

THE IMPACT OF PUBLIC SCHOOL CHOICE: EVIDENCE FROM LOS ANGELES' ZONES OF CHOICE *

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Abstract

Does a school district that expands school choice provide better outcomes for students than a neighborhood-based assignment system? This paper studies the Zones of Choice program, a school choice initiative that created small high school markets in some neighborhoods but left attendance-zone boundaries in place throughout the rest of the district. We study market-level impacts of choice on student achievement and college enrollment using a differences-in-differences design. Student outcomes in ZOC markets increased markedly, narrowing achievement and college enrollment gaps between ZOC neighborhoods and the rest of the district. The effects of ZOC are larger for schools exposed to more competition, supporting the notion that competition is a key channel. Demand estimates suggest families place substantial weight on schools' academic quality, providing schools with competition-induced incentives to improve their effectiveness. The evidence demonstrates that public school choice programs have the potential to improve school quality and reduce neighborhood-based disparities in educational opportunity.

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I Introduction

In recent years, centralized school choice systems have become increasingly popular for allocating K-12 students to schools, a shift away from traditional neighborhood-based assignment (Abdulkadiroğlu and Sönmez 2003; Neilson 2021). This alternative approach to education markets expands students’ access to effective schools, introduces potential improvements in allocative efficiency, and under certain conditions, competition can lead to improvements in the quality of education (Chubb and Moe 1990; Friedman 1955; Hoxby 2000, 2003). Large school districts, such as those in New York City, Denver, and New Orleans have adopted such systems (Abdulkadiroğlu, Agarwal and Pathak 2017; Harris and Larsen 2015; Pathak and Sönmez 2008, 2013). However, existing research is unclear on how student outcomes compare under the two market structures. Does a public school district that expands school choice provide better outcomes for students than a neighborhood-based assignment system? What market-level effects do systems of public school choice produce, if any?

This paper tackles these important questions by studying the Zones of Choice (ZOC) program, an initiative of the Los Angeles Unified School District (LAUSD). The program’s design provides a natural experiment where roughly 30–40 percent of the district operates under school choice systems mirroring expansions in other districts, while the remaining neighborhoods operate under the status quo of neighborhood assignment. In particular, the program creates small local high school markets of varying size in some neighborhoods but leaves traditional attendance zone boundaries in place throughout the rest of the district. ZOC students are eligible to attend any school within their neighborhood-based zone, even if it is not the closest one, and a centralized mechanism is used to ration access to oversubscribed schools. The design of this program provides a novel setting to study market-level effects of choice as opposed to individual effects of choice that are common in literature (Abdulkadiroğlu et al. 2011; Abdulkadiroğlu, Pathak and Walters 2018; Cullen, Jacob and Levitt 2006). The focus on market-level effects, which approximate general equilibrium-like effects from a reduced-form perspective, fills a gap in the literature and provides a more complete overview of the underlying channels and mechanisms of the short- and medium-run effects of choice and competition.

We orient the empirical analysis around a stylized model of school choice and competition in which families choose a school based on its proximity, its quality, and their idiosyncratic tastes. On the supply side, we assume school principals are rewarded for larger market shares but must exert effort to improve school quality. We then model ZOC as an expansion of households’ choice set, simultaneously introducing strategic considerations between schools in their quality determination. The model gives rise to a simple statistic that captures households’ expected welfare gain from the choice set expansion: “option value gain” (OVG). The changing distribution of OVGs across students in response to competition governs schools’ incentives to increase quality and thus serves as a useful empirical statistic to study the role of competitive effects. The theoretical framework predicts that the introduction of ZOC will improve school quality and the improvement will be concentrated among schools exposed to more competition as measured by OVG.

We test these predictions using a difference-in-differences design that compares changes in

outcomes between ZOC and non-ZOC students. To isolate the impact of ZOC on school quality, we decompose treatment effects into effects on student-school match quality and effects on schools' value added, interpreting the latter as a measure of school quality. Estimates of quantile treatment effects on school quality then allow us to assess whether the lowest-performing schools improve more. We then pivot to the demand side and use students' rank-ordered preference lists to estimate preferences and calculate OVG empirically. Looking at the heterogeneity of treatment effects with respect to OVG allows us to study how the causal impacts of ZOC vary with the extent of competition. Last, studying preferences for school quality allow us to reconcile ZOC supply-side effects with the incentives schools faced as captured through the choices families make.

We find large positive effects of ZOC on student achievement and four-year college enrollment. Event-study estimates reveal that by the sixth year of the program, ZOC students' English and language arts (ELA) exam performance improved by 0.16σ relative to comparable non-ZOC students. ZOC also raised four-year college enrollment by roughly 5 percentage points, a 25 percent increase from the baseline ZOC student mean, an effect mostly explained by increases in enrollment at California State University (CSU) campuses. Both of these effects lead to vast reductions in between-neighborhood inequality in educational outcomes. A decomposition of the achievement impacts reveals that improvements in school quality mostly explain the effects, leading to a substantial reduction in neighborhood-based achievement gaps. Next, we find that improvements in school quality are concentrated among the lowest-performing schools, a finding consistent with the theoretical framework. Further supporting the competitive effects hypothesis, we find that the effects of the program are larger for schools and students with higher OVGs. These findings suggests that the competition-induced incentives generated by ZOC are a key mechanism for its effects on school performance.

Our subsequent analysis pivots to studying the demand side. Estimates of preferences derived from rank-ordered preference lists are consistent with the ZOC effects. We find that parents' reported preferences place a higher weight on school effectiveness compared to other school characteristics, including a school's student body. This finding supports the notion that parents' choices provide schools the incentives to improve student learning. This finding contrasts with other studies' findings (e.g., Abdulkadiroğlu et al. 2020 and Rothstein 2006) and with evidence that lower-income families are less sensitive to school quality (Burgess et al. 2015; Hastings, Kane and Staiger 2005). We hypothesize that the homogeneity of families with respect to ethnicity and socioeconomic status reduces to the scope to sort into schools based on easily observable peer attributes. This naturally leads to a setting where families may systematically choose schools based on other school attributes more likely to correlate with school quality. Recent evidence from Campos (2023) finds that families' beliefs about school quality are not too far off from the truth, alleviating concerns that families may imperfectly perceive school quality.

We address a variety of concerns related to our empirical approach. We find that alternative sources of competition from charter and magnet schools do not differentially affect ZOC neighborhoods, alleviating concerns that our results are driven by these alternative schooling models. We also find that the composition of students did not differentially change after the

program expansion. Last, we conduct an intent-to-treat-like analysis and find qualitatively similar results.

To probe at additional mechanisms, we find several pieces of evidence suggesting that changes in schooling practices played a role. The most relevant relates to an uptick in suspensions, suggesting that ZOC schools pivoted toward a schooling practice strongly correlated with the no-excuses approach to urban education, also shown to elevate the outcomes of Black and Latino children in other settings (Angrist, Pathak and Walters 2013; Dobbie and Fryer Jr 2011; Fryer 2014).¹ We conclude by demonstrating that intermediate outcomes are also affected; namely that students improved their college preparedness, as captured by changes in course portfolio and improved SAT scores, conditional on taking the SAT. Overall, we add to the growing body of evidence suggesting that the no-excuses-like practices—that is, disciplinary practices—elevates student outcomes in urban settings, but we also show that students in this setting were positive about the resulting changes.

We argue that certain features of ZOC may explain why our findings contrast with those of many previous studies. ZOC allows for relatively personalized interactions between ZOC administrators and parents, making it easier for parents to acquire information (Page, Castleman and Meyer 2020). In particular, administrator-led information sessions provide parents with a potentially rich opportunity to learn about differences in school quality. Moreover, because choice is within zones rather than district wide, ZOC parents face manageable choice sets, which may help them avoid the choice overload issues present in other school choice settings (Beuermann et al. 2023; Corcoran et al. 2018). These features combine to create a setting in which acquiring adequate information about schools is more likely. Last, as ZOC neighborhoods are highly segregated, the options available to families differed minimally in terms of student body composition, potentially nudging parents to select schools in terms of other characteristics more correlated with school effectiveness.

This paper contributes to several strands of research. Most closely, it contributes to the literature studying the supply-side effects of school choice policies or reforms. One strand of the literature relies on cross-district or cross-municipality comparisons to estimate the effects of choice (Hoxby 2000, 2003; Hsieh and Urquiola 2006; Rothstein 2007) and reaches mixed conclusions. Other papers have focused on choice options, such as Catholic, voucher, or charter schools, that directly compete with nearby school districts for students (Card, Dooley and Payne 2010; Dee 1998; Neal 1997). Our paper focuses on within-district public school competition and, as a consequence, is one of the first pieces of evidence demonstrating that the increasingly popular district-wide choice reforms can meaningfully improve student outcomes and reduce educational inequality. In addition, we provide compelling evidence that competition in the public sector is a key mechanism explaining the improvements in student outcomes.

Another set of papers focus on the individual effects of school choice (Abdulkadiroğlu et al. 2011; Abdulkadiroğlu, Pathak and Walters 2018; Cullen, Jacob and Levitt 2006; Deming et al. 2014; Muralidharan and Sundararaman 2015). Our paper goes beyond that and focuses on market-level effects which relate to benefits accrued to all students in the market, as opposed to

¹We find complementary evidence that tracking practices and classroom assignment policies changed, alluding to further changes in schooling practices not necessarily associated with the no-excuses approach.

just participants. The natural experiment we leverage allows us to estimate how two otherwise seemingly similar trending markets evolve both in the short- and medium-run. Therefore, this paper is relevant to the growing number of districts and municipalities around the world introducing choice through centralized assignment systems (Neilson 2021) and highlights the potential of these systems to generate sustained improvements in student outcomes relative to traditional neighborhood-based assignment.

Last, this paper demonstrates that an important neighborhood attribute—school quality—is malleable and thus contributes to the literature studying the impacts of neighborhoods (Bergman et al. 2019; Chetty and Hendren 2018; Chetty, Hendren and Katz 2016; Chyn 2018; Kling, Liebman and Katz 2007). Although recent evidence demonstrates that moving to higher-opportunity neighborhoods tends to produce positive long-run outcomes, it remains an open question what factors mediate these effects (Chyn and Katz 2021). A common hypothesis points to differences in school quality. For example, Laliberté (2021) finds that variation in school quality across neighborhoods explains roughly 50–70 percent of the effects of neighborhoods in Montreal, Canada. Our paper shows that a potential key determinant of neighborhood quality is malleable and school- or neighborhood-specific policies are a means of reducing neighborhood-based disparities in outcomes (Fryer and Katz 2013).

The rest of this paper is organized as follows. Section II outlines the features of the program and our data sources. Section III outlines the conceptual framework for the subsequent analysis, and Section IV discusses the data. Section V reports evidence on how the program affected student achievement and college enrollment. Section VI estimates demand and studies the role of competition, and Section VII presents evidence on additional mechanisms and discusses institutional features that may have contributed to the results. Section VIII concludes.

II Institutional Details

II.A The Choice Landscape in Los Angeles and a Brief History of ZOC

ZOC is an initiative of LAUSD, the second-largest school district in the United States. It is a significant expansion of choice for high schools in Los Angeles, but there was an existing and rapidly changing choice landscape that preceded the program. Before ZOC, families in Los Angeles had the option to enroll in charter schools, apply to magnet programs within LAUSD, and opt for intra-district transfers, provided capacity. The ZOC expansion is partly a response to the evolving choice landscape and the enrollment trends that preceded it.

As has been common in several large urban school districts around the country, LAUSD continues to experience enrollment decline, potentially amplified by charter growth (see Online Appendix Figures A.1 and A.2). The charter landscape was rapidly evolving in the decade before the ZOC expansion. The number of charter high schools, as reported in the Common Core Data, increased from 65 in 2002 to 306 in 2012. Charter high schools residing in ZOC neighborhoods represented 38 percent of the charter school growth over that decade. Families’ out-of-district options increased yearly, and as a consequence, LAUSD high school enrollment started a downward trend in 2008.

Magnet programs are more prevalent than intra-district transfers, so we discuss this option in

detail. Magnet program trends in the decade preceding the ZOC expansion were more stagnant compared to charter growth. There were 38 magnet programs available to high school students until 2010, with the creation of 4 new ones between 2010 and 2012. Magnet enrollment was flat, representing roughly 8–9 percent of all LAUSD high school enrollment during this time period. Even as these programs have expanded across the district, 2018 was the year with the largest market share of 12.8 percent. In summary, while families have many options, relatively few families opt for the magnet high school sector.

ZOC emerged from the Belmont Zone of Choice, located in the Pico Union area of downtown Los Angeles. This community-based program combined several aspects of the various ongoing reforms. A pressing concern among community advocates was the overcrowding of their neighborhood schools. The school construction program studied in Lafortune, Rothstein and Schanzenbach (2018) addressed the overcrowding by creating large high school complexes that housed multiple pilot schools and small learning communities.² Community organizers helped develop the Belmont Zone of Choice by creating an informal enrollment and assignment system for eligible residents. Families residing within the Belmont Zone of Choice were eligible to apply to the various schools located within the zone. The Belmont pilot started in 2007 and continued informally for five years.

The continuing exodus of students from the district and increasing community pressure for access to better schools partly led the school board to consider removing attendance zone boundaries (see *Resolution to Examine Increasing Choice and Removing Boundaries from Neighborhood Schools*) and devising other ways of expanding school choice (see *Resolution on Expanding Enrollment and Equal Access through LAUSD Choice*) in early 2012. The school board’s task force recognized the community’s positive response to the Belmont pilot and began replicating the model in other suitable neighborhoods. By July 2012, a ZOC office was established along with 16 zones. Figure I shows that in 2010, the program mostly covered disadvantaged students.

In contrast to the Belmont Zone of Choice, the new zones were organized and administered by a central district office and used formal assignment and enrollment mechanisms. They also had ambitious goals: access to more effective schools, improvement in student-school match quality, and increased parental involvement. Each of these points was explicitly mentioned in the school board minutes and motivated the expansion of ZOC.

II.B Program Features and Incentives

ZOC expands students’ high school options by combining catchment areas into choice zones and, in some cases, pulling schools with undefined assignment areas into zones. This effectively expands families’ choice sets to include several nearby options. The program expansion we study includes other notable changes as well.

The program is centrally run by a team of administrators who focus only on aspects of ZOC that run on a yearly cycle. The most time-extensive period of the year is the application cycle in which parents of eighth-grade students submit zone-specific applications containing rank-

²LAUSD defines pilot schools as a network of public schools that have autonomy over budget, staffing, governance, curriculum and assessment, and the school calendar. Ties to the labor union remain and is a key distinction between non-LAUSD charter schools and LAUSD pilot schools.

ordered preference lists. Admission into any particular school is not guaranteed, although some priority is given based on proximity, incumbency, and sibling status.

The neighborhood-based program design allows high schools to know where their pool of future students is enrolled. School and district administrators take advantage of this feature by coordinating various parental informational sessions hosted by either feeder middle schools or candidate high schools. Concurrently, some clusters of schools organize community events outside of school hours to pitch their schools to potential students. These events continue for roughly six weeks until rank-ordered preference applications are due in mid-November. Although schools differ in the amount of effort they devote to recruitment, they do not have the leverage to give priority to particular students as some schools can in other school choice settings.

The program expansion also formalizes assignment practices across all zones. The school district uses parents' rank-ordered preference lists to determine assignments using a centralized algorithm, analogous to a Boston—or immediate acceptance—mechanism. Schools that are oversubscribed fill seats using randomly assigned lottery numbers and school-specific priorities. Because LAUSD uses an immediate acceptance mechanism, parents have strategic incentives and may choose to misreport their preferences to guarantee admission into schools they might not prefer the most.

Strategic incentives notwithstanding, many parents list non-neighborhood schools as their most preferred options. Figure II shows that roughly 65–70 percent of applicants list a school that is not their neighborhood school as their most preferred option. Priorities and capacity constraints preclude all applicants from enrolling in their most preferred school, so approximately 30 percent of applicants enroll in a school that is not their neighborhood school. The 30 percent after the policy expansion is a noticeable increase from 7 percent the year before. Importantly, although capacity constraints are binding at some schools within each zone, the concurrent district-wide enrollment decline provides a setting in which schools can absorb additional students. The declining enrollment means that most schools, including initially popular schools, are not operating at capacity, making the threat of competition more significant.

Public schools in Los Angeles have several reasons to care about losing students to competitors in their zone. Although LAUSD does not employ a student-centered funding model in which school budgets are exactly proportional to student enrollment, rigid schedules determine resource and staff allocation. A drop in enrollment could mean schools have to reduce their teaching, counseling, nursing, or administrative staff. Anecdotal evidence suggests principals care about this possibility, providing them with incentives to care about their schools' zone market share.

Another, admittedly more speculative, reason is principals' career concerns. An extensive literature has documented the potential of career concerns to dynamically induce incentives for public sector workers (Dewatripont, Jewitt and Tirole 1999). In LAUSD, roughly 10 percent of principals between 2008 and 2018 took administrative positions at the district headquarters, which can be seen as glittering prizes (Bertrand et al. 2020). Viewed through this lens, ZOC introduces a tournament-like structure, in the sense of Lazear and Rosen (1981), in which principals have incentives to outperform other principals.

The next section presents a conceptual framework that takes these incentives as given in a stylized model of school choice and competition. The model implications guide most of the empirical exercises throughout the rest of the paper.

III Conceptual Framework

We begin with a stylized model of the status quo that consists of neighborhood monopolies competing with an outside option, and then we introduce ZOC, highlighting how the program altered school incentives, and discuss its potential benefits.³ We use j to denote both schools and neighborhoods, indicating there is one school per neighborhood. Let students indexed by i reside in neighborhood $j(i) \in \{1, \dots, J\}$, which contains one school also indexed by j . Each school j operates as a monopoly in its neighborhood but faces competition from an outside option indexed by 0.

Students can enroll in either their neighborhood school $j(i)$ or the outside option. Student i 's utility from attending school $j \in \{0, j(i)\}$ is

$$U_{ij} = U(\alpha_j, \mathbf{X}_i, d_{ij}, \varepsilon_{ij}) = V_{ij}(\alpha_j, \mathbf{X}_i, d_{ij}) + \varepsilon_{ij},$$

where α_j is school quality as defined in the achievement model in Online Appendix C, d_{ij} is distance to school j , \mathbf{X}_i captures observable heterogeneity of student preferences, and ε_{ij} captures any remaining unobserved preference heterogeneity, which we assume is additively separable.⁴

We can further decompose V_{ij} into a school j mean utility component that depends on school quality α_j , an additively separable component capturing remaining observable preference heterogeneity, and linear distance costs:

$$V_{ij} = \omega\alpha_j + \mu_j(\mathbf{X}_i) - \lambda d_{ij}.$$

With a logit error structure for the unobserved preference heterogeneity, school market shares are

$$S_j(\alpha_j; \mathbf{X}, \mathbf{d}) = \frac{1}{N_j} \sum_{i \in j(i)} \frac{e^{V_{ij}}}{1 + e^{V_{ij}}}.$$

On the school side, we assume principals are rewarded for higher enrollment shares and exert effort $e_j \in [\underline{e}, \bar{e}]$ to adjust their α_j and change their school's popularity δ_j (Card, Dooley and Payne 2010). Principals' utility is determined by

$$u_j = \theta S_j(\alpha_j; \mathbf{X}, \mathbf{d}) - e_j,$$

where θ is the relative utility weight on enrollment shares and e_j is the amount of effort exerted on student learning that directly affects test scores. Last, we assume that school quality is an

³We assume residential location decisions are made in a pre-period and are not a first-order concern for this initial ZOC cohort. The outside option mostly reflects nearby charter schools in each neighborhood.

⁴Note that we normalize the utility of the outside option to zero.

increasing concave function of the level of effort e_j , $\alpha_j = f(e_j)$.

Because of cross-neighborhood enrollment restrictions in place before the ZOC expansion, each principal sets school effectiveness α_j independently of other school district principals. Therefore, each principal sets school quality α_j according to

$$f'(e_j) = \frac{1}{\theta \omega S'_{\alpha_j}(\alpha_j; \mathbf{X}, \mathbf{d})} \quad j = 1, \dots, J.$$

Differences in student characteristics and in distance to the outside option generate a pre-ZOC heterogeneous vector of equilibrium effort levels, $\mathbf{e}_0 = (e_{10}, \dots, e_{J0})$, with a corresponding pre-ZOC vector of equilibrium school effectiveness, $\boldsymbol{\alpha}_0 = (\alpha_{10}, \dots, \alpha_{J0})$.

Turning to the introduction of the program, ZOC effectively removes cross-neighborhood enrollment restrictions for some neighborhoods. We model this as an expansion of the choice set from the neighborhood school j to the full list of ZOC schools \mathcal{J} . Therefore, the choice set of a student residing in one of these neighborhoods expands from $J_i = \{0, j(i)\}$ to $\mathcal{J}^+ = \mathcal{J} \cup 0$. Because of the spatial differentiation of schools and student heterogeneity, the value of each additional schooling option varies across students.

We define a student's OVG as the difference in expected maximum utility under the new choice set \mathcal{J}^+ and that under the original choice set J_i , scaled by the distance cost parameter λ .

Definition 1. A student with neighborhood school $j(i)$ whose choice set expands to \mathcal{J}^+ has an OVG defined as

$$OVG_i = \frac{1}{\lambda} \left(E[\max_{k \in \mathcal{J}^+} U_{ik}] - E[\max_{k \in J_i} U_{ik}] \right).$$

With i.i.d. extreme-value type I errors,

$$OVG_i = \frac{1}{\lambda} \left(\ln \left(\sum_{k \in \mathcal{J}^+} e^{V_{ik}} \right) - \ln \left(\sum_{k \in J_i} e^{V_{ik}} \right) \right).$$

Viewed from the demand side, OVG is a measure of a student's expected welfare gain in terms of distance, under the assumption that every option is equally accessible (Train 2009). Intuitively, a student with high OVG gains access to relatively popular schools and values them highly after netting out distance cost differences; these students are likely to access new schools. For students with low OVG, either they gain access to schools that are less popular than their local school or cost factors make the new schools unattractive; in either case, these students are less willing to access new schools.

The expected welfare gain statistic has an alternative, but qualitatively similar, interpretation when incorporating it into the model of school quality provision. To see this, first define $\Delta_{ijk} \equiv V_{ij} - V_{ik}$. Then we can express the probability of student i enrolling in school j in terms of their OVG:

$$P_{ij} = \begin{cases} e^{-\lambda OVG_i - \lambda OVG_{i0}} & \text{if } j(i) = j \\ e^{\Delta_{ijj'} - \lambda OVG_i - \lambda OVG_{i0}} & \text{if } j(i) = j' \neq j. \end{cases}$$

Here, $OVG_{i0} = \frac{1}{\lambda} \left(\ln(1 + e^{V_{ij(i)}}) - V_{ij(i)} \right)$ is student i 's fixed outside option OVG, while OVG_i is the OVG from expanding the choice set from J_i to \mathcal{J}^+ . P_{ij} are decreasing in OVG, indicating that students with high OVG_i who gain access to more preferable schools are more likely to enroll in non-neighborhood schools. This intuition can be extended to constructing school market shares:

$$S_j = \frac{1}{N} \left(\underbrace{\sum_{j(i)=j} e^{-\lambda OVG_i - \lambda OVG_{i0}}}_{\text{Neighborhood } j \text{ students}} + \underbrace{\sum_{k \neq j} \sum_{j(i)=k} e^{\Delta_{ijk} - \lambda OVG_i - \lambda OVG_{i0}}}_{\text{Other students in } \mathcal{J}} \right). \quad (1)$$

From this perspective, we can think about a setting in which the choice set expands by one additional school and the heterogeneity of students and schools will generate different reductions in market shares across incumbent schools. Baseline differences in OVG capture differences in implied competitive pressure at the onset of the program, serving as a competition index summarizing differences in competitive incentives.

To complete the model, we now discuss the existence of an equilibrium. The introduction of ZOC introduces a strategic effort game among principals in \mathcal{J} . Whereas principals $j \notin \mathcal{J}$ still independently maximize their utility subject to the draw of students in their zones, principals $j \in \mathcal{J}$ choose a best response level of effort in anticipation of other principals' $j \in \mathcal{J}$ best responses. The following proposition demonstrates that there is an equilibrium to the principal effort game that ZOC introduces.

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

$$e^{BR}(e) = \left(e_1(e_{-1}, e)^{BR}, \dots, e_J(e_{-J}, e)^{BR} \right).$$

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

Proof. See Online Appendix B. □

III.A Empirical Map

The framework presented above generates stylized predictions that govern the rest of the empirical analysis. The first implication relates to classic notions of competitive effects in education (Friedman 1955; Hoxby 2003), in which schools exposed to more competition differentially improve to sustain their demand.⁵

⁵The implications rely on two additional assumptions: first, each affected school must initially serve at least 50 percent of students in their coverage area, a neighborhood monopoly assumption that is verified in the data. Second, the quality elasticity of demand for each student must be sufficiently high to produce the proposed impacts on quality differentials within zones. We believe these assumptions are reasonable. Alternative models of competition, such as McMillan (2004), lead to reductions in school productivity. In this class of model with two types, H and L and an assumption that costs of educating high types is higher, there can be instances where more competition leads to reductions in school productivity. The lack of socioeconomic diversity in ZOC neighborhoods coupled with the fact that costs of education low types tend to be higher (Augenblick, Myers and Anderson 1997) assuages concerns about perverse incentives in the ZOC setting.

Implication 1. *For each $j \in \mathcal{J}$, the change in school quality is*

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

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

$$\Delta\alpha_j = 0.$$

We use a difference-in-differences design comparing changes in achievement between ZOC students and non-ZOC students to evaluate this implication empirically. To more plausibly isolate changes in school quality, we estimate a generalized value-added model (Abdulkadiroğlu et al. 2020) that allows us to decompose achievement effects into treatment effects on schools’ value added and treatment effects on student-school match quality. Changes in match quality imply students sort more effectively into schools that suit their particular needs, while competitive effects imply differential changes in α_j . Differentiating between these two effects is important empirically as it provides additional information about the source of the gains.

Implication 2 incorporates OVG into the empirical analysis. In particular, it tests for the presence of competitive effects.

Implication 2. *School quality $\alpha_j = f(e_j^{BR}(e_{-j}, e))$ is increasing in OVG for each school j .*

OVG is an index that summarizes the expected welfare gain to students from an expansion in their choice sets. But from a school’s perspective, the relative popularity of other schools at the onset of the program—captured by OVG—will induce differential responses to the program. For example, and through the lens of the model among two identical schools, the one exposed to relatively more popular schools—and thus exposed to students with higher OVGs—will experience a larger improvement in its quality. These observations allow us to interpret OVG as an index of competition. We leverage student- and school-level variation in OVG to construct empirical tests for the presence of competitive effects.

IV Data

Our analysis draws from three sources of data. We start with LAUSD data covering school enrollment, student demographics, home addresses, and standardized test scores for all students enrolled in the district between 2008 and 2019. These data are merged with ZOC data (provided by the ZOC office) consisting of centralized assignments and rank-ordered preference submissions from all applicants between 2013 and 2020. Last, we link National Student Clearinghouse (NSC) data and observe college outcomes for cohorts of students graduating between 2008 and 2019. We create several samples in our analysis: a market-level sample, a matched market-level sample, and a lottery sample.

IV.A Analysis Samples

The main sample covers LAUSD students and schools for the years 2008–2019 and does not include data on charter school students in Los Angeles County.⁶ We begin by restricting to student-level observations in 11th grade, the grade-year with continuous testing throughout the sample period. Besides the grade restriction, we do not impose other student-level restrictions in the sample selection.⁷

We then impose additional restrictions at the school level, restrictions that are identical for both ZOC and non-ZOC schools. We exclude continuation, special education, or magnet schools without strict neighborhood assignment boundaries.⁸

Next, we restrict to schools that are open before the ZOC expansion to ensure we have a balanced set of schools before and after the expansion. In some zones, large high school complexes house multiple programs and schools. For the purposes of the evaluation, we consider a program a different school if there is a distinct identifier the district uses for that program.⁹ For the purposes of the analysis, we only consider control group students enrolled at any schools we do not omit above; we call this the unmatched sample.

ZOC students are observably different from non-ZOC students, and to attempt to address the unbalanced nature of the two groups, we create a matched market-level sample. We match each school to a non-ZOC comparable school in the same poverty share and Hispanic share deciles, breaking ties with a propensity score discussed in Online Appendix E.1. We refer to this as the matched sample.

IV.B Outcome Data

Our primary outcomes are student achievement and four-year college enrollment. The latter come from the NSC, and the former are provided by LAUSD. There are important factors to mention about the achievement data we use in our analysis. First, there was a moratorium on testing in California in 2014. In response to this, we omit the cohort of students who were in 11th grade in 2014 in any analysis involving achievement outcomes. This feature is unlikely to introduce any complications in the analysis.

⁶Non-affiliated charter schools within Los Angeles County do not report their data to LAUSD, so we do not observe outcomes for charter school students. In supplementary robustness exercises, we use aggregate school-level data from the Common Core data files that the National Center for Education Statistics (NCES) maintains.

⁷A potential concern with focusing on 11th-grade observations with test scores is differential attrition rates out of the sample that could introduce bias in our analysis. In Online Appendix Figure E.13 we report attrition rates over time for ZOC and non-ZOC cohorts. We do not find evidence of differential attrition rates between both cohorts.

⁸There are not any continuation, special education, or magnet schools in ZOC, so this restriction is vacuous for ZOC schools. The restriction therefore imposes similarity of control group schools and ZOC schools. In addition, in our sample there are magnet programs and magnet schools. Many schools have magnet programs nested within the school; we do not drop these schools as most of their enrollment stems from the neighborhood schools and we treat students assigned to these programs as part of the broader school. Standalone magnet schools, a far smaller quantity of schools in LAUSD, are ones we drop as they are not part of the neighborhood-based assignment scheme in the rest of the district. Last, we consider samples that allow for the inclusion of magnet schools in the non-ZOC pool of schools, and the results look qualitatively similar.

⁹Some small or pilot schools within larger high school complexes change their name during the sample period, and this sometimes leads to a change in their identifier. In cases we cannot associate the program with a continuous school or program, we drop it from the sample. Overall, our analysis aims to compare incumbent programs and schools before and after the ZOC expansion.

Second, the state transitioned from the California Standards Test (CST) to the Smarter Balanced Test Assessment Consortium (SBAC) between 2013 and 2015. This is a state-level shock that affected all schools in the state in the same manner. If, however, there were changes in how scores are scaled that disproportionately affects ZOC schools, then one may be concerned that any before and after changes are driven by the changing scale of the score distribution. While we do not have item-level data to check if this is a concern, we complement our analysis with an outcome that is immune from this change: four-year college enrollment.¹⁰ We observe college outcomes for all cohorts in the analysis and do not omit the 2014 cohort in analysis involving college enrollment outcomes.

Third, throughout the analysis we mostly emphasize impacts on ELA (also referred to as reading scores in the text). ELA exams are identical for all 11th-grade students before and after the transition to the SBAC; that is, every cohort of students takes the same exam in their grade-year. As for math, during the CST regime, students took an exam that closely corresponded with their math course enrollment; some students took an exam focusing on algebra, while others took one emphasizing geometry, for example. This introduces ambiguities in comparisons of math achievement across students. For transparency, we report effects on both ELA and math but choose to emphasize effects on ELA scores. Online Appendix A discusses additional data details and reports the set of ZOC schools used in the analysis.

IV.C Descriptive Statistics

Columns 1 and 2 of Table I report mean characteristics for ZOC and non-ZOC cohorts. ZOC students enter high school performing approximately 21–23 percent of a standard deviation more poorly than non-ZOC students in both ELA and math. Most ZOC students are Hispanic, roughly 88 percent or 20 percentage points higher than non-ZOC students. ZOC students are also more socioeconomically disadvantaged than other students in the district. Eighty-five percent are classified as poor by the district, and only 3 percent have parents who graduated from college, 50 percent less than non-ZOC students. Online Appendix Table A.2 reports analogous school-level differences.

We report matched non-ZOC mean characteristics in Column 4 of Table I. The limited pool of schools we can draw from, due to the restrictions imposed above, limits our capacity to eliminate baseline differences between ZOC and non-ZOC students. Thus, the matching strategy mostly eliminates schools with significantly large achievement levels and selects control group schools that more closely reflect the typical school in the district. Importantly, the matching strategy mostly balances English learner status, poverty status, and special education status, factors important for funding within LAUSD. A residual achievement gap of 11–13 percent of a standard deviation remains as students enter high school. This achievement gap serves as a benchmark for our market-level estimates.

¹⁰In Online Appendix A.3 we report a decomposition that attributes the potential share of mean changes attributable to changing score distributions and find suggestive evidence that the change in the exam is not a serious concern.

V Empirical Analysis

V.A Achievement and College Enrollment Effects

We use a difference-in-differences strategy to estimate market-level effects, comparing changes in outcomes between ZOC students and students enrolled at comparable schools. This analysis unpacks how students in one side of the market exposed to choice and competition fared in comparison to other students under neighborhood-based assignments. Our empirical strategy takes into account the dynamic nature of these effects over the short and medium term. As mentioned earlier, we present estimates for both the matched and unmatched samples, but the results are consistent across both groups throughout the analysis.

For a given matched or unmatched sample and student outcome Y_i , such as achievement or four-year college enrollment, we consider the specification

$$Y_i = \mu_{j(i)} + \mu_{t(i)} + \sum_{k \neq -1} \beta_k ZOC_{j(i)} \times \mathbf{1}\{t(i) - 2013 = k\} + \mathbf{X}_i' \psi + u_i, \quad (2)$$

where $\mu_{j(i)}$ and $\mu_{t(i)}$ are school and year fixed effects, $ZOC_{j(i)}$ is an indicator for student i attending a ZOC school, and \mathbf{X}_i is a vector of student characteristics. If both groups' outcomes trend similarly, the coefficients β_k are period- k -specific difference-in-differences estimates capturing the causal impact of ZOC. The design builds in placebo tests that help identify potential violations of the parallel trends assumption: for $k < 0$, a nonzero β_k would suggest a violation of the parallel trends assumption. Throughout, we report standard errors that are clustered at the school level, although the results are robust to two-way clustering that accounts for correlation within schools across years and across schools within a given year. Last, it is important to emphasize that the ZOC expansion is a canonical difference-in-differences setting that is immune from biases discussed in recent literature (Roth et al. 2022).

V.A.I Event-Study Results

Figure IIIa reports estimates of Equation 2 for student achievement on reading exams. The achievement trends for ZOC students are similar to those for non-ZOC students in the years leading up to the expansion of the program, providing support for the parallel trends assumption. We find modest achievement effects for early cohorts of students who were partly affected by the program at the time they took achievement exams in 11th grade. For the first cohort with full exposure to the program, ZOC achievement improved by 0.09σ relative to the improvement among non-ZOC students and continued to improve, leveling out at roughly 0.16σ by the seventh year of the program. Only Appendix Figure E.16 reports math score treatment effects that are nearly identical to ELA treatment effects.¹¹ Importantly, the results look similar in both matched and unmatched samples, indicating our findings are not driven by convenient sample selection introduced by the matching strategy.

¹¹Riehl and Welch (2023) finds that differences in effect sizes across Math and Reading are partly due to differences in incentives teachers/schools face. In our setting, roughly 27-29 and 22-24 percent of ZOC-residing students were marginally proficient in Reading and Math, respectively, as they entered high school. The similarity in proficiency rates suggests that teachers did not have an incentive to disproportionately focus on improving Math instead of Reading performance. This may partly explain the similarity in treatment effects across subjects.

The event-study results for four-year college enrollment are reported in Figure IIIb. Similar to achievement effects, we do not find evidence that college enrollment rates among ZOC students trended differently in the years before the program expansion. College enrollment effects mirror achievement effects in that students less exposed to the program experience smaller effects; by the time of first cohort with full exposure to ZOC, ZOC college enrollment rates improved by an additional 5 percentage points compared with the non-ZOC change.

It helps to benchmark these effects. One way to do this is to compare the treatment effects with the pre-ZOC 11th-grade achievement gaps, which are roughly 0.2σ in the unmatched sample and $0.11\text{--}0.13\sigma$ in the matched sample. This suggests a substantial reduction in within-district neighborhood-based achievement gaps. As for college enrollment effects, the unconditional four-year college enrollment gap was roughly 2 percentage points in the pre-period, making the effect sufficiently large to reverse the four-year college enrollment gap by the end of the sample.

We find that most of the college treatment effects are on enrollment in CSU campuses, with minimal impact on University of California (UC) enrollment, and we find some suggestive evidence of diversion from private universities. Online Appendix Figure E.2 demonstrates that community college enrollment was unaffected. Last, Online Appendix Figure E.3 shows that ZOC high school graduation rates increased by roughly 7–8 percentage points; these effects correspond to a roughly 10–12 percent increase from the baseline mean graduation rate. Although suggestive, the evidence demonstrates that otherwise low-performing students increased their performance on standardized exams, and some were also compelled to graduate high school. Overall, the findings in this section demonstrate that the introduction of public school choice within a large urban district benefited students.

Online Appendix D contains heterogeneity estimates, including distributional estimates and estimates for different subgroups of interest. Most treatment effects are concentrated among lower socioeconomic status Hispanic students, many of whom also had low incoming achievement.

V.A.II Robustness Checks

We begin by demonstrating stable trends in student composition in Online Appendix Figure E.10, assuaging sorting concerns on observable student characteristics. We complement this evidence by showing that our primary estimates are unaffected by students who strategically sort into ZOC schools. We accomplish this by restricting estimates to students who do not move during their middle school tenure; this evidence is reported in Online Appendix Figure E.11 and Online Appendix Figure E.12. This assuages concerns about sorting on unobservables that predict mobility.

While the policy aims to increase within-zone choice, students may be self-selecting into the ZOC sector, introducing additional sorting concerns. An alternative approach to address these concerns is to define treatment at students' eighth-grade neighborhood level, ignoring the decision to enroll in a ZOC school or not. This mirrors the empirical strategies of other school choice reforms (Billings, Deming and Rockoff 2014; Fryer 2014). In particular, we define treatment at the level of students' eighth-grade neighborhood and remain agnostic about the

school that students eventually sort into, an approach that generates intent-to-treat effects. Because we ignore the enrollment decision, this approach is less stringent in the sample selection criteria and includes schools that open post-reform and a wider swath of magnet programs. Online Appendix E.4 discusses additional details about this empirical approach.

Figure IIIc reports event-study evidence from this alternative approach, with findings mirroring the baseline findings with slightly attenuated magnitudes of treatment effects. In contrast to a 0.16σ effect on student achievement by year six in the baseline strategy, the intent-to-treat analysis finds a 0.12σ effect by year six. Similarly, instead of a 0.05 percentage point increase in college enrollment rates, Figure III d reports a 0.036 percentage point increase in college enrollment by year six. Both specifications do not point to differential trends between students who live in ZOC neighborhoods and those who do not before the reform. Alternative specifications discussed further in Online Appendix E.4 find similar results. Through a variety of approaches, we find little evidence that sorting influences our baseline estimates.

In Online Appendix E.5, we further discuss other contemporaneous policies and the role of charter and magnet school competition. We find little evidence to suggest that other contemporaneous policies drive our results (see Online Appendix Figure E.14), and our competition analysis in the following section leverages ZOC-specific variation to further assuage concerns about other correlated policies and shocks. Last, we do not find evidence that ZOC neighborhoods were differentially affected by charter or magnet school competition (see Online Appendix Figure E.4, Online Appendix Figure E.5, Online Appendix Figure E.6, and Online Appendix Figure E.7).

V.B Probing the Role of Competition

The achievement effects show that ZOC student achievement improved at a remarkable pace compared with improvements of students enrolled at similar schools. As of now, there are many factors that could contribute to those findings. If parents chose schools better suited to their children’s needs, then match effects would explain a portion of the gains (Abdulkadiroğlu et al. 2020; Bau 2019; Bruhn 2019). Alternatively, changes in school effectiveness in response to competitive pressure could have contributed to the gains. We decompose the treatment effects to assess the relative role of these margins. We then pivot to assess treatment-effect heterogeneity with respect to baseline school quality to further probe the role of competition.

V.B.I Decomposition of Achievement Effects

Online Appendix C discusses the achievement model we estimate that allows for a decomposition of effects into school and match quality (Abdulkadiroğlu et al. 2020). To start, we focus on treatment effects explained by changes in school quality, commonly referred to as school value-added. Online Appendix Figure E.15a reports event-study estimates isolating that component of achievement. We do not find evidence of differential trends in the pre-period, and in line with the event-study evidence on achievement, we find a clear trend break in ZOC student school effectiveness, accounting for most of the observed achievement effects. The treatment effects displayed in Online Appendix Figure E.15a capture both relative improvements in school quality over time and allocative changes of students to higher quality schools. We find that most of the

effects are captured by improvements in school quality, although we do observe that allocative changes also play a small role.¹²

In contrast, Figure E.15b shows that match effects play a minor role in explaining the observed achievement effects. Again, we find evidence that trends in match quality were similar before ZOC, but the trend break after is much smaller in magnitude. Although parents' scope for choosing more suitable schools expands, we do not find evidence of large gains on this margin.¹³

V.B.II School Effectiveness Treatment Effect Heterogeneity

We now turn to school effectiveness treatment effect heterogeneity. In particular, we ask whether lower-performing schools experienced relatively larger improvements than higher-performing schools. To pinpoint treatment effects at different deciles of the distribution, we estimate unconditional quantile treatment effects using the methods developed in Chernozhukov, Fernández-Val and Melly (2013). This approach amounts to estimating the ZOC value-added CDF and a counterfactual distribution, followed by an inversion of each to obtain the implied unconditional quantile treatment effects. Figure IV reports the implied treatment effects at various quantiles. These estimates clearly show that most gains are concentrated in the bottom half of the school effectiveness distribution, with modest and potentially negative impacts at the top, although we cannot distinguish these from statistical noise.

Piecing the evidence from Sections V.B.I and V.B.II provide suggestive evidence that schools respond to competition, with the schools facing the most pressure improving the most. However, these results partly hinge on families incentivizing schools to care about their contribution to student learning. This motivates a pivot to parents' preferences in the next section, which then allows us to quantify the competition schools faced at the start of the program and directly assess the role of competition.

VI Demand and OVG

Turning to the demand side allows us to assess whether parents' choices are consistent with the supply-side evidence and to further probe the competitive effects interpretation of the results. To study the former, we can relate estimates of school mean utility to measures of school and peer quality to assess the consistency of parents' choices with the supply-side response. To probe for competitive effects, information from rank-ordered preference lists allows us to construct a measure of students' expected welfare gain from the program, a statistic that can also be interpreted as a measure of competitive incentives at the start of the program. Both exercises require us to estimate the demand parameters introduced in the conceptual framework.

¹²Online Appendix Table E.1 reports the details related to this exercise.

¹³There is evidence of substantial match effects in the context of inter-district school choice (Bruhn 2019), but the evidence regarding school match effects is mixed (Abdulkadiroğlu et al. 2020; Bruhn 2019; Bruhn, Campos and Chyn 2023).

VI.A Estimating Demand Parameters

We use rank-ordered preference data submitted by ZOC applicants to estimate demand parameters (Abdulkadiroğlu et al. 2020; Agarwal and Somaini 2020; Beuermann et al. 2023; Hastings, Kane and Staiger 2005). The model in Section III allowed school popularity to vary by student characteristics \mathbf{X}_i , and we incorporate this feature by categorizing students into three baseline achievement cells and allowing school popularity to vary by achievement cell. Student i 's indirect utility from attending school j is

$$U_{ij} = \underbrace{\delta_{jc(i)} - \lambda_{c(i)} d_{ij}}_{V_{ij}} + \varepsilon_{ij},$$

where δ_{jc} summarizes school j 's popularity among students in achievement cell c , d_{ij} is the distance from student i 's residence to school j , and ε_{ij} captures idiosyncratic preference heterogeneity. Importantly, we also allow for heterogeneity in distance costs across covariate cells (Hastings, Kane and Staiger 2005). We normalize $V_{ij} = 0$ for one arbitrary program in each zone.

We estimate the parameters of this model using two estimation approaches, with the key differences being assumptions about strategic behavior in reporting preferences. In either approach, we observe a complete ranking over schools in zone $z(i)$ with varying numbers of schooling options $Z(i)$ across zones, $R_i = (R_{1i}, R_{2i}, \dots, R_{Z(i)i}) \in \mathcal{R}$, where \mathcal{R} is the set of all possible rank-ordered lists.

Our first estimation approach assumes applicants reveal their preferences truthfully and $\varepsilon_{ij} \sim EVT1|\delta_{jc}, d_{ij}$, standard assumptions in the discrete choice literature. With these assumptions, the preference profile for each applicant is as follows:

$$R_{ik} = \begin{cases} \arg \max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg \max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}. \quad (3)$$

From Hausman and Ruud (1987), we know that the conditional likelihood of observing list R_i is

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r|U_{ir} < U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \quad (4)$$

We aggregate the log of Equation 4 across individuals to construct the complete likelihood and to estimate parameters of the utility specification via maximum likelihood.

While this approach allows for relative ease in estimation, a key limitation is the assumption that applicants do not act strategically in stating their preferences. Truthful statements are unlikely if applicants are strategic under an immediate acceptance mechanism (Agarwal and Somaini 2018, 2020) or if they do not understand the mechanism's rules or do have biased beliefs (Kapor, Neilson and Zimmerman 2020). Although strategic behavior is likely in ZOC neighborhoods, we emphasize that schools observe reported preferences—truthful or not—and respond to this demand accordingly. Nonetheless, demand estimates that account for strategic incentives are informative about the potential incentives schools may face under alternative

centralized assignment policies, such as the increasingly popular deferred acceptance mechanism. We estimate an alternate model of demand in Online Appendix F and find qualitatively similar results, so we proceed with the simple model that assumes families do not behave strategically in their reports.

For each estimation approach, we estimate parameters separately for different zone-year-cell combinations, and we use the estimated parameters to estimate preferences for school quality and to construct empirical OVG estimates. To estimate preferences, we relate time-varying estimates of δ_{jct} to measures of school and peer quality to assess the consistency of parents' choices with the supply-side evidence. To construct estimates of OVG, we only use estimates derived from the first cohorts of the program to ensure our measures of competitive incentives more adequately capture demand-side pressures at the start of the program.

VI.B Parents' Valuation of School Effectiveness

In this section, we relate estimates of δ_{jct} to school effectiveness α_{jt} , average school peer quality Q_{jt}^P , and average school match quality Q_{jct}^M implied by the student achievement decomposition presented in Online Appendix C. We estimate

$$\delta_{jct} = \xi_{cz(j)t} + \omega_P Q_{jt}^P + \omega_S \alpha_{jt} + \omega_M Q_{jct}^M + u_{jct}, \quad (5)$$

where ξ_{czt} are cell-by-zone-by-year fixed effects. Mean utilities, peer quality, treatment effects, and match effects are scaled in standard deviations of their respective distributions so that the estimates can be interpreted as the standard deviation change in mean utility associated with a 1 standard deviation increase in a given characteristic. Standard errors are clustered at the zone-by-cell level, but we also report p -values from wild bootstrap iterations that allow for clustering at the zone level. The results are qualitatively similar under both inference approaches.

Table II reports estimates of Equation 5. Columns 1 and 2 of Panel A show that parents exhibit stronger preferences for both higher-achieving peers and effective schools, although preferences for effective schools are more precisely estimated. In particular, a 1 standard deviation increase in school effectiveness is associated with a 0.137 standard deviation increase in school popularity, while a 1 standard deviation increase in peer quality is associated with a 0.116 standard deviation increase in mean utility. In Column 4, we include the three components of the student achievement model and find that parents place relatively more weight on school effectiveness, even when we condition on peer ability.

The results in Panel A correlate mean utilities with measures of school and peer quality but do not consider other school attributes potentially correlated with these measures of quality. Panel B includes additional school-level covariates, including school type indicators, teacher attributes, and course offering attributes to assess the sensitivity of the findings. The key finding that school quality is the strongest predictor of preferences is reinforced after including other school-level covariates. The robustness of the findings is partly explained by the relatively weak correlation between school effectiveness and observable school attributes. Last, in Panel C we consider models that allow non-linearities in distance costs. The preference estimates are robust to this as well.

These findings contrast with findings in other settings, where preference estimates suggest parents place more weight on peer quality than school quality (Abdulkadiroğlu et al. 2020; Ainsworth et al. 2022; Rothstein 2006). In Section VII.B, we discuss some institutional features of ZOC that may contribute to the disparate findings.

VI.C Option Value Gain

Differences in OVG across students can provide further insights into the effects of competition. Through the lens of the model in Section III, schools exposed to students with higher OVG should exert additional effort, so we should expect heterogeneous treatment effects with respect to OVG if schools respond to incentives induced by students' OVG. Evidence of OVG treatment effect heterogeneity would therefore provide support for the competitive effects hypothesis.¹⁴ For the analysis, we classify a student as having high OVG if their estimated OVG is in the top two quartiles of the OVG distribution within their cohort.¹⁵ Importantly, because we know student addresses, we can classify high-OVG students before and after the ZOC expansion and even if they do not eventually enroll in a ZOC school.

Student-level OVG is informative about which students gain access to more popular schools net of distance costs. We may expect a student with higher OVG to experience larger gains because either they switch to a higher-quality program or their neighborhood school experiences a differential improvement due to the relative pressure they face. To explore the extent of these possibilities, we estimate models that leverage differences in OVG across students and schools in various ways. To do this, we augment the difference-in-differences framework from Section V.A with interaction terms that capture functions of student OVG. We consider the following specification:

$$Y_i = \mu_{j(i)} + \mu_{t(i)} + \beta Post_t \times ZOC_{j(i)} + \gamma Post_t \times ZOC_{j(i)} \times f(OVG_i) + \mathbf{X}_i \psi + u_{it}, \quad (6)$$

where $f(OVG_i)$ is a function of student-level OVG, and the vector \mathbf{X}_i includes the same controls as before and is augmented with the main effects for $f(OVG_i)$ students and other relevant interaction terms. We consider $f(OVG_i) = OVG_i$, which we refer to as student-level OVG, $f(OVG_i) = O\bar{V}G_{j(i)}$ where $O\bar{V}G_{j(i)}$ is school-level average OVG, and $f(OVG_i) = OVG_{3,4}$ where $OVG_{3,4}$ is an indicator if a student's estimated OVG is in the top two quartiles of the OVG distribution. The parameters of interest β and γ inform us about ZOC effects, with γ capturing the differential ZOC effect for high-OVG students. The competitive effects hypothesis implies that both $\beta > 0$ and $\gamma > 0$.

Table III reports estimates of OVG treatment effect heterogeneity. Panel A reports heterogeneity estimates with respect to school-level OVG, while Panel B and Panel C report heterogeneity estimates with respect to individual-level OVG. Across the three panels, Column 1 reports estimates of β and γ , both of which suggest that OVG explains a substantial share of the positive achievement impacts documented in Section V.A.I and, importantly, $\gamma > 0$. However,

¹⁴Online Appendix Figure G.1 displays the distribution of OVG across students, and Online Appendix Table G.1 reports OVG correlates.

¹⁵We use OVG estimates implied by the model where the unobserved preference heterogeneity is extreme value type 1. Only under this assumption does OVG have a straightforward empirical analog we can calculate.

the fact that OVG is a non-linear function of observable student characteristics could imply the high-OVG effects are indicative of other sources of treatment effect heterogeneity. Columns 2–6 gradually add interaction terms with other observable characteristics to see whether they can explain the OVG heterogeneity; the OVG interaction terms are remarkably stable across most columns and panels. To further explore the extent to which improvements are driven by particular zones, Column 7 estimates a model with zone-by-year effects, identifying γ from within-zone-by-year variation. The results in the column reveal that even within zones, high-OVG students experienced larger improvements in achievement, a finding that further zooms in on within-zone competition and finds evidence suggesting it played a role. The preferred estimates in Panel C, where student-level OVG is grouped into low- and high-OVG groups, suggest that students with estimated OVG in the top two quartiles experienced sizable additional achievement gains relative to other ZOC students.

Overall, the findings reported in Table III suggest that students who gained access to relatively more popular schools experienced the largest improvements in achievement. The variation induced by OVG allowed us to more plausibly isolate variation in competition at the onset of the program, and the evidence suggests that schools differentially responded to this variation and improved accordingly. Next, we discuss institutional features that may have facilitated these improvements.

VII Discussion

Understanding the precise mechanisms behind the achievement and college enrollment effects in schools is challenging due to limited data. To explore these mechanisms, we take three approaches. Firstly, we examine the role of teaching practices, specifically the no-excuses approach, which has been found to predict treatment effects in both charter and public schools (Angrist, Pathak and Walters 2013; Dobbie and Fryer Jr 2011; Fryer 2014). Second, we utilize additional survey data to gauge students’ perceptions of their teachers’ effort. Third, we investigate intermediary outcomes to understand changes in student behavior that may precede the observed impacts on test scores and college enrollment. To conclude, we discuss specific features of ZOC schools that may have contributed to the competitive effects we have identified.

VII.A Additional Mechanisms and Intermediate Outcomes

Prior work suggests that discipline is a significant factor in the no-excuses approach. We observe an increase in suspension incidents, indicating a change in disciplinary practices and a possible shift in school philosophy. Panel A of Table IV reports effects on student-level suspension incidents. Column 3 demonstrates that ZOC and non-ZOC suspension rates were on similar trends before the policy expansion, and Column 4 reports difference-in-differences estimates. In terms of the extensive margin, suspension incidents increase by roughly 5 percentage points, amounting to a 31 percent increase from the baseline mean. Looking at the intensive margin reveals a qualitatively similar pattern; an increase of 0.06 suspension days per student, amounting to a 28 percent increase from the baseline mean. Consistent with the notion of increased expectations—also correlated with no-excuses practices—we find reductions in absenteeism, also documented

by Imberman (2011) for start-up charter schools. These findings mirror Angrist, Pathak and Walters (2013) in that effective urban charter schools impact achievement, disciplinary incidents, and attendance. This evidence suggests that teaching practices sharply changed between ZOC and non-ZOC schools.¹⁶

We next analyze students’ perceptions of teacher effort using the School Experience Survey. Online Appendix Figure G.3 shows that ZOC students experienced a greater increase in the belief that teachers help them with coursework compared to non-ZOC students. Any potential changes in student perceptions can reflect either genuine changes in teacher effort in response to changed incentives (Barlevy and Neal 2012; Biasi 2021) or changes in schooling practices perceived as changes in effort. Although this does not inform us about what teachers or schools did, it is reassuring to find evidence that ZOC students perceived a change relative to non-ZOC students.

Finally, we examine intermediate outcomes related to college preparation. Panel B of Table IV shows that ZOC students’ UC and CSU course requirements increase, which contributes to college enrollment impacts. While SAT-taking rates do not change significantly, SAT scores improve for those who do take the test, with increases amounting to a roughly 0.16σ increase in SAT scores.¹⁷ These findings suggest that ZOC students adjust their class choices and effort, leading to improved college readiness.

In summary, our analysis suggests changes in schooling practices that mediate the treatment effects observed. These changes involve teacher effort, school philosophy, and various dimensions of educational practices.

VII.B Institutional Features of ZOC

Parents’ choices and preferences, discussed in Table II, potentially created the right incentives for schools to improve student learning. In this section, we briefly discuss some institutional features that may have helped pave the way for the array of findings in this paper.

First, it is important to emphasize the lack of choice overload hypothesized to create settings that potentially attenuate competitive incentives (Beuermann et al. 2023; Corcoran et al. 2018). ZOC choice sets include at most five campuses to choose from, a significant reduction in comparison to choice settings in New York City, for example. This creates a setting where it is more feasible to adequately learn about all schooling options.

An often-advanced hypothesis for parents’ modest preferences for school quality relates to information barriers. Campos (2023) investigates the severity of information frictions in ZOC markets by first teaching families about school and peer quality and their differences and then subsequently eliciting beliefs before information provision. The typical ZOC parents’ beliefs tend to be not too distinct from the truth, indicating information frictions are not too severe.

Last, one notable feature of the ZOC setting is the homogeneity of students within each

¹⁶Student satisfaction does not appear to be negatively affected by the changes in school philosophy. Online Appendix Figure G.3 reports treatment effects on students’ perceived satisfaction and shows that, if anything, ZOC students report higher rates of satisfaction following the policy expansion.

¹⁷The mean SAT score in California in 2017 (the last year of SAT score data in our sample) was 1055 with a standard deviation of 186. Therefore, an increase in the total SAT score of 30.34 points amounts to a 0.16σ increase in SAT scores.

zone, eliminating the selection of schools based on income or race. If income and race were characteristics that parents use to proxy for effective schools, this would give rise to a more salient preference for peer quality. The relative homogeneity of students within zones is one potential reason why the ZOC preference estimates contrast with those in other settings, and as a byproduct, incentivizes schools to compete on quality. While competition helped produce positive short- and medium-run effects, there is a growing body of evidence pointing to adverse impacts of segregated schools or positive impacts of desegregating schools (Billings, Deming and Rockoff 2014; Card and Rothstein 2007; Johnson 2011). It remains unclear whether racially isolated K-12 education might have adverse effects on ZOC students. Furthermore, it is an open question whether similar programs integrating students across different racial and income levels would yield similar effects.

VIII Conclusion

Schools play a pivotal role in shaping children’s lives, and school assignment policies are important as they significantly influence educational equity, diversity, resource allocation, and overall student outcomes. At the forefront of the K-12 policy discussion is whether students are better off under traditional neighborhood-based assignment or if they benefit from more centralized systems of choice.

This paper studies the transition from neighborhood-based assignment to a version of centralized assignment, a program referred to as Zones of Choice (ZOC). This provides a rich setting to study the market-level effects of choice and competition among public schools, and the rich data arising from the centralized assignment system permit a thorough analysis of both parental demand and the incentives governing the supply-side response.

We show that ZOC has led to gains in student achievement and four-year college enrollment rates, both sufficiently large to close existing achievement and college enrollment gaps between ZOC students and other students in the district. Consistent with the competitive effects conjecture, changes in schools’ value added explain most of the achievement effect, and changes in match quality are small. Importantly, the program’s effects operate mostly through market-level changes as opposed to individual effects experienced by those necessarily exercising choice. These findings are consistent with demand estimates that indicate parents place more weight on school effectiveness than on peer quality, suggesting that ZOC schools are incentivized to improve. Using a measure of competition derived from applicant preferences, we show that treatment effects are largest for schools facing the greatest pressure to improve. Therefore, through various avenues, we find evidence that schools improved because of increased competition.

Collectively, our findings reveal that neighborhood-based public school choice programs can elevate students’ educational outcomes, but they also raise several questions. While we find empirical evidence supporting multiple predictions of stylized models of school demand and competition, our model does not inform us about what produces the predicted gains and does not speak to potentially adverse long-run effects of racial and economic segregation of students. The mechanisms through which schools adjust, the factors contributing to parents’ ability to distinguish between effective and ineffective schools, and the long-run effects of the program are

important topics for future research.

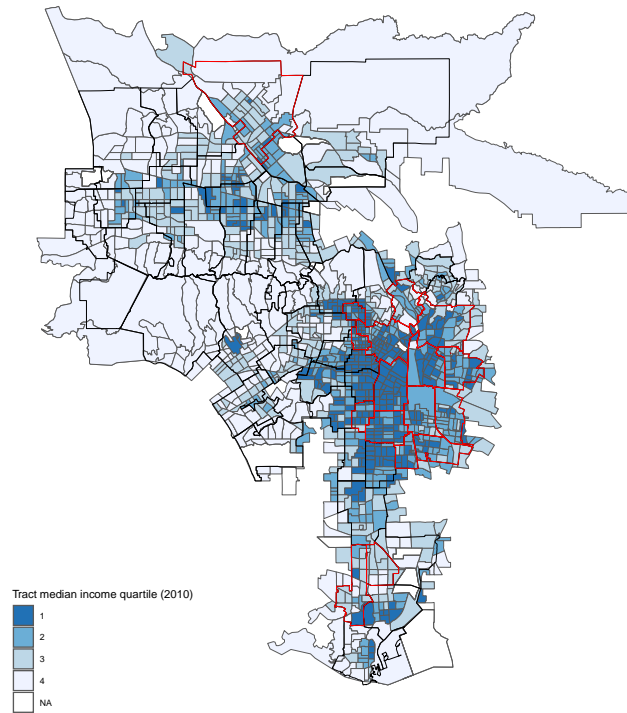


Figure I

ZOC and 2010 Census Tract Income

Notes: This figure plots census tracts across Los Angeles County. Each census tract is shaded according to the median income quartile they belong to in 2010, across all other census tracts in Los Angeles County. High school and ZOC attendance zone boundaries are overlaid on top, with ZOC boundaries outlined in red.

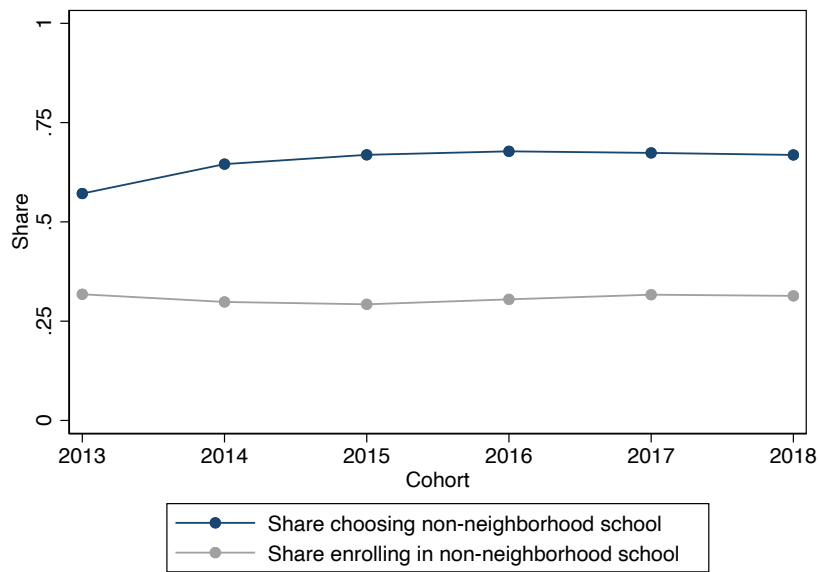


Figure II

Demand and Enrollment for Non-Neighborhood Schools

Notes: This figure reports statistics concerning application behavior of ZOC applicants. If we observe a ZOC applicant enroll in an LAUSD high school in ninth grade, we classify them as staying in the district. If we observe a ZOC applicant rank a school other than their neighborhood school as their most preferred option, we say they chose a non-neighborhood school. If we observe a student enroll in a school that is not their neighborhood school, we say they enrolled in a non-neighborhood school. We determine neighborhood schools based on students' addresses and attendance zone boundaries in 2011.

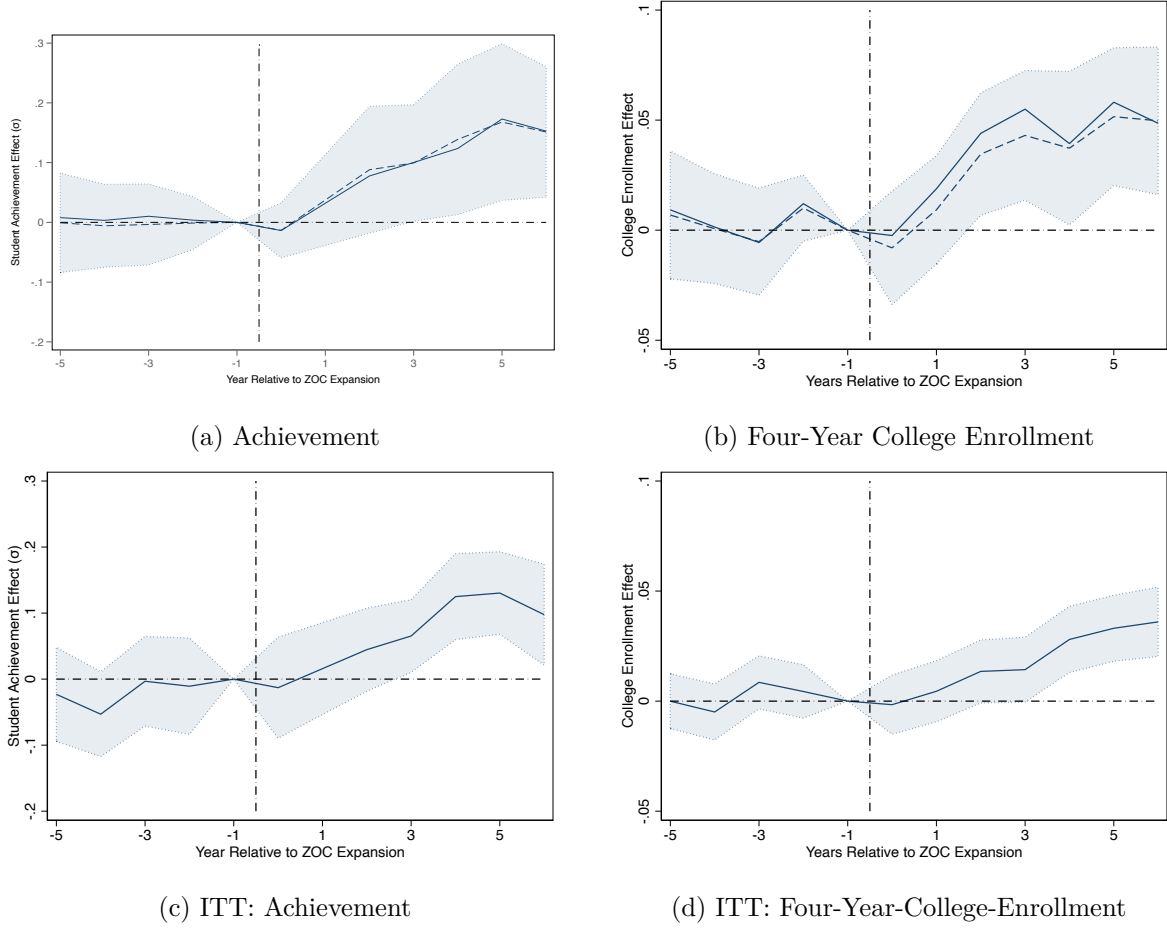


Figure III

Market-Level Effects

Notes: Panel A and Panel B of 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 outcomes relative to the year before the policy. The dashed blue line in Panel A traces out estimates in the matched sample, and the solid line corresponds to estimates from the unmatched sample. 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 school level, and 95 percent confidence intervals are displayed by the shaded regions. Panel C and Panel D report intent-to-treat estimates where the treatment is assigned at the neighborhood level as opposed to the school level. The neighborhood is determined by a students' middle school address. This is discussed in detail in Online Appendix E.4. For Panel C and Panel D, standard errors are clustered at the attendance zone level, and 95 percent confidence intervals are displayed by the shaded regions.

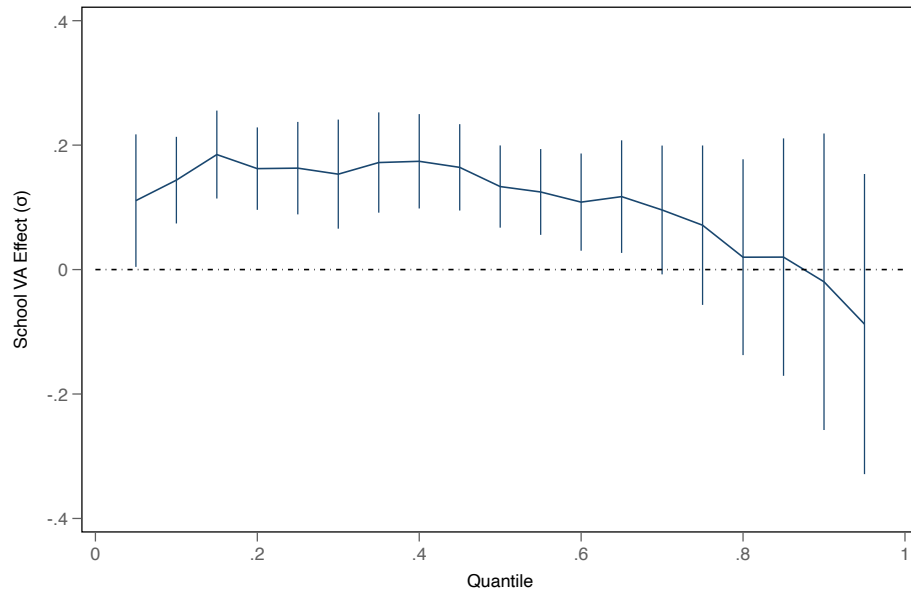


Figure IV

Quantile Treatment Effects on School Effectiveness

Notes: This figure reports unconditional quantile treatment effects estimated by inverting both the observed ZOC average treatment effect) distribution and the estimated counterfactual distribution in the final year of our sample and using methods outlined in Chernozhukov, Fernández-Val and Melly (2013); Chernozhukov et al. (2020). Bootstrapped standard errors are used to construct 95 percent confidence regions.

Table I
ZOC and Non-ZOC Student Characteristics, 2013–2019

	(1) ZOC	(2) Non-ZOC	(3) Difference	(4) Matched Non-ZOC	(5) Difference
8th Grade ELA Scores	-.055	.175	-.23*** (.05)	.077	-.132*** (.047)
8th Grade Math Scores	-.039	.177	-.216*** (.048)	.075	-.114*** (.043)
Missing Any Lagged Test Score	.152	.183	-.032** (.015)	.192	-.04** (.017)
Black	.041	.11	-.069*** (.024)	.119	-.078*** (.029)
Hispanic	.879	.672	.207*** (.044)	.718	.161*** (.045)
White	.018	.111	-.092*** (.019)	.085	-.066*** (.017)
English Learner	.102	.077	.025** (.011)	.084	.018 (.013)
Special Education	.032	.032	.001 (.002)	.032	0 (.002)
Female	.506	.509	-.003 (.01)	.507	-.001 (.01)
Migrant	.155	.165	-.011 (.012)	.161	-.007 (.014)
Spanish at home	.741	.548	.193*** (.045)	.591	.15*** (.047)
Poverty	.852	.775	.077*** (.024)	.805	.047* (.024)
Parents College +	.029	.061	-.032*** (.008)	.047	-.018*** (.007)
Students	53437	82421		61902	

Notes: 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. Column (4) reports group means for the set of students enrolled in matched schools and thus consists of the control group in the empirical analysis. Column (5) reports the difference between Column (1) and Column (4), with a standard error in parentheses below the mean difference. Eighth-grade Math and ELA scores correspond to CST scores before 2014 and to SBAC after 2014. English Learner is defined to be one if a student is flagged as having any English learner status. Special Education is defined to be one if a student has any special education status. Migrant is defined to be one if the student is flagged as having a birth country other than the United States; it is self-reported. Spanish at home is defined to be one if a family reports speaking Spanish at home as the primary language. Poverty is defined to be one if a student is enrolled in a Community Eligibility (CEP) school, and if they are not, it is defined to be one if the student is a free or reduced-price lunch student. Parents College + is defined to be one if at least one parent reports having earned a bachelor's degree or higher. All standard errors are robust and clustered at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table II
Preferences for School Attributes

	(1)	(2)	(3)	(4)
Panel A: Baseline Rank-ordered Logit Estimates				
School Quality	0.137*** (0.0365) [0.035]			0.129*** (0.0358) [0.071]
Peer Quality		0.116 (0.135) [0.645]		0.0393 (0.139) [0.967]
Match Quality			0.118 (0.108) [0.211]	0.0495 (0.0699) [0.233]
R-squared	0.440	0.429	0.437	0.431
Panel B: Rank-ordered Logit + School Controls				
School Quality	0.138*** (0.0385) [0.057]			0.151*** (0.0412) [0.056]
Peer Quality		-0.0522 (0.100) [0.880]		-0.129 (0.0904) [0.489]
Match Quality			0.0678 (0.0865) [0.378]	0.0564 (0.0682) [0.128]
R-squared	0.660	0.651	0.653	0.647
Panel C: Rank-ordered Logit + School Controls + Quadratic Distance				
School Quality	0.134*** (0.0375) [0.057]			0.147*** (0.0402) [0.073]
Peer Quality		-0.0652 (0.100) [0.815]		-0.134 (0.0914) [0.513]
Match Quality			0.0665 (0.0864) [0.369]	0.0524 (0.0682) [0.1331]
Observations	596	596	596	596
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 δ_{jct} estimates from rank-ordered logit models, and Panel B augments the regression models with time-varying school attributes and characteristics. Panel C uses mean utilities estimated from models with quadratic distance costs and also includes time-varying school attributes as controls. The school attributes and characteristics include STEM, social justice, college academy, art, and business program indicators, along with teacher attributes and school-level course offering attributes. Each observation is weighed by the inverse of the squared standard error of the mean utility estimate. Standard errors are clustered at the cell-by-zone level and are reported in parentheses. Numbers in brackets report p -values from wild bootstrap iterations for models that cluster errors at the zone level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table III
Option Value Gain and Treatment Effect Heterogeneity

	(1) Reading	(2) Reading	(3) Reading	(4) Reading	(5) Reading	(6) Reading	(7) Reading
	Panel A: School-level OVG						
PostZOC	0.085** (0.041)	0.080* (0.043)	0.043 (0.054)	0.063 (0.053)	0.083** (0.041)	0.076 (0.060)	
PostZOC \times SchoolOVG	0.002*** (0.001)	0.002*** (0.001)	0.002*** (0.001)	0.002** (0.001)	0.002*** (0.001)	0.001 (0.001)	0.001 (0.001)
	Panel B: Individual-level OVG						
PostZOC	0.096*** (0.035)	0.091** (0.037)	0.053 (0.049)	0.074 (0.047)	0.093*** (0.035)	0.087 (0.056)	
PostZOC \times OVG	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)
	Panel C: Individual-level Aggregated OVG						
PostZOC	0.084** (0.036)	0.078** (0.038)	0.045 (0.051)	0.069 (0.048)	0.081** (0.036)	0.081 (0.057)	
PostZOC \times $OVG_{3,4}$	0.153*** (0.028)	0.153*** (0.028)	0.149*** (0.027)	0.146*** (0.027)	0.153*** (0.028)	0.090*** (0.024)	0.088*** (0.024)
Gender		X				X	X
Race/Ethnicity			X			X	X
SES				X		X	X
Lagged Test Scores					X	X	X
Zone-Year FE							X
Observations	221,954	221,954	221,954	221,954	221,954	221,954	221,954

Notes: This table reports estimates from difference-in-differences regressions with the same controls as event-study models from Equation 2 and additional interaction terms for option value gain (OVG) heterogeneity. Panel A reports treatment effect heterogeneity estimates with respect to school-level OVG, where OVG is aggregated at the school level. Panel B reports heterogeneity estimates where OVG is at the individual level. Last, Panel C reports heterogeneity estimates where $OVG_{3,4}$ is an indicator for a student's presence in the top two quartiles of the student OVG distribution. This final aggregation summarizes the heterogeneity estimates by creating a course grouping of high- and low-OVG students. All estimates include main effects for student OVG, lagged test scores, and all relevant interaction terms necessary to identify the triple interaction coefficient of interest. Standard errors are robust and clustered at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table IV
Additional Mechanisms and Intermediate Outcomes

	(1) N	(2) \bar{Y}	(3) Pre \times ZOC	(4) Post \times ZOC
Panel A: Behavior				
Suspension Incidents	314,808	0.149	0.006 (0.024)	0.046** (0.019)
Suspension Days	314,808	0.208	-0.003 (0.035)	0.059** (0.025)
Total Absent Days	314,808	32.620	-2.013 (1.578)	-3.554* (2.182)
Panel B: College Preparation				
Met UC-CSU Requirements	314,808	0.521	0.015 (0.015)	0.030* (0.017)
Took SAT	314,808	0.425	-0.012 (0.015)	0.008 (0.015)
SAT Score	100,600	1296.015	9.905 (8.310)	30.348*** (6.606)
Math SAT Score	100,600	435.611	3.346 (3.265)	9.615*** (2.416)
Verbal SAT Score	100,600	429.842	3.213 (2.846)	8.721*** (2.263)
Writing SAT Score	87,225	430.562	4.030 (2.746)	7.231*** (2.193)

Notes: This table reports difference-in-difference estimates for a variety of models. Each row corresponds to estimates from a separate regression of the row variable on school indicators, year indicators, pre-period indicators interacted with ZOC indicators, and post indicators interacted with ZOC indicators. The left out year is the year before the policy expansion. Column 2 reports outcome means in the year before the policy expansion, Column 3 reports the pre-trend term and Column 4 reports the difference-in-difference estimates in the treatment period. Panel A reports estimates for behavioral outcomes. Suspension incidents, Suspension days, and Total Absent Days are aggregated across Grade 9 to Grade 11. Panel B reports estimates of effects on college preparation. The first outcome is an indicator for satisfying University of California (UC) and California State University (CSU) college application requirements. Took SAT is an indicator for a student taking the SAT at any point during their high school tenure. SAT score outcomes correspond to the max SAT scores; very few students in the sample take the SAT more than once. Standard errors are robust, clustered at the school level, and reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

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Online Appendix for:
**The Impact of Public School Choice:
Evidence from Los Angeles' Zones of Choice**

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June 2023

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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 character-

istics, 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

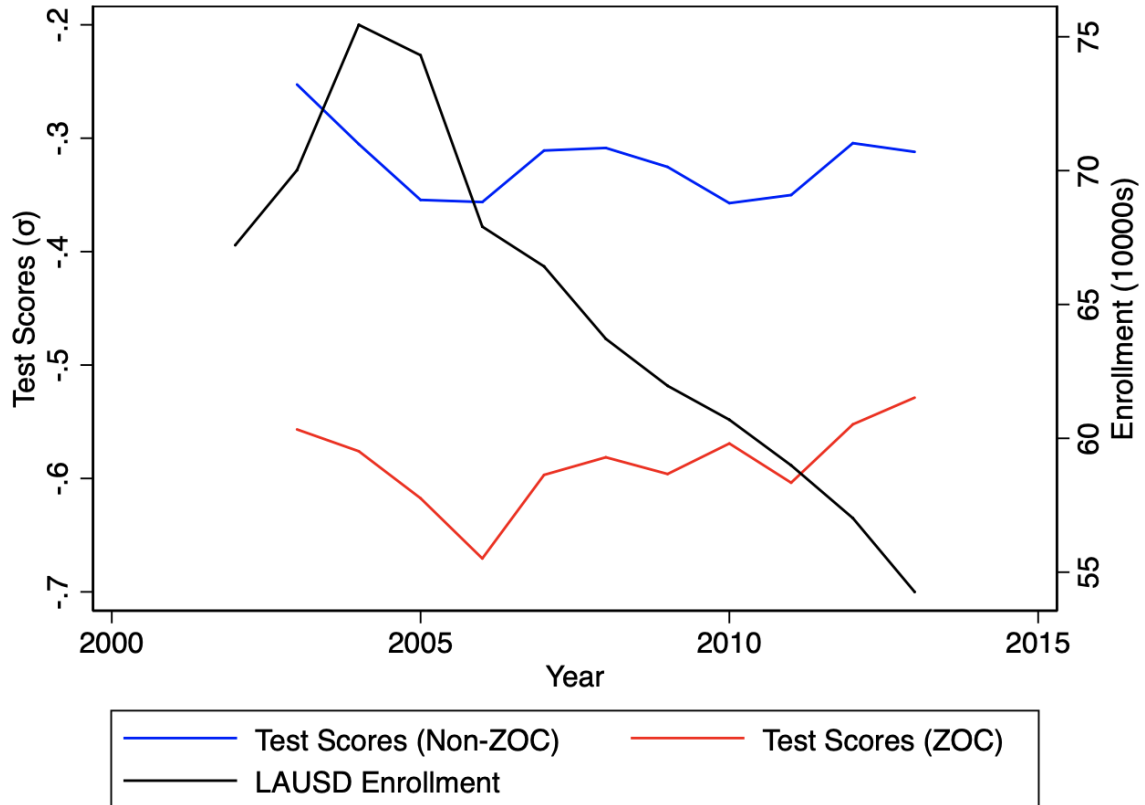
	(1) ZOC	(2) Non-ZOC	(3) Difference	(4) Matched Non-ZOC	(5) Difference
8th Grade ELA Scores	-.11	.12	-.23*** (.057)	.031	-.125** (.054)
8th Grade Math Scores	-.069	.129	-.198*** (.056)	.042	-.098* (.053)
8th Grade Math Scores	.139	.159	-.019 (.019)	.153	-.014 (.018)
Black Share	.034	.141	-.107*** (.028)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



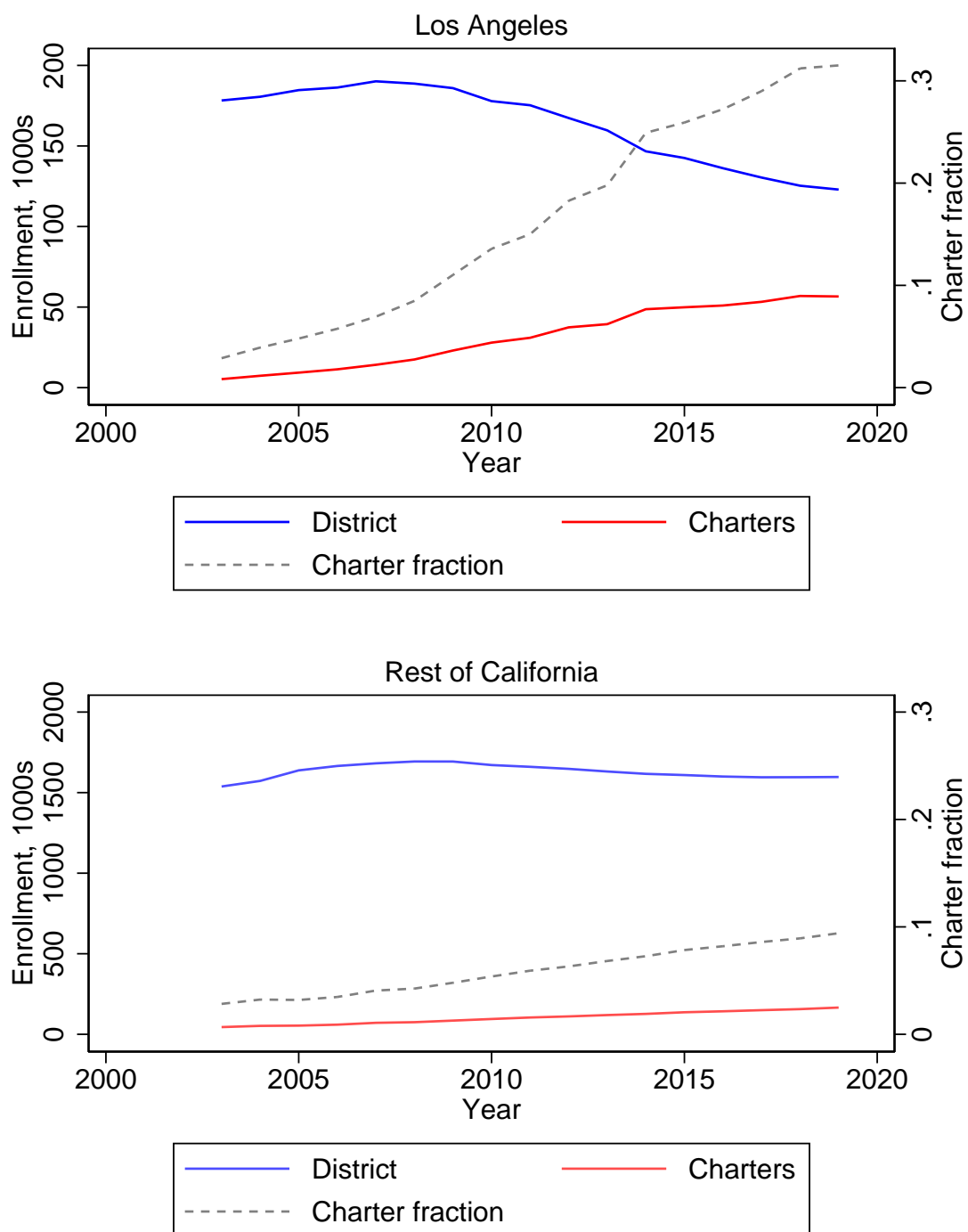
Notes: Enrollment numbers come from administrative data provided by Los Angeles Unified School District (LAUSD). The California Department of Education provides California Standards Test statewide means and standard deviations, which we use to standardize test scores in this figure. Test scores are ninth-grade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

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

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

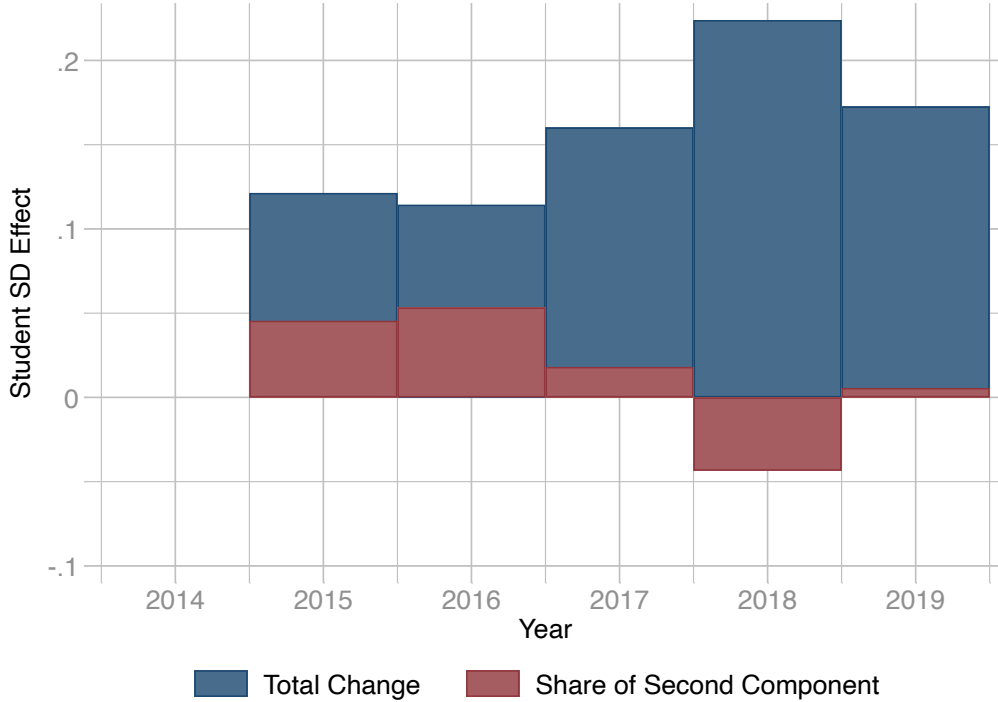
This implies that the difference-in-differences estimates are:

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

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

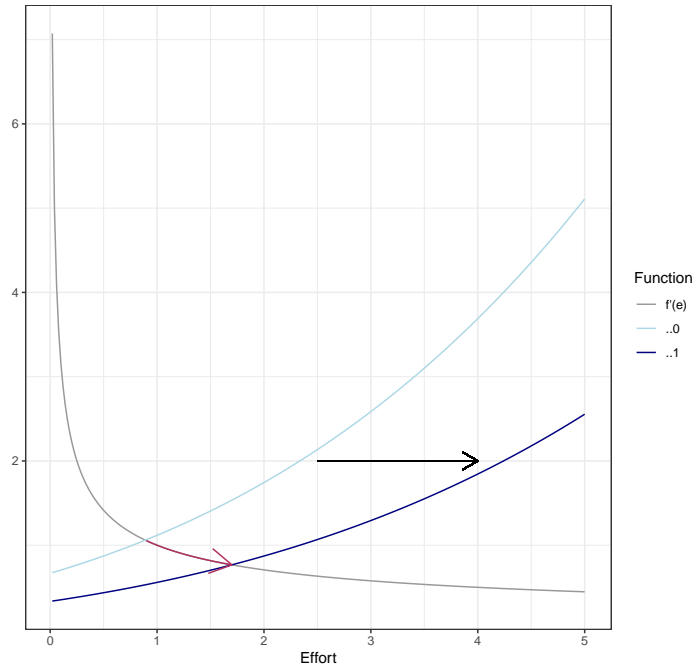
$$\Phi(e_j) = \frac{1}{\theta\omega \frac{1}{N} \sum_i P_{ij}(e_j; d_{ij}, X_i)(1 - P_{ij}(e_j; , d_{ij}, X_i))},$$

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:*

$$e^{BR}(e) = \left(e_1(e_{-1}, e)^{BR}, \dots, e_J(e_{-J}, e)^{BR} \right).$$

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 $k \neq j$:

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

□

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 $\eta_{ij}^1 > \frac{P_{ij}^0}{P_{ij}^1} \eta_{ij}^0$, 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

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

$$\begin{aligned}
&= \theta \left(\frac{1}{N} \sum_{i \in \mathcal{J}} P_{ij}^1 \eta_{ij}^1 - \frac{1}{N_j} \sum_{i:j(i)=j} P_{ij}^0 \eta_{ij}^0 \right) \\
&> \theta \left(\frac{1}{N} \sum_{i \in \mathcal{J}} P_{ij}^1 \frac{P_{ij}^0 \eta_{ij}^0}{P_{ij}^1} - \frac{1}{N_j} \sum_{i:j(i)=j} P_{ij}^0 \eta_{ij}^0 \right) \\
&= \frac{1}{N_j} \sum_{i:j(i) \neq j} P_{ij}^0 \eta_{ij}^0 \\
&> 0.
\end{aligned}$$

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:

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

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 $\Phi_j(e_j^{BR}(e_{-j}, e), e_{-j})$ and $f'(e_j^{BR}(e_{-j}, e))$, where $e_j^{BR} > e_{j0}$. □

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

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

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

Therefore, for a marginal increase in \mathbf{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 \mathbf{X}_i and school effects α_j yields¹

$$A_{ij} = \alpha_j + \mathbf{X}_i' \beta_j + u_{ij}, \quad (1)$$

where u_{ij} has a mean of zero and is uncorrelated with \mathbf{X}_i by construction. The vector of student characteristics \mathbf{X}_i is normalized $E[\mathbf{X}_i] = 0$ 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 \mathbf{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 :

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

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:

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

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 $\mathbf{X}_i'(\beta_j - \bar{\beta})$ 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} :

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

This assumes that assignments to schools are as good as random, conditional on \mathbf{X}_i . The vector of covariates \mathbf{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

¹We suppress time indices for notational ease.

²For 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 \hat{A}_i . To test for bias, we treat \hat{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:

$$A_i = \xi + \phi \hat{A}_i + \sum_{\ell} \kappa_{\ell} Z_{i\ell} + \mathbf{X}_i' \delta + \varepsilon_i \quad (3)$$

$$\hat{A}_i = \psi + \sum_{\ell} \pi_{\ell} Z_{i\ell} + \mathbf{X}_i' \xi + e_i. \quad (4)$$

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 (.105) [0]	1.111 (.134) [.41]	1.024 (.112) [.830]
First-Stage F	277.507	37.016	17.8
Bias Tests:			
Forecast Bias (1 d.f.)	12.528 [0]	.683 [.409]	.046 [.831]
Overidentification (180 d.f.)	172.281 [.647]	187.744 [.331]	176.74 [.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		Difference
	Mean (1)	SD (2)	Mean (3)	SD (4)	
Female	.058 (.041)	.041 (.005)	.032 (.006)	.069 (.014)	.026*** (.008)
Black	-.146 (.288)	.288 (.045)	-.098 (.017)	.191 (.017)	-.048 (.042)
Hispanic	-.053 (.165)	.165 (.022)	-.048 (.013)	.152 (.014)	-.005 (.026)
English learner	-.44 (.135)	.135 (.016)	-.229 (.02)	.23 (.015)	-.211*** (.027)
Poverty	.008 (.066)	.066 (.01)	.009 (.011)	.122 (.032)	-.001 (.014)
Migrant	-.03 (.069)	.069 (.007)	-.001 (.007)	.076 (.01)	-.029** (.011)
Parents College +	.02 (.131)	.131 (.021)	.016 (.009)	.105 (.008)	.004 (.02)
Spanish spoken at home	.073 (.074)	.074 (.009)	.013 (.007)	.081 (.007)	.059*** (.012)
Lagged ELA Scores	.48 (.052)	.052 (.005)	.348 (.015)	.169 (.013)	.132*** (.016)
Lagged Math Scores	.107 (.04)	.04 (.004)	.064 (.007)	.082 (.009)	.042*** (.009)
8th Grade Suspensions	.009 (.045)	.045 (.007)	-.002 (.004)	.041 (.005)	.011 (.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		Difference	ZOC	Non-ZOC	Diff-in-Diff
	Mean (1)	SD (2)	Mean (3)	SD (4)		Mean (6)	Mean (7)	
Female	0.041	0.052	0.040	0.075	0.001 (0.011)	0.053	0.037	0.016 (0.018)
Black	-0.216	0.246	-0.224	0.434	0.008 (0.057)	0.017	0.044	-0.027 (0.061)
Hispanic	-0.191	0.261	-0.171	0.316	-0.020 (0.049)	0.116	0.097	0.019 (0.049)
English Learner	-0.458	0.122	-0.422	0.210	-0.036 (0.028)	-0.368	-0.170	-0.198*** (0.038)
Poverty	0.061	0.109	0.040	0.105	0.021 (0.019)	-0.040	-0.038	-0.002 (0.020)
Migrant	0.015	0.064	-0.006	0.115	0.021 (0.015)	-0.026	0.014	-0.040** (0.017)
Parents College +	0.012	0.155	-0.009	0.161	0.022 (0.028)	0.019	0.059	-0.040 (0.037)
Spanish Spoken at Home	0.071	0.056	0.036	0.051	0.035*** (0.010)	-0.008	-0.001	-0.007 (0.011)
Lagged ELA Scores	0.632	0.101	0.601	0.140	0.031 (0.020)	-0.012	-0.038	0.026 (0.028)
Lagged Math Scores	0.118	0.061	0.112	0.072	0.006 (0.011)	0.019	0.008	0.010 (0.016)
8th-Grade Suspensions	-0.035	0.027	-0.038	0.035	0.003 (0.005)	-0.028	-0.016	-0.012 (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 (0.035)	0.036 (0.039)	0.135 (0.057)
ATE	221,569	-0.010 (0.023)	0.022 (0.029)	0.092 (0.043)
Match Effect	221,569	0.002 (0.004)	0.003 (0.003)	0.009 (0.005)
Panel B: Heterogeneity				
White	11,812	-0.017 (0.069)	-0.002 (0.129)	-0.023 (0.147)
Hispanic	173,489	0.018 (0.037)	0.046 (0.037)	0.164 (0.054)
Black	19,740	-0.079 (0.084)	-0.108 (0.100)	-0.047 (0.138)
Female	113,427	0.020 (0.034)	0.024 (0.037)	0.136 (0.056)
Poverty	172,661	0.007 (0.034)	0.040 (0.038)	0.154 (0.057)
No Poverty	48,908	-0.021 (0.062)	0.012 (0.059)	0.024 (0.080)
English Learner	28,459	-0.011 (0.033)	0.013 (0.035)	0.030 (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:

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

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.

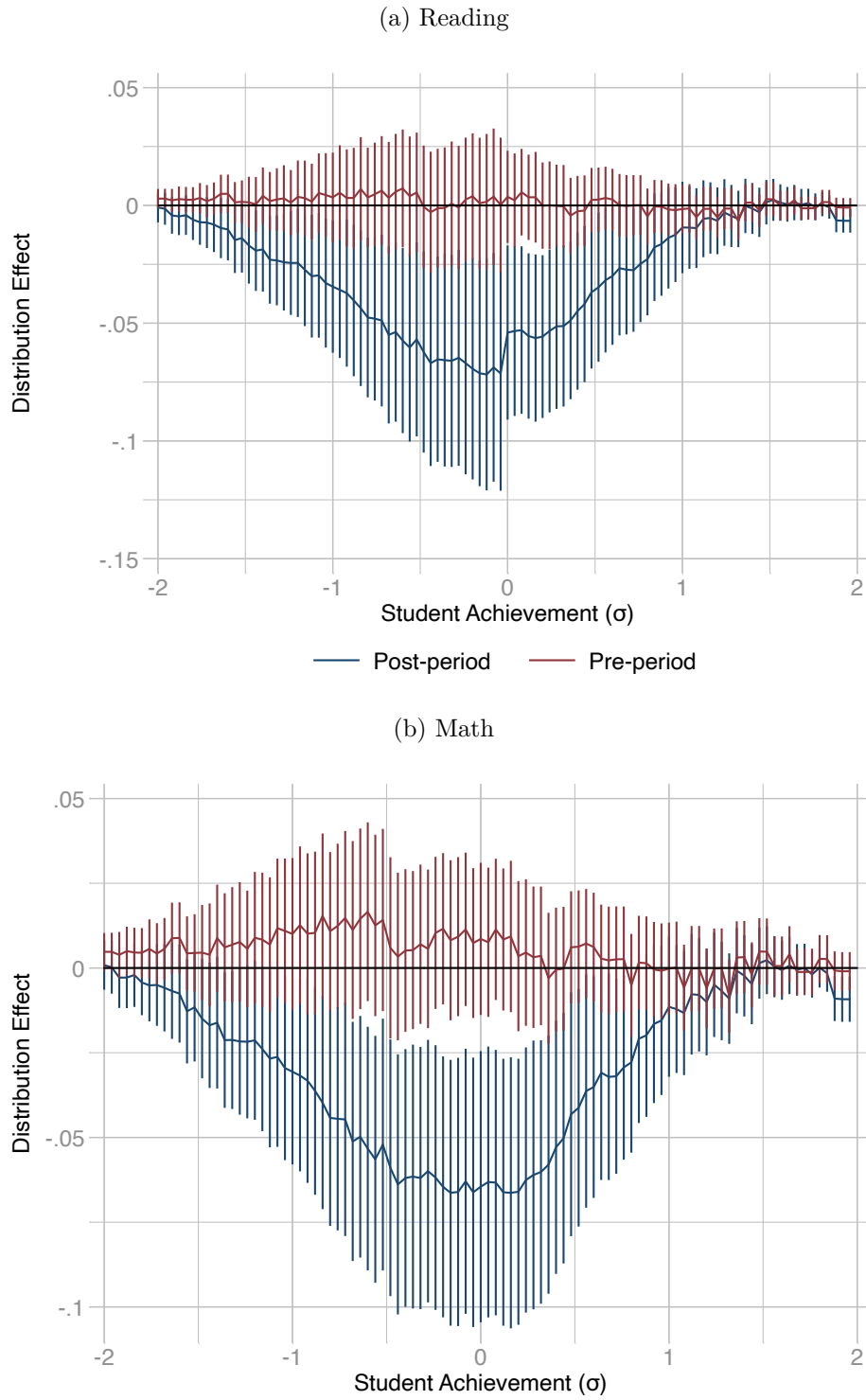
Figure D.3 shows that treatment effects are not just concentrated among students who are more likely to enroll in college, and, as with the previous results, the treatment effects are larger as exposure to the program increases for later cohorts. Although the treatment effects for students in the top 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.

³Variables 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.

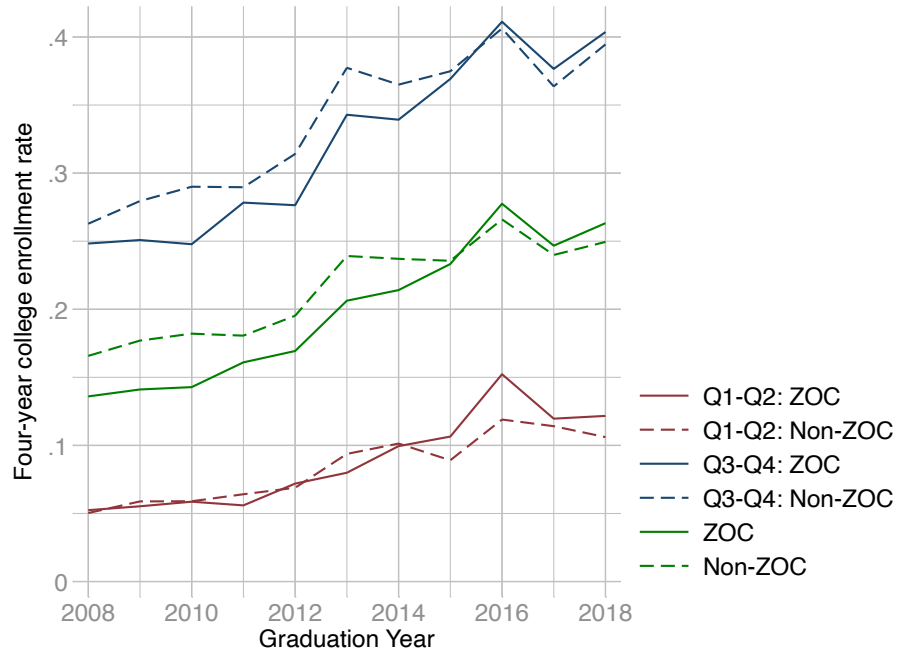
⁴Appendix Figure D.2 reports trends by different quartile groups.

Figure D.1: Student Achievement Distributional Impacts



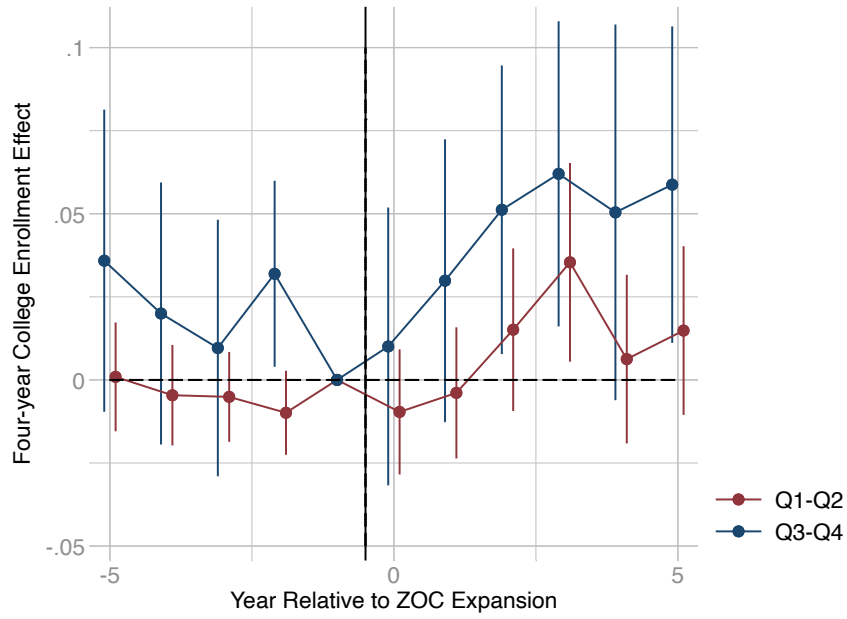
Notes: This figure reports estimates of β_a and γ_a from Equation 5 for 100 equally distanced points between -2 and 2 . The blue lines and bars correspond to β_a , difference-in-differences estimates on the probability of students scoring below a on their student achievement exams. Similarly, the red 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



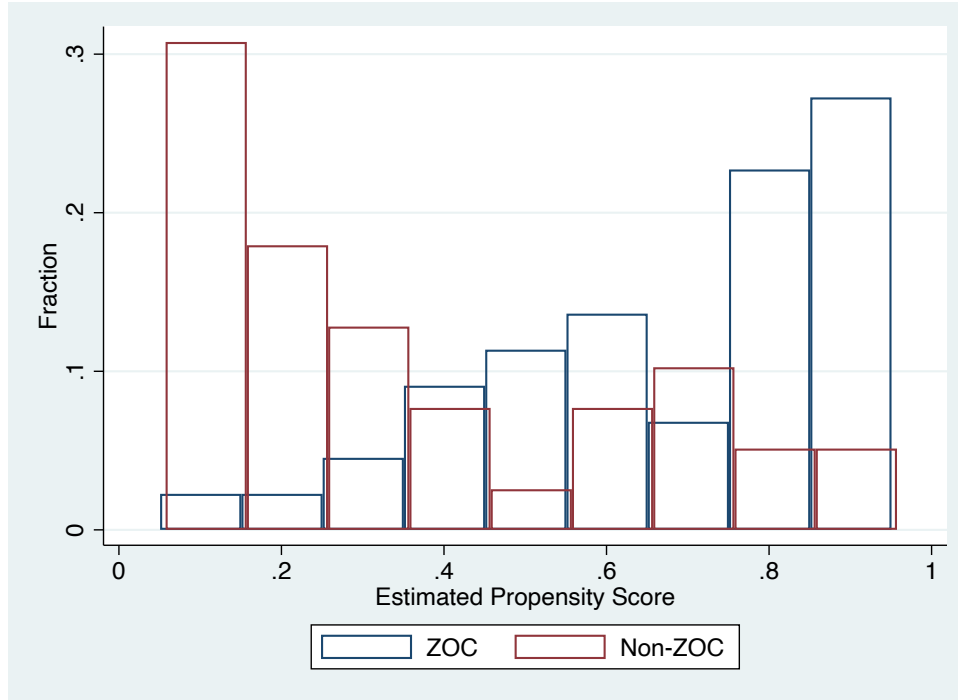
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates for four-year college enrollment rates relative to the year before the policy. Estimates in blue 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



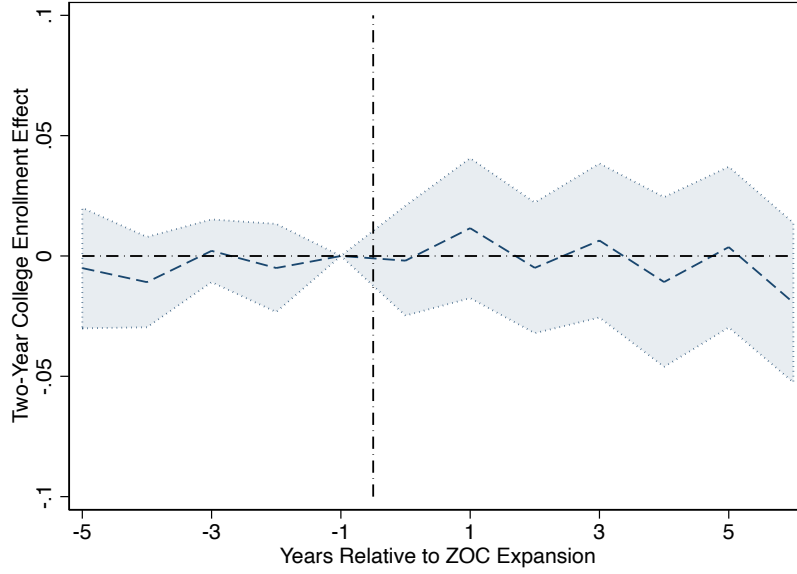
Notes: This figure reports histograms for the estimated school-level propensity scores by treatment status. Bin widths are equal to 0.1.

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.

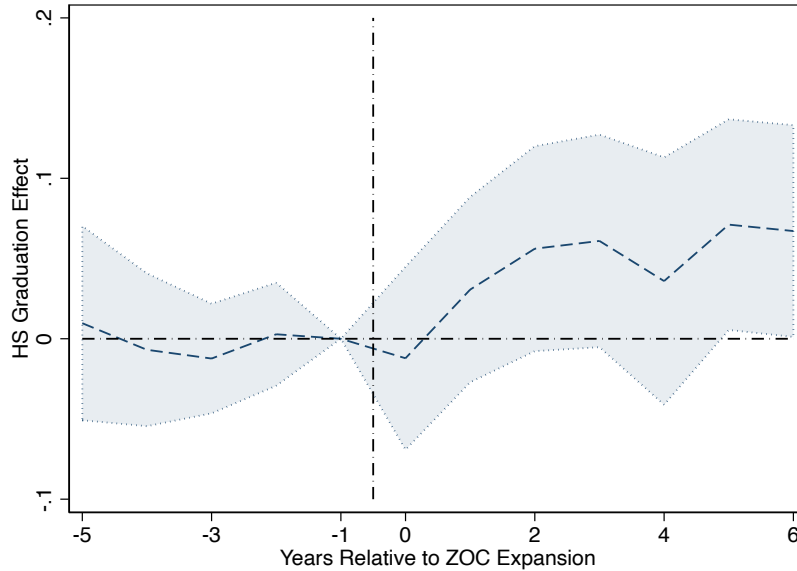
Appendix Figure E.3 reports high school graduation effects and shows that ZOC high school graduation rates differentially improved following the program's expansion. The evidence is far noisier than other evidence but does suggest that ZOC boosted student outcomes in terms of achievement, high school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

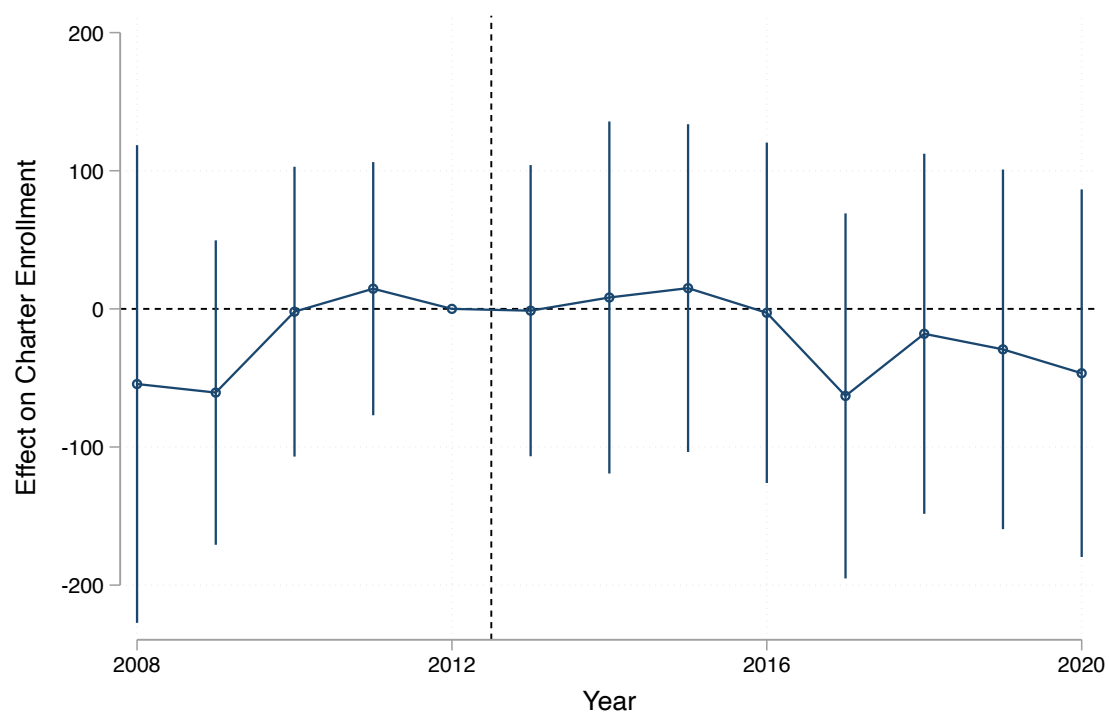
Appendix Figure E.4 reports event-study evidence comparing charter enrollment trends in ZOC neighborhoods to non-ZOC neighborhoods using the school-level sample. The evidence reveals that charter enrollment trends are not trending differently both before and after the ZOC expansion. This suggests that 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.

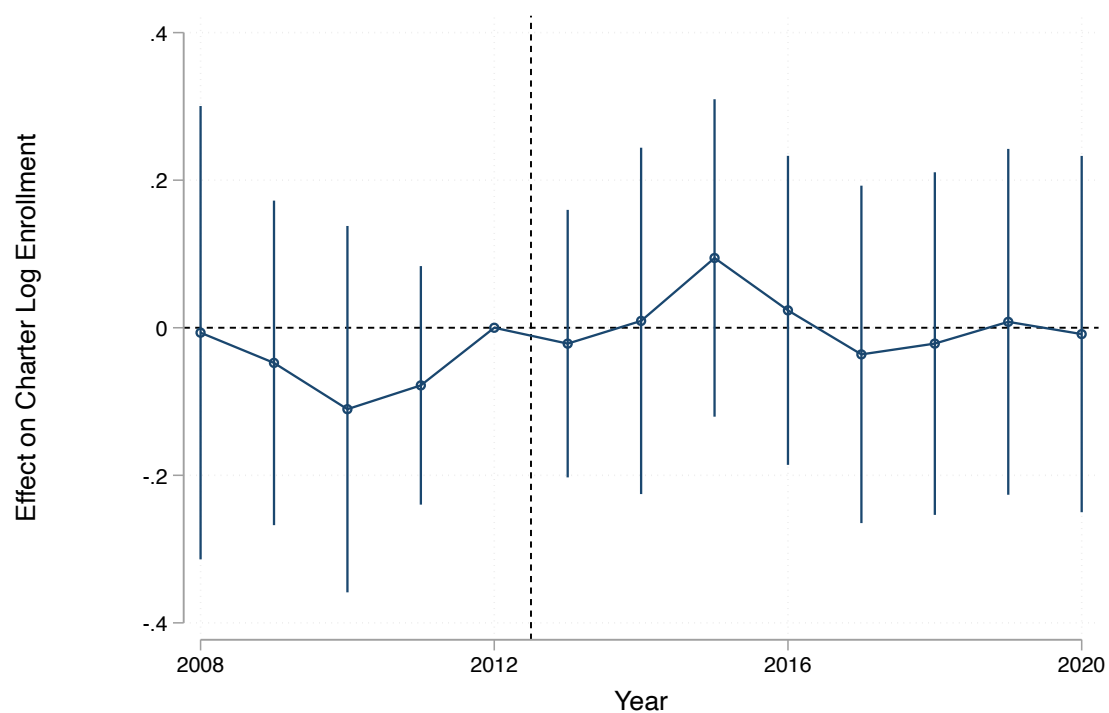
⁵The sample used for this analysis is the same as in the primary analysis.

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



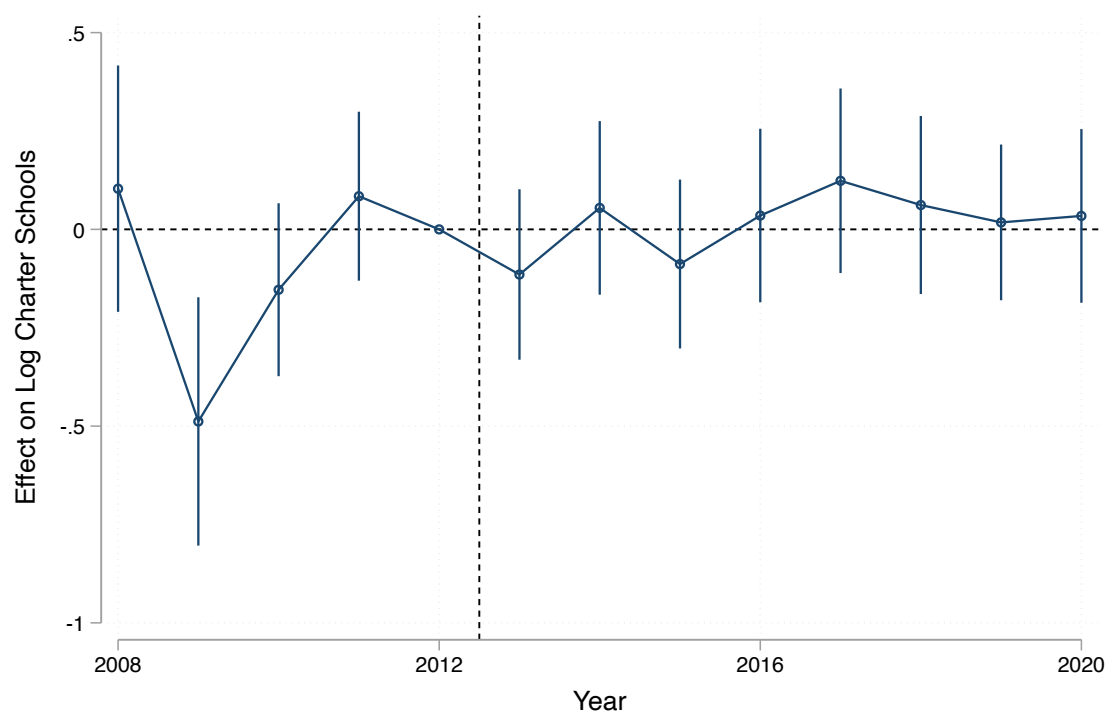
Notes: This figure reports estimates from regressions of charter-school-level log enrollment on year indicators, school indicators, and ZOC neighborhood indicators interacted with event-time indicators. The interaction term estimates are reported with 2012 as the omitted year. Charter school enrollment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



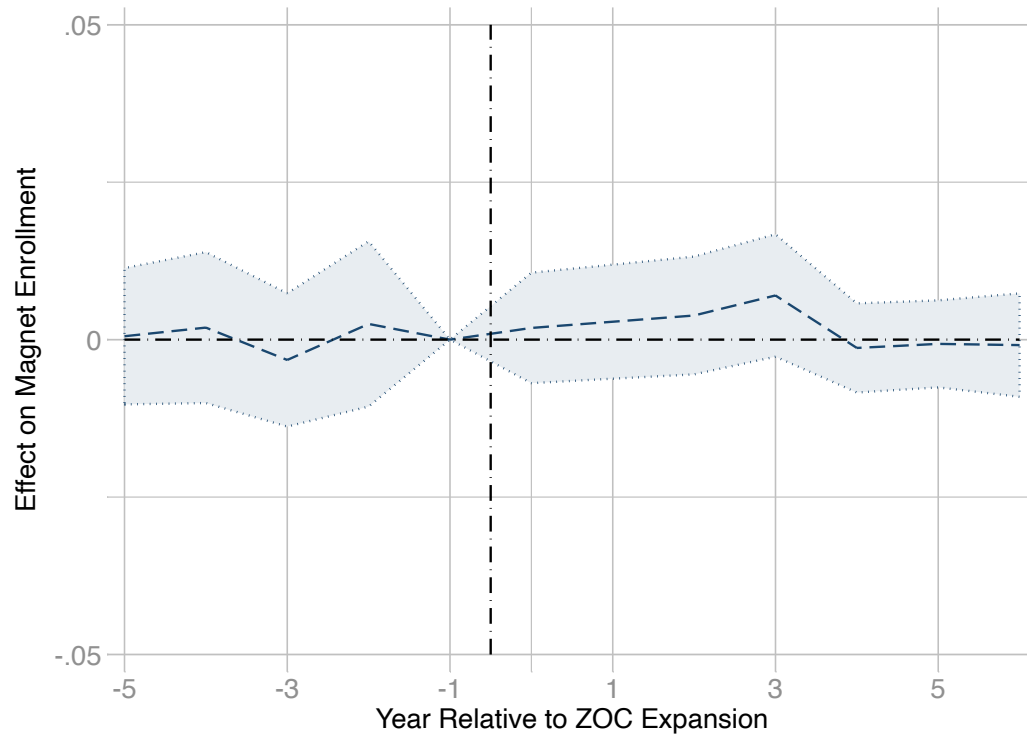
Notes: This figure reports estimates from regressions of charter-school-level log enrollment on year indicators, school indicators, and ZOC neighborhood indicators interacted with event-time indicators. The interaction term estimates are reported with 2012 as the omitted year. Charter school enrollment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

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

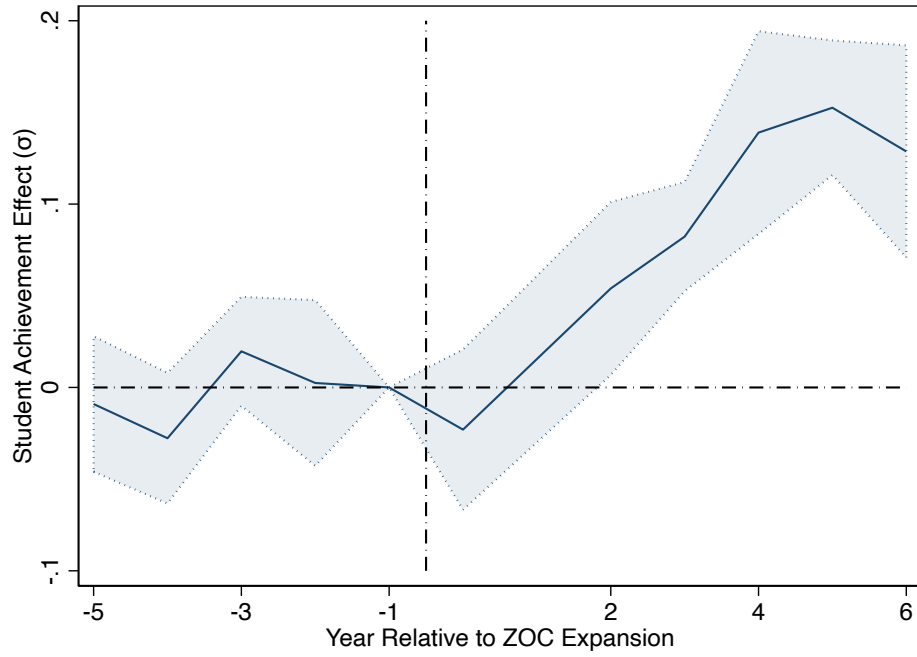
We define neighborhood in two ways. The first is at the attendance zone level and the second is at the neighborhood level. There are a total of 64 attendance zone levels that are fixed in the pre-period. There are a total of 23,833 census blocks in our sample. The latter specification allows us to absorb 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.

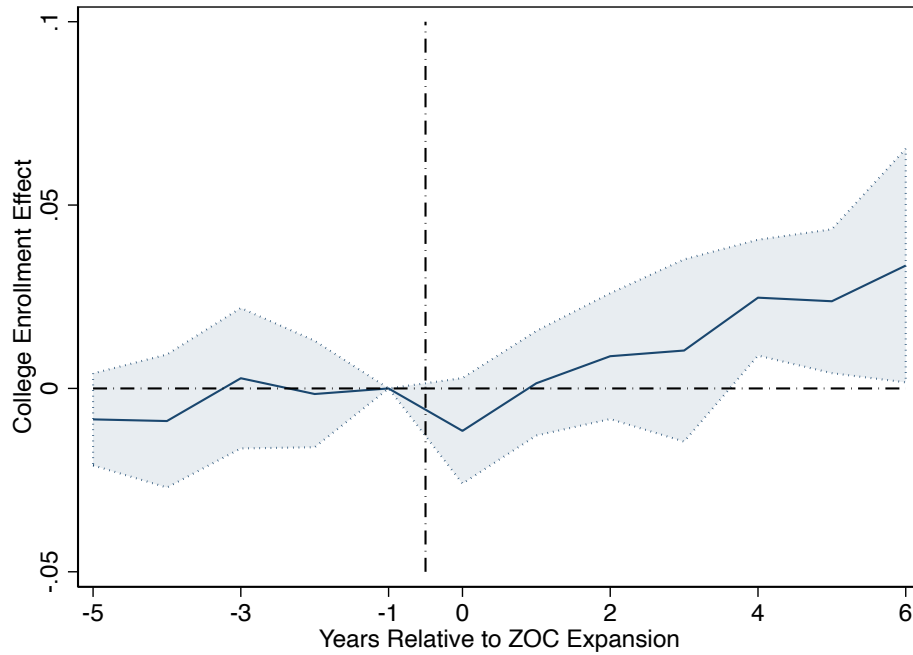
The estimates where neighborhood is defined at the students' middle school census block level are reported in the main text in Figure III, and are similar to the previous evidence. The robustness of the evidence to alternative research designs that define treatment at some pre-high school choice level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

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

(a) Achievement Event Study

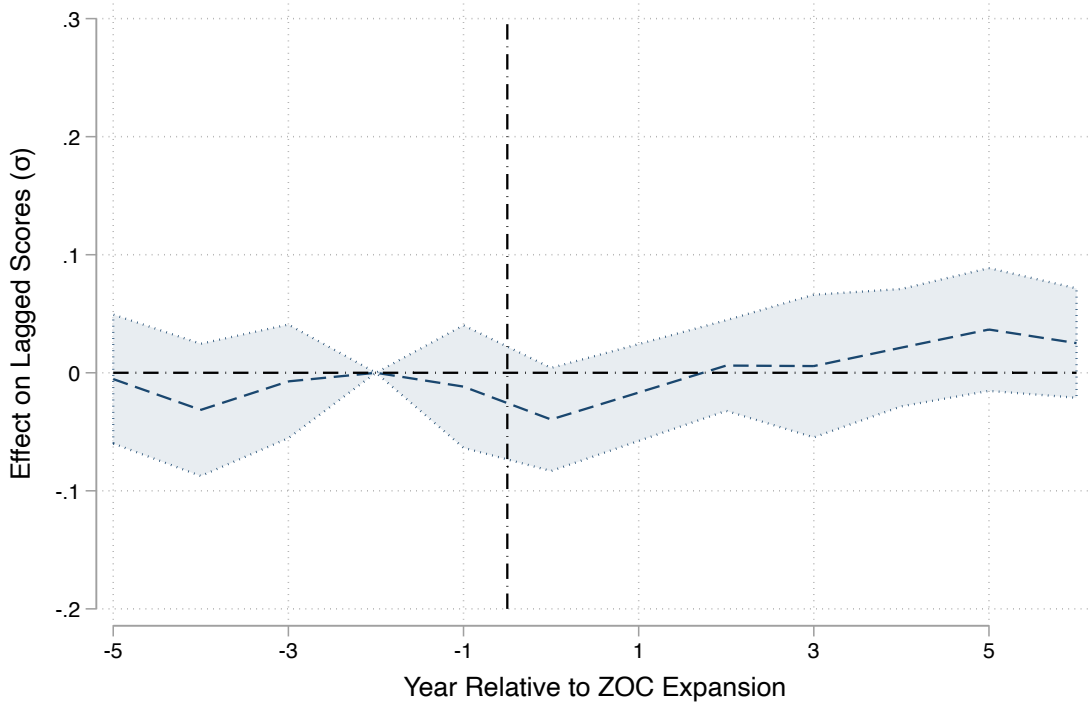


(b) Four-Year-College-Enrollment Event Study: Attendance Zone Assignment



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.

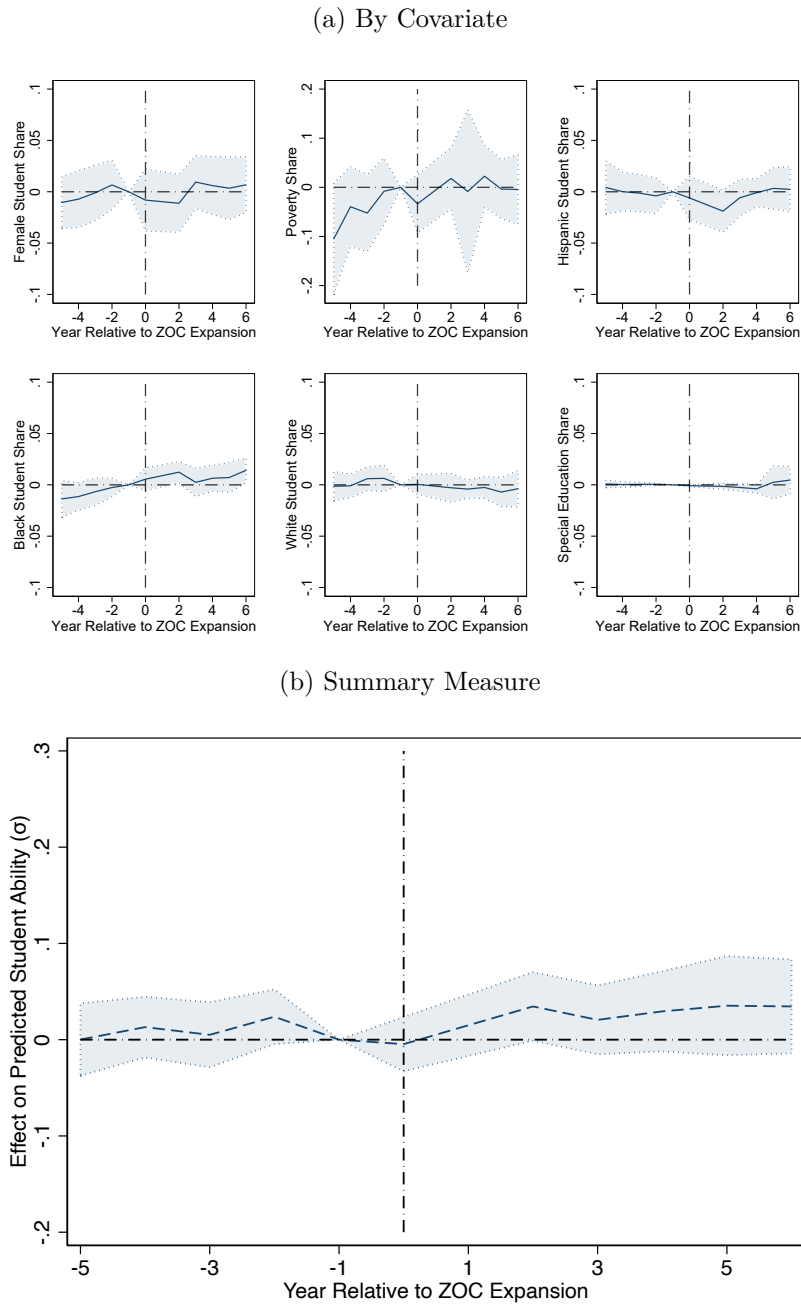
Appendix Figure E.11 and Appendix Figure E.12 considers strategic sorting. Some students that are observationally similar may have strategically sorted into ZOC neighborhoods after the program expansion. These types of moves are not detected in the evidence in Appendix Figure E.10. To assess the potential 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 disproportionately disadvantaged neighborhoods and our findings may be due to changes in school funding levels. If so, we should observe a coinciding increase

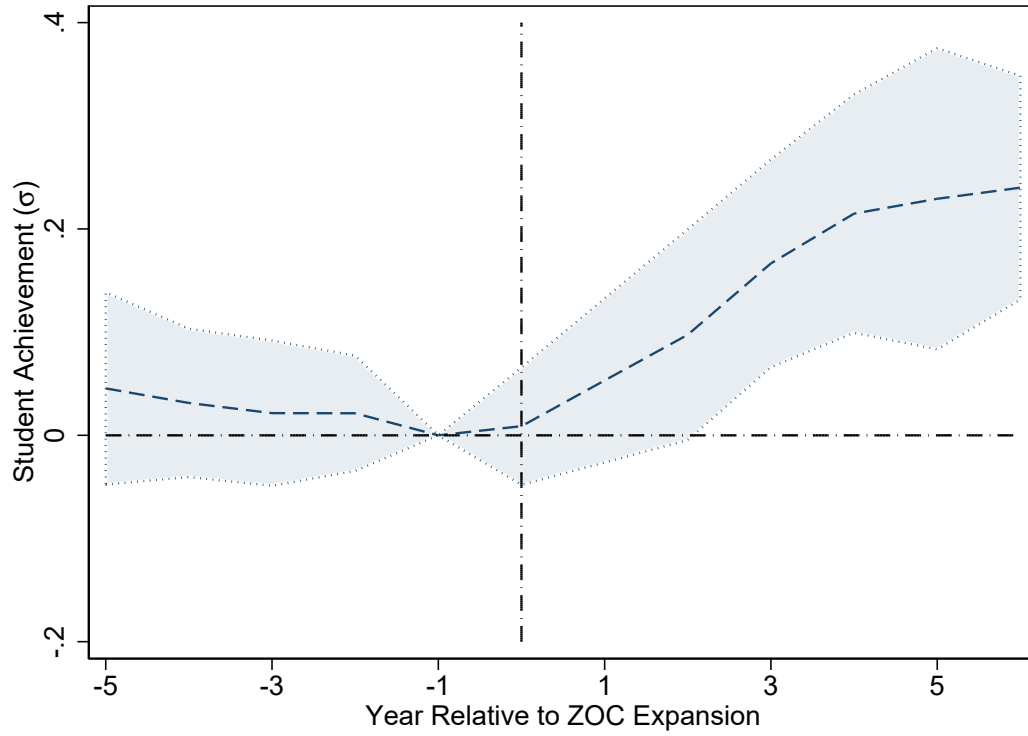
in achievement among middle school students at around the same time we observe increases in achievement among high school students. Appendix Figure E.14 demonstrates that is not the case. This provides suggestive evidence that changes in school funding, as governed by the LCFF, do not explain our main findings.

Figure E.10: Changes in Student Demographics



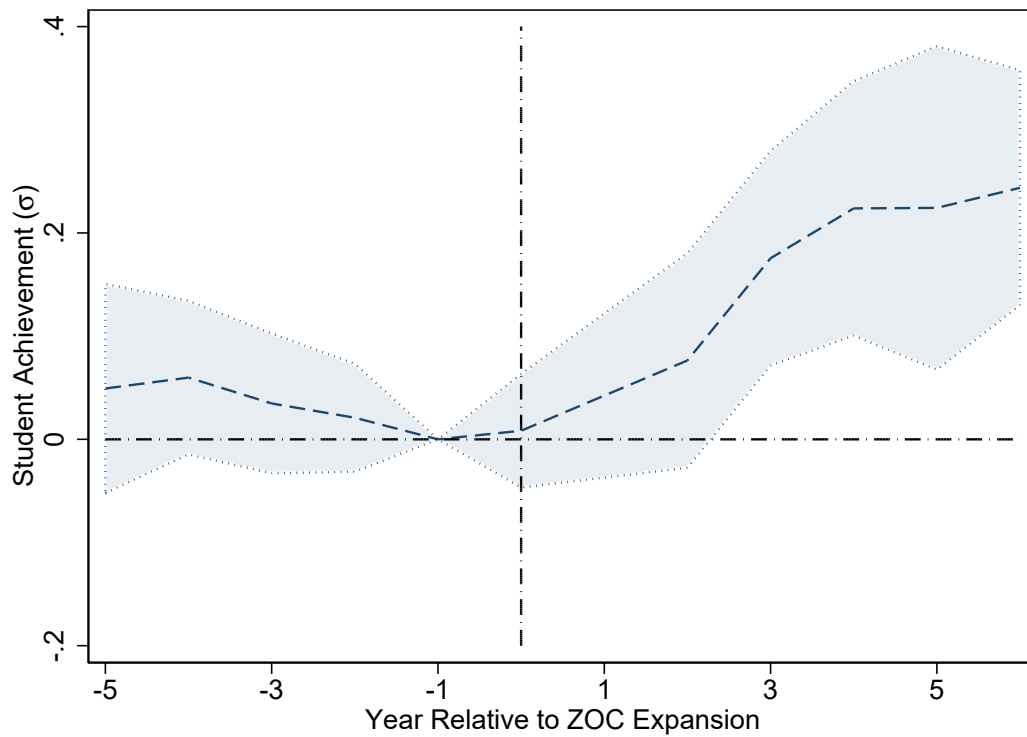
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in the change of student characteristics, labeled on subfigure vertical axes, between ZOC and non-ZOC 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



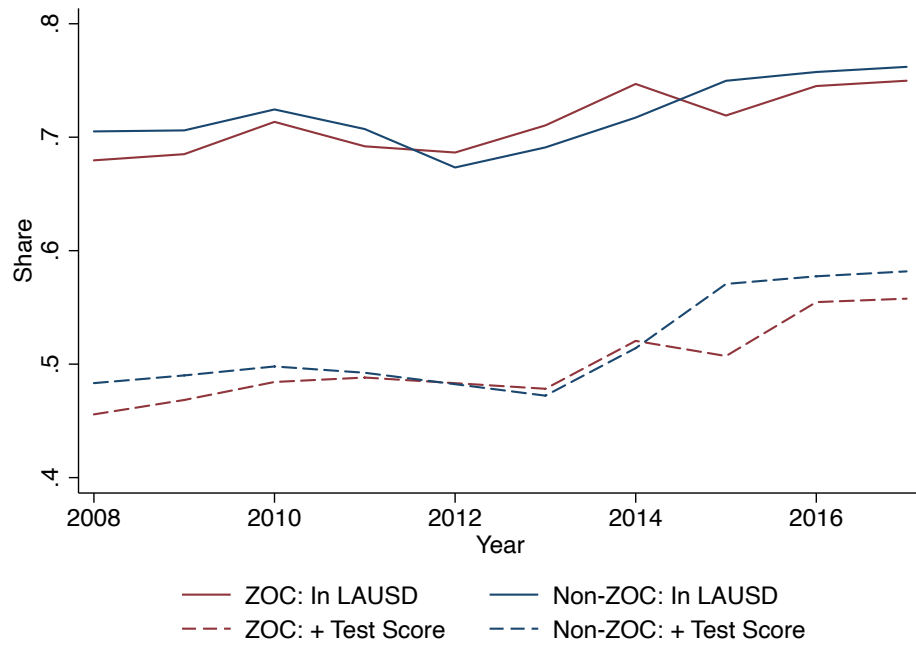
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The sample is restricted to students who did not move in the eighth grade, the year before households submitted ZOC applications. The coefficient 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

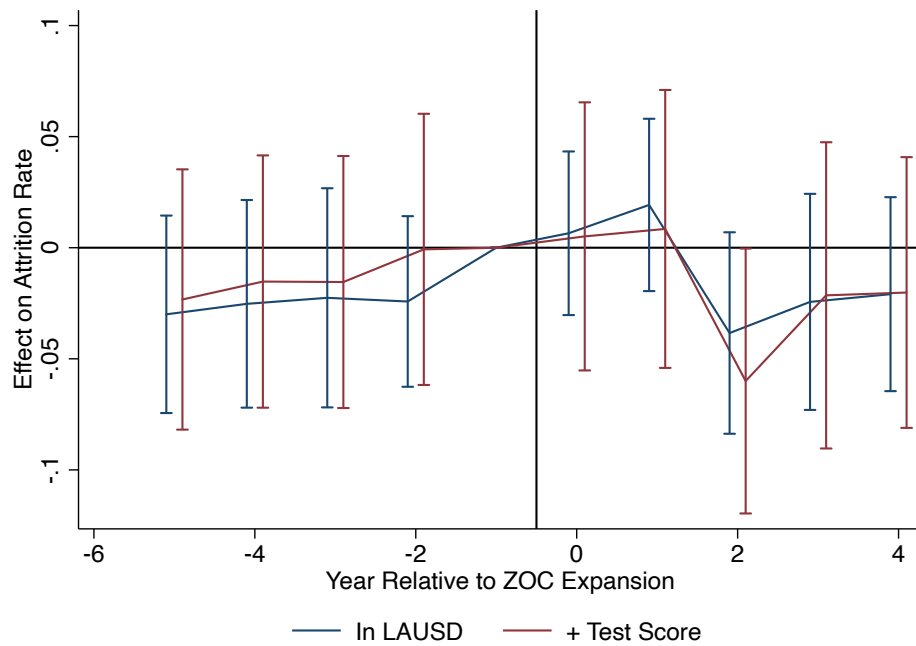


Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The sample is restricted to students who did not move in eighth grade *and* did not move at any time during middle school. The coefficient β_k 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



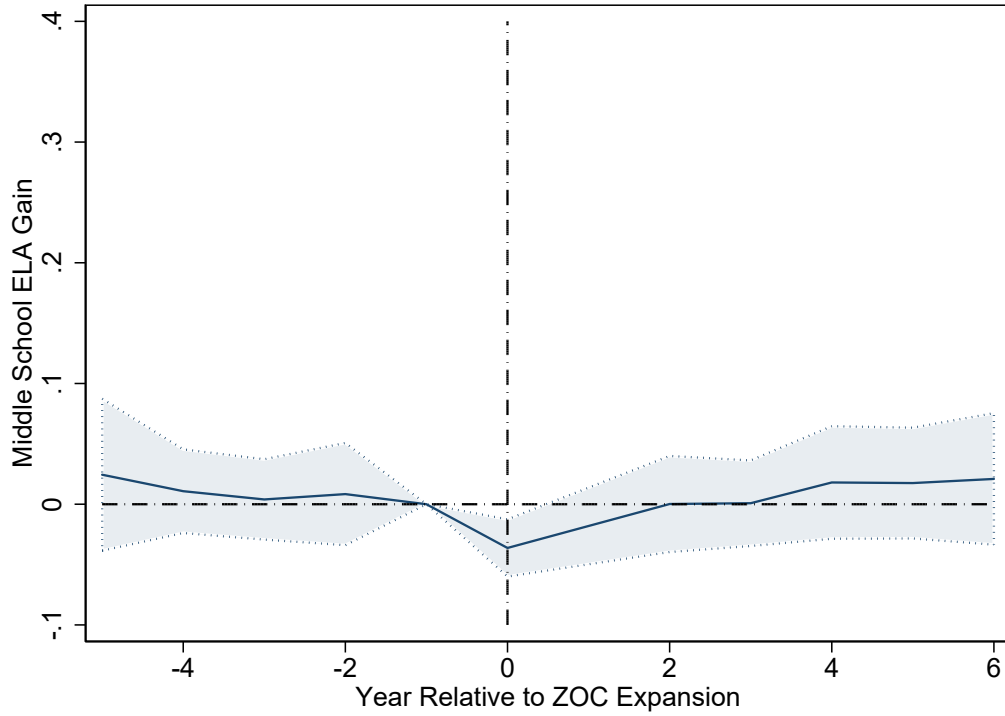
(a) Trends in Attrition Rates



(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

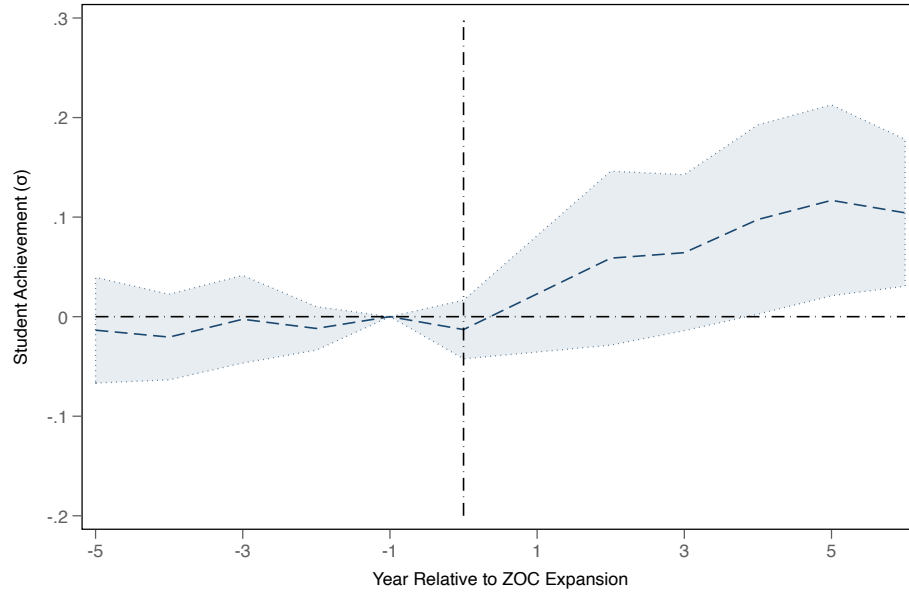


Notes: This figure reports estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The outcome is student achievement growth between seventh and eighth grades, measured in student achievement standard deviations and predating 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.

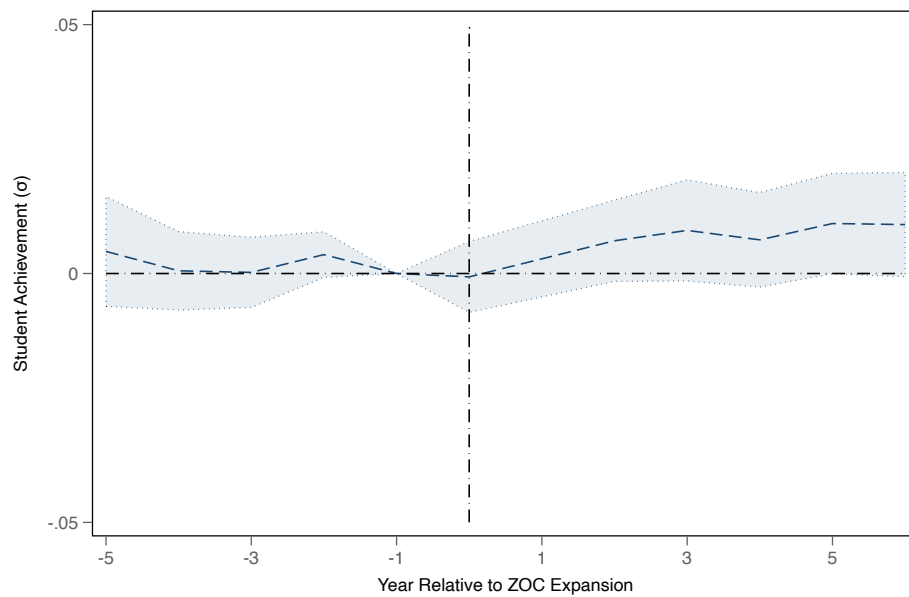
E.6 Decomposition Evidence and Math Estimates

Figure E.15: Decomposition Event Studies

(a) Average Treatment Effect Event Study

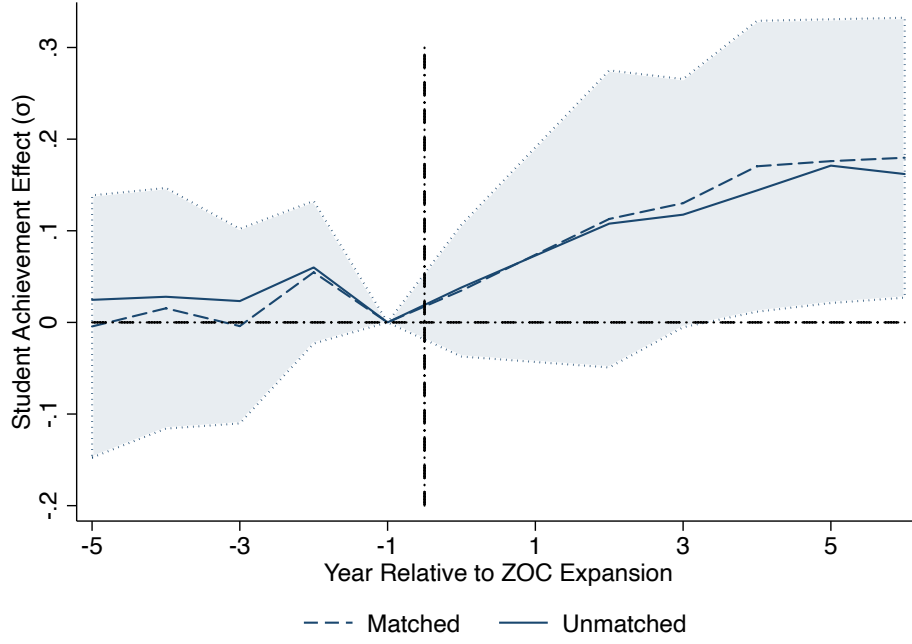


(b) Match Effect Event Study



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.

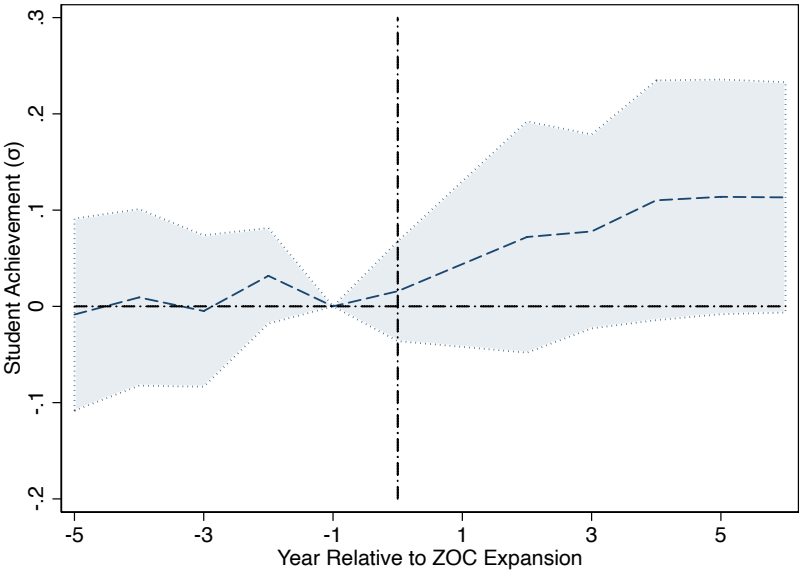
Figure E.16: Math Achievement Event Study



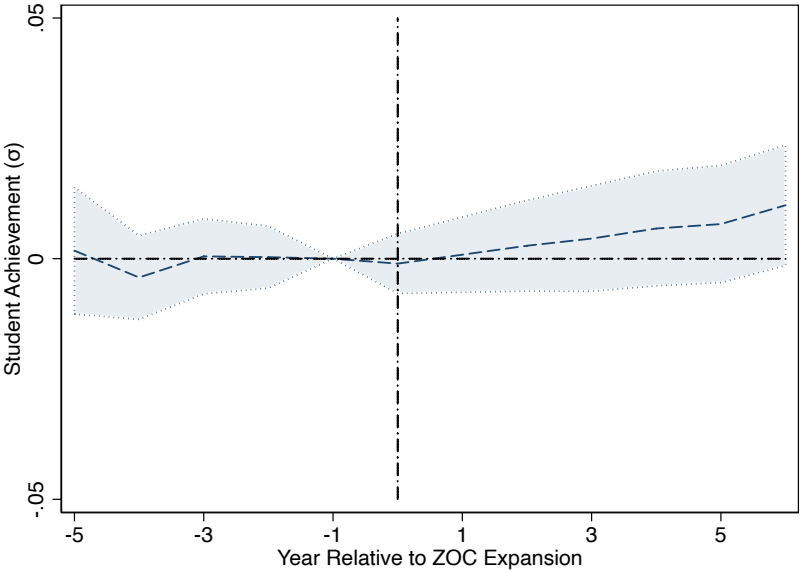
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2 (of the main text), where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. The dashed blue line in Panel A traces out estimates that adjust for covariates \mathbf{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 $p_i = (p_{i1}, \dots, p_{iJ})$ be applicant i 's admission chances at their available options.⁶ We now assume that the unobserved preference heterogeneity $\varepsilon_i = (\varepsilon_{i1}, \dots, \varepsilon_{iJ}) \sim \mathcal{N}(0, \Sigma)$, 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

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

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

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

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 $(\Delta^0 = \{\delta_{jc}^0\}, \lambda^0, \Sigma^0)$ and U_i^0 . The initial vector of latent utilities is a solution to the linear program

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

We then iterate through the following sequence of conditional posteriors:

$$\begin{aligned} \Delta^{s+1} &\mid U_i^s, \Sigma^s \\ \Sigma^{s+1} &\mid U_i^s, \Delta^{s+1} \\ U_i^{s+1} &\mid U_i^s, \Delta^{s+1}, \Sigma^{s+1}, C(\mathcal{R}_{R_i}^*). \end{aligned}$$

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,

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

⁷For 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 (0.0847) [0.339]			0.0325 (0.0750) [0.419]
Peer Quality		0.119 (0.152) [0.310]		0.0871 (0.163) [0.5435]
Match Quality			0.0495 (0.165) [0.787]	0.0386 (0.173) [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

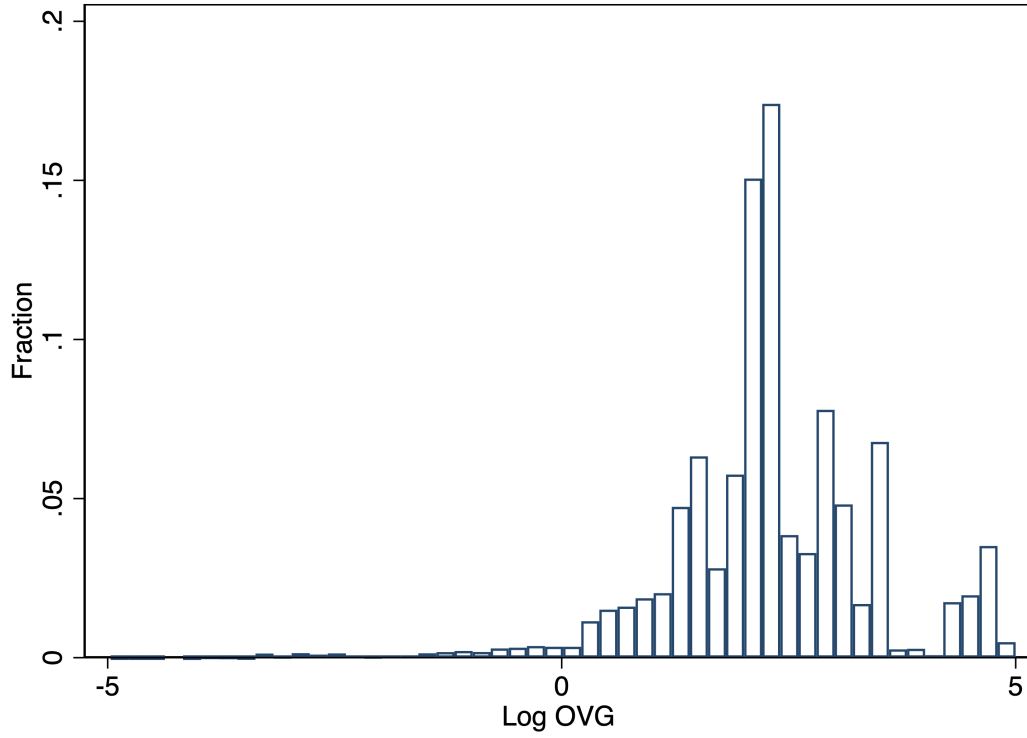
Notes: This table reports estimates from regressions of school-popularity measures δ_{jct} for each school among students in achievement cell c in cohort t on estimated school average treatment effect, ability, and match effects all scaled in standard deviation units. Panel A uses estimates that account 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



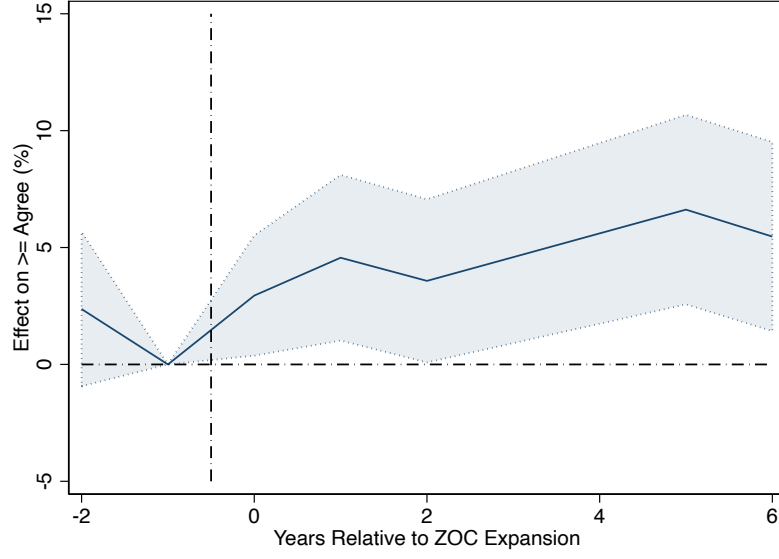
Notes: This figure presents a histogram of estimated log option value gain (OVG) across all students and all years. Preference parameters used in OVG estimation are estimated using only the first cohort's preferences. OVG for later cohorts is constructed using these estimated parameters.

Table G.1: Option Value Gain Correlations

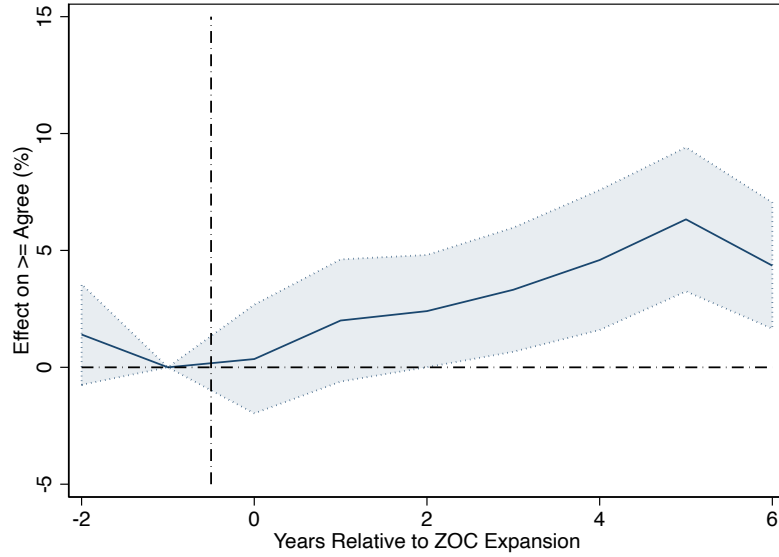
	(1) Log OVG	(2) Log OVG
Black	0.299** (0.125)	0.124 (0.0899)
Hispanic	0.328*** (0.0795)	0.0320 (0.0431)
Parent College +	-0.00977 (0.0792)	-0.00668 (0.0309)
Poverty	-0.150*** (0.0311)	-0.0124 (0.0182)
Female	0.0355 (0.0296)	-0.00624 (0.0179)
Spanish at Home	0.272*** (0.0422)	0.00668 (0.0250)
English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