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The Impact of Neighborhood School Choice: Evidence from Los Angeles's Zones of Choice Program

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The Impact of Neighborhood School Choice: Evidence from Los Angeles's Zones of Choice Program*

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Abstract

This paper evaluates the Zones of Choice (ZOC) program in Los Angeles, a 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. We study the impacts of ZOC on student achievement and college enrollment using a matched difference-in-differences design that compares changes in outcomes for ZOC schools and demographically similar non-ZOC schools. Our findings reveal that ZOC has boosted student outcomes markedly, closing 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 changes in student match quality, and the school-effectiveness gains are concentrated among the lowest-performing schools. We interpret these findings through the lens of a model of school demand in which schools exert costly effort to improve quality. The model allows us to measure the increase in competition facing each ZOC school based on household preferences and the spatial distribution of schools. We demonstrate 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 the potential of public school choice to alter an important component of neighborhood quality, reduce neighborhood-based disparities in educational opportunity, and produce sustained improvements in student outcomes.

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Students in the United States have traditionally been assigned to schools by attendance-zone boundaries. Critics of this local-monopoly model argue that it provides weak incentives for schools to improve quality and might not operate in students' best interests. These criticisms have paved the way for a growing number of reforms designed to expand school choice. A surge of reforms promises to increase access to high-performing schools, increase the potential for student-school match-quality improvements, and while doing so, introduce competitive pressure that could compel ineffective schools to improve (Chubb and Moe, 1990, Friedman, 1955, Hoxby, 2003). However, empirical studies of school choice experiments have generated mixed results regarding the effects and efficacy of school choice (Abdulkadiroğlu et al., 2018, Lavy, 2010, Muralidharan and Sundararaman, 2015, Neilson, 2013, Rouse, 1998). Therefore, whether expanding school choice can produce sustained improvements in student outcomes and reduce achievement gaps remains an open question.

The fact that school choice reforms simultaneously change students' access to schools, potentially improve match quality, and increase competition, gives researchers the degrees of freedom to study these various aspects usually in isolation. One body of research concerns the impacts of access to specific types of schools, such as charter schools and exam schools, on student outcomes (Abdulkadiroğlu et al., 2011, Angrist et al., 2002, Cullen et al., 2006, Deming et al., 2014, Hoxby et al., 2009, Krueger and Zhu, 2004, Rouse, 1998, Tuttle et al., 2012). While these studies often feature compelling research designs and are useful in identifying effective schools and their best practices (Angrist et al., 2013), they typically ignore questions about competition and the equilibrium effects of school choice. Another body of research—spanning multiple countries—focuses on competition and finds mixed effects (Allende, 2019a, Bau, 2019, Card et al., 2010, Figlio and Hart, 2014, Figlio et al., 2020, Gilraine et al., 2019, Hsieh and Urquiola, 2006, Muralidharan and Sundararaman, 2015, Neilson, 2013). Student-school match effects have received less attention—Bau (2019) is a notable exeption—but remain an important channel through which school choice could enhance allocative efficiency (Hoxby, 2003). Few studies jointly consider all these factors in a single setting, and rarely do they focus on increasingly popular intradistrict reforms.

This paper fills this gap by studying Zones of Choice (ZOC), an initiative of the Los Angeles Unified School District (LAUSD) that created small local markets with high schools of varying size in some neighborhoods but leaves traditional attendance-zone boundaries in place throughout the rest of the district. More specifically, the initiative established sixteen zones, primarily in relatively disadvantaged parts of LAUSD. These zones cover roughly 30–40 percent of all high school students in LAUSD; the remaining LAUSD students remain subject to traditional neighborhood school assignments. ZOC students are eligible to attend any school within their zone, even if it is not the closest one, and a centralized (immediate acceptance) mechanism is used to ration access to oversubscribed schools. We conduct a comprehensive analysis of supply- and demand-side responses to ZOC to determine how these changes in market structure have altered the distribution of school quality and affected student outcomes.

Our empirical analysis is guided by 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 model ZOC as an expansion of households' choice set. 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 determines schools' incentives to increase quality. The theoretical framework predicts that the introduction of ZOC will improve school quality and that the improvement will be concentrated among schools exposed to more competition as measured by OVG.

We empirically test these predictions using a matched difference-in-differences design that compares changes in outcomes for ZOC schools with corresponding changes for an observationally similar set of control schools elsewhere in the district. To estimate the impact of ZOC on school quality, we decompose treatment effects into effects on student-school match quality and effects on schools' value added. Estimates of quantile treatment effects on schools' value added allow us to assess whether the lowest-performing schools improve more as predicted by the model. We then use students' rank-ordered-preference lists to 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.

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. 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.

A distributional analysis shows that student improvements appear throughout the middle and lower parts of the student-achievement distribution, with smaller effects on the highest-achieving students, while college-enrollment effects appear for students with both low- and high-baseline four-year-college-enrollment probabilities. We show that improvements in school quality are concentrated among the lowest-performing schools, a finding consistent with the theoretical framework. Moreover, we find that the effects of the program are larger for schools and students with higher OVGs. This suggests that the competition-induced incentives generated by ZOC are a key mechanism for its effects on school performance.

Estimates of demand derived from rank-ordered-preference lists are consistent with the findings. 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 contrasts with other studies' findings (for example, 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).

Another possible source of gains concerns students' enrollment in popular, higher-quality schools not available to them before the program. The effects of this mechanism contrast with the market-level effects discussed above, which capture improvements among schools in general. We rely on randomized admissions lotteries to estimate the causal impact of enrolling in a most preferred school, a research design common for evaluating school choice policies (Abdulkadiroğlu et al., 2011, Cullen et al., 2006, Deming et al., 2014, Rouse, 1998). The market-level effects help explain why we find modest impacts of attending a more preferred school. We show that the impacts of accessing popular schools shrink as differences between most preferred and fallback schools narrow over time in response to the improvements of ZOC schools captured by the market-level impacts. Importantly, this analysis demonstrates that the most significant benefits of the program arise from improvements of all ZOC schools and not from the re-allocation of students. These findings underscore the importance of market-level effects when evaluating school choice programs and also emphasize that school quality his highly malleable.

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 within a zone. 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 (Corcoran et al., 2018). These features combine to create a setting in which acquiring adequate information about schools is more likely. We also highlight that the centralized assignment mechanism ZOC employs does not allow for additional school-specific priorities that incentivize screening strategies, reducing the benefits of investing in recruiting efforts to sustain demand. 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.

Related Literature

This paper contributes to several strands of research. Most importantly, it contributes to the literature estimating market-level effects of school choice. One strand of literature relies on cross-district or cross-municipality comparisons to estimate market-level effects (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 (Altonji et al., 2005, Dee, 1998, Neal, 1997). Although intradistrict school choice initiatives are growing (Neilson, 2021, Pathak, 2011), few studies have focused on intradistrict market-level effects. We fill this gap by focusing on an intradistrict natural experiment in Los Angeles that allows us to study market-level effects and competition among public schools. We provide striking evidence that public school choice reforms can induce competitive effects even in settings in which competition-induced incentives are more ambiguous compared to private to public competition. 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.

Prior literature grappled in various ways with the difficulty of measuring competition. For example, Figlio and Hart (2014) study competitive effects when proximity-based exposure to competition varies, Gilraine et al. (2019) consider how competitive effects vary between the entry of horizontally differentiated schools and that of non-horizontally differentiated schools, and Card et al. (2010) considers the salience of demand-side pressures captured by the composition of students. We leverage market structure heterogeneity and baseline preferences to construct competition indices. This policy-specific variation allows us to further test the competitive-effects interpretation of our results, but more generally, it demonstrates that information contained in rank-ordered-preference lists can also be useful to measure competitive pressures schools face in other settings with similar institutional features.

We also contribute to an extensive literature using lotteries—sometimes mandated in oversubscribed schools (Chabrier et al., 2016) and other times embedded in centralized assignment mechanisms (Abdulkadiroğlu et al., 2017)—to evaluate various school choice reforms. Lotteries have been an effective tool for estimating causal impacts of attending voucher schools (Abdulkadiroğlu et al., 2018, Angrist et al., 2002, Howell et al., 2002, Krueger and Zhu, 2004, Rouse, 1998), attending charter schools (Abdulkadiroğlu et al., 2011, Angrist et al., 2016, Hoxby et al., 2009, Tuttle et al., 2012), or exercising choice in district-wide open-enrollment programs (Cullen et al., 2006, Deming et al., 2014). We contribute to this literature by including a lottery study in the empirical analysis; we find that most of the program's benefits are due to market-level effects and not within-zone re-allocation of students across schools. Our findings provide a reason why other evaluations of intradistrict school choice policies (Cullen et al., 2006, Hastings et al., 2005) have found limited achievement effects: intradistrict school choice policies generate market-level effects that may attenuate achievement gains from attending oversubscribed schools.

Last, this paper demonstrates that an important neighborhood attribute—school quality—is malleable. The paper 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. 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 that school- or neighborhood-specific policies are a means of reducing neighborhood-based disparities in outcomes (Fryer Jr and Katz, 2013).

The rest of this paper is organized as follows. In Section 1 we outline the features of the policy and our data sources; Section 2 outlines the conceptual framework for the subsequent analysis; Section 3 discusses the data; Section 4 presents the market-level analysis; Section 5 estimates demand and OVG; Section 6 presents lottery estimates; Section 7 presents evidence on changes within schools and discusses ZOC institutional features that may have contributed to our results; and Section 8 concludes.

1 Institutional Details

1.1 A Brief History of Zones of Choice

ZOC is an initiative of LAUSD, the second-largest school district in the United States. As has been common in several large urban school districts around the country, LAUSD was and continues to experience enrollment decline, potentially amplified by charter growth (see Appendix Figures L.1 and L.2). As a consequence, LAUSD experimented with various policies to partly address this and other district-specific issues. These policies included the largest school construction program in US history (Lafortune et al., 2018), the expansion of pilot schools, the use of conversion charter schools, the development of pilot-like schooling models (Kearns et al., 2020), and the creation of a novel school choice zone known at the time as the Belmont Zone of Choice in 2007.

ZOC began with 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. 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 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 sixteen zones. Figure 1 shows that 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. The new zones 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:

1. **Access.** "Develop a plan that would consider removing boundaries for schools in order 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."

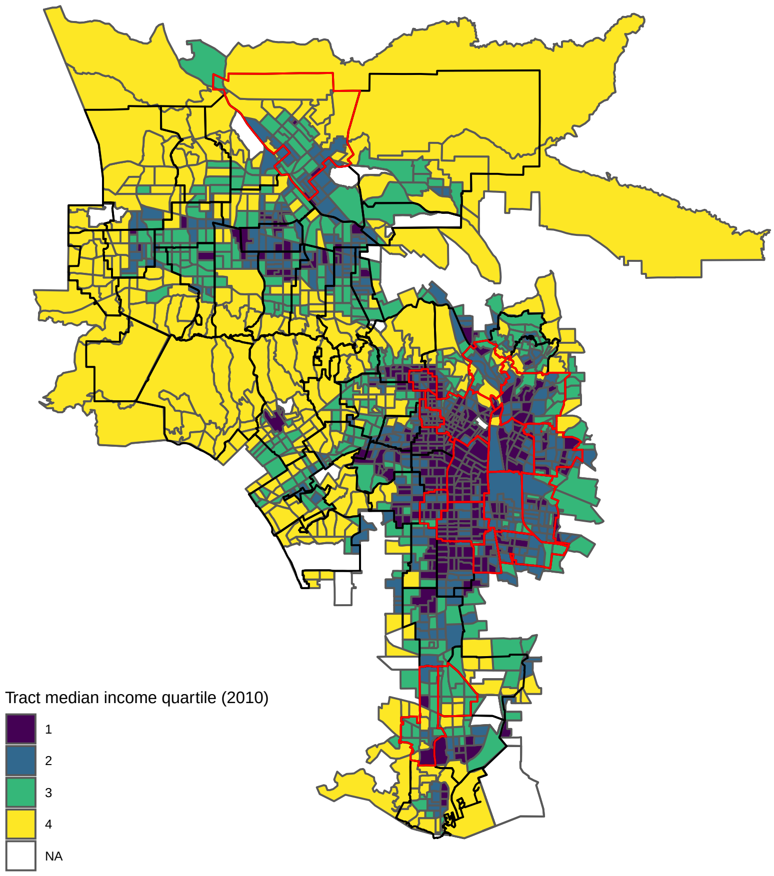
3. **Parental involvement.** "Research validates that parental involvement in public schools is a key factor in producing measurable gains in student academic success, closing the achievement gap."

1.2 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 included 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

Figure 1: Zones of Choice 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.

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 makes it clear to high schools 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.

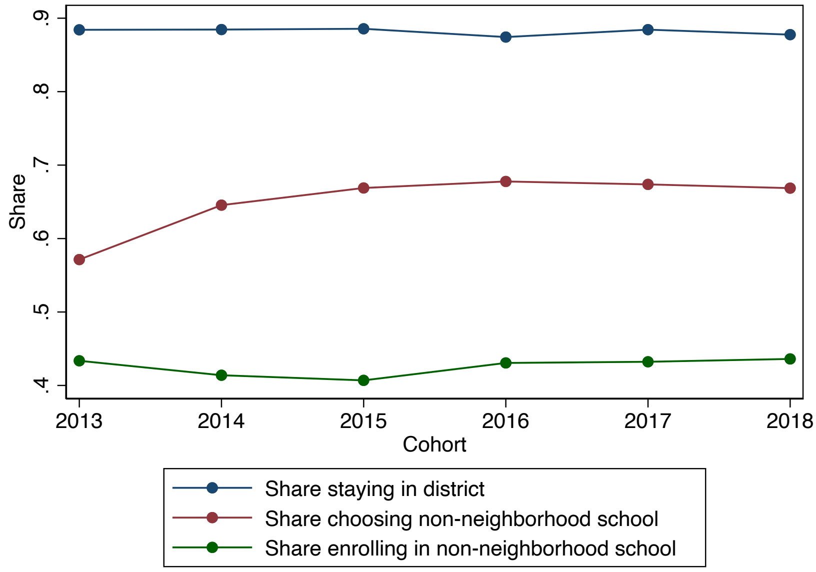
Strategic incentives notwithstanding, many parents list non-neighborhood schools as their most preferred options. Figure 2 shows that roughly 65–70 percent of applicants list a school that is not their neighborhood school as their most preferred option. Priorities and capacity constraints preclude all applicants from enrolling in their most preferred school, so approximately 40–45 percent of applicants enroll in a school that is not their neighborhood school. Importantly, although capacity constraints are binding at some schools within each zone, the concurrent district-wide enrollment decline provide 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

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.

a stylized model of school choice and competition. The model implications guide most of the empirical exercises throughout the rest of the paper.

2 Conceptual Framework

We begin with a stylized model for the status quo which 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.[1] We use j to denote both schools and neighborhoods, indicating there is one school per neighborhood. Let students indexed by i reside in neighborhood $j(i) \in \{1, \cdots, J\}$, 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.[2]

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

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

where α_j is school quality as defined in the achievement model in Section 4.2.1, d_{ij} is distance

[1] We assume residential location decisions are made in a pre-period and are not a first-order concern for this initial ZOC cohort.

[2] One reason for starting with one-sided neighborhood-monopoly competition is that it generates a pre-ZOC equilibrium with heterogeneous quality.

to school j, $\boldsymbol{X}_i$ captures preference heterogeneity with respect to student characteristics, and ε_{ij} captures any remaining unobserved preference heterogeneity that we assume is additively separable.

We can further decompose V_{ij} into a school j mean utility component $\delta(\alpha_j, \boldsymbol{X}_i)$ and another component capturing linear distance costs λd_{ij}:[3]

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

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

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

Last, we normalize outside option utility to zero. With a logit error structure for the unobserved preference heterogeneity, school market shares are[4]

$$\begin{aligned} S_j(\alpha_j; \boldsymbol{X}, \boldsymbol{d}) &= \frac{1}{N_j} \sum_{i \in j(i)} P_{ij} \\ &= \frac{1}{N_j} \sum_{i \in j(i)} \frac{e^{V_{ij}}}{1 + e^{V_{ij}}} \end{aligned}$$

and the outside-option market share is

$$S_0 = \frac{\sum_j N_j(1 - S_j)}{\sum_j N_j}.$$

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).[5] Principals' utility is determined by

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

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.[6] Last, we assume that school quality is an

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

[4]We assume that because ZOC schools start as local neighborhood monopolies, they have a majority of the neighborhood's market share. This assumption gives rise to several of the model's implications. We make the assumption because of the assumptions about unobserved preference heterogeneity, but it may not be necessary in models without that error structure.

[5]In addition to the incentives discussed above, neighborhood-specific market shares can be viewed as a direct revelation of a principal's productivity, and given expanding charter-sector growth in Los Angeles during the period, they are a first-order concern for both principals and district administrators. Alternatively, see 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.

[6]The introduction of ZOC introduces a principal/school effort game, so all market shares are implicitly best-response functions. Details are discussed in Appendix B.

increasing concave function of the level of effort e_j,

$$\alpha_j = f(e_j).$$

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

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

Differences in student characteristics and relative distances across neighborhoods to the outside option generate a pre-ZOC heterogeneous vector of equilibrium effort levels,

$$\boldsymbol{e}_0 = (e_{10}, \cdots, e_{J0}),$$

with a corresponding pre-ZOC vector of equilibrium school effectiveness,

$$\begin{aligned}\boldsymbol{\alpha}_0 &= (f(e_{10}), \cdots, f(e_{J0})) \\ &= (\alpha_{10}, \cdots, \alpha_{J0}).\end{aligned}$$

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

We define a student's option value gain (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 option value gain defined as*

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

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

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

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 households 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 households 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 student i's 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, $OVG_{i0} = \frac{1}{\lambda}\Big(\ln(1 + e^{V_{ij(i)}}) - V_{ij(i)}\Big)$ 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 that 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:

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

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—implied by standard fixed-point theorems—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:*

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

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

Proof. See Appendix B.1. ∎

2.1 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 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 provide additional information about the source of the gains.[8]

The next implication is that the between-school quality gap within a zone decreases, indicating a compression in the school-effectiveness distribution.

Implication 2. *For any two schools $i, j \in \mathcal{J}$ such that $\alpha_i > \alpha_j$, the change in the quality gap $\Delta\alpha_{i,j}$ is decreasing:*

$$\Delta\alpha_{i,j} = (f(e_j^{BR}) - f(e_i^{BR})) - (f(e_{j0}) - f(e_{i0})) < 0$$

To test this empirically, we estimate distributional and unconditional quantile treatment effects on school effectiveness. Evidence that most of the improvements come from lower-performing schools is consistent with this model implication.

Implication 3 incorporates OVG into the empirical analysis. In particular, it tests for the presence of competitive effects.

Implication 3. *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.

[7] The implications rely on two additional assumptions: First, each affected school initially serving 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.

[8] We abstract from modeling preferences for peers, as it introduces complications to the model. Allende (2019b) estimates a structural model accounting for preferences for peers.

For example and through the lens of the model among two identical schools, the one exposed to 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.[9]

3 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 2002 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.

3.1 Analysis Samples

The market-level sample covers 2008–19. We begin by restricting to student-level observations in eleventh-grade, the grade-year with continuous testing throughout the sample period. Besides the grade restriction, we impose no other student-level restrictions in the sample selection.[10]

We impose additional restrictions at the school level. For non-ZOC schools, we exclude continuation, special education, or magnet schools without strict neighborhood assignment boundaries.[11] For ZOC schools, we first restrict to schools that are open before the ZOC expansion. In some ZOC settings, large high school complexes house multiple programs and schools. For the purposes of the evaluation, we consider a program a school if there is a distinct principal running the school.[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. The set of ZOC schools used in the analysis are reported in Appendix Table A.1.

[9] One 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}. In particular, if the preference heterogeneity is large, so $\sigma_\varepsilon^2 \to \infty$, then $OVG_i \approx OVG = \frac{\ln|\mathcal{J}_z|}{\lambda}$ for all i. So 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 $V_{ij} = \frac{\delta_j - \lambda d_{ij}}{\sigma} \to 0$ as $\sigma^2 \to \infty$, 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.

[10] A potential concern with selecting only eleventh-grade observations is differential attrition rates out of the sample that could introduce bias in our analysis. In Appendix Figure F.6 we report attrition rates over time for ZOC and non-ZOC cohorts. We do not find evidence of differential attrition rates between ZOC and non-ZOC students.

[11] We consider samples that allow for the inclusion of magnet programs and results look qualitatively similar.

[12] Some 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 are not able to associate the program with a continuous school or program, we drop them from the sample.

ZOC students are observably different from non-ZOC students. 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 D. We refer to this as the matched sample.

3.2 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. 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 that were in eleventh-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 (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 scaling of the score distribution. While we do not have item-level data to check if this is a concern, we complement our analysis with an outcome that is immune from this change: four-year college enrollment.[13] 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 were identical for all eleventh-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.

3.3 Descriptive Statistics

Column 1 and Column 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. 85 percent are classified as poor by the district and only 3 percent of students have parents who graduated from college, 50 percent less than non-ZOC students.

We report matched non-ZOC mean characteristics in Column 4 of Table 1. The limited pool of schools we can draw from due to restrictions imposed above limits our capacity to eliminate

[13] In Appendix A.1 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.

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 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.

Table 1: Descriptive Statistics for Los Angeles Unified School District Eighth Graders, 2013–19

	(1) ZOC	(2) Non-ZOC	(3) Difference	(4) Matched Non-ZOC	(5) Difference	(6) Lottery
8th Grade ELA Scores	-.055	.175	-.23***	.077	-.132***	.038
			(.05)		(.047)	
8th Grade Math Scores	-.039	.177	-.216***	.075	-.114***	.066
			(.048)		(.043)	
Black Share	.041	.11	-.069***	.119	-.078***	.018
			(.024)		(.029)	
Hispanic	.879	.672	.207***	.718	.161***	.871
			(.044)		(.045)	
White	.018	.111	-.092***	.085	-.066***	.015
			(.019)		(.017)	
English Learner	.102	.077	.025**	.084	.018	.068
			(.011)		(.013)	
Special Education	.032	.032	.001	.032	0	.057
			(.002)		(.002)	
Female	.506	.509	-.003	.507	-.001	.502
			(.01)		(.01)	
Migrant	.155	.165	-.011	.161	-.007	.143
			(.012)		(.014)	
Spanish at Home	.741	.548	.193***	.591	.15***	.736
			(.045)		(.047)	
Poverty	.852	.775	.077***	.805	.047*	.874
			(.024)		(.024)	
Parents College +	.029	.061	-.032***	.047	-.018***	.028
			(.008)		(.007)	
Schools	38	49		38		
Students	53437	82421		61902		5878

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.

4 Empirical Analysis

4.1 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. We match each ZOC school to a school with similar Hispanic and poverty shares, breaking ties using a propensity score estimated in an earlier step (Arnold, 2019).[14] As shown in Table 1, the pool of candidate control schools is limited and the matching strategy mostly eliminates schools whose students tend to perform well above district averages. Throughout the analysis, results look similar among matched and unmatched samples and we adjust estimates for differences in student characteristics.

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

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

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 X_i is a vector of student characteristics. If both groups' outcomes trend similarly, the coefficients β_k are period-k-specific difference-in-differences estimates capturing the causal impact of ZOC. The design builds in placebo tests that help identify potential violations of the parallel-trends assumption: for $k < 0$, a nonzero β_k would suggest a violation of the parallel-trends assumption. Throughout, standard errors are clustered at the school level, although results are robust to two-way clustering that accounts for correlation within schools across years and across schools within a given year.

4.1.1 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 by the time they took achievement exams in eleventh grade. For the first cohort with full exposure to the program, ZOC achievement improves by 0.09σ relative to the improvement among non-ZOC students and continues to improve, leveling out at roughly 0.16σ by the seventh year of the program. Appendix Figure E.2a reports math-score treatment effects that are nearly identical to ELA treatment effects.[15] Importantly, the results look similar in both matched and unmatched samples, indicating our findings are not driven by convenient sample

[14]Propensity scores are estimated using cross-sectional data on schools the year before the program expanded. Propensity scores come from logistic regressions of ZOC indicators on school-average ELA and math scores and on race, sex, and socioeconomic-status shares. Appendix D discusses the matching strategy and results in further detail.

[15]We 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.

selection introduced by the matching strategy.[16]

Event-study results for four-year-college enrollment are reported in Figure 3b. Similarly 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 improve by an additional 5 percentage points compared with the non-ZOC change.

It helps to benchmark these effects. The ZOC student-achievement gap that is present when students enter high school is eliminated by eleventh grade. We can also benchmark these effects by comparing the treatment effects with the pre-ZOC eleventh-grade achievement gaps, which are roughly 0.2σ in the unmatched sample and $0.11 - 0.13\sigma$ in the matched sample. Either benchmark suggests a substantial reduction in within-district neighborhood-based achievement gaps.[17] 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 enrollment, and we find some suggestive evidence of diversion from private universities. We also do not find evidence of effects on community college enrollment. Therefore, the college-enrollment event-study evidence suggests ZOC has been effective in pushing students to enter college.

4.1.2 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 that the improvements found in the previous section are concentrated among high achievers or that the gains of some students come at the expense of others. For college outcomes, it is plausible that ZOC nudges more marginal students into college but does not affect students whose college-enrollment propensities are low. In this section, we study distributional treatment-effect heterogeneity to explore these possibilities.

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

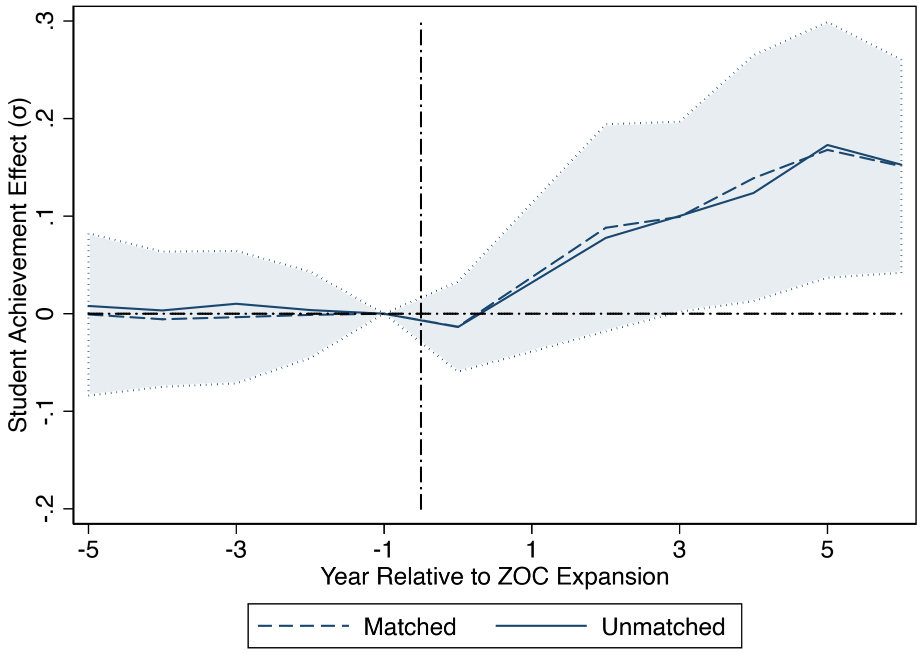
$$\mathbf{1}\{A_i \leq a\} = \mu_{j(i)} + \mu_{t(i)} + \gamma_a PreZOC_{it} + \beta_a PostZOC_{it} + \boldsymbol{X}_i'\psi + u_i. \tag{3}$$

[16] In Appendix Figure L.5, we report estimates that don't restrict the set of comparison schools to comparable schools defined in Section 3. The results look qualitatively similar and are more precisely estimated. Therefore, our main results are the most conservative.

[17] Appendix Figure L.4 reports estimates of the eleventh-grade ZOC achievement gap over time, showing it is decreasing and eliminated by the sixth year of the program and also providing additional evidence supporting the parallel-trends assumption.

Figure 3: Achievement and College-Enrollment Event Studies

(a) Achievement Event Study



(b) Four-Year-College-Enrollment 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 Zones of Choice (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 that adjust for covariates $\boldsymbol{X}_i$, and the solid line corresponds to estimates that are not regression adjusted. Panel B reports estimates that adjust for covariates. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed in the shaded regions.

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 4 reports the distributional estimates. We find that most of the improvements—indicated by negative treatment effects at different distribution points—take place in the bottom half of the distribution and that estimates at the top are centered around zero. These results suggest that 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-interevention distributional effects pointing to additional evidence in support of the parallel trends assumption. We explore this further in Appendix G, in which we estimate counterfactual distributions to provide more details about the distributional effects using various decompositions. Overall, we show that treatment effects are largest among lower-achieving students.

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.[18] 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 5 shows that treatment effects are not just concentrated among students who are more likely to enroll in college, and as with previous results, the treatment effects are larger as exposure to the program increases for later cohorts. Although treatment effects for students in the top two quartile groups are larger in magnitude, the treatment effects for students in the bottom two quartile groups represent a roughly 40 percent increase from the baseline mean as compared with a roughly 20 percent increase for students in the top two quartile groups.[19]

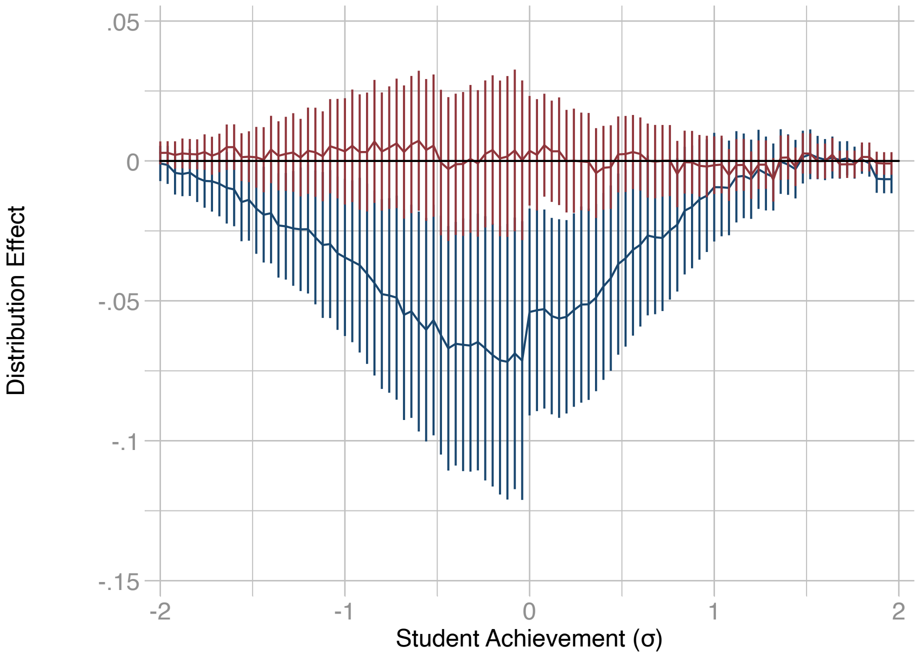
The heterogeneity analysis provides evidence that ZOC is effective in increasing achievement among students who would have otherwise performed poorly and that those gains do not come at the expense of high-achieving students. We have also shown that ZOC improves four-year-college-enrollment outcomes, regardless of students' predicted probabilities of going to college, which suggests that the gains are not just concentrated among relatively low-achieving students, unlike with achievement effects.

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

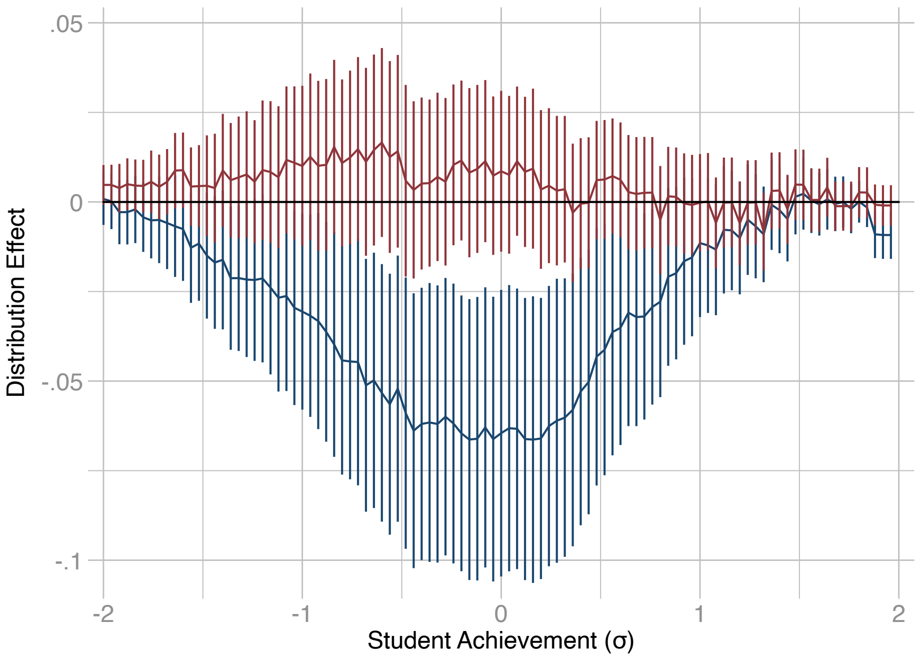
[19]Appendix Figure L.3 reports trends by different quartile groups.

Figure 4: Student-Achievement Distributional Impacts

(a) Reading



(b) Math

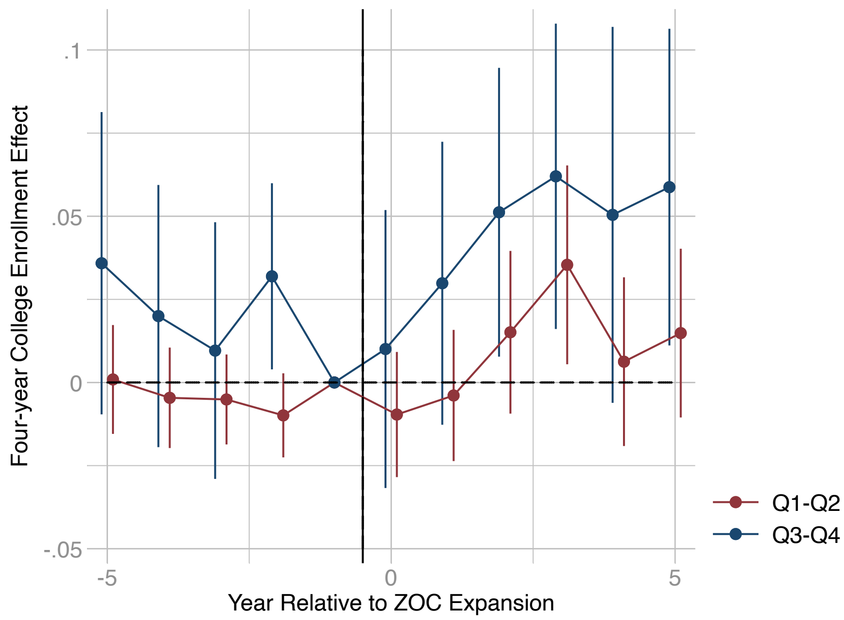


Notes: This figure reports estimates of β_a from Equation 3 for one hundred equally distanced points between -2 and 2. β_a corresponds to a difference-in-differences estimate on the probability of students scoring below a on their student-achievement exams. Standard errors are clustered at the school level, and 95 percent confidence regions by bars around the point estimates.

4.1.3 Robustness Checks

There are several potential threats to identification. Although 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 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 F.1 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.

Figure 5: 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 Zones of Choice 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.

It remains possible that some students, similar on observables, strategically sorted into ZOC neighborhoods and differ on unobservables. We partially address this concern by restricting the sample to students that did not move into a ZOC neighborhood in eighth grade. Estimates are also the same when we restrict the sample to students who did not sort into a ZOC neighborhood during middle school. Appendix Figures F.2 and F.3 report these estimates. The similarity of estimates for the combination of movers and nonmovers and for just nonmovers suggests that biases induced by strategic sorting are not driving our results.

We also estimate models using within-student variation, adjusting the parallel-trends assumption to parallel trends in achievement *growth*. Specifically, we estimate

$$\Delta A_i = \mu_t + \mu_{j(i)} + \sum_{k \neq -1} \beta_k ZOC_{j(i)} \times \mathbf{1}\{t(i) - 2013 = k\} + \boldsymbol{X}_i' \psi + u_{it},$$

where ΔA_i is a student's achievement gain between eighth and eleventh grades. The estimates β_k are identified by within-student variation by comparing changes in ZOC-student gains with changes in non-ZOC-student gains before and after the program's expansion. Appendix Figure F.4 reports these estimates, which are qualitatively similar to baseline estimates.

Other contemporaneous policies that may have differentially affected ZOC schools and students are also a concern. Notably, the Local Control Funding Formula (LCFF) substantially altered the funding of school districts in California and was implemented one year after the ZOC expansion. 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, for several reasons, the LCFF is unlikely to pose a problem. First, 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.

That evidence notwithstanding, we conduct a placebo exercise to assess the potential presence of LCFF effects. 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 F.5 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.

4.2 Decomposition of Achievement Effects

The achievement effects show that ZOC-student achievement improves at a remarkable pace compared with improvements of students enrolled at similar schools. There are two potential sources of such gains. If parents choose schools better suited to their children's needs, then match effects would explain a portion of the gains. Alternatively, changes in school effectiveness in response to competitive pressure could contribute to the gains. In this section, we decompose the achievement effects to provide a more refined understanding of the source of the gains.

4.2.1 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. We adopt the potential-outcome model of Abdulkadiroğlu et al. (2020), a generalized value-added model that allows for student-school match effects. Students indexed by i attend one school from a menu of schools $j \in J$. A projection of potential achievement A_{ij} on student characteristics $\boldsymbol{X}_i$ and school effects α_j yields[20]

$$A_{ij} = \alpha_j + \boldsymbol{X}_i' \beta_j + u_{ij}, \tag{4}$$

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

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

Adding and subtracting a_i from Equation 4 allows us to express the potential achievement of student i at school j as the product of three factors: ability, the relative effectiveness of school j, and student-school match quality M_{ij}. Therefore, potential outcomes can be written as follows:

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

Student ability a_i is invariant to the school a student attends, ATE_j is school j's causal effect on achievement relative to the average school, and M_{ij} captures j's suitability for student i. A positive M_{ij} could arise if students sort into schools based on returns to their particular attributes as captured by $\boldsymbol{X}_i'(\beta_j - \bar{\beta})$ or unobserved factors $(u_{ij} - \bar{u}_i)$ that make student i suitable for school j.[21]

4.2.2 Value-Added Model Estimation and Bias Tests

For the decomposition, we estimate treatment effects on α_j and the observable component of M_{ij}. Treatment effects on the former are due to changes in school quality, and treatment effects on the latter are due to changes in student-school match quality. These models have similar identifying assumptions discussed in the preceding section but require an additional assumption. We rely on a selection-on-observables assumption to obtain unbiased estimates of M_{ij} and α_j:

$$E[A_{ij}|X_i, j(i) = j] = \alpha_j + \boldsymbol{X}_i' \gamma_j; \quad j = 1, \cdots, J. \tag{5}$$

[20] We suppress time indices for notational ease.

[21] For example, variation in the poverty gap across schools j introduces the scope for poor students to sort into schools in which such students 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.

This assumes that assignments to schools are as good as random conditional on $\boldsymbol{X}_i$. The vector of covariates $\boldsymbol{X}_i$ includes race, sex, poverty indicators, migrant indicators, English-learner status, and lagged test scores, with lagged test scores being sufficiently rich in some settings to generate α_{jt} estimates with decent average predictive validity or minimal forecast bias (Chetty et al., 2014a, Deming et al., 2014). 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 $\widehat{A}_i$. To test for bias, we treat $\widehat{A}_i$ as an endogenous variable in a two-stage-least-squares framework using L lottery-offer dummies $Z_{i\ell}$ that we collect across zones and cohorts:

Table 2: Forecast Bias and Overidentification Tests: 2013–17 Cohorts

	(1) Uncontrolled	(2) Constant Effect	(3) Preferred
Forecast Coefficient	.612	1.205	1.01
	(.213)	(.112)	(.09)
First Stage F	8.89	11.699	12.035
Bias Tests:			
Forecast Bias (1 d.f.)			
P-value	[.068]	[.077]	[.972]
Overidentification (116 d.f)			
P-value	[.131]	[.526]	[.435]

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 that applied to an oversubscribed school within a school choice zone. Column (1) measures school effectiveness as the school-mean outcome, while Column (2) uses time-invariant value-added estimates and Column (3) uses time-varying and heterogeneous value-added estimates from Equation 4. 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.

$$A_i = \xi + \phi \widehat{A}_i + \sum_{\ell} \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i' \delta + \varepsilon_i \tag{6}$$

$$\widehat{A}_i = \psi + \sum_{\ell} \pi_\ell Z_{i\ell} + \boldsymbol{X}_i' \xi + e_i. \tag{7}$$

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., 2014a, 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 2 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. (2014a), 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 0.61, indicating the uncontrolled model does not pass the first test. Column 2 reports a model corresponding to the null hypothesis that value added is constant across years. This represents the scenario in which school effectiveness does not adjust in response to the program. We reject this model and find it has poor average predictive validity. In Column 3, we report results for our preferred model outlined in Equation 4. 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 12, passing the rule-of-thumb test. This evidence notwithstanding, we report the reduced-form estimates and first-stage estimates in Appendix Figure I.1 corresponding to the overidentification test. While the results in Table 2 do not entirely rule out bias in OLS value-added estimates, they are reassuring.

4.2.3 Event-Study Results

Viewed through the lens of the model of school competition discussed in Section 2, we expect to find evidence of improvements in α_j, indicating that ZOC has had a causal impact on student learning. Figure 6a reports event-study estimates for school effectiveness. We do not find evidence of differential trends in the pre-period. In line with the event-study evidence on achievement effects, we find a clear trend break in the relative improvements in ZOC school effectiveness, accounting for most of the observed achievement effects.

An alternative source of gains arises from the choices families make. An expanded choice set introduces scope to select schools that more adequately suit students' needs and, as a consequence, indicates the potential for achievement effects even in the absence of competitive effects. Figure 6b shows that match effects play a minor role in the observed achievement effects. Again, we find evidence that trends in match quality were similar before ZOC, but the trend break following ZOC 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.

Two comments are warranted about this evidence. First, the α_j effects are consistent with the competitive-effects conjecture but still do not rule out other contemporaneous shocks as a potential explanation. In the following section, we leverage ZOC-specific variation in OVG to test the competitive-effects hypothesis. That variation captures (albeit imperfectly) differences in the competitive pressures schools faced at the start of the program—where the differences should more plausibly be uncorrelated with other contemporaneous shocks—providing a 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. The decomposition nonetheless provides evidence consistent with the first implication of the model discussed in Section 2.

4.3 School-Effectiveness Treatment-Effect Heterogeneity

We now turn to Implication 2, which states that lower-performing schools should improve more than higher-performing schools, implying a decrease in within-zone dispersion of school quality. Following the framework used to study distributional effects on student achievement, we assess whether most of the gains come from the bottom half of the distribution.

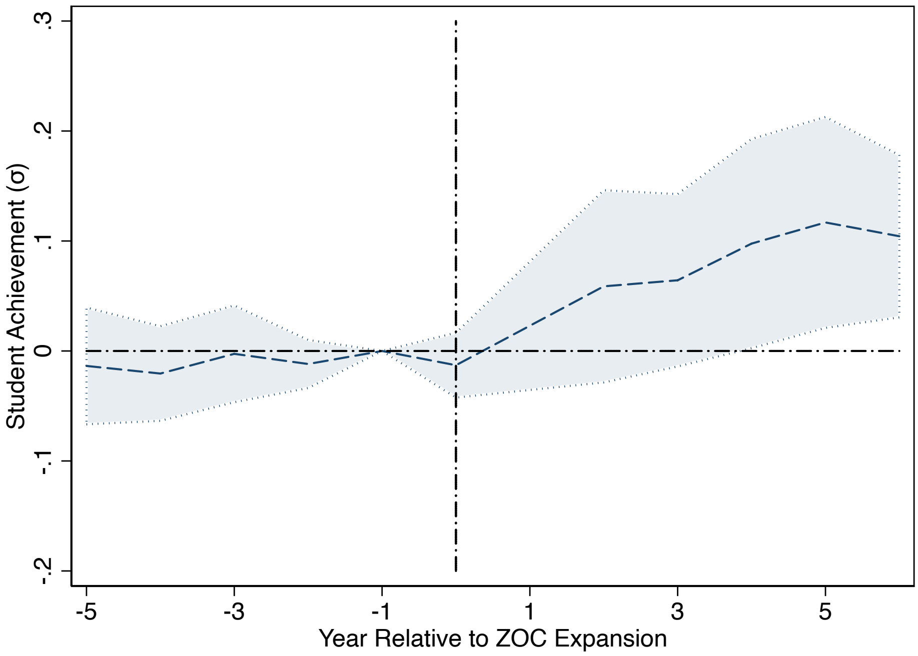
Figure 7 reports distributional estimates, where indicators $\mathbf{1}\{\alpha_{jt} \leq \alpha\}$ are the outcome variables in school-level difference-in-differences regressions for one hundred equally spaced points α in the support of the school-effectiveness distribution. We find improvements along most of the distribution except for the top quartile, where we observe minimal impacts. For example, the estimates suggest that the probability of ZOC value added being less than the district average decreased by roughly 10 percentage points. In contrast, there was not a meaningful differential change in the cumulative distribution function (CDF) in upper regions of the distribution. This evidence suggests that most of the changes in the school-quality distribution are concentrated among lower-performing schools, consistent with the conjecture that the lowest-performing schools improve most. We provide evidence supporting the parallel-trends assumption across the VA distribution in Appendix Figure G.2, providing reassuring evidence for the underlying assumptions of this design.

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. Additional details are described in Appendix G. Figure 7b reports the implied treatment effects at various quantiles. These estimates more clearly show that most of the 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. Both of these estimates suggest that the ZOC distribution experienced a compression relative to the non-ZOC distribution.

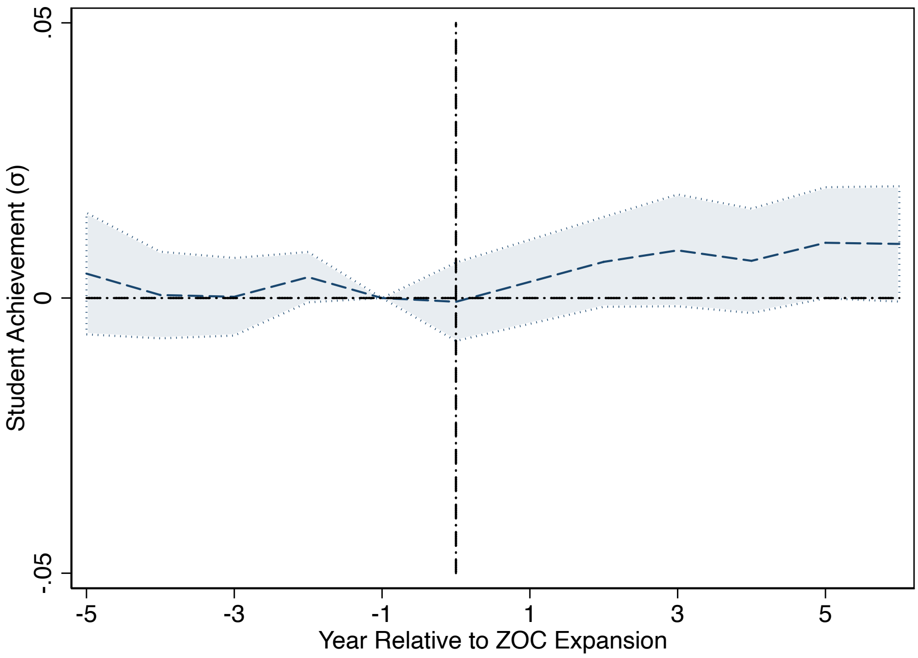
Piecing the evidence from Sections 4.2 and 4.3 together suggests that schools respond to competition, with the schools facing the most pressure to improve improving the most. However, these results partly hinge on families incentivizing schools to care about their contribution to student learning. This motivates us to pivot to studying parents' preferences in the next section. Our focus on preferences will also allow us to construct the competition indices discussed in Section 2 and help us further shed light about competitive effects.

Figure 6: Decomposition Event Studies

(a) Averge-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 Zones of Choice (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 in the shaded regions.

5 Demand and Option-Value Gain

Our analysis of the supply side demonstrated that schools adjusted their quality in response to the program's introduction. Turning to the demand side will allow 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.

5.1 Estimating Demand Parameters

We use rank-ordered-preference data submitted by ZOC applicants to estimate demand parameters (Abdulkadiroğlu et al., 2020, Agarwal and Somaini, 2019, Beuermann et al., 2018, Hastings et al., 2005).[22] The model in Section 2 allowed school popularity to vary by student characteristics $\boldsymbol{X}_i$, 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, therefore,

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

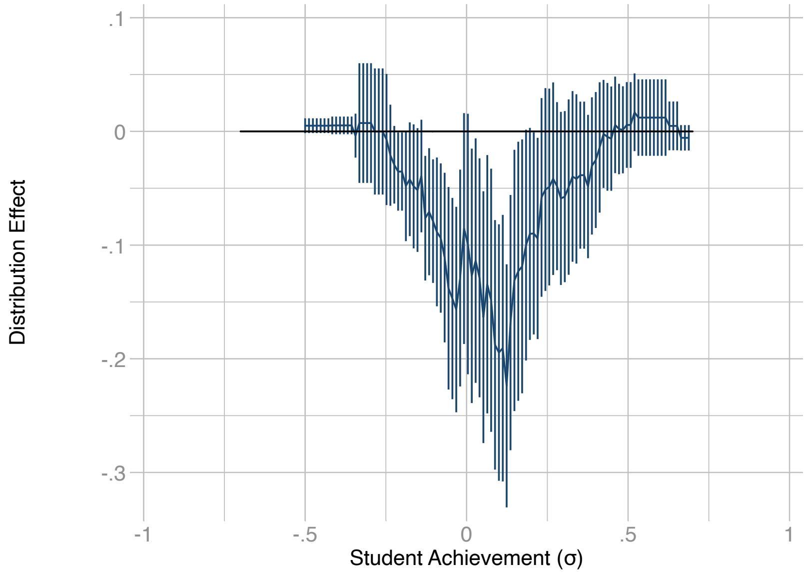
where δ_{jc} summarizes school j's popularity among students in achievement cell c, d_{ij} is distance from student i's residence to school j, and ε_{ij} captures idiosyncratic preference heterogeneity. We normalize $V_{ij} = 0$ for the one program in each zone.

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, $R_i = (R_{1i}, R_{2i}, \cdots, R_{Z(i)i}) \in \mathcal{R}$,[23] where R is the set of all possible rank-ordered lists.

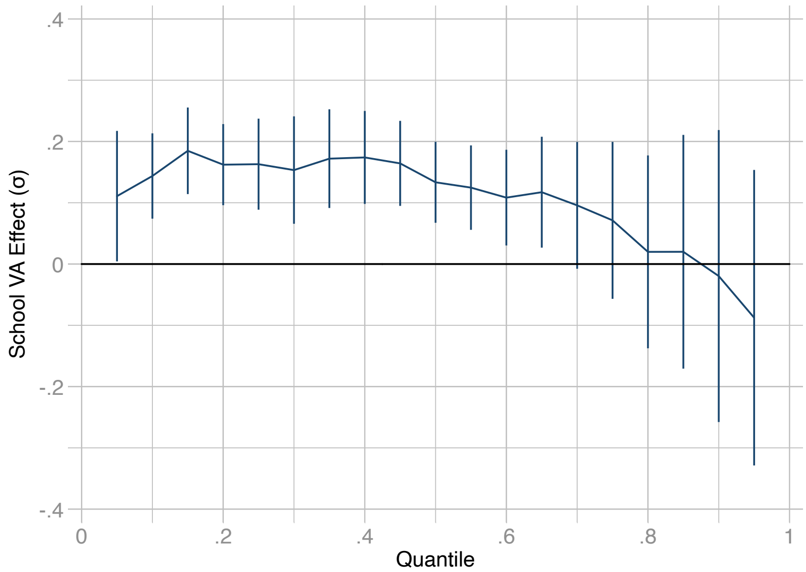
[22]The ZOC setting provides an advantageous feature in that students residing within a zone must rank *all* schools within their zone and only schools within their zone. Therefore, we observe complete rankings for all students within each zone, regardless of attendance, and we do not face the issues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

[23]Although applicants are required to rank all options in their zone, there are some applications without complete rankings. We consider these short lists in the estimation procedure.

Figure 7: Distribution and Quantile Treatment Effects on School Effectiveness



(a) Distribution Effects on School ATE



(b) Quantile Treatment Effects

Notes: Panel A reports point post-intervention difference-in-differences estimates from regressions of school-level indicators $\mathbf{1}\{\alpha_{jt} <= y\}$ on year indicators, school indicators, school-level student incoming achievement, pre, and post indicators interacted with Zones of Choice (ZOC) indicators for one hundred equally spaced points y between -0.7 and 0.7. Standard errors are clustered at the school level, and 95 percent confidence intervals are shown by shaded regions. Panel B reports unconditional quantile treatment effects estimated by inverting both the observed ZOC average-treatment-effect (ATE) 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.

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

$$R_{ik} = \begin{cases} \arg\max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg\max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}. \tag{8}$$

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

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r | U_{ir} < U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \tag{9}$$

We aggregate the log of Equation 9 across individuals to construct the complete likelihood and to estimate parameters of the utility specification via maximum likelihood.

While the first 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, 2019, Pathak and Sönmez, 2013) or if applicants do not understand the mechanism's rules or do have biased beliefs (Kapor et al., 2020). Although this is likely in ZOC neighborhoods, schools observe reported preferences—truthful or not—and respond to this demand accordingly. Nonetheless, our second estimation approach allows for strategic incentives.

The estimation approach that allows for strategic estimation departs from the standard model by first observing that applicants take into account their admissions chances in their reports. Let $p_i = (p_{i1}, \cdots, p_{iJ})$ be applicant i's admission chances at their available options.[24] We now assume that the unobserved preference heterogeneity $\varepsilon_i = (\varepsilon_{i1}, \cdots, \varepsilon_{ij}) \sim \mathcal{N}(0, \Sigma)$, where Σ is an unrestricted covariance matrix allowing for flexible heteroscedasticity and correlated preference shocks. From this perspective, R_i is a choice over a lottery in the set $\mathcal{L} = \{L_{R_i} \mid R_i \in \mathcal{R}\}$. Given a vector of latent indirect utilities $U_i \in \mathbb{R}^J$ and admissions chances p_i, an applicant reports $R_i \in \mathcal{R}$ only if

$$L_{R_i} \cdot U_i > L_{R_i'} \cdot U_i \quad \text{for all } R_i' \in \mathcal{R}. \tag{10}$$

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 10 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

[24] We construct bootstrapped-rational-expectations admissions probabilities following Agarwal and Somaini (2018).

optimal only if

$$L_{R_i} \cdot U_i > L_{R_i'} \cdot U_i \quad \text{for all } R_i' \in \mathcal{R}^*_{R_i}, \tag{11}$$

where $\mathcal{R}^*_{R_i}$ is a set that can be obtained from making a one-preference permutation of programs within R_i. Equation 11 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*.[25]

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

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

We then iterate through the following sequence of conditional posteriors:

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

In the last step of the above sequence, we condition on utility space $C(\mathcal{R}^*_{R_i})$ that rationalizes R_i. The one-shot permutations change the conditioning set in the last step of the sequence, 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 100,000 iterations. We estimate separate models for each zone-cell-year combination and discard the first 10,000 draws in order to allow for burn-in.

After obtaining estimates of Δ and λ by both estimation procedures, we can turn to analyzing factors that parents weigh when choosing schools and further probe the competitive-effects interpretation of our results. In each approach, we estimate parameters separately for different zone-year-cell combinations. In our analysis, 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 use estimates derived from the first cohorts of the program to ensure our measures of competitive incentives adequately capture demand-side pressures at the start of the program.

5.2 Parents' Valuation of School Effectiveness

In this section, we relate estimates of δ_{jct} to school effectiveness α_{jt}, average school peer quality Q^P_{jt}, and average school match quality Q^M_{jct} implied by the student-achievement decomposition

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

presented in Section 4.2.1. We estimate

$$\delta_{jct} = \xi_{cz(j)t} + \omega_P Q^P_{jt} + \omega_S \alpha_{jt} + \omega_M Q^M_{jct} + u_{jct}, \tag{12}$$

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 one–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 3 reports estimates of Equation 12. 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 account for strategic incentives in estimating mean utilities. In Panel B, 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 B 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 are more in tune with demand 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.

These findings contrast with findings in other settings, where parents are found to select schools based on achievement levels as opposed to gains (Abdulkadiroğlu et al., 2020, Hastings et al., 2005, Rothstein, 2006). In contrast to other settings, one notable feature of the ZOC setting is the homogeneity of students within each zone, effectively eliminating 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 selection on levels as found in other settings. The relative homogeneity of students within zones is one potential reason why the ZOC preference estimates contrast with those in other settings (for example, Abdulkadiroğlu et al. (2020) and Rothstein (2006)). 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.

Table 3: Preferences for School Attributes

	(1)	(2)	(3)	(4)
	Panel A: Rank-ordered Logit Estimates			
School Quality	0.137***			0.129***
	(0.0365)			(0.0358)
	[0.035]			[0.071]
Peer Quality		0.116		0.0393
		(0.135)		(0.139)
		[0.645]		[0.967]
Match Quality			0.118	0.0495
			(0.108)	(0.0699)
			[0.211]	[0.233]
Observations	596	596	596	596
R-squared	0.440	0.429	0.437	0.431
	Panel B: Strategic Estimates			
School Quality	0.0543			0.0392
	(0.104)			(0.0918)
	[0.779]			[0.778]
Peer Quality		0.126		0.0947
		(0.188)		(0.204)
		[0.701]		[0.698]
Match Quality			0.0379	0.0259
			(0.191)	(0.200)
			[0.769]	[0.805]
Observations	526	526	526	526
R-squared	0.615	0.615	0.615	0.616
Zone X Cell X Year FE	X	X	X	X

Notes: This table reports estimates from regressions of school-popularity measures δ_{jct} for each school among students in achievement cell c in cohort t on estimated school average treatment effect, ability, and match effects all scaled in standard deviation units. Panel A uses δ_{jct} estimates from rank-ordered logit models, and Panel B 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 clustering at the zone level and few clusters.

5.3 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 2, 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. Therefore, evidence of OVG treatment-effect heterogeneity provides support for the competitive-effects hypothesis.

We use preference parameters corresponding to the first cohort of ZOC students to estimate student OVG for all cohorts.[26] Figure C.1 displays the distribution of OVG across students, and Table C.1 reports OVG correlates.[27] A student is classified as a high-OVG student if their calculated OVG is in the top two quartiles of the OVG distribution within their cohort. Importantly, because we know student addresses we are able to classify high-OVG students before and after the ZOC expansion even if they do not eventually enroll in a ZOC school.[28]

Figure C.3 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.

Student-level OVG is informative about which students gain access to more popular schools, accounting for distance costs. High-OVG students can experience potential improvements in achievement through two channels: switching to higher quality schools or through relatively larger improvements in their neighborhood school. We estimate models that leverage differences in OVG across students to explore the extent of these possibilities. To do this, we augment the difference-in-differences framework from Section 4.1 with interaction terms that capture whether students are classified as high-OVG students. We consider the following specification:

$$Y_i = \mu_{j(i)} + \mu_{t(i)} + \beta Post_t \times ZOC_{j(i)} + \gamma Post_t \times ZOC_{j(i)} \times HighOVG_i + \boldsymbol{X}_i \psi + u_{it} \quad (13)$$

where $HighOVG_i$ classifies students as high-OVG students if their estimated OVG is in the top

[26]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 cohorts to construct measures of OVG that are free of this potential influence.

[27]The average OVG for the first cohort was roughly eighteen, meaning the typical ZOC household was willing to drive eighteen additional miles (thirty-six round trip) per day to access the schools in their choice set. A back-of-the-envelope calculation using average gas prices in Los Angeles in 2012 and the fuel efficiency of the average vehicle reveals that the average household was willing to pay $1,080 for its new menu of schools.

[28]In particular, because we can assign OVG to students in the pre-period due, there are ZOC-residing students high-OVG students 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 identification of high-OVG effects.

two quartiles of the student OVG distribution, and the vector $\boldsymbol{X}_i$ includes the same controls as before and is augmented with main effects for high-OVG students and other relevant interaction terms. The parameters of interest β and γ inform us about ZOC effects, with γ capturing the differential ZOC effect for high-OVG students.

Table 4 reports estimates of OVG treatment-effect heterogeneity. In Column 1 we report estimates of β and γ, and the estimates suggest that OVG explains a substantial share of the positive achievement impacts documented in Section 4.1.1. However, 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 high-OVG effects; the OVG interaction terms are remarkably stable across most columns.

The findings reported in Table 4 suggest that students who gained access to relatively more popular schools experienced the largest improvements in achievement. These students may have experienced larger improvements by either switching to higher-quality schools or staying at their neighborhood schools, which experienced relatively larger improvements. 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 Column 7 reveal that even within zones, high-OVG students experienced larger improvements in achievement, a finding further consistent with the competitive effects hypothesis.

6 Lottery Analysis

The preceding market-level analysis demonstrated a remarkable improvement in ZOC-student achievement, and the improvements is closely tied to improvements in schools' impact on test-score gains. Alternative research designs leverage lottery variation to study the impacts of attending particular charter, pilot, intradistrict choice, or voucher school programs (Abdulkadiroğlu et al., 2011, 2018, Chabrier et al., 2016, Cullen et al., 2006, Rouse, 1998). We complement the market-level analysis with this alternative design and show that the majority of the ZOC benefits stem from market-level effects.

6.1 Standard Lottery Design

Lottery studies on public school open-enrollment programs (Cullen et al., 2006, Deming et al., 2014) assess whether students' academic performance improves if they attend a most preferred school. In the ZOC setting, students' choice sets expand and we ask whether students obtain a premium from attending a most preferred school, relative to other lower-ranked ZOC schools they may attend if they do not get an offer from their most preferred school. We relate achievement A_i to indicators of most-preferred-school enrollment D_i in the following way:

$$A_i = \beta D_i + \sum_{\ell} \gamma_\ell d_{i\ell} + X_i'\delta + u_i.$$

Table 4: Option-Value-Gain and Treatment-Effect Heterogeneity

	(1) Reading	(2) Reading	(3) Reading	(4) Reading	(5) Reading	(6) Reading	(7) Reading
PostZOC	0.084	0.078**	0.045	0.069	0.081**	0.081	
		(0.038)	(0.051)	(0.048)	(0.036)	(0.057)	
PostZOC $\times OVG_{3,4}$	0.153***	0.153***	0.149***	0.146***	0.153***	0.090***	0.088***
	(0.024)	(0.028)	(0.027)	(0.027)	(0.028)	(0.024)	(0.024)
PostZOC $\times$							
Poverty				-0.010		-0.009	
				(0.016)		(0.015)	
Parent College +				0.023		0.029	
				(0.020)		(0.022)	
Migrant				-0.090***		-0.097***	
				(0.026)		(0.026)	
Spanish at Home				0.048**		0.031*	
				(0.022)		(0.016)	
Special Education				0.054		0.021	
				(0.034)		(0.033)	
Black			-0.042			-0.083***	
			(0.028)			(0.026)	
Hispanic			0.046			-0.007	
			(0.034)			(0.032)	
Female		0.011				0.017*	
		(0.009)				(0.009)	
Lagged Scores					-0.038***	-0.041***	
					(0.011)	(0.011)	
Observations	221,954	221,954	221,954	221,954	221,954	221,954	221,954
R-squared	0.516	0.511	0.512	0.512	0.512	0.512	0.516

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. $SchoolOVG_{3,4}$ is an indicator for a student's presence in the top two quartiles of the student OVG distribution . Additional rows correspond to estimates of coefficients corresponding to additional interactions between post indicators, Zones of Choice (ZOC) indicators, and row variables. All estimates include main effects for student OVG and lagged test scores. Standard errors are clustered at the school level.

Here, $d_{i\ell}$ are lottery dummies and X_i are baseline characteristics included to boost precision. Lottery offers Z_i are used as instruments for D_i in the following first-stage relationship:

$$D_i = \pi Z_i + \sum_{\ell} \rho_\ell d_{i\ell} + X_i' \xi + e_i.$$

These designs exploit the fact that conditional on $d_{i\ell}$, offers are as good as random, identifying β as the causal impact of attending a most preferred school. Random lottery offers arise in oversubscribed charter and voucher programs but more generally are embedded in student-assignment mechanisms such as those employed in Denver and New York (Abdulkadiroğlu et al., 2017, 2020) and ZOC.

If we also assume lottery offers only influence test scores through most preferred attendance and weakly increase the likelihood of enrollment in most preferred schools, then β is a local average treatment effect (LATE), meaning that it represents the causal impact of attending a most preferred school among the students induced to attend a most preferred school through their lottery offer. The LATE framework is useful in our setting because it allows us to estimate control-complier means (Abadie, 2002) and trace out differences in school quality between most preferred and less preferred schools over time.

Appendix I contains additional lottery details. We report balance tests to show the conditional randomness of lottery offers. The appendix also reports attrition differentials to ensure our lottery estimates are not driven by selective attrition out of the sample.

6.2 Results

Table 5 reports lottery estimates for various outcomes; Panel A reports achievement effects, and Panel B reports effects for other outcomes. We find that the probability of enrolling in a most preferred school increases by roughly 50 percentage points if a student is offered a seat. Panel A shows that students offered a seat at their most preferred school experience a 0.045σ gain in their eleventh-grade math scores but a minimal impact on their ELA scores. The implied LATE on compliers is twice the reduced-form effects. Panel B assesses whether attending a most preferred school affects other important outcomes such as enrolling in college, getting suspended, or taking more advanced courses; we do not find evidence that attending a most preferred school has an additional impact on four-year-college enrollment, suspensions, or taking advanced courses. These results indicate that while market-level effects on college enrollment are large, there is no additional college-enrollment premium from attending a most preferred school.

At first glance, these results suggest minimal impacts of attending most preferred schools. This could be because parents are not choosing more effective schools (in terms of value added) or because market-level effects are causing changes in premiums of most preferred schools. We explore this in Table 6, with impacts on ELA and math presented in Panels A and B, respectively. Column 3 reveals that only the first two cohorts of compliers experience ELA gains by eleventh grade; the following three cohorts do not experience gains distinguishable from noise. In Columns 4 and 5, we report control-complier means to assess how differences in premiums for most preferred schools change over time. Comparing these two columns shows control-complier achievement improving over time, with a less pronounced improvement among treated compliers. Columns 4 and 5 imply that school-effectiveness premiums are narrowing during this period, meaning the ELA achievement premiums present for earlier cohorts are eliminated. The pattern is not as evident for math scores, but we do find treatment effects narrowing across cohorts similarly to ELA effects.

The evidence reveals an initial premium of attending a most preferred school, but subsequent market-level effects diminish this premium as the lower-performing schools catch up with the initially higher-performing schools. Importantly, we probe for the market-level effects through different research designs and find evidence supporting the same conclusions. These pieces of evidence reveal that the majority of the program's impacts arise through the market-level effects, which result in compression of school quality within zones and eliminate premiums for most preferred schools but lead to overall improvements for ZOC students.

Table 5: Lottery Estimates

		FS	RF	TSLS
		(1)	(2)	(3)
		Panel A: Achievement		
ELA		.49***	.009	.019
		(.041)	(.022)	(.044)
	N		7731	
Math		.49***	.045**	.092**
		(.04)	(.02)	(.041)
	N		7710	
		Panel B: Other Outcomes		
College		.499***	.005	.01
		(.046)	(.014)	(.029)
	N		5820	
Ever Suspended		.49***	-.002	-.004
		(.04)	(.003)	(.005)
	N		7779	
Took Honors Course		.49***	0	-.001
		(.04)	(.001)	(.002)
	N		7779	

Notes: Each panel reports first-stage, reduced-form, and two-stage-least-squares estimates instrumenting attendance at most preferred schools with lottery offers. Panel A reports student-achievement effects, pooling all cohorts together. Panel B reports effects on indicators for ever enrolling in a four-year college, ever suspended by eleventh grade, and taking any honors course by eleventh grade. We do not observe NSC outcomes for the last cohort, so we do not include them in the estimates. Standard errors are clustered at the lottery level for all estimates and reported in parentheses.

Table 6: Lottery Estimates by Cohort, 2013–17

	(1) FS	(2) RF	(3) TSLS	(4) CCM	(5) TCM
		Panel A: ELA			
First and Second Cohorts	0.467***	0.047*	0.101**	[.071]	[.172]
	(0.063)	(0.024)	(0.048)		
Third and Fourth Cohorts	0.492***	-0.022	-0.045	[.201]	[.157]
	(0.053)	(0.029)	(0.058)		
Fifth Cohort	0.444***	0.002	0.005	[.244]	[.249]
	(0.089)	(0.047)	(0.105)		
		Panel B: Math			
First and Second Cohorts	0.467***	0.052	0.110	[.049]	[.159]
	(0.063)	(0.040)	(0.088)		
Third and Fourth Cohorts	0.492***	0.044*	0.089*	[.005]	[.094]
	(0.053)	(0.025)	(0.052)		
Fifth Cohort	0.444***	-0.001	-0.003	[.081]	[.078]
	(0.089)	(0.036)	(0.081)		

Notes: This table reports two-stage-least-squares estimates of how attending a most preferred school affects student achievement, separately for different groups of cohorts and separately by subject. Column (1) reports first-stage estimates, while Column (2) reports reduced-form estimates and Column (3) reports two-stage-least-squares estimates. Estimates in Column (3) adjust for sex, race, baseline scores on math and English and language arts (ELA), poverty, parental education, and other demographics reported in Table I.1. Column (4) reports control-complier means (CCM), and Column (5) reports treated-complier means (TCM), both reported in brackets; the difference between TCM and CCM is reported in Column (3). Standard errors, clustered at the lottery level, are in parentheses.

7 Discussion

Although this paper has documented evidence of substantial improvements in school quality for most ZOC schools relative to non-ZOC schools, the mechanisms behind these changes remain unclear. Changes in inputs—such as teacher quality and class size—represent one potential mechanism (Krueger, 1999). Another mechanism is changes in management practices associated with differences in productivity in firms (Bloom and Van Reenen, 2007, Gosnell et al., 2020) and in schools (Angrist et al., 2013, Bloom et al., 2015, Fryer Jr, 2014). Last, specific institutional features may mediate the effects of an increase in school choice. In this section, we discuss each of these possibilities.

7.1 Changes in School Inputs

Appendix K.1 reports evidence on changes in inputs such as teacher characteristics, teacher quality, and class size.[29] We do not find evidence that school inputs differentially changed between ZOC and non-ZOC schools. Therefore, changes in inputs cannot explain the improvements in school quality.

7.2 Changes in Management Practices

We do not have data to correlate treatment effects with changes in management practices—such as the no-excuses approach pursued by effective charter and public schools (Angrist et al., 2013, Fryer Jr, 2014). We focus on changes in classroom assignment policies because they allow us to indirectly probe for changes in management practices. Any changes of in assignment practices are likely determined by changes in principals' decisions. Appendix J addresses changes in student–teacher racial match, and Appendix K addresses changes in classroom assignment policies. We find evidence of increases in student–teacher racial match in ZOC schools, which has been shown to improve the achievement of minorities (Dee, 2004, 2005, Fairlie et al., 2014, Gershenson et al., 2018). We also find evidence of reductions in tracking. While the literature finds mixed results on the effects of tracking (Betts, 2011, Bui et al., 2014, Card and Giuliano, 2016, Cohodes, 2020, Duflo et al., 2011), the changes we find suggest other possible organizational changes among ZOC schools.

To explore the potential for additional changes, we study survey responses from a district-wide school-experience survey. Since 2011, LAUSD has administered a survey to all students, parents, and school staff asking them questions about their sentiment, school climate, and the academic environment. The survey changed substantially in its early years, so it is difficult to adequately track responses for most survey items over time.

One survey item asks students how they perceive teacher effort, and it is somewhat—but not perfectly—stable over time. 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.[30] Although it may seem that this survey item is meant to capture information about teacher effort, reported changes in perceptions about their effort may also be due to other changes occurring within the school. For example, if principals are allocating teachers to students in ways that improve match quality—teacher–student racial-match quality, for example—then students may be more likely to agree with the statement in the survey item even if teachers are not exerting additional effort.

[29] Teacher quality (value added) is estimated before the policy, so changes in teacher quality are among the set of teachers working in the district before the policy change.

[30] Between 2011 and 2014, students were asked to respond "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.

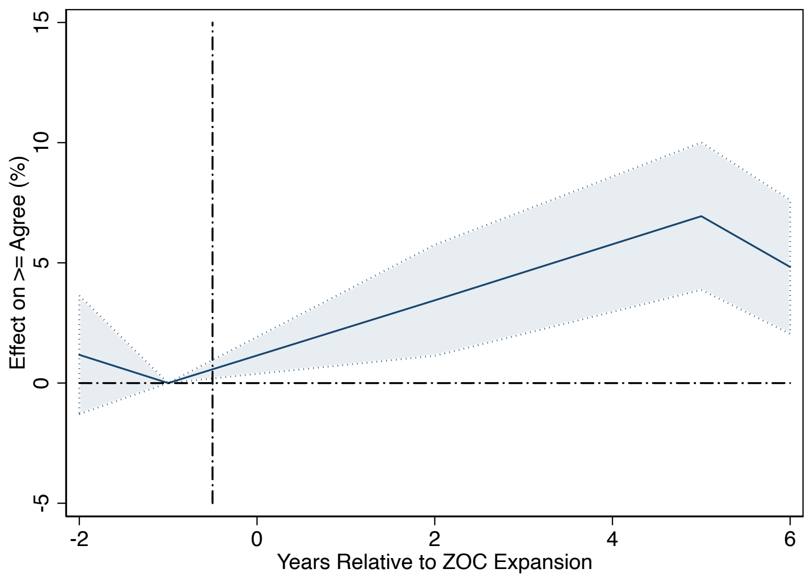
Since 2014, the survey item has been as follows:

My teachers work hard to help me with my schoolwork when I need it.

Therefore, the survey evidence serves to inform us about changes within schools that we cannot adequately observe in our primary data sources. Figure 8 demonstrates that ZOC students, compared with non-ZOC students, exhibited a greater increase in 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 actually did, it is reassuring to find evidence that ZOC students perceived a change relative to non-ZOC students.

In summary, we cannot decisively conclude that either changes in exposure to same-race teachers or changes in tracking practices contributed to the ZOC achievement and college-enrollment effects, but these findings do indicate a differential change in how ZOC schools operated during the period under study. The survey evidence suggests that other schooling practices may have also changed in ZOC schools.

Figure 8: Los Angeles Unified School District School-Experience Survey Evidence



7.3 Institutional Features of ZOC

Our findings show that a subtle change to the neighborhood-based assignment scheme in some Los Angeles neighborhoods led to sharp increases in student-achievement and four-year-college-enrollment outcomes. We find several pieces of evidence suggesting competition played a role. These treatment effects are large in comparison with the more modest effects of competition in public schools estimated in the literature (Card et al., 2010, Figlio and Hart, 2014, Figlio et al., 2020, Gilraine et al., 2019, Ridley and Terrier, 2018). Furthermore, consistent with the notion that schools adjusted their quality because of increased competition, we find that parents exhibited a stronger preference for value added than for other school characteristics, including schools' peer composition. While parents' reported preferences allows us to provide a more consistent narrative, it still stands in contrast with a growing body of evidence that parents select schools based on achievement levels instead of achievement gains (Abdulkadiroğlu et al., 2014, 2020, Rothstein, 2006). These differences raise the question: why is Los Angeles different?

ZOC administrators devote considerable resources to ensuring each cohort is informed about the application process and knows its schooling options; and administrators 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 they conduct 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). We emphasize that zones in our setting are small compared with the universal high school admissions process in New York, for example. In a setting such as New York, 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.

ZOC schools are also constrained in their means of adjusting quality. In particular, the returns to investing in screening strategies are limited among ZOC schools because the assignment mechanism does not permit additional screening priorities like those available in many New York schools (Abdulkadiroğlu et al., 2020, Corcoran et al., 2018). Therefore, even in a setting in which parents select schools based on achievement levels as opposed to gains, indicating a stronger preference for peer characteristics, recruitment efforts yield lower returns if screening strategies are restricted. The preference estimates suggest ZOC parents value factors correlated with gains more than factors correlated with levels, and the restricted nature of screening strategies may have further paved the way for the changes in school quality we find.

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. While it is possible that the relative homogeneity eliminates sorting on race or income, an outcome that would attenuate competitive effects, it does raise issues about how these racially and economically segregated schools impact long-run outcomes.

Card and Rothstein (2007) find robust evidence that in the short run, gaps in SAT test scores are larger in more segregated cities but neighborhood-segregation effects operate mostly through income and not race. Looking at medium-run outcomes, Billings et al. (2014) find that the end of race-based busing in Charlotte-Mecklenburg schools (in North Carolina) led to decreases in graduation and four-year-college enrollment for students assigned to schools with higher minority shares. Johnson (2011) focuses on long-run impacts and finds that court-ordered desegregation orders led to improvements in educational attainment, future earnings, and adult health for Black individuals. While we find that both short- and medium-run outcomes have improved, it remains to be seen whether ending K–12 education in racially isolated schools will harm the affected students. It remains an open question whether the impacts of another, similar program would be similar if it created zones that integrated students across race and income levels.

8 Conclusion

This paper studied a novel expansion of public school choice in Los Angeles: ZOC. The unique design and implementation of ZOC provide a rich setting to study the effects of competition among public schools, and the rich data set arising from the centralized assignment system permits 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. To distinguish between the effects of competition and the effects of improvements in student-school match quality, we decompose the achievement effects. Consistent with the competitive-effects conjecture, changes in schools' value added explain most of the achievement effect and changes in match quality are small. These findings are consistent with demand estimates that suggest 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.

Our market-level analysis helps explain why earlier cohorts benefited more from accessing in-demand schools than later cohorts did. This pattern is explained by the competition-induced incentives of less preferred schools, which led to a reduction over time in the premiums for most preferred schools. Importantly, the two complementary research designs help us show that most of the program's benefits arise through market-level effects and not through students' access to the more popular schools.

Our findings reveal that public school choice programs can elevate students' educational outcomes, but they also raise several questions. ZOC presents an inherent trade-off between improving short-run outcomes through school competition and potentially hurting long-run outcomes through entrenching school-segregation patterns. 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.

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A Data Appendix

Table A.1: Zones of Choice 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	

A.1 Potential Impact of the Change to the SBAC

There is an additional factor to consider in the ZOC difference-in-difference estimates: the changing CST and SBAC distributions. One way to look at how this change potentially impacts difference-in-difference estimates is to decompose the change into two components. The first component holds the distribution fixed and a second component 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 $_t$ correspond to the district grade-year standard deviation in year t.

The change in mean standardized mean achievement for group g is

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

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

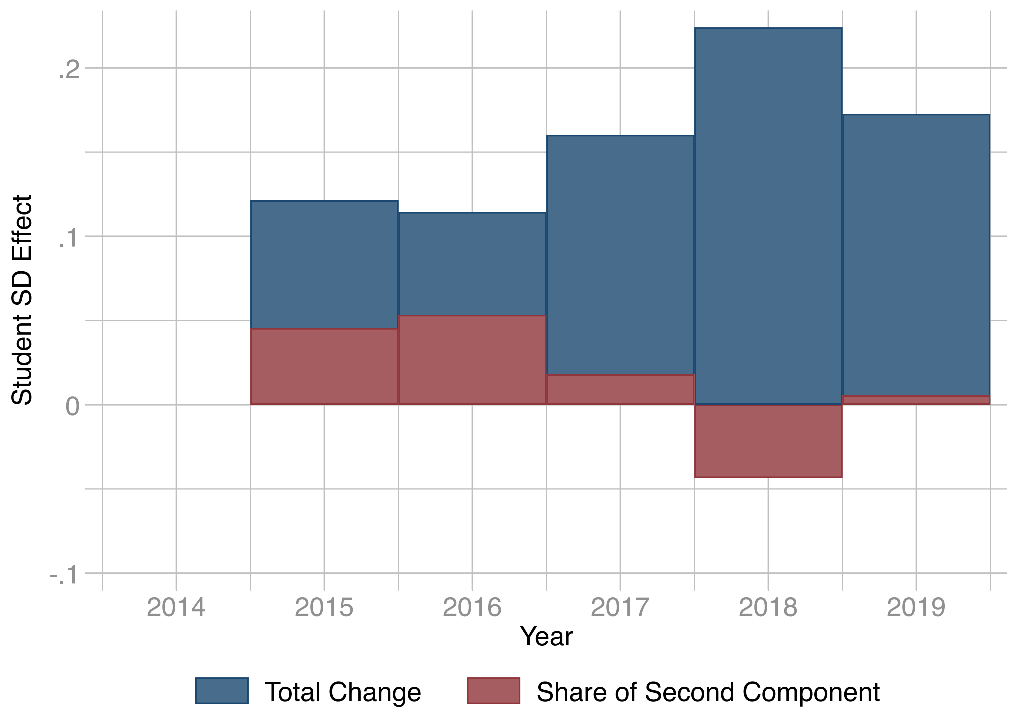
This implies that the difference-in-differences between ZOC and non-ZOC is

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

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

We report raw difference-in-difference estimates for the affected years below. Overall, the change in the dispersion of the scores 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.1: Influence of the Changing Score Distribution



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

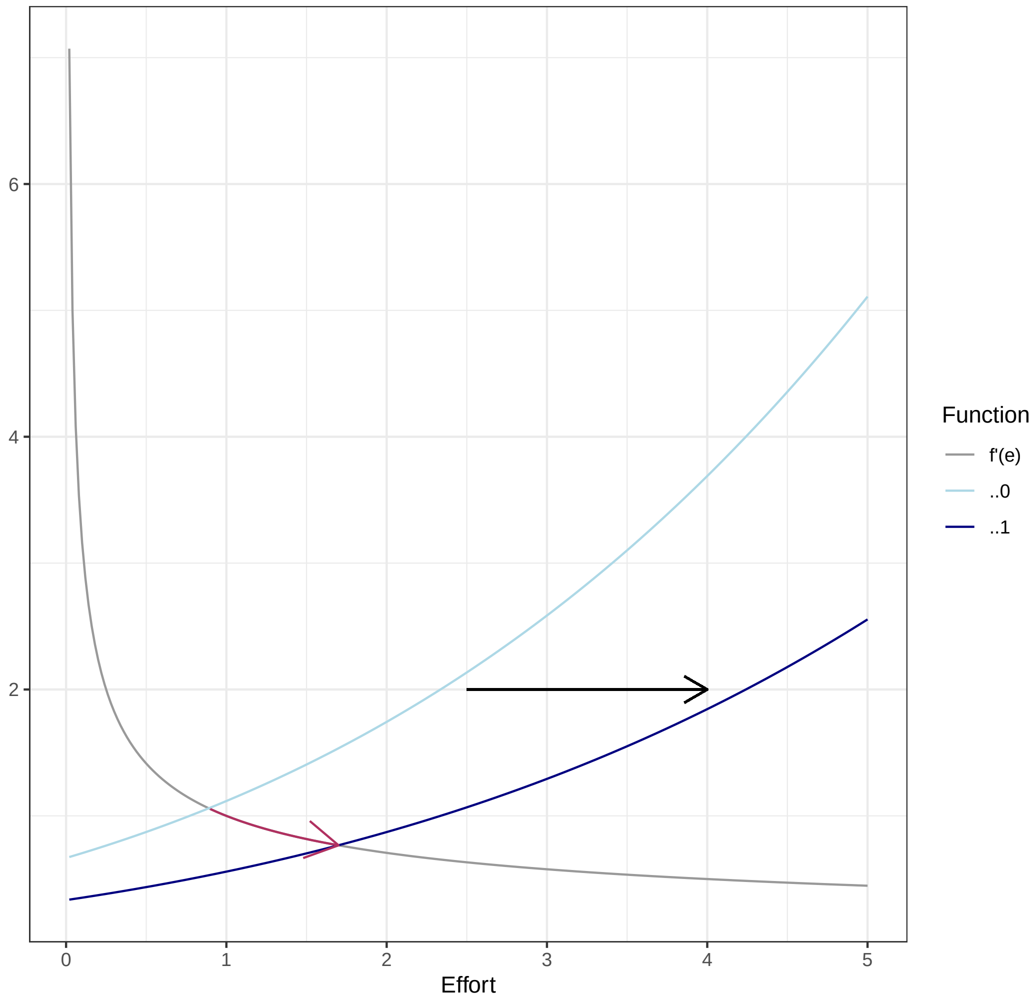
$$\Phi(e_j) = \frac{1}{\theta\omega \frac{1}{N} \sum_i P_{ij}(e_j; d_{ij}, X_i)(1 - P_{ij}(e_j; , d_{ij}, X_i))}$$

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. Strategic interactions complicate this intuition because principals' best responses lead to further shifts in Φ, and potential upward shifts leading to ambiguous effort levels relative to the pre-ZOC equilibrium.

Proposition 1 shows that there is a Nash equilibrium in the principal-effort game. Proposition 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 pre-ZOC equilibrium effort levels for all schools $j \in \mathcal{J}$. Proposition 3 further shows that schools with the initially lowest effort increase their effort the most, leading to a compression in the within-zone quality distribution. Last, Proposition 4 provides a comparative-static result indicating that an increase in OVG from an equilibrium would lead to further increases in effort. This last proof again relies on the intuition gained from shifts in Φ.

Figure B.1: Change in Equilibrium



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

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

There exists an $e^* \in [\underline{e}, \bar{e}]^J$ such that $e^{BR}(e^*) = e^*$. There 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 payoff of principal j is increasing in the effort of another principal $k \neq j$:

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

□

Proposition B.2. *If each school j has at least 50 percent market share before the ZOC expansion and the post-ZOC quality elasticity of demand for each student i for school j satisfies $\eta_{ij}^1 > \frac{P_{ij}^0}{P_{ij}^1}\eta_{ij}^0$, 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 for each school j, the principal's optimal level of effort is determined at the point at which Ψ and f' intersect. Therefore, principal j will find 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

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

That shows that the nonstrategic response would be to increase effort for each principal j. The effort game, however, makes it so that principals take into account other principals' responses.

Starting from $\Phi_1(e_{j0}, e_{-j})$, increases in effort from principals $j' \neq j$ would lead to further downward shifts in Φ, all else constant:

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

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

□

Proposition B.3. *For any two schools $i, j \in \mathcal{J}$ such that $e_i > e_j$, the change in the quality gap $\Delta e_{i,j}$ between the two schools in response to a marginal increase in effort Δe is*

$$\begin{aligned}\Delta e_{i,j} &\approx (f'(e_i) - f'(e_j))\Delta e \\ &< 0.\end{aligned}$$

Proposition B.4. *Effort e_j^{BR} is increasing in OVG for each school j.*

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

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

Therefore, for a marginal increase in $\boldsymbol{OVG}$, Φ shifts further downward, leading to increases in effort; and strategic complementarities in Proposition 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). □

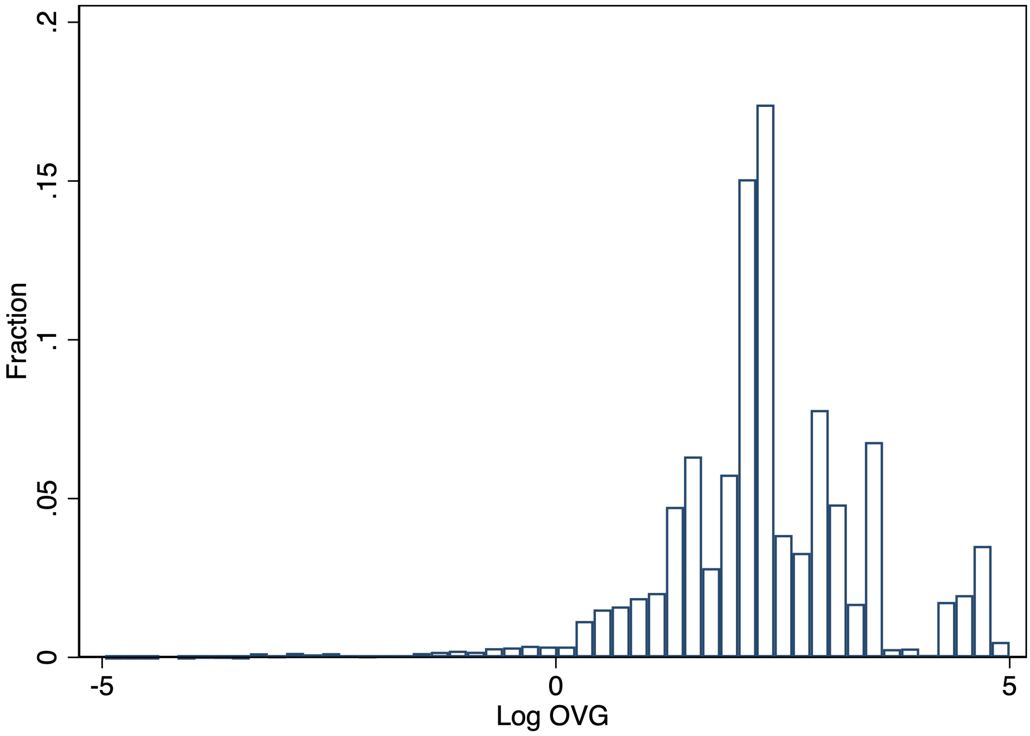
C Option-Value-Gain Details

Table C.1: Option-Value-Gain Correlations

	(1)	(2)
	Log OVG	Log OVG
Black	-0.0143	0.0957
	(0.133)	(0.0907)
Hispanic	0.118	0.0142
	(0.0896)	(0.0428)
Parent College +	0.0148	-0.00139
	(0.0862)	(0.0319)
Poverty	-0.168***	-0.00630
	(0.0332)	(0.0184)
Female	0.0271	-0.0126
	(0.0311)	(0.0181)
Spanish at Home	0.290***	0.0201
	(0.0438)	(0.0260)
English Learner	0.0217	-0.0249
	(0.0451)	(0.0269)
Migrant	0.163***	0.00864
	(0.0433)	(0.0218)
Middle School Suspensions	0.0129	-0.0199
	(0.0805)	(0.0539)
Distance to Most Preferred	0.00655***	0.00508***
	(0.000988)	(0.000691)
Low-Score Group	-0.159***	-0.0363
	(0.0470)	(0.0262)
Avg-Score Group	-0.0468	0.0393*
	(0.0421)	(0.0223)
Zone FE		X
Observations	12,499	12,499
R-squared	0.014	0.667

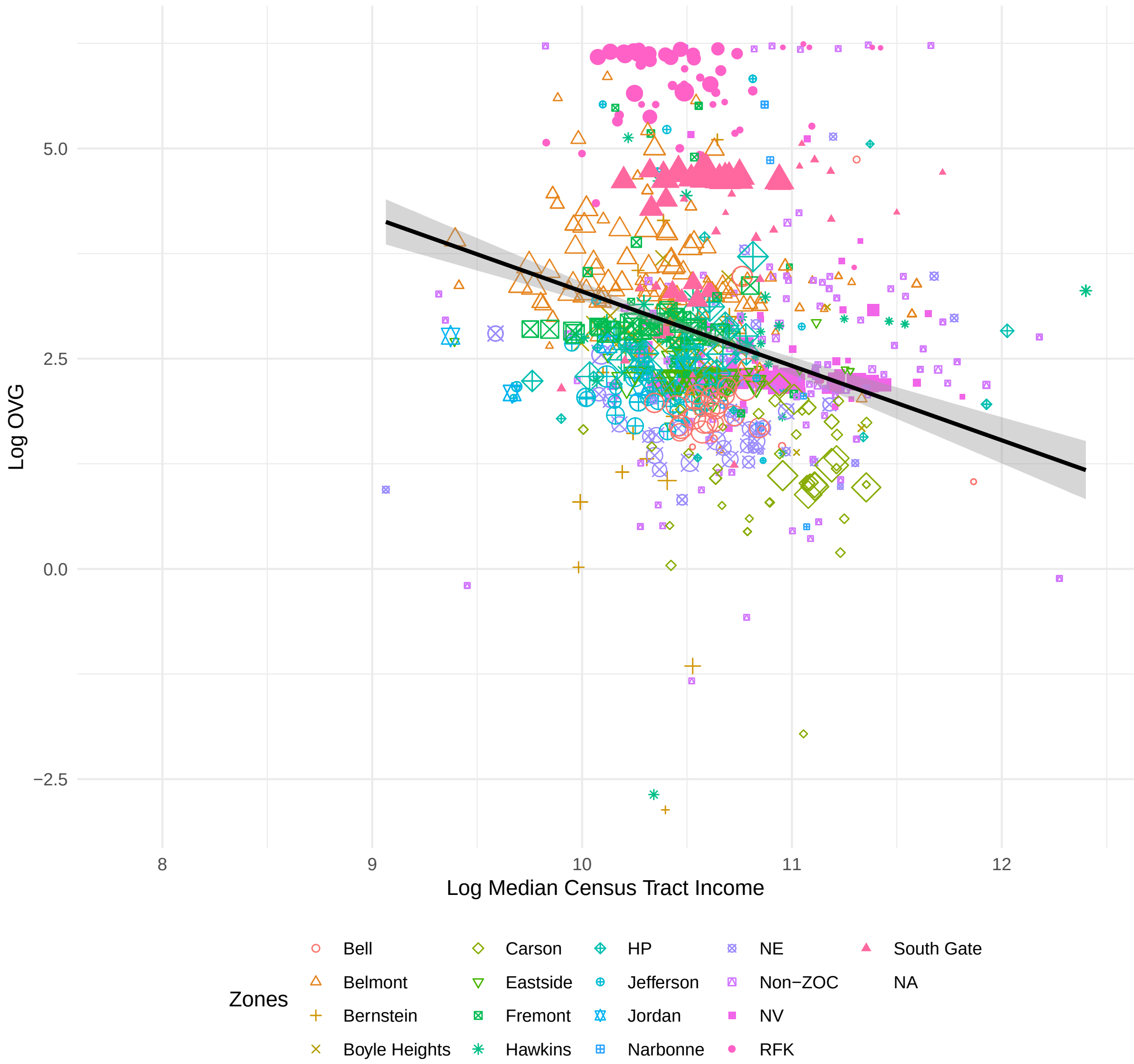
Notes: This table reports coefficients from multivariate regressions of log of option-value gain (OVG) on row covariates. The sample is restricted to the initial cohort of Zones of Choice students. Column (1) does not include zone fixed effects, while Column (2) does. Robust standard errors are reported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

Figure C.1: Log Option-Value-Gain Distribution



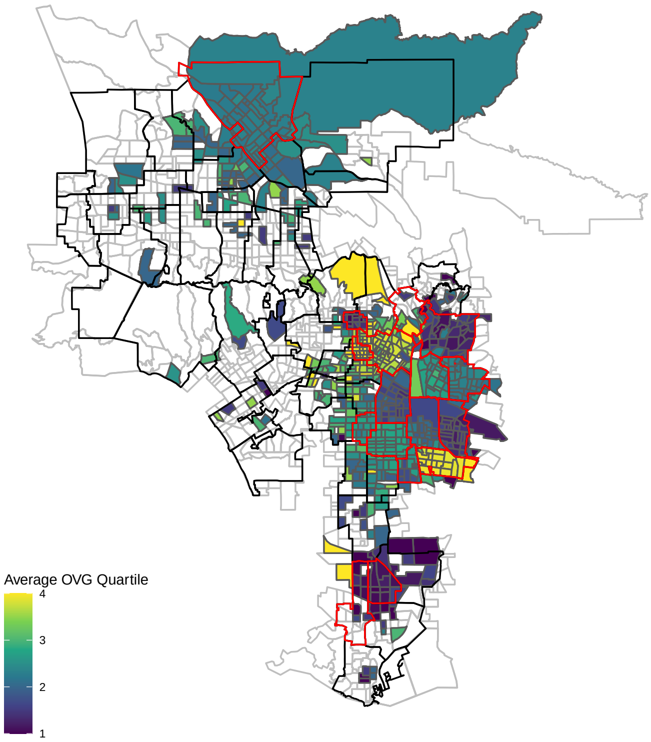
Notes: This figure presents a histogram of estimated log option-value gain (OVG) across all students and all years. Preference parameters used in OVG estimation are estimated using only the first cohort's preferences. OVG for later cohorts is constructed using these estimated parameters.

Figure C.2: Correlation of Option-Value Gain and Census-Tract Income



Notes: This map displays a scatter plot of census-tract average log option-value gain (OVG) and log median census-tract income from the 2010 US Census. Points are colored and shaped to reflect specific zones to demonstrate the contributions coming from different zones. Regression line displayed has slope -0.40615 (zone clustered SE=0.1986).

Figure C.3: Census-Tract Average Student Option-Value-Gain 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 Zones of Choice (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.

D Propensity-Score Estimation

Table D.1: School-Level Balance

	(1) ZOC	(2) Non-ZOC	(3) Difference
School Value Added	-.15	.018	-.168***
			(.052)
Incoming Test Scores	-.154	.134	-.287***
			(.066)
Black	.034	.122	-.087***
			(.025)
Hispanic	.89	.652	.237***
			(.041)
English Learner	.156	.091	.065***
			(.016)
Female	.518	.515	.002
			(.012)
Migrant	.179	.188	-.009
			(.014)
Spanish at Home	.782	.551	.231***
			(.044)
Poverty	.786	.717	.068**
			(.03)
Parents College +	.059	.136	-.077***
			(.015)
Incoming Suspensions	.155	.175	-.02
			(.017)
Incoming Cohort Size	371.604	342.469	29.135
			(34.761)
Schools	49	93	

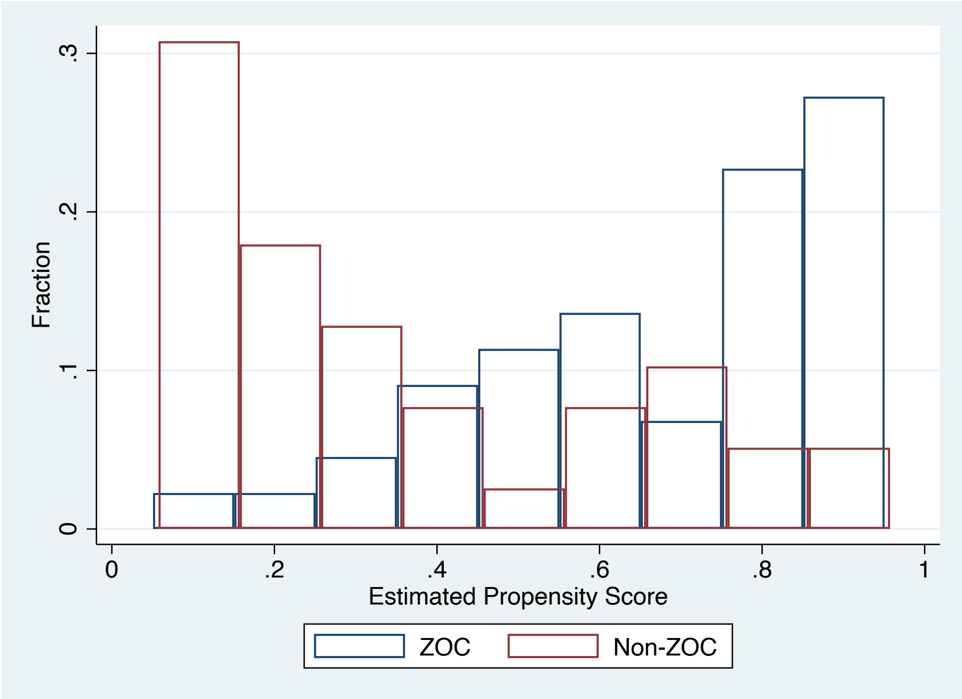
Notes: This table reports estimates from cross-sectional school-level bivariate regressions of the row variable on Zones of Choice (ZOC) school indicators in 2012. All regressions are weighted by school enrollment except for the model in which school enrollment is the outcome. Column (1) reports ZOC school means, Column (2) reports non-ZOC school means, and Column (3) reports the difference with robust standard errors in parentheses below.

Table D.2: Propensity-Score Model Estimates

	(1) ZOC	(2) ZOC	(3) ZOC
School Value Added	-0.377		
	(0.920)		
Incoming Test Scores	0.810	0.485	
	(1.152)	(0.838)	
Black	-8.281*	-8.221**	-8.497**
	(4.230)	(4.087)	(4.124)
English Learner	0.581	0.444	-0.435
	(2.943)	(2.887)	(2.450)
Female	-1.140	-1.085	-1.034
	(1.726)	(1.660)	(1.663)
Hispanic	-2.597	-2.772	-3.336
	(2.414)	(2.401)	(2.111)
Migrant	5.533*	5.221*	5.520*
	(2.897)	(2.934)	(2.835)
Parents College +	-22.68***	-22.35***	-21.11***
	(6.398)	(6.442)	(5.993)
Poverty	3.415**	3.498**	3.553**
	(1.672)	(1.640)	(1.587)
Spanish at Home	1.065	1.229	1.717
	(2.513)	(2.512)	(2.366)
Incoming Suspensions	-4.332**	-4.390*	-4.742**
	(2.151)	(2.262)	(2.225)
Incoming Cohort Size	0.00288*	0.00290*	0.00306**
	(0.00149)	(0.00149)	(0.00145)
Observations	142	142	142

Notes: This table reports estimates from multivariate logit regressions of Zones of Choice (ZOC) school indicators on row variables. Column (1) corresponds to the model used in the matching strategy, and Columns (2) and (3) show estimates that remove measures of academic performance. Robust standard errors are reported in parentheses.

Figure D.1: Propensity-Score Overlap

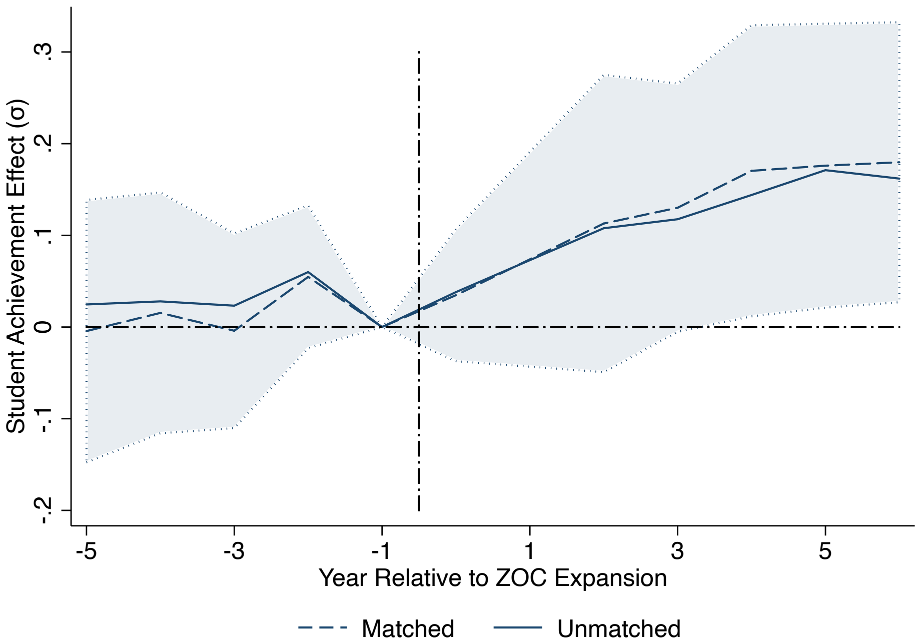


Notes: This figure reports histograms for the estimated school-level propensity scores by treatment status. Bin widths are equal to 0.1.

E Additional Event-Study Evidence

E.1 Math Estimates

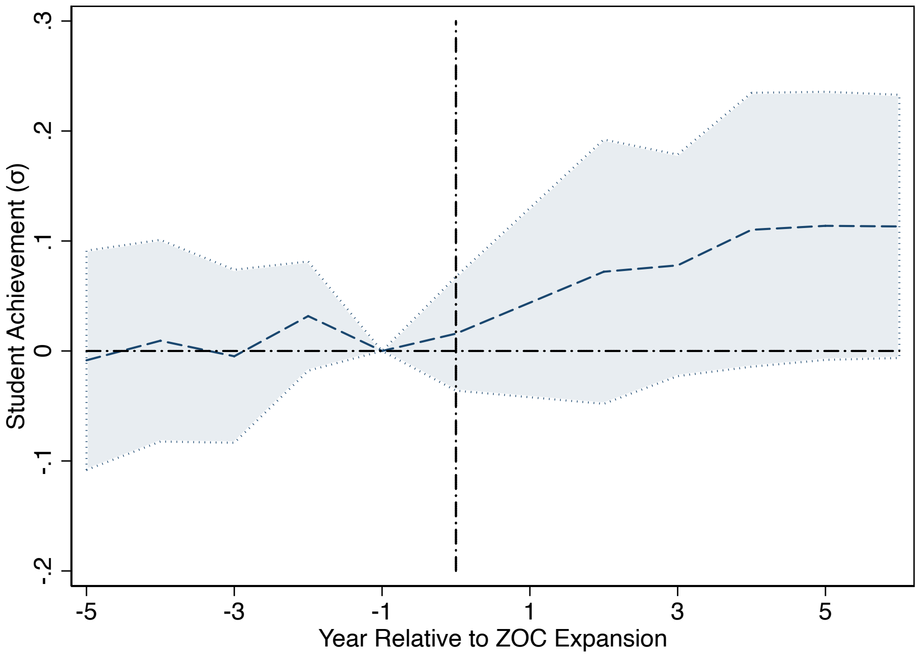
Figure E.1: Math-Achievement Event Study



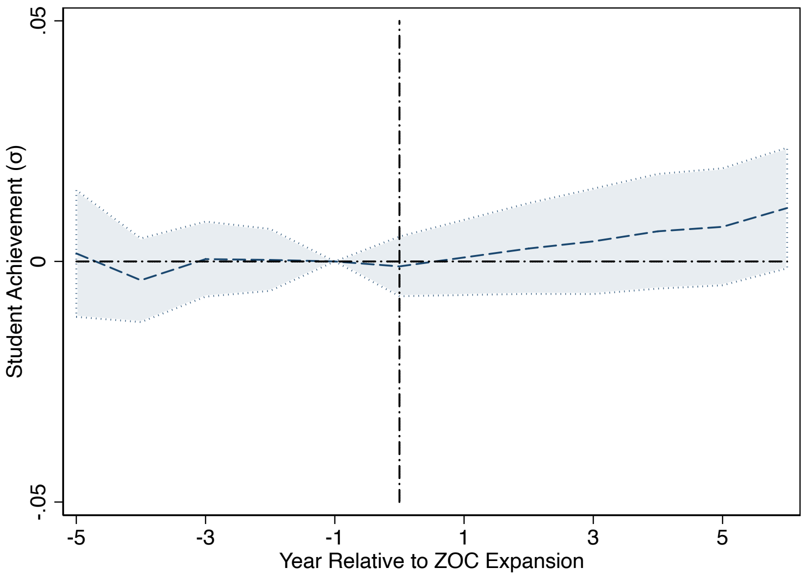
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the Zones of Choice (ZOC) expansion. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. The dashed blue line in Panel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

Figure E.2: Math Average Treatment Effect and Match Event Studies

(a) Average Treatment Effect



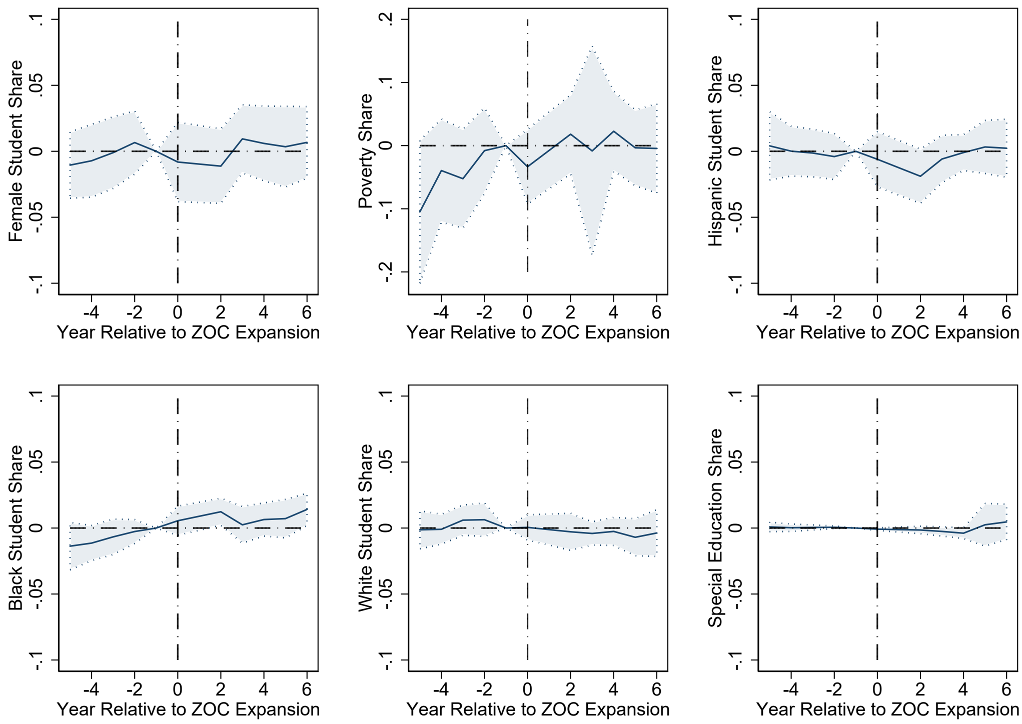
(b) Match



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 Zones of Choice (ZOC) expansion. The coefficient β_k shows difference-in-differences estimates of outcomes relative to the year before the policy. Standard errors are double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

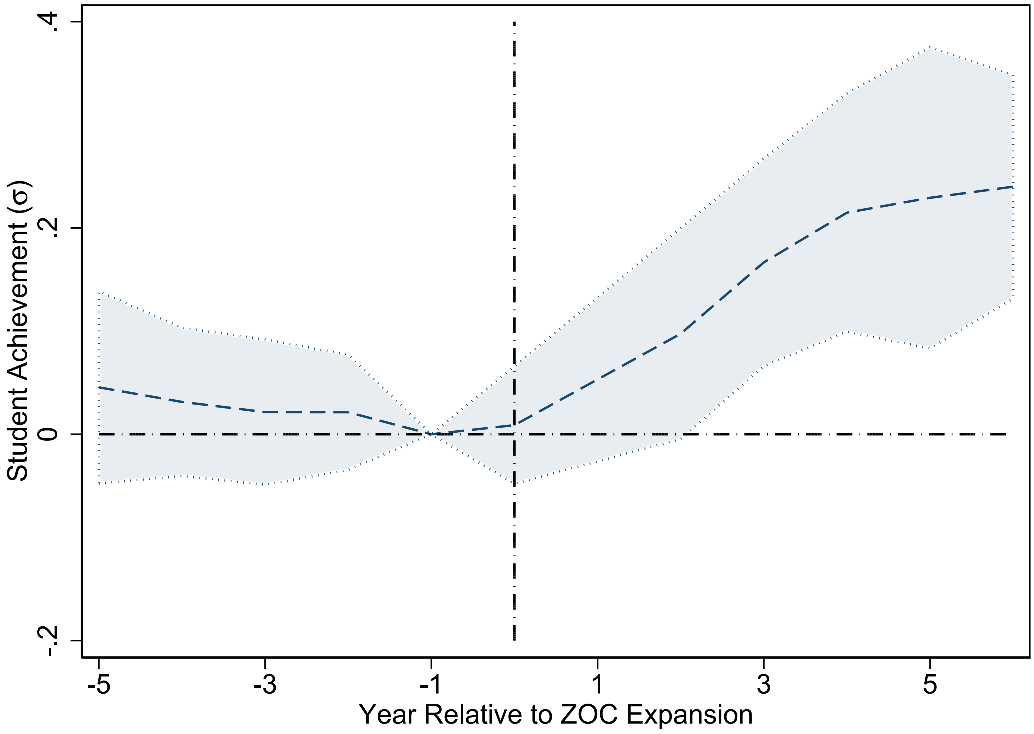
F Robustness Exercises

Figure F.1: Changes in Student Demographics



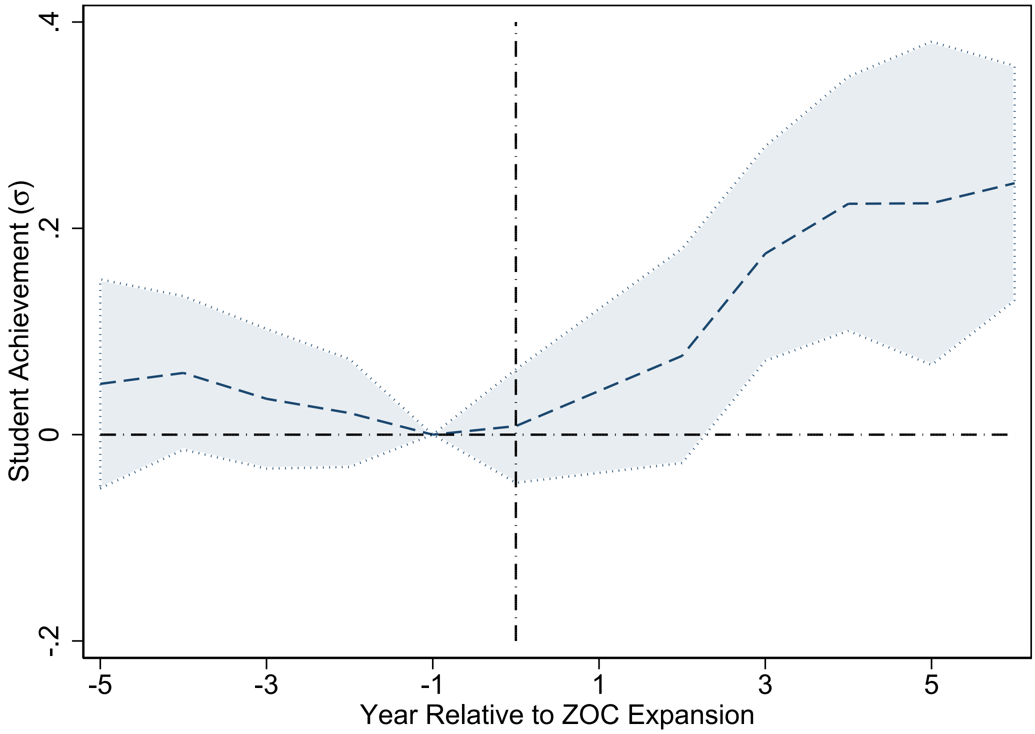
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the Zones of Choice (ZOC) expansion. The coefficient β_k shows the difference in the change of student characteristics, labeled on subfigure vertical axes, between ZOC and non-ZOC students relative to the year before the expansion. The solid blue line traces out estimates. Standard errors are double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

Figure F.2: Achievement Event Study Restricted to Students Who Didn't Move in Eighth Grade



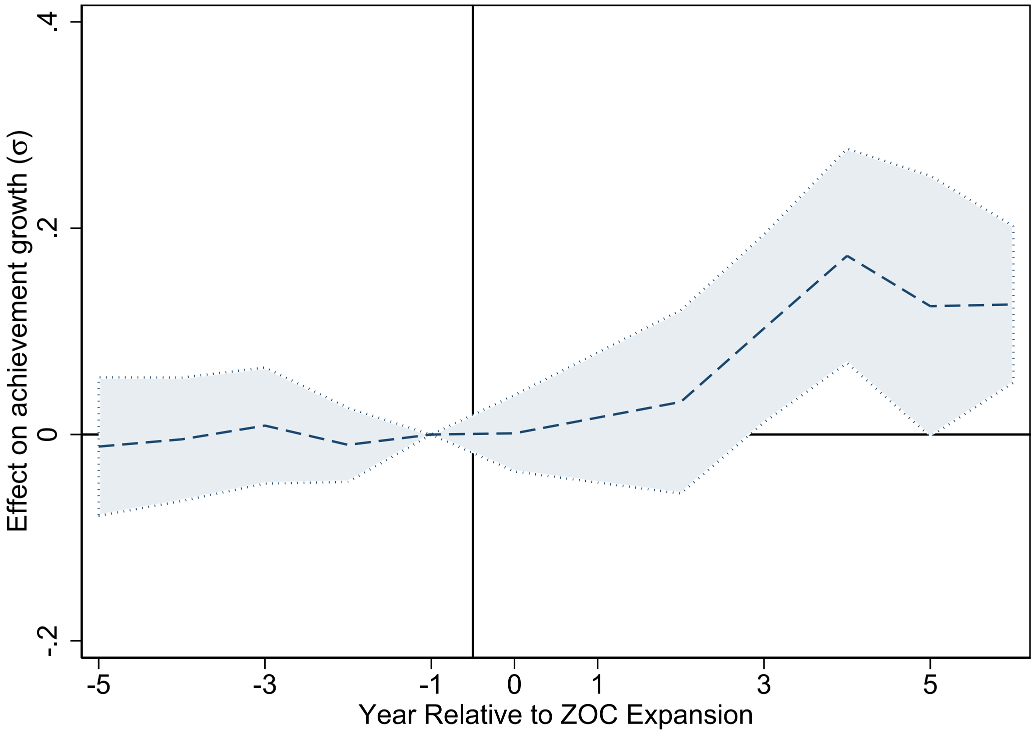
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the Zones of Choice (ZOC) expansion. The sample is restricted to students that did not move in eighth grade, the year before households submitted ZOC applications. The coefficient 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 double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

Figure F.3: Achievement Event Study Restricted to Students Who Didn't Move in Middle School



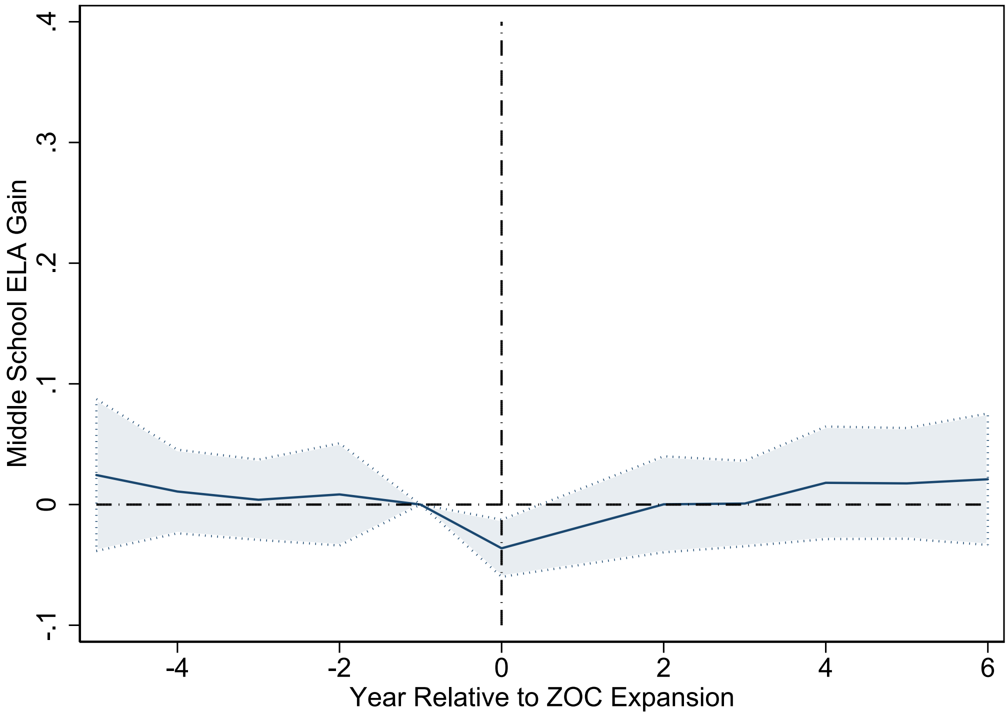
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the Zones of Choice (ZOC) expansion. The sample is restricted to students that did not move in eighth grade *and* did not move at any time during middle school. The coefficient β_k 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 double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

Figure F.4: Within-Student Achievement Gain



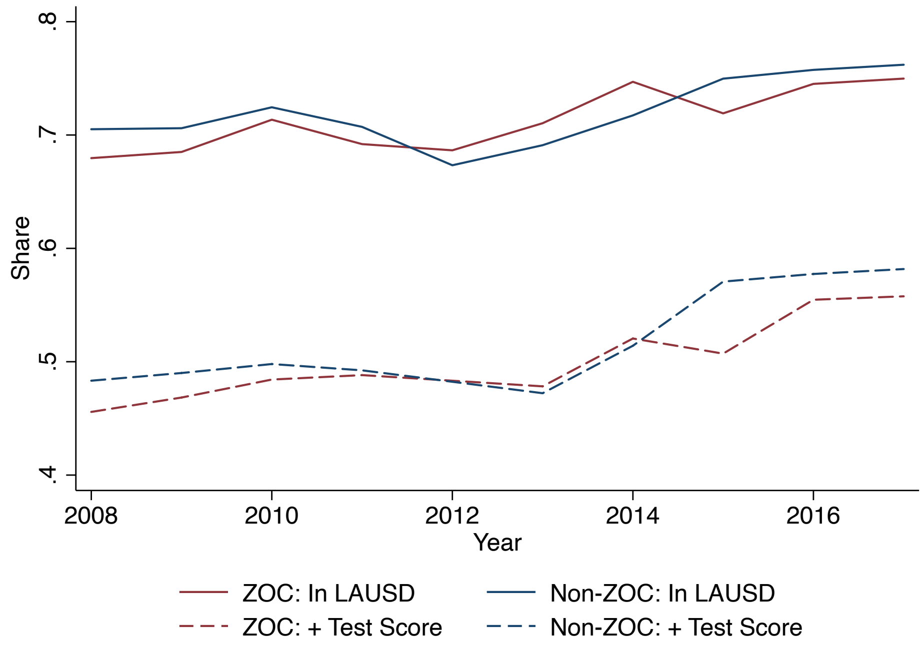
Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the Zones of Choice (ZOC) expansion. The outcome is student-achievement growth between eighth and eleventh grades, measured in student-achievement standard deviations. The coefficient β_k shows the difference in changes in 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 double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

Figure F.5: Falsification Test — Zones of Choice Impact on Middle School Gains

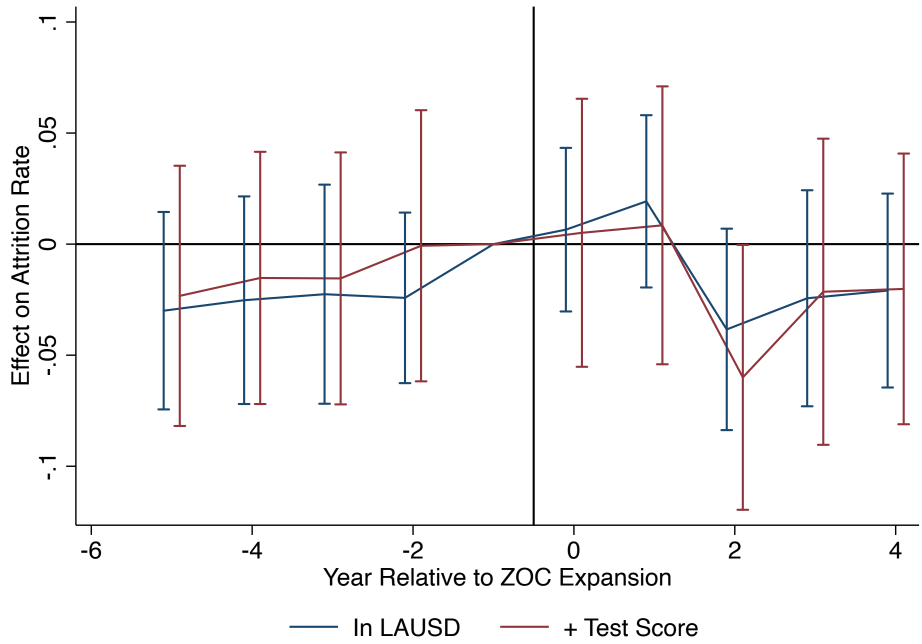


Notes: This figure reports estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the Zones of Choice (ZOC) expansion. The outcome is student-achievement growth between seventh and eighth grades, measured in student-achievement standard deviations and predating 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 double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

Figure F.6: 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 ninth grade that are present in eleventh grade and also the share of students in eleventh grade with test scores. Panel (b) reports unadjusted event-study analogs of Panel (a).

G Estimating Counterfactual Distributions

In this section, we discuss the methods used to estimate the counterfactual distributions used to construct quantile treatment effects in Figure 7b. These methods come from Chernozhukov et al. (2013) and Chernozhukov et al. (2020).

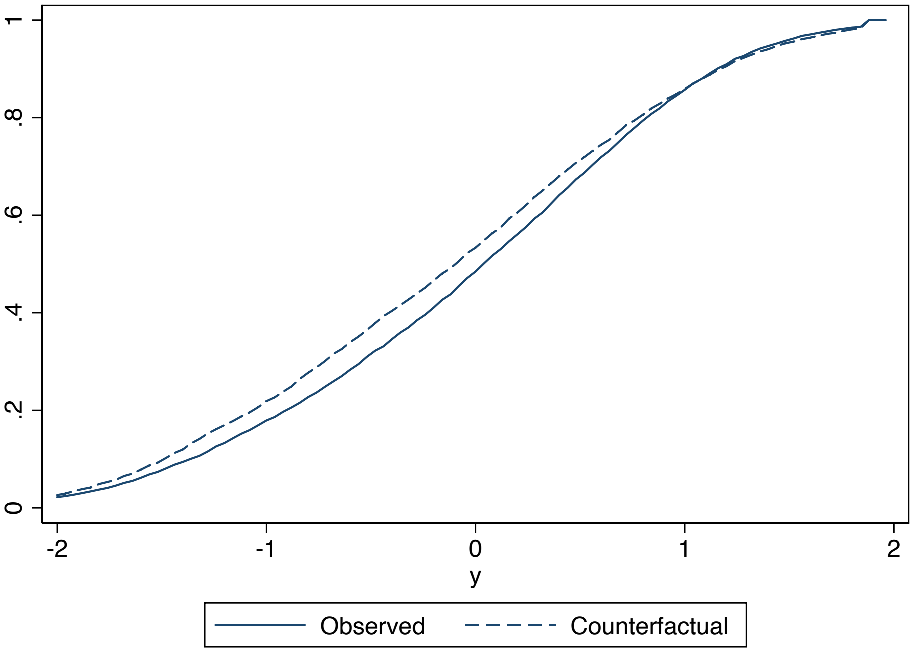
First, we outline the notation we use to construct counterfactual distributions that guide the rest of the empirical analysis. Let $F_{kkt}(a)$ be the observed distribution of an outcome A for group $k \in \{z, n\}$ at time $t = 0, 1$. Here the two groups are ZOC students (or schools), where z corresponds to ZOC and n corresponds to the control group. The pre-period consists of the year before the policy and the post-period consists of the last year in our data. The counterfactual distribution of A that would have prevailed for group z if it faced the conditional distribution of group n is

$$F_{nz}(a) = \int_{\mathcal{X}_z} F_{A_n|X_n}(a|x) dF_{X_z}(x)$$

and is constructed by integrating the conditional distribution of achievement of non-ZOC students with respect to the characteristics of ZOC students.

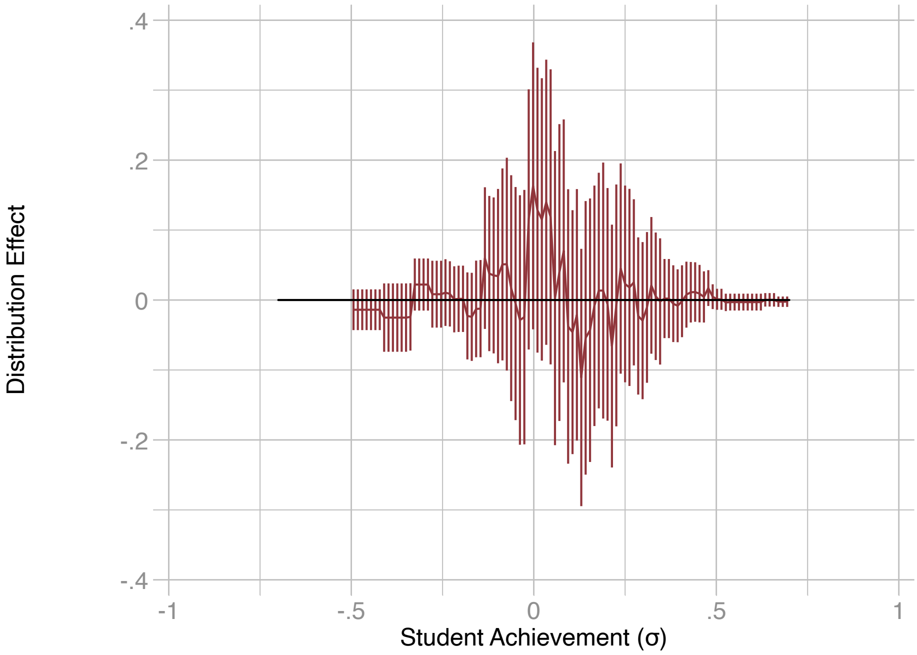
The counterfactual assignment comes from the fact that we can *integrate* one conditional distribution with respect to another group's characteristics and, in essence, assign each ZOC student to a corresponding location in the non-ZOC conditional achievement distribution based on her observable characteristics. Therefore, unconditional quantile treatment effects are constructed by inverting both the observed and estimated counterfactual CDFs at different quantiles and taking the difference.

Figure G.1: Empirical and Counterfactual Cumulative Distribution Function for Zones of Choice students in 2019



Notes: This figure reports the observed and counterfactual student-achievement distributions for Zones of Choice (ZOC) students in 2019. The counterfactual distribution is calculated by integrating the estimated non-ZOC conditional achievement distribution with respect to ZOC student characteristics at each point of support as discussed in Chernozhukov et al. (2013, 2020).

Figure G.2: School VA Pre-Intervention Distribution Effects



Notes: This reports point estimates from pre-intervention difference-in-differences estimates from regressions of school-level indicators $\mathbf{1}\{\alpha_{jt} <= y\}$ on year indicators, school indicators, school-level student incoming achievement, pre, and post indicators interacted with Zones of Choice (ZOC) indicators for one hundred equally spaced points y between -0.7 and 0.7. Standard errors are clustered at the school level, and 95 percent confidence intervals are shown by shaded regions.

H Model Estimates

H.1 Achievement Model Estimates

To estimate the parameters of the decomposition, we rely on a selection-on-observables assumption and estimate Equation 4 via OLS. Table H.1 reports summary statistics for the school-specific returns β_j. We find substantial heterogeneity in these school-specific returns. While a Black student at the average school performs roughly 0.2σ worse than a white student, the standard deviation of the Black-white achievement gap across ZOC schools is 0.34σ and 0.6σ at other schools. We do not find meaningful mean differences between ZOC and non-ZOC schools in the β_j. The standard deviations of β_j are larger among non-ZOC schools, which may be due to these schools representing a larger share and more heterogeneous set of LAUSD students. It is plausible that the β_j also changed in response to the policy, so we estimated a version of the model where β_j are different in the pre- and post-periods. Appendix Table H.2 reports the estimates, but we do not find evidence that there were meaningful changes induced by the policy for most characteristics.

Table H.1: Summary Statistics for School-Specific Returns to Student Characteristics

	ZOC		Non-ZOC		
	Mean	SD	Mean	SD	Difference
	(1)	(2)	(3)	(4)	(5)
Female	.078	.044	.049	.14	.029*
	(.044)	(.005)	(.014)	(.036)	(.015)
Black	-.208	.34	-.18	.599	-.029
	(.34)	(.062)	(.061)	(.096)	(.078)
Hispanic	-.077	.219	-.075	.487	-.002
	(.219)	(.063)	(.049)	(.113)	(.058)
English Learner	-.682	.15	-.461	.323	-.221***
	(.15)	(.012)	(.033)	(.047)	(.039)
Poverty	.045	.081	.011	.169	.034*
	(.081)	(.009)	(.017)	(.026)	(.021)
Migrant	-.008	.083	-.015	.289	.007
	(.083)	(.009)	(.029)	(.074)	(.032)
Parents College +	.009	.121	-.008	.481	.017
	(.121)	(.025)	(.049)	(.136)	(.052)
Spanish Spoken at Home	.082	.095	.002	.172	.079***
	(.095)	(.012)	(.018)	(.021)	(.022)
Lagged ELA Scores	.61	.058	.629	.367	-.02
	(.058)	(.006)	(.037)	(.144)	(.038)
Lagged Math Scores	.134	.041	.052	.371	.081**
	(.041)	(.006)	(.038)	(.142)	(.038)
8th-Grade Suspensions	-.05	.064	-.043	.075	-.007
	(.064)	(.009)	(.008)	(.007)	(.012)

Notes: This table reports estimated means and standard deviations of school-specific returns β_j. Estimates come from OLS regressions that school 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. Standard errors are reported in parentheses.

Table H.2: Summary Statistics of Time-Varying Match Effects

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

H.2 Utility Model Estimates

Table H.3: Utility Model Estimates

		Standard Deviations		
	Mean	Total SD	Within	Between
School Mean Utility	-	.505	.21	.459
Distance Costs				
First Cohort	-.082			
	(.036)			
Second Cohort	-.229			
	(.025)			
Third Cohort	-.092			
	(.016)			
Fourth Cohort	-.077			
	(.015)			
Fifth Cohort	-.1			
	(.017)			
Number of Schools		56		

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 not allowed to vary across cells, so we report parameter estimates for each cohort with robust standard errors in parentheses.

I Lottery Appendix

In this section, we present additional details related to the lottery analysis presented in Section 6. We first discuss balance and differential-attrition estimates, which are core elements of the validity of the lottery analysis. Next, we discuss the procedure we adopted to test for bias in the value-added estimates we use throughout our analysis.

I.1 Balance and Attrition

Centralized assignment mechanisms—like those employed within ZOC—randomly allocate seats to oversubscribed schools, implying that baseline characteristics of students in the lottery sample should not differ by offer status. Table I.1 checks this by comparing lottery winners and losers across numerous baseline characteristics. Column 1 and Column 2 report group averages for students with and without lottery offers, respectively, and Column 3 reports the difference. Across eleven baseline characteristics, we do not find evidence that lottery winners differ from lottery losers, and we fail to reject the null hypothesis that all differences are jointly zero.

Another threat to internal validity is nonrandom attrition. For example, if high-achieving lottery losers are more likely to enroll in local charter schools—and thus exit the sample—than lower-achieving lottery losers, then the estimates will be biased due to nonrandom attrition. We can check for this type of sample-selection bias by estimating differential follow-up rates between lottery winners and lottery losers. If differences in follow-up rates are small, then sample-selection bias should also be minimal.

Table I.2 reports follow-up rates for each lottery cohort, along with attrition differentials between lottery winners and lottery losers. We observe approximately three-fourths of all students in our lottery sample across years in eleventh grade. For the most part, attrition differentials are small and insignificant; the 2015 cohort is the lone cohort for which this is not the case. The main conclusions are robust to dropping this cohort from the analysis, and thus there is no immediate concern that the lottery estimates are biased by post-lottery selective attrition.

Table I.1: Lottery Balance

	Not Offered	Offered	Difference
	(1)	(2)	(3)
ELA Scores	-.026	-.048	-.022
			(.031)
Math Scores	-.038	-.038	0
			(.037)
Suspensions	.082	.079	-.004
			(.013)
Black	.029	.027	-.002
			(.003)
Hispanic	.886	.886	.001
			(.008)
White	.013	.014	.002
			(.003)
English Learner	.13	.136	.006
			(.01)
Migrant	.137	.146	.009
			(.01)
Spanish at Home	.743	.749	.006
			(.012)
Poverty	.863	.873	.011
			(.011)
College	.028	.023	-.005
			(.005)
P-value			.909

Notes: This table compares characteristics of students receiving offers to their most preferred school to students not receiving offers. Column (1) reports mean characteristics for applicants not offered a seat, while Column (2) reports mean characteristics for applicants offered a seat. Column (3) reports the mean difference, coming from regressions that control for lottery indicators. The last row shows p-values from tests that all differences are jointly equal to zero. Standard errors are in parentheses and clustered at the lottery level.

Table I.2: Attrition Rates by Cohort

	Follow-Up Rates			Attrition Differential	
	Any Score	Math	ELA	Math	ELA
	(1)	(2)	(3)	(4)	(5)
2013	.69	.68	.67	.009	.017
				(.027)	(.028)
2014	.72	.71	.72	.01	.017
				(.023)	(.022)
2015	.71	.70	.70	.04	.045
				(.017)	(.019)
2016	.74	.74	.74	.004	.008
				(.026)	(.024)
2017	.74	.73	.74	-.032	-.029
				(.02)	(.02)
All Cohorts	.74	.73	.74	.003	.006
				(.02)	(.008)

Notes: This table reports follow-up rates and attrition differentials for each lottery cohort. Column (1) reports the share of lottery applicants with test scores in eleventh grade. Columns (2) and (3) report subject-specific shares of applicants with math and ELA scores in eleventh grade, respectively. Columns (4) and (5) report subject-specific attrition differentials between lottery applicants offered seats at their most preferred school and those not offered seats. Attrition differentials are coefficients from regressions of a follow-up indicator on an offer indicator, controlling for sex, race, and other demographic characteristics reported in Table I.1. Standard errors, reported in parentheses, are clustered at the lottery level.

Figure I.1: Reduced-Form Effects on First Stage by Lottery

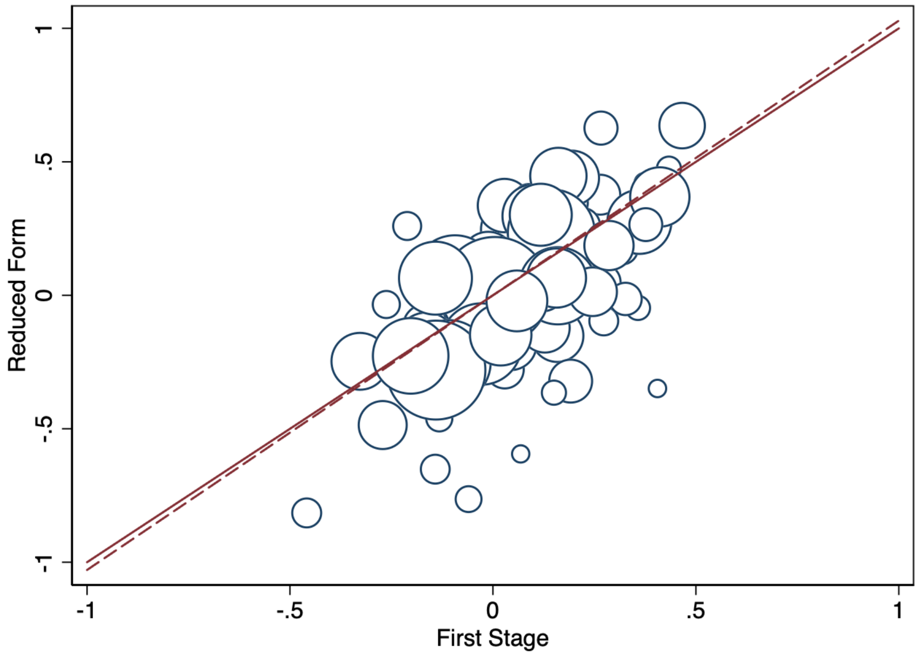


Table I.4: Complier Characteristics by Cohort

	2013	2014	2015	2016	2017	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
English Learner	.223	.145	.181	.094	.132	[.184]
	(.043)	(.012)	(.024)	(.015)	(.032)	
Female	.497	.513	.477	.478	.512	[.853]
	(.033)	(.026)	(.034)	(.044)	(.045)	
Poverty	.837	.8	.950	.945	.97	[0]
	(.014)	(.064)	(.012)	(.021)	(.012)	
Hispanic	.974	.967	.934	.9440	.940	[.292]
	(.012)	(.014)	(.021)	(.019)	(.024)	
Black	.012	.015	.022	.006	.035	[.543]
	(.005)	(.01)	(.013)	(.005)	(.016)	
White	.003	.007	.02	.015	.008	[.535]
	(.003)	(.006)	(.014)	(.008)	(.004)	
Migrant	.15	.105	.161	.106	.09	[.016]
	(.027)	(.014)	(.017)	(.017)	(.019)	
ZOC Fallback (among Control Compliers)	.825	.893	.957	.924	.951	[.353]
	(.084)	(.098)	(.035)	(.065)	(.023)	

Table I.3: Oversubscribed Schools

School Name	Zone	Number of Lotteries
Legacy High School - STEAM	Bell	3.0
Legacy High School - VAPA	Bell	3.0
Maywood Academy	Bell	2.0
Bell High School	Bell	3.0
Belmont High School	Belmont	1.0
Miguel Contreras Learning Center	Belmont	5.0
Bernstein High School	Bernstein	1.0
Boyle Heights High School	Boyle Heights	2.0
Mendez High School	Boyle Heights	4.0
Roosevelt High School	Boyle Heights	4.0
Carson High School	Carson	3.0
Garfield High School	Eastside	2.0
Torres High School	Eastside	2.0
Solis Learning Academy	Eastside	1.0
Rivera - STEAM Academy	Fremont	3.0
Rivera - Performing Arts School	Fremont	4.0
Rivera - Communications and Technology School	Fremont	4.0
Dymally High School	Fremont	3.0
RIVERA LC PUB SRV	Fremont	3.0
Hawkins High School	Hawkins	4.0
Marquez High School - HPIAM	Huntington Park	4.0
Marquez High School - LIBRA	Huntington Park	5.0
Marquez High School - SJ	Huntington Park	4.0
Huntington Park High School	Huntington Park	3.0
Angelou High School	Jefferson	2.0
Jefferson High School	Jefferson	3.0
Santee Education Complex	Jefferson	2.0
Nava College Preparatory	Jefferson	2.0
Jordan High School	Jordan	2.0
Narbonne High School	Narbonne	2.0
Cesar Chavez Learning Academies	North Valley	4.0
San Fernando High School	North Valley	4.0
Sylmar High School Complex	North Valley	3.0
Lincoln High School	Northeast	1.0
RFK - School of Global Leadership	RFK	2.0
RFK - Visual Arts & Humanities	RFK	1.0
RFK - Los Angeles School of the Arts	RFK	4.0
RFK - UCLA Community School	RFK	5.0
RFK - New Open World Academy	RFK	3.0
International Studies Center	South Gate	3.0
South East High School	South Gate	1.0
South Gate High School	South Gate	1.0

Notes: This table lists all the schools appearing in the lottery sample and the number of lotteries.

J Changes in Teacher–Student Racial Match

We focus on changes in the classroom-level student–teacher racial match. We focus on race because there is a growing body of evidence suggesting exposure to same-race teachers can improve both short- and long-run outcomes of underrepresented racial minorities, which 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.[31] We track the number of same-race teachers students are exposed to and study ZOC impacts on racial-match propensity. Figure J.1 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 ninth and eleventh grades.[32] 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.

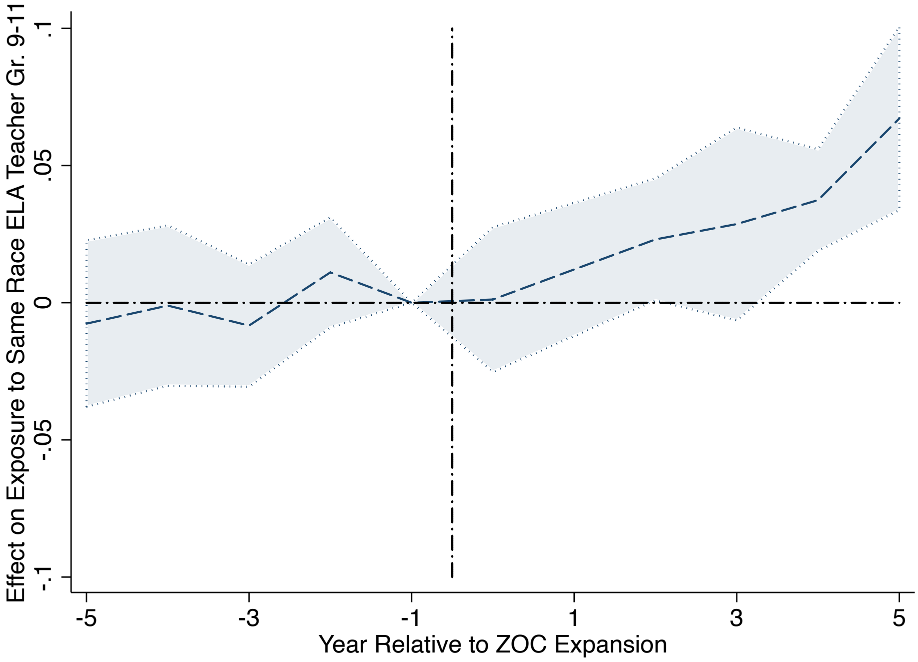
Impacts of same-race teachers have been shown to produce both short- and long-run improvements in outcomes for underrepresented racial minorities (Dee, 2004, Fairlie et al., 2014, Gershenson et al., 2018). In particular, Gershenson et al. (2018) find that Black students randomly assigned a Black teacher 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. We find some suggestive evidence of this and discuss it in Appendix K.

We emphasize that we cannot decisively conclude that either changes in exposure to same-race teachers or suggested changes in tracking practices contributed to the ZOC achievement and college-enrollment effects, but these findings do reveal evidence of a differential change in how ZOC schools operated during the period. These findings suggest that other schooling practices may have also changed among ZOC schools.

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

[32] Estimates using the share of same-race ELA teachers students are exposed to result in qualitatively similar estimates, albeit noisier ones.

Figure J.1: 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 Zones of Choice (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 nine and eleven. Standard errors are double clustered at the school-by-year level, and 95 percent confidence intervals are displayed in the shaded regions.

K Changes in Tracking Practices and Teacher-Hiring Practices

To explore this possibility, 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 not only captures how much a student is segregated based on the peers they share classes with, but also captures 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—conditional on a tracking scheme, 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).[33] Figure K.1 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 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

$$\begin{aligned}\widehat{ASI}_{it} = \mu_{j(i)t} &+ \beta_A' Post_t \times ZOC_{j(i)} \times f(A_{it}^8) \\ &+ \beta_B' Pre_t \times ZOC_{j(i)} \times f(A_{it}^8) \\ &+ \gamma_1' Post_t \times f(A_{it}^8) + \gamma_2' ZOC_{j(i)} \times f(A_{it}^8) + f(A_{it}^8) + u_{it},\end{aligned}$$

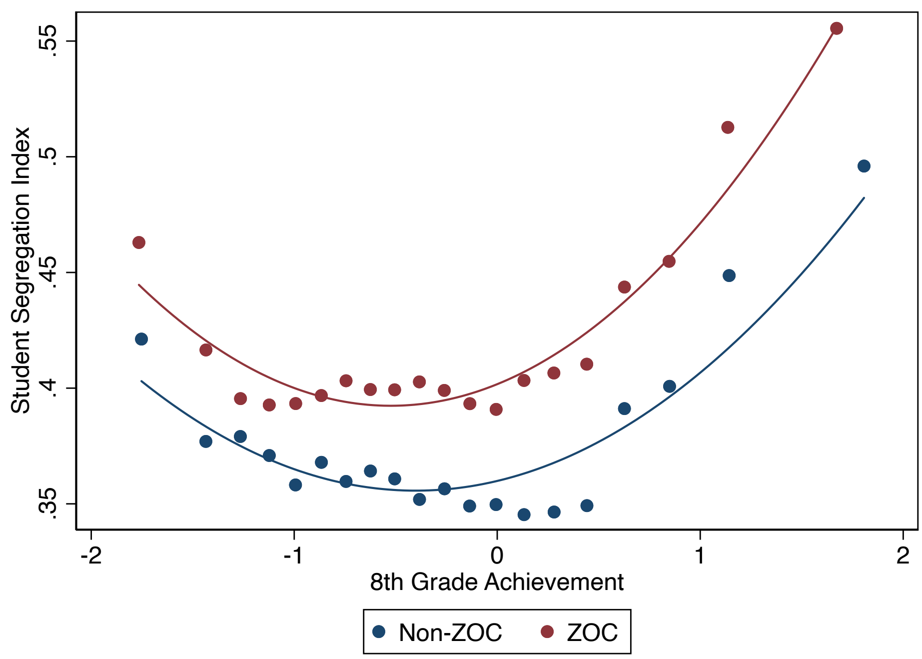
where $f(A_{it}^8)$ is a polynomial in students' incoming achievement and μ_{jt} are school-by-year effects indicating this model is identified from changes in the *within-school-cohort* segregation gap between students with incoming achievement A_{it} and those with $A_{it} = 0$. Therefore, $\beta_A' \times f(A_{it}^8)$ 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 K.2 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 not detect any differential

[33] Appendix L provides estimation details and statistics. We also provide results using classroom incoming-achievement standard deviations and school-level between-classroom shares of variance.

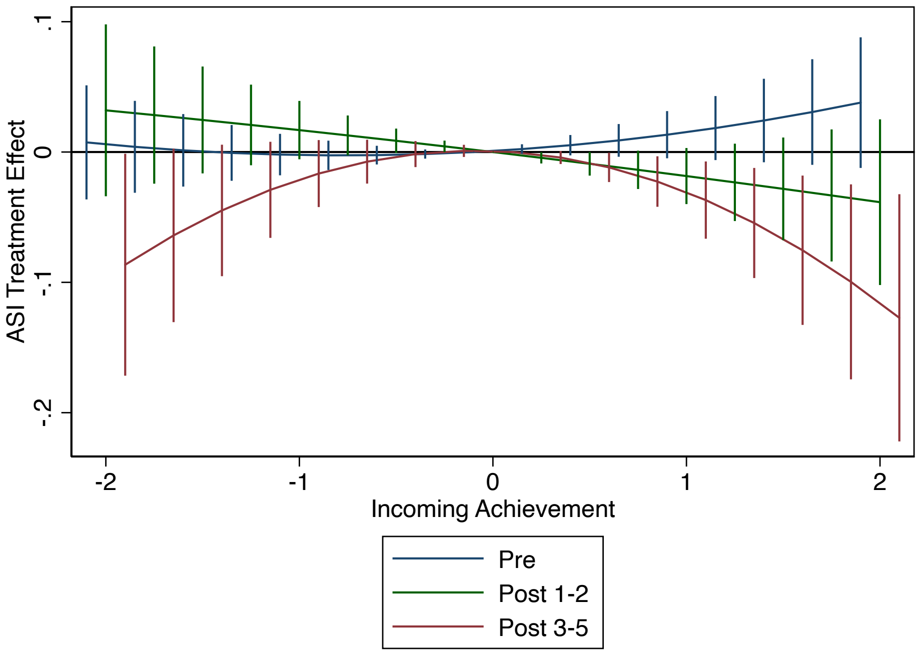
changes in within-school segregation gaps, but we do observe them in the later post-periods. In particular, we find that segregation gaps decreased for both high- and low-achieving students, suggesting ninth-grade classrooms became more integrated in terms of students' incoming achievement. The literature 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 do not speak to what the exact changes in tracking practices were, but they do 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 K.1: Estimated ASI Averages by Incoming Achievement



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 Zones of Choice (ZOC) dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the school level.

Figure K.2: ASI Treatment Effects by Incoming Achievement



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 Zones of Choice dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the school level.

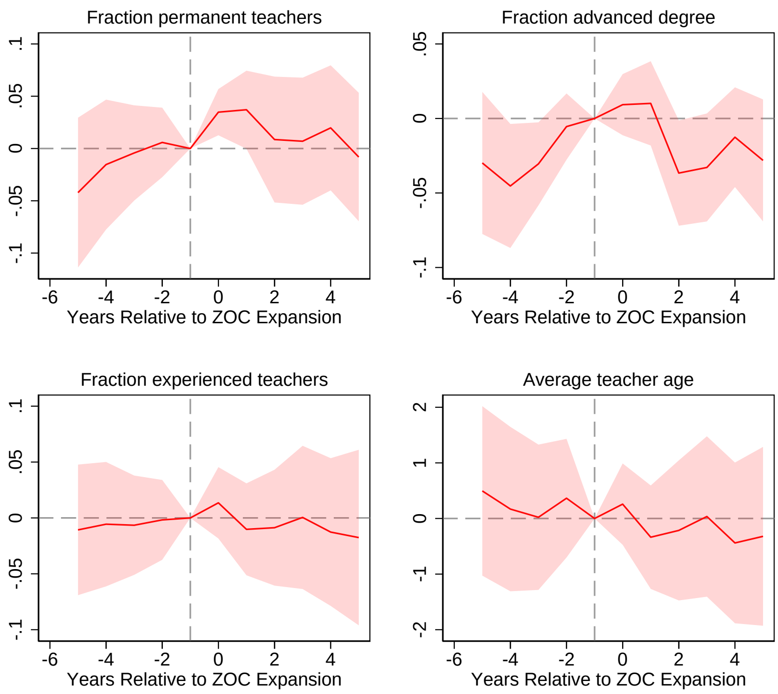
K.1 Changes in School Inputs

Variation in schooling inputs and practices explains variation in treatment effects in other settings (Angrist et al., 2013, Walters, 2015). In our setting, schooling practices—such as the no-excuses approach—are not too variable across schools, but schools do have some leverage to alter the composition of inputs, such as course offerings and teacher characteristics and quality. Therefore, we assess the extent to which inputs changed between ZOC and non-ZOC schools and also directly correlate treatment effects with changes in schooling inputs.

We don't find evidence of differences in the changes of teacher characteristics between ZOC schools and non-ZOC schools, as documented in Figure K.3. Similarly, Figure K.4 shows that both the quantity and quality of teachers did not change between the two sectors.[34] This provides evidence of the lack of changes in schooling inputs across both sectors, but within-zone changes in schooling inputs could still explain variation in treatment effects.

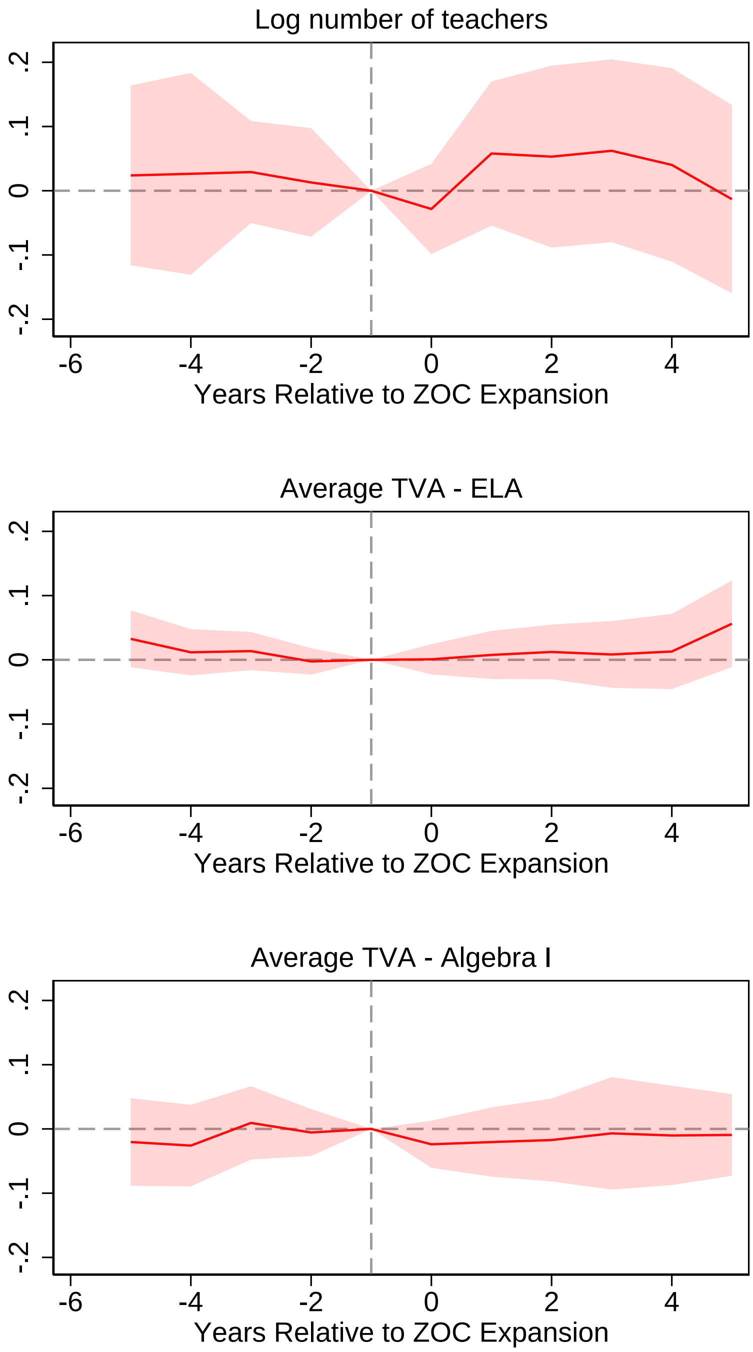
[34]We estimate within-school teacher value added in the pre-period and track changes in teacher quality with respect to the baseline-estimate teacher value added.

Figure K.3: 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 Zones of Choice (ZOC) dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the school level.

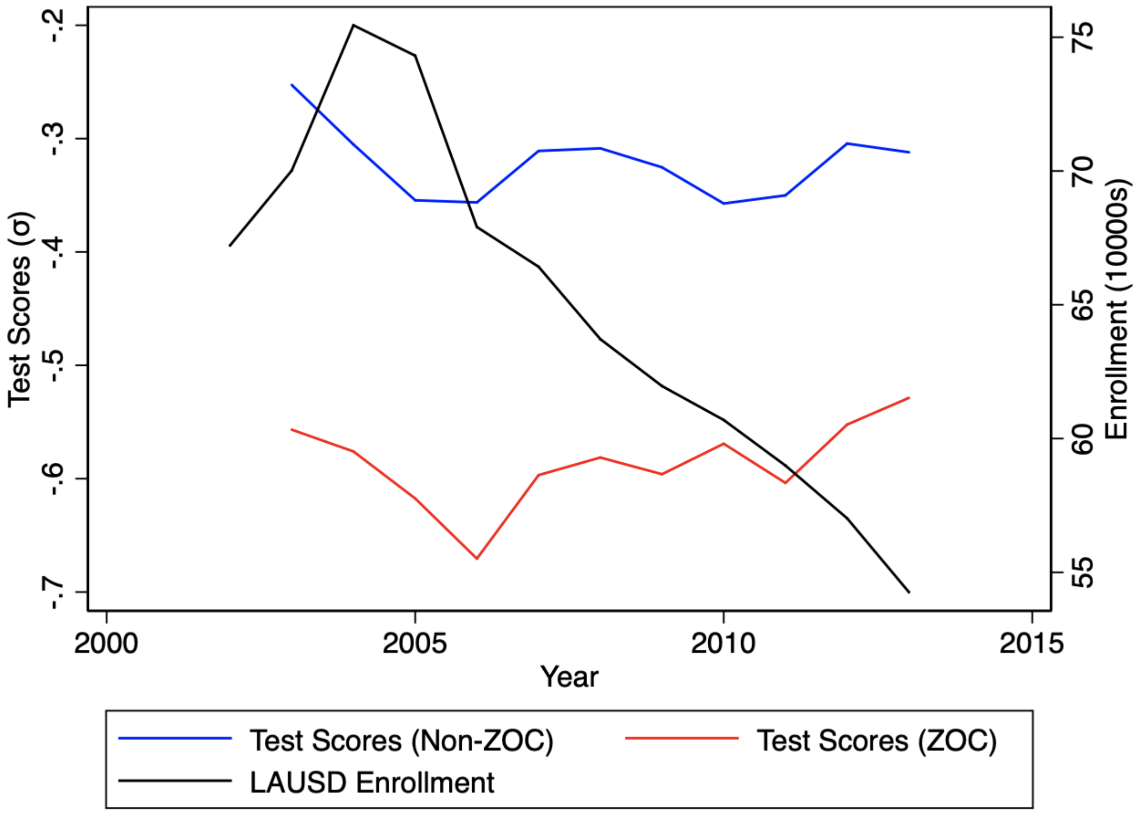
Figure K.4: Teacher-Quantity and Teacher-Quality Event Studies



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 Zones of Choice (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.

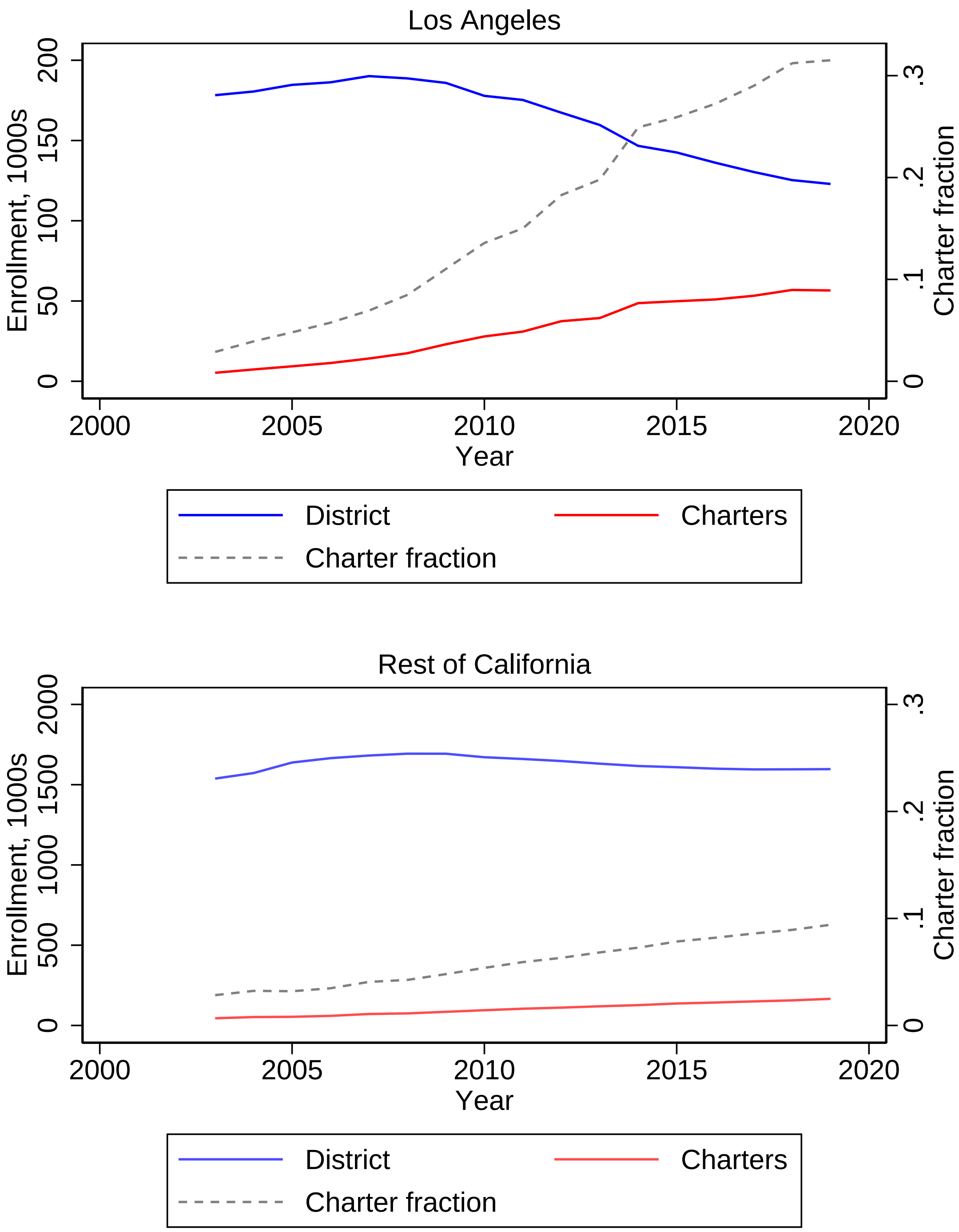
L Additional Empirical Results

Figure L.1: Los Angeles Unified Schoo District: 2002–13



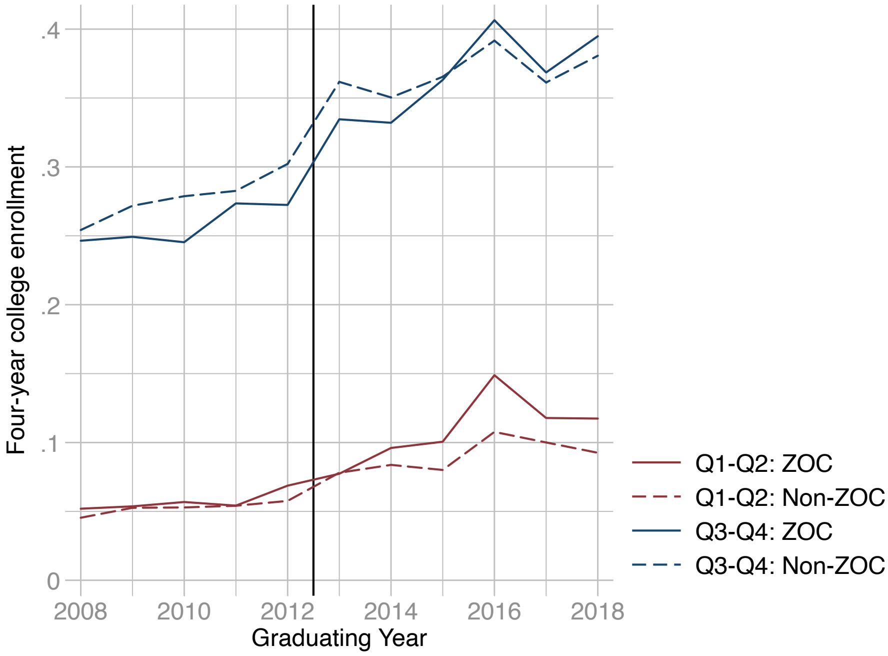
Notes: Enrollment numbers come from administrative data provided by Los Angeles Unified School District (LAUSD). The California Department of Education provides California Standards Test statewide means and standard deviations, which we use to standardize test scores in this figure. Test scores are ninth-grade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

Figure L.2: Los Angeles and California Enrollment



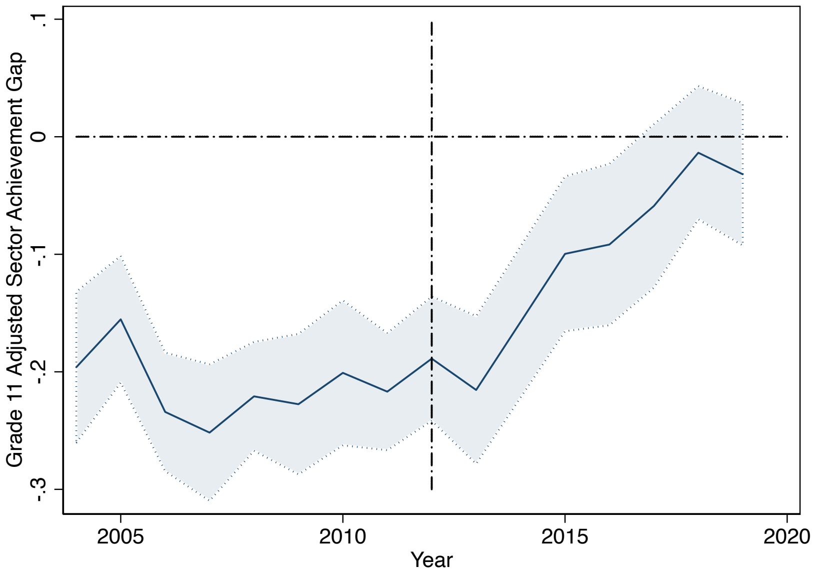
Notes: This figure shows enrollment in thousands for grades nine through twelve, separately for district and charter schools. Enrollment data are from the California Department of Education.

Figure L.3: Four-Year-College-Enrollment Rates by Predicted Quartile Group



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

Figure L.4: Eleventh-Grade Zones of Choice Achievement Gaps



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

Figure L.5: Event-study not restricting control group schools to comparable schools