Online Appendix for:

The Impact of Public School Choice: Evidence from Los Angeles' Zones of Choice

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A Data Appendix

A.1 Additional ZOC Details

The ZOC program initially included 16 zones, but in recent years, the program has expanded to include more high school zones and middle and elementary schools. In this section, we provide some additional information governing our treated school selection process.

For the purposes of the analysis, we restrict to schools that existed in the school district for a sufficient amount of time before the policy expansion. Several schools opened in the years after the expansion, and those programs are excluded from the market-level analysis. Table A.1 reports the 38 schools that are included in the analysis as treated schools. Note that the Hawkins Zone of Choice is not included. The schools that are part of the Hawkins Zone of Choice opened the year before the policy expansion, so we do not have sufficient pre-period data to include these schools in the market-level analysis.

Also note from that table that although there are nearly 100 total programs available to choose from, many programs are part of a larger school. For the purposes of the analysis, we consider schools as the treated unit and students enrolled in treated schools as treated students, and this is one reason why the table is reduced to 38 schools. Another reason is that we omit schools that open in the post-period. For full transparency, we report the associated schools that are part of a zone that do not make it into the analysis. Finally, the RFK Zone of Choice is one zone in the analysis that does not amount to a choice set expansion. The RFK school complex houses many schools, and the ZOC expansion formalized the application and enrollment process governing this complex. This formalization is part of the treatment we consider in the analysis. Importantly, all results are robust to excluding the RFK Zone of Choice, so their inclusion or omission is not driving any of the findings reported in the paper.

Appendix Table A.2 reports baseline differences between ZOC and non-ZOC schools. This table is analogous to Table 1 in the main paper but weighs every school equally in producing group means. Similar to Table 1, ZOC schools are noticeably different on observable characteristics, and matching balances some of these baseline differences.

Table A.1: ZOC Schools in the Evaluation

Zone	School	Other Schools in the Same Zone
Bell	Legacy Learning Center	
Bell	Bell Senior High	
Bell	Elizabeth Learning Center	
Bell	Maywood Senior High	
Belmont	Contreras - Academic Leadership Community	
Belmont	Roybal Learning Center	
Belmont	Belmont Senior High	
Belmont	Contreras - Global Studies	
Belmont	Contreras - Business and Tourism	
Belmont	Cortines Center	
Bernstein	Bernstein STEM Academy	
Bernstein	Bernstein Senior High	
Boyle Heights	Mendez Senior High	
Boyle Heights	Roosevelt Senior High	
Carson	Carson Complex	Academy of Medical Arts, Academies of Education and Empowerment
Eastside	Garfield Senior High	Solis
Eastside	Torres - STEM Academy	Solis
Eastside	Torres - Social Justice Leadership	Solis
Eastside	Torres - Humanitas Academy of Art and Technology	Solis
Eastside	East Los Angeles Renaissance Academy	Solis
Fremont	Fremont Senior High	Rivera
HP	Huntington Park Senior High	Marquez
Jefferson	Santee Education Ceter	
Jefferson	Jefferson Senior High	
Jordan	Jordan Senior High	Non-district Charter
NE	Lincoln Senior High	
NE	Wilson Senior High	
NV	Sylmar Charter High School	
NV	San Fernando Senior High	
Narbonne	Narbonne HARTS LA	
Narbonne	Narbonne Senior High	
RFK	RFK - New World Academy	
RFK	RFK - School for the Visual Arts and Humanities	
RFK	RFK - Los Angeles School for the Arts	
RFK	RFK - UCLA Community School	
RFK	RFK - Ambassador School of Global Leadership	
South Gate	South East Senior High	
South Gate	South Gate Senior High	

Notes: The first column reports the names of each school included in the evaluation. The second column reports names of schools that are not included.

Table A.2: School-Level Descriptive Statistics

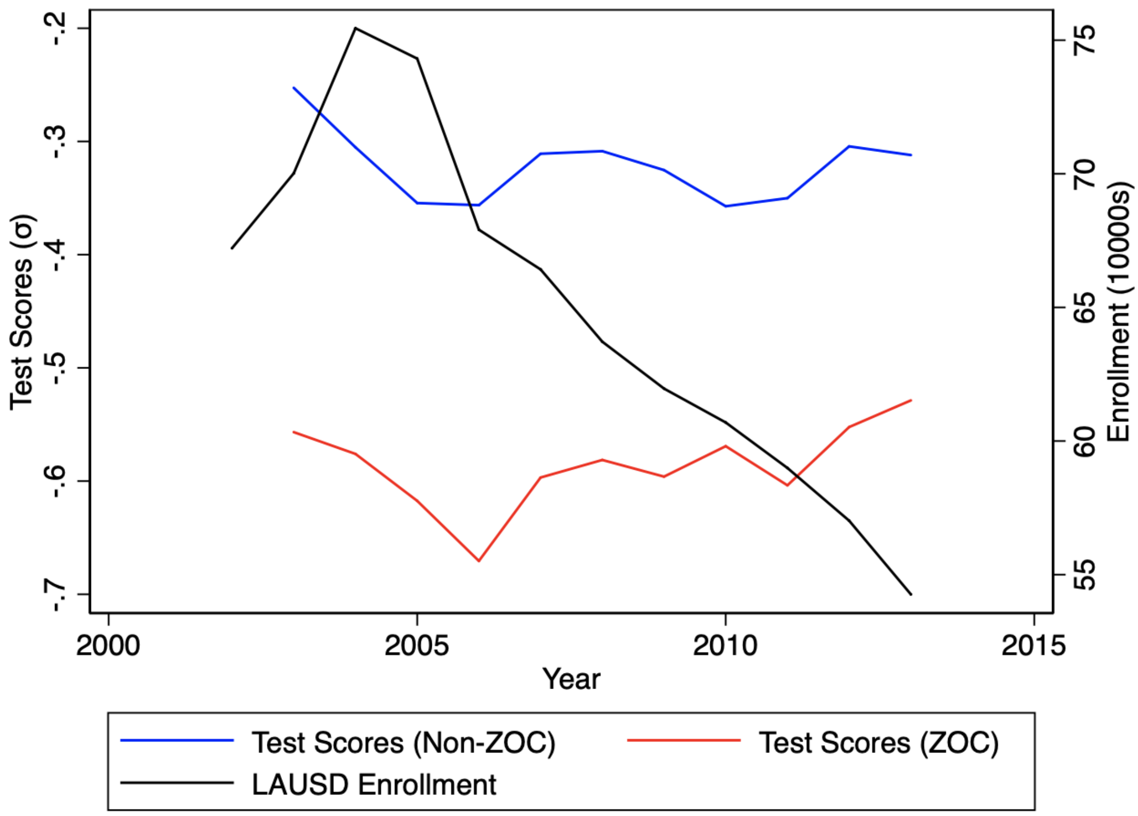
	(1) ZOC	(2) Non-ZOC	(3) Difference	(4) Matched Non-ZOC	(5) Difference
8th Grade ELA Scores	-.11	.12	-.23***	.031	-.125**
			(.057)		(.054)
8th Grade Math Scores	-.069	.129	-.198***	.042	-.098*
			(.056)		(.053)
8th Grade Math Scores	.139	.159	-.019	.153	-.014
			(.019)		(.018)
Black Share	.034	.141	-.107***	.149	-.119***
			(.028)		(.033)
Hispanic	.889	.679	.21***	.717	.177***
			(.037)		(.04)
White	.015	.093	-.078***	.066	-.049***
			(.017)		(.015)
English Learner	.221	.157	.064***	.173	.041*
			(.02)		(.023)
Female	.496	.505	-.009	.504	-.007
			(.013)		(.014)
Migrant	.211	.193	.018	.191	.018
			(.016)		(.017)
Spanish at home	.773	.581	.191***	.623	.151***
			(.04)		(.043)
Poverty	.788	.690	.098***	.714	.069*
			(.034)		(.036)
Parents College +	.056	.105	-.049***	.082	-.024**
			(.012)		(.01)
Schools	38	48		38	

Notes: This table reports school-level mean attributes of ZOC and non-ZOC schools. Columns (1) and (2) report group means corresponding to row variables. Column (3) reports the difference between Column (1) and Column (2) and reports a standard error in parentheses below the mean difference. All standard errors are robust.

A.2 Enrollment Trends in Los Angeles

LAUSD, like other large urban school districts, has suffered from enrollment decline over the past two decades. Appendix Figure A.1 reports high school enrollment over time, showing a peak in 2004 and a steady decline since. Across the entire district, enrollment has decreased by roughly 37 percent from the peak in 2004. Average test scores between ZOC and non-ZOC high schools are noticeably trending similarly leading in the years leading to the program expansion.

Figure A.1: Los Angeles Unified School District: 2002–2013

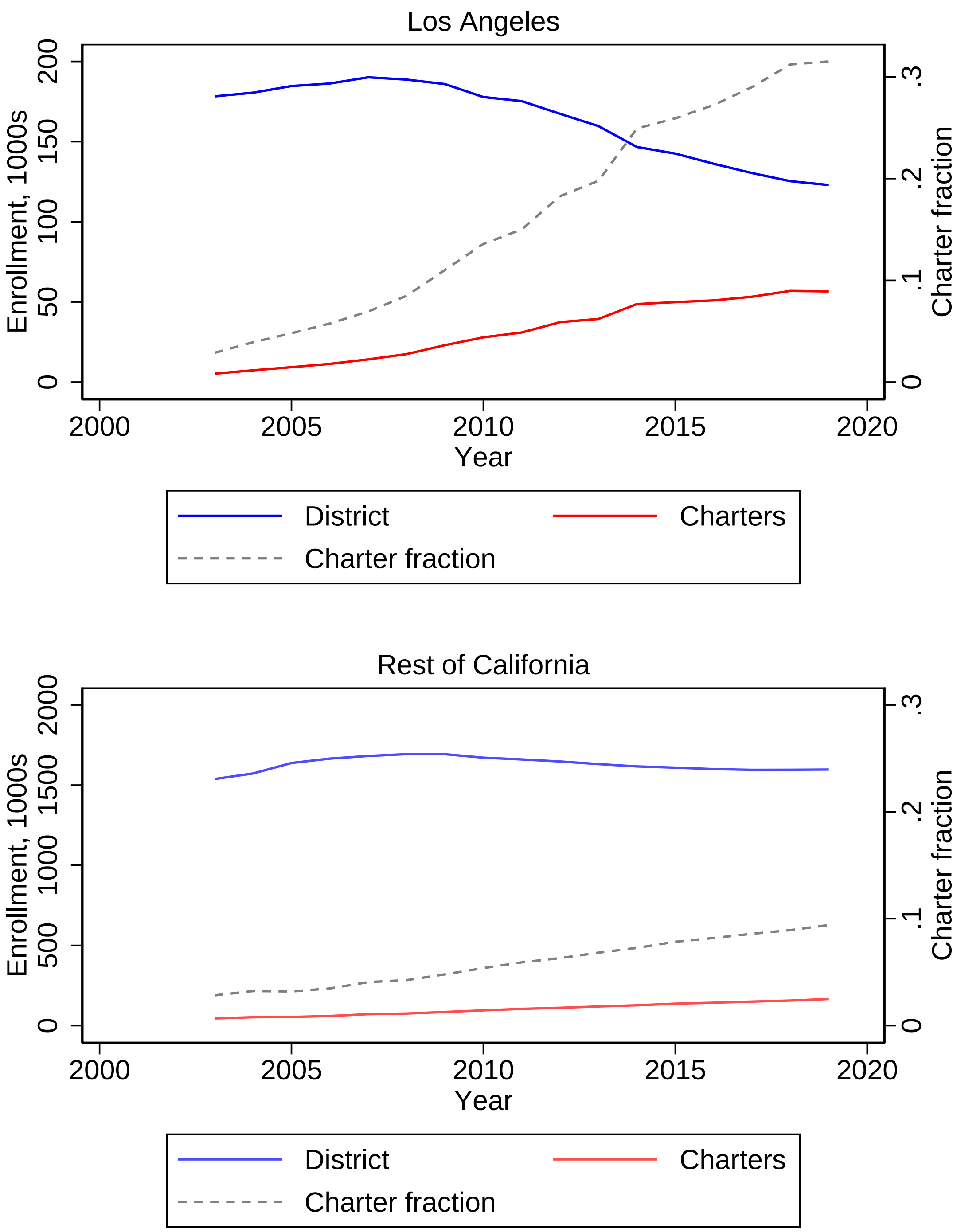


Notes: Enrollment numbers come from administrative data provided by Los Angeles Unified School District (LAUSD). The California Department of Education provides California Standards Test statewide means and standard deviations, which we use to standardize test scores in this figure. Test scores are ninth-grade scores on the ELA exam, which is uniform across schools and students.

Appendix Figure A.2 zooms out and compares charter enrollment trends in Los Angeles to those in the rest of the state. Two patterns stand out that are worth discussing. The enrollment decline has disproportionately affected Los Angeles, which is partly due to a coinciding rise in charter enrollment. The charter share of enrollment increased from less than 5 percent in 2004 to roughly 30 percent in 2019, while enrollment increased from just below 10,000 students to approximately 50,000 students. These trends are less pronounced for the rest of the state, although we do observe a more modest increase in the charter market share in the rest of the state.

The observations in the previous figure immediately introduce concerns that our findings are driven by charter competition as opposed to ZOC competition. Appendix E.3 addresses these concerns. We do not find evidence of differential changes in charter enrollment along both intensive and extensive margins between ZOC and non-ZOC neighborhoods, which assuages concerns that charter competition explains our findings.

Figure A.2: Los Angeles and California Enrollment



Notes: This figure shows enrollment in thousands for grades 9 through 12, separately for district and charter schools. Enrollment data are from the California Department of Education.

A.3 Potential Impact of the Change to the SBAC

Changing CST and SBAC distributions is an additional factor to consider in the ZOC difference-in-difference estimates. One way to look at how this change potentially impacts these estimates is to decompose the change into two components, one that holds the distribution fixed and a second that is attributable to the changing distribution.

Let $\bar{Y}_t^g$ correspond to group g mean test scores in year t, μ_t correspond to the district grade-year mean test score in year t, and σ_t correspond to the district grade-year standard deviation in year t. The change in mean standardized mean achievement for group g is

$$\Delta \bar{Y}^g = \frac{1}{\sigma_0}\Big((\bar{Y}_1^g - \mu_1) - (\bar{Y}_0^g - \mu_0)\Big) + \Big(\frac{1}{\sigma_1} - \frac{1}{\sigma_0}\Big)(\bar{Y}_1^g - \mu_1),$$

where the second component captures a component driven by the changing distribution (i.e., the change in σ).

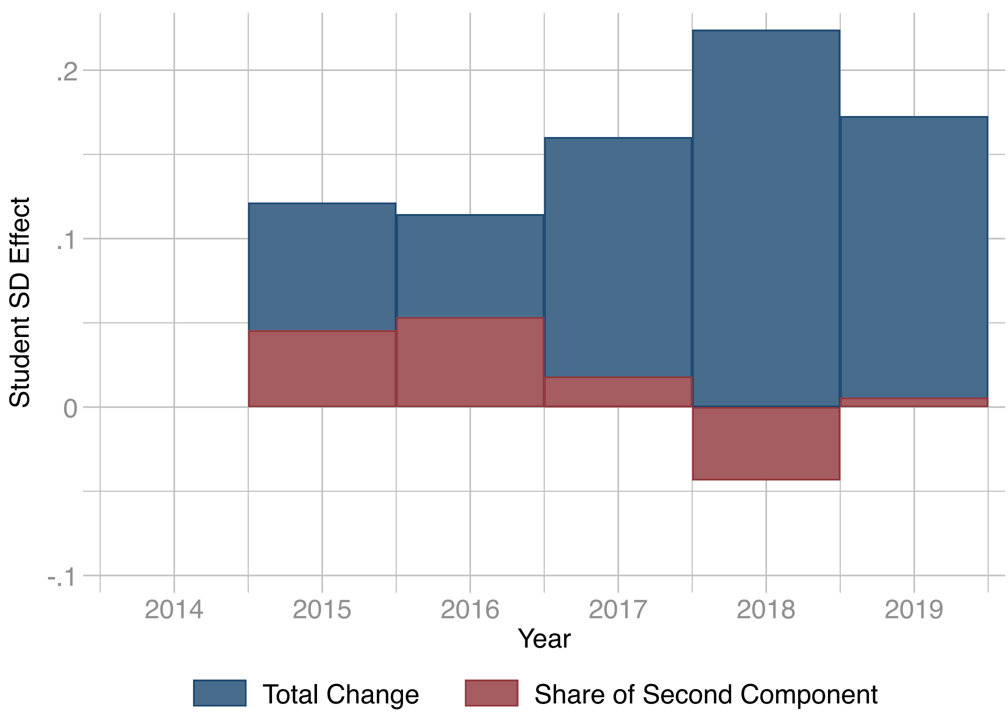
This implies that the difference-in-differences estimates are:

$$\Delta \bar{Y}^z - \Delta \bar{Y}^n = \underbrace{\frac{1}{\sigma_0}\Big((\bar{Y}_1^z - \bar{Y}_0^z) - (\bar{Y}_1^n - \bar{Y}_0^n)\Big)}_{\Delta \ holding \ \sigma \ fixed} + \underbrace{\Big(\frac{1}{\sigma_1} - \frac{1}{\sigma_0}\Big)}_{\Delta \ in \ \sigma}(\bar{Y}_1^z - \bar{Y}_1^n).$$

The equation above shows that the difference-in-differences estimate will be inflated if $\sigma_0 > \sigma_1$. In other words, if the distribution compresses, then any mean differences are amplified and vice versa.

We report raw difference-in-difference estimates for the affected years in Appendix Figure A.3. Overall, the change in the score dispersion seems to have minimally affected difference-in-difference estimates as we move forward in time. This reduces the concern about the overall influence of the changing score distribution driving our results.

Figure A.3: Influence of the Changing Score Distribution



Notes: This figure reports estimated difference-in-difference decomposition estimates. The maroon component is the portion of the change attributable to distributional inflation factor. The navy bars correspond to the overall effect.

B A Model of School Choice and School Quality

B.1 Proofs

It is useful to define some notation and the pre-ZOC equilibrium before proceeding. The first-order conditions require that each principal j sets their effort according to

$$f'(e_j) = \frac{1}{\theta\omega\frac{1}{N}\sum_i P_{ij}(e_j; d_{ij}, X_i)(1 - P_{ij}(e_j; , d_{ij}, X_i))}.$$

Define the right-hand side as

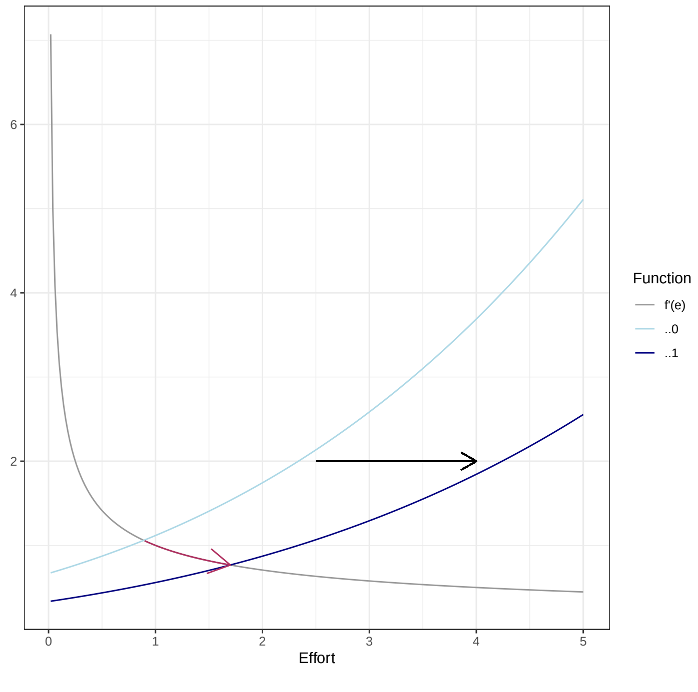
$$\Phi(e_j) = \frac{1}{\theta\omega\frac{1}{N}\sum_i P_{ij}(e_j; d_{ij}, X_i)(1 - P_{ij}(e_j; , d_{ij}, X_i))},$$

and let $\Phi(e_j, e_{-j})$ correspond to the strategic analog of $\Phi(e_j)$ that depends on other principals' effort levels. An equilibrium in both the pre-ZOC and post-ZOC regimes will be governed by the intersection of Φ and f'. Appendix Figure B.1 depicts this visually.

The transition from a pre-ZOC equilibrium to a post-ZOC equilibrium for a given school j is governed by shifts in Φ, with downward (or rightward) shifts of Φ leading to an increase in equilibrium effort levels. Strategic interactions complicate this intuition because principals' best responses lead to further shifts in Φ, and potential upward shifts lead to ambiguous effort levels relative to the pre-ZOC equilibrium.

Proposition B.1 shows that there is a Nash equilibrium in the principal effort game. Proposition B.2 shows that provided schools are operating as functional neighborhood monopolies before ZOC and the quality elasticity of demand increases sufficiently, principals exert more effort after competition is introduced. Strategic complementarities play a role in ensuring the post-ZOC equilibrium levels are strictly greater than the pre-ZOC equilibrium effort levels for all schools $j \in \mathcal{J}$. Last, Proposition B.3 provides a comparative static result indicating that an increase in OVG from an equilibrium would lead to further increases in effort. This last proof again relies on the intuition gained from shifts in Φ.

Figure B.1: Change in Equilibrium



Proposition B.1 (Proposition 1)**.** *Let $e^{BR}(e^*) = e^*$ denote the following vector-valued function:*

$$e^{BR}(e) = \Big(e_1(e_{-1}, e)^{BR}, \cdots, e_J(e_{-J}, e)^{BR}\Big).$$

There exists an $e^* \in [\underline{e}, \bar{e}]^J$ such that $e^{BR}(e^*) = e^*$. There also exists an equilibrium to the principal effort game.

Proof. The existence of equilibria follows from the fact that the principal effort game is a game with strategic complementarities and thus both maximum and minimum equilibria exist (Vives, 1990, 2005). Strategic complementarities follow from showing that the marginal payoff of principal j is increasing in the effort of another principal $k \neq j$:

$$\begin{aligned}\frac{\partial^2 u_j}{\partial e_j \partial e_k} &= \theta g'(\alpha_j)\Big(\sum_i P_{ij}(e_j, e_{-j}) P_{ik}(e_j, e_{-j}))g'(\alpha_k) f'(e_k)\Big) \\ &> 0.\end{aligned}$$

□

Proposition B.2. *If each school j has at least 50 percent market share before the ZOC expansion and the post-ZOC quality elasticity of demand for each student i for school j satisfies $\eta^1_{ij} > \frac{P^0_{ij}}{P^1_{ij}}\eta^0_{ij}$, then for each $j \in \mathcal{J}$, the change in principal effort is*

$$\Delta e_j = e_j^{BR}(e_{-j}, e) - e_{j0} > 0$$

and for each $j \in \mathcal{J}^c$, the change in principal effort is

$$\Delta e_j = 0.$$

Proof. Figure B.1 shows that each principal's optimal level of effort is determined at the point at which Ψ and f' intersect. Therefore, principal j finds it optimal to increase their effort if their curve Φ shifts downward.

The heuristic proof proceeds in two steps. First, we show that introducing competition implies a downward shift in Φ, which leads to an increase in effort in a nonstrategic setting in which principals independently maximize their utility (ignoring the actions of others). Then we show that the anticipated increases in effort from other principals lead to further downward shifts in Φ, implying an equilibrium in which each school j increases its effort.

Let e_{j0} denote school j's pre-ZOC effort level with corresponding

$$\Phi(e_{j0}) = \frac{1}{\theta g'(\alpha_j)\frac{1}{N_j}\sum_{i:j(i)=j} P_{ij}(e_{j0}; g'(\alpha_j), \mu_j, d_{ij}, X_i)(1 - P_{ij}(e_{j0}; \omega, \mu_j, d_{ij}, X_i))}.$$

The introduction of ZOC introduces additional students and a principal effort game, changing Φ to

$$\Phi(e_{j0}, e_{-j}) = \frac{1}{\theta g'(\alpha_j)\frac{1}{N}\sum_{i\in\mathcal{J}} P_{ij}(e_{j0}, e_{-j}; g'(\alpha_j), \mu_j, d_{ij}, X_i)(1 - P_{ij}(e_{j0}, e_{-j}; \omega, \mu_j, d_{ij}, X_i))}.$$

Therefore, the first step shows that $\Phi(e_{j0}) > \Phi(e_{j0}, e_{-j})$, which is equivalent to showing

$$\begin{aligned}\frac{1}{\Phi_1(e_{j0}, e_{-j})} - \frac{1}{\Phi(e_{j0})} &= \theta\tilde{S}^1_j(e_{j0}, e_{-j}) - \theta\tilde{S}^0_j(e_{j0}) \\ &= \theta\Big(\frac{1}{N}\sum_{i\in\mathcal{J}} P^1_{ij}(1 - P^1_{ij})g'(\alpha_j) - \frac{1}{N_j}\sum_{i:j(i)=j} P^0_{ij}(1 - P^0_{ij})g'(\alpha_j)\Big)\end{aligned}$$

$$
\begin{aligned}
&= \theta\Big(\frac{1}{N}\sum_{i\in\mathcal{J}} P^1_{ij}\eta^1_{ij} - \frac{1}{N_j}\sum_{i:j(i)=j} P^0_{ij}\eta^0_{ij}\Big) \\
&> \theta\Big(\frac{1}{N}\sum_{i\in\mathcal{J}} P^1_{ij}\frac{P^0_{ij}\eta^0_{ij}}{P^1_{ij}} - \frac{1}{N_j}\sum_{i:j(i)=j} P^0_{ij}\eta^0_{ij}\Big) \\
&= \frac{1}{N_j}\sum_{i:j(i)\neq j} P^0_{ij}\eta^0_{ij} \\
&> 0.
\end{aligned}
$$

This shows that the nonstrategic response would be to increase effort for each principal j. The effort game, however, makes it so that principals take into account other principals' responses. Starting from $\Phi_1(e_{j0}, e_{-j})$, increases in effort from principals $j' \neq j$ would lead to further downward shifts in Φ, all else constant:

$$
\begin{aligned}
\frac{\partial\Phi(e_j, e_{-j})}{\partial e_{j'}} &= -\frac{1}{\tilde{S}^1_j(e_j, e_{-j})^2}\theta g'(\alpha_j)\Big(\frac{1}{N}\sum_{i\in\mathcal{J}}\frac{-\partial P_{ij}}{\partial e_{j'}}\Big) \\
&= -\frac{1}{\tilde{S}^1_j(e_j, e_{-j})^2}\theta g'(\alpha_j)\Big(\frac{1}{N}\sum_{i\in\mathcal{J}} P_{ij}P_{ij'}g'(\alpha_j)\Big) \\
&< 0.
\end{aligned}
$$

Alternatively, the strategic complementarities in effort also would point to similar dynamics. Therefore, combining strategic complementarities with the fact that schools exert strictly more effort because of downward shifts in Φ allows us to sign the change in effort for each school j. Therefore, provided schools commence the game operating as neighborhood monopolies with high market shares and households' quality elasticity of demand is sufficiently high after the ZOC rollout, the resulting best response for school j results in the intersection of $\Phi_j(e^{BR}_j(e_{-j}, e), e_{-j})$ and $f'(e^{BR}_j(e_{-j}, e))$, where $e^{BR}_j > e_{j0}$.

□

Proposition B.3. *Effort e^{BR}_j is increasing in OVG for each school j.*

Proof. Let $\boldsymbol{OVG} = (OVG_1, \cdots, OVG_N)$ be a vector of student-level OVG. Suppose we depart from equilibrium e^*. For a given school j, we have

$$
\frac{\partial\Phi(e^{BR}_j, e^{BR}_{-j})}{\partial OVG_i} = \frac{-\theta g'(\alpha_j)\lambda P_{ij}P_{-ik}}{\Big(\theta g'(\alpha_j)\frac{1}{N}\sum_i P_{ij}(e^{BR}_j, e^{BR}_{-j}; d_{ij}, X_i)(1 - P_{ij}(e^{BR}_j, e^{BR}_{-j};, d_{ij}, X_i)\Big)^2}.
$$

Therefore, for a marginal increase in $\boldsymbol{OVG}$, Φ shifts further downward, leading to increases in effort, and the strategic complementarities in Proposition B.2 imply a new equilibrium in which schools all exert more effort.

Alternatively, increases in OVG can be seen as increases in an exogenous parameter t, and the best response dynamics induced by strategic complementarities imply weakly larger effort levels (Echenique, 2002; Vives, 2005). □

C Achievement Model and Validation

C.1 A Model of Student Achievement

In this section, we define our notion of school quality and introduce parameters that define our measure of student-school match quality. Measures of school quality are useful in our analysis for several reasons.

We consider a generalized value-added model that allows for student-school match effects (Abdulkadiroğlu et al., 2020). Students indexed by i attend one school from a menu of schools $j \in J$. A projection of potential achievement A_{ij} on student characteristics $\boldsymbol{X}_i$ and school effects α_j yields[1]

$$A_{ij} = \alpha_j + \boldsymbol{X}_i'\beta_j + u_{ij}, \tag{1}$$

where u_{ij} has a mean of zero and is uncorrelated with $\boldsymbol{X}_i$ by construction. The vector of student characteristics $\boldsymbol{X}_i$ is normalized $E[\boldsymbol{X}_i] = 0$ so that $E[A_{ij}] = \alpha_j$ is the average achievement at school j for the district's average student. The vector β_j measures the school-j-specific return to student i's characteristics $\boldsymbol{X}_i$ and introduces the scope for match effects. As in Abdulkadiroğlu et al. (2020), we can denote the ability of student i as student i's average achievement across schools j:

$$a_i = \bar{\alpha} + \boldsymbol{X}_i'\bar{\beta} + \bar{u}_i.$$

Adding and subtracting a_i from Equation 1 allows us to express the potential achievement of student i at school j as the product of three factors: ability, the relative effectiveness of school j, and student-school match quality M_{ij}. Therefore, potential outcomes can be written as follows:

$$A_{ij} = a_i + \underbrace{(\alpha_j - \bar{\alpha})}_{ATE_j} + \underbrace{\boldsymbol{X}_i'(\beta_j - \bar{\beta}) + (u_{ij} - \bar{u}_i)}_{M_{ij}}.$$

Student ability a_i is invariant to the school a student attends, ATE_j is school j's causal effect on achievement relative to the average school, and M_{ij} captures j's suitability for student i. A positive M_{ij} could arise if a student sorts into schools based on returns to their particular attributes as captured by $\boldsymbol{X}_i'(\beta_j - \bar{\beta})$ or unobserved factors $(u_{ij} - \bar{u}_i)$ that make student i suitable for school j.[2] Appendix C.3 reports achievement model estimates.

C.2 Value-Added Model Estimation and Bias Tests

The decomposition exercise requires estimates of α_{jt} and β_j and, as a consequence, requires an additional assumption. We rely on a selection-on-observables assumption to obtain unbiased estimates of β_j and α_{jt}:

$$E[A_{ij}|X_i, j(i) = j] = \alpha_j + \boldsymbol{X}_i'\beta_j; \quad j = 1, \cdots, J. \tag{2}$$

This assumes that assignments to schools are as good as random, conditional on $\boldsymbol{X}_i$. The vector of covariates $\boldsymbol{X}_i$ includes race, sex, poverty indicators, migrant indicators, English learner status, and lagged test scores, with lagged test scores being sufficiently rich in some settings to generate α_{jt} estimates with decent average predictive validity or minimal forecast bias (Chetty, Friedman and Rockoff, 2014; Deming, 2014). Under this assumption, we can obtain unbiased estimates of α_{jt} and γ_j using OLS regressions of achievement on school-by-year enrollment indicators and

[1] We suppress time indices for notational ease.

[2] For example, variation in the poverty gap across school j introduces the scope for poor students to sort into schools in which they perform better, introducing potential gains on that margin. In contrast, some schools may be suitable for some students for idiosyncratic reasons, captured by u_{ij}, thus introducing gains in unobserved match effects.

student covariates discussed above interacted with time-invariant school enrollment indicators. Nonetheless, selection on observables is a strong assumption, and value-added estimates with good average predictive validity are still potentially subject to bias (Rothstein, 2017).

We use the procedure outlined by Angrist et al. (2017) to test for bias in the VAM estimates. We can construct predictions using the value-added model we estimate, which we denote as $\widehat{A}_i$. To test for bias, we treat $\widehat{A}_i$ as an endogenous variable in a two-stage least squares framework using L lottery offer dummies $Z_{i\ell}$ that we collect across zones and cohorts:

$$A_i = \xi + \phi \widehat{A}_i + \sum_{\ell} \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i' \delta + \varepsilon_i \tag{3}$$

$$\widehat{A}_i = \psi + \sum_{\ell} \pi_\ell Z_{i\ell} + \boldsymbol{X}_i' \xi + e_i. \tag{4}$$

If lotteries shift VAM predictions in proportion to the shift of realized test scores A_i, on average, then $\phi = 1$, which is a test of forecast bias (Chetty, Friedman and Rockoff, 2014; Deming, 2014). The overidentifying restrictions further allow us to test whether this applies to each lottery and thus to test the predictive validity of each lottery.

Table C.1 reports results for three value-added models. Column 1 reports results for a model that omits any additional covariates beyond school-by-year dummies; this is the uncontrolled model. As discussed in Deming (2014), Chetty, Friedman and Rockoff (2014), and Angrist et al. (2017), models that do not adjust for lagged achievement tend to perform poorly in their average predictive validity. Indeed, we find the forecast coefficient to be 0.63, indicating that the uncontrolled model does not pass the first test. Column 2 reports estimates from a constant effects VAM specification where $\alpha_{jt} = \alpha_j$. The constant effects model represents the scenario in which school effectiveness does not adjust in response to the program. While we cannot formally reject that the model is forecast unbiased, the forecast coefficient is rather large at 1.11, pointing to the constant effects model's poor average predictive validity.

In Column 3, we report results for our preferred model outlined in Equation 1. The forecast coefficient is essentially 1, and the p-value on the overidentification test fails to reject the null. One remaining concern is many weak instrument bias, which would bias the forecast coefficient on the corresponding OLS estimates. The first-stage F-statistic is roughly 17.8, passing the rule-of-thumb test that has come under recent scrutiny for just-identified single IV models (Lee et al., 2021). While the results in Table C.1 do not entirely rule out bias in OLS value-added estimates, they are reassuring.

Table C.1: Forecast Bias and Overidentification Tests: 2013–2017 Cohorts

	(1) Uncontrolled	(2) Constant Effect	(3) Preferred
Forecast Coefficient	.63	1.111	1.024
	(.105)	(.134)	(.112)
	[0]	[.41]	[.830]
First-Stage F	277.507	37.016	17.8
Bias Tests:			
Forecast Bias (1 d.f.)	12.528	.683	.046
	[0]	[.409]	[.831]
Overidentification (180 d.f)	172.281	187.744	176.74
	[.647]	[.331]	[.555]

Notes: This table reports the results of lottery-based tests for bias in estimates of school effectiveness. The sample is restricted to students in the baseline sample who applied to an oversubscribed school within a school choice zone. Column (1) measures school effectiveness as the school mean outcome, Column (2) uses time-invariant value-added estimates, and Column (3) uses time-varying and heterogeneous value-added estimates from Equation 1. The forecast coefficients and overidentification tests reported in Columns (1)–(3) come from two-stage least squares regressions of test scores on OLS-fitted values estimated separately, instrumenting OLS-fitted values with school-cohort-specific lottery offer indicators, controlling for baseline characteristics.

C.3 Achievement Model Estimates

Table C.2 reports summary statistics for the school-specific returns β_j. We find substantial heterogeneity in these returns. While we find substantial heterogeneity in the estimates across schools, we do not find meaningful mean differences between ZOC and non-ZOC schools for most β_j. It is plausible that the β_j also changed in response to the policy, so we estimate a version of the model where β_j are different in the pre- and post-periods. Appendix Table C.3 reports the estimates, but we do not find evidence that there were meaningful changes induced by the policy for most characteristics.

Table C.2: Summary Statistics for School-Specific Returns to Student Characteristics and School Effectiveness

	ZOC		Non-ZOC		
	Mean	SD	Mean	SD	Difference
	(1)	(2)	(3)	(4)	(5)
Female	.058	.041	.032	.069	.026***
	(.041)	(.005)	(.006)	(.014)	(.008)
Black	-.146	.288	-.098	.191	-.048
	(.288)	(.045)	(.017)	(.017)	(.042)
Hispanic	-.053	.165	-.048	.152	-.005
	(.165)	(.022)	(.013)	(.014)	(.026)
English learner	-.44	.135	-.229	.23	-.211***
	(.135)	(.016)	(.02)	(.015)	(.027)
Poverty	.008	.066	.009	.122	-.001
	(.066)	(.01)	(.011)	(.032)	(.014)
Migrant	-.03	.069	-.001	.076	-.029**
	(.069)	(.007)	(.007)	(.01)	(.011)
Parents College +	.02	.131	.016	.105	.004
	(.131)	(.021)	(.009)	(.008)	(.02)
Spanish spoken at home	.073	.074	.013	.081	.059***
	(.074)	(.009)	(.007)	(.007)	(.012)
Lagged ELA Scores	.48	.052	.348	.169	.132***
	(.052)	(.005)	(.015)	(.013)	(.016)
Lagged Math Scores	.107	.04	.064	.082	.042***
	(.04)	(.004)	(.007)	(.009)	(.009)
8th Grade Suspensions	.009	.045	-.002	.041	.011
	(.045)	(.007)	(.004)	(.005)	(.007)
Value-Added	.068	.160	-0.023	0.238	0.082***
					(.008)

Notes: This table reports estimated means and standard deviations of school-specific returns β_j. The bottom row reports mean and standard deviation estimates of school effectiveness. Estimates come from OLS regressions of ELA scores on school by year indicators and interactions of school indicators with sex, race, poverty, parental education, indicators for living in a Spanish-speaking home, migrant indicators, middle school suspensions, and eighth-grade ELA and math scores. Columns (1) and (2) show Zones of Choice (ZOC) school estimates and Columns (3) and (4) show other Los Angeles Unified School District high school estimates; Column (5) reports their difference. Robust errors are reported in parentheses.

Table C.3: Summary Statistics of Time-Varying Match Effects

	Before					Change		
	ZOC		Non-ZOC			ZOC	Non-ZOC	
	Mean	SD	Mean	SD	Difference	Mean	Mean	Diff-in-Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.041	0.052	0.040	0.075	0.001	0.053	0.037	0.016
					(0.011)			(0.018)
Black	-0.216	0.246	-0.224	0.434	0.008	0.017	0.044	-0.027
					(0.057)			(0.061)
Hispanic	-0.191	0.261	-0.171	0.316	-0.020	0.116	0.097	0.019
					(0.049)			(0.049)
English Learner	-0.458	0.122	-0.422	0.210	-0.036	-0.368	-0.170	-0.198***
					(0.028)			(0.038)
Poverty	0.061	0.109	0.040	0.105	0.021	-0.040	-0.038	-0.002
					(0.019)			(0.020)
Migrant	0.015	0.064	-0.006	0.115	0.021	-0.026	0.014	-0.040**
					(0.015)			(0.017)
Parents College +	0.012	0.155	-0.009	0.161	0.022	0.019	0.059	-0.040
					(0.028)			(0.037)
Spanish Spoken at Home	0.071	0.056	0.036	0.051	0.035***	-0.008	-0.001	-0.007
					(0.010)			(0.011)
Lagged ELA Scores	0.632	0.101	0.601	0.140	0.031	-0.012	-0.038	0.026
					(0.020)			(0.028)
Lagged Math Scores	0.118	0.061	0.112	0.072	0.006	0.019	0.008	0.010
					(0.011)			(0.016)
8th-Grade Suspensions	-0.035	0.027	-0.038	0.035	0.003	-0.028	-0.016	-0.012
					(0.005)			(0.008)

Notes: This table reports estimated means and standard deviations of school-specific returns β_j that are allowed to be different in the pre- and post-period. Estimates come from OLS regressions of ELA scores on school-by-year indicators and interactions of school indicators with sex, race, poverty, parental education, indicators for living in a Spanish-speaking home, migrant indicators, middle school suspensions, and eighth-grade ELA and math scores, interacted with pre and post indicators. Columns (1) and (2) show ZOC school estimates, and Columns (3) and (4) show other Los Angeles Unified School District high school estimates. Column (5) reports their difference. Column 6 and Column 7 report mean changes in the estimated β_j for ZOC and non-ZOC schools separately. Column 8 reports the difference-in-difference estimate. Standard errors are reported in parentheses.

D Heterogeneity

Panel B of Table D.1 reports heterogeneity estimates, estimating the baseline model restricted to different samples. Heterogeneity by race is noisily estimated for Black and White students; in some zones, such as Boyle Heights, we find a total of 30 Black students and 35 White students, compared to roughly 8,000 Hispanic students, across the entire sample period. These limitations make it challenging to truly assess racial differences in treatment effects, with the resulting estimates containing large confidence intervals.

Taking the estimates at face value, however, suggests that White and Black students did not experience similar achievement gains as their Hispanic counterparts. Heterogeneity by sex suggests that both male and female students equally benefited from the ZOC expansion. Heterogeneity by socioeconomic status reveals that most gains came from students the district classified as poor, with negligible but noisily estimates for non-poor students. Students classified as English learners also do not appear to have experienced sizable treatment effects. To summarize the heterogeneity evidence, most treatment effects are concentrated among lower socioeconomic status Hispanic students, many of whom also had low incoming achievement.

Table D.1: Difference-in-Differences Estimates

	(1) N	(2) Pre-ZOC	(3) Post ZOC 0-2	(4) Post-ZOC 3-6
		Panel A: Achievement Decomposition		
Achievement	221,569	0.000	0.036	0.135
		(0.035)	(0.039)	(0.057)
ATE	221,569	-0.010	0.022	0.092
		(0.023)	(0.029)	(0.043)
Match Effect	221,569	0.002	0.003	0.009
		(0.004)	(0.003)	(0.005)
		Panel B: Heterogeneity		
White	11,812	-0.017	-0.002	-0.023
		(0.069)	(0.129)	(0.147)
Hispanic	173,489	0.018	0.046	0.164
		(0.037)	(0.037)	(0.054)
Black	19,740	-0.079	-0.108	-0.047
		(0.084)	(0.100)	(0.138)
Female	113,427	0.020	0.024	0.136
		(0.034)	(0.037)	(0.056)
Poverty	172,661	0.007	0.040	0.154
		(0.034)	(0.038)	(0.057)
No Poverty	48,908	-0.021	0.012	0.024
		(0.062)	(0.059)	(0.080)
English Learner	28,459	-0.011	0.013	0.030
		(0.033)	(0.035)	(0.043)

Notes: This table reports difference-in-difference estimates for a variety of models and samples. Each model is a regression of the row variables on event-time indicators, school indicators, and ZOC indicators interacted with pre- and post-period indicators. The omitted year is the year before the ZOC expansion. The columns report corresponding pre- and post-period changes relative to the omitted year. Panel A uses the entire sample and reports decomposition estimates. The "Achievement" corresponds to the baseline specification, "ATE" corresponds to treatment effects on enrolled school quality, and "Match Effect" corresponds to student-school match quality. Panel B considers different samples to assess heterogeneity by subgroups. Standard errors are reported in parentheses and are robust and clustered at the school level.

D.1 Distributional Effects

While mean impacts are informative, distributional impacts shed light on treatment effect heterogeneity that is based on students' incoming achievement levels. One may be concerned the improvements found in the previous section are concentrated among high achievers or that the gains of some students come at the expense of others. For college outcomes, it is plausible that ZOC nudges more marginal students into college but does not affect students whose college enrollment propensities are low. In this section, we study distributional treatment effect heterogeneity to explore these possibilities.

To study heterogeneity in the achievement treatment effect, we modify the baseline empirical strategy and estimate the following difference-in-differences models:

$$\mathbf{1}\{A_i \leq a\} = \mu_{j(i)} + \mu_{t(i)} + \gamma_a PreZOC_{it} + \beta_a PostZOC_{it} + \boldsymbol{X}_i'\psi + u_i. \quad (5)$$

Here, β_a is the distributional effect at a, and γ_a are analogous but for pre-period effects, both relative to the year before the policy intervention. Specifically, β_a measures the effect of ZOC on the probability that student achievement is less than a, and differences in β_a inform us about heterogeneous impacts across the distribution of student achievement. Estimates of γ_a point to evidence concerning pre-intervention differential trends across the entire student achievement distribution.

Figure D.1 reports the distributional estimates. We find that most of the improvements—indicated by negative treatment effects at different distribution points—occur in the bottom half of the distribution and that estimates at the top are centered around zero. These results suggest most of the treatment effects are concentrated among low-achieving students and that these benefits do not come at the expense of high-achieving students. Importantly, we do not find evidence of any pre-intervention distributional effects pointing to additional evidence in support of the parallel trends assumption across the entire achievement distribution.

The dichotomous nature of college enrollment outcomes complicates the distributional analysis. To overcome this problem, we approach the analysis in two steps. First, among students in the pre-period, we predict four-year college enrollment using a logit LASSO for variable selection.[3] Using the estimated parameters from the model, we predict every student's probability of four-year college enrollment and group students into quartile groups. We then estimate quartile-group-specific event-study models. This approach estimates heterogeneous treatment effects on four-year college enrollment based on students' likelihood of enrolling in college as predicted by their observable characteristics.

Figure D.3 shows that treatment effects are not just concentrated among students who are more likely to enroll in college, and, as with the previous results, the treatment effects are larger as exposure to the program increases for later cohorts. Although the treatment effects for students in the top two quartile groups are larger in magnitude, the treatment effects for students in the bottom two quartile groups represent a roughly 40 percent increase from the baseline mean as compared with a roughly 20 percent increase for students in the top two quartile groups.[4]

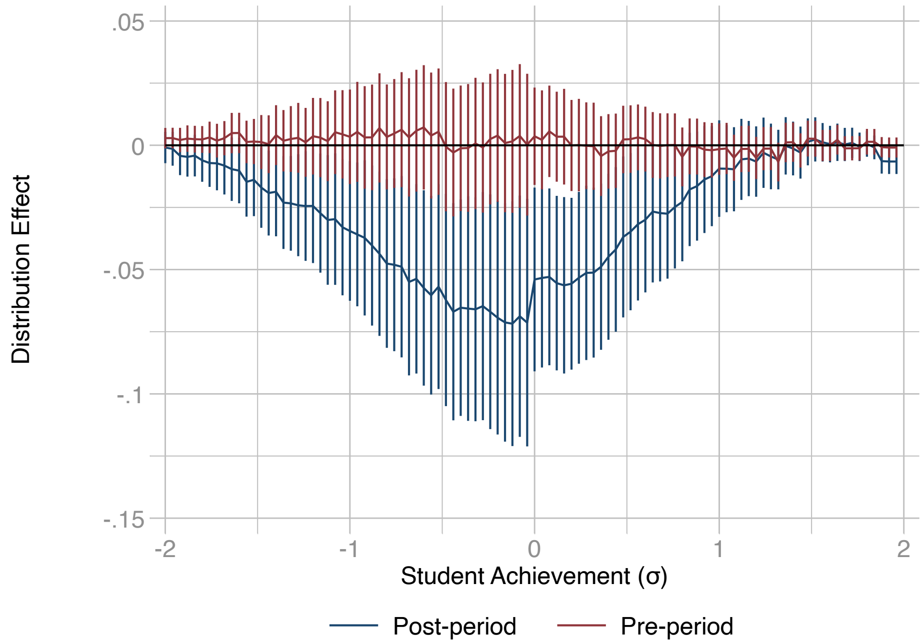
The heterogeneous impacts on achievement and college enrollment raise a few points worth emphasizing. First, ZOC was effective at increasing achievement among students who would have otherwise performed poorly, and those gains do not come at the expense of high-achieving students. Moreover, for students who would have otherwise performed poorly in the absence of the program, there is also suggestive evidence that they also increased their educational attainment as captured by high school graduation (see Appendix Figure E.3). In contrast, for students with higher levels of incoming achievement, ZOC was much more limited in improving their learning but did improve their four-year college enrollment chances that were not just diversions from two- to four-year colleges (see Appendix Figure E.2). Overall, students' margins of improvement varied, with the initially low performers experiencing higher test score improvements and those on the college enrollment margin benefiting along that dimension.

[3] Variables in the model include all variables in Table 1 and their interactions. We use all pre-period years starting in 2008 and ending in 2012.

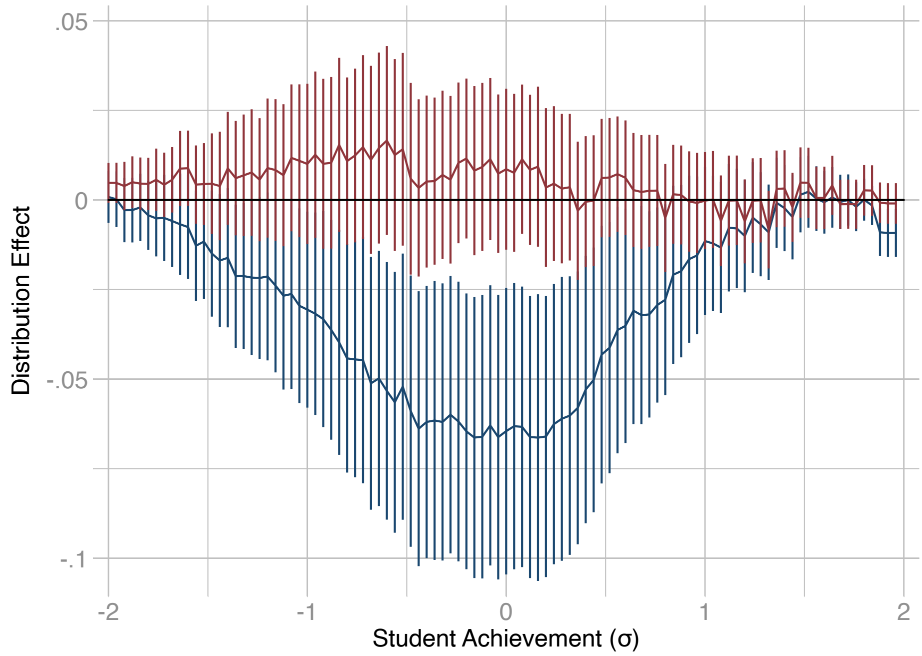
[4] Appendix Figure D.2 reports trends by different quartile groups.

Figure D.1: Student Achievement Distributional Impacts

(a) Reading

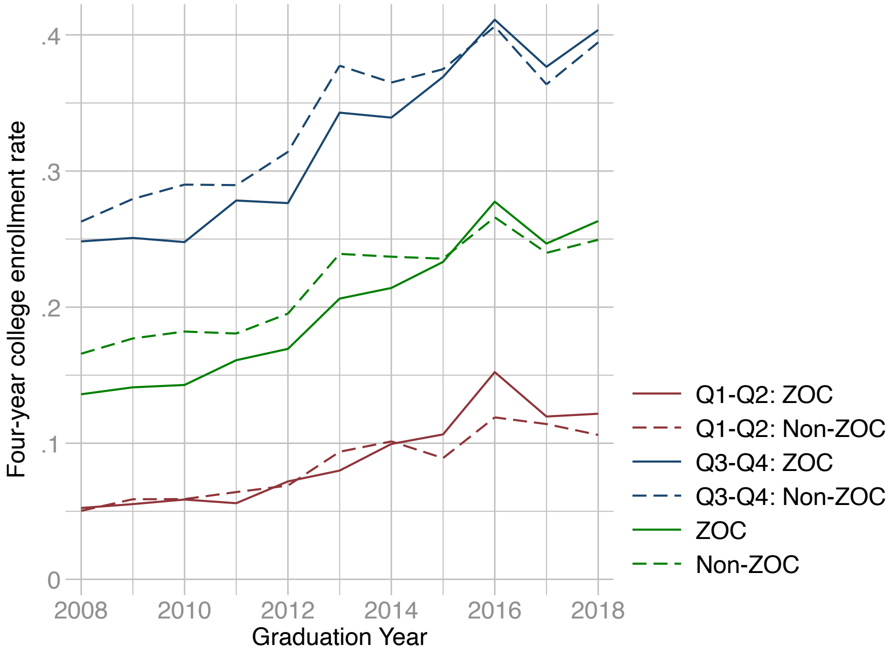


(b) Math



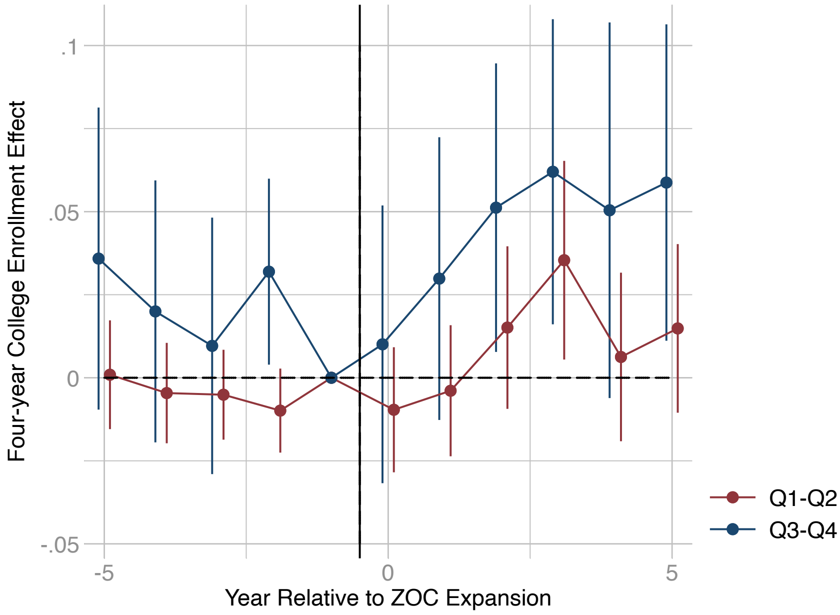
Notes: This figure reports estimates of β_a and γ_a from Equation 5 for 100 equally distanced points between –2 and 2. The blue lines and bars correspond to β_a, difference-in-differences estimates on the probability of students scoring below a on their student achievement exams. Similarly, the red lines and bars correspond to γ_a, difference-in-difference estimates in the pre-period. Standard errors are clustered at the school level, and 95 percent confidence regions are displayed by bars around the point estimates.

Figure D.2: Four-Year College Enrollment Rates by Predicted Quartile Group



Notes: This figure reports college enrollment rates for students in different quartile groups by ZOC and non-ZOC student status. Solid lines correspond to ZOC students, and dashed lines correspond to non-ZOC students. Red lines correspond to students in the bottom two quartiles of the predicted college enrollment probability distribution, and blue lines are defined similarly for the top two quartiles. Predicted probabilities are generated from logit models where a LASSO procedure is used to determine covariates for prediction purposes.

Figure D.3: Four-Year College Enrollment Effects by Predicted Quartile Groups



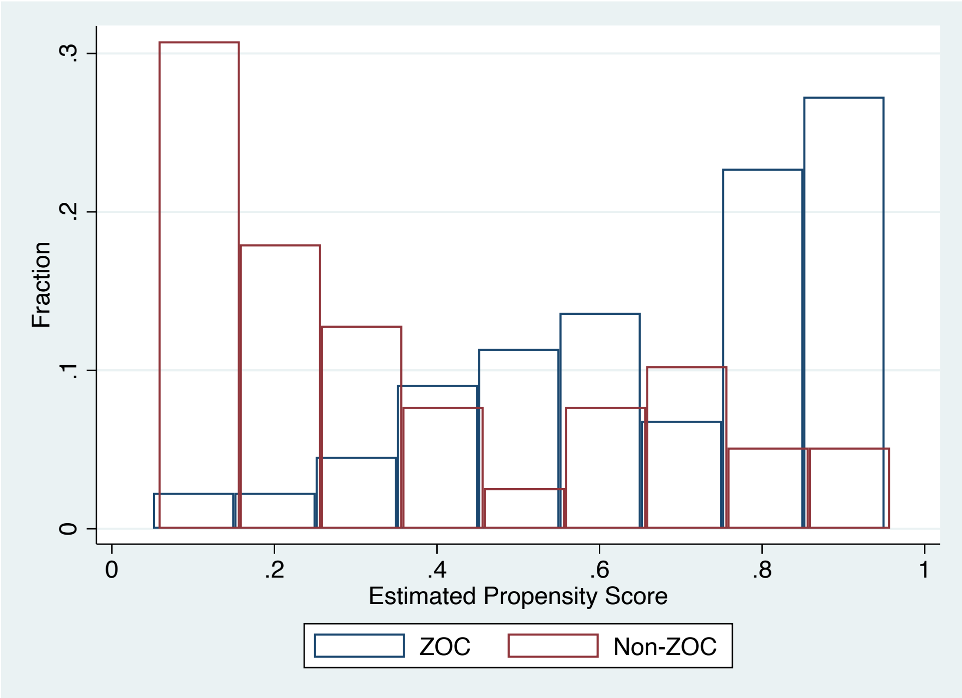
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates for four-year college enrollment rates relative to the year before the policy. Estimates in blue correspond to models for students in the top two quartiles of the predicted four-year college enrollment probability distribution, and estimates in red correspond to the bottom two quartiles. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by vertical lines around point estimates.

E Additional Evidence and Robustness Exercises

E.1 Propensity Score Estimation

The propensity scores used in the paper for the matching procedure are derived from logit models predicting ZOC status using measures of student ability, value-added, and an array of student demographics used elsewhere in the paper. Appendix Figure E.1 reports overlap and demonstrates there is support across the estimated propensity score distribution.

Figure E.1: Propensity Score Overlap



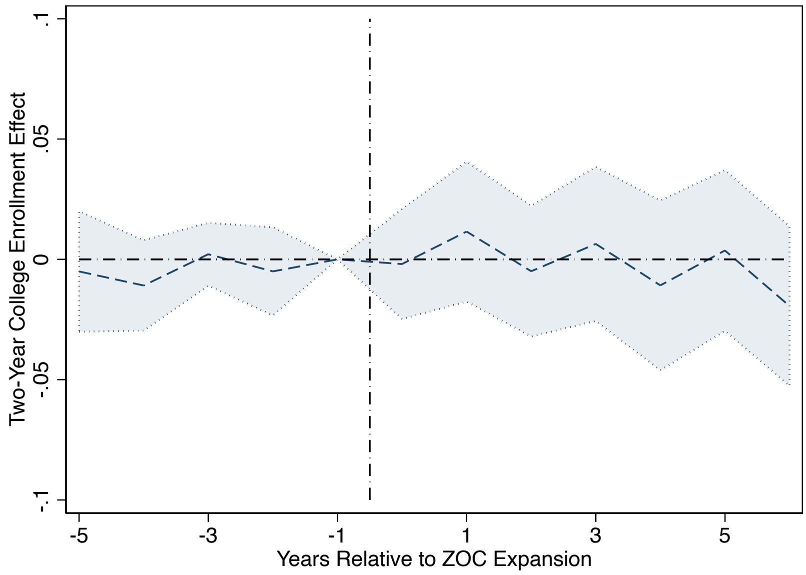
Notes: This figure reports histograms for the estimated school-level propensity scores by treatment status. Bin widths are equal to 0.1.

E.2 Additional Evidence

Appendix Figure E.2 reports two-year college enrollment effects and shows that two-year college enrollment rates are unaffected by the ZOC expansion. This does not imply that community college students were not diverted to four-year colleges or that otherwise non-college enrollees were not bumped into community colleges, however. The evidence does potentially suggest that the share of students nudged into two-year colleges was offset by a similar share of students diverted away from community college into four-year colleges.

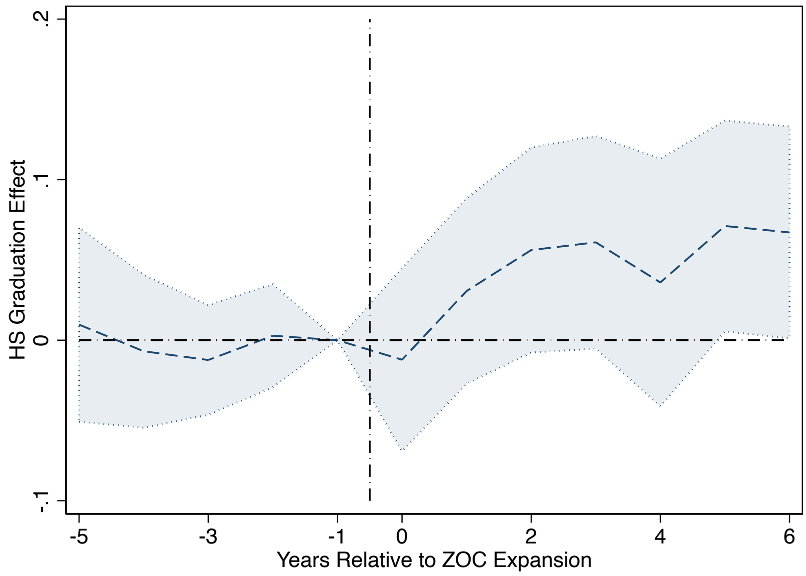
Appendix Figure E.3 reports high school graduation effects and shows that ZOC high school graduation rates differentially improved following the program's expansion. The evidence is far noisier than other evidence but does suggest that ZOC boosted student outcomes in terms of achievement, high school graduation, and college enrollment.

Figure E.2: Two-Year College Enrollment Effects



Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The outcome is an indicator for two-year college enrollment, and the sample is the same as the primary achievement event-study evidence. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. Standard errors are robust and clustered at the school level, and 95 percent confidence intervals are displayed by shaded regions.

Figure E.3: High School Graduation Effects



Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The outcome is an indicator for high school graduation, and the sample is the set of ninth-grade students for each cohort. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. Standard errors are robust and clustered at the school level, and 95 percent confidence intervals are displayed by shaded regions.

Table E.1: Change in Effectiveness Decomposition

	(1) Zones of Choice	(2) Non-Zones of Choice
Total Change	.164	.026
$\Delta\alpha$	.144	.015
$\Delta\omega$	.02	.011
N	38	38

Notes: This table reports estimates from a decomposition of the change in school effectiveness between ZOC and non-ZOC schools between 2012 and 2019 governed by either changes in enrollment shares or changes in school effectiveness. We can decompose the aggregate change in ZOC school effectiveness as follows:

$$\begin{aligned}\Delta\alpha &= \sum_{j\in ZOC}\omega_j^{2019}\alpha_j^{2019} - \sum_{j\in ZOC}\omega_j^{2012}\alpha_j^{2012} \\ &= \sum_{j\in ZOC}\omega_j^{2012}(\alpha_j^{2019}-\alpha_j^{2012}) + \sum_{j\in ZOC}(\omega_j^{2019}-\omega_j^{2012})\alpha_j^{2019}.\end{aligned}$$

The first component captures the change due to changes in α_j, and the second component captures changes due to changes in enrollment shares ω_j. The table reports decompositions for ZOC and non-ZOC schools that are part of the analysis.

E.3 Assessing the Role of Charter and Magnet Competition

In this section, we compare charter enrollment trends in ZOC neighborhoods to non-ZOC neighborhoods. This is motivated from the fact that LAUSD suffered from declining enrollment throughout the sample period with a coinciding increase in the charter market share. One immediate concern is that charter competition, differentially affecting ZOC neighborhoods, can explain our main findings.

To probe at this possibility, we complement our analysis with data from the National Center for Education Statistics (NCES). We collect school-level enrollment data for all charter schools in the Los Angeles area from 2008 to 2020. These data include geographic coordinates of each school, allowing us to classify each as belonging to a ZOC neighborhood or not; we refer to this as the school-level sample. For extensive margin analysis, we consider neighborhood-level aggregates, where we aggregate the total number of charter schools by attendance zone level; we refer to this as the neighborhood-level sample. With these data, we now discuss the evidence on charter competition during our sample period.

Using the school-level sample, we consider the following difference-in-differences model:

$$Y_{it} = \alpha_i + \alpha_t + \sum_{k\neq 2012}\beta_k ZOC_i \times \mathbf{1}\{t(i)=k\} + u_{it},$$

where Y_{it} corresponds to enrollment levels or log enrollment of school i in year t, α_i are school indicators, α_t are year indicators, and ZOC_i are ZOC neighborhood indicators interacted with event-time indicators. Standard errors are robust and clustered at the school level.

Using the neighborhood-level sample, we consider the following difference-in-differences model:

$$Y_{nt} = \alpha_n + \alpha_t + \sum_{k\neq 2012}\beta_k ZOC_n \times \mathbf{1}\{t(i)=k\} + u_{nt},$$

where Y_{nt} corresponds to the total number of charter schools in neighborhood n in year t, and other variables are defined as above, switching schools with neighborhoods where appropriate.

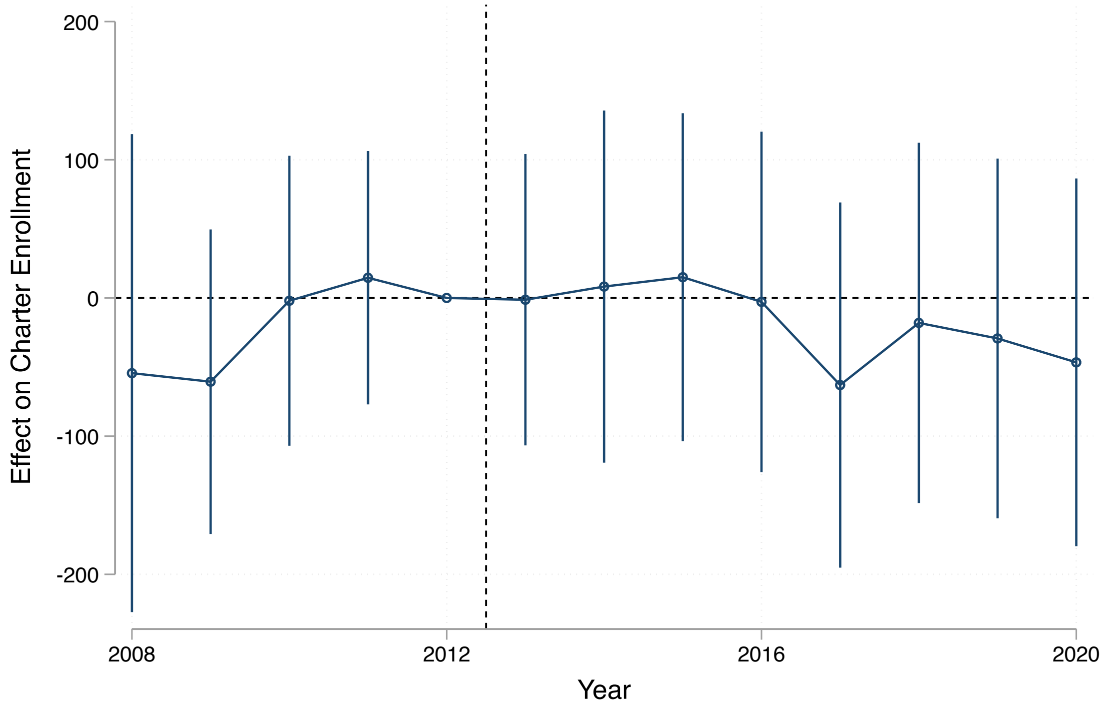
Appendix Figure E.4 reports event-study evidence comparing charter enrollment trends in ZOC neighborhoods to non-ZOC neighborhoods using the school-level sample. The evidence reveals that charter enrollment trends are not trending differently both before and after the ZOC expansion. This suggests that competition from charter schools affected ZOC and non-ZOC neighborhoods equally and assuages concerns that competition at the intensive margin explains our findings.

Appendix Figure E.5 considers log enrollment and finds similar evidence. Nonetheless, while existing charter schools may not have experienced differential increases in enrollment, ZOC neighborhoods may have experienced an increase in the number of charter schools relative to the increase in non-ZOC neighborhoods; this is competition at the extensive margin. Appendix Figure E.6 reports this evidence and similarly finds weak evidence that extensive margin competition trends differently both before and after the ZOC expansion. While the 2009 coefficient points to a potential differential trends, we are unable to reject the joint null hypothesis that all coefficients in the pre-period are equal to zero. We view the combination of evidence as encouraging and suggestive that charter competition is not a primary driver of our empirical results.

Last, in part as a response to charter competition, LAUSD expanded its magnet offerings throughout the sample period. Appendix Figure E.7 demonstrates that magnet school enrollment for students living in a ZOC neighborhood was not differentially affected during our sample period.[5] This indicates that although there has been a persistent increase in the magnet offerings during the sample period, both students who live in a ZOC neighborhood and those who do not trended similarly into magnet adoption. This final piece of evidence assuages concerns that magnet programs explain our findings.

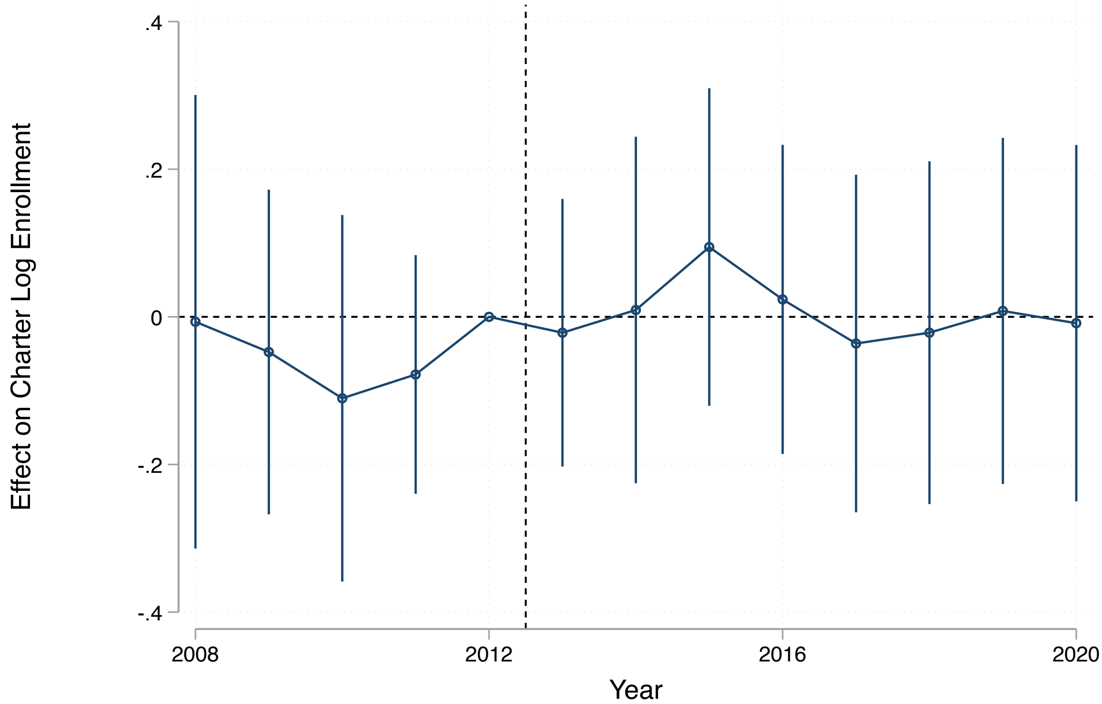
[5] The sample used for this analysis is the same as in the primary analysis.

Figure E.4: Intensive Margin: Effects on Charter Enrollment



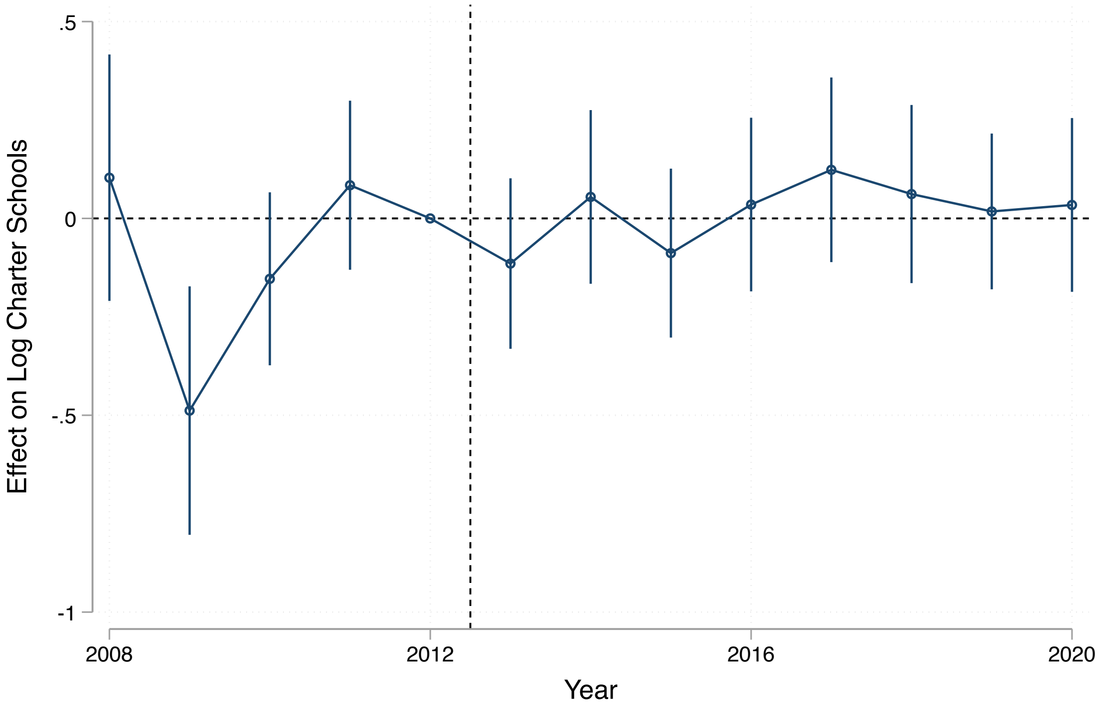
Notes: This figure reports estimates from regressions of charter-school-level log enrollment on year indicators, school indicators, and ZOC neighborhood indicators interacted with event-time indicators. The interaction term estimates are reported with 2012 as the omitted year. Charter school enrollment data come from the NCES. Standard errors are robust and clustered at the neighborhood level.

Figure E.5: Intensive Margin: Effects on Charter Log Enrollment



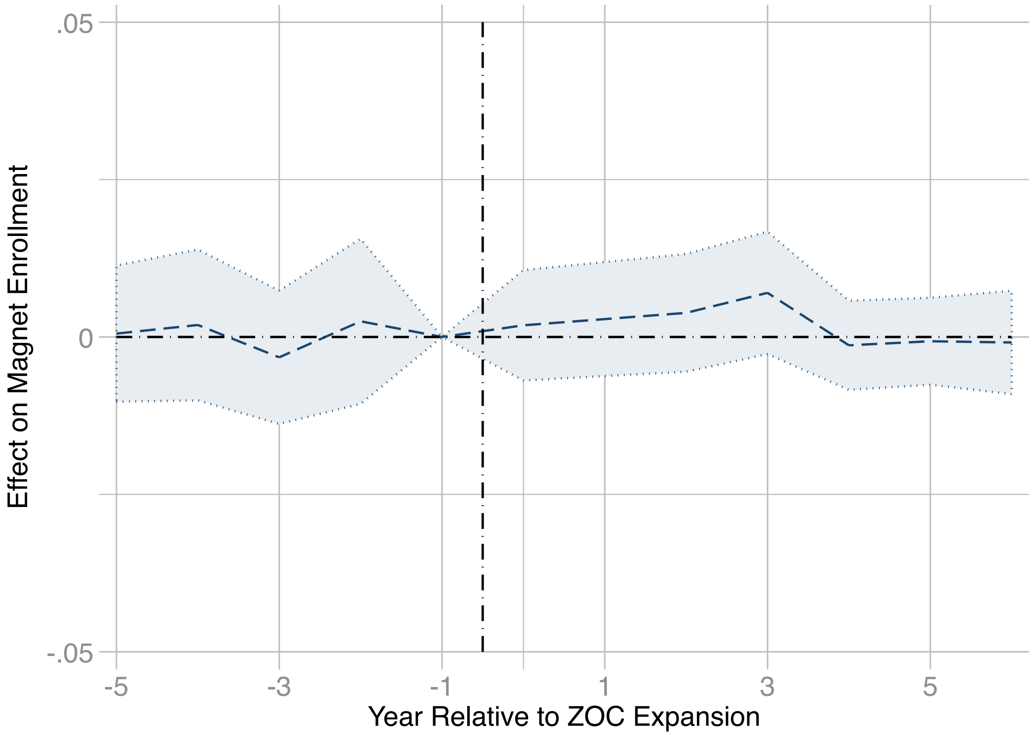
Notes: This figure reports estimates from regressions of charter-school-level log enrollment on year indicators, school indicators, and ZOC neighborhood indicators interacted with event-time indicators. The interaction term estimates are reported with 2012 as the omitted year. Charter school enrollment data come from the NCES. Standard errors are robust and clustered at the neighborhood level.

Figure E.6: Extensive Margin: Effects on Charter School Count



Notes: This figure reports estimates from regressions of neighborhood-level log number of charters on year indicators, school indicators, and ZOC neighborhood indicators interacted with event-time indicators. The interaction term estimates are reported with 2012 as the omitted year. Charter school enrollment data come from the NCES. Standard errors are robust and clustered at the neighborhood level.

Figure E.7: Magnet Enrollment Rate Comparisons



Notes: This table reports event-study coefficients from a regression of student-level indicators of magnet enrollment in ninth grade on neighborhood indicators, year indicators, and ZOC neighborhood indicators interacted with event-time indicators. Standard errors are robust and clustered at the neighborhood level. Shaded regions represent 95 percent confidence intervals.

E.4 Attendance Zone-Level Treatment

A primary concern in the research design outlined in Section V is the potential sorting of students into ZOC neighborhoods and schools. While we can show student demographics are not trending differently (Appendix Figure E.10) and that estimates are robust when restricting to the subset of students who do not move during middle school (Appendix Figures E.11 and E.12), we now present evidence from an alternative research design that is more robust to sorting concerns.

The evidence in this section has two main differences from the evidence presented throughout the main text. The first relates to sample selection criteria. In the main text, we restricted to what we refer to as *comparable* schools, but in this section we do not impose those restrictions. Second, we define treatment at the neighborhood level, defined by students' addresses during middle school. Therefore, subsequent comparisons are comparisons in trends between students who live in a ZOC neighborhood and those who do not. This approach produces comparisons that are less connected to actual sorting decisions made by students at the high school enrollment stage and is in similar spirit to Billings, Deming and Rockoff (2014) and Fryer (2014).

The specification is similar to Equation 2 (of the main text), with the key difference being that $ZOC_{z(i)}$ is defined at the neighborhood level as opposed to the school level:

$$Y_i = \mu_{z(i)} + \mu_{t(i)} + \sum_{k \neq -1} \beta_k ZOC_{z(i)} \times \mathbf{1}\{t(i) - 2013 = k\} + \boldsymbol{X}_i' \psi + u_i. \tag{6}$$

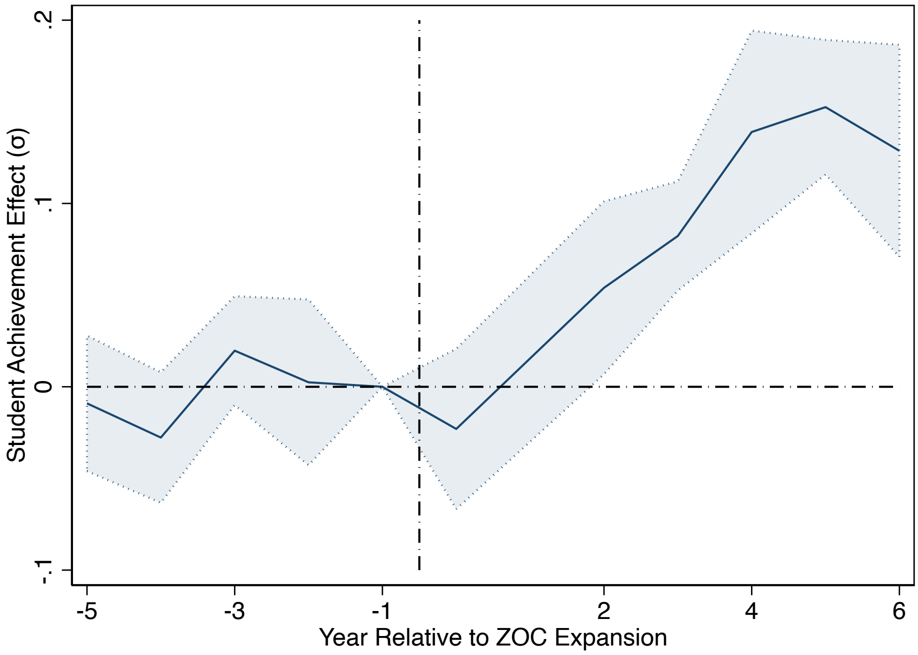
We define neighborhood in two ways. The first is at the attendance zone level and the second is at the neighborhood level. There are a total of 64 attendance zone levels that are fixed in the pre-period. There are a total of 23,833 census blocks in our sample. The latter specification allows us to absorb richer sources of time-invariant neighborhood-level heterogeneity. Throughout, we estimate robust standard errors that are clustered at the neighborhood level.

Appendix Figures E.8a and E.8b report the estimates from the alternative strategy, with treatment defined at the attendance zone level. As would be expected, the point estimates are slightly attenuated and more imprecise in the college sample. In contrast to a 0.16σ and 5 percentage point impact, we find a roughly $.13\sigma$ and 3 percentage point impact by year 6 on achievement and college enrollment, respectively.

The estimates where neighborhood is defined at the students' middle school census block level are reported in the main text in Figure III, and are similar to the previous evidence. The robustness of the evidence to alternative research designs that define treatment at some pre-high school choice level provide reassuring evidence against sorting concerns.

Figure E.8: Achievement and College-Enrollment Event Studies: Attendance Zone Level Assignment

(a) Achievement Event Study



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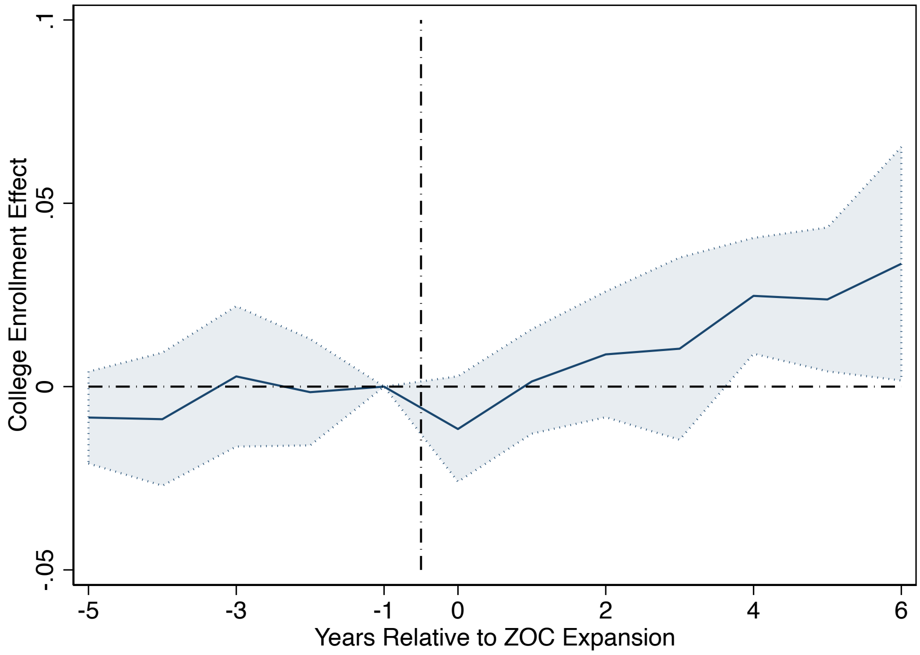
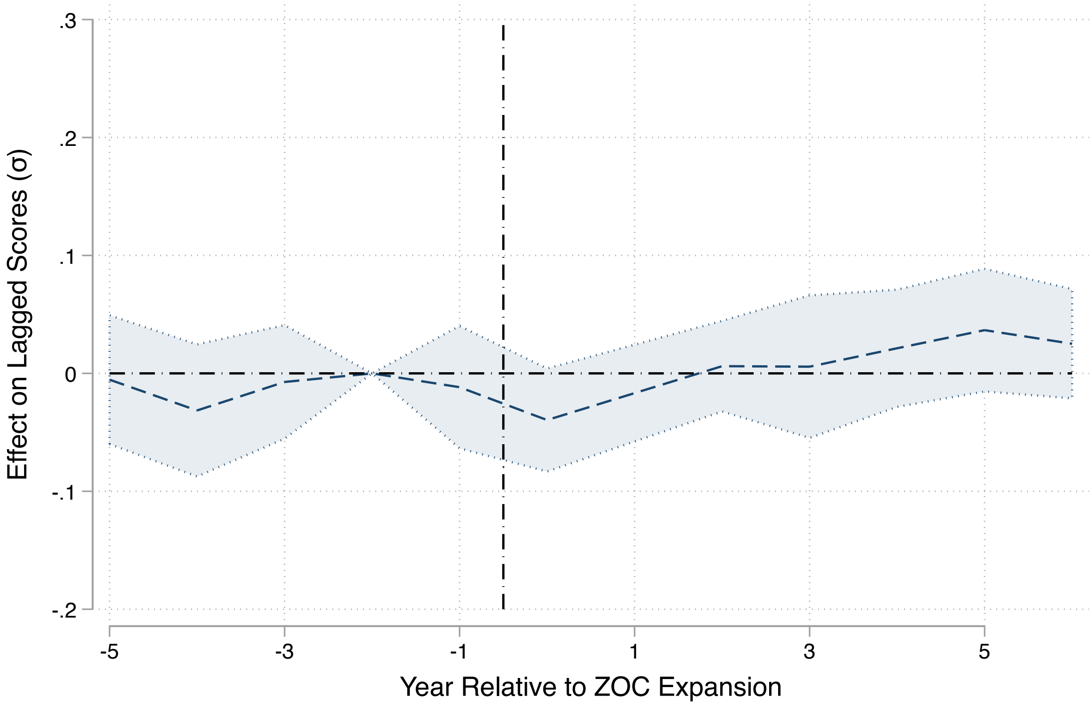
(b) Four-Year-College-Enrollment Event Study: Attendance Zone Assignment



Notes: This figure reports estimate of β_k analogous to those defined in Equation 6 (of the appendix), where k is the number of years since the ZOC expansion. Treatment is defined at the attendance zone level. The coefficient β_k shows difference-in-differences estimates for outcomes relative to the year before the policy. Panel A reports treatment effects on student achievement and Panel B reports treatment effects on four-year college enrollment. Standard errors are clustered at the attendance zone level, and 95 percent confidence intervals are displayed by the shaded regions.

Figure E.9: Treatment effects on lagged test scores



Notes: This figure reports estimate of β_k analogous to those defined in Equation 6 (of the appendix), where k is the number of years since the ZOC expansion. Treatment is defined at the census block level. The outcome is lagged achievement, measured in eighth-grade. The coefficient β_k shows difference-in-differences estimates for outcomes relative to the year before the policy. Standard errors are clustered at the attendance zone level, and 95 percent confidence intervals are displayed by the shaded regions.

E.5 Other Robustness Checks

In this section, we discuss a few additional robustness probes that were alluded to in the main text. The first relates to potential concerns about changes in student composition and sorting. Appendix Figure E.10 demonstrates that changes in observable student demographics are not a serious concern; Panel A reports estimates for each covariate separately and Panel B reports a summary index.

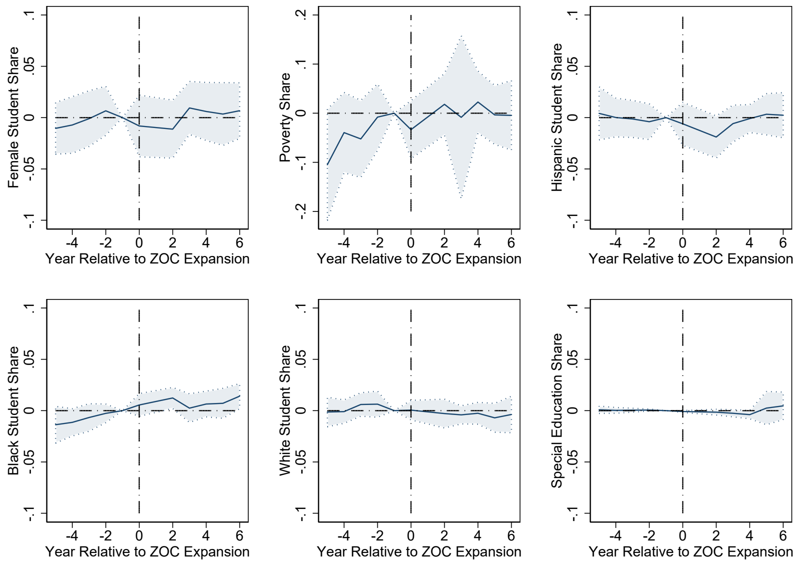
Appendix Figure E.11 and Appendix Figure E.12 considers strategic sorting. Some students that are observationally similar may have strategically sorted into ZOC neighborhoods after the program expansion. These types of moves are not detected in the evidence in Appendix Figure E.10. To assess the potential of bias from such strategic movers, we consider a model that excludes students who moved in eighth grade and another model that excludes students who moved at any point during middle school. Both figures report qualitatively similar results as presented in the main text, assuaging concerns that strategic sorting into ZOC neighborhoods is driving our primary results.

Next, we consider differential attrition out of the sample in Appendix Figure E.13. We do not find strong evidence of differential attrition out of the sample. This attenuates concerns that some of our estimates are driven by differences in attrition rates.

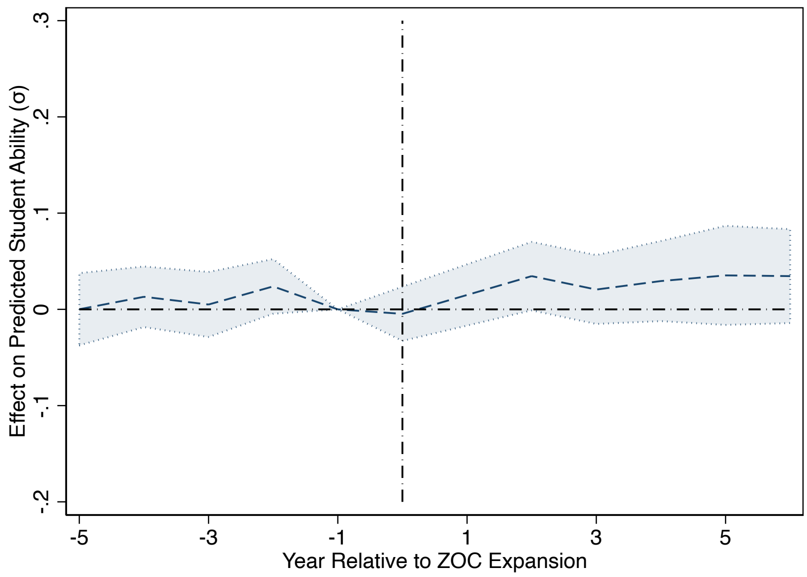
Next, we consider a placebo exercise that estimates treatment effects on middle school achievement gains among ZOC-residing middle school students. This exercise is motivated by the fact that LCFF funding disporportionately disadvantaged neighborhoods and our findings may be due to changes in school funding levels. If so, we should observe a coinciding increase in achievement among middle school students at around the same time we observe increases in achievement among high school students. Appendix Figure E.14 demonstrates that is not the case. This provides suggestive evidence that changes in school funding, as governed by the LCFF, do not explain our main findings.

Figure E.10: Changes in Student Demographics

(a) By Covariate

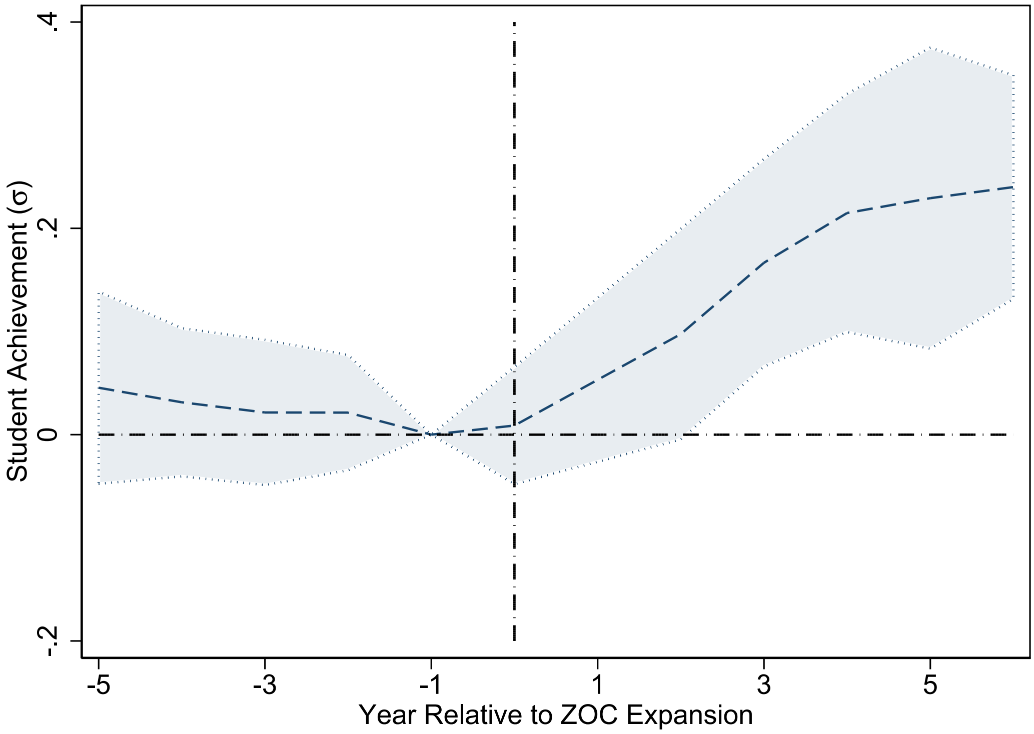


(b) Summary Measure



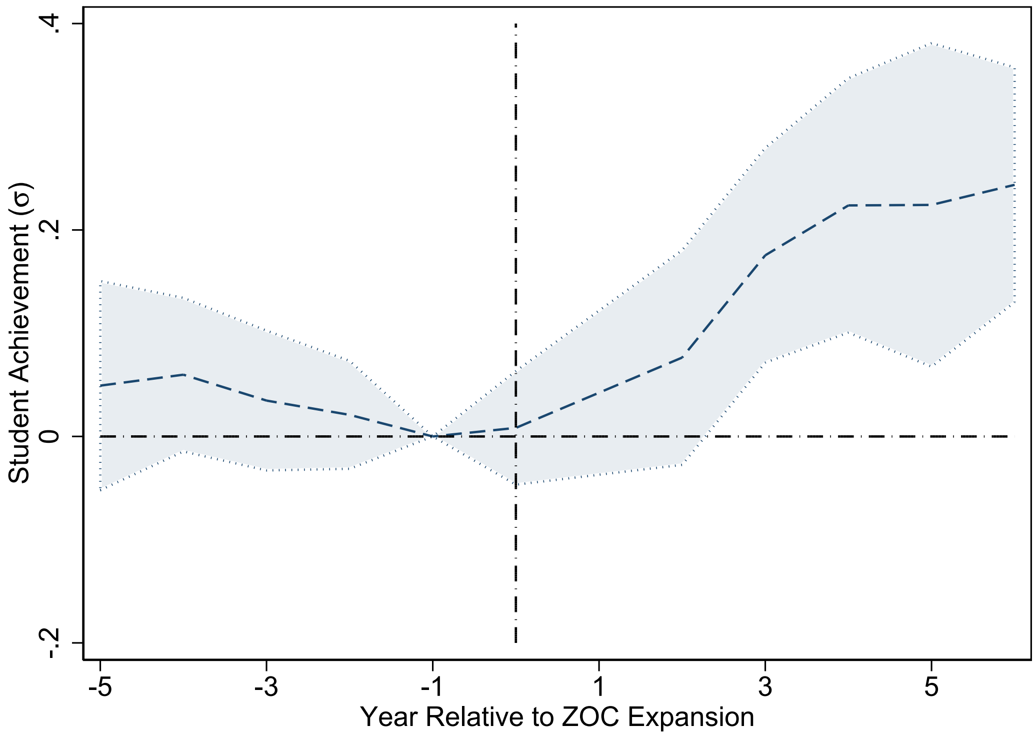
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in the change of student characteristics, labeled on subfigure vertical axes, between ZOC and non-ZOC students relative to the year before the expansion. Panel A reports effects for each covariate separately and Panel B reports effects on a summary index of these covariates and lagged achievement. The summary index is the predicted ability estimate derived from the decomposition outlined in Appendix C.1. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

Figure E.11: Achievement Event Study Restricted to Students Who Did Not Move in the Eighth Grade



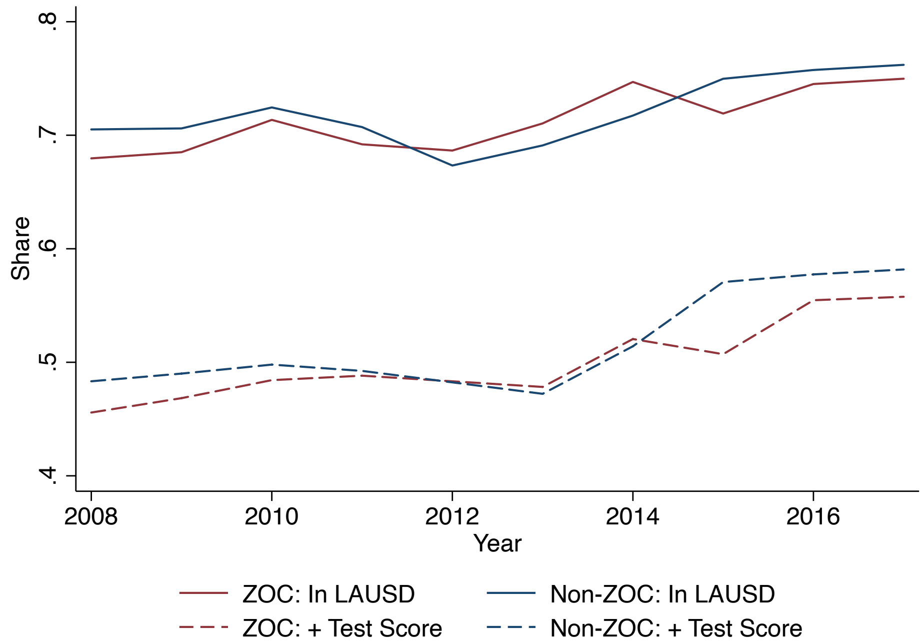
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The sample is restricted to students who did not move in the eighth grade, the year before households submitted ZOC applications. The coefficient β_k shows the difference in changes in achievement, labeled on vertical axes, between ZOC and non-ZOC students relative to the year before the expansion. The solid blue line traces out estimates. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

Figure E.12: Achievement Event Study Restricted to Students Who Did Not Move in Middle School

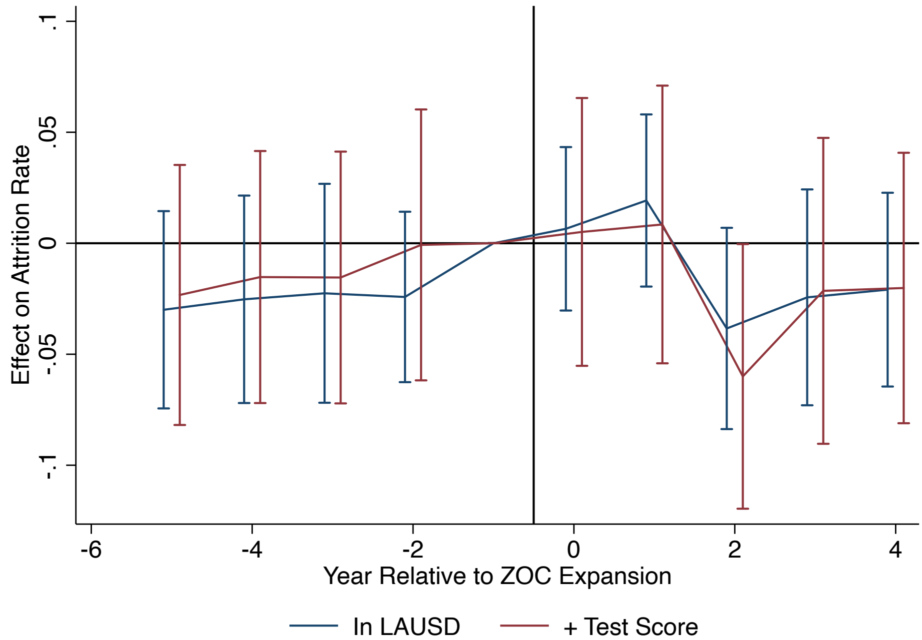


Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The sample is restricted to students who did not move in eighth grade *and* did not move at any time during middle school. The coefficient β_k shows the difference in changes in achievement, labeled on vertical axes, between ZOC and non-ZOC students relative to the year before the expansion. The solid blue line traces out estimates. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

Figure E.13: Attrition Estimates



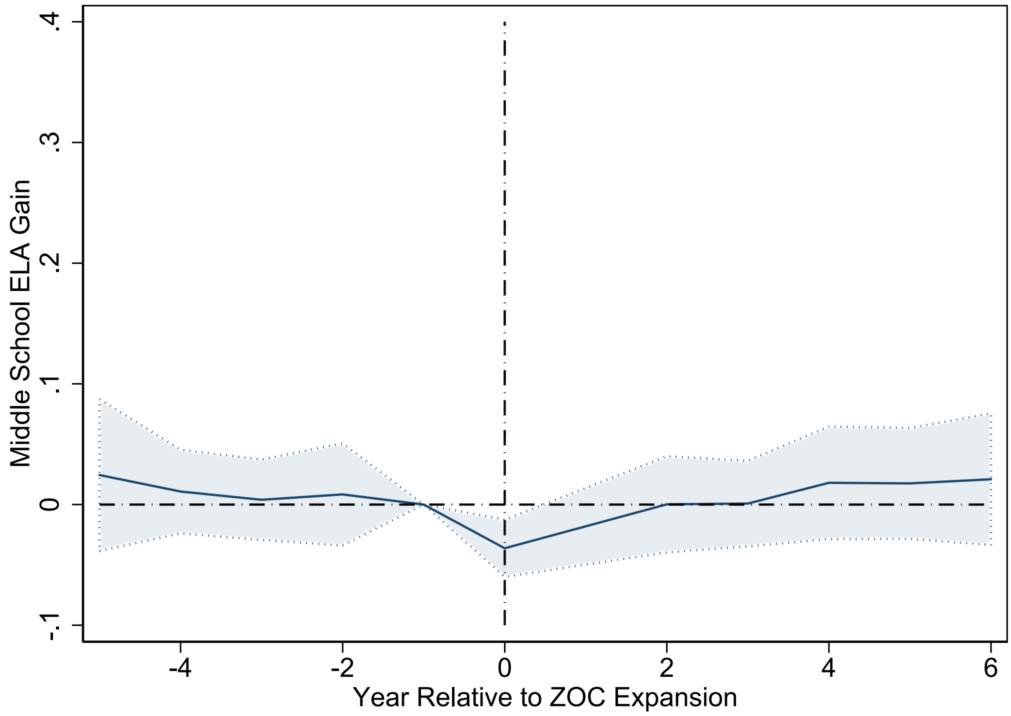
(a) Trends in Attrition Rates



(b) Attrition Event-Study Estimates

Notes: This set of figures explores nonrandom attrition out of the sample. Panel (a) reports the share of students enrolled in a high school in 9th grade who are present in 11th grade and also the share of students in 11th grade with test scores. Panel (b) reports unadjusted event-study analogs of Panel (a).

Figure E.14: Falsification Test: ZOC Impact on Middle School Gains

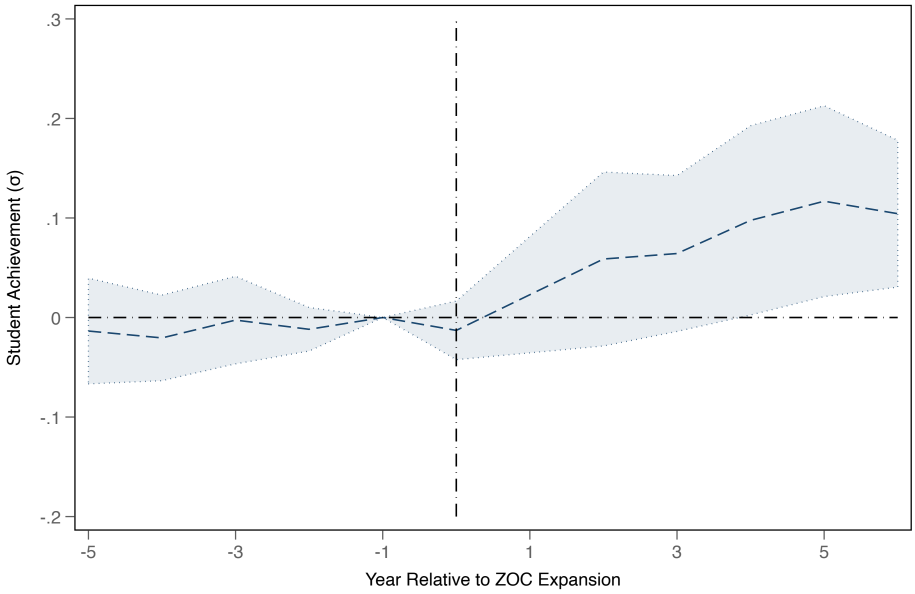


Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The outcome is student achievement growth between seventh and eighth grades, measured in student achievement standard deviations and predating students' ZOC participation. The coefficient β_k shows the difference in changes in lagged achievement growth, labeled on vertical axes, between ZOC and non-ZOC students relative to the year before the expansion. The solid blue line traces out estimates. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

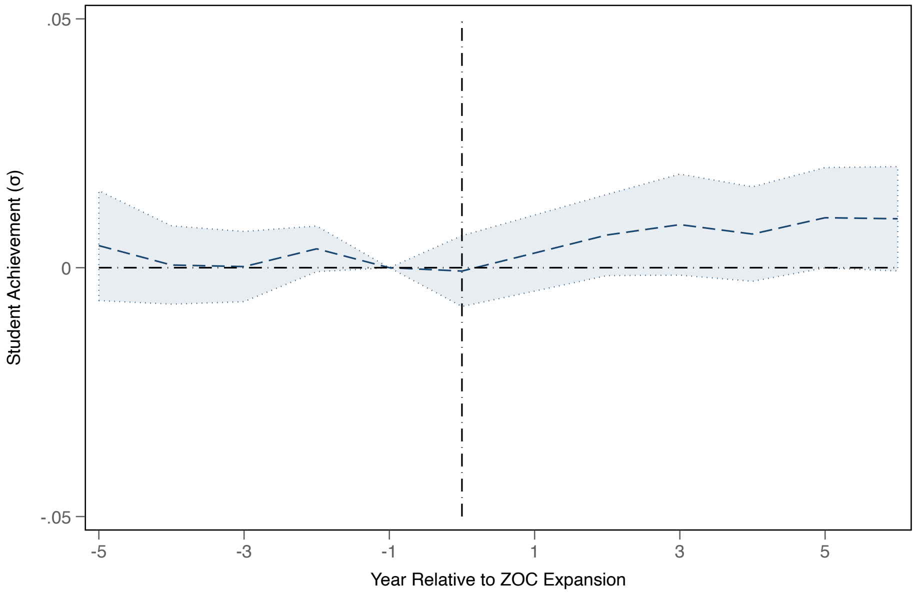
E.6 Decomposition Evidence and Math Estimates

Figure E.15: Decomposition Event Studies

(a) Average Treatment Effect Event Study

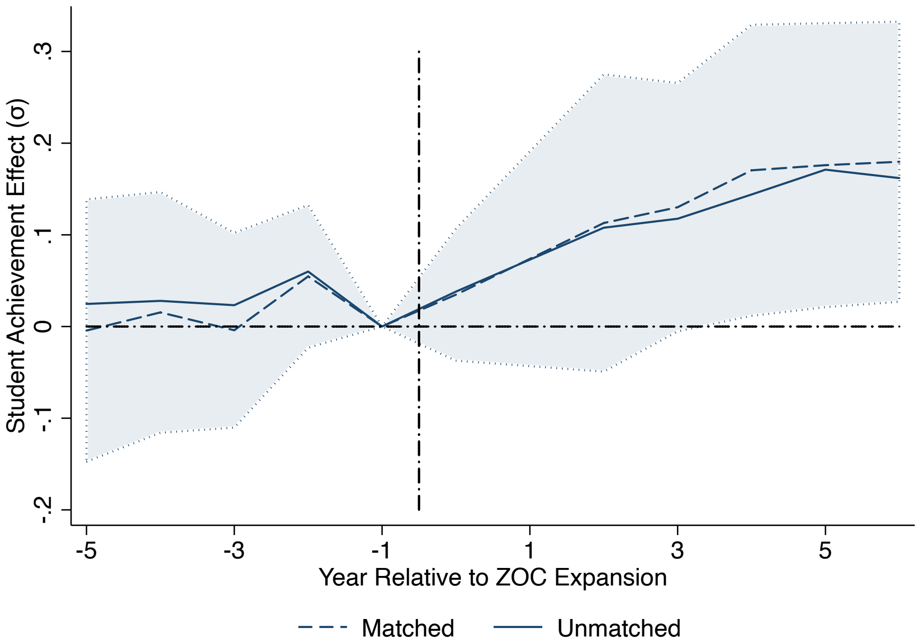


(b) Match Effect Event Study



Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in achievement σ between ZOC and non-ZOC students relative to the difference in the year before the expansion. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

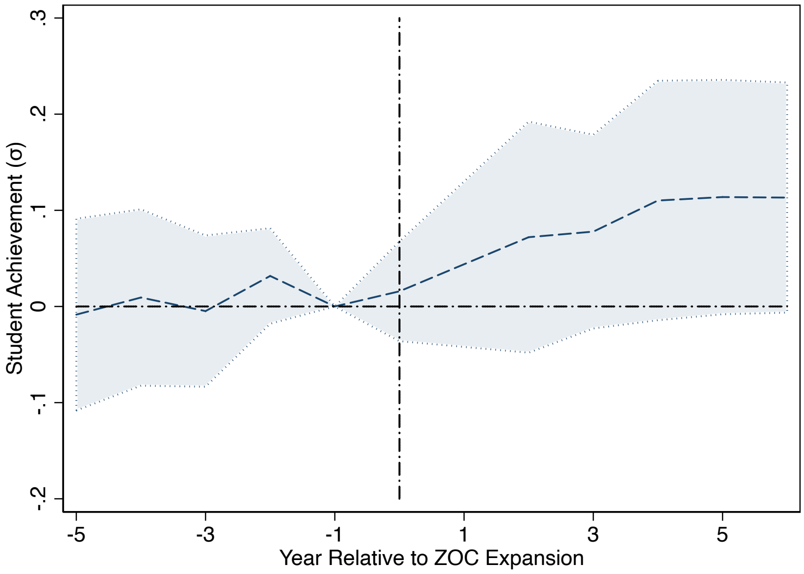
Figure E.16: Math Achievement Event Study



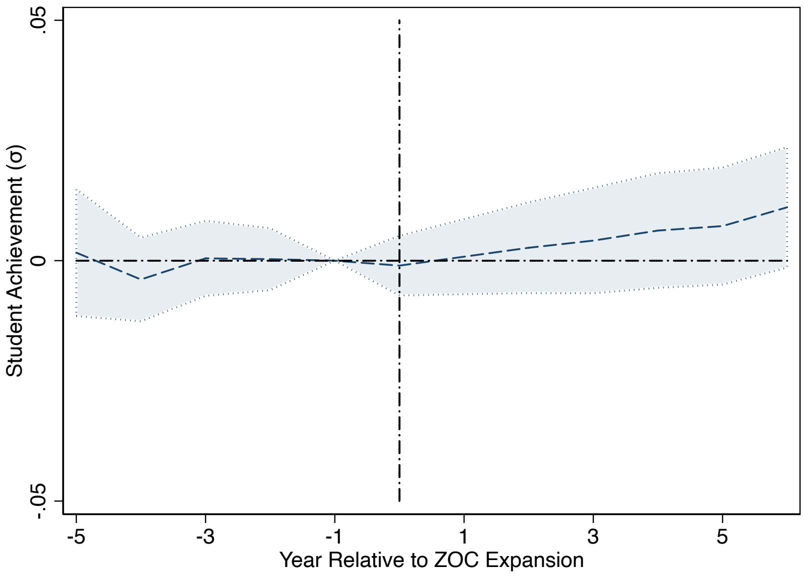
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. The dashed blue line in Panel A traces out estimates that adjust for covariates $\boldsymbol{X}_i$, and the solid line corresponds to estimates that are not regression adjusted. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

Figure E.17: Math Average Treatment Effect and Match Event Studies

(a) Average Treatment Effect



(b) Match



F Demand Estimation Under Strategic Reports

The estimation approach that allows for strategic estimation departs from the standard model by first observing that applicants take into account their admissions chances in their reports. Let $p_i = (p_{i1}, \cdots, p_{iJ})$ be applicant i's admission chances at their available options.[6] We now assume that the unobserved preference heterogeneity $\varepsilon_i = (\varepsilon_{i1}, \cdots, \varepsilon_{ij}) \sim \mathcal{N}(0, \Sigma)$, where Σ is an unrestricted covariance matrix allowing for flexible heteroscedasticity and correlated preference shocks and, importantly, drops the independence of irrelevant alternatives assumption that is common in models with extreme value errors. From this perspective, R_i is a choice over a lottery in the set $\mathcal{L} = \{L_{R_i} \mid R_i \in \mathcal{R}\}$. Given a vector of latent indirect utilities $U_i \in \mathbb{R}^J$ and admissions chances p_i, an applicant reports $R_i \in \mathcal{R}$ only if

$$L_{R_i} \cdot U_i > L_{R_i'} \cdot U_i \quad \text{for all } R_i' \in \mathcal{R}. \tag{7}$$

In contrast to the first model, the empirical likelihood of this model does not have a straightforward closed-form expression. In a seminal paper, Agarwal and Somaini (2018) overcome this limitation by using the Gibbs sampler of McCulloch and Rossi (1994) to obtain draws of the parameters from a Markov chain of draws initiated from any set of parameters $(\Delta_0 = \{\delta_{jc0}\}, \lambda_0, \Sigma_0)$. The posterior mean of this sampler is asymptotically equivalent to the maximum likelihood estimator.

While the Gibbs sampler allows us to obtain feasible parameters, we encounter some issues that may be relevant in other settings. Equation 7 requires comparisons of the chosen R_i with all other $R_i \in \mathcal{R}$, which becomes infeasible for relatively large zones in our setting. Larroucau and Rios (2018) observe that if admissions chances are independent across options, then R_i is optimal only if

$$L_{R_i} \cdot U_i > L_{R_i'} \cdot U_i \quad \text{for all } R_i' \in \mathcal{R}^*_{R_i}, \tag{8}$$

where $\mathcal{R}^*_{R_i}$ is a set that can be obtained from making a one-preference permutation of programs within R_i. Equation 8 substantially reduces the number of comparisons required in the Gibbs sampling procedure, allowing us to simulate draws even in zones with relatively large rank-ordered preference lists. Larroucau and Rios (2018) dub this set of comparisons *one-shot permutations*.[7]

In practice, one-shot permutations impose additional constraints on the region we draw latent utilities U_{ij} from and effectively change the truncation points for subsequent draws. We initiate the sampler with $(\Delta^0 = \{\delta^0_{jc}\}, \lambda^0, \Sigma^0)$ and U^0_i. The initial vector of latent utilities is a solution to the linear program

$$U^0_i \cdot (L_{R_i} - L_{R_i'}) \geq 0 \quad \text{for all} R_i' \in \mathcal{R}^*_{R_i}.$$

We then iterate through the following sequence of conditional posteriors:

$$\begin{aligned}
&\Delta^{s+1} | \; U^s_i, \Sigma^s \\
&\Sigma^{s+1} | \; U^s_i, \Delta^{s+1} \\
&U^{s+1}_i | \; U^s_i, \Delta^{s+1}, \Sigma^{s+1}, \; C(\mathcal{R}^*_{R_i}).
\end{aligned}$$

In the last step of the above sequence, we condition on utility space $C(\mathcal{R}^*_{R_i})$ that rationalizes R_i. The one-shot permutations change the conditioning set in the last step of the sequence,

[6] We construct bootstrapped rational expectation admissions probabilities following Agarwal and Somaini (2018).

[7] For settings in which short lists are common, Larroucau and Rios (2018) further show that restricting comparisons to the set of one-shot permutations and one-shot swaps yields the optimal R_i. In our setting, short lists are not common, so we mainly rely on the dimension reduction obtained by restricting comparisons to one-shot permutations. Idoux (2022) provides an alternative estimation approach in the presence of short lists.

leading to a substantial reduction in the dimension of the linear program that is solved for each student in each step. To obtain our estimates, we use a chain of 200,000 iterations and discard the first 10,000 draws to allow for burn-in.

Appendix Table F.1 reports estimates that account for strategic incentives and find somewhat similar results although estimated with more noise. Taken at face value, the estimates in Panel A suggest that families have a weaker preference for school quality, conditional or unconditional on peer quality, but they nonetheless place positive weight on school quality. The imprecision in the estimates make it hard to infer differences in preferences in this set of estimates, but we emphasize that the estimates in Panel A to Panel C of Table II (of the main text) are more in tune with the demand that principals observe. That is, schools observe the number of families that ranked them first, second, third, and so on, and it is unlikely that principals consider strategic incentives when inferring demand for their schools. Nonetheless, both set of estimates point to same qualitative conclusion: parents tend to value school quality when making choices, and this provides schools incentives to care about their contributions to student learning.

Table F.1

Preferences for School Attributes

	(1)	(2)	(3)	(4)
	Panel A: Strategic Estimates			
School Quality	0.0474			0.0325
	(0.0847)			(0.0750)
	[0.339]			[0.419]
Peer Quality		0.119		0.0871
		(0.152)		(0.163)
		[0.310]		[0.5435]
Match Quality			0.0495	0.0386
			(0.165)	(0.173)
			[0.787]	[0.8248]
Observations	526	526	526	526
R-squared	0.615	0.615	0.615	0.616
Zone X Cell X Year FE	X	X	X	X

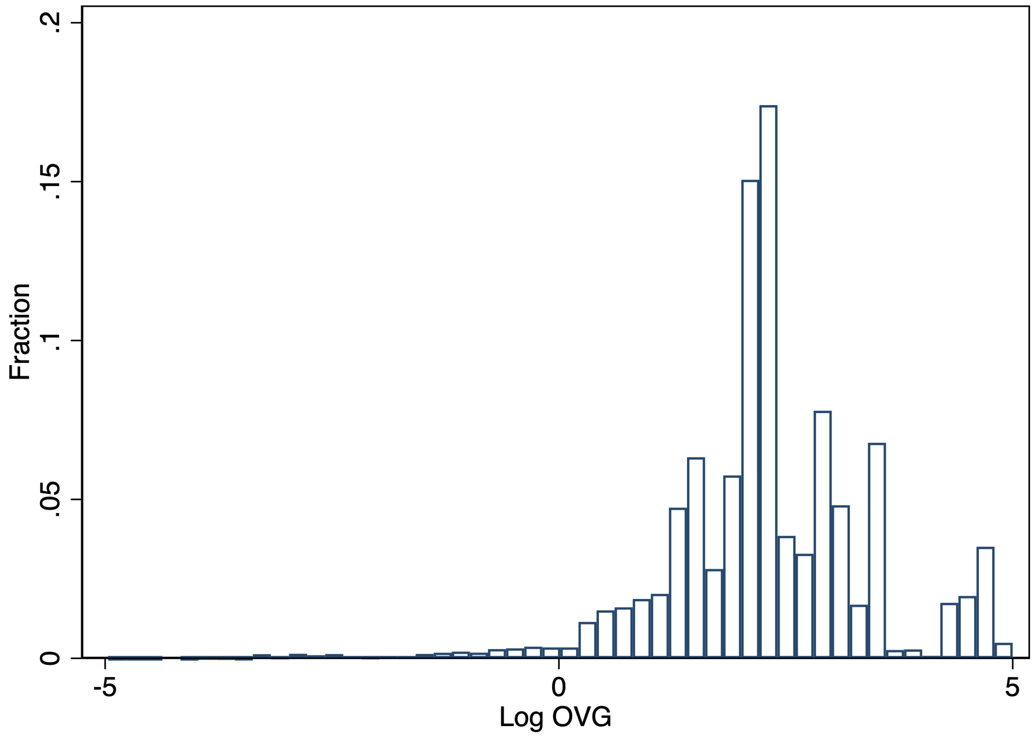
Notes: This table reports estimates from regressions of school-popularity measures δ_{jct} for each school among students in achievement cell c in cohort t on estimated school average treatment effect, ability, and match effects all scaled in standard deviation units. Panel A uses estimates that account for strategic incentives and estimated using a Gibbs sampler. Each observation is weighed by the inverse of the squared standard error of the mean utility estimate and standard errors are clustered at the cell by zone level and reported in parentheses. Numbers in brackets report p-values from Wild bootstrap iterations for models that cluster errors at the zone level and few clusters.

G Additional Details About Mechanisms

G.1 Competition

This section reports summary statistics for the competition index which we refer to as OVG in the main text. To begin, Appendix Figure G.1 displays the distribution of OVG across students, and Appendix Table G.1 reports OVG correlates.

Figure G.1: Log Option Value Gain Distribution



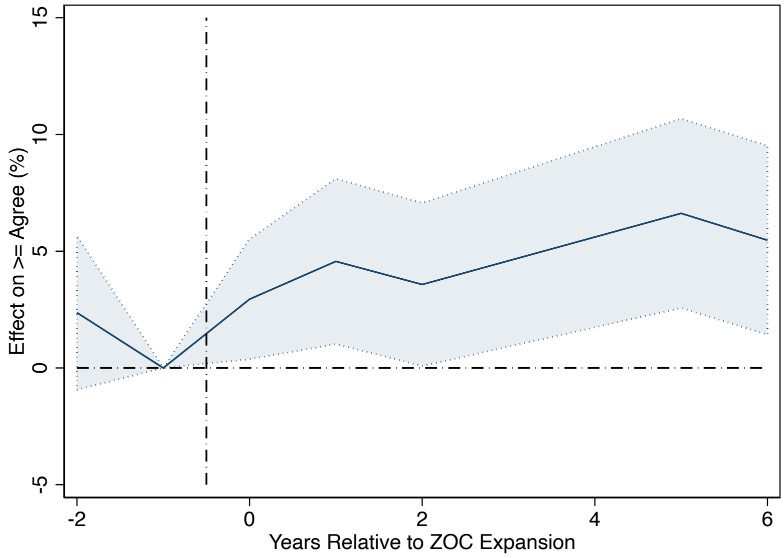
Notes: This figure presents a histogram of estimated log option value gain (OVG) across all students and all years. Preference parameters used in OVG estimation are estimated using only the first cohort's preferences. OVG for later cohorts is constructed using these estimated parameters.

Table G.1: Option Value Gain Correlations

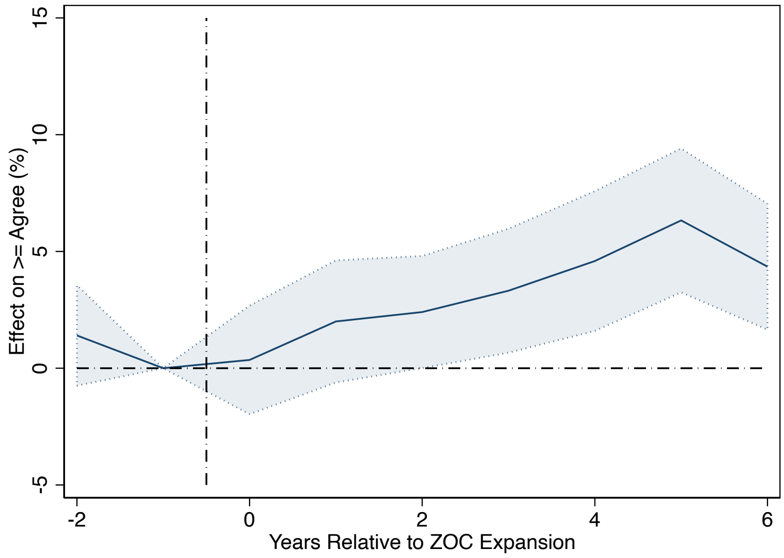
	(1) Log OVG	(2) Log OVG
Black	0.299**	0.124
	(0.125)	(0.0899)
Hispanic	0.328***	0.0320
	(0.0795)	(0.0431)
Parent College +	-0.00977	-0.00668
	(0.0792)	(0.0309)
Poverty	-0.150***	-0.0124
	(0.0311)	(0.0182)
Female	0.0355	-0.00624
	(0.0296)	(0.0179)
Spanish at Home	0.272***	0.00668
	(0.0422)	(0.0250)
English Learner	0.0275	-0.0261
	(0.0433)	(0.0271)
Migrant	0.0952**	-0.00943
	(0.0393)	(0.0219)
Middle School Suspensions	0.00468	-0.0120
	(0.0764)	(0.0514)
Distance to most preferred	0.00625***	0.00496***
	(0.000912)	(0.000650)
Low Score Group	-0.0753*	0.0326
	(0.0435)	(0.0245)
Avg Score Group	-0.0509	-0.0113
	(0.0389)	(0.0212)
Observations	12,519	12,519
R-squared	0.015	0.640

Notes: This table reports coefficients from multivariate regressions of log of option value gain (OVG) on row covariates. The sample is restricted to the initial cohort of ZOC students. Column (1) does not include zone fixed effects, while Column (2) does. Robust standard errors are reported in parentheses.

Figure G.2: LAUSD School Experience Survey Evidence



(a) Student Happiness



(b) Teacher Effort

Notes: This figure plots estimates of β_k analogous to those defined in Equation 1 but for a school-level regression. The index k represents years since the ZOC expansion, and the coefficient β_k shows difference-in-differences estimates for outcomes relative to the year before the policy. The outcomes are school-level shares of respondents at least agreeing with the survey item. Panel A reports estimates for student satisfaction outcomes and Panel B reports estimates on students' perceptions about teacher effort. Because the School Experience Survey initiated in 2011, we do not have additional years of pre-period data. Regressions are weighted by the response rate at each school, assigning more weight to schools with higher response rates. Standard errors are clustered at the school level, and the shaded regions display 95 percent confidence intervals.

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