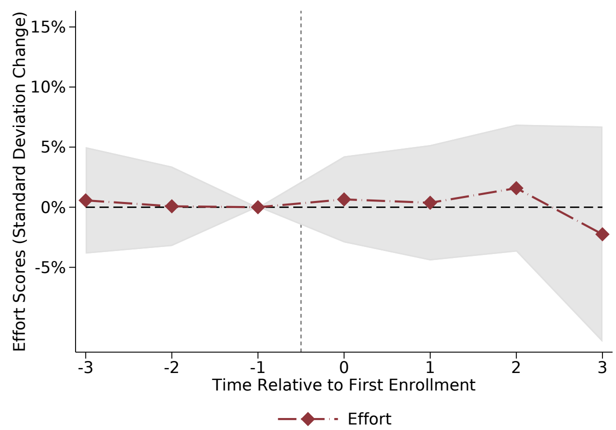
(a) Test scores: Math



(b) Test scores: ELA



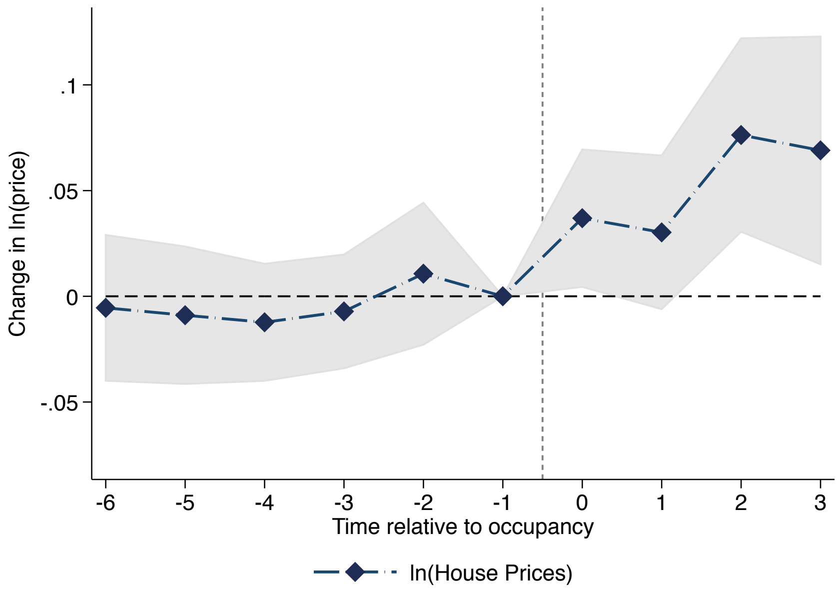
(c) Attended days



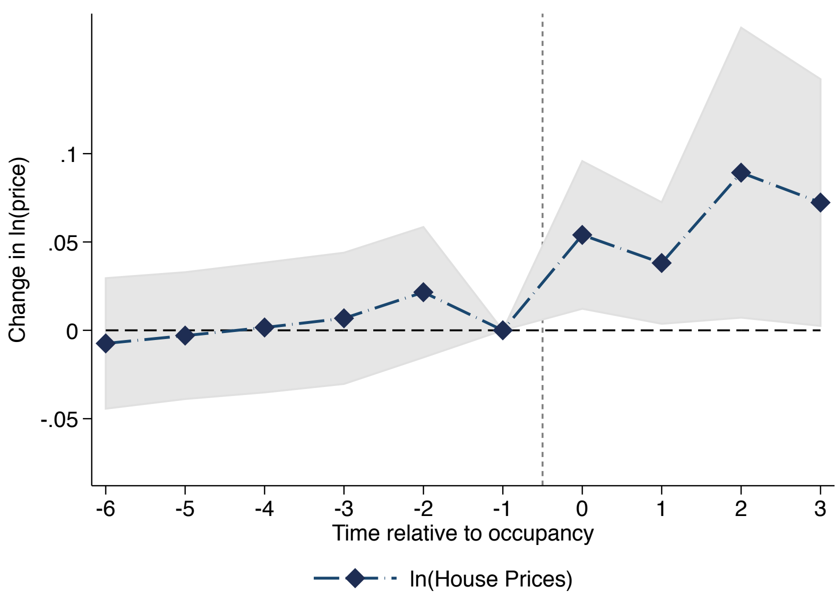
(d) Teacher-reported student effort

Notes: Figures show estimated coefficients from event study regressions following equation (3) for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Event time is centered relative to the year of the peer outflow. Dependent variables are standardized math test scores for students in grades 2-7 (panel A), standardized english-language arts test scores for students in grades 2-11 (panel B), annual days attended (panel C), and standardized teacher-reported effort scores for students in grades K-5 (panel D). The shaded areas denote 95% confidence intervals for the estimated coefficients. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Figure 8: House price effects



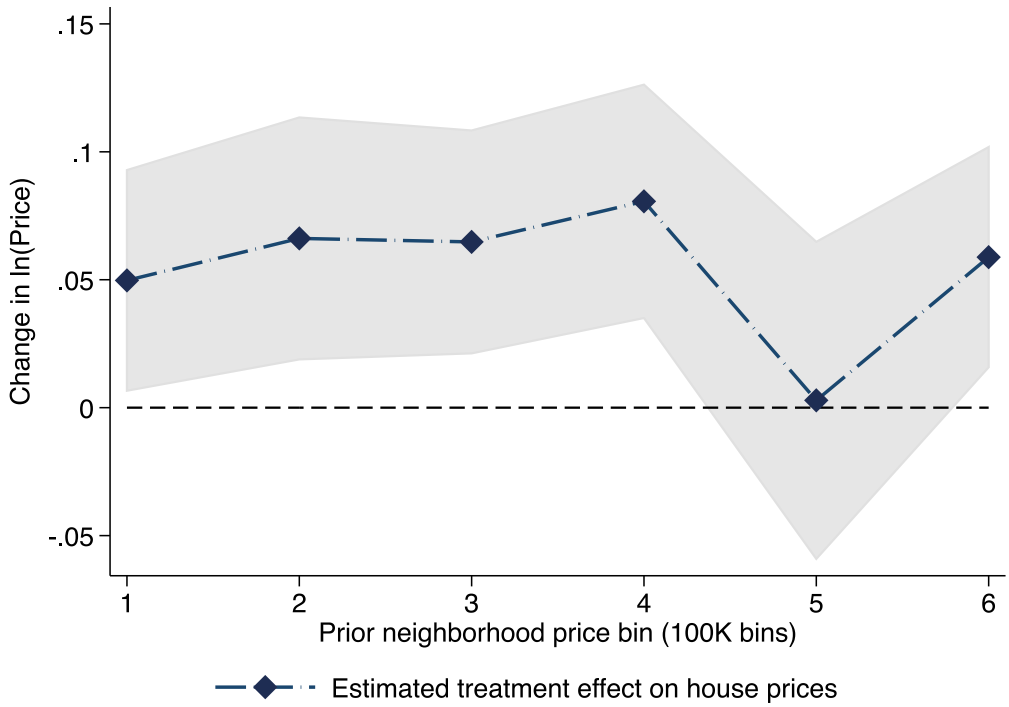
(a) House prices: Only treated



(b) House prices: All LAUSD

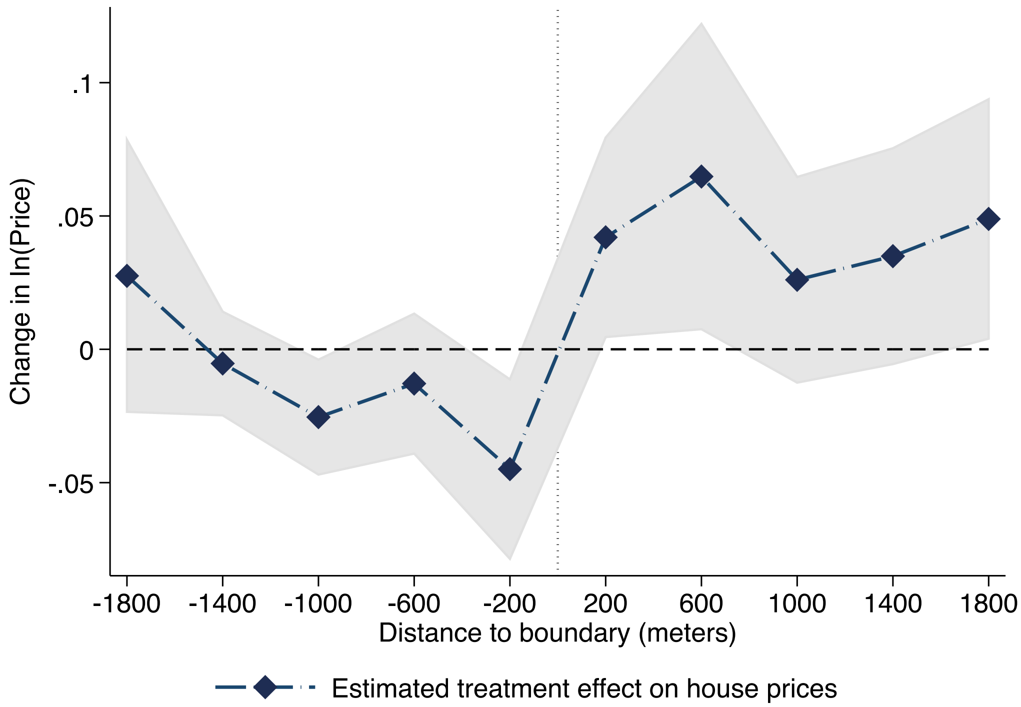
Notes: Figures show estimated coefficients from event-study regressions following equation (6). Dependent variable in both panels is the ln(sale price). In panel A, only properties that are ever in a new school attendance zone are included in the estimation, corresponding to baseline estimates presented in column 4 of Table 10. In panel B, all properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 2 of Table 10. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure 9: Heterogeneity: By neighborhood mean prior house prices



Notes: Figure shows estimated coefficients from a difference-in-differences regression based on equation (5), where the treatment indicator is interacted with indicators for $100,000 bins of prior neighborhood average prices. Bin 1 also includes average neighborhood house prices less than $100K, while bin 6 includes all neighborhoods with average house prices above $600K; all other bins only include a $100K range. Prior neighborhood average house prices are calculated using data from pre-construction property sales from 1995-2001. All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 2 of Table 10. All specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure 10: Spillovers: Effects by distance to school attendance boundary



Notes: Figure shows estimated coefficients from a difference-in-differences regression based on equation (5), where the treatment indicator is interacted with indicators for 400 meter bins of distance to the new school attendance zone in 2012. Properties with positive (negative) distance are inside (outside) the new school attendance zones. Properties outside the attendance zone and within 2 km of a new school attendance zone are assigned the construction date corresponding to the nearest new school attendance zone boundary. Each point reports the estimated coefficient for the treatment indicator interacted with the corresponding distance bin. Points are located at the midpoint of each distance bin (i.e. the estimate at 200m corresponds to the 0-400m distance bin). All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 2 of Table 10. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Tables

Table 1: Summary statistics, new school projects

	Mean	Median	Min	Max
Total cost (million USD)	81.9	56.5	11.1	578.7
New student seats	1,050	800	162	3,440
New classrooms	40.3	32	6	130
Building SQFT	100,585	70,115	12,507	391,840
Completion year	2008	2008	2002	2012
Site designation to completion (yrs)	5.18	5	2	9
Construction to completion (yrs)	2.11	2	1	5
New School Codes	1.25	1	1	5
Total New School Campuses	114			
Total New School Codes	143			

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

Table 2: Summary statistics, LAUSD student data

	All LAUSD	Never Treated	Always Treated	Switchers	Stayers
Free/reduced-price lunch	0.82	0.80	0.93	0.95	0.91
Hispanic	0.73	0.71	0.85	0.89	0.83
Black	0.11	0.11	0.05	0.05	0.07
White	0.09	0.10	0.03	0.02	0.05
Asian	0.04	0.04	0.04	0.02	0.03
Parent: any college	0.26	0.27	0.23	0.16	0.21
English spoken at home	0.32	0.34	0.27	0.17	0.21
Predicted test score	-0.26	-0.24	-0.29	-0.40	-0.34
Math score ($t = -1$)				-0.35	-0.19
ELA score ($t = -1$)				-0.52	-0.37
Days attended ($t = -1$)				156.73	155.37
N student-years	7,284,175	6,471,912	108,611	703,652	1,307,071

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

Table 3: Summary statistics, LA County assessor data

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

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

Table 4: Student effects, cognitive

	Math Score			ELA Score		
	(1)	(2)	(3)	(4)	(5)	(6)
New School * Trend	0.029***	0.034***	0.031***	0.019***	0.020***	0.017***
	(0.007)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
New School		-0.021	-0.028		-0.003	-0.014
		(0.017)	(0.017)		(0.008)	(0.009)
Trend			0.004*			0.004***
			(0.002)			(0.002)
Cumul. 4yr Effect	0.086***	0.080***	0.064***	0.058***	0.058***	0.037***
	(0.021)	(0.022)	(0.024)	(0.011)	(0.011)	(0.012)
Grade FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X
N student-years	2,935,156	2,935,156	2,935,156	4,716,377	4,716,377	4,716,377
N students	735,811	735,811	735,811	971,568	971,568	971,568
N treated students	87,132	87,132	87,132	99,685	99,685	99,685
N treated schools	78	78	78	126	126	126
R2	0.82	0.82	0.82	0.84	0.84	0.84

Notes: Table reports estimates of parametric event study models corresponding to equation (4). 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 (4). Row 4 reports the implied cumulative test score effect after four years, equal to $3\beta_2$ in columns 1 and 4, and $\beta_1 + 3\beta_2$ in columns 2-3 and 5-6. Dependent variable is the standardized math test score (grades 2-7) in columns 1-3. In columns 4-6 the dependent variable is the standardized ELA test score (grades 2-11). Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table 5: Student effects, non-cognitive

	Days Attended			Effort Score		
	(1)	(2)	(3)	(4)	(5)	(6)
New School	3.973***	3.398***	3.314***	0.061***	0.063***	0.056***
	(0.551)	(0.599)	(0.633)	(0.017)	(0.017)	(0.018)
New School * Trend		0.777***	0.745***		-0.002	-0.008
		(0.196)	(0.204)		(0.012)	(0.013)
Trend			0.036			0.006
			(0.085)			(0.004)
Grade FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X
N student-years	5,350,867	5,350,867	5,350,867	1,924,572	1,924,572	1,924,572
N students	1,121,933	1,121,933	1,121,933	552,855	552,855	552,855
N treated students	116,947	116,947	116,947	71,636	71,636	71,636
N treated schools	143	143	143	75	75	75
R2	0.51	0.51	0.51	0.63	0.63	0.63

Notes: Table reports estimates of parametric event study models corresponding to equation (4). 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 (4). Dependent variable is the annual days attended in columns 1-3. In columns 4-6 the dependent variable is the standardized average teacher-reported effort score (grades K-5). Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table 6: Student effects, robustness

	Baseline	No Stayers	Only Treated	Only Switchers	Balanced
ELA Score					
New School * Trend	0.019***	0.022***	0.018***	0.016***	0.027*
	(0.004)	(0.004)	(0.005)	(0.005)	(0.014)
Math Score					
New School * Trend	0.029***	0.029***	0.034***	0.035***	0.059*
	(0.007)	(0.007)	(0.011)	(0.012)	(0.033)
Days Attended					
New School	3.97***	4.33***	4.02***	4.43***	8.54***
	(0.55)	(0.57)	(0.78)	(0.79)	(1.65)
Effort Score					
New School	0.061***	0.061***	0.077***	0.089***	0.045
	(0.017)	(0.017)	(0.024)	(0.027)	(0.060)

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

Table 7: Student effects heterogeneity, mechanisms

	Math	ELA	Attendance	Effort
Pooled (switchers only)	0.035^{***}	0.016^{***}	4.432^{***}	0.089^{***}
	(0.012)	(0.005)	(0.789)	(0.027)
By multi-track:				
Multi track	0.039^{***}	0.017^{***}	8.214^{***}	0.097^{***}
	(0.014)	(0.006)	(0.788)	(0.029)
Single track	0.019	0.011^{*}	2.741^{***}	0.042
	(0.013)	(0.006)	(0.728)	(0.031)
p-value	0.15	0.37	0.00	0.11
By prior SQFT pp:				
Low prior SQFT pp	0.031^{**}	0.017^{***}	6.254^{***}	0.071^{***}
	(0.013)	(0.006)	(0.744)	(0.027)
High prior SQFT pp	0.037^{**}	0.007	4.668^{***}	0.098^{***}
	(0.017)	(0.006)	(0.797)	(0.033)
p-value	0.66	0.15	0.03	0.43
By share permanent classrooms:				
Low share permanent	0.038^{***}	0.019^{***}	6.133^{***}	0.092^{***}
	(0.013)	(0.005)	(0.666)	(0.024)
High share permanent	0.020	0.003	4.955^{***}	-0.013
	(0.018)	(0.007)	(0.903)	(0.044)
p-value	0.29	0.02	0.14	0.01
By prior building age:				
Below median age	0.026^{**}	0.013^{**}	5.625^{***}	0.075^{**}
	(0.013)	(0.005)	(0.647)	(0.032)
Above median age	0.045^{***}	0.017^{**}	6.095^{***}	0.092^{***}
	(0.017)	(0.007)	(0.863)	(0.035)
p-value	0.23	0.53	0.52	0.68
By prior building FCI:				
Low FCI	0.030^{*}	0.016^{***}	6.806^{***}	0.125^{**}
	(0.018)	(0.006)	(0.922)	(0.052)
High FCI	0.035^{**}	0.013^{**}	4.910^{***}	0.068^{**}
	(0.014)	(0.006)	(0.632)	(0.027)
p-value	0.78	0.79	0.02	0.29

Notes: Table reports estimates of one parameter event study models. Dependent variables are ELA scores (column 1), math scores (column 2), annual days attended (column 3), and standardized teacher-reported effort scores (column 4). Panel A repeats one-parameter estimates from column 4 of Table 6. Panel B reports estimates of coefficients interacted with prior school multi-track status. Remaining panels show coefficients on the interactions for being below or above the median in terms of prior school SQFT per pupil (panel C), prior school share permanent classrooms (panel D), prior school age (panel E), and prior school FCI (panel F). Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table 8: Teacher changes at new schools

(a) Demographics

	(1) Age	(2) Experience	(3) MA+	(4) Pr(New)
New School	-3.289***	-2.659***	0.042***	0.054***
	(0.336)	(0.227)	(0.012)	(0.005)
Grade FEs	X	X	X	X
Year FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	3,935,106	3,927,063	3,931,757	5,902,165
N students	926,501	925,300	926,203	1,140,815
N treated students	108,323	108,124	108,252	121,887
N treated schools	137	137	137	143
R2	0.32	0.36	0.28	0.29

(b) Value-added

	VA: Average (pre-switch)		VA: Novice/Experienced gap	
	(1) Math	(2) ELA	(3) Math	(4) ELA
New School	-0.005	-0.010**	-0.003	0.015
	(0.007)	(0.004)	(0.012)	(0.012)
Grade FEs	X	X	X	X
Year FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	2,443,716	4,265,444	1,267,199	2,347,897
N students	689,206	955,346	432,813	672,731
N treated students	82,315	94,956	60,155	75,073
N treated schools	69	119	54	83
R2	0.61	0.56	0.38	0.33

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

Table 9: Student effects, "staying" students

	Math Score		ELA Score		Days Attended		Effort Score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post * Trend: Stayers	-0.001	-0.013**	0.009***	-0.001		0.212		-0.013
	(0.006)	(0.006)	(0.003)	(0.003)		(0.183)		(0.012)
Post: Stayers		0.014		0.014**	3.653***	3.049***	0.007	-0.018
		(0.012)		(0.007)	(0.494)	(0.652)	(0.021)	(0.021)
Trend: Stayers		0.010***		0.006***		0.125		0.015***
		(0.003)		(0.002)		(0.107)		(0.004)
Grade FEs	X	X	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X
N student-years	2,562,332	2,562,332	4,161,767	4,161,767	4,729,758	4,729,758	1,650,087	1,650,087
N students	654,687	654,687	883,676	883,676	1,019,337	1,019,337	480,544	480,544
N treated students	144,220	144,220	164,644	164,644	171,870	171,870	109,717	109,717
N treated cohort	22,753	22,753	28,795	28,795	34,530	34,530	19,221	19,221
R2	0.82	0.82	0.84	0.84	0.52	0.52	0.63	0.63

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

Table 10: House price effects

(a) New school zones

	Neighborhood Fixed Effects					Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New School	-0.011	0.060***	0.059***	0.044***	0.055***	0.045***	0.059***
	(0.014)	(0.018)	(0.016)	(0.011)	(0.015)	(0.013)	(0.016)
Yr FEs	X				X		X
Yr-HSZ FEs		X	X	X		X	
Month FEs	X	X	X	X	X	X	X
Sch Zone FEs	X	X	X	X	X		
Prop Controls	X	X	X	X	X		
Prop FEs						X	X
New Sch Zones	X	X	X	X	X	X	X
w/in 1km			X				
All LAUSD	X	X					
Number of sales	505,781	505,781	255,481	161,775	161,782	87,523	87,551
R2	.81	.82	.79	.78	.75	.91	.9

(b) "Stayers" school zones

	Neighborhood Fixed Effects				Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
Post: School	0.042***	-0.008	0.023	-0.009	-0.010	-0.014
	(0.012)	(0.017)	(0.018)	(0.019)	(0.031)	(0.025)
Yr FEs	X			X		X
Yr-HSZ FEs		X	X		X	
Month FEs	X	X	X	X	X	X
Sch Zone FEs	X	X	X	X		
Prop Controls	X	X	X	X		
Prop FEs					X	X
All LAUSD	X	X				
Number of sales	343,997	343,997	180,504	180,504	107,458	107,458
R2	.82	.83	.82	.81	.93	.93

Notes: Tables report estimates from difference-in-differences regressions following equations (5) and (7). Dependent variable is the ln(sale price). In panel A the coefficient of interest is an indicator for being in a new school zone, whereas in panel B it is an indicator for being in an existing school zone that was affected by student outflows to a new school. In panel A, columns 1-5 include neighborhood fixed effects and property specific controls; columns 6-7 include property fixed effects. Columns 1, 5, and 7 report estimates using year effects; remaining columns include year-by-high school zone fixed effects. Columns 1-2 include all properties in LAUSD. Column 3 restricts the sample to only properties within a new school zone or within a 1km of a new school zone. Columns 4-7 include only properties within a new school zone by 2012: "never-treated" properties are excluded. In panel B, properties in new school zones are excluded from estimation; columns 1-4 report estimates corresponding to equation (5), with neighborhood fixed effects and property specific controls. Columns 5-6 show estimates with property fixed effects. Columns 1, 4, and 6 of panel B include year effects, while remaining columns include year-by-high school zone effects. Columns 1-2 include all properties in LAUSD, while columns 3-6 restrict the sample to only those properties in school zones affected by student outflows. All specifications include month effects. Standard errors are clustered by neighborhood.

Table 11: House price effects, by distance to school assignment zone boundary

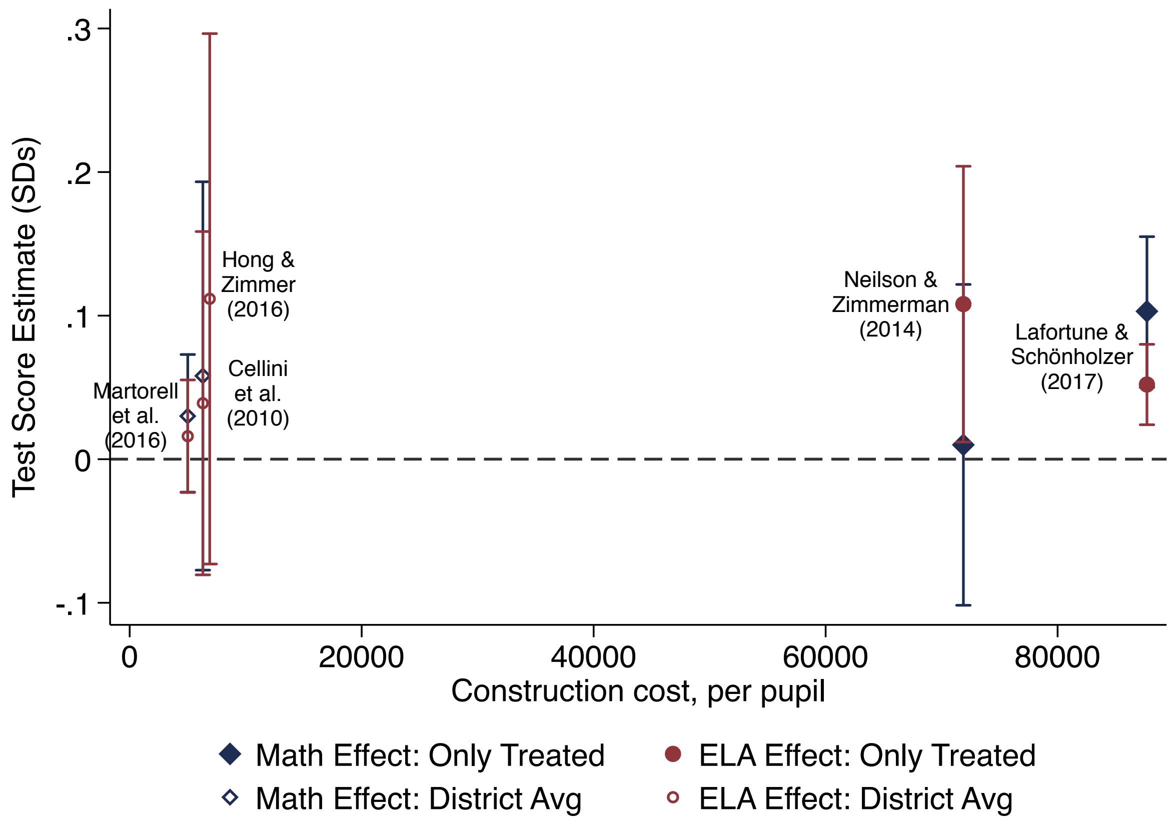
	(1)	(2)	(3)	(4)
New School: inside zone	0.060***	0.061***	0.054***	0.051***
	(0.018)	(0.018)	(0.017)	(0.018)
Distance to boundary		0.019		0.017
		(0.014)		(0.013)
Inside zone * dist to boundary		-0.005		-0.004
		(0.010)		(0.010)
New School: outside w/in 2km			-0.013	-0.046***
			(0.010)	(0.017)
Outside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

Notes: Table reports estimated coefficients from difference-in-differences regressions based off of equation (5). Dependent variable is the ln(sale price). Column 1 repeats baseline estimates reported in column 2 of Table 10. Column 2 adds coefficients for property-level distance to the school assignment boundary and the interaction between distance to the boundary and the new school zone treatment variable. Column 3 includes an additional treatment variable for properties outside but within 2km of the new school attendance zone, where the completion date assigned to these properties corresponds to that of the nearest new school attendance zone. Column 4 combines columns 2 and 3, and adds an interaction with distance to the boundary for properties outside but within 2km of the new school zone. All properties in LAUSD in the data sample are included in estimation. All specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Appendix A: Appendix Figures and Tables

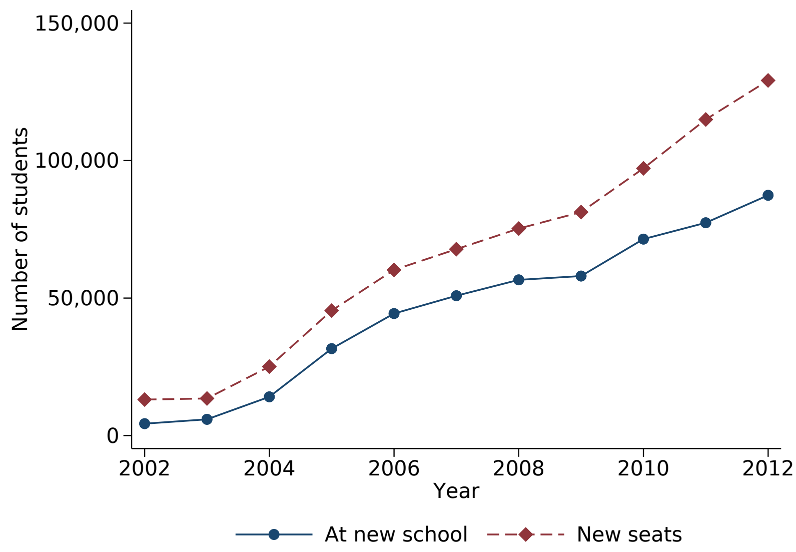
Appendix Figures:

Figure A1: Student effects comparison from capital expenditure literature



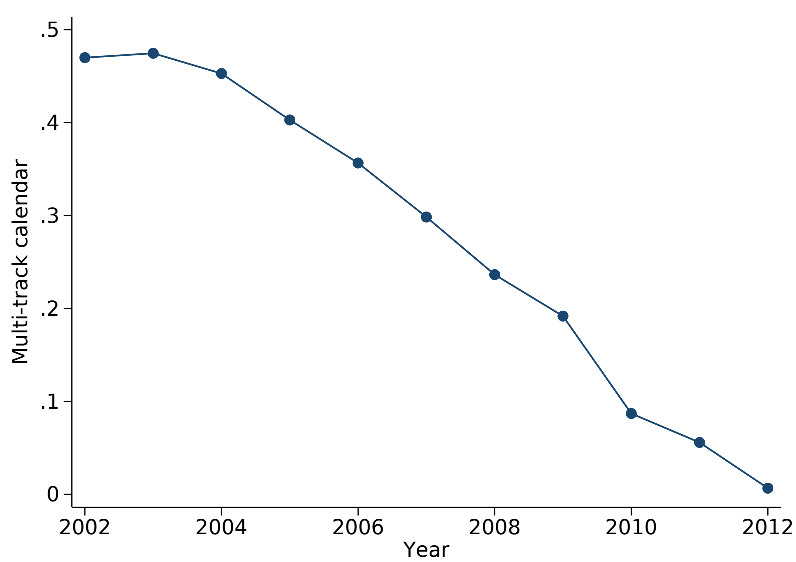
Notes: Figure plots estimated coefficients from related papers in economics evaluating the effects of school capital expenditures (y axis) against per-pupil expenditures in each study (x axis). Blue diamond shaped markers denote math test score estimates whereas red circular markers denote English / Language Arts test score estimates (both in standard deviation units). Solid markers denote estimates on directly treated students from Neilson and Zimmerman (2015) and Lafortune and Schönholzer (2017), 4 years after school construction or student occupancy, respectively. For these studies, construction cost is calculated per treated pupil. Hollow markers denote estimates from studies examining district average test scores after passage of a capital construction bond, where construction cost per pupil is the average over all students in the district. For these studies, estimates 6 years after bond passage are reported.

Figure A2: Students at newly constructed schools



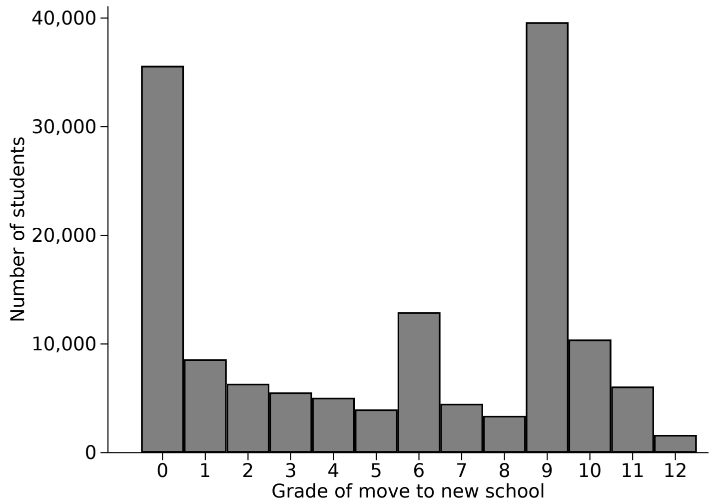
Notes: Figure shows time series of total new seats (from new construction project database) and the number of students attending newly constructed school facilities (from the student microdata).

Figure A3: School age and multi-track calendars in LAUSD



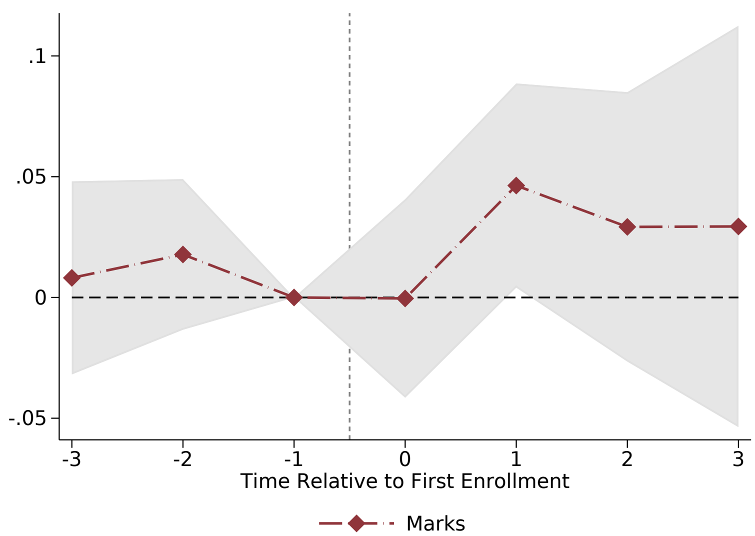
Notes: Figure reports proportion of students attending a school on a multi-track calendar, by year.

Figure A4: Grade of switch to new school facilities



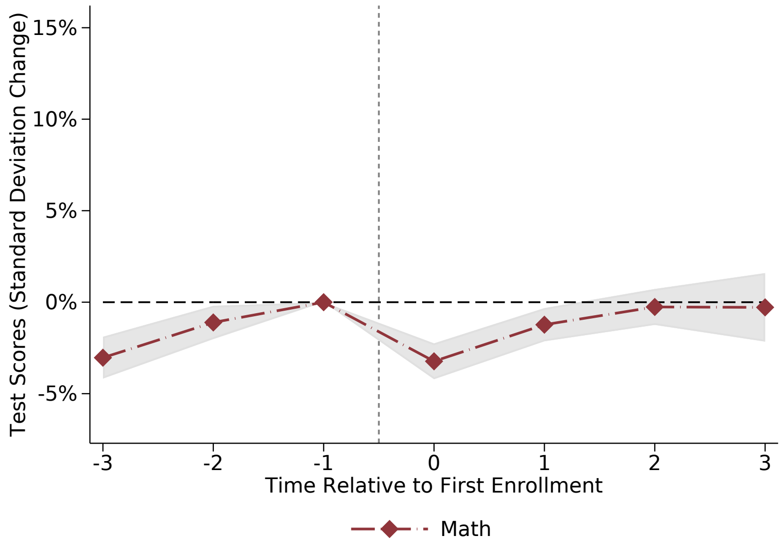
Notes: Figure shows grade of switch for students switching to new facilities. Y-axis reports number of students.

Figure A5: Event study estimates, teacher-reported marks

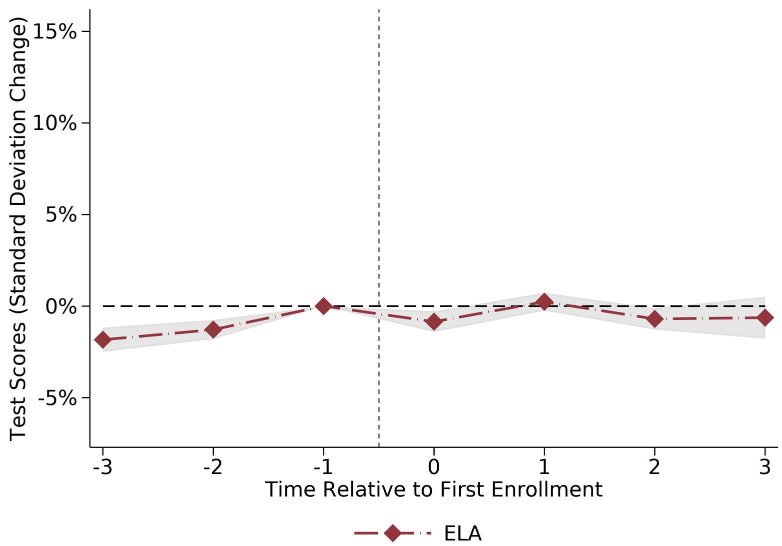


Notes: Figure shows estimated coefficients from event study regressions following equation (3). Dependent variable is the standardized teacher-reported marks (averaged over all subjects) for students in grades K-5. The shaded areas denote 95% confidence intervals for the estimated coefficients. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Figure A6: Student switching, non-new facility related



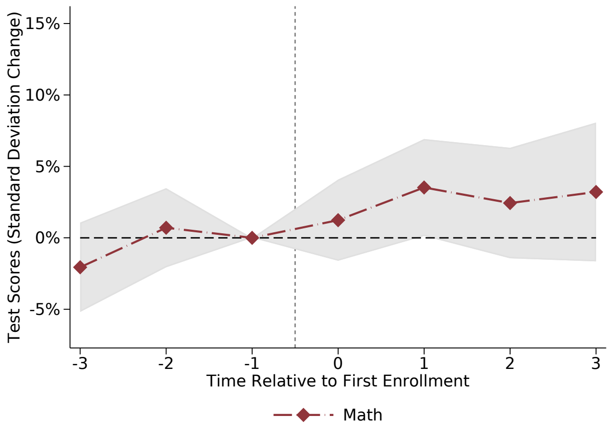
(a) Math



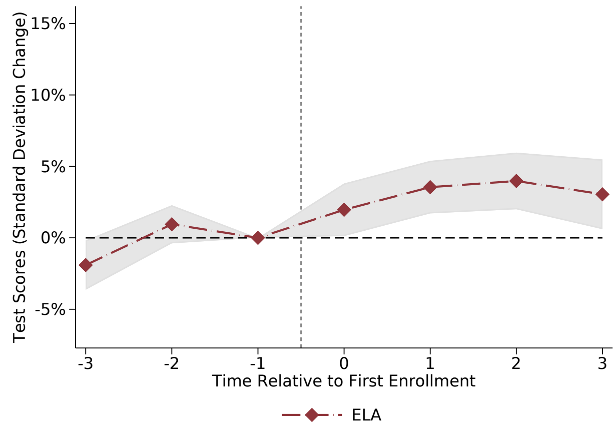
(b) ELA

Notes: Figures show estimated coefficients from event study regressions following equation (3), for students who switch schools for reasons unrelated to new school facilities. Dependent variables are standardized math test scores for students in grades 2-7 (panel A) and standardized english-language arts test scores for students in grades 2-11 (panel B). Test scores are standardized relative to the statewide mean and standard deviation for each year-grade-subject exam. The shaded areas denote 95% confidence intervals for the estimated coefficients. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

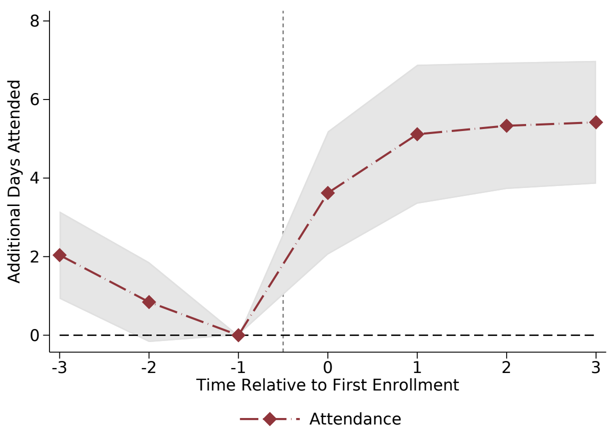
Figure A7: Student effects: Stayers, 20% cohort exit threshold



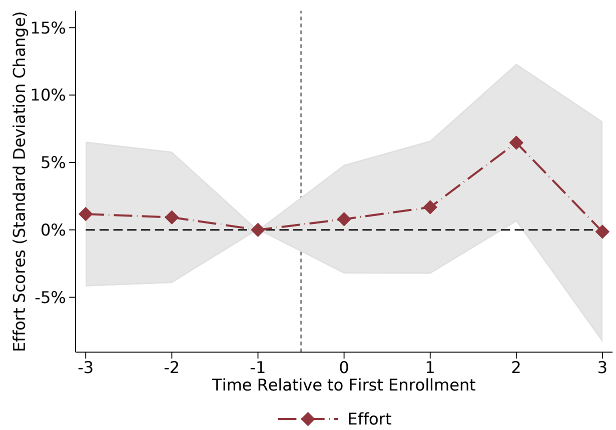
(a) Test scores: Math



(b) Test scores: ELA



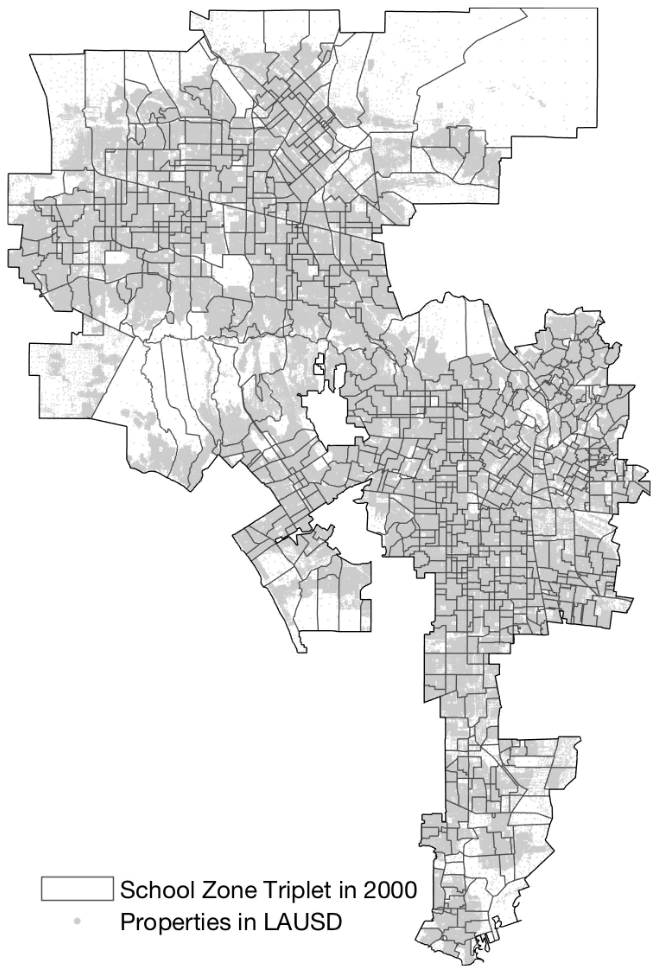
(c) Attended days



(d) Teacher-reported student effort

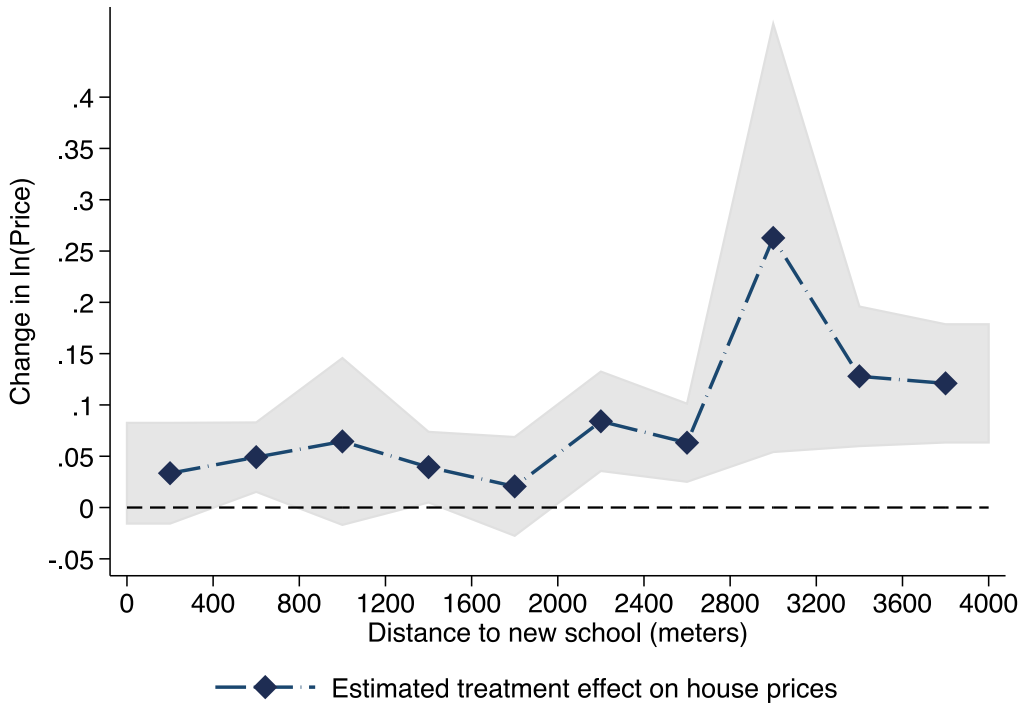
Notes: Figures show estimated coefficients from event study regressions following equation (3), for students that had 20% or more of their school-grade cohort exit to a newly constructed school. Figures are analogous to Figure **??**, with the threshold for "stayers" raised from 10% to 20% of a student's cohort. Event time is centered relative to the year of the peer outflow. Dependent variables are standardized math test scores for students in grades 2-7 (panel A), standardized english-language arts test scores for students in grades 2-11 (panel B), annual days attended (panel C), and standardized teacher-reported effort scores for students in grades K-5 (panel D). The shaded areas denote 95% confidence intervals for the estimated coefficients. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Figure A8: Neighborhood boundaries in LAUSD, based on 2000 school zones



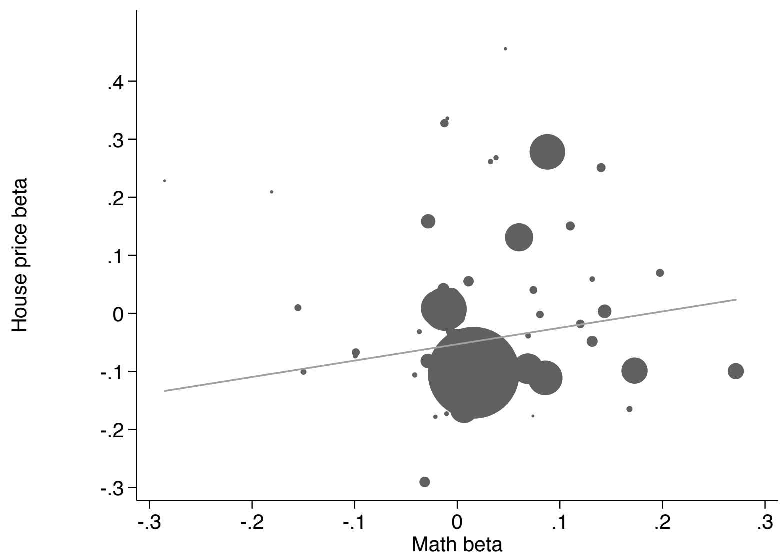
Notes: Figure shows school assignment zone triplets in LAUSD using 2000 assignment boundaries, which are used to define neighborhoods in the estimation of real estate effects. Solid lines denote neighborhood boundaries. Each gray dot represents one property from the LA County Assessor data.

Figure A9: Spillovers: Effects by distance to new school

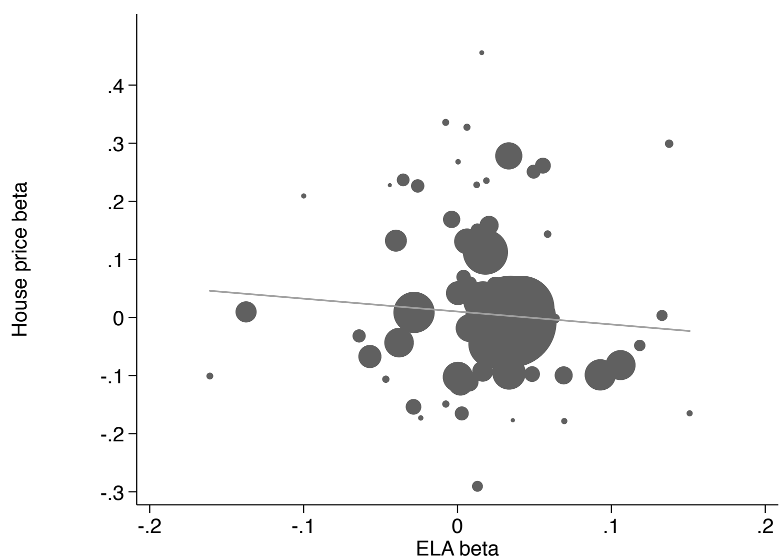


Notes: Figure shows estimated coefficients from a difference-in-differences regression based on equation (5), where the treatment indicator is interacted with indicators for 400 meter bins of distance to the new school in 2012. Each point reports the estimated coefficient for the treatment indicator interacted with the corresponding distance bin. Points are located at the midpoint of each distance bin (i.e. the estimate at 200m corresponds to the 0-400m distance bin). All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 2 of Table 10. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure A10: Correlation between house price and test score effects



(a) Math



(b) ELA

Notes: Figures show scatterplots of estimated school-level test score gains in math (panel A) and ELA (panel B) against estimated house price effects in the corresponding school attendance zone. The solid line in each figure displays the bivariate regression line. Points and regression lines are weighted by the product of the inverse sampling variances of the estimated test score gain and the estimated house price change for a given school. The size of each point is proportional to the weight. In panel A the point estimate on the regression line is 0.28 (SE 0.29) and in panel B the point estimate is -0.22 (SE 0.30).

Appendix Tables

Table A1: School-level changes

(a) Switching students

	Calendar		School		Peers	
	(1) Multi-track	(2) Max days	(3) Age	(4) Stu/tch	(5) Peers: Bl/Hisp	(6) Peers: predicted
New School	-0.267***	1.762***	-71.086***	0.312***	0.027***	-0.024***
	(0.029)	(0.262)	(1.132)	(0.084)	(0.004)	(0.006)
Grade FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X
N student-years	6,601,535	5,898,902	6,519,509	3,155,009	6,594,053	4,601,340
N students	1,224,566	1,186,057	1,217,043	779,669	1,222,196	939,620
N treated students	122,172	120,164	122,112	96,526	122,043	97,856
N treated schools	143	140	143	79	143	126
R2	0.68	0.51	0.79	0.75	0.88	0.85

(b) Staying students

	Calendar		School		Peers	
	(1) Multiple	(2) Max days	(3) Age	(4) Stu/tch	(5) Peers: Bl/Hisp	(6) Peers: predicted
Post: Stayers	-0.249***	2.305***	1.572	-0.278**	-0.016***	0.022***
	(0.027)	(0.304)	(1.074)	(0.123)	(0.003)	(0.004)
Grade FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X
N student-years	5,837,507	5,214,065	5,759,302	2,737,469	5,830,319	4,053,776
N students	1,119,399	1,081,590	1,111,323	686,610	1,117,171	853,479
N treated students	178,022	176,086	177,607	132,170	177,847	161,177
N treated schools	801	791	752	500	802	787
R2	0.70	0.53	0.68	0.75	0.88	0.85

Notes: Table reports estimates of difference-in-differences models corresponding one-parameter versions of equation (4), 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 in a given school-year (column 1), total instructional days in a given school-year (column 2), school age (column 3), class size (i.e. pupils per teacher) for students in grades K-5 (column 4), school leave-out mean proportion black and/or hispanic (column 5), and school leave-out mean predicted test scores (column 6). Panel A reports estimates for students attending new school facilities. Panel B reports analogous estimates for staying students: here β_1 is an indicator for having experienced a 10% or greater school-grade cohort exit to a newly constructed school. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table A2: Teacher changes at existing schools

(a) Demographics

	(1) Age	(2) Experience	(3) MA+	(4) Pr(New)
Post: Stayers	1.117***	0.768***	0.007	-0.000
	(0.248)	(0.182)	(0.010)	(0.004)
Grade FEs	X	X	X	X
Year FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	3,935,106	3,927,063	3,931,757	5,902,165
N students	926,501	925,300	926,203	1,140,815
N treated students	156,306	156,183	156,282	176,213
N treated schools	797	797	797	802
R2	0.32	0.35	0.28	0.29

(b) Value-added

	VAM: Average (pre-switch)		VAM: Experienced/Novice gap	
	(1) Math	(2) ELA	(3) Math	(4) ELA
Post: Stayers	-0.009	0.002	-0.007	0.016
	(0.007)	(0.003)	(0.022)	(0.014)
Grade FEs	X	X	X	X
Year FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	2,175,273	3,722,137	1,267,199	2,347,897
N students	640,572	887,238	432,813	672,731
N treated students	132,846	156,769	104,810	134,051
N treated schools	609	785	585	731
R2	0.33	0.35	0.38	0.33

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

Table A3: Principal experience

	(1) Exper (Dist)	(2) Exper (Sch)	(3) New (Dist)	(4) New (Sch)
New School	-0.830***	-1.064***	0.135***	0.191***
	(0.146)	(0.103)	(0.021)	(0.021)
Grade FEs	X	X	X	X
Year FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	5,655,314	5,655,314	5,655,314	5,655,314
N students	1,116,451	1,116,451	1,116,451	1,116,451
N treated students	128,248	128,248	128,248	128,248
N treated schools	135	135	135	135
R2	0.61	0.51	0.50	0.42

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

Table A4: Student effects, heterogeneity

	Math	ELA	Attendance	Effort
Pooled	0.029***	0.019***	3.973***	0.061***
	(0.007)	(0.004)	(0.551)	(0.017)
By Sex:				
Female	0.036***	0.025***	3.823***	0.073***
	(0.007)	(0.004)	(0.547)	(0.018)
Male	0.022***	0.014***	4.131***	0.053***
	(0.007)	(0.004)	(0.567)	(0.019)
p-value	0.00	0.00	0.04	0.19
By parental education:				
No college	0.029***	0.021***	4.284***	0.050***
	(0.007)	(0.004)	(0.598)	(0.017)
Any college	0.026***	0.014***	3.278***	0.107***
	(0.010)	(0.004)	(0.494)	(0.023)
p-value	0.69	0.03	0.00	0.00
By school level:				
Elementary	0.028***	0.017***	1.608***	0.061***
	(0.007)	(0.004)	(0.333)	(0.017)
Middle	0.038	-0.002	3.368***	
	(0.026)	(0.007)	(0.526)	
High		0.030***	5.464***	
		(0.007)	(1.055)	
p-value	0.72	0.00	0.00	
By grade of switch:				
Reg (KG,G6,G9)	0.019**	0.018***	5.305***	0.040
	(0.009)	(0.004)	(0.643)	(0.025)
Irregular	0.038***	0.022***	1.976***	0.071***
	(0.009)	(0.005)	(0.532)	(0.020)
p-value	0.08	0.49	0.00	0.31

Notes: Table reports estimates of parametric event study models corresponding one-parameter versions of equation (4). Columns 1 and 2 include only the coefficient for the change in growth (β_2); β_1 and β_3 are constrained to be zero. Columns 3 and 4 include only the coefficient for the immediate new school effect (β_1); β_2 and β_3 are constrained to be zero. Dependent variables are standardized english-language arts test scores (column 1), standardized math test scores (column 2), annual days attended (column 3), and standardized average teacher-reported effort scores (column 4). Panel A repeats baseline one-parameter estimates from columns 1 and 4 of Tables 4 and 5. The remaining panels report estimates of coefficients interacted with student gender (panel A), parental education (panel B), school level (panel C), and whether a student switched in a typical (KG, G6, G9) or atypical grade (panel D). P-values for the test of equality of the coefficient(s) are reported in the third row of each panel. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table A5: House price effects, by school level

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

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

Table A6: House price effects, using post-2012 data or post-construction neighborhood definitions

(a) Including post-2012 data

	Neighborhood Fixed Effects					Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New School	-0.021*	0.053***	0.049***	0.034***	0.049***	0.043***	0.046***
	(0.013)	(0.015)	(0.013)	(0.011)	(0.013)	(0.012)	(0.014)
Yr FEs	X				X		X
Yr-HSZ FEs		X	X	X		X	
Month FEs	X	X	X	X	X	X	X
Sch Zone FEs	X	X	X	X	X		
Prop Controls	X	X	X	X	X		
Prop FEs						X	X
New Sch Zones	X	X	X	X	X	X	X
w/in 1km			X				
All LAUSD	X	X					
Number of sales	593,414	593,414	298,507	188,222	188,229	114,519	114,542
R2	.81	.82	.79	.77	.74	.91	.9

(b) Neighborhoods based on 2012 boundaries

	Neighborhood Fixed Effects					Repeat Sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New School	-0.010	0.068***	0.067***	0.046***	0.055***	0.045***	0.059***
	(0.014)	(0.019)	(0.017)	(0.013)	(0.016)	(0.014)	(0.017)
Yr FEs	X				X		X
Yr-HSZ FEs		X	X	X		X	
Month FEs	X	X	X	X	X	X	X
Sch Zone FEs	X	X	X	X	X		
Prop Controls	X	X	X	X	X		
Prop FEs						X	X
New Sch Zones	X	X	X	X	X	X	X
w/in 1km			X				
All LAUSD	X	X					
Number of sales	505,795	505,779	255,481	161,773	161,779	87,523	87,551
R2	.81	.82	.8	.78	.75	.91	.9

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

Table A7: House price effects, robustness to sample restrictions

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

Notes: Table reports estimated coefficients from difference-in-differences regressions corresponding to estimates of equation (5). Dependent variable is the ln(sale price). Column 1 repeats baseline estimates presented in Table 10 panel A column 2. Column 2 makes no restriction on sale price, including the top and bottom 1% of sales based on price. Column 3 relaxes the restriction on renovated and/or torn-down properties, including these properties with an additional indicator variable for having been renovated and/or torn-down in the controls. Column 4 includes large properties, with greater than one acre of space. Column 5 includes non-residential properties. All specifications include neighborhood fixed effects, property specific controls, and month fixed effects. Standard errors are clustered by neighborhood.

Appendix B: Computing teacher value-added

To estimate teacher value-added scores, we use a subsample of students for which the following criteria are met: (1) Student-year observations have non-missing test scores and are currently in grades 3-7 in math, and 3-11 in ELA; value-added scores are not computed for grade 2 teachers so as to have at least one prior score for a student; (2) Student-year observations have non-missing teacher assignment;[56] (3) Student-year observations are in classrooms with at least 7 students. Consider the following data-generating process for test scores, closely following Kane and Staiger (2008) and Chetty et al. (2014):

$$y_{i,t} = \alpha_{t,g(i,t)} + X'_{it}\beta + \nu_{it} \tag{8}$$

$$\nu_{it} = \mu_{j(i,t),t} + \epsilon_{it} \tag{9}$$

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

To compute value-added for a given teacher-year, we estimate equation (8), and then compute the average residual within each teacher-year cell: $\text{VA}_{jt} \equiv \overline{\nu}_{jt}$. Unlike many prior studies, we do not use an Empirical Bayes or similar procedure to "shrink" these noisy estimates of value-added, as we will only use these measures as dependent variables and are therefore less concerned about measurement error (and potentially more concerned about biased estimates).[57]

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

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

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

Appendix C: Treatment effect heterogeneity

Student effects: Heterogeneity

Heterogeneity in estimated student effects is presented in Table A4. Row 1 reports pooled estimates using the entire sample, which correspond to baseline estimates presented in column 1 of Table 4. In the remaining rows, the one-parameter treatment effect coefficients are interacted with student demographic and other characteristics.[58] Estimated cognitive effects are nearly twice as large for girls than boys, and the differences are statistically significant ($p < 0.01$) for both math and ELA. Effects on student effort are also larger for girls, although the magnitude of the difference is smaller and not significant. The pattern is the opposite for attendance, as effects on the number of days attended are larger for boys than girls, although the magnitude of the difference is small. These differences suggest that substandard classroom facilities may inhibit girls' learning more than boys, although the mechanisms underlying this difference are unclear.

When results are split by level of parental education, a mixed picture emerges. Estimated effects on math scores, ELA scores, and attendance are larger for students with parents who did not attend any level of postsecondary education, although the difference in math scores is small and insignificant. For student effort, estimated effects are over twice as large for students with parents who have any level of postsecondary education than for those whose parents have a high school education or less. Overall, the results provide little evidence that improvements in school facilities systematically benefit students from lower socio-economic backgrounds.[59] Recall however, as shown in Table 2, that there is little variation in socio-economic status in LAUSD: nearly 90% of treated students are eligible for free or reduced-price lunch and less than one-fifth have parents with any level of post-secondary education.

Table A4 also shows estimates split by school level. Cognitive effects are insignificant for students who attend new middle schools, although for math, the difference between elementary and middle school effects is insignificant. For ELA, effects are large and significant in both elementary and high school, and are essentially zero for students who switch to a new middle school. For attendance, a clear pattern emerges: effects increase monotonically with school level, and are the largest for students in new high schools. Insofar as student motivation is impacted by new facilities and drives changes in student attendance, we would expect effects to grow with grade level as older students have greater autonomy over attendance decisions than younger students, whose daily

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

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

attendance is more directly dictated by parental influence.

Finally, estimated effects are also split by whether a student switches schools during a "regular" grade transition (KG, G6, G9) or switches to a new school in another grade. "Irregular" grade transitions in off-grades occurred immediately following school construction, when students were transferred between schools to fill enrollment at the new school. Overall, effects are similar for both types of switching students, with only a large and significant difference in estimated attendance gains. Estimated effects on cognitive outcomes and student effort are somewhat larger for initial switchers who switch during an irregular grade transition, although only the difference in math scores is statistically significant at the 10% level. For student attendance, effects are significantly larger (5.2 days vs 1.9 days) for regular grade switchers. Students switching at a typical grade transition are mostly switching in grades 6 and 9, which explains most of the difference in days attended, as attendance gains are larger for middle and high school students than elementary school switchers.[60]

Real estate effects: By school level

Estimates reported in Table 10 and Figure 8 include properties that received multiple new schools. The average treated property in the sample was in the school attendance area of 1.1 new school constructions, implying the the effect of receiving a single school (elementary, middle or high) would be 9% lower than the baseline estimates, roughly a 5.5% increase in house prices per new school construction using baseline estimates from column 2. In Table A5 we report house price effects separately by school level. Results indicate that effects are largest for new elementary and high schools, although we cannot statistically reject differences in estimated coefficients in all specifications. Qualitatively, results are consistent with student effect heterogeneity reported in Table A4, which provided evidence that test score effects were larger and more significant for newly constructed elementary and high school than for new middle schools. As middle schools represent the shortest duration of student attendance (3 years, vs 4 for high school and up to 6 for elementary), it is unsurprising that the effects may be smaller.

[60] See Figure A4 for the distribution of student switching grades to new schools.