

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

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Abstract

Does a public school district that expands school choice provide better outcomes for students than a neighborhood-based assignment system? Do public school choice systems produce market-level effects? This paper studies these questions by studying the Zones of Choice program, a novel school choice initiative that created small high school markets in some neighborhoods but left traditional attendance-zone boundaries in place throughout the rest of the district. The policy design allows us to uniquely study market-level impacts of choice on student achievement and college enrollment using a differences-in-differences design. We find that student outcomes in ZOC markets increased markedly, narrowing achievement and college enrollment gaps between ZOC neighborhoods and the rest of the district. These gains are largely explained by general improvements in school effectiveness rather than by changes in student match quality. To explore the role of competition in driving these gains, we construct a competition index that leverages differences in school popularity and the spatial differentiation of students and schools at the program's onset. We find that the effects of ZOC are larger for schools exposed to more competition, supporting the notion that competition is a key channel through which ZOC exerts its impacts. Demand estimates derived from rank-ordered preference lists suggest families place substantial weight on schools' academic quality, and this weight provides schools with competition-induced incentives to improve their effectiveness. Our findings demonstrate that public school choice programs have the potential to improve school quality, reduce neighborhood-based disparities in educational opportunity, and thus produce sustained improvements in student outcomes.

Keywords: School choice, school quality, competition, value-added

JEL Classification: I21, I24

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1 Introduction

In recent years, centralized school choice systems have become increasingly popular for allocating K-12 students to schools in the public sector, 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 et al., 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 provides an unusual and unique 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, 2018, Cullen et al., 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 suggest 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 et al., 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. First, we probe at the potential role of charter competition in our setting. We find that charter enrollment in affected neighborhoods is not trending differently than unaffected neighborhoods, both at the intensive and extensive margin, assuaging concerns about charter competition as an alternative explanation. Second, we assess the role of magnet school competition and find that within-district

growth in magnet programs do not differentially change between affected and unaffected neighborhoods, alleviating concerns about this competition. Third, find that ZOC expansion does not predict a summary index of student covariates, addressing concerns about compositional changes. To further assess non-random selection into the program, we restrict the sample to students who did not move into a ZOC neighborhood in eighth grade and consider a sample that excludes students who moved at any time during middle school; we find similar effects in samples of students who do not strategically sort into ZOC neighborhoods. Fourth, we consider models that consider treatment assignment at the neighborhood level as opposed to the *enrolled* school level and find similar results, further assuaging concerns about selection into the program. Last, we assess the role of potential state-level funding shocks and find that non-high school students in ZOC neighborhoods, who would similarly benefit from neighborhood-based funding shocks, did not experience a differential change in outcomes. This last finding rules out differential funding changes induced by the Local Control Funding Formula (LCFF) as an explanation for our findings.

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 et al., 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 et al., 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

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.

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 et al., 2010, Dee, 1998, Neal, 1997). Our paper is unique in focusing 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, 2018, Cullen et al., 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 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 et al., 2016, Chyn, 2018, Kling et al., 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 2 outlines the features of the program and our data sources. Section 3 outlines the conceptual framework for the subsequent analysis, and Section 4 discusses the data. Section 5 reports evidence on how the program affected student achievement and college enrollment. Section 6 estimates demand and studies the role of competition, and Section 7 presents evidence on additional mechanisms and discusses institutional features that may have contributed to the results. Section 8 concludes.

2 Institutional Details

2.1 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 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, they still represent roughly 12.6 percent of all high school enrollment as of 2019. In summary, while families have many options at their disposal, 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 et al. (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 1 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:

²LAUSD defines pilot schools as a network of public schools that have autonomy over budget, staffing, governance, curriculum & 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.

1. **Access.** “Develop a plan that would consider removing boundaries for schools to give parents the flexibility for their children to take advantage of all seats in high-performing schools.”
2. **Match quality.** “Every child is unique with special talents, strengths and needs, and school placement decisions must therefore be made in the best educational needs of each individual student.”
3. **Parental involvement.** “Research validates that parental involvement in public schools is a key factor in producing measurable gains in student academic success, closing the achievement gap.”

2.2 Program Features and Incentives

ZOC expands students’ high school options by combining catchment areas into school 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 incumbent eighth graders submit zone-specific applications containing rank-ordered preference lists. Admission into any particular school is not guaranteed, although some priority is given based on proximity, incumbency, and sibling status. Most ZOC students are enrolled in feeder middle schools that directly feed into ZOC high schools, mimicking neighborhood-based transitions between schools but allowing parents to exercise choice in the transition to high school.

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 2 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 et al., 1999a). 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.

3 Conceptual Framework

We begin with a stylized model for 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 Section 5.2.1, d_{ij} is distance

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.

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 $\delta(\alpha_j, \mathbf{X}_i)$ and another component capturing linear distance costs λd_{ij} .⁵

$$V_{ij}(\alpha_j, \mathbf{X}_i, d_{ij}) = \delta_j(\alpha_j, \mathbf{X}_i) - \lambda d_{ij}.$$

Mean utility $\delta(\alpha_j, \mathbf{X}_i)$ depends on school quality α_j and an additively separable component capturing remaining observable preference heterogeneity:

$$\delta_j(\alpha_j, \mathbf{X}_i) = \omega \alpha_j + \mu_j(X_i).$$

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 et al., 2010).⁶ 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 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

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

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

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

⁵Schools in school choice zones are all relatively close to each other, making linear distance costs a plausible parameterization.

⁶See Dewatripont et al. (1999a) and Dewatripont et al. (1999b) for models suggesting principals could care about market share, as it is an implicit signal of their potential future productivity and thus affects career progression within the district. Indeed, many LAUSD administrators working in the district headquarters started as teachers, became principals, and then were promoted to an administrative role in the district headquarters.

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, note that with an expanded choice set, the probability of student i enrolling in school $j \in \mathcal{J}^+$ is

$$P_{ij} = \frac{e^{V_{ij}}}{1 + \sum_{k \in \mathcal{J}} e^{V_{ik}}}.$$

If we 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 Appendix B.1. □

3.1 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.⁷

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

⁷The 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. 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 et al., 1997) assuages concerns about perverse incentives in the ZOC setting.

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.⁸

4 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.

4.1 Analysis Samples

The main sample covers 2008–2019. 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.⁹

⁸One attempt at measuring competition would be to use the number of competitors instead of OVG. Through the lens of the model, this would impose harsh restrictions on the unobserved preference heterogeneity ε_{ij} or impose that schools are homogeneous with the latter clearly not true. In particular, if the preference heterogeneity is large, so that $\sigma_\varepsilon^2 \rightarrow \infty$, then $OVG_i \approx OVG = \frac{\ln |\mathcal{J}_z|}{\lambda}$ for all i . Thus, OVG would be closely approximated by the log number of options, and differences in school quality or distance would matter less. To see this, note that $V_{ij} = \frac{\delta_j - \lambda d_{ij}}{\sigma} \rightarrow 0$ as $\sigma^2 \rightarrow \infty$, implying $OVG_i \approx \frac{1}{\lambda} \left(\ln \sum_{\mathcal{J}_z} e^0 \right) = \frac{\ln |\mathcal{J}_z|}{\lambda}$ for all students i . In this extreme example, differences in the number of options can be a good index to summarize students’ expected utility gains, but more generally, using the number of options as the governing statistic would impose a very particular structure on preferences.

⁹A potential concern with selecting only 11th-grade observations is differential attrition rates out of the sample that could introduce bias in our analysis. In Appendix Figure E.15 we report attrition rates over time for ZOC

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.¹⁰

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.¹¹ 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.¹² 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.¹³

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 Appendix C. We refer to this as the matched sample. Appendix A provides additional data construction details that may be of interest.

4.2 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.¹⁴ 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.

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.

and non-ZOC cohorts. We do not find evidence of differential attrition rates between both cohorts.

¹⁰There are not any continuation, special education, or magnet programs 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 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. In addition, we consider samples that allow for the inclusion of magnet programs in the non-ZOC pool of schools, and the results look qualitatively similar.

¹¹We emphasize that these restrictions produce conservative estimates. Appendix Figure E.17 reports estimates for samples that do not apply these restrictions.

¹²Some 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.

¹³The restrictions we applied to the sample eliminate roughly 8–14 of students from the analysis, with a majority coming from students enrolled in non-traditional schools such as continuation schools. Our primary estimates are robust to excluding these restrictions.

¹⁴The LAUSD-NSC link is done by LAUSD, so we do not have additional NSC data above and beyond the LAUSD sample.

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.¹⁵ 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. Appendix A discusses additional data details and reports the set of ZOC schools used in the analysis.

4.3 Descriptive Statistics

Columns 1 and 2 of Table 1 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. Appendix Table A.2 reports analogous school-level differences.

We report matched non-ZOC mean characteristics in Column 4 of Table 1. 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.

5 Empirical Analysis

5.1 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. The dynamic nature of

¹⁵In Appendix A.3 we provide 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.

our empirical strategy alludes to effects to how these effects evolve over time in the short and medium run. As discussed above, we report estimates for both the matched and unmatched samples, but throughout the analysis, the results look similar among both samples.

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.¹⁶ 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. In addition, inference is robust to clustering at the matched pair level as suggested by Abadie and Spiess (2021) for inference in settings with an initial matching step like ours. 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).

5.1.1 Event-Study Results

Figure 3a 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. Appendix Figure E.20a reports math score treatment effects that are nearly identical to ELA treatment effects.¹⁷ Importantly, the results look similar in both matched and unmatched samples, indicating our findings are not driven by convenient

¹⁶Between-sector sorting into ZOC schools remains a concern that we address in Section 5.1.2. To be specific, within-ZOC sorting is not a concern per se as the policy induces sorting within markets. The primary concern is about strategic sorting between ZOC and non-ZOC markets before and after the reform. We address these issues with an intent-to-treat analysis and specifications that consider the set of students who did not sort into ZOC markets.

¹⁷We focus on reading throughout the rest of the analysis because reading exams are grade specific throughout the sample, allowing for more parsimonious value-added estimation in the decomposition exercises that follow. Nonetheless, we find similar results when focusing on math scores; we report the results in Appendix E. Riehl 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.

sample selection introduced by the matching strategy.¹⁸

The event-study results for four-year college enrollment are reported in Figure 3b. 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.¹⁹ 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. Appendix Figure E.2 demonstrates that community college enrollment was unaffected. Last, 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.

5.1.2 Robustness Checks and Alternative Explanations

There are several potential threats to identification that we now address. The first set of exercises considers the matched sample—despite the results being robust to using the unmatched sample—and we focus on the role of sorting both on observable characteristics and unobservables and placebo checks to assess the role of other contemporaneous shocks affecting ZOC neighborhoods. The second set of exercises considers intent-to-treat-like models, where treatment assignment is at the eighth-grade neighborhood level. These exercises are more immune to bias from self-selection into ZOC schools and corroborate the sorting evidence in the first set of robustness exercises. Last, we consider the role of charter competition and the increasing magnet sector to assuage concerns that alternative forms of competition drive our primary findings.

First, even though we do not document evidence of differential trends before the ZOC expansion, changes in the composition of students will bias our estimates. This may happen

¹⁸In Appendix Figure E.17, we report estimates that do not restrict the set of comparison schools to comparable schools defined in Section 4. The results look qualitatively similar and are even more precisely estimated.

¹⁹Appendix Figure E.4 reports estimates of the 11th-grade ZOC achievement gap over time, showing that it was decreasing and was eliminated by the sixth year of the program and also provides additional evidence supporting the parallel trends assumption.

due to differential sorting into or out of ZOC neighborhoods due to changes in access to certain schools. For example, if school quality capitalizes into housing values, then changes in neighborhood school quality resulting from combining catchment areas will result in changes to property values (Bayer et al., 2007, Black, 1999) and changes in household composition (Nechyba, 2000). To assess these concerns, Appendix Figure E.12a reports event studies in which the outcomes correspond to different observable student characteristics. The evidence suggests that differential changes in observables between the two sectors are not an immediate concern.

The primary concern with our main empirical strategy is that treatment is defined at the school level, allowing students to strategically sort into schools. We address this concern by restricting the sample to students who did not move into a ZOC neighborhood in eighth grade, with results displayed in Appendix Figure E.13. We also consider a sample that excludes students who moved at any time during middle school, with the results displayed in Appendix Figure E.14. The results are remarkably similar in both, suggesting that biases induced by strategic sorting are not driving our findings.

Next, we further consider potential bias from students self-selecting into ZOC schools following the policy expansion. This mirrors the empirical strategies of other school choice reforms (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. This approach is less stringent in the sample selection criteria, including students enrolled in schools that open post-reform and a wider swath of magnet programs. Appendix E.3 discusses additional details regarding this empirical approach.

Appendix Figure E.10a 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, Appendix Figure E.10b 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 Appendix E.3, find similar results.

Other contemporaneous policies that may have differentially affected ZOC schools and students are also a concern. Notably, the LCFF substantially altered the funding of school districts in California and was implemented one year after the ZOC expansion. There are several reasons. Although the LCFF is a state-level policy, supplemental grants were allocated for schools with high shares of disadvantaged students, potentially leading to a disproportionate benefit to ZOC schools. But the LCFF is unlikely to pose a problem for several reasons.²⁰

Nonetheless, we conduct a placebo exercise to assess the potential presence of LCFF effects.

²⁰First, the matching strategy we use balances poverty, special education, and English-learner status, which are three defining characteristics for supplemental grants. The balance suggests that any additional funding going to schools with high shares of disadvantaged students would be equally absorbed between control and treated schools in our analysis sample. In addition, the American Civil Liberties Union successfully sued LAUSD for not distributing the targeted funds according to the law. Moreover, Lee and Fuller (2020) find that by 2019, the bottom three quartiles of poverty share high schools received an increase in funding of 27 percent compared with a 24 percent increase for the top quartile, suggesting ZOC schools did not experience a disproportionate change in funding during our sample period. Last, Fejarang-Herrera (2020) finds no effect of concentration grant money on student outcomes.

The intuition behind the placebo exercise is that if there was any LCFF impact in ZOC neighborhoods, then this would affect ZOC students not just in high school but also in middle school because of shared neighborhoods. Therefore, we test whether the program had any impact on lagged middle school test score gains. Appendix Figure E.16 presents estimates of Equation 2 in which the outcome is $\Delta A_i = A_i^8 - A_i^7$ —that is, students’ middle school gain in achievement, which predated their ZOC enrollment. The evidence suggests that ZOC did not impact students before they entered high school, showing that differential selection into ZOC and any potential LCFF effect predating ZOC enrollment are not causes for concern. This evidence also provides reassuring evidence in terms of differential selection into the program on pre-intervention gains, supplementing the evidence contained in Appendix Figure E.12a.

Last, we assess the role of charter and magnet school competition. The charter share of enrollment grew significantly during the sample period, and there may be concerns that ZOC neighborhoods were differentially affected. Appendix Figures E.5 and Figure E.6 assess this potential threat empirically. In particular, we compare charter enrollment trends among charters in ZOC neighborhoods to charter enrollment trends outside of ZOC neighborhoods. We do not find evidence of a differential change in enrollment, suggesting that ZOC neighborhoods were not differentially affected along the intensive margin. Appendix Figure E.7 reports extensive margin effects and similarly finds no evidence of differential changes in the number of charter schools located in ZOC neighborhoods. Similar to charters, and in part a response to charter competition, LAUSD significantly expanded its magnet offerings.²¹ These last set of findings assuage concerns that alternative forms of competition explain our primary findings.

5.2 Decomposition of Achievement Effects

The achievement effects show that ZOC student achievement improved at a remarkable pace compared with improvements of students enrolled at similar schools. There are two potential sources of such gains. 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.²² In this section, we decompose the achievement effects to provide a more refined understanding of the source of the gains.

5.2.1 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. First, decomposing changes in achievement in an underlying education production function that includes school quality sheds light on the potential mechanisms. Second, with measures of school quality, we can gauge how supply-side changes in school quality relate to demand-side preferences, which we explore in Section 6.

²¹Most new magnet offerings are contained within a larger campus and effectively operate as a program within the primary campus. Very few new schools opened as magnet programs during this time period.

²²A third channel exists due to a reallocation of students from less effective to more effective schools, holding school and match quality constant. We consider this channel in Appendix Table E.1.

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 yields²³

$$A_{ij} = \alpha_j + \mathbf{X}_i' \beta_j + u_{ij}, \quad (3)$$

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 3 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 .²⁴ Appendix G.1 reports achievement model estimates.

5.2.2 Results

To start, we focus on treatment effects explained by changes in school quality α_{jt} . Appendix Figure E.18a 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 Figure E.18a capture both relative improvements in α_{jt} and allocative changes of students to schools with higher α_{jt} . We find that most of the effects are captured by improvements in α_{jt} , although we do observe that allocative changes also play a small role.²⁵

²³We suppress time indices for notational ease.

²⁴For 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.

²⁵To explore if the effects displayed in Figure E.18a are mainly driven by allocative changes, we decompose aggregate changes in α_{jt} into two components. The first component holds enrollment shares constant at pre-policy levels and isolates the share of the aggregate improvement driven by changes in α_{jt} . The second component holds α_{jt} constant and isolates the share of the aggregate improvement driven by changes in enrollment shares ω_{jt} . We

Next, we isolate the portion of the achievement effects driven by improvements in student match quality, a source of gains that arise from the choices families make. Intuitively, an expanded choice set introduces scope to select schools that more adequately suit particular students’ needs and, as a consequence, indicates the potential for achievement effects even in the absence of competitive effects. Figure E.18b 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. Although evidence of substantial match effects in the context of inter-district school choice (Bruhn, 2019), the evidence is mixed in terms of cross-school match effects (Abdulkadiroğlu et al., 2020, Bruhn, 2019, Bruhn et al., 2023). Panel A of Appendix Table E.2 summarizes the decomposition evidence and additional heterogeneity analysis.

Three comments are warranted about this evidence. First, the effects explained by α_{jt} are consistent with the competitive effects conjecture but still do not rule out other contemporaneous shocks as a potential explanation. In Section 6.3, we leverage policy-specific variation in OVG to further test the competitive effects hypothesis. That variation captures (albeit imperfectly) differences in the competitive pressures schools faced at the start of the program and thus provides more direct test of the competitive effects hypothesis. Second, the roughly homogeneous population of ZOC students—both within and between zones—suggests that the scope for match effects on observables is minimal, so it may not be surprising that we find that observable match effects play a minimal role explaining the overall effects. Third, relatively low variation in β_j among ZOC schools compared to non-ZOC schools could also mute any potential effects; we find some suggestive evidence for that for some attributes but not all.

5.3 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 et al. (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 4 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 5.2 and 5.3 provide suggestive evidence that schools respond to competition, with the schools facing the most pressure improving the most.²⁶ However,

find that most of the aggregate improvement among ZOC schools is due to changes in α_{jt} and not enrollment shares ω_{jt} . Appendix Table E.1 reports the estimates from this exercise. We find modest allocative effects, with a more pronounced effect driven by improvements in α_{jt} among ZOC schools relative to non-ZOC schools.

²⁶This claim ultimately depends on the elasticity of enrollment with respect to school effectiveness, and whether the elasticity is decreasing in effectiveness. The estimates reported in Table 2 combined with logit assumption governing unobserved preference heterogeneity provide a setting where this claim is true.

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.

6 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—derived from rank-ordered preference lists—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.

6.1 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 et al., 2005).²⁷ The model in Section 3 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 et al., 2005).²⁸ 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 as-

²⁷The ZOC setting provides an advantageous feature in that students residing within a zone must rank all schools within their zone and only schools within their zone. Therefore, we observe complete rankings for all students within each zone regardless of attendance, and we do not face the issues that arise with endogenous choice sets.

²⁸Linear distance costs are reasonable in a setting with small markets. Nonetheless, imposing this for each zone may be a particularly strong assumption given the spatial variation within markets. We consider models that allow for quadratic distance costs as robustness checks discussed later.

sumptions, the preference profile for each applicant is as follows:

$$R_{ik} = \begin{cases} \arg \max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg \max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}. \quad (4)$$

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

$$\mathcal{L}(R_i | \delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r | U_{ir} < U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \quad (5)$$

We aggregate the log of Equation 5 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 et al., 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 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.

6.2 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 Section 5.2.1. We estimate

$$\delta_{jct} = \xi_{cz(j)t} + \omega_P Q_{jt}^P + \omega_S \alpha_{jt} + \omega_M Q_{jct}^M + u_{jct}, \quad (6)$$

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 2 reports estimates of Equation 6. 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 contrast to other settings, one notable feature of the ZOC setting is the homogeneity of students within each zone, effectively eliminating the selection of schools 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 (e.g., Abdulkadiroğlu et al., 2020 and Rothstein, 2006).

Another reason stems from the lack of choice overload present in the ZOC setting. Beuermann et al. (2023) similarly estimate preferences in Trinidad and Tobago, where families' choice sets are substantially smaller than New York City (Abdulkadiroğlu et al., 2020) or Romania (Ainsworth et al., 2022), and find that families do place significant weight on school quality. ZOC choice sets include at most five campuses to choose from, so it is likely that the lack of choice overload is also contributing to the findings.

Last, an often-advanced hypothesis for parents' modest preferences for school quality relates to information frictions. 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 underestimate school quality by 5 percentile points. This indicates that ZOC families tend to have approximately accurate beliefs about school quality, on average. This is another reason why we may find a relatively salient preference for school quality in ZOC markets. In Section 7 we further discuss why certain features of ZOC may have facilitated families' acquisition of information, addressing potential information barriers that are also prevalent in other settings.

6.3 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 3, 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.²⁹ 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.³⁰ 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.³¹

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 5.1 with interaction terms that capture functions of student OVG. We consider the following specification:

$$Y_i = \mu_{j(i)} + \mu_{t(i)} + \beta Post_t \times ZOC_{j(i)} + \gamma Post_t \times ZOC_{j(i)} \times f(OVG_i) + \mathbf{X}_i\psi + u_{it}, \quad (7)$$

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 3 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 5.1.1 and, importantly, $\gamma > 0$. However,

²⁹We use preference parameters corresponding to the first cohort of ZOC students to estimate student OVG for all cohorts. We impose this restriction to avoid the program's influence on the demand of future cohorts. Therefore, we project the preferences of the initial cohort on subsequent and past cohorts to construct measures of OVG that are free of the program's influence on demand. Appendix Figure H.1 displays the distribution of OVG across students, and Appendix Table H.1 reports OVG correlates.

³⁰We 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.

³¹In particular, because we can assign OVG to students in the pre-period, there are high-OVG students who reside in ZOC neighborhoods both before and after the policy expansion. In addition, even among students classified as high-OVG students, some eventually enroll in ZOC schools, while others do not. These features are crucial for identifying high-OVG effects.

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.³² Column 8 further probes this heterogeneity in a descriptive exercise that conditions on school value-added, an outcome affected by the treatment. This effectively absorbs the ZOC main effect, but the treatment effect heterogeneity remains to suggest that the achievement of students who would have otherwise enrolled in less popular schools increased more than other ZOC, regardless of the school they actually enrolled in.

Overall, the findings reported in Table 3 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.

7 Discussion

In this section, we emphasize changes in management practices and their proxies (Angrist et al., 2013, Bloom and Van Reenen, 2007, Bloom et al., 2015, Fryer, 2014) and intermediate outcomes that are precursors to college enrollment, achievement, and high school graduation effects. We conclude by discussing particular ZOC features that potentially facilitated the competitive effects we find.

7.1 Additional Mechanisms and Intermediate Outcomes

The stark achievement and college enrollment effects are a consequence of changes occurring within schools, but nailing the precise mechanisms is challenging mostly due to data limitations. For example, an extensive growing body of evidence finds that no-excuses teaching practices tend

³²The grouping of students into low- and high-OVG students is subject to some error that attenuates estimates. In particular, $HighOVG = \mathbf{1}\{\hat{OVG}_i = OVG_i - e_i > c\}$, so that our estimation error e_i will lead us to misclassify students; some high OVG students will be classified as low, and some low OVG students will be classified as high. We can probe at this by considering excluding observations around c , the cutoff point in our classification. The intuition being that as we move away from c , we arrive at settings where e_i has to be significantly large to allow misclassification, so that we approximate situations where the share of high OVG students we estimate matches the true share. These situations are less prone to suffer from attenuation bias. We find that to be the case, indicating the estimates reported in Table 3 are conservative.

to predict treatment effects in both charter and traditional public schools (Angrist et al., 2013, Dobbie and Fryer Jr, 2011, Fryer, 2014), but we do not have information about these teaching practices within ZOC schools.³³ While it is virtually impossible to study these in isolation in our setting, we can focus on disciplinary outcomes to probe at that teaching practice. In prior work (Dobbie and Fryer Jr, 2011), emphasis on discipline is one of the most predictive practices of the No-Excuses approach. We therefore probe one of the strongest correlates of the no-excuses approach—emphasis on discipline—by studying ZOC effects on suspensions. Effects on suspensions would suggest that school philosophy changed in a similar way that predicted treatment effects in other settings (Angrist et al., 2013, Dobbie and Fryer Jr, 2011, Fryer, 2014).

Panel A of Table 4 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. This evidence suggests that disciplinary practices sharply changed between ZOC and non-ZOC schools, an imperfect but suggestive finding suggesting that school philosophy changed and somewhat mirrored the no-excuses approach to urban education (Thernstrom and Thernstrom, 2004). 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 et al. (2013) in that effective urban charter schools impact achievement, disciplinary incidents, and attendance.³⁴

Next, we study students’ perceptions of their teachers using the School Experience Survey (SES) that LAUSD administers every year starting in 2011. One survey item asks students how they perceive teacher effort, and it is somewhat—but not perfectly—stable over time.³⁵ The survey item aims to get a sense of how students feel about teachers’ willingness to help them with their coursework when they need help. 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. Appendix Figure H.8 shows that ZOC students, compared with non-ZOC students, exhibited a greater increase in the likelihood of agreeing that their teachers help them with coursework when they need it. 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.

Through various approaches, we provide suggestive evidence that changes in schooling prac-

³³The no-excuses approach to urban education emphasizes discipline and comportment, traditional reading and math skills, instruction time, and selective teacher hiring (Angrist et al., 2013, Thernstrom and Thernstrom, 2004).

³⁴The evidence suggests a change in the underlying philosophy and teaching practices that correlate with the no-excuses approach. However, harsh disciplinary practices have been criticized, and evidence suggests this leads to a culture of stress where students may not feel best suited for success (Golann, 2021). Appendix Figure H.8 reports treatment effects on students’ perceived satisfaction and shows that, if anything, ZOC students report higher rates of satisfaction following the policy expansion. While not decisive, the evidence suggests that changes in the implied school philosophy did not detrimentally affect student satisfaction in the ZOC setting.

³⁵Between 2011 and 2014, students were asked to respond with “Strongly Agree,” “Agree,” “Disagree,” or “Strongly Disagree” to the following statement: “If I don’t understand something in class, my teachers work with me until I do.” Since 2014, the survey item has been as follows: “My teachers work hard to help me.”

tices occurred, and these changes are a natural mediator of the treatment effects documented throughout the paper. In Appendix H we consider additional changes, including changes in tracking practices (Betts, 2011, Bui et al., 2014, Card and Giuliano, 2016, Cohodes, 2020, Duflo et al., 2011) and student-teacher racial matches (Dee, 2004, 2005, Fairlie et al., 2014, Gershenson et al., 2018). The motley of findings point to changes along many dimensions, including teachers and their effort, principals, and overall school philosophy.

We conclude our empirical analysis by focusing on intermediate outcomes that are a precursor to the treatment effects we document elsewhere in the paper. Panel B of Table 4 reports difference-in-differences estimates on various measures of college preparation. To begin, we find that ZOC students' UC and CSU minimum course requirements increased by 6 percent. This outcome is a natural intermediate outcome that eventually contributes to college enrollment impacts. We do not find that ZOC students increased their SAT-taking rates, but conditional on taking the SAT, we find substantial increases in SAT scores. The 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. Focusing on specific sub-components of the test, we find improvements in all domains of roughly equal magnitudes in terms of the California SAT score distribution. The evidence presented in Panel B suggests that ZOC students adjusted their portfolio of classes and effort, leading to improved college readiness in terms of the minimum requirements to apply and in domains where teaching to the test is less pronounced within public schools. This final piece of evidence points to potential changes in both student and teacher effort.

7.2 Institutional Features of ZOC

We now highlight specific ZOC institutional features that may have helped pave the way for this constellation of findings that stand in contrast or differ from prior literature.

ZOC-specific administrators are an instrumental part of the school choice process. They devote considerable resources to ensuring each cohort is informed about the application process and knows its schooling options, and they also provide anecdotal information about the defining characteristics of their schools or the ZOC program. Each administrator is assigned a zone or pair of zones and conducts dozens of informational sessions in the months leading to the application deadline. Importantly, this approach ensures some level of personalization between parents and the ZOC administrator assigned to their zone, and personalization has been shown to improve information usage (Page et al., 2020) and improve neighborhood quality choice (Bergman et al., 2019).

Another potentially important factor is the relatively small neighborhood-based choice sets that families face. In a setting such as New York, for example, where parents must select from a menu of more than 750 schools, they may resort to simplified strategies in selecting schools (Corcoran et al., 2018). Not only does ZOC's more personalized approach allow more information to be provided about the program, but the restricted nature of parents' choice sets implicitly eliminates choice overload concerns present in other school choice settings. Beuermann et al. (2023) emphasize the potential importance of smaller choice sets in a setting that also finds that parents place significant weight on school quality.

One final ZOC-specific feature is the relative homogeneity of the student population. Roughly 85–90 percent of ZOC students are classified as Hispanic and poor by the school district. The zones avoided combining catchment areas that differed vastly in socioeconomic composition, further limiting heterogeneity within zones. The highly segregated nature of the zones produces two countervailing forces that are worth further discussion. On the one hand, it is possible that the relative homogeneity helped incentivize competition among schools by eliminating sorting on race or income. On the other hand, there is a growing body of evidence pointing to adverse impacts of segregated schools or positive impacts of desegregating schools (Billings et al., 2014, Card and Rothstein, 2007, Johnson, 2011). While we find that both short- and medium-run ZOC student outcomes improved, it remains to be seen whether ending K–12 education in racially isolated schools will harm this same set of students. In addition, it remains an open question whether another similar program would produce the same effects if it created zones that integrated students across race and income levels.

8 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.

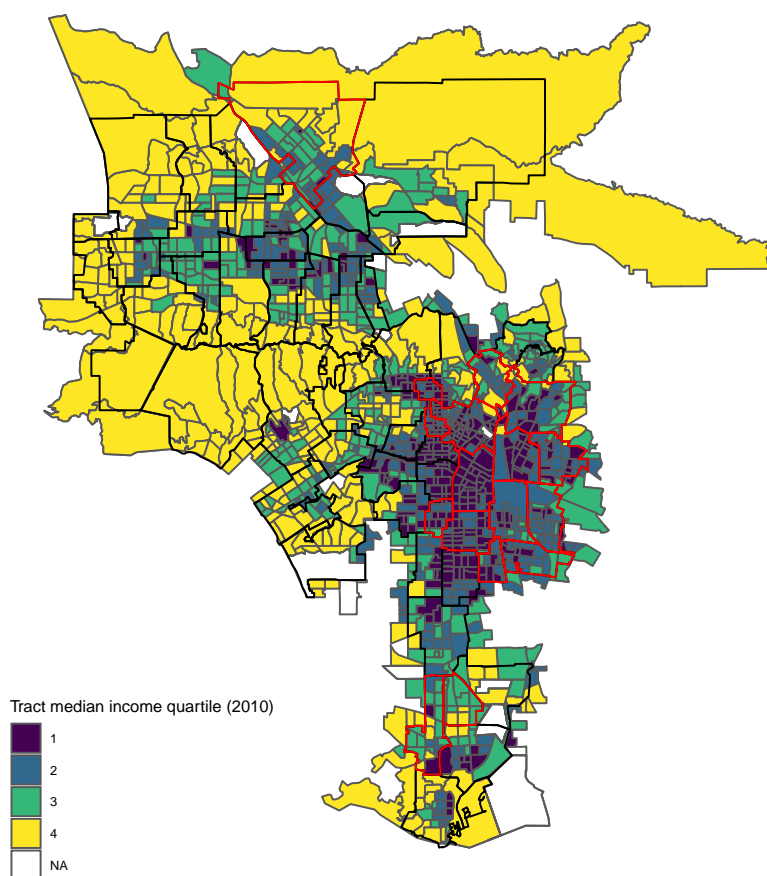
This paper studies the transition from neighborhood-based assignment to a version of centralized assignment, a program referred to as Zones of Choice (ZOC). The unique design and implementation of ZOC provide a rich setting to study the market-level effects of choice and competition among public schools, 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

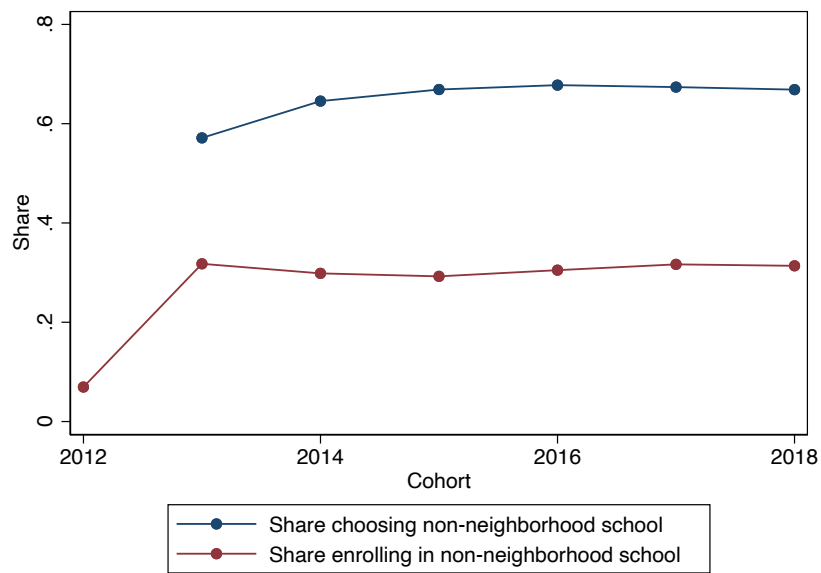
competition, our model does not inform us about what produces the predicted gains and does not speak to potentially adverse long-run effects of racial and economic segregation of students. The mechanisms through which schools adjust, the factors contributing to parents' ability to distinguish between effective and ineffective schools, and the long-run effects of the program are important topics for future research.

Figure 1: ZOC and 2010 Census Tract Income



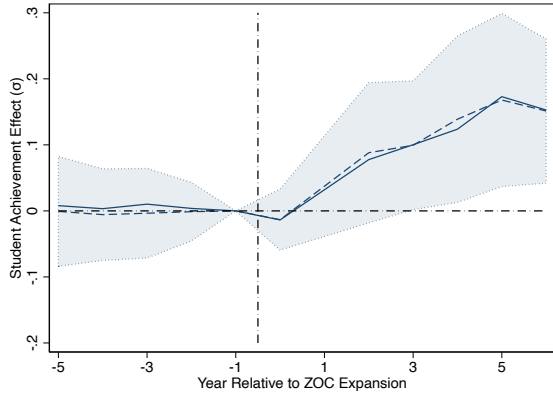
Notes: This figure plots census tracts across Los Angeles County. Each census tract is shaded according to the median income quartile they belong to in 2010, across all other census tracts in Los Angeles County. High school and ZOC attendance zone boundaries are overlaid on top, with ZOC boundaries outlined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

Figure 2: Demand and Enrollment for Non-Neighborhood Schools

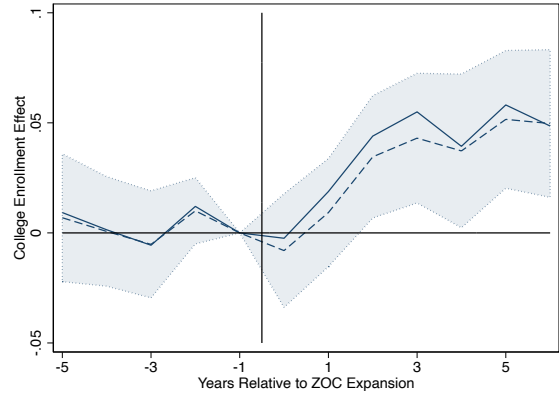


Notes: This figure reports statistics concerning application behavior of ZOC applicants. If we observe a ZOC applicant enroll in an LAUSD high school in ninth grade, we classify them as staying in the district. If we observe a ZOC applicant rank a school other than their neighborhood school as their 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.

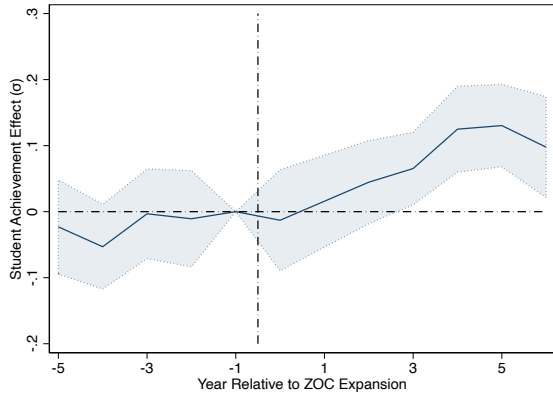
Figure 3: Market-Level Effects



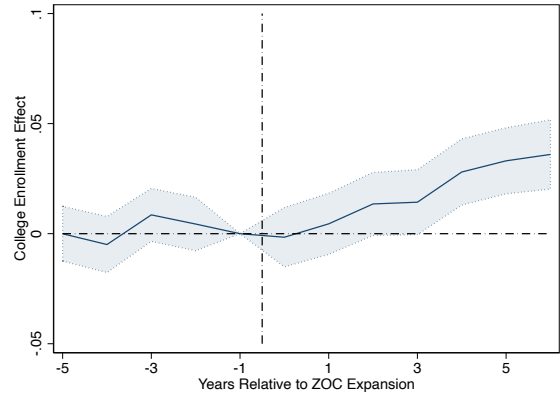
(a) Achievement



(b) Four-Year College Enrollment



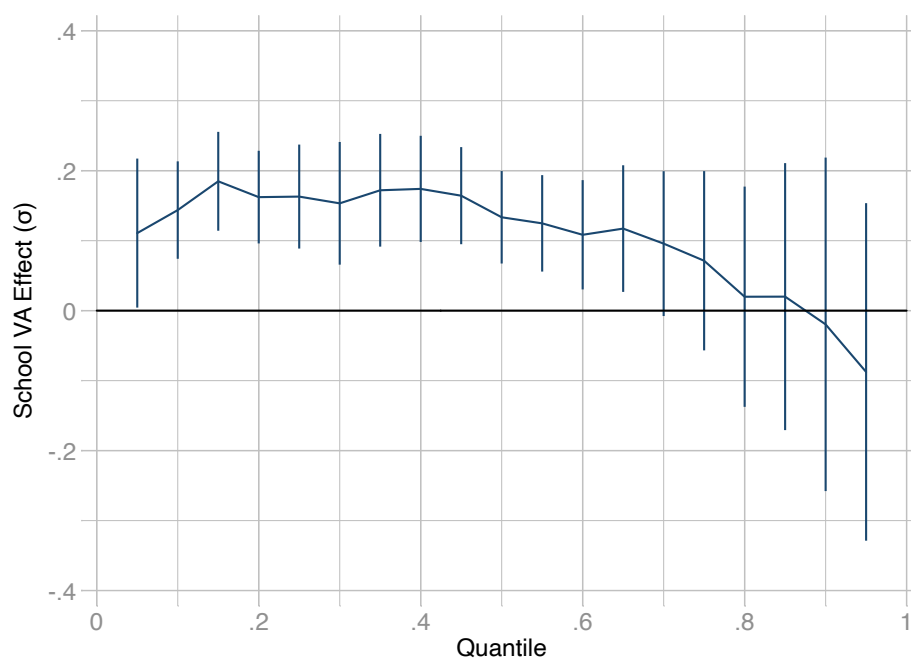
(c) ITT: Achievement



(d) ITT: Four-Year-College-Enrollment

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 Equation 12 of Appendix E.3. 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 4: 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 et al. (2013, 2020). Bootstrapped standard errors are used to construct 95 percent confidence regions.

Table 1: 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 Share	.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. All standard errors are clustered at the school level.

Table 2: 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 [0.369]	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.

Table 3: Option Value Gain and Treatment Effect Heterogeneity

	(1) Reading	(2) Reading	(3) Reading	(4) Reading	(5) Reading	(6) Reading	(7) Reading	(8) 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)		0.009 (0.025)
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)	0.001*** (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)		0.005 (0.025)
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)	0.093*** (0.017)
Gender		X				X	X	X
Race/Ethnicity			X			X	X	X
SES				X		X	X	X
Lagged Test Scores					X	X	X	X
School VA								X
Zone-Year FE							X	
Observations	221,954	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 clustered at the school level.

Table 4: 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.

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

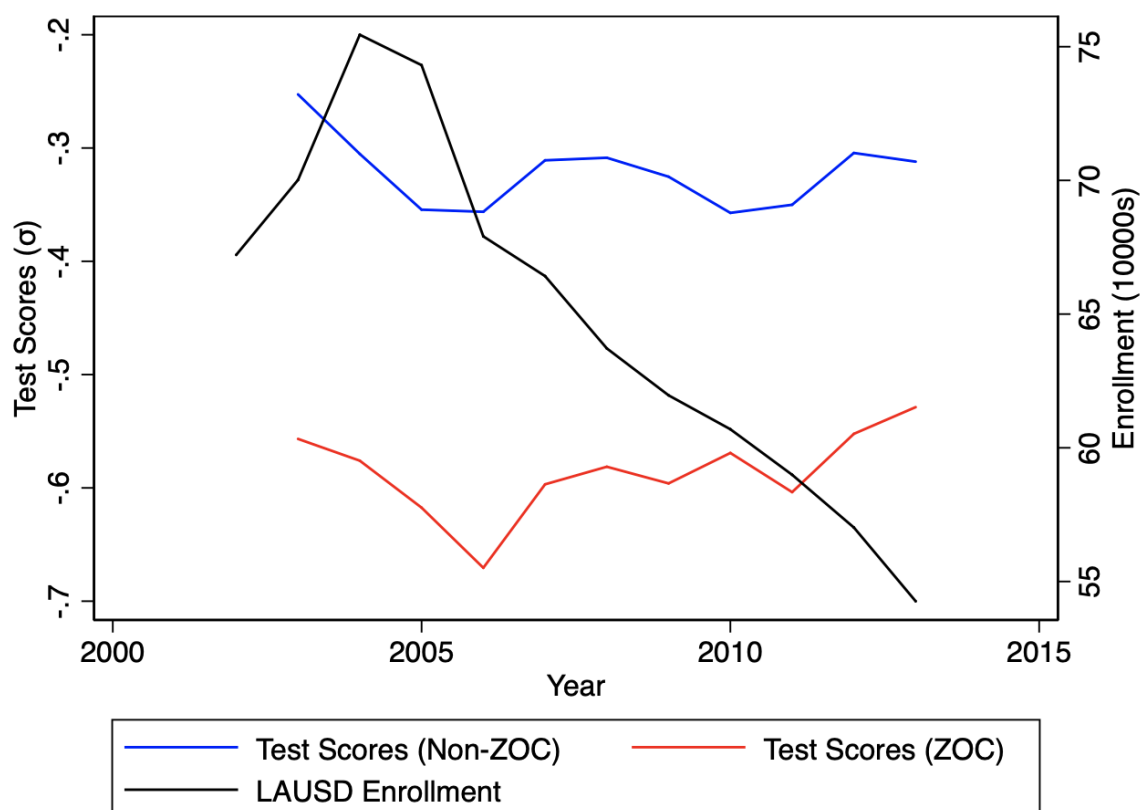
	(1)	(2)	(3)	(4)	(5)
	ZOC	Non-ZOC	Difference	Matched Non-ZOC	Difference
8th Grade ELA Scores	-.11	.12	-.23*** (.057)	.031	-.125** (.054)
8th Grade Math Scores	-.069	.129	-.198*** (.056)	.042	-.098* (.053)
8th Grade Math Scores	.139	.159	-.019 (.019)	.153	-.014 (.018)
Black Share	.034	.141	-.107*** (.028)	.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)
Students	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

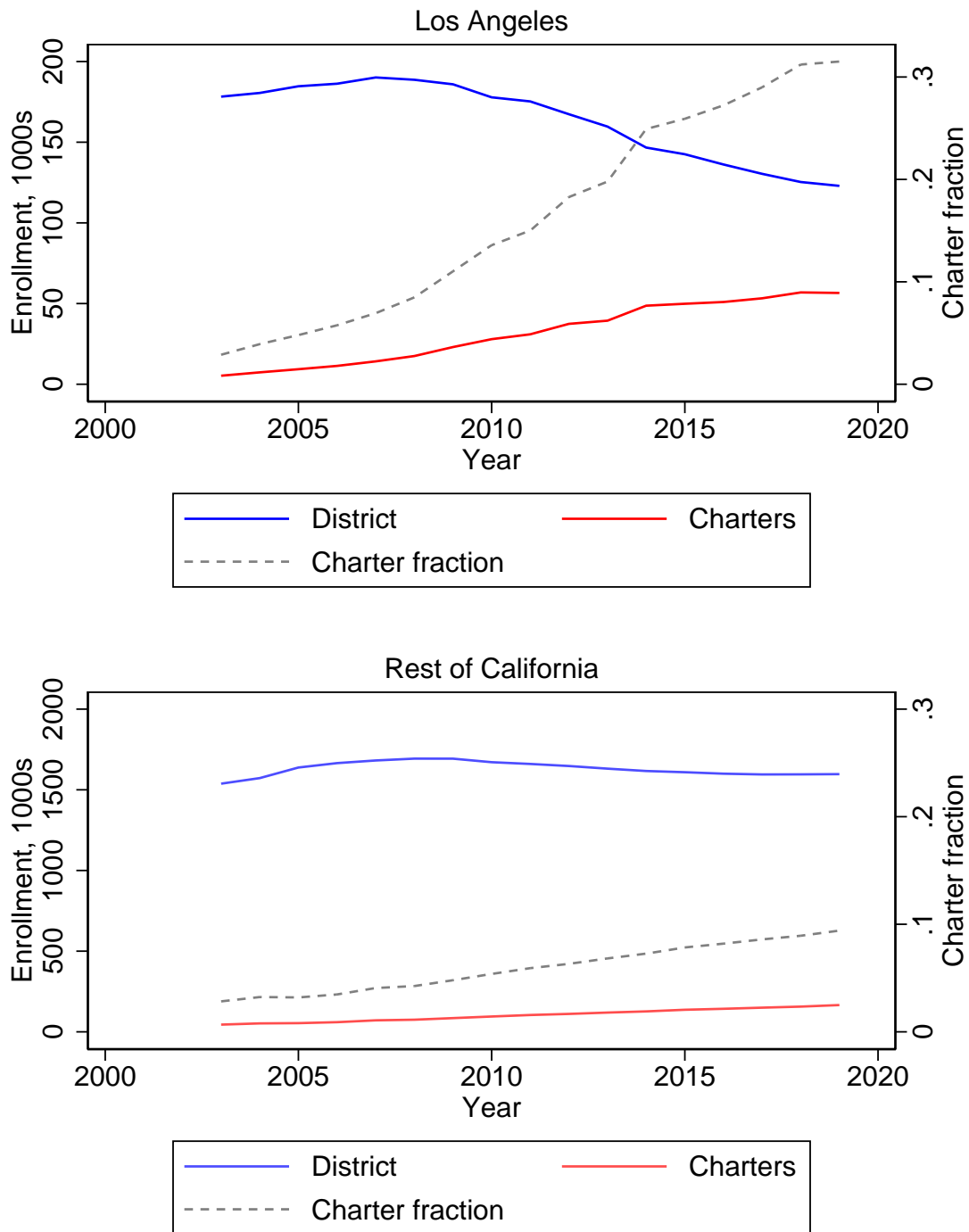


Notes: Enrollment numbers come from administrative data provided by Los Angeles Unified School District (LAUSD). The California Department of Education provides California Standards Test statewide means and standard deviations, which we use to standardize test scores in this figure. Test scores are ninth-grade 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.2](#) 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).

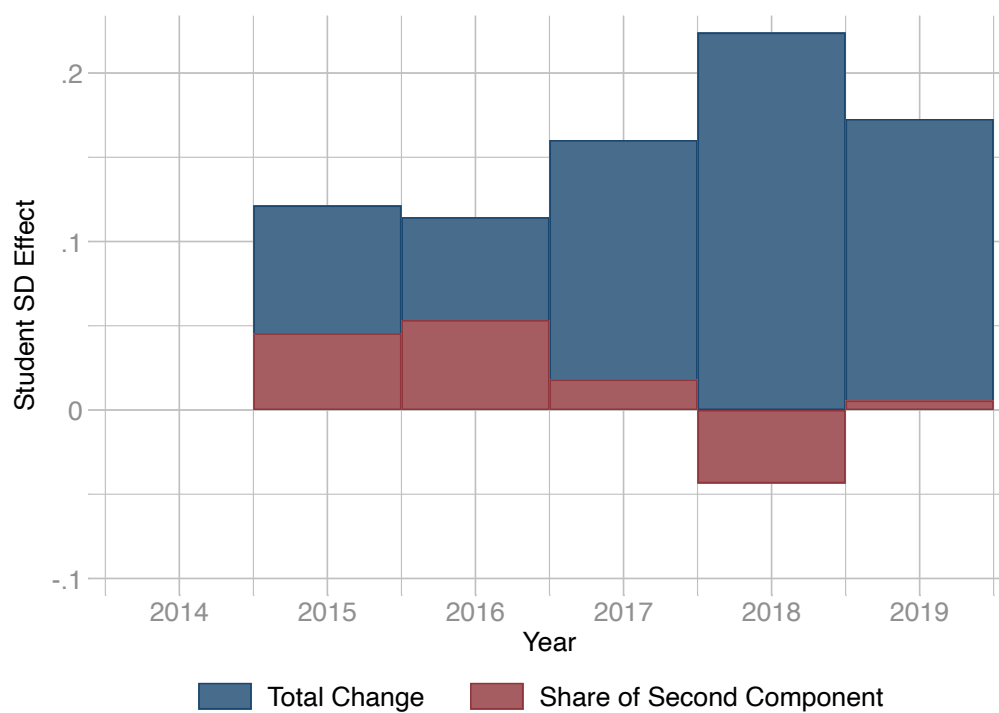
This implies that the difference-in-differences estimates are:

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

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

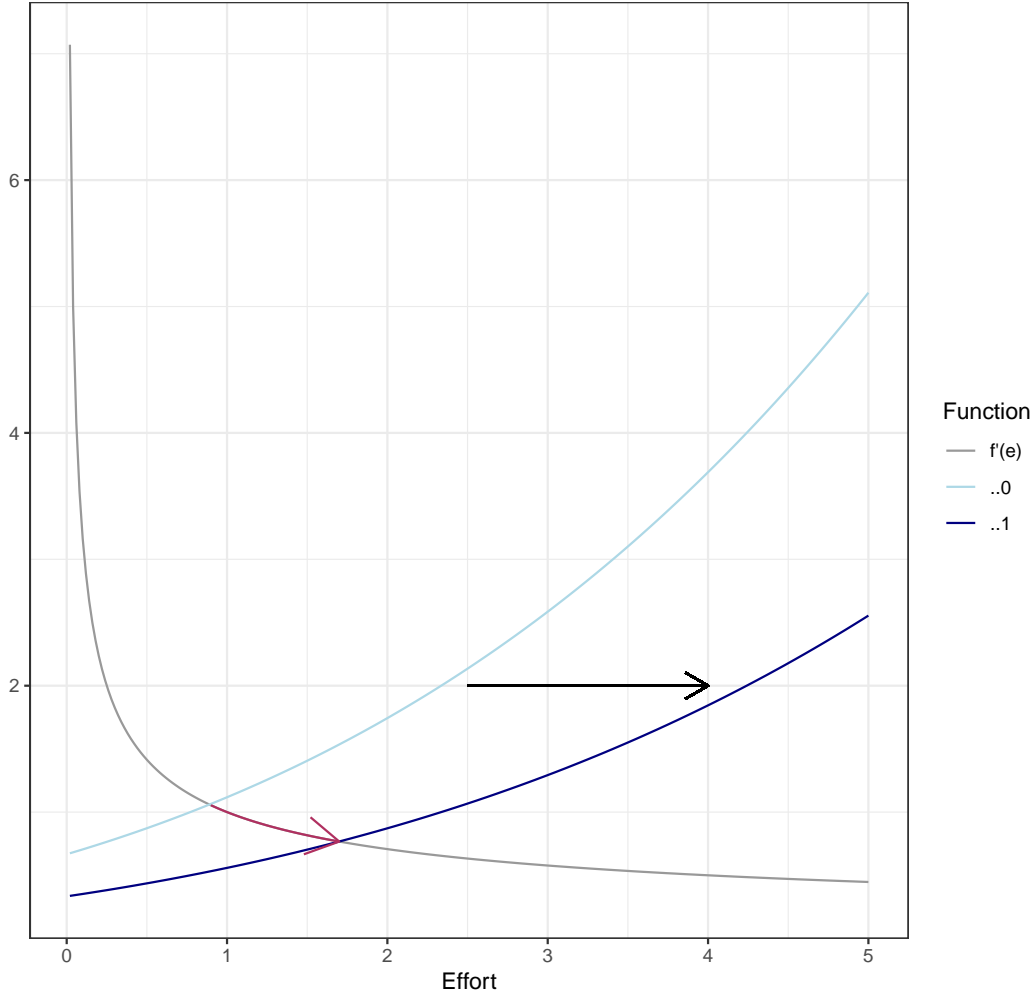
$$\Phi(e_j) = \frac{1}{\theta\omega \frac{1}{N} \sum_i P_{ij}(e_j; d_{ij}, X_i)(1 - P_{ij}(e_j; , d_{ij}, X_i))},$$

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 $k \neq j$:

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

□

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

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

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:

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

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

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

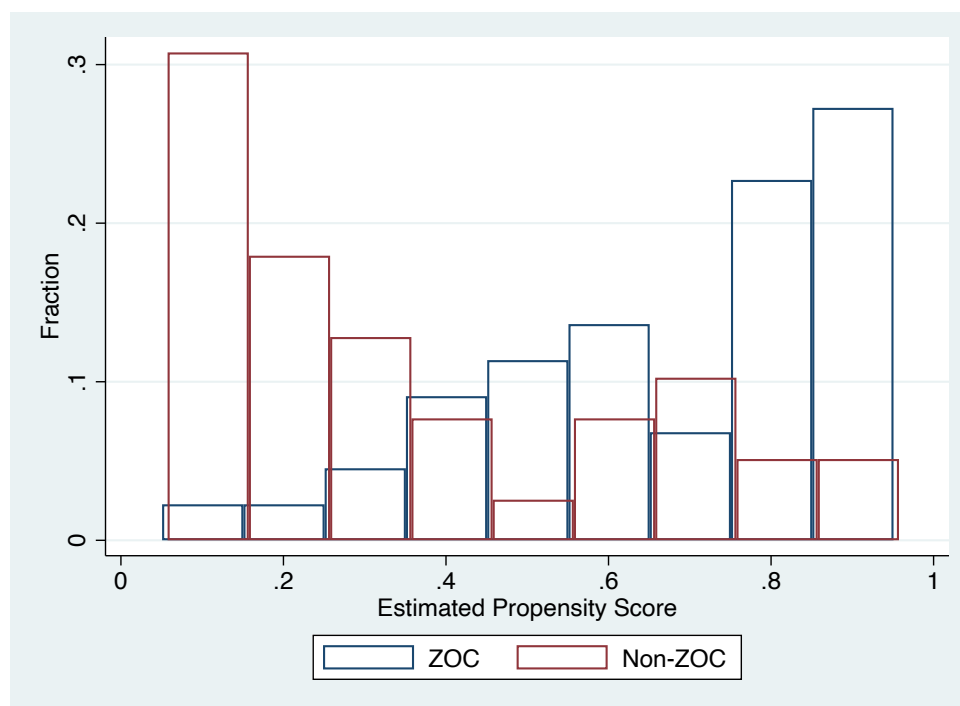
Therefore, for a marginal increase in \mathbf{OVG} , Φ shifts further downward, leading to increases in effort, and the strategic complementarities in Proposition B.2 imply a new equilibrium in which schools all exert more effort.

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 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 C.1 reports overlap and demonstrates there is support across the estimated propensity score distribution.

Figure C.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.

D Distributional Effects and VAM Tests

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:

$$\mathbf{1}\{A_i \leq a\} = \mu_{j(i)} + \mu_{t(i)} + \gamma_a \text{PreZOC}_{it} + \beta_a \text{PostZOC}_{it} + \mathbf{X}_i' \psi + u_i. \quad (8)$$

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.³⁶ 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.2 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

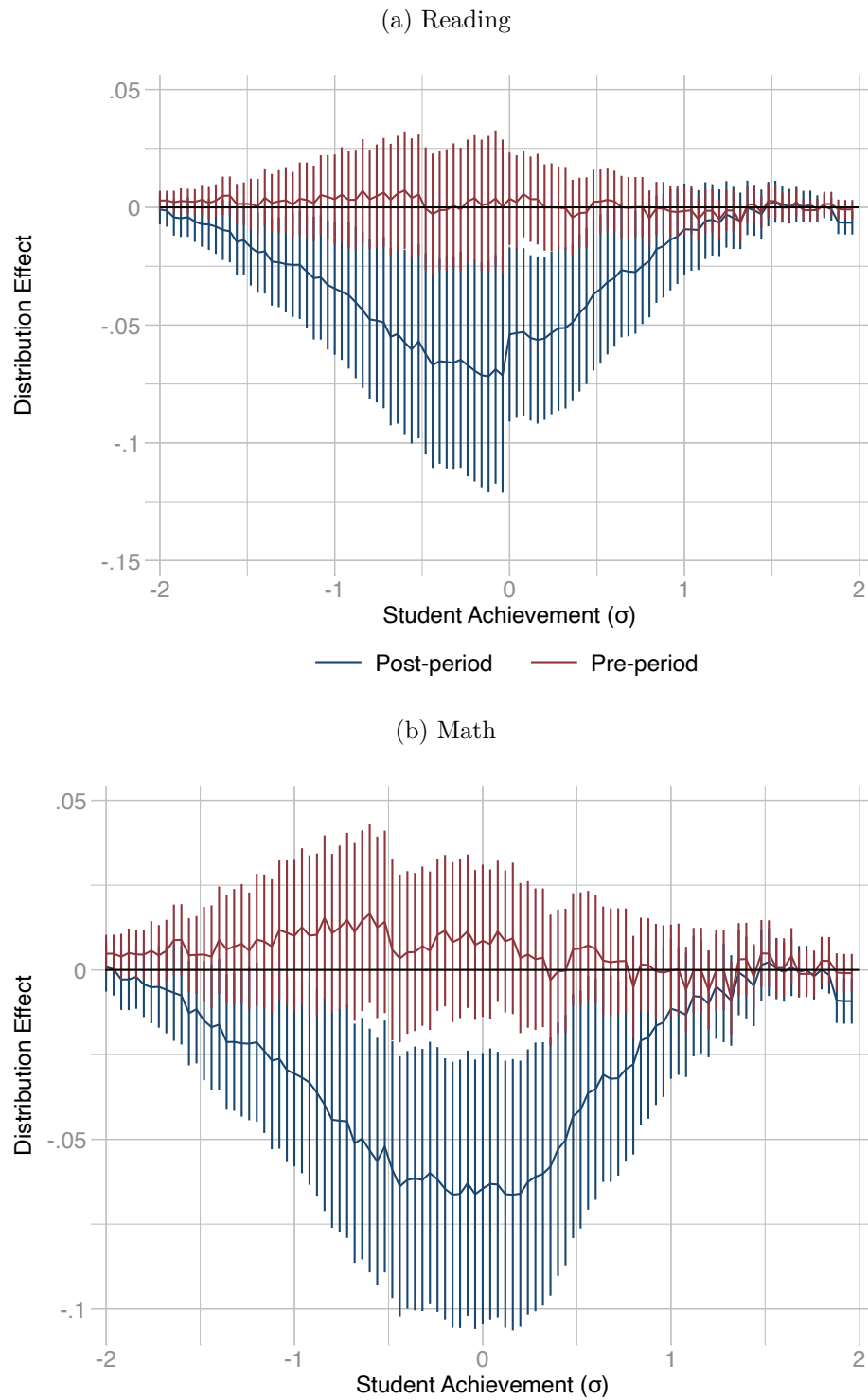
³⁶Variables in the model include all variables in Table 1 and their interactions. We use all pre-period years starting in 2008 and ending in 2012.

baseline mean as compared with a roughly 20 percent increase for students in the top two quartile groups.³⁷

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.

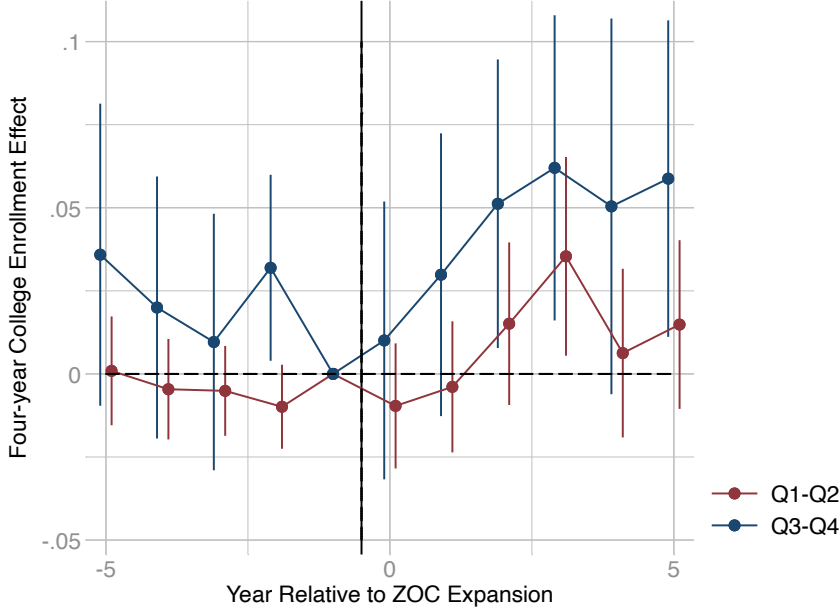
³⁷Appendix Figure E.1 reports trends by different quartile groups.

Figure D.1: Student Achievement Distributional Impacts



Notes: This figure reports estimates of β_a and γ_a from Equation 8 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 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.

D.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} :

$$E[A_{ij}|X_i, j(i) = j] = \alpha_j + \mathbf{X}_i' \beta_j; \quad j = 1, \dots, J. \quad (9)$$

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 et al., 2014, Deming et al., 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 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:

$$A_i = \xi + \phi \hat{A}_i + \sum_{\ell} \kappa_{\ell} Z_{i\ell} + \mathbf{X}'_i \delta + \varepsilon_i \quad (10)$$

$$\hat{A}_i = \psi + \sum_{\ell} \pi_{\ell} Z_{i\ell} + \mathbf{X}'_i \xi + e_i. \quad (11)$$

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 et al., 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 D.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 et al. (2014), Chetty et al. (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 3. 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 D.1 do not entirely rule out bias in OLS value-added estimates, they are reassuring.

Table D.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 3. 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.

E Additional Evidence and Robustness Exercises

E.1 Additional Evidence

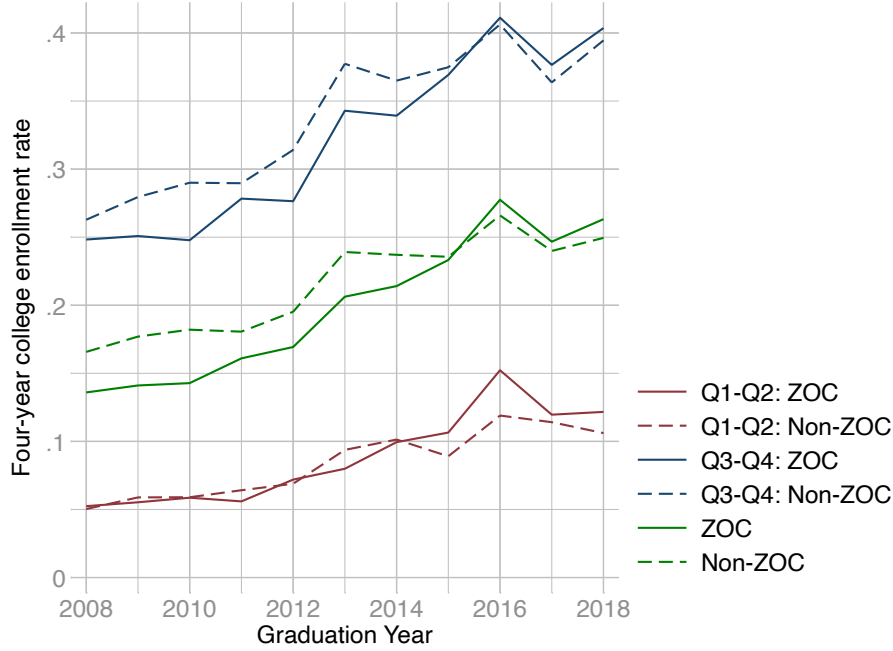
This section reports additional evidence alluded to in the main text. We begin by reporting time-series figures of college enrollment rates during our sample period, followed by an analysis of college enrollment effects for other non-four-year colleges. We conclude with a decomposition of the primary evidence discussed in a footnote of the main text.

Appendix Figure [E.1](#) reports raw college enrollment rates for various groups of ZOC and non-ZOC students. There is an overall increasing trend in the rates, with the mean college enrollment rate at the time of the policy expansion hovering at around 20 percent, with a roughly 2 percentage point ZOC-non-ZOC gap.

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.1: Four-Year College Enrollment Rates by Predicted Quartile Group



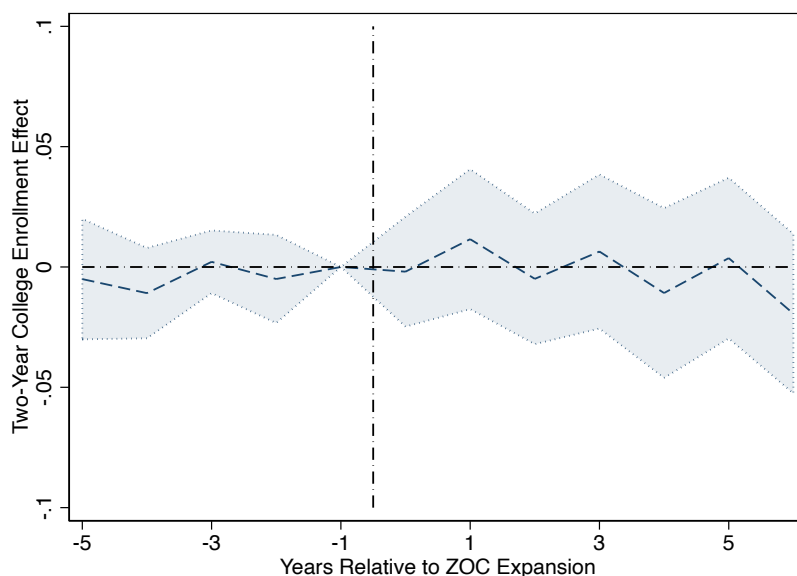
Notes: This figure reports college enrollment rates for students in different quartile groups by ZOC and non-ZOC student status. Solid lines correspond to ZOC students, and dashed lines correspond to non-ZOC students. Red lines correspond to students in the bottom two quartiles of the predicted college 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.

E.1.1 Heterogeneity

Panel B of Table E.2 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.

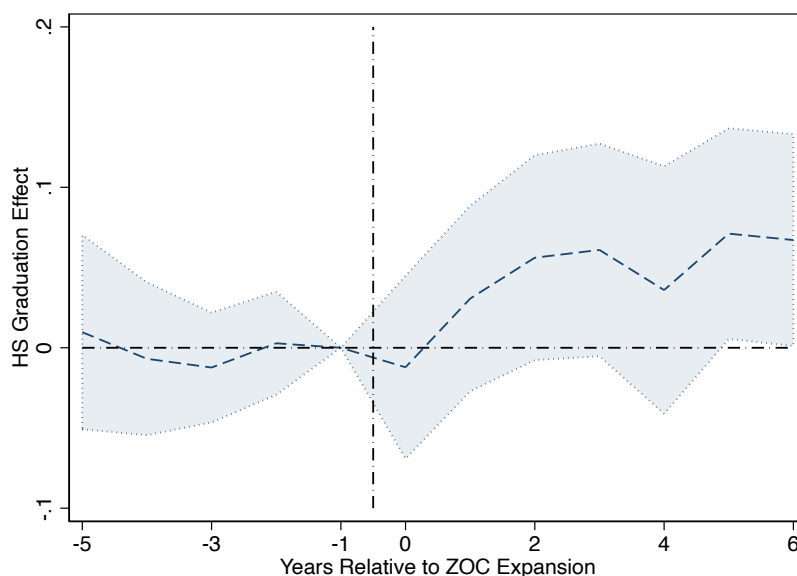
Taking the estimates at face value, however, suggests that White and Black students did not experience similar achievement gains as their Hispanic counterparts. Heterogeneity by sex suggests that both male and female students equally benefited from the ZOC expansion. Heterogeneity by socioeconomic status 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.

Figure E.2: Two-Year College Enrollment Effects



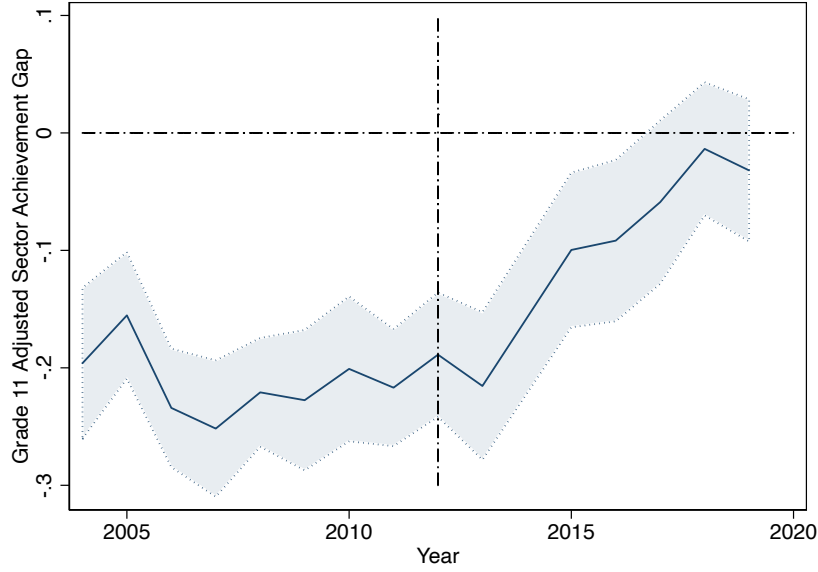
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The outcome is an indicator for two-year college enrollment, and the sample is the same as the primary achievement event-study evidence. The coefficient β_k 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.

Figure E.4: Eleventh-Grade ZOC Achievement Gaps



Notes: This figure reports estimates from regressions of student achievement on ZOC indicators interacted with year dummies, adjusting for student characteristics. We report estimates of achievement gaps in the solid lines with 95 percent confidence intervals reported by shaded regions.

Table E.1: Change in Effectiveness Decomposition

	(1)	(2)
	Zones of Choice	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.

Table E.2: Difference-in-Differences Estimates

	(1)	(2)	(3)	(4)
	N	Pre-ZOC	Post ZOC 0-2	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.

E.2 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.

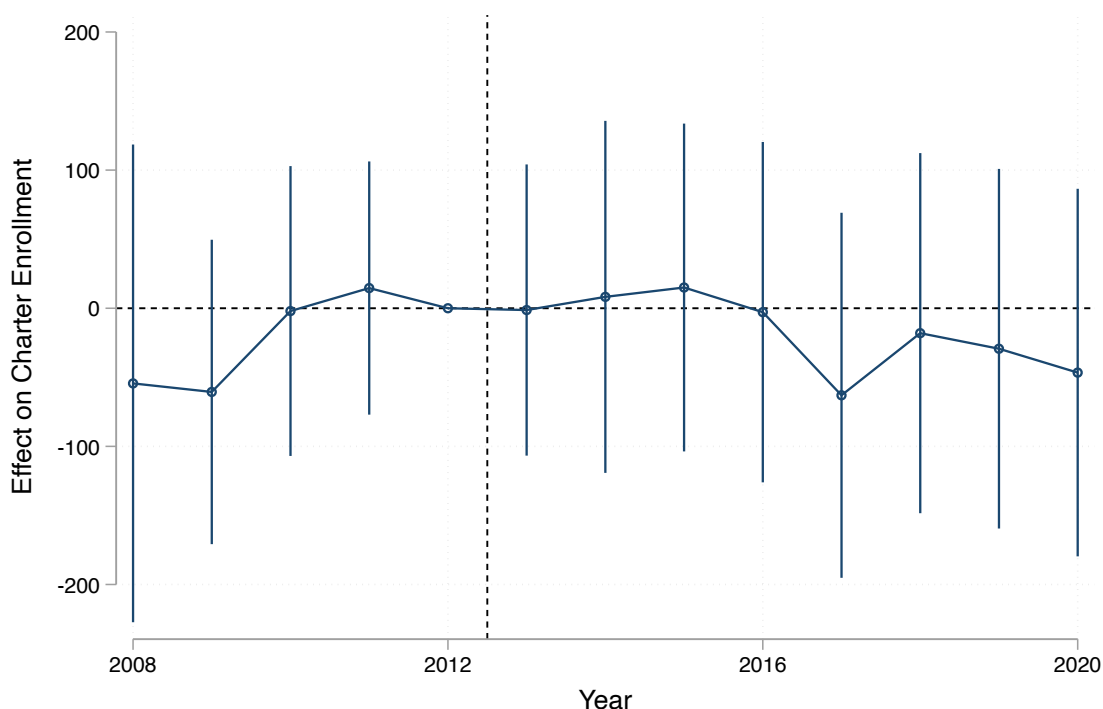
Appendix Figure E.5 reports event-study evidence comparing charter enrollment trends in ZOC neighborhoods to non-ZOC neighborhoods using the school-level sample. The evidence reveals that charter enrollment trends are not trending differently both before and after the ZOC expansion. This suggests that 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.6 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.7 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.8 demonstrates that magnet school enrollment for students living in a ZOC neighborhood was not differentially affected during our sample period.³⁸ 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.

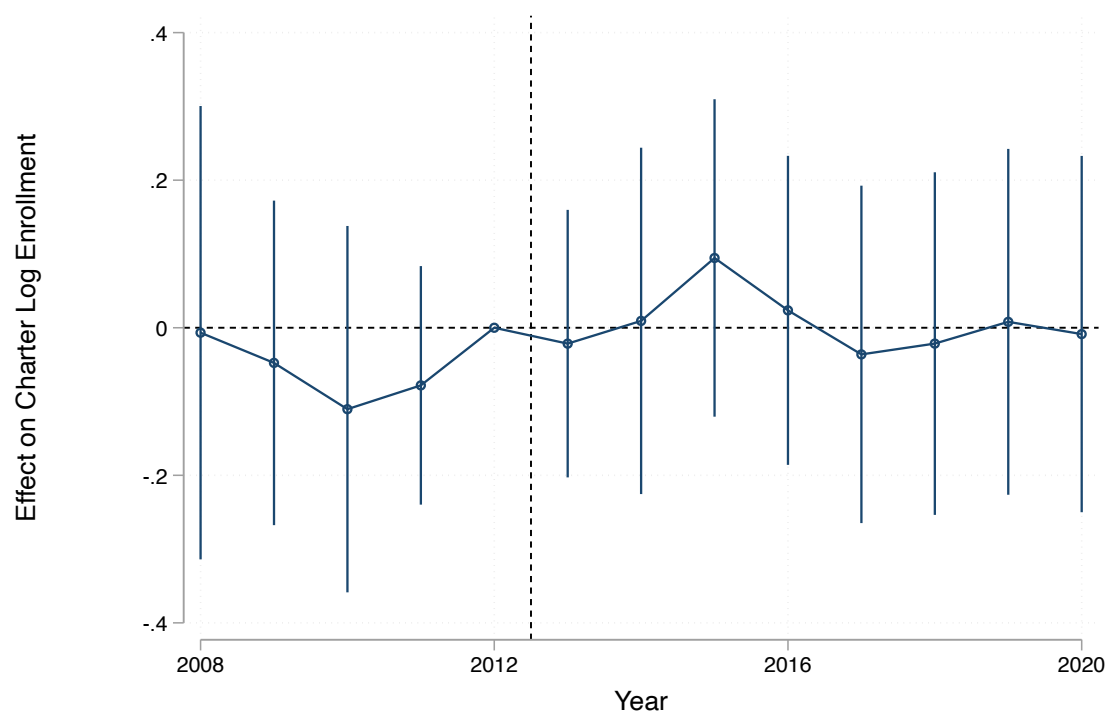
Figure E.5: Intensive Margin: Effects on Charter Enrollment



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.

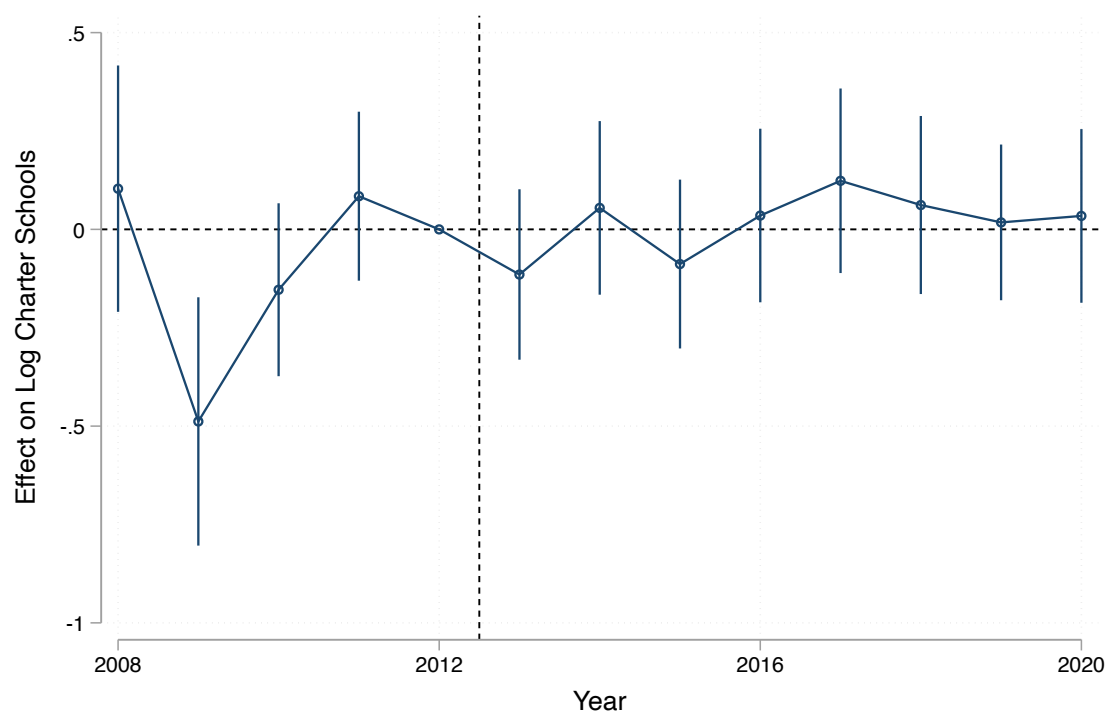
³⁸The sample used for this analysis is the same as in the primary analysis.

Figure E.6: Intensive Margin: Effects on Charter Log Enrollment



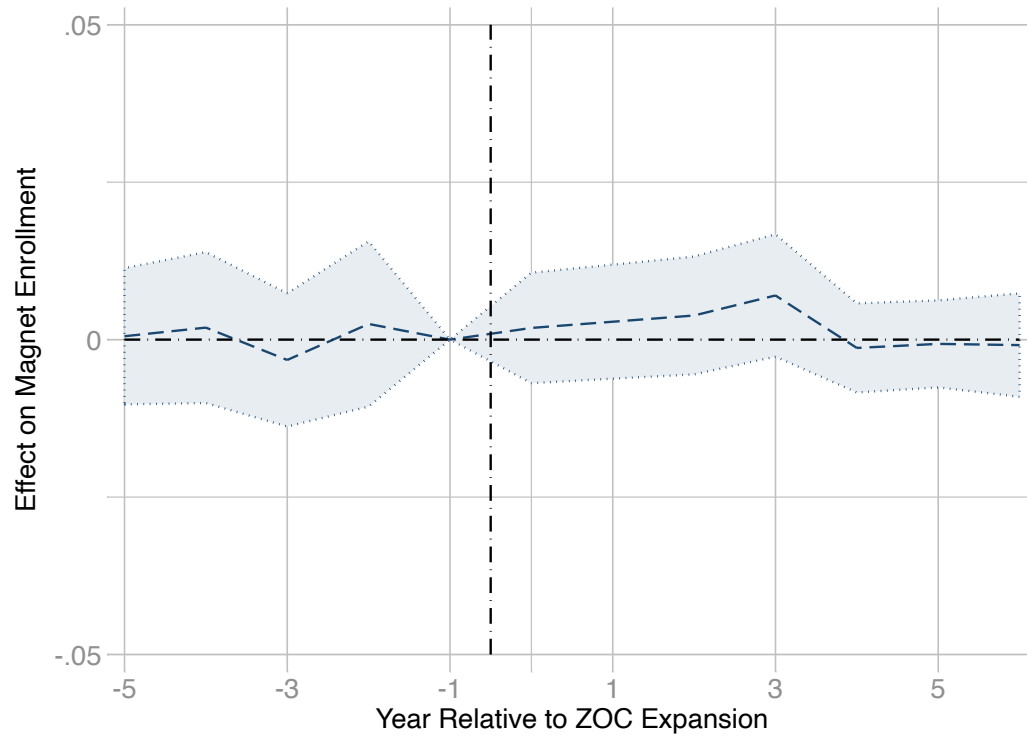
Notes: This figure reports estimates from regressions of charter-school-level log enrollment on year indicators, school indicators, and ZOC neighborhood indicators interacted with event-time indicators. The interaction term estimates are reported with 2012 as the omitted year. Charter school enrollment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

Figure E.7: 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.8: 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.3 Attendance Zone-Level Treatment

A primary concern in the research design outlined in Section 5.1 is the potential sorting of students into ZOC neighborhoods and schools. While we can show student demographics are not trending differently (Appendix Figure E.12) and that estimates are robust when restricting to the subset of students who do not move during middle school (Appendix Figures E.13 and E.14), 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 et al. (2014) and Fryer (2014).

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

$$Y_i = \mu_{z(i)} + \mu_{t(i)} + \sum_{k \neq -1} \beta_k ZOC_{z(i)} \times \mathbf{1}\{t(i) - 2013 = k\} + \mathbf{X}_i' \psi + u_i. \quad (12)$$

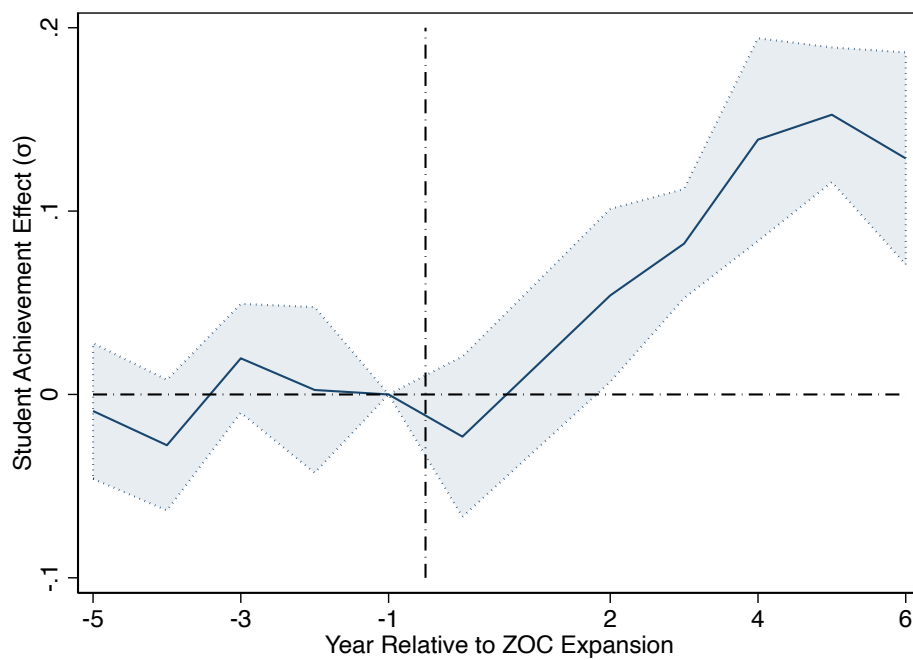
We define neighborhood in two ways. The first is at the attendance zone level and the second is at the neighborhood level. There are a total of 64 attendance zone levels that are fixed in the pre-period. There are a total of 23,833 census blocks in our sample. The latter specification allows us to absorb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

Appendix Figures E.9a and E.9b report the estimates from the alternative strategy, with treatment defined at the attendance zone level. The results are qualitatively similar to the primary event-study evidence outlined Section 5.1. 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.

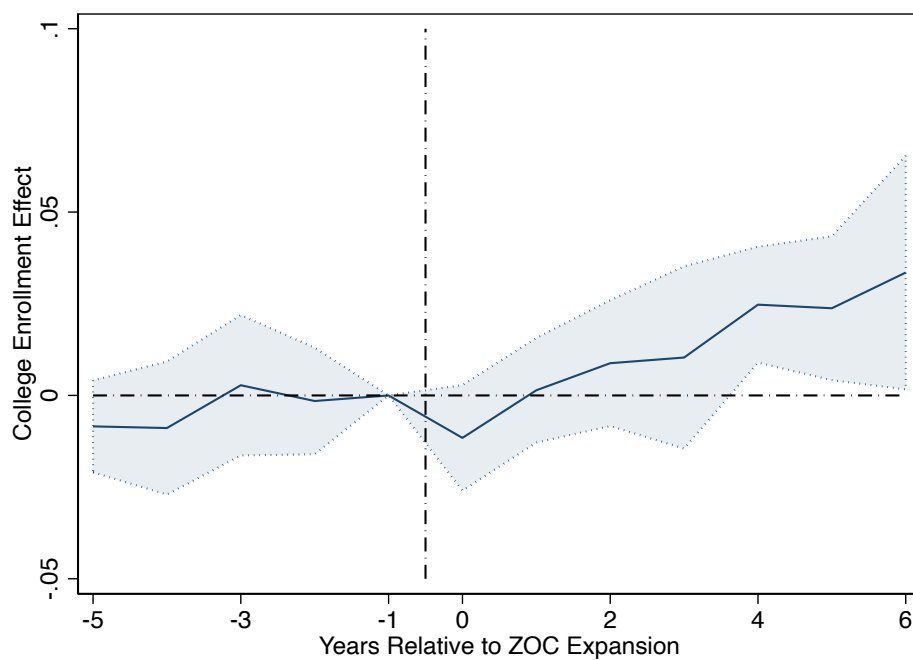
Next, we report estimates where neighborhood is defined by a student's census block during middle school. The results, displayed in Appendix Figures E.10a and E.10b, are again qualitatively similar, with a more robust treatment effect for four-year college enrollment rates. The robustness of the evidence to alternative research designs that define treatment at some pre-high school choice level provide reassuring evidence against sorting concerns. In the next section, we report additional exercises that assuage potential sorting concerns in the preferred specification in the paper.

Figure E.9: Achievement and College-Enrollment Event Studies: Attendance Zone Level Assignment

(a) Achievement Event Study



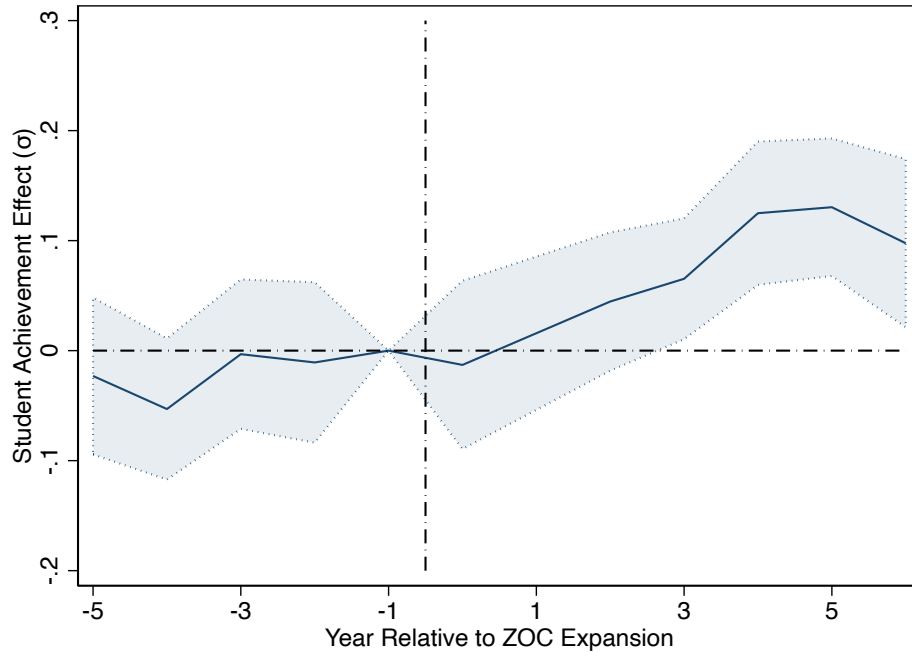
(b) Four-Year-College-Enrollment Event Study: Attendance Zone Assignment



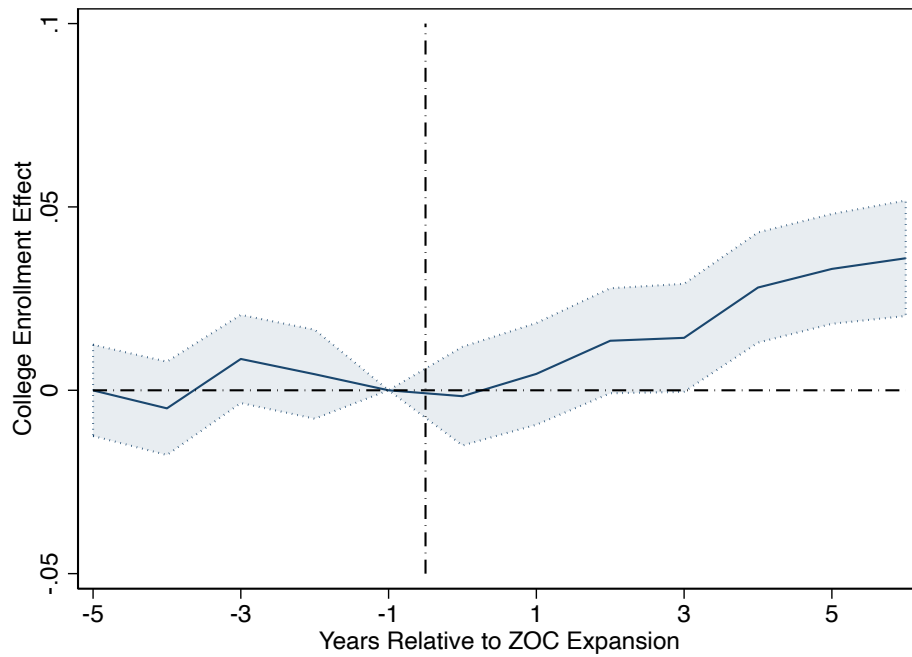
Notes: This figure reports estimate of β_k analogous to those defined in Equation 12, where k is the number of years since the ZOC expansion. Treatment is defined at the attendance zone level. The coefficient β_k shows difference-in-differences estimates for outcomes relative to the year before the policy. 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.10: Achievement and College-Enrollment Event Studies: Census Block Assignment

(a) Achievement Event Study

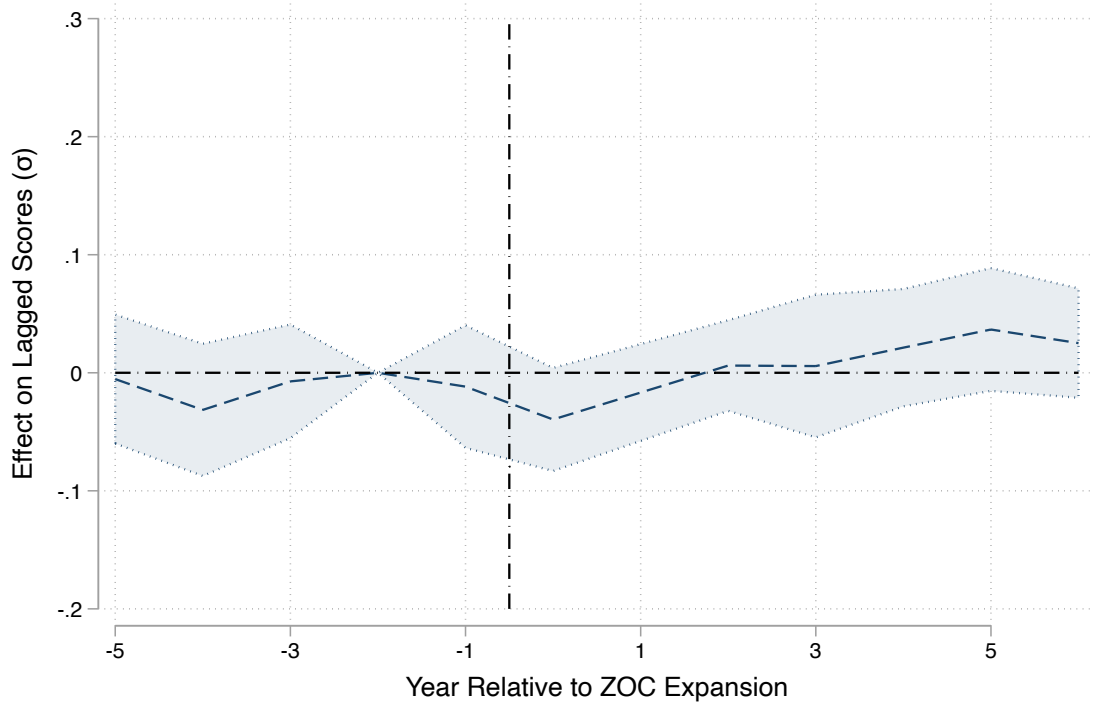


(b) Four-Year-College-Enrollment Event Study



Notes: This figure reports estimate of β_k analogous to those defined in Equation 12, where k is the number of years since the ZOC expansion. Treatment is defined at the census block 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.11: Treatment effects on lagged test scores



Notes: This figure reports estimate of β_k analogous to those defined in Equation 12, 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.4 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.12 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.13 and Appendix Figure E.14 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.12. 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.

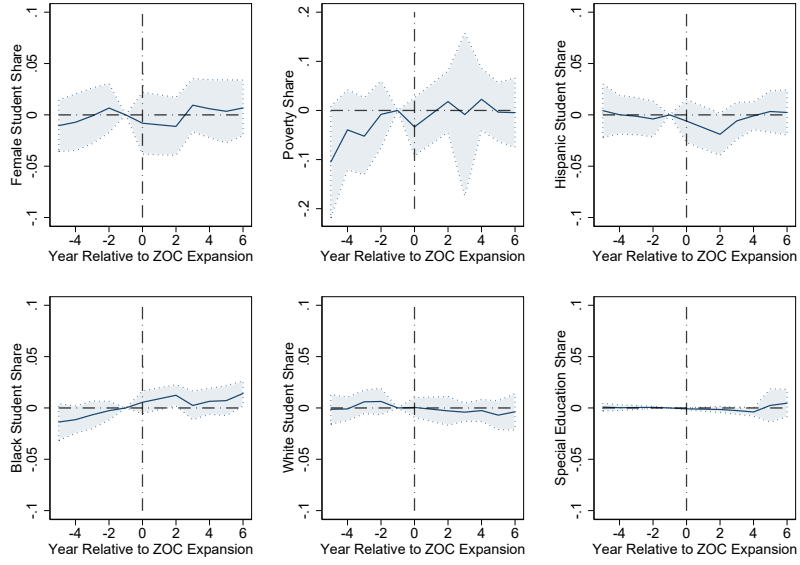
Next, we consider differential attrition out of the sample in Appendix Figure E.15. We do not find strong evidence of differential attrition out of the sample. This attenuates concerns that some of our estimates are driven by differences in attrition rates.

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.16](#) 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.

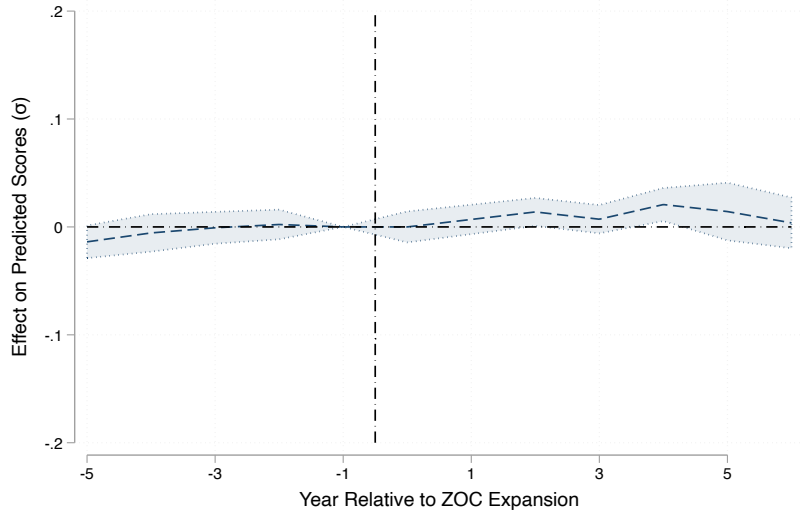
Last, we report estimates from a model that does not impose the school sample selection criteria used in our primary analysis. In particular, the sample of schools in these analyses include all magnet programs, continuation schools, and any schools that opened after the program expansion. Appendix Figure [E.17](#) reports these estimates and we find that our school sample selection criteria produce conservative estimates. Nonetheless, we found it important to restrict to the set of comparable schools in our primary analysis and chose to stick with the sample selection criteria discussed in the main text. The attenuation observed in the intent-to-treat analysis reported in Appendix Section [E.3](#) is relative to effects observed in [E.17](#).

Figure E.12: Changes in Student Demographics

(a) By Covariate

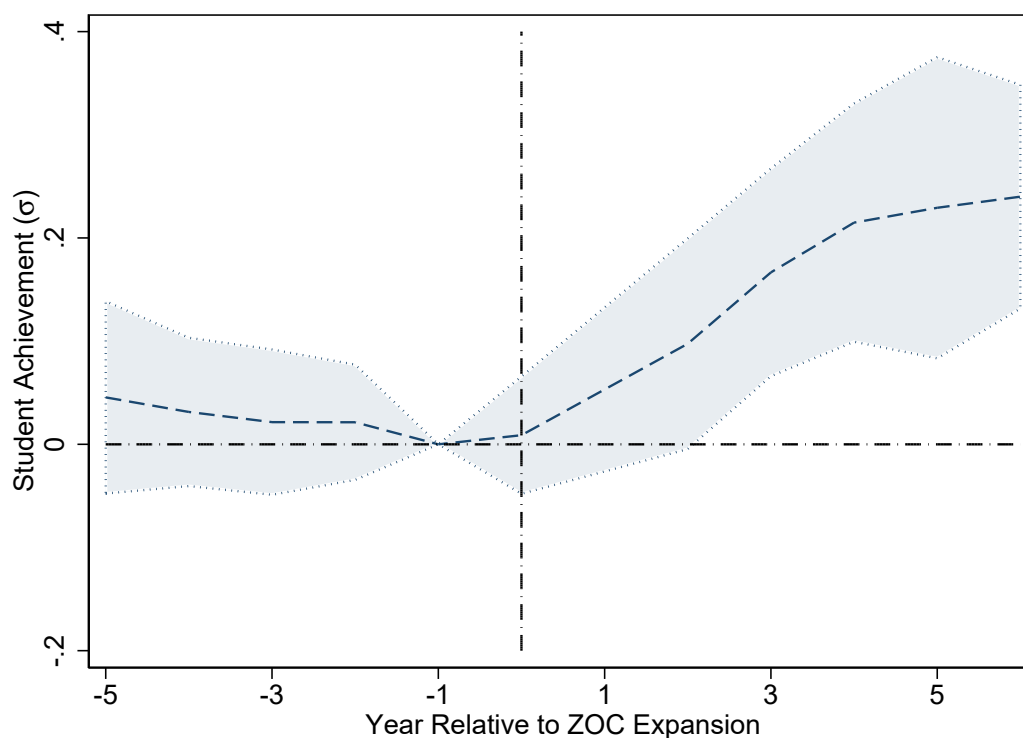


(b) Summary Measure



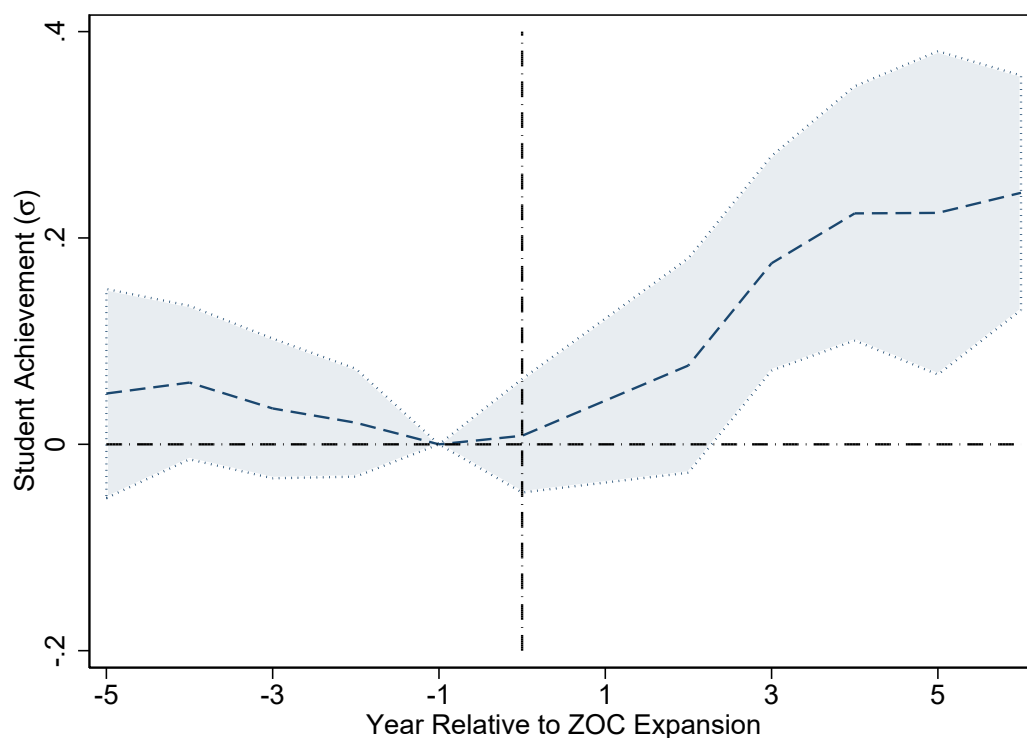
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in the change of student characteristics, labeled on subfigure vertical axes, between ZOC and non-ZOC students relative 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. The summary index is constructed by predictions of achievement using the baseline covariates of interest. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

Figure E.13: Achievement Event Study Restricted to Students Who Did Not Move in the Eighth Grade



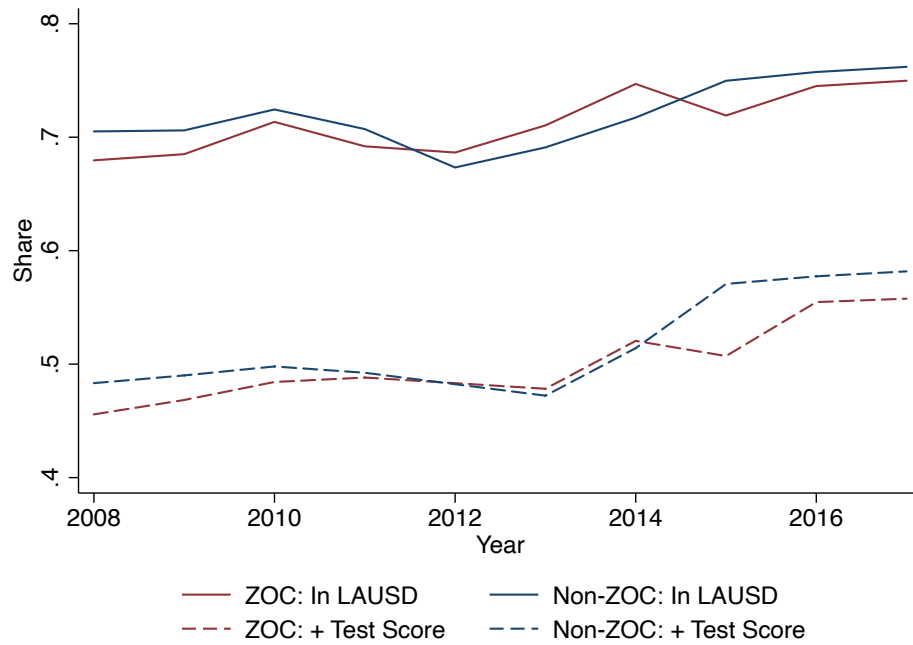
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The sample is restricted to students who did not move in the eighth grade, the year before households submitted ZOC applications. The coefficient β_k shows the 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.14: Achievement Event Study Restricted to Students Who Did Not Move in Middle School

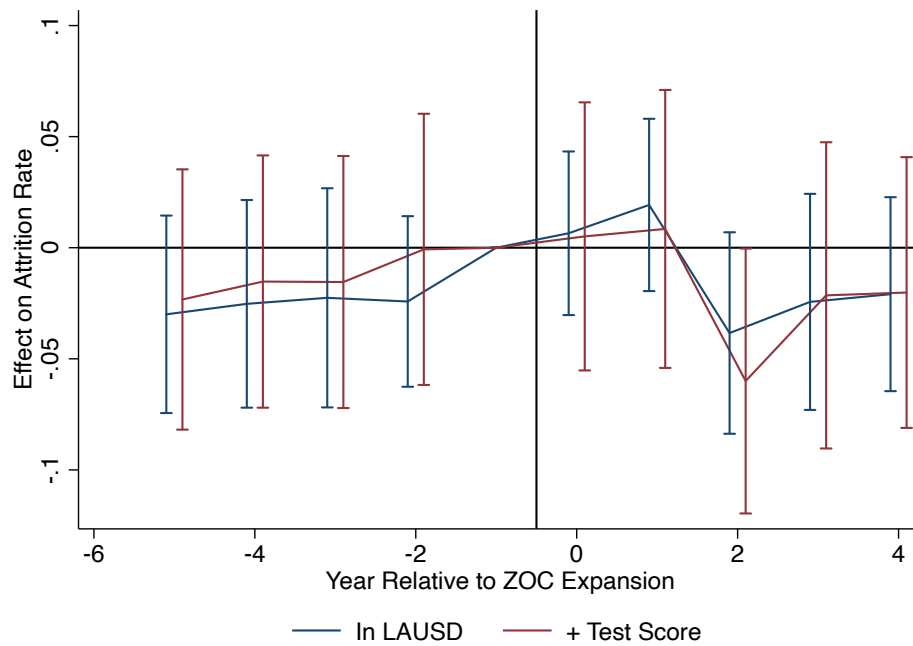


Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The sample is restricted to students who did not move in eighth grade *and* did not move at any time during middle school. The coefficient β_k shows the difference 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.15: Attrition Estimates



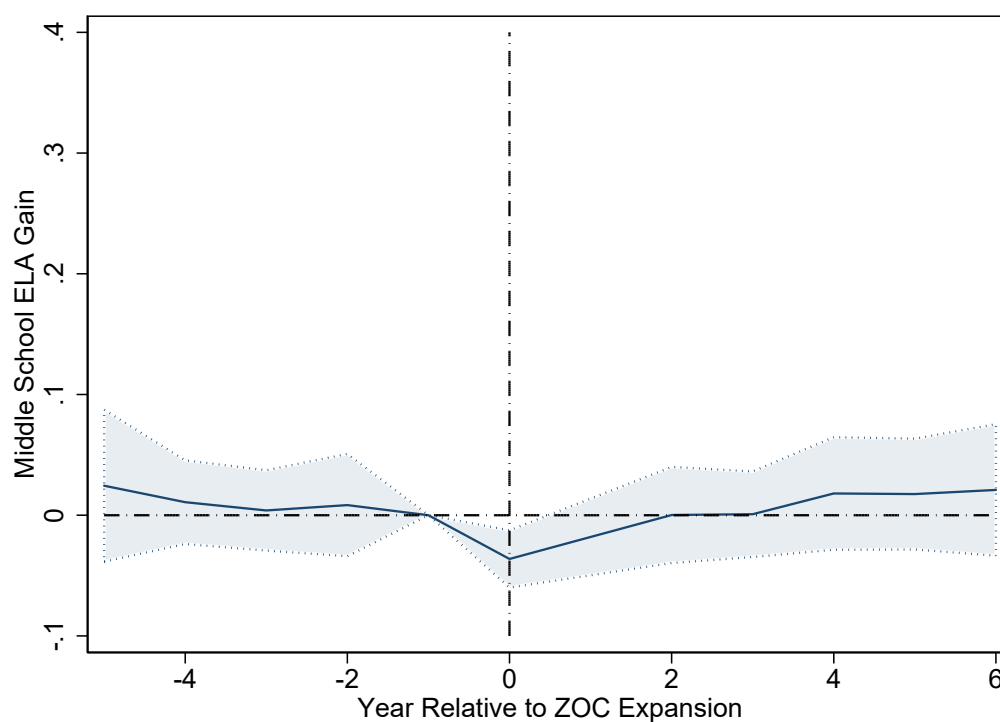
(a) Trends in Attrition Rates



(b) Attrition Event-Study Estimates

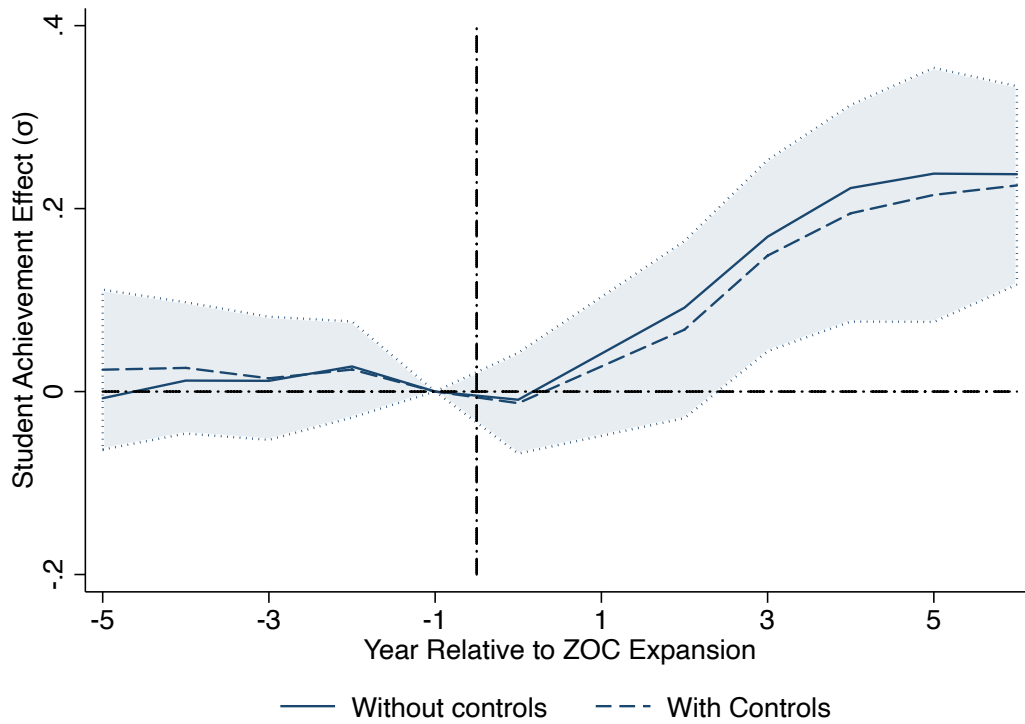
Notes: This set of figures explores nonrandom attrition out of the sample. Panel (a) reports the share of students enrolled in a high school in 9th grade who are present in 11th grade and also the share of students in 11th grade with test scores. Panel (b) reports unadjusted event-study analogs of Panel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

Figure E.16: Falsification Test: ZOC Impact on Middle School Gains



Notes: This figure reports 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 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.

Figure E.17: Event Study Not Restricting Control Group Schools to Comparable Schools

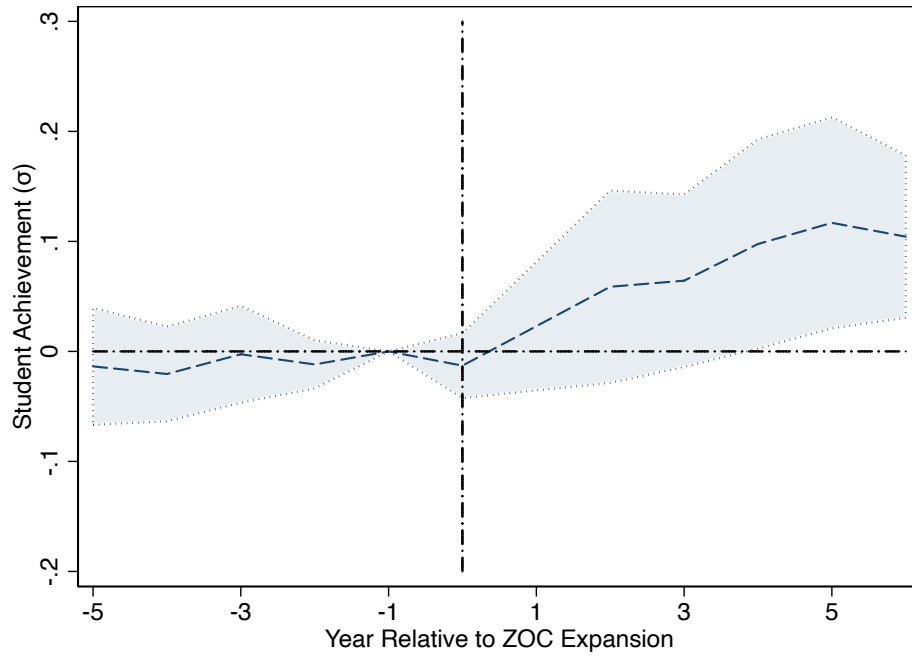


Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates for outcomes relative to the year before the policy. The dashed blue line traces out estimates in models with baseline student covariates, and the solid line corresponds to estimates from models without controls. The sample used in these estimates is unrestricted and includes all magnet programs, continuation schools, and schools that opened after the program expansion. Standard errors are robust and clustered at the school level, and 95 percent confidence regions are reported with shaded areas.

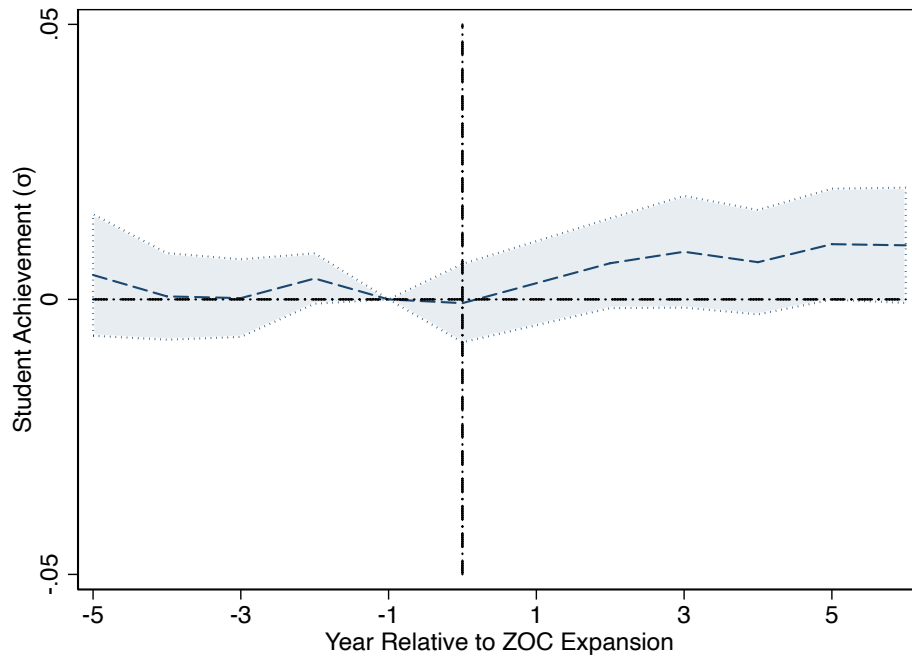
E.5 Decomposition Evidence and Math Estimates

Figure E.18: Decomposition Event Studies

(a) Average Treatment Effect Event Study

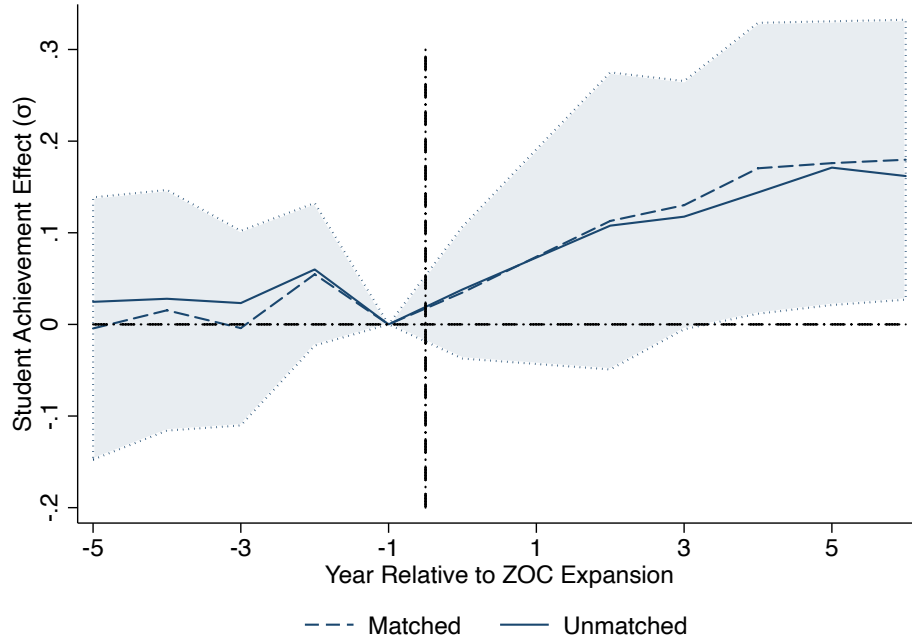


(b) Match Effect Event Study



Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, 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.

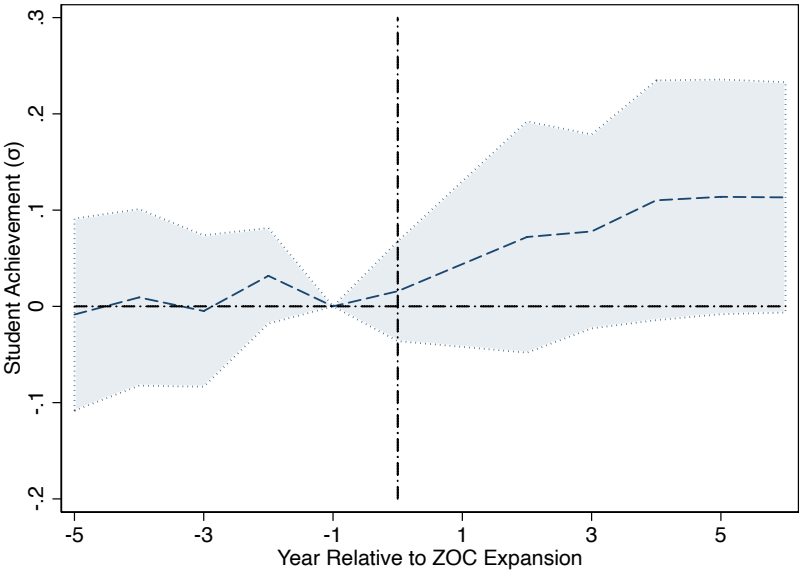
Figure E.19: Math Achievement Event Study



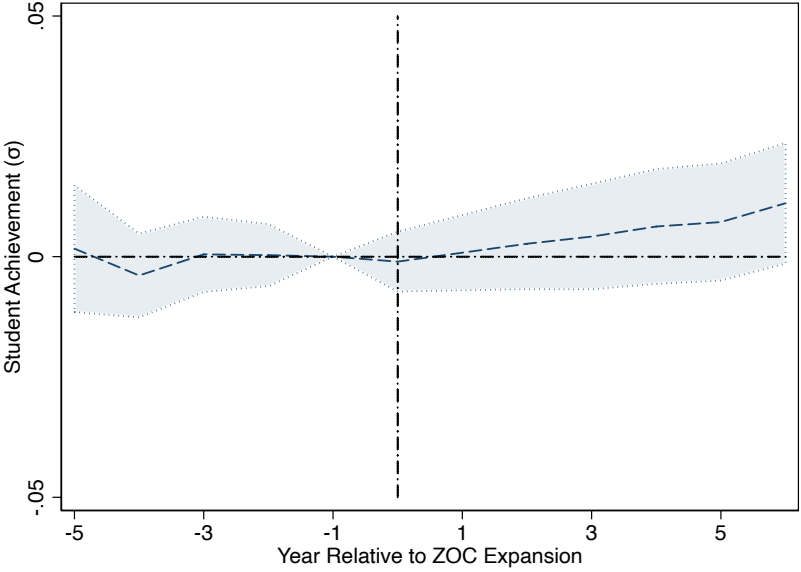
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. The dashed blue line in Panel A traces out 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.20: 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.³⁹ 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

$$L_{R_i} \cdot U_i > L_{R'_i} \cdot U_i \quad \text{for all } R'_i \in \mathcal{R}. \quad (13)$$

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 13 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

$$L_{R_i} \cdot U_i > L_{R'_i} \cdot U_i \quad \text{for all } R'_i \in \mathcal{R}_{R_i}^*, \quad (14)$$

where $\mathcal{R}_{R_i}^*$ is a set that can be obtained from making a one-preference permutation of programs within R_i . Equation 14 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*.⁴⁰

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

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

³⁹We construct bootstrapped rational expectation admissions probabilities following Agarwal and Somaini (2018).

⁴⁰For 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.

We then iterate through the following sequence of conditional posteriors:

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

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, 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.

In Panel A of Appendix Table F.1, we report 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 of Table 2 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_{jet} 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 Model Estimates

G.1 Achievement Model Estimates

Table [G.1](#) 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 [G.2](#) reports the estimates, but we do not find evidence that there were meaningful changes induced by the policy for most characteristics.

Table G.1: Summary Statistics for School-Specific Returns to Student Characteristics and School Effectiveness

	<u>ZOC</u>		<u>Non-ZOC</u>		Difference
	Mean	SD	Mean	SD	
	(1)	(2)	(3)	(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 G.2: Summary Statistics of Time-Varying Match Effects

	Before					Change		
	ZOC		Non-ZOC		Difference	ZOC	Non-ZOC	
	Mean	SD	Mean	SD		Mean	Mean	Diff-in-Diff
	(1)	(2)	(3)	(4)		(6)	(7)	(8)
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.

G.2 Utility Model Estimates

Table G.3: Utility Model Estimates

	Standard Deviations			
	Mean	Total SD	Within	Between
School Mean Utility	-	.655	.211	.62
Distance Costs				
First Cohort	-.121 (.375)			
Second Cohort	-.238 (4.095)			
Third Cohort	-.097 (.184)			
Fourth Cohort	-.189 (1.299)			
Fifth Cohort	-.335 (.989)			
Number of Schools	40			

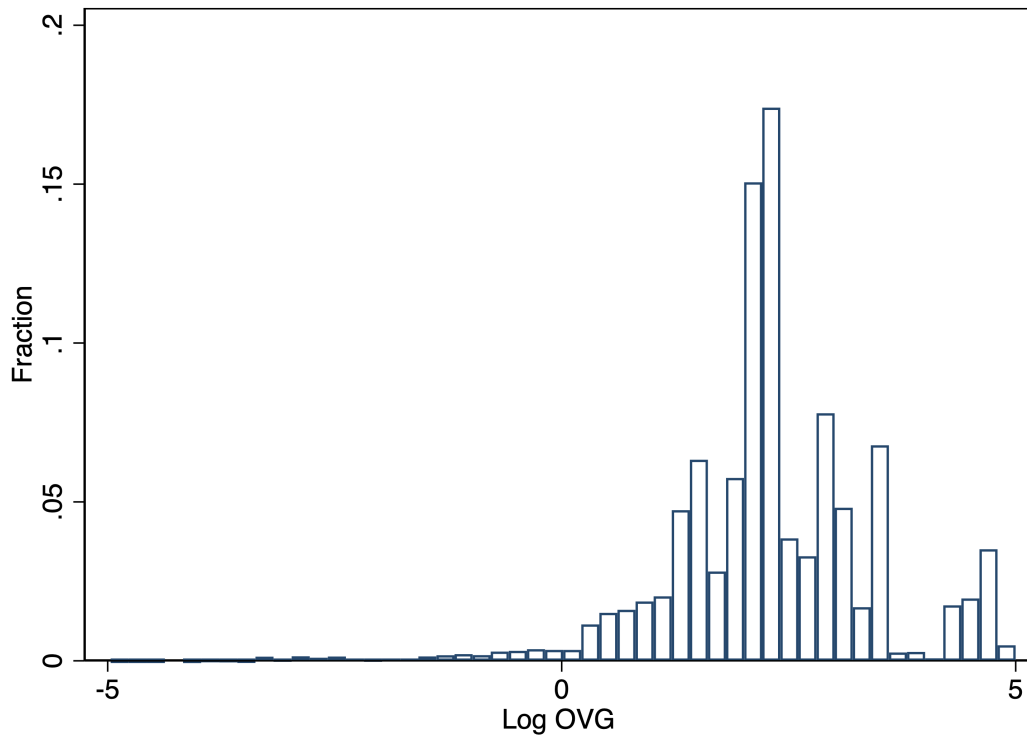
Notes: This table reports standard deviations of estimated school mean utilities and estimated distance costs by cohort. We create school-by-incoming-achievement cells to estimate within standard deviations. Therefore, within standard deviations correspond to variation in mean utility within a covariate cell school group over time. Distance costs are also allowed to vary across cells, and we report means and standard deviations in parentheses.

H Additional Details About Mechanisms

H.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 H.1 displays the distribution of OVG across students, and Appendix Table H.1 reports OVG correlates. . Last, Appenedix Figure H.2 displays the average student OVG quartile in each US census tract, providing a visual description of where most of the high-OVG students are located. Most of the students in the top two quartiles of the student OVG distribution come from three zones: Belmont, North Valley, and South Gate. While the Belmont ZOC offers students the most options, the other two offer a more modest menu of options. South Gate, for example, only provides three campuses to choose from, with one campus being extremely popular and contributing to high OVG. Other students with high OVG come from a mixture of zones, highlighting the importance of accounting for not just school popularity but also distance costs when estimating the value of introducing new options. Important for the empirical analysis is that although many high-OVG students reside in the Belmont, North Valley, and South Gate zones, there are high-OVG students scattered across all zones.

Figure H.1: Log Option Value Gain Distribution



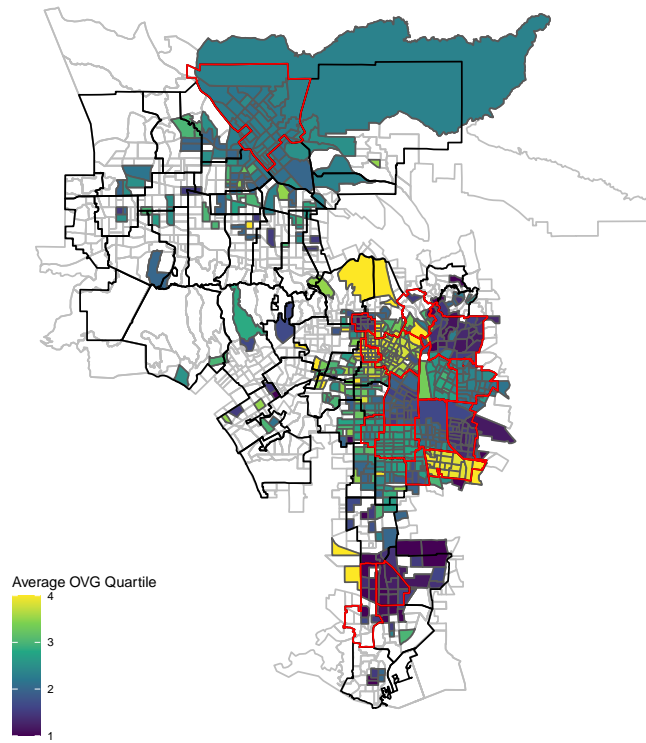
Notes: This figure presents a histogram of estimated log option value gain (OVG) across all students and all years. Preference parameters used in OVG estimation are estimated using only the first cohort's preferences. OVG for later cohorts is constructed using these estimated parameters.

Table H.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 H.2: Census Tract Student-Level OVG Quartiles



Notes: This map displays census tract student-level option value gain (OVG) quartile averages. That is, for each census tract with at least two ZOC students, we calculate the average OVG quartile of students in that census tract and report the resulting average. Gray polygons correspond to census tracts, black polygons correspond to non-ZOC attendance zone boundaries, and red polygons correspond to ZOC attendance zone boundaries. Some census tracts outside of ZOC boundaries contain ZOC students, but these contain less than 1 percent of all ZOC students. The existence of these students in the data is probably due to lags in updating student addresses within the district.

H.2 Changes in Teacher-Student Racial Match

In this section we focus on changes in the classroom-level student-teacher racial match. Our focus on race follows from a growing body of evidence suggesting that exposure to same-race teachers can improve both short- and long-run outcomes of underrepresented racial minorities, who comprise over 90 percent of ZOC students (Dee, 2004, 2005, Fairlie et al., 2014, Gershenson et al., 2018). While these changes only provide suggestive evidence, they do point to changes occurring within schools, including changes we cannot document with our data.

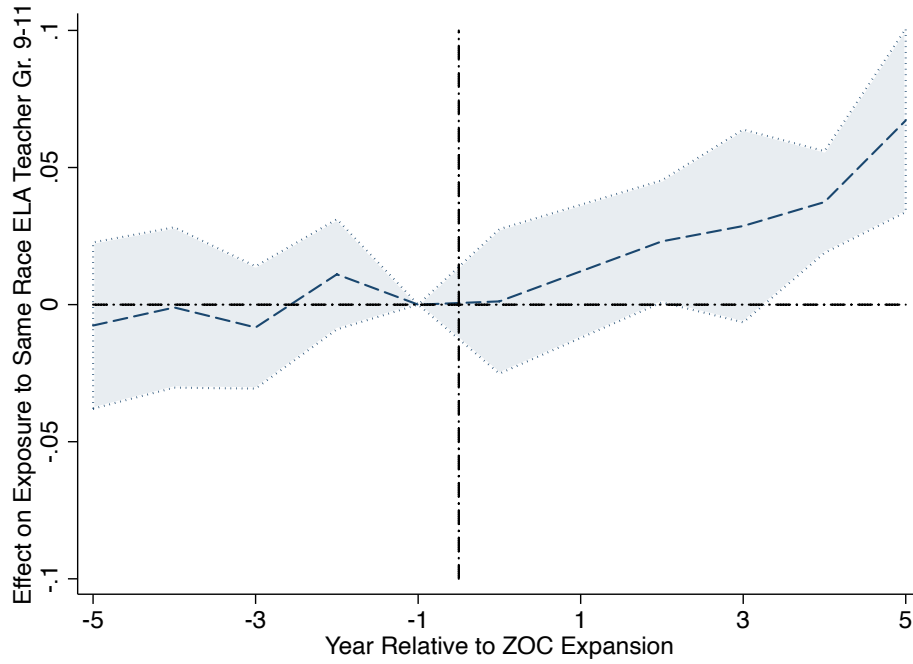
To study same-race exposure, we turn to course-level data matching students to teachers.⁴¹ We track the number of same-race teachers that students are exposed to and study ZOC impacts on racial match propensity. Figure H.3 reports event-study estimates analogous to Equation 2 where the outcome is an indicator equal to 1 if a student is exposed to a same-race teacher in each core ELA course in each year between 9th and 11th grades.⁴² There is no evidence that racial match propensities trended differently before the policy, but we do find ZOC impacts on same-race exposure. The stringent requirement of exposure to a same-race teacher in every year attempts to isolate a systematic change in exposure likelihood. Moreover, the lack of differences in changing hiring practices between ZOC and non-ZOC schools suggests that the increases in racial match are due not to an increased pool of same-race teachers but to a potential within-school change in the way students were assigned to teachers.

Impacts of same-race teachers have been shown to produce both short- and long-run improvements in outcomes for underrepresented racial minorities (Dee, 2004, Fairlie et al., 2014, Gershenson et al., 2018). In particular, Gershenson et al. (2018) find that Black students randomly assigned a Black teacher in the STAR experiment were 4 percentage points (13 percent) more likely to enroll in college. While students in the STAR experiment were elementary school students, the college enrollment effects are comparable in magnitude to ZOC impacts. In general, increased exposure to same-race teachers could impact outcomes through either role model effects or race-specific teaching skills; either could have contributed in part to the ZOC achievement and college enrollment effects. The suggestive evidence of changes in the within-school allocation of students to teachers based on race could, as a consequence, imply changes in tracking practices within schools or vice versa, a point we explore further in the following appendix section.

⁴¹We have course-level data for one less year, so our analysis that depends on these data covers one less year.

⁴²Estimates using the share of same-race ELA teachers students are exposed to result in qualitatively similar estimates albeit are noisier.

Figure H.3: Same-Race Teacher Event Study



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 variable is an indicator equal to 1 if a student is exposed to a same-race teacher in a core ELA course in each year between grades 9 and 11. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions.

H.3 Changes in Tracking Practices

In this section, we study potential changes in tracking practices. To begin, we categorize students into six groups based on their incoming achievement and estimate student-level achievement-based segregation indices defined in Echenique et al. (2006). The advantage of the student-level achievement segregation index (ASI) is that it captures both how much a student is segregated based on the peers they share classes with and the influence of how segregated their peers are. For example, two high-achieving students in the same school could be tracked into two similar honors courses, each with a different pool of classmates. Suppose both pools of classmates are also high achieving but differ in the composition of students they share other classes with. Differences in a student’s classmates’ classmate exposure would generate differences in achievement-based segregation for two otherwise similar students both enrolled in highly segregated courses. Therefore, changes in ASI could result from changing tracking practices at the extensive margin—the presence of highly segregated classrooms—but also at the intensive margin—depending on how isolated certain groups are.

To isolate achievement-based tracking, we focus on ninth-grade course enrollments, a time period where principals have less information about students and test scores probably receive more weight in course assignment. For each cohort of students within a school, we categorize them into six groups based on their standardized test scores in eighth grade and estimate their ASI using the procedure outlined in Echenique et al. (2006). Figure H.4 reports ZOC and non-ZOC ASI averages at multiple incoming achievement cells. Even though there are level differences in ASI between ZOC and non-ZOC students, both share a common feature that students at the tails of the achievement distribution have a higher average ASI. This observation is indicative of tracking practices existing in both ZOC and non-ZOC schools, with tracking practices being more pronounced for high-achieving students.

To assess how tracking practices changed between ZOC and non-ZOC schools, we estimate

$$\begin{aligned}\widehat{ASI}_{it} = & \mu_{j(i)t} + \beta'_A Post_t \times ZOC_{j(i)} \times f(A_{it}^8) \\ & + \beta'_B Pre_t \times ZOC_{j(i)} \times f(A_{it}^8) \\ & + \gamma'_1 Post_t \times f(A_{it}^8) + \gamma'_2 ZOC_{j(i)} \times f(A_{it}^8) + f(A_{it}^8) + u_{it},\end{aligned}$$

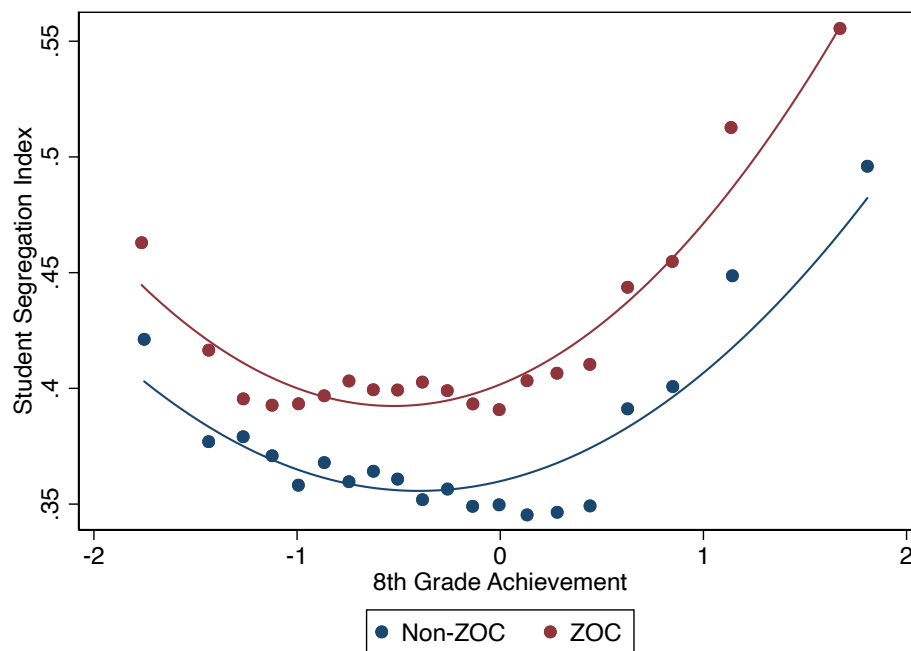
where $f(A_{it}^8)$ is a polynomial in students’ incoming achievement and μ_{jt} are school-by-year effects indicating this model is identified from changes in the within-school-cohort segregation gap between students with incoming achievement A_{it} and those with $A_{it} = 0$. Therefore, $\beta'_A \times f(A_{it}^8)$ captures the causal impact of ZOC on the within-school segregation gap between students with incoming achievement A_{it}^8 and those with incoming achievement at the average $A_{it} = 0$, and β'_B captures any differential changes in the pre-period amounting to a check on differential pre-trends in within-school segregation gaps.

Figure H.5 reports the estimates at multiple points of incoming achievement. Differential changes in the pre-period are not present in the estimates, providing support for the parallel trends assumptions. For the first few post-periods, we also do not detect any differential changes in within-school segregation gaps, but we do observe them in the later post-periods. In particular, we find that segregation gaps decreased for both high- and low-achieving stu-

dents, suggesting ninth-grade classrooms became more integrated in terms of students' incoming achievement.

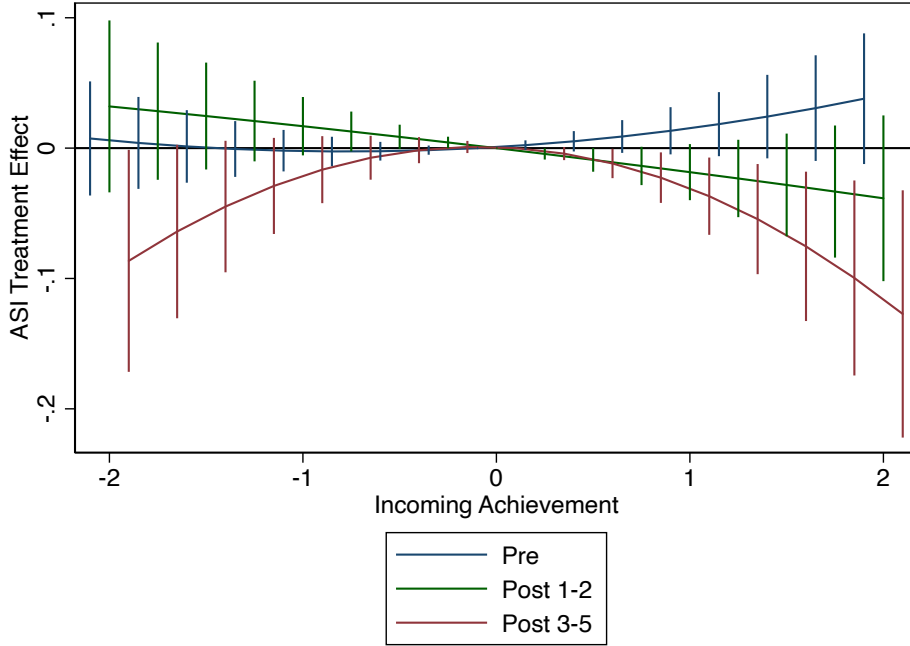
The literature is mixed in terms of the effects of tracking on student achievement and achievement inequality (Betts, 2011, Bui et al., 2014, Card and Giuliano, 2016, Cohodes, 2020, Duflo et al., 2011). The findings suggest that both lower- and higher-achieving students were placed in classrooms with more diverse students. The effects of these changes depend on the education production function, teacher incentives, and the distribution of student achievement (Duflo et al., 2011). Thus, there are conditions in which the changes in ASI could lead to positive effects on achievement.

Figure H.4: Estimated ASI Averages by Incoming Achievement



Notes: This figure reports binscatter plots of estimated ASI averages separately for ZOC and non-ZOC students. A quadratic polynomial line is fit to the data points.

Figure H.5: ASI Treatment Effects by Incoming Achievement



Notes: This figure reports school-level difference-in-differences estimates from regressions defined above. The blue line aggregates all pre-period effects, the green line aggregates post 1-2 effects, and the red lines aggregates post 3-5 effects. 95 percent confidence intervals are reported by vertical lines. Standard errors are robust and clustered at the school level.

H.4 Changes in School Inputs

To assess the role that changes in school inputs explain the treatment effects, we compare changes in inputs between ZOC and non-ZOC schools before and after the policy expansion. We focus on the arguably most important input: teachers. Three points summarize our findings related to teachers.

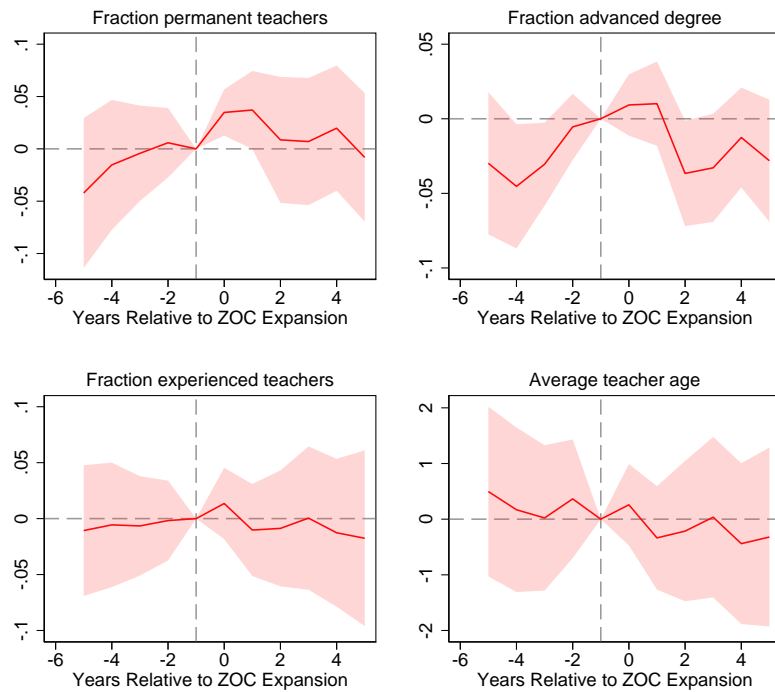
First, we do not find any differential changes in the qualifications of ZOC teachers as captured by experience, age, status, and advanced degree attainment. Second, ZOC schools did not differentially hire additional teachers, ruling out class size changes. Third, we do not find evidence that changes in LAUSD-incumbent teacher quality explain the effects.⁴³ While we do not find that ZOC schools differentially attracted higher-quality teachers, among the pool of existing teachers before the policy, our evidence does not rule out that ZOC principals may have attracted higher out-of-district teachers, although this seems unlikely. Overall, the evidence demonstrates that school inputs did not differentially change between ZOC and non-ZOC schools, suggesting that changes in inputs do not explain improvements in school quality.

We conclude with Appendix Figure H.8, which reports treatment effects on school experience survey items. These measures indirectly capture potential changes occurring within schools, including but not limited to just inputs. Overall, ZOC students experienced differential im-

⁴³Teacher quality (value added) is estimated for all teachers in the years before the policy expansion. Therefore, our analysis focuses on changes in teacher quality exclude hires that happened after the policy expansion.

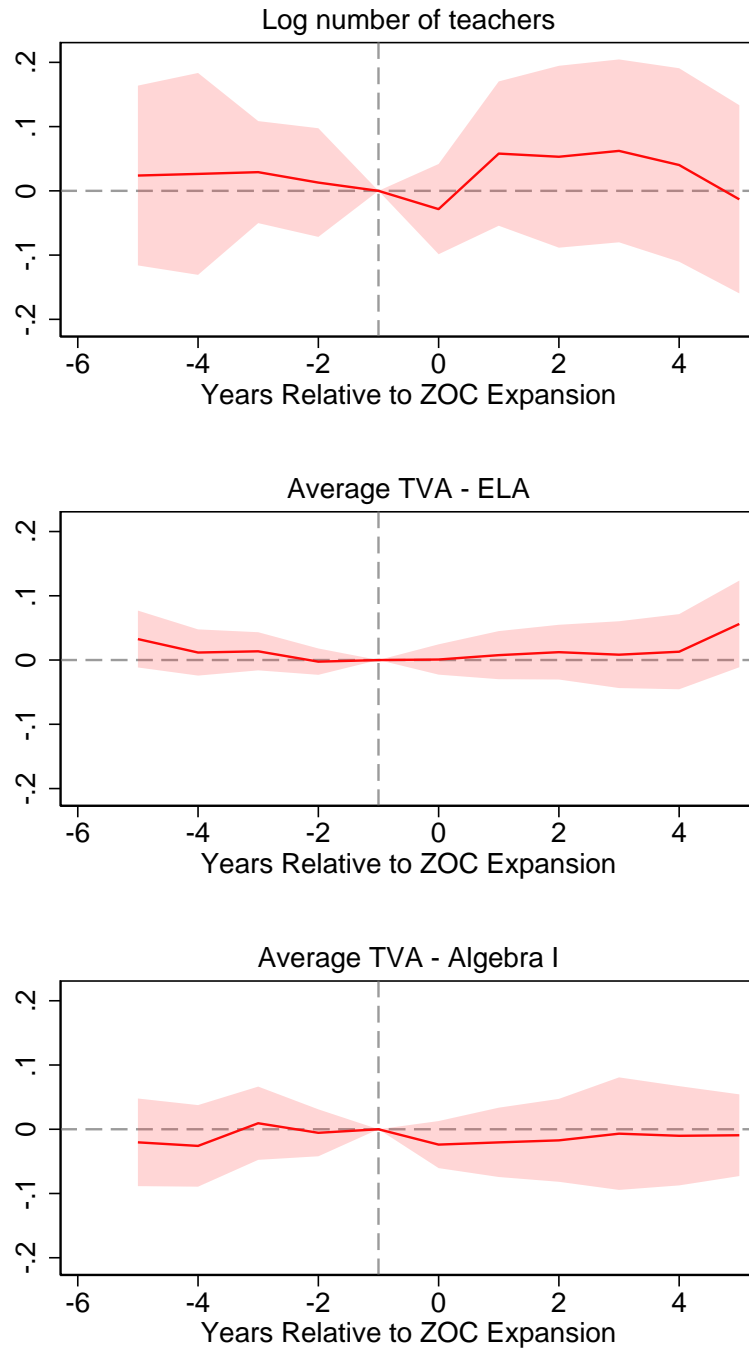
provements in their teachers' effort and also experienced differential improvements in overall satisfaction.

Figure H.6: Teacher Characteristic Event Studies



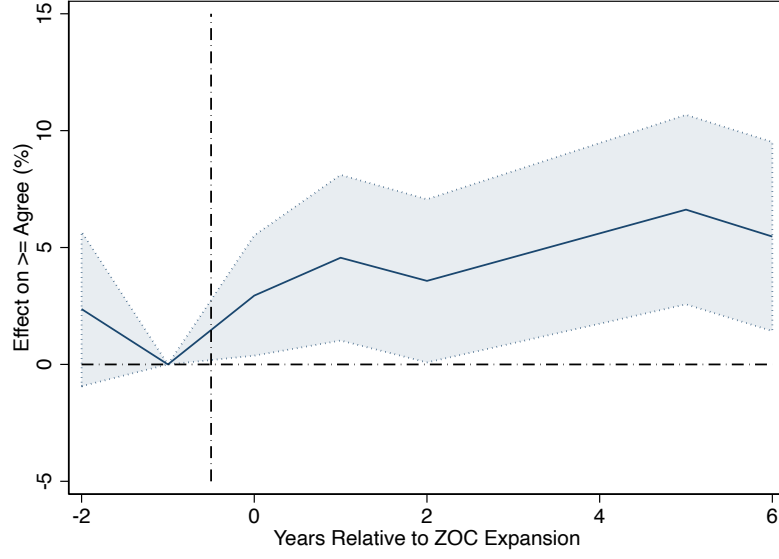
Notes: This figure reports school-level event-study estimates from regressions of an outcome on school fixed effects, year fixed effects, and event-time indicators interacted with ZOC dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the school level.

Figure H.7: Teacher Quantity and Teacher Quality Event Studies

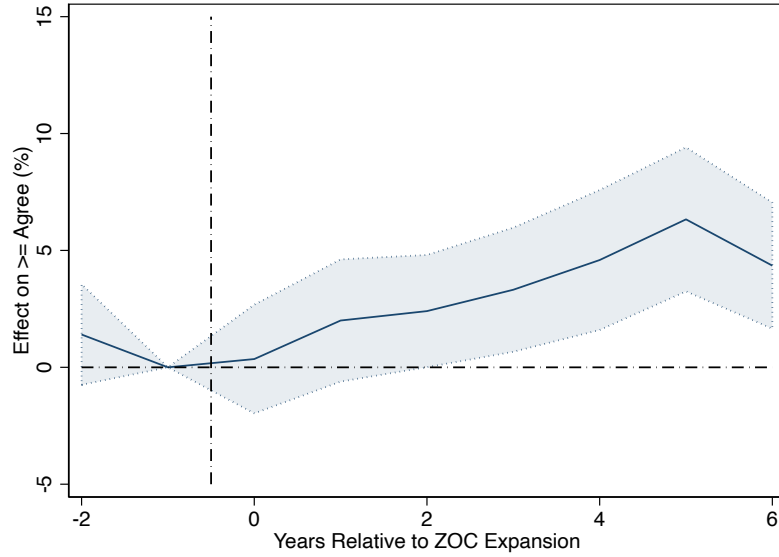


Notes: This figure reports school-level event-study estimates from regressions of an outcome on school fixed effects, year fixed effects, and event-time indicators interacted with ZOC dummies. For outcomes corresponding to teacher value added, we estimate teacher value added in the pre-period, and thus averages only contain teachers in the sample before the policy. Standard errors are clustered at the school level.

Figure H.8: 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 2 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 outcome 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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