Online Appendix

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

Julien Lafortune & David Schönholzer

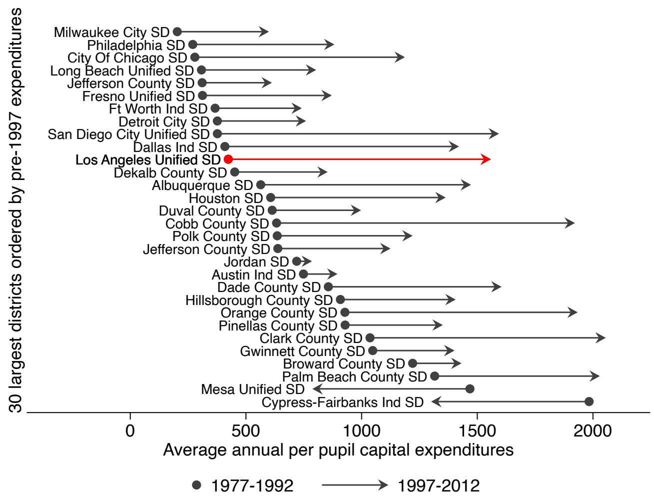
June 25, 2022

Contents

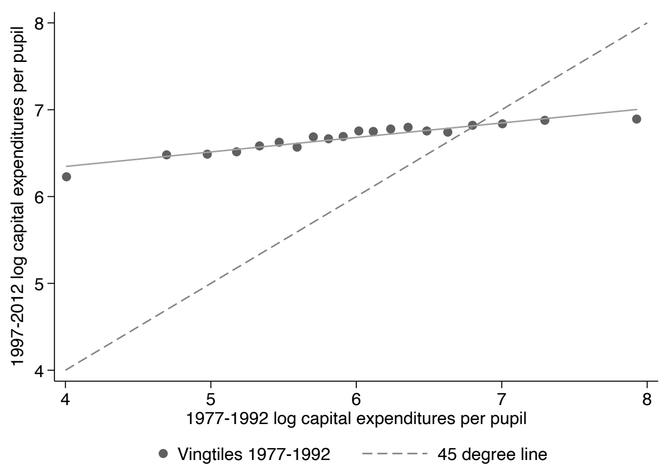
A Appendix Figures and Tables

A.1 Appendix Figures

Figure A1: LAUSD Capital Spending in Context



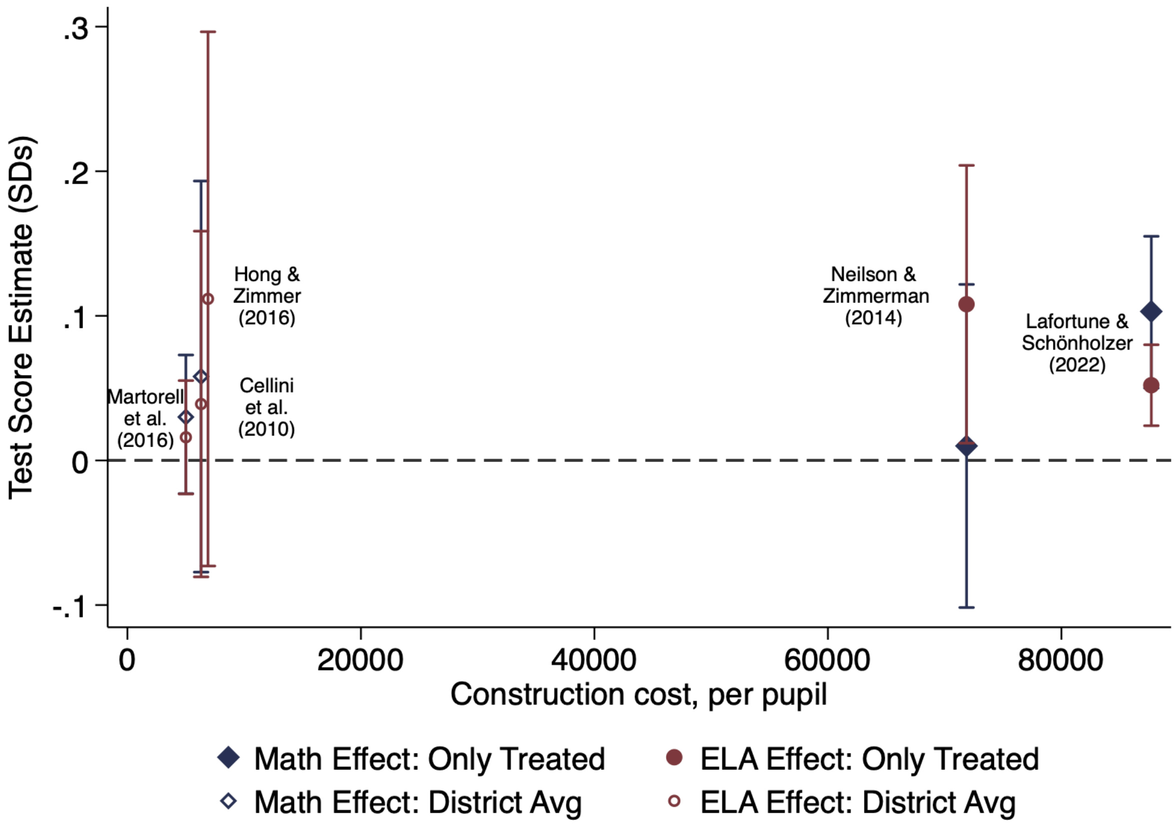
(a) School capital spending for 30 largest districts



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

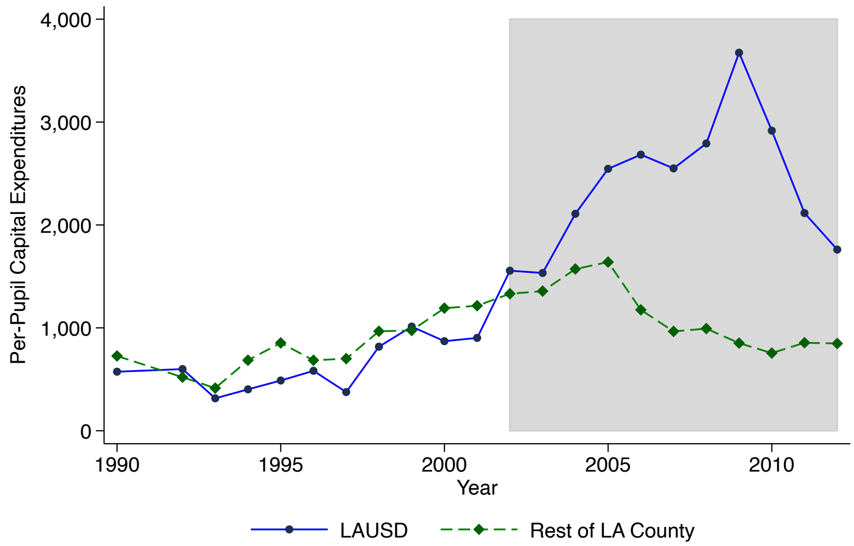
Notes: Top panel shows average per-pupil capital spending for the thirty largest school districts in the U.S. (other than the New York district, which is a dependent agency of the City of New York) over the period 1977-1992 and 1997-2012. Bottom panel shows means in 5% percentile bins (vingtiles) of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

Figure A2: Student effects comparison from capital expenditure literature

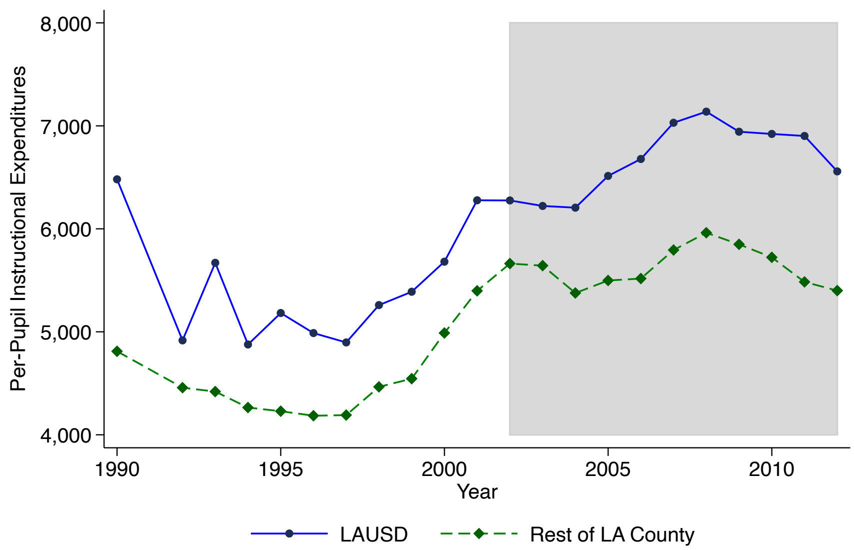


Notes: Figure plots estimated coefficients from related papers in economics evaluating the effects of school capital expenditures (y axis) against per-pupil expenditures in each study (x axis). Blue diamond shaped markers denote math test score estimates whereas red circular markers denote English / Language Arts test score estimates (both in standard deviation units). Solid markers denote estimates on directly treated students from Neilson and Zimmerman (2015) and Lafortune and Schönholzer (2022), 4 years after school construction or student occupancy, respectively. For these studies, construction cost is calculated per treated pupil. Hollow markers denote estimates from studies examining district average test scores after passage of a capital construction bond, where construction cost per pupil is the average over all students in the district. For these studies, estimates 6 years after bond passage are reported.

Figure A3: Spending per pupil, LAUSD vs LA County



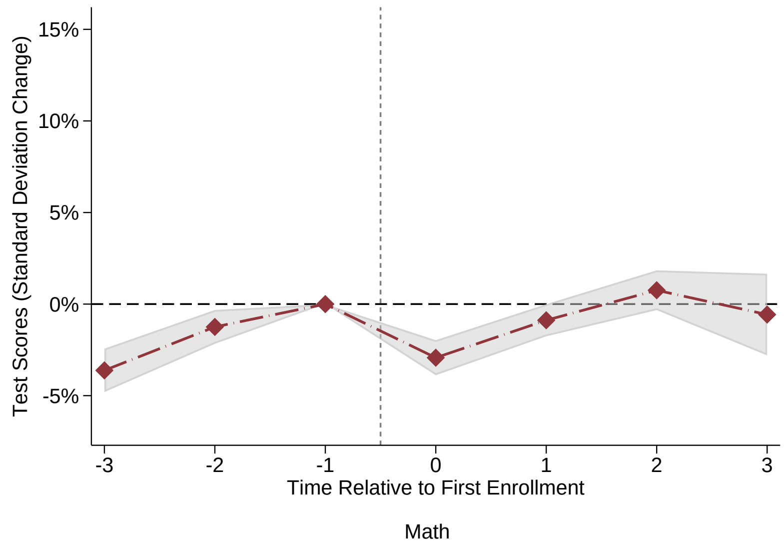
(a) Capital



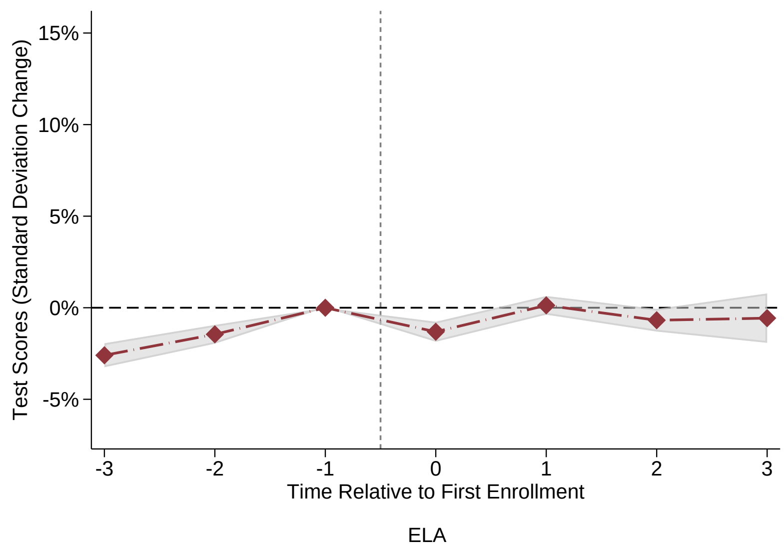
(b) Instructional

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

Figure A4: Student switching, non-new facility related



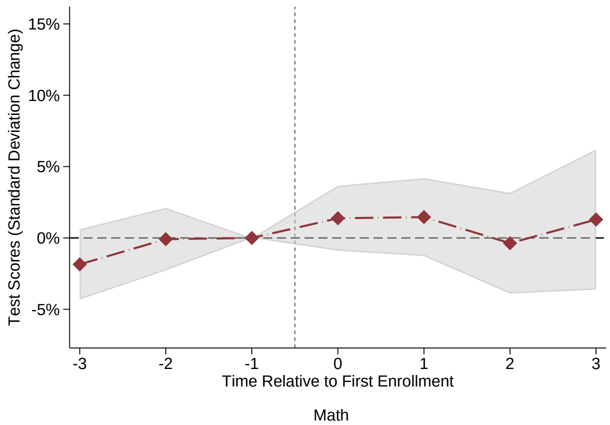
(a) Math



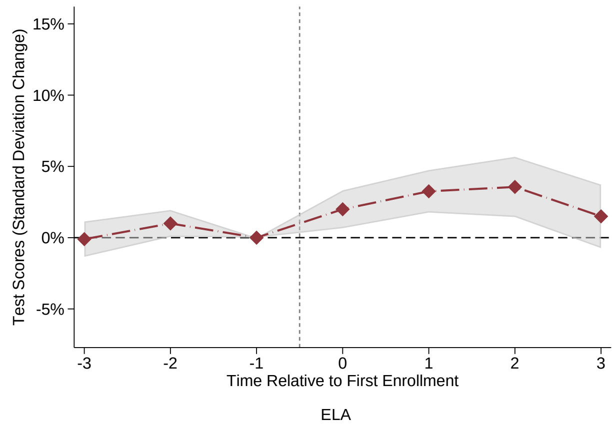
(b) ELA

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

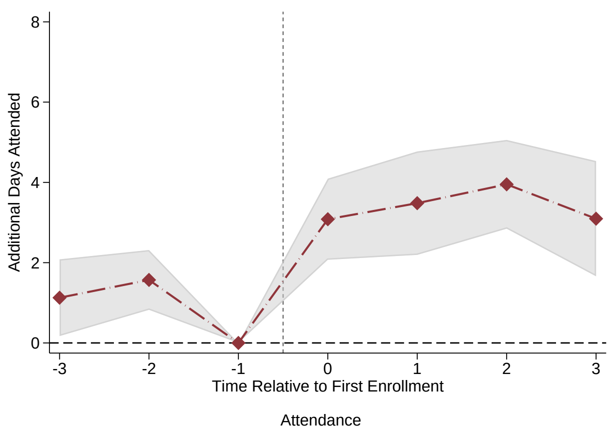
Figure A5: Student effects: Stayers



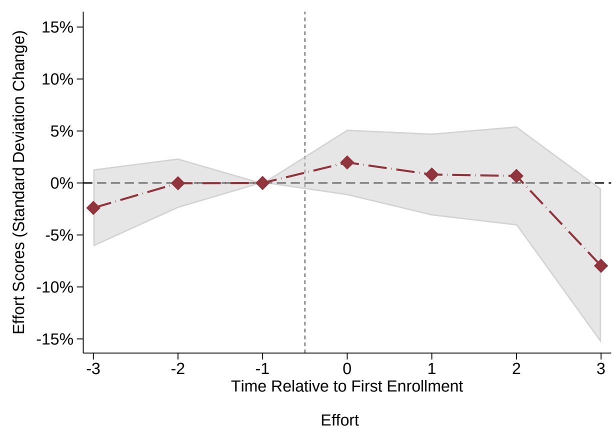
(a) Test scores: Math



(b) Test scores: ELA



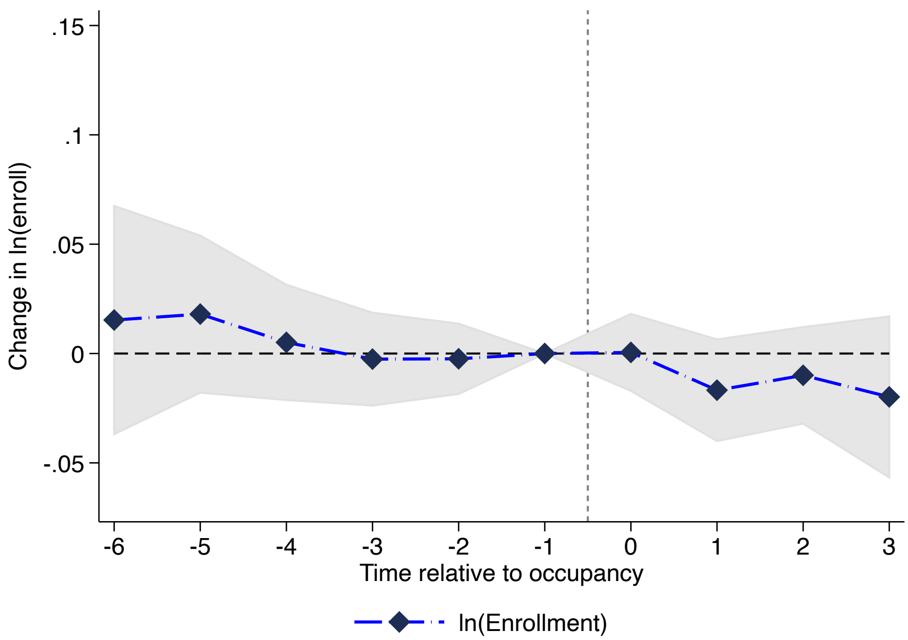
(c) Attended days



(d) Teacher-reported student effort

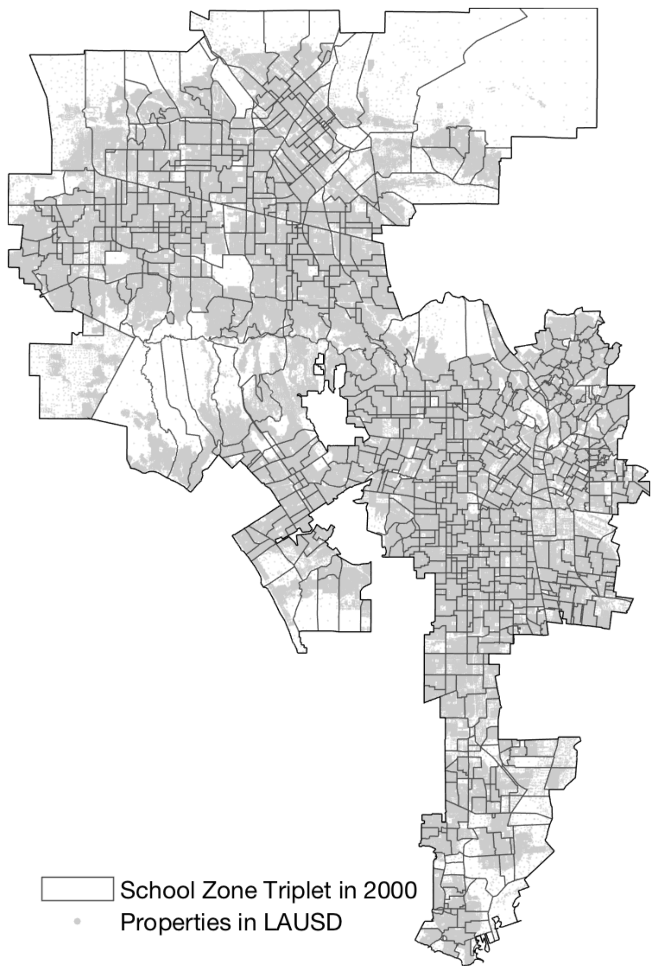
Notes: Figures show estimated coefficients from event study regressions following equation (1) for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Event time is centered relative to the year of the peer outflow. Dependent variables are standardized math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

Figure A6: Neighborhood student enrollment effects



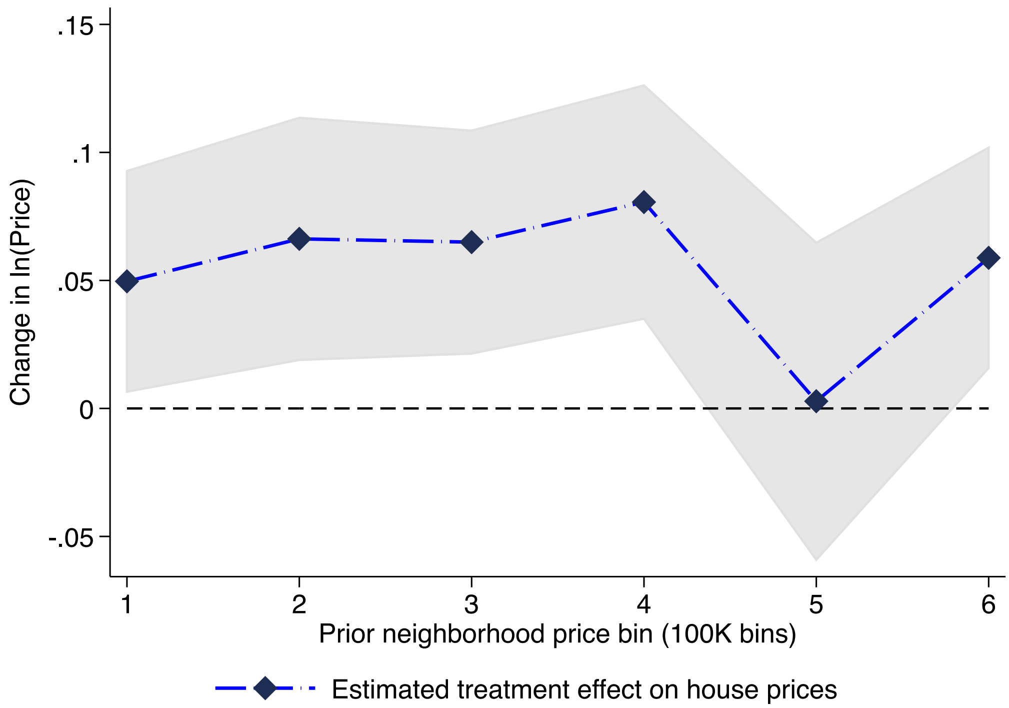
Notes: Figures show estimated coefficients from event-study regressions following equation (4). Dependent variable in both panels is the ln(enrollment). Enrollment is defined as the number of students attending an LAUSD school living in a particular neighborhood (i.e. school assignment zone triplet). All properties in LAUSD in the data sample are included in estimation, corresponding to the specification used in column (1) of Table 8. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure A7: Neighborhood boundaries in LAUSD, based on 2000 school zones



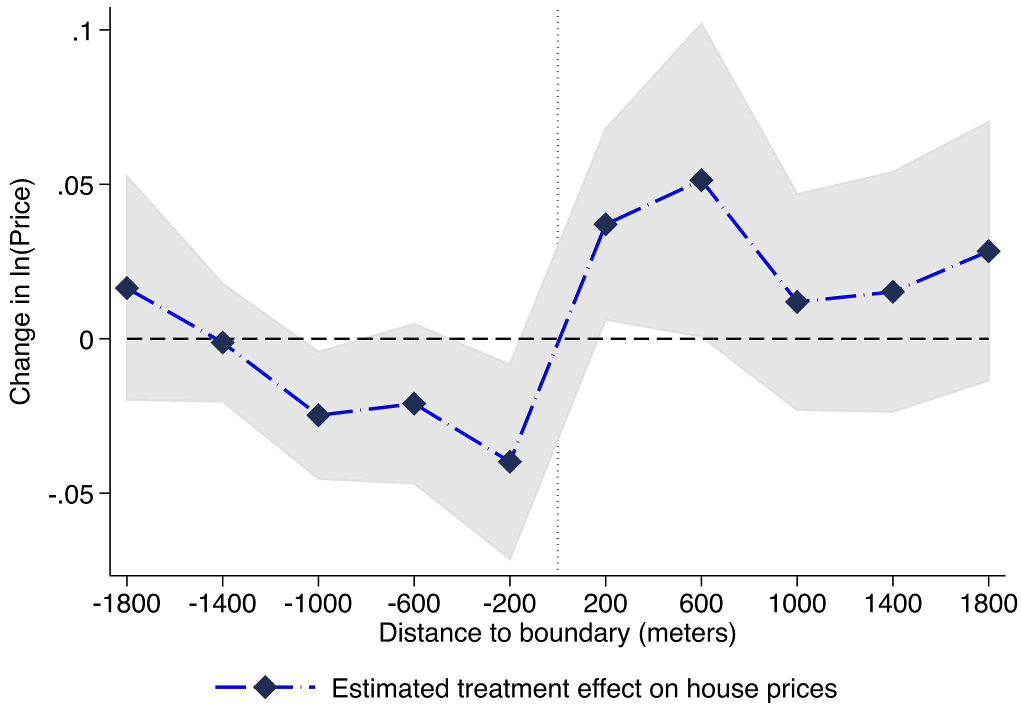
Notes: Figure shows school assignment zone triplets in LAUSD using 2000 assignment boundaries, which are used to define neighborhoods in the estimation of real estate effects. Solid lines denote neighborhood boundaries. Each gray dot represents one property from the LA County Assessor data.

Figure A8: Heterogeneity: By neighborhood mean prior house prices



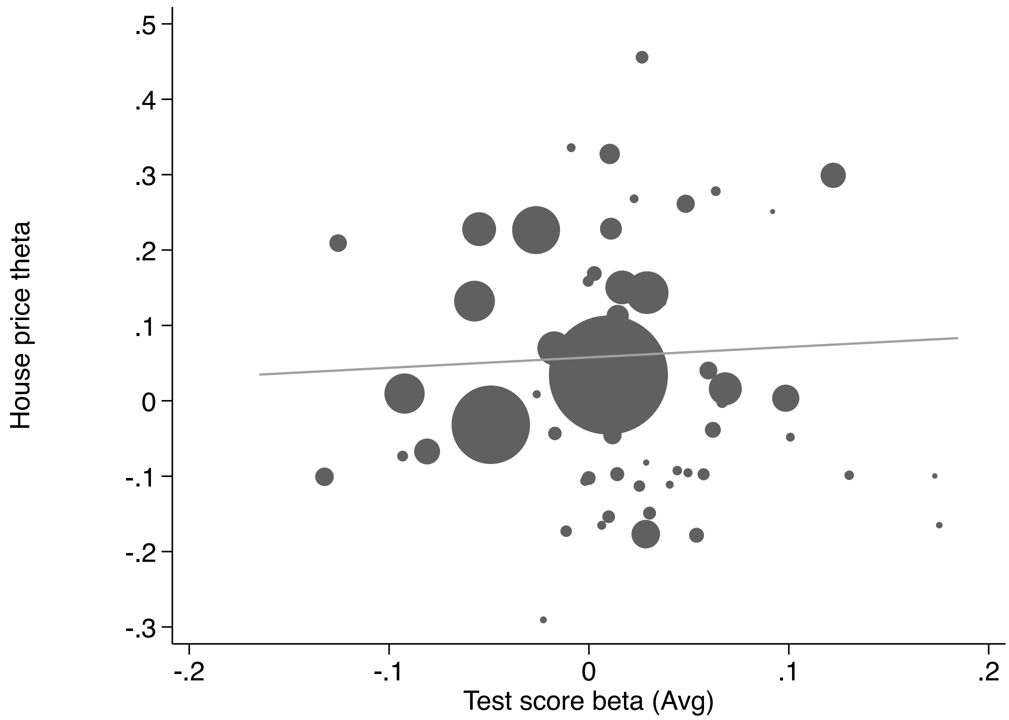
Notes: Figure shows estimated coefficients from a difference-in-difference regression based on equation (3), where the treatment indicator is interacted with indicators for $100,000 bins of prior neighborhood average prices. Bin 1 also includes average neighborhood house prices less than $100K, while bin 6 includes all neighborhoods with average house prices above $600K; all other bins only include a $100K range. Prior neighborhood average house prices are calculated using data from pre-construction property sales from 1995-2001. All properties in LAUSD in the data sample are included in estimation, corresponding to baseline estimates presented in column 1 of Table 8. 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 A9: Spillovers: Effects by distance to school attendance boundary



Notes: Figure shows estimated coefficients from a difference-in-difference regression based on equation (3), where the treatment indicator is interacted with indicators for 400 meter bins of distance to the new school 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 1 of Table 8. Specifications include property-specific controls, year-by-high school zone fixed effects, neighborhood fixed effects, and month fixed effects. Standard errors are clustered by neighborhood.

Figure A10: Correlation between house price and test score effects



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

A.2 Appendix Tables

Table A1: Student effects, "staying" students

	Math Score		ELA Score		Days Attended		Effort Score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post * Trend	-0.002	-0.009	0.005	0.000		0.078		-0.030
	(0.005)	(0.005)	(0.003)	(0.003)		(0.192)		(0.010)
Post		0.008		0.019	3.090	2.340	0.013	0.014
		(0.011)		(0.007)	(0.433)	(0.543)	(0.017)	(0.016)
Trend		0.006		0.001		0.220		0.008
		(0.002)		(0.001)		(0.116)		(0.004)
Grade FEs	X	X	X	X	X	X	X	X
PLD-Yr FEs	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X
N student-years	2,475,534	2,475,534	3,864,575	3,864,575	4,898,155	4,898,155	2,379,586	2,379,586
N students	640,751	640,751	855,498	855,498	1,060,584	1,060,584	604,309	604,309
N treated stu	146,255	146,255	166,614	166,614	180,403	180,403	121,221	121,221
N treated cohort	24,064	24,064	30,765	30,765	38,713	38,713	29,062	29,062

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

Table A2: School-level changes for staying students

	Calendar		School		Peers	
	(1) Multiple	(2) Max days	(3) Age	(4) Stu/tch	(5) Peers: Bl/Hisp	(6) Peers: Index
Post: Stayers	-0.212	0.944	1.602	-0.152	-0.013	0.019
	(0.024)	(0.263)	(0.803)	(0.125)	(0.003)	(0.004)
Grade FEs	X	X	X	X	X	X
PLD-Yr FEs	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X
N student-years	5,656,014	5,143,415	5,558,386	2,497,391	5,657,182	3,732,945
N students	1,133,136	1,090,311	1,121,033	633,944	1,133,390	817,334
N treated students	184,744	183,023	184,286	126,343	184,755	161,503
N treated schools	804	794	755	503	804	786

Notes: Table reports estimates corresponding to one-parameter versions of equation (2), for students that had 10% or more of their school-grade cohort exit to a newly constructed school. Event time is centered relative to the year of the peer outflow. Only the coefficient for the immediate effect β_1 is included; β_2 and β_3 are constrained to be zero. Dependent variables are multi-track status (column 1), total instructional days (column 2), school age (column 3), class size (i.e. pupils per teacher) for students in grades K-5 (column 4), school leave-out mean proportion black and/or hispanic (column 5), and school leave-out mean predicted test scores (column 6). All specifications include fixed effects for student, grade, and year-by-physical location (local) district. Standard errors are two-way clustered by school and student.

Table A3: Teacher changes at new schools

	Demographics			VA Average (pre-switch)		VA Gap (new-veteran)	
	(1) Experience	(2) MA+	(3) Pr(New)	(4) Math	(5) ELA	(6) Math	(7) ELA
New School	-3.226	0.019	0.054	-0.002	-0.020	-0.014	-0.024
	(0.185)	(0.006)	(0.006)	(0.006)	(0.014)	(0.012)	(0.008)
Grade FEs	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X
N student-years	6,077,041	6,076,967	5,710,074	3,032,964	4,626,788	1,987,680	2,403,114
N students	1,209,150	1,209,147	1,156,794	767,121	1,005,452	582,416	659,670
N treated students	125,080	125,084	126,949	92,272	103,800	78,462	85,588
N treated schools	134	134	143	75	116	63	62

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

Table A4: Teacher changes at existing schools

	Demographics			VA Average (pre-switch)		VA Gap (new-veteran)	
	(1) Experience	(2) MA+	(3) Pr(New)	(4) Math	(5) ELA	(6) Math	(7) ELA
Post: Stayers	0.697	-0.003	-0.009	-0.013	-0.003	-0.005	-0.012
	(0.096)	(0.003)	(0.004)	(0.007)	(0.008)	(0.021)	(0.011)
Grade FEs	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X
N student-years	6,077,041	6,076,967	5,710,074	2,361,378	2,966,196	1,987,680	2,403,114
N students	1,209,150	1,209,147	1,156,794	653,530	739,424	582,416	659,670
N treated students	182,891	182,890	182,705	137,688	151,199	130,449	144,666
N treated schools	789	789	804	616	727	602	614

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

Table A5: Principal experience

	(1) Exper (Dist)	(2) Exper (Sch)	(3) New (Dist)	(4) New (Sch)
New School	-0.867	-1.104	0.148	0.208
	(0.150)	(0.103)	(0.024)	(0.021)
Grade FEs	X	X	X	X
PLD-Yr FEs	X	X	X	X
Stu FEs	X	X	X	X
N student-years	5,319,924	5,319,924	5,319,924	5,319,924
N students	1,119,111	1,119,111	1,119,111	1,119,111
N treated students	120,057	120,057	120,057	120,057
N treated schools	134	134	134	134

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

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

Controls	(1) Math	(2) ELA	(3) Attendance	(4) Effort
None	0.029	0.015	5.464	0.018
	(0.008)	(0.004)	(0.602)	(0.018)
Predicted peer characteristics	0.028	0.015	5.438	0.017
	(0.008)	(0.004)	(0.509)	(0.018)
Teacher fixed effects	0.021	0.014	6.547	0.004
	(0.004)	(0.003)	(0.493)	(0.014)
Principal fixed effects	0.031	0.014	3.355	0.099
	(0.009)	(0.004)	(0.936)	(0.028)
School calendar	0.028	0.014	2.354	0.016
	(0.007)	(0.004)	(0.475)	(0.019)
Congestion	0.029	0.015	5.465	-0.004
	(0.008)	(0.004)	(0.604)	(0.020)
All mediators	0.026	0.013	3.714	0.011
	(0.005)	(0.003)	(0.894)	(0.022)
Observations	2,227,009	3,401,127	3,412,143	1,523,709

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

Table A7: Student effects, by student characteristics

	Math	ELA	Attendance	Effort
Pooled	0.027	0.017	4.736	0.025
	(0.007)	(0.004)	(0.547)	(0.015)
By Gender:				
Female	0.035	0.024	4.775	0.038
	(0.008)	(0.004)	(0.539)	(0.016)
Male	0.019	0.009	4.698	0.013
	(0.007)	(0.005)	(0.574)	(0.017)
p-value	0.00	0.00	0.71	0.05
By parental education:				
No college	0.028	0.018	4.945	0.019
	(0.007)	(0.004)	(0.575)	(0.016)
Any college	0.022	0.013	3.780	0.056
	(0.009)	(0.004)	(0.541)	(0.018)
p-value	0.42	0.13	0.00	0.01
By prior achievement:				
Below Median	0.067	0.027	7.952	0.160
	(0.012)	(0.007)	(0.723)	(0.019)
Above Median	-0.008	0.009	3.851	-0.165
	(0.020)	(0.007)	(0.592)	(0.018)
p-value	0.00	0.01	0.00	0.00

Notes: Table reports estimates of one-parameter event study models corresponding to equation (2). Columns 1 and 2 include only the coefficient for the change in trend β_2; β_1 and β_3 are constrained to 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 zero. Dependent variables are standardized test scores (columns 1, 2), annual days attended (column 3), and standardized effort scores (column 4). The first panel repeats baseline estimates from column 1 of Tables 3 and 4. The remaining panels report interactions with student gender (panel 2), parental education (panel 3), and prior test scores / attendance/ effort score (panel 4). P-values for the test of equality of the coefficients are reported in the third row of each panel. Specifications include fixed effects for student, year, and grade. Standard errors are two-way clustered by school and student.

Table A8: Student effects, by context of switch

	Math	ELA	Attendance	Effort
Pooled	0.027	0.017	4.736	0.025
	(0.007)	(0.004)	(0.547)	(0.015)
By residential mobility				
Mover	0.025	0.018	3.566	0.025
	(0.008)	(0.004)	(0.524)	(0.016)
Non-mover	0.028	0.015	5.782	0.027
	(0.007)	(0.005)	(0.630)	(0.019)
p-value	0.45	0.24	0.00	0.88
By school level:				
Elementary	0.027	0.016	2.704	0.025
	(0.008)	(0.005)	(0.489)	(0.015)
Middle	0.026	-0.005	3.503	
	(0.023)	(0.007)	(0.824)	
High		0.028	7.122	
		(0.008)	(1.065)	
p-value	0.98	0.00	0.00	
By grade of switch:				
Reg (KG,G6,G9)	0.018	0.015	5.893	0.051
	(0.009)	(0.005)	(0.651)	(0.030)
Irregular	0.036	0.019	3.207	0.009
	(0.009)	(0.005)	(0.560)	(0.015)
p-value	0.08	0.42	0.00	0.20

Notes: Table reports estimates of one-parameter event study models corresponding to equation (2). Columns 1 and 2 include only the coefficient for the change in trend β_2; β_1 and β_3 are constrained to 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 zero. Dependent variables are standardized test scores (columns 1, 2), annual days attended (column 3), and standardized effort scores (column 4). The first panel repeats baseline estimates from column 1 of Tables 3 and 4. The remaining panels report interactions with residential mobility (panel 2), school level (panel 3), and whether the grade of switch was “typical” (KG, G6, G9) or not (panel 4). P-values for the test of equality of the coefficients 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 A9: Student effects, test scores (excluding “stayers”)

	Math						English Language Arts					
	OLS			2SLS			OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New School * Trend	0.028	0.034	0.032	0.025	0.029	0.028	0.018	0.019	0.018	0.019	0.024	0.022
	(0.007)	(0.009)	(0.009)	(0.011)	(0.012)	(0.014)	(0.004)	(0.004)	(0.004)	(0.007)	(0.008)	(0.008)
New School		-0.028	-0.031		-0.015	-0.018		-0.001	-0.004		-0.016	-0.022
		(0.017)	(0.017)		(0.020)	(0.021)		(0.009)	(0.009)		(0.009)	(0.011)
Trend			0.002			0.002			0.001			0.003
			(0.003)			(0.004)			(0.002)			(0.002)
Cumul. Effect	0.083	0.074	0.066	0.076	0.071	0.065	0.055	0.055	0.050	0.058	0.056	0.043
	(0.022)	(0.023)	(0.024)	(0.034)	(0.035)	(0.043)	(0.013)	(0.013)	(0.014)	(0.021)	(0.022)	(0.028)
Grade FEs	X	X	X	X	X	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X	X	X	X	X
N student-years	2,211,961	2,211,961	2,211,961	2,211,961	2,211,961	2,211,961	3,457,958	3,457,958	3,457,958	3,457,958	3,457,958	3,457,958
N students	577,831	577,831	577,831	577,831	577,831	577,831	779,126	779,126	779,126	779,126	779,126	779,126
N treated students	86,595	86,595	86,595	86,595	86,595	86,595	96,013	96,013	96,013	96,013	96,013	96,013
N treated schools	77	77	77	77	77	77	124	124	124	124	124	124

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

Table A10: Student effects, other outcomes (excluding "stayers")

	Days Attended						Effort					
	OLS			2SLS			OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New School	4.877	4.037	3.094	4.749	4.021	3.059	0.026	0.030	0.031	0.030	0.033	0.034
	(0.538)	(0.643)	(0.670)	(0.520)	(0.654)	(0.702)	(0.015)	(0.016)	(0.017)	(0.019)	(0.019)	(0.021)
New School * Trend		1.302	0.928		1.232	0.846		-0.007	-0.007		-0.007	-0.006
		(0.447)	(0.460)		(0.594)	(0.643)		(0.013)	(0.013)		(0.018)	(0.021)
Trend			0.454			0.474			-0.001			-0.001
			(0.110)			(0.147)			(0.005)			(0.007)
Grade FEs	X	X	X	X	X	X	X	X	X	X	X	X
PLD-Year FEs	X	X	X	X	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X	X	X	X	X
N student-years	4,516,938	4,516,938	4,516,938	4,516,938	4,516,938	4,516,938	2,249,107	2,249,107	2,249,107	2,249,107	2,249,107	2,249,107
N students	990,335	990,335	990,335	990,335	990,335	990,335	571,266	571,266	571,266	571,266	571,266	571,266
N treated students	119,158	119,158	119,158	119,158	119,158	119,158	91,100	91,100	91,100	91,100	91,100	91,100
N treated schools	143	143	143	143	143	143	80	80	80	80	80	80

Notes: Table reports estimates of parametric event study models corresponding to equation (2), excluding "stayer" students who experience outflows to newly constructed schools. Estimates are analogous those presented in Table 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 (2). Dependent variable is the annual days attended in columns 1-3. In columns 4-6 the dependent variable is the standardized average teacher-reported effort score (grades K-5). All specifications include fixed effects for student, grade, and year-by-physical location (local) district. Standard errors are two-way clustered by school and student.

Table A11: Student effects, robustness

	Baseline	Only Treated	Only Switchers	Balanced
ELA Score				
New School * Trend	0.017	0.016	0.014	0.039
	(0.004)	(0.005)	(0.005)	(0.015)
Math Score				
New School * Trend	0.027	0.034	0.035	0.045
	(0.007)	(0.012)	(0.012)	(0.034)
Days Attended				
New School	4.74	3.65	3.69	5.58
	(0.55)	(0.74)	(0.76)	(1.93)
Effort Score				
New School	0.025	0.024	0.031	0.069
	(0.015)	(0.017)	(0.019)	(0.048)

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

Table A12: Student effects, using lagged address from two years prior as instrument

	Math Score			ELA Score			Days Attended			Effort Score		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
New School * Trend	0.022	0.025	0.025	0.027	0.029	0.031		1.224	1.104		-0.011	-0.009
	(0.013)	(0.014)	(0.016)	(0.009)	(0.010)	(0.010)		(0.623)	(0.687)		(0.019)	(0.023)
New School		-0.012	-0.012		-0.004	0.000	5.564	4.655	4.391	0.035	0.042	0.045
		(0.023)	(0.024)		(0.012)	(0.014)	(0.640)	(0.746)	(0.910)	(0.023)	(0.022)	(0.024)
Trend			0.000			-0.002			0.121			-0.003
			(0.003)			(0.002)			(0.173)			(0.007)
Cumul. Effect	0.066	0.063	0.063	0.082	0.082	0.092						
	(0.039)	(0.04)	(0.049)	(0.026)	(0.026)	(0.033)						
Grade FEs	X	X	X	X	X	X	X	X	X	X	X	X
PLD-Yr FEs	X	X	X	X	X	X	X	X	X	X	X	X
Stu FEs	X	X	X	X	X	X	X	X	X	X	X	X
N student-years	2,851,854	2,851,854	2,851,854	4,397,778	4,397,778	4,397,778	5,572,957	5,572,957	5,572,957	2,761,809	2,761,809	2,761,809
N students	724,087	724,087	724,087	945,740	945,740	945,740	1,170,739	1,170,739	1,170,739	692,490	692,490	692,490
N treated students	86,412	86,412	86,412	95,771	95,771	95,771	118,967	118,967	118,967	90,998	90,998	90,998
N treated schools	77	77	77	124	124	124	143	143	143	80	80	80

Notes: Table reports estimates of parametric event study models corresponding to equation (2), using a student's lagged residence from 2 years prior to switching to a new school to instrument attendance. Estimates are analogous to the 2SLS estimates presented in Tables 3 and 4. Dependent variable is the standardized math test score (grades 2-7) in columns 1-3; the standardized ELA test score (grades 2-11) in columns 4-6; the number of days attended in columns 7-9; and the standardized effort score (grade KG-5) in columns 10-12. All specifications include fixed effects for student, grade, and year-by-physical location (local) district. Standard errors are two-way clustered by school and student.

Table A13: 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
					(0.022)
Only New Middle					0.008
					(0.018)
Only New High					0.071
					(0.034)
p, Elem effects =0	.0007			.068	.0026
p, Mid effects =0		.17		.86	.66
p, HS effects =0			.019	.028	.034
p, All effects =0				.041	.0036
p, All effects equal				.18	.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	381,577	374,859	480,906	505,715	471,229
R2	.83	.83	.82	.82	.83

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

Table A14: 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	0.059	0.048
	(0.018)	(0.028)	(0.020)	(0.018)	(0.014)
Baseline sample	X	X	X	X	X
Price outliers		X			
Renovated			X		
Large/multi-unit				X	X
Non-residential					X
Number of sales	505,714	512,507	525,397	512,973	625,516
R2	.82	.75	.75	.8	.72

Notes: Table reports estimated coefficients from difference-in-difference regressions corresponding to estimates of equation (3). Dependent variable is the ln(sale price). Column 1 repeats baseline estimates presented in column 1 of Table 8. 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 year-by-high school zone fixed effects, neighborhood fixed effects, property specific controls, and month fixed effects. Standard errors are clustered by neighborhood.

Table A15: "Stayers" school zones

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

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

Table A16: Predicted house prices

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

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

B Effects on Staying Students and Sending Neighborhoods

B.1 Effects on Staying Students

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

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

Figure A5 shows event-study estimates of test score and other outcomes for stayers. Stayers see small but significant increases in ELA scores (panel B) following the cohort outflow to the new facility. For math (panel A), effects are smaller and insignificant. The increase in days attended (panel C) is immediate and significant - students attend 3.1 (SE 0.5) more days relative to the year prior to the cohort outflow. Panel D shows estimates for standardized effort scores. Point estimates are all very close to zero and insignificant, with the exception of the binned endpoint for 4 or more years of exposure, which is negative and significant.

Parametric versions of the estimates corresponding to equation (2) are reported in Table A1. For each outcome, both one- and three-parameter estimates are shown. Columns 1 and 2 report estimates for math test scores. Estimates in column 1 show no change in test score growth in the years following the cohort outflow, while estimates in column 2 show that once pre-existing trends

[1] Results are robust to alternative thresholds, although the sample of "stayers" diminishes considerably above a 20% threshold.

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

are included, there is a small insignificant immediate effect that fades out within the following year. For ELA (columns 3 and 4), the pattern is different, and the parametric estimates more closely align with the event study estimates in Figure A5 . Column 3 shows a 0.5% (SE 0.3%) of a standard deviation increase in ELA test score growth in the years following the cohort outflow. However, once the post indicator and trend variable are included in column 4, all of the effect loads onto the post coefficient, with no ensuing growth or fade-out of effects. This pattern of cognitive effects differs from that of students attending new schools: effects accrue immediately, and either fade out (math), or remain roughly constant (ELA).

Columns 5 and 6 report estimates for days attended. Stayers see a 3.1 (SE 0.4) day increase in days attended; this effect attenuates to 2.3 (SE 0.5) days with the inclusion of trend variables. Columns 7 and 8 show no immediate effects on teacher-reported effort, with evidence of small negative effects after several years. As shown in Panel D of Figure Figure A5, this is driven by negative but imprecise effects several years post-outflow. Taken together, these results indicate positive indirect effects induced by peer outflows to new school facilities, but only for ELA test scores[3] and attendance.

Besides reductions in overcrowding due to peer outflows to newly constructed schools, what sort of changes in the school environment were experienced at these existing schools? Appendix Table A2 presents estimates of the changes staying students experienced after they experienced a cohort outflow. Results indicate that stayers experienced a significant decline in multi-track calendar usage (21pp) and a significant increase in the total number of instructional days (0.94) per year. Both staying and switching students experienced a similar decline in multi-track calendars, and increase in the total number of instructional days per year. This is driven by the fact that in LAUSD, students at multi-track schools often had fewer instructional days per academic year.[4] Class sizes saw a negligible and insignificant decline for students who stay behind. Comparing these estimates to the estimated 3 day increase in total attended days in Table A1 implies that roughly one-third of the attendance effect is mechanically driven by increased number of days. Columns 5 and 6 report changes in the average peer group. Consistent with the fact that switching students were slightly more disadvantaged and lower-scoring than staying students, stayers see reductions in peer minority shares and increases in predicted scores of peers due to cohort outflows to new facilities.

Overall, indirect effects appear to be driven by reductions in overcrowding and the switch from

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

[4] 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.

multi-track calendars to traditional schedules.[5] Attendance effects are roughly half the size as for switching students, once the increase in the number of instructional days is factored in. Test score effects are only significant in ELA and not math, and are much smaller than those estimated for students who switch into new schools. Taken together, these results indicate that overcrowding reductions are not a primary mechanism driving effects at newly constructed schools. In the next section we examine the mechanisms underlying the new school effects in greater detail.

B.2 Effects on Sending Neighborhoods

As discussed in Section B.1, schools that experienced large student outflows to new schools saw significant reductions in overcrowding and multi-track calendar utilization, and small but significant increases in the share of more advantaged students. Students at these schools also experienced gains in ELA scores and attendance. To what extent were these gains at existing "sending" schools capitalized into local house prices? In Appendix Table A15 we report difference-in-differences estimates where treatment is similarly defined for existing "sending" schools that experienced student outflows to newly constructed facilities.[6] Specifications in columns 1 and 2 correspond to those in columns 1 and 2 of panel A; specifications in columns 2-5 correspond to those in columns 3-6 in panel A. Estimates provide little indication that house prices increased in the sending school neighborhoods. These results suggest that (a) parental valuation of new schools is driven by non-test score/amenity improvements at new schools, independent of the school calendar or level of overcrowding, and/or (b) improvements in school quality due to reductions in overcrowding and multi-track calendar utilization are less salient to prospective homebuyers, who may instead rely on school facility condition as a signal for underlying school quality. Later, in Section E we will use a residential choice model to interpret the valuation and efficiency of the program; our findings imply that most of the valuation of the new schools is driven by non-test score and/or amenity improvements.

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

[6] "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.

C Further Evidence on Mechanisms

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

Contemporaneous changes in peers, class sizes, and the school environment are documented in Table 5. One of the stated goals of the LAUSD school construction program was to eliminate the use of multi-track academic calendars that required schools to continuously operate year-round. Before the construction program, half of LAUSD students attended multi-track schools. By reducing overcrowding in neighborhood schools, district officials were able to begin new schools on traditional two-semester calendars, as well as convert existing schools back to traditional calendars.

Column 1 of Table 5 reports difference-in-differences and event study estimates of the likelihood of being exposed to a multi-track calendar. Switching to a new school was accompanied by a 28 percentage point reduction in the likelihood that a student was exposed to a multi-track calendar. This conversion also meant that many students in new schools experienced additional instructional days: as reported, students switching to a new school had on average nearly one additional instructional day per year, relative to the prior year at an existing school (column 2, Table 5 in main paper). Taking the 2SLS estimate of 5 additional days attended per year from Table 4, this implies that roughly one-fifth of the observed attendance effect is mechanically due to a change in school calendar.

At new schools, class sizes were actually somewhat larger: on average, teachers at new schools taught classes with 0.24 more students per teacher (Column 4 of Table 5 in main paper). The magnitude of this difference, however, is quite small; roughly speaking, the district was approximately able to maintain similar pupil-teacher ratios at new school facilities by transferring teachers to new facilities in roughly equal proportion to students. Thus, changes in class size do not contribute to the estimated new school effects.

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

Appendix Table 5 in main text).[7]

C.2 Teacher observables and principal quality

Student gains at new schools could be attributable to systematic differences in teacher and/or principal quality between new and existing schools. New facilities provide improved working environments for staff, and could attract better quality staff from either within or outside the district.[8] In this section we examine the changes in teacher and principal demographics, and teacher value-added associated with a student's switch to a new school facility.

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

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

C.2.1 Estimating value-added

Observable teacher characteristics, however, generally explain little of the variation in test-score based measures of teacher quality. We next estimate teacher-specific value-added scores to further examine the extent of any teacher quality differences at new schools. To estimate value-added, we use a subsample of students for which the following criteria are met: (1) Student-year observations

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

[8] Complementarities between facility quality and teacher effort could also result in improved teacher productivity at new schools. Unfortunately, any such improvements are not easy to separate from general school- or student-level improvements.

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;[9] (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, Friedman and Rockoff (2014):

$$y_{i,t} = \alpha_{t,g(i,t)} + X'_{it}\beta + \nu_{it} \tag{C1}$$

$$\nu_{it} = \mu_{j(i,t),t} + \epsilon_{it} \tag{C2}$$

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 (C1), and then compute the average residual within each teacher-year cell: $\text{VA}_{jt} \equiv \overline{\nu}_{jt}$. Unlike many prior studies, we do not use an Empirical Bayes or similar procedure to shrink these noisy estimates of value-added: here we only use value-added estimates as dependent variables, and using posterior means as left-hand side variables can introduce bias.[10]

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

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

Standard value-added models can confound school and teacher effects. For example, new schools could improve 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

[9]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

[10]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.

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.[11]

Next, 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:

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

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

These estimates are reported in columns 6 and 7 of Table A3, and provide little evidence that newly hired teachers were of higher quality at new schools. [12] Overall, systematic differences

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

[12] As we find evidence of negative sorting of existing teachers on value-added, the difference in estimates between

in teacher quality cannot account for observed student test score gains. In the longer-run, it is still possible that higher-quality facilities could attract and retain better teachers, although further research is necessary to determine if this channel to improve teacher quality is empirically relevant.[13]

C.3 Adjusting for changes in the school environment

How do these changes in the school environment mediate the effects found at new schools? For a classroom or school characteristic to explain any part of the new school effect, it must be the case that (1) there is a change in the characteristic between the new and existing schools attended by switching students, and (2), the characteristic must have a (causal) effect on the student outcomes we study.[14] Contemporaneous changes in teacher, principal, peer, and other school characteristics have been previously documented (Table 5 in main paper; Tables A3, and A5). Prior research has shown that many of these mechanisms may be important determinants of student outcomes.

In Table A6 we examine how the main effect estimates vary with the inclusion of time varying controls for changes in the school and classroom environment at new schools. The first row of Table A6 reports baseline one-parameter effect estimates from (2), for the subsample of students with non-missing values for school characteristics.[15] The second row includes an index of peer quality, based on a prediction of test scores using demographic characteristics. The third and fourth rows include teacher and principal fixed effects, respectively. The fifth row includes a control for whether a multi-track school calendar was used in that school-year, while the sixth row includes a control for school congestion, based on the ratio of current enrollment in a facility to 2013 enrollment, after nearly all of the overcrowding in the district had been eliminated. Finally, the seventh row jointly includes all aforementioned variables in the estimation.

Including controls for peer quality has no impact on any of the effect estimates. Teacher fixed effects slightly attenuate the coefficient on math test scores, but not ELA. On the other hand, attendance estimates actually increase with the inclusion of teacher effects. Taken together with previous evidence that teacher at new schools had, if anything, lower value-added scores, we take this as evidence that teacher quality is not a first-order mechanism mediating the new school effects we document. Similarly, the inclusion of principal fixed effects has little impact on test score estimates, although they attenuate the attendance effect and strongly increase the effort effect.

columns 4 and 6 and columns 5 and 7 would be positive if newly hired teachers were of higher quality at new facilities.

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

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

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

Principals often have discretion over school-wide attendance policies, and may also affect school culture more broadly. However, given that there are relatively few principal switchers between existing and new facilities in the data, we are wary of over-interpreting these results.

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

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

D Real Estate Effect Heterogeneity

D.1 Effects by neighborhood price level

While new school quality was similar across treated neighborhoods,[16] the tax price of the new facilities faced by district residents was greater in areas with higher property values.[17] In Section IV, 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 A8 we report

[16]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.

[17]Unlike in the model presented in Section E, 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.

heterogeneity in estimated treatment effects by neighborhood prior mean house prices. We define neighborhood prior mean house prices as the average house price in a neighborhood over all pre-treatment years in the sample, 1995-2001. Estimates of β from equation (3) in the main paper are shown interacted with \$100,000 bins of neighborhood prior mean house prices.[18] 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.

D.2 Local boundary and spillover effects

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

Estimates in Figure A9 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. Results indicate that within the new school zones, capitalization is roughly constant at approximately 5% for all distance bins. We find no evidence of smaller effects closer to the boundary. Properties within 400 meters but outside of the boundary see statistically significant declines in house prices of 4.9% (SE 1.7%) post-construction, providing evidence of negative spillovers for properties that are just outside the new school zone. These negative spillover effects quickly diminish, however; point estimates for distances greater than 1.2 km are positive, though insignificant. This pattern of results is consistent with cross-neighborhood substitution within very narrowly defined markets, wherein demand for properties located marginally outside the new school zones decreases for prospective homebuyers searching within the vicinity a new school.

[18] 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.

E Household valuation model

E.1 Setup and program costs

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

$$\frac{\partial R_j}{\partial A_j} = \frac{\tau N}{\sum_{k \geq 1} N_k} \equiv \tau n \tag{E1}$$

for $j \geq 1$, while no program funds are spent in control neighborhood $j = 0$.[19] The inverse share of treated households $n = N / \sum_{j \geq k} N_k$ also represents how many dollars are spent on treated neighborhoods for each dollar raised in taxes, and so τn is the cost of the program per household in treated neighborhoods (which we can observe in the data). Because we normalize units of school amenities to the program size and marginal utility of income is assumed to be one across all households, the marginal cost of the program equals the marginal rate of transformation between school amenities and income, MRT. This will be useful to show that, under efficiency, the marginal rate of substitution equals the marginal rate of transformation.

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

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

E.2 Program willingness to pay

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

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

$$\theta_j = \left[\frac{\partial V_j}{\partial c}\right]^{-1} \left(\frac{\partial V_j}{\partial A_j} + \frac{\partial V_j}{\partial Y_i}\frac{\partial Y_j}{\partial A_j}\right) + \xi_j = \mathrm{MRS}^A + \mathrm{MRS}^Y \beta_j + \varepsilon_j \tag{E2}$$

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

The capitalization approach aims to capture both of these sources of program benefits, but recent evidence points towards little parental valuation of school effectiveness (Abdulkadiroğlu et al., 2020). The later life earnings approach avoids this issue by replacing household valuation of achievement revealed in the housing market MRS^Y with (usually external) estimates of the labor market value of human capital W, which may be superior to household forecasts of earnings gains due to academic achievement. Program benefits in neighborhood j are then simply $W\beta_j$. However, the later life earnings approach omits neighborhood attractiveness MRS^A, which may make up an important part of program benefits. The hybrid approach we develop below combines the strength of both of these approaches by estimating benefits in j as $\text{MRS}^A + W\beta_j$: the housing market value of neighborhood attractiveness plus the labor market value of school effectiveness.

To isolate the value of neighborhood attractiveness, consider that achievement is largely uncorrelated with other factors driving heterogeneous program responses, that is $Cov(\beta_j, \varepsilon_j) = 0$ (we discuss the plausibility of this assumption further at the end of this section). Then, notice that the population regression coefficient of the house price effect on the achievement effect in equation (E2) corresponds to household valuation of achievement:

$$\frac{Cov(\theta_j, \beta_j)}{Var(\beta_j)} = \text{MRS}^Y \tag{E3}$$

This is the object of interest in the vast literature on housing valuation of school performance surveyed in Black and Machin (2011), which studies house price responses to variation in student achievement.[20] Using this relationship, we can quantify the share of housing valuation due to the student achievement benefits of the program as

$$\gamma \equiv \frac{\text{MRS}^Y E\left[\beta_j\right]}{\text{MRS}^A + \text{MRS}^Y E\left[\beta_j\right]} = \frac{E\left[\beta_j\right] Cov\left(\theta_j, \beta_j\right)}{E\left[\theta_j\right] Var\left(\beta_j\right)}, \tag{E4}$$

which says that the share of household valuation due to achievement equals the regression coefficient in (E3) rescaled by the ratio of mean effects. This is a general result that can, in principle, be applied to all studies of educational programs for which mean effects on housing and test scores as well as their covariance are available.

As mentioned, for the regression of neighborhood house price effects on achievement effects to yield household valuation of achievement, MRS^Y, we require that other neighborhood characteristics are largely uncorrelated with achievement effects such that $Cov(\beta_j, \varepsilon_j) = 0$. This is generally a strong assumption, and it would be violated if, for instance, neighborhoods with fewer college-educated parents had both higher potential for house price growth and achievement gains. While we are not able to exclude all possible sources of such a bias, we note two points for why our estimate of household valuation of achievement may still be reasonably reliable. First, as shown in table A7, achievement effects do not vary significantly with parental education or residential mobility. So at least in terms of these two important dimensions of household characteristics, there is no indication that achievement effects are strongly correlated with other factors. Second, we discuss below that our estimate is very similar those in the literature on capitalization of school quality using a variety

[20] To see this, integrate both sides of (E2) over A_j to arrive at $P_j = c + Y_j\text{MRS}^Y + u_j$, which corresponds to equation (10.1) in (Black and Machin, 2011).

of identification strategies. True household valuation of achievement is unlikely to deviate strongly from the consensus in this literature.

E.3 Marginal value of public funds and efficiency

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

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

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

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

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

$$\begin{aligned}\mathrm{MVPF} &\equiv (1-\gamma)\,\mathrm{MVPF}^C + \mathrm{MVPF}^E \\ &= \frac{\mathrm{MRS}^A + W\cdot E\left[\beta_j\right]}{\mathrm{MRT}} = \frac{(1-\gamma)\,E\left[\theta_j\right] + W\cdot E\left[\beta_j\right]}{\tau n}.\end{aligned} \tag{E5}$$

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

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

F Cost-benefit analysis

With these relationships in hand, we can now quantify the costs and benefits and decompose the latter into valuation for school effectiveness and school attractiveness. The results of this exercise can be seen in Table 9 in the main paper. We begin with program costs. According to the 2005-2009 American Community Survey (ACS), there were 1.52 million non-vacant housing units in LAUSD. The total cost of the program was \$9.17 billion, meaning that the average cost to a treated household of the program (τ) is approximately \$6,045 in present value. Given that just under one in three households lives in treated neighborhoods, the cost per treated household is around \$18,430.

Moving to program benefits, we begin with the benefits reflected in the real estate market. The average sale price (within-sample) of properties in zones that received new schools was \$494,650. Using the estimates in Table 8 in the main paper, the median house price change in treated neighborhoods is 5.7%. This implies houses in treated neighborhoods gained \$28,195 in value, with a resulting gross capitalization benefit of \$14.06 billion. The ratio of these benefits to costs, which corresponds to MVPF^C, yields a value of 1.53. Thus, housing capitalization suggests the program was inefficiently small.

We now turn to the benefits in the form of later life earnings. Using the estimates presented in Chetty et al. (2011), we can project forward the gain in future earnings from the observed test score gains. Chetty et al. (2011) use experimental variation in classroom quality to estimate that a 0.1 standard deviation increase in test scores[21] leads to a 1.3% increase in earnings at age 27.[22]To extrapolate our estimates forward, we first compute the present discounted value of future earnings

[21]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.

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

for future cohorts:

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

where E_t = earnings gain at each age, which we compute under the assumption of a constant percentage gain of 1.3% per 0.1 SD increase in test scores, using age-earnings profiles from the March CPS.[23] The average elementary school student is 11 years old, therefore we discount forward 16 years to age 27, and count benefits until retirement at age 67. From our data, roughly 16% of students entering elementary school, 13% of students entering middle school, and 25% of students entering high school in LAUSD were in a newly constructed school facility. Plugging this in and using the estimated effects on math test scores, assuming a 3% discount rate, yields a present discounted value of future earnings per cohort of $177 million. From our facilities data, we estimate that a brand new facility would take roughly 35 years to depreciate to the mean condition of existing facilities in LAUSD. Assuming the effects are constant for this 35 year horizon and discounting the earnings of future cohorts implies a gain in later life earnings of $3.9 billion in present discounted value.[24] The total program cost was $9.17 billion, implying that the gain in later life earnings from test score improvements covers roughly 42% of the total program cost, which corresponds to a MVPF^E of around 0.42. If we were to consider only later life earnings, we would conclude that the program was inefficiently large.[25]

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

Specifically, we proceed as follows to arrive at this estimate. First, we compute neighborhood specific treatment effects by interacting our baseline treatment effect coefficients with neighborhood fixed effects.[26] Second, we regress the resulting 65 neighborhood-specific house price effects on their corresponding average achievement effects, weighted by the number of households per neighborhood.

[23] 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.

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

[25] 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.

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

This gives us the coefficient in equation (E3), which we estimate to be 0.168, or \$83,101 after rescaling using average house prices in the district – that is, a one standard-deviation better new school sees house prices rise by 16.8% more (see Figure A10). This is similar to the median estimate in the literature surveyed by Black and Machin (2011), whose median effect across 15 papers is about 0.14 per student-level standard deviation in achievement.[27] Third, we rescale this coefficient by the ratio of mean effects, as given by equation (E4), which results in $\gamma = 0.22$.[28]

This implies a housing valuation of academic benefits of around \$2.96 billion, which is about 76% of estimated later life earnings. Unlike recent work finding that parental preferences for schools are almost entirely determined by peers instead of school effectiveness (Abdulkadiroğlu et al., 2020), this result suggests that households value academic benefits reasonably well. Perhaps, some of the non-academic benefits of educational programs considered in residential choice steer households towards schools in a way that more closely matches the academic value of the program, unlike school choice conditional on residential location.

Finally, we combine these findings to estimate program benefits and the marginal value of public funds using expression (E5). We find that total program benefits using the hybrid approach are around \$14.85 billion, with a marginal value of public funds of around 1.62. The ratio of future earnings valuation (MVPF^E) to total valuation (MVPF) is 0.24, suggesting that around 24% of total program value is due to test score improvements, with the rest due to the capitalization of non-test score improvements. Unsurprisingly, given that the majority of benefits derive from non-academic program benefits and housing capitalization of academic benefits is fairly close to later life earnings, these quantities are quite similar to real estate capitalization alone, as captured in MVPF^C. We conclude from this finding that, while both capitalization and later life earnings are important, using only benefits arising from later life earnings may severely underestimate program benefits.

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

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