Neighborhood School Choice and Competition in Public Schools: Evidence from Los Angeles' Zones of Choice *

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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 the ZOC program 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 the ZOC program boosted student outcomes markedly, closing achievement and college enrollment gaps between ZOC neighborhoods and the rest of the district. These gains are explained by general improvements in school effectiveness rather than changes in student match quality, and school-specific 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 were larger for schools exposed to more competition, supporting the notion that competition is a key channel driving the impacts of ZOC. Demand estimates derived from rank-ordered preference lists suggest families place substantial weight on schools' academic quality, providing schools competitive incentives to improve their effectiveness. An analysis using randomized admission lotteries shows that the treatment effects of admission to preferred schools declined after the introduction of ZOC, a pattern that is explained by the relative improvements of less-preferred schools. Our findings demonstrate the potential for public school choice to improve student outcomes while also underscoring the importance of studying market-level impacts when evaluating school choice programs.

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Students in the United States have traditionally been assigned to schools by attendance zone boundaries. Critics of this local monopoly schooling model argue that it provides weak incentives for schools to improve quality and may not operate in students' best interests. These criticisms have paved the way for a growing number of reforms designed to expand school choice. These reforms promise to increase access to high-performing schools, expand the scope 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.

An extensive literature has taken a segmented approach in studying school choice reforms. One body of research studies the impacts of access to specific types of schools on student outcomes, such as charter schools and exam schools (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 large literature spanning multiple countries has found mixed effects of competition (Allende, 2019a, 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). Although substantively important, student-school match effects have received relatively less attention but remain an important channel that could enhance allocative efficiency (Hoxby, 2003). Few studies are able to jointly study all of these factors in a single setting.

This paper studies the Zones of Choice (ZOC), an initiative of the Los Angeles Unified School District (LAUSD) that created small local markets of high schools of varying sizes in some neighborhoods, but left traditional attendance zone boundaries in place throughout the rest of the district. The initiative established sixteen zones primarily in relatively disadvantaged parts of LAUSD. These zones covered roughly 30-40 percent of all high school students in LAUSD, while the remaining LAUSD students remained 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 provide a comprehensive analysis of supply- and demand-side responses to ZOC to determine how these changes in market structure altered the distribution of school quality and affected student outcomes.

Our empirical analysis is motivated by a stylized model of school choice and competition, in which families choose schools based on proximity, quality, and idiosyncratic tastes. On the supply-side, we assume principals are rewarded for larger market shares but must exert effort to improve school quality. The ZOC program is modeled as an expansion of households' choice set in this stylized setting. The model gives rise to a simple statistic that describes households' expected welfare gain as a result of this choice set expansion, which we label the option value gain

(OVG). The distribution of OVGs across students also governs schools' incentives to increase quality in response to competition. Our theoretical framework predicts that the introduction of ZOC will improve school quality, and that these improvements will be concentrated among schools exposed to more competition as measured by OVG.

We empirically assess these predictions using a matched difference-in-differences design that compares changes in outcomes for ZOC schools to corresponding changes for an observably similar set of control schools elsewhere in the district. To estimate the impacts of ZOC on overall school quality, we decompose treatment effects into treatment effects on student-school match quality and treatment effects on school value-added. Estimates of quantile treatment effects on school value-added allow us to assess if the lowest-performing schools improved more, as predicted by the model. We then use students' rank-ordered choice lists to calculate an empirical version of OVG. Looking at treatment effect heterogeneity 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 the ZOC program 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 0.2σ 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 that is driven by increases in enrollment at California State University (CSU) campuses. These impacts are mostly due to improvements in school effectiveness and are large enough to close substantial initial gaps in outcomes between ZOC and non-ZOC areas.

A distributional analysis shows that student improvements appear throughout the middle and lower part 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 values of OVG. This suggests that the competitive incentives generated by the ZOC program are a key mechanism mediating its effects on school performance.

ZOC effects may also arise from students enrolling in popular and higher quality schools not available to them before the program. These effects contrast with the market-level effects discussed above that capture average improvements among all ZOC schools. We use 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 most-preferred school. We show that the impacts of accessing popular schools shrank as differences between most-preferred and fallback schools narrowed due to overall improvements of ZOC schools, as captured by the market-level impacts. Importantly, the analysis using randomized admissions lotteries demonstrates that the most salient benefits of the program were due to overall improvements of ZOC schools as opposed to re-allocation benefits. These findings underscore the importance of market-level effects when evaluating school choice programs.

Estimates of demand derived from rank-ordered lists help explain this paper's findings. We find that parents place a relatively higher weight on school effectiveness as opposed to other school characteristics, including a school's peer composition. This is a stark contrast to other settings (e.g., Abdulkadiroğlu et al. (2020) and Rothstein (2006)), and further contrasts evidence suggesting that lower-income families, like ZOC families, are less sensitive to school quality (Burgess et al., 2015, Hastings et al., 2005). ZOC parents' choices reflecting stronger preferences for school effectiveness, as opposed to student composition, are consistent with the improvements in school quality we find.

We argue that particular features of the ZOC program may further explain why our findings contrast with many previous studies. The ZOC program incorporated relatively personalized interactions between ZOC administrators and parents, making it easier for ZOC parents to acquire information (Page et al., 2020). In particular, ZOC administrator-led information sessions provide a potentially rich setting to learn about differences in school quality within zones. Moreover, because choice was within zones rather than district wide, ZOC parents faced manageable choice sets that may have helped them avoid choice overload issues present in other school choice settings (Corcoran et al., 2018). These features combined to create a setting where acquiring adequate information about schools was 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.

Related Literature

This paper contributes to an extensive literature surrounding school choice. First, we add to a list of studies estimating market-level effects of school choice reforms. An earlier strand of literature relied on cross-district or cross-municipality comparisons to estimate market-level effects (Hoxby, 2000, 2003, Hsieh and Urquiola, 2006, Rothstein, 2007), and reached mixed conclusions. Other empirical papers, typically in settings with different education markets from the United States, take a more model-driven approach and estimate general equilibrium models of school competition; they find positive impacts of competition on achievement (Allende, 2019a,b, Neilson, 2013). We focus on a natural experiment in Los Angeles, that allows us to transparently identify treatment and control groups and study competitive effects and further differentiate between effects on school quality and match effects.

The literature has grappled with measuring competition in various ways. Other researchers have leveraged market-specific heterogeneity to study competitive effects. Figlio and Hart (2014) study competitive effects when exposure to competition varies, Gilraine et al. (2019) consider how competitive effects vary by the entry of horizontally differentiated schools and non-horizontally differentiated schools, and Card et al. (2010) considers the salience of demand-side pressures captured by the composition of students. All three find evidence of modest impacts on achievement from competition. In our setting we have policy-induced variation in competition that we use to further assess competitive effects. In that sense, our contributions to the school competition literature is twofold: we use a natural experiment that transparently identifies treatment and control groups, but we further leverage policy-specific variation

to measure competition avoiding degrees of freedom concerns of past literature.

We complement our supply-side program evaluation with a focus on parents' demand, contributing to a growing literature studying parental demand, and in particular, the relationship between preferences, a school's peer composition, and a school's quality (Abdulkadiroğlu et al., 2020, Beuermann et al., 2018, Rothstein, 2006). Our demand estimates also add to a growing list of papers using preference data from centralized assignment mechanisms to investigate school demand (Agarwal and Somaini, 2018, 2019, Beuermann et al., 2018, Kapor et al., 2020). While estimating preferences alone is the focus of these previous papers, our setting considers how the demand-side interacts with a supply-side response.

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

The rest of this paper is organized as follows. In Section 1 we outline the features of the policy and the 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 occurring within schools and provides a discussion of key differences between ZOC and other school choice reforms; and lastly, Section 8 concludes.

1 Institutional Details

1.1 A Brief History of Zones of Choice

The ZOC program is an initiative of the Los Angeles Unified School District (LAUSD), the second-largest school district in the United States. In the years preceding the program expansion we study, the school district suffered from stagnant academic growth and began experiencing enrollment losses due to charter school enrollment growth (see Appendix Figure L.1 and Appendix Figure L.2). The mixture of stagnant academic growth and charter competition sparked policies altering the organization of schools within the district. These policies included the largest school construction program U.S. history (Lafortune et al., 2018), the expansion of pilot schools, conversion charter schools, and pilot-like schooling models (Kearns et al., 2020), and a novel choice zone known at the time as the Belmont Zone of Choice.

The ZOC program began with the Belmont Zone of Choice, located in the Pico Union area of downtown Los Angeles. This local program was a community-based response that 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 organically develop the Belmont Zone of Choice by creating an informal enrollment and assignment system for eligible residents. The Belmont pilot started in 2008 and continued informally for five years.

The continuing exodus of students from the district and increasing community pressure to access better schools led the school board to consider removing attendance zone boundaries (see *Resolution to Examine Increasing Choice and Removing Boundaries from Neighborhood Schools)* and 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 pursued replicating the model in other amenable neighborhoods. By July 2012, a Zones of Choice office was established along with 16 zones. Figure 1 shows that the program mostly covered disadvantaged students in Los Angeles.

In contrast to the Belmont ZOC, the new zones were organized and administered from a central district office and used formal assignment and enrollment mechanisms. The new zones also had ambitious goals that addressed core tenets of school choice policies—access to more effective schools, improvements 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 the Zones of Choice program:

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..."*

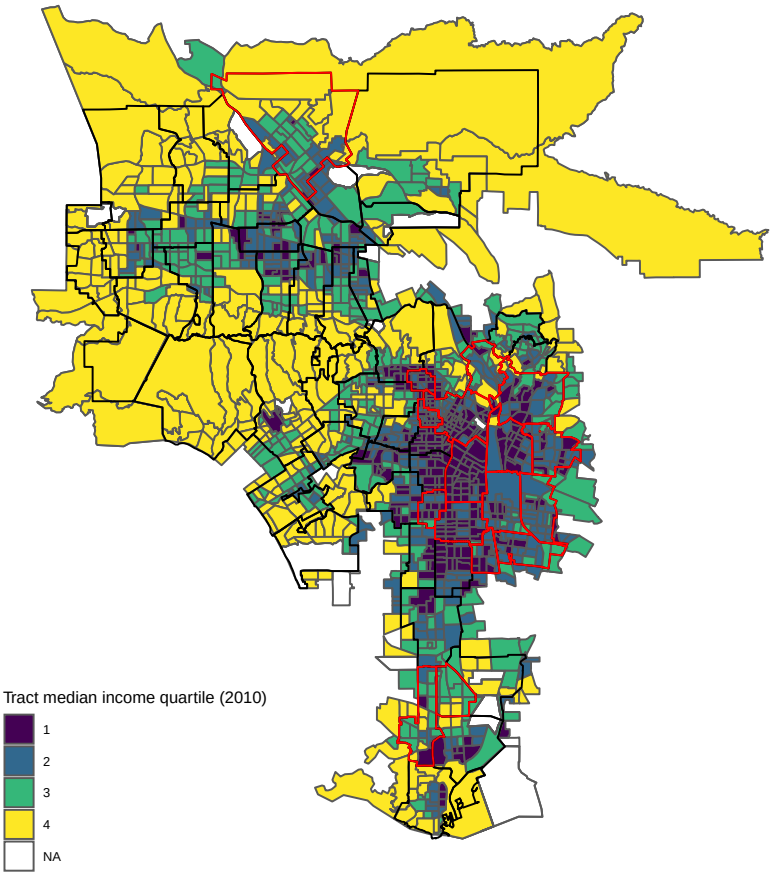
3. **Parental involvement** - *"Research validates that parental involvement in public schools is a key factor in producing measurable gains in student academic success, closing the achievement gap..."*

1.2 Program Features

The ZOC program expands students' high school options by combining catchment areas into zones of choice and, in some cases, pulling in schools with ambiguous assignment schemes into zones. The program is centrally run by a team of administrators who focus only on Zones of Choice activities that run on a yearly cycle. The most time-extensive is the yearly application cycle where applications containing rank-ordered preferences from current eighth-grade parents are collected. Applications are requested from all families residing within ZOC boundaries, with rising students eligible for the program in the following year. Importantly, admission into any

school is not guaranteed, although certain priorities are given to students based on proximity, incumbency, and sibling status. Most rising ZOC students are enrolled in feeder middle schools that directly feed into ZOC high schools, mimicking neighborhood-based transitions between schools but allowing parents to exercise choice in the transition to high school.

Figure 1: Zones of Choice and 2010 Census Tract Income



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 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 schooling hours to get a chance to pitch their school to potential students. These events continue for roughly six weeks before 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 students additional priorities as some schools can in other school choice settings.

After receiving parental preferences, the school district determines assignments using a centralized algorithm, analogous to a Boston—immediate acceptance—mechanism. Schools that are oversubscribed determine seats using randomly assigned lottery numbers. Families can appeal their assignment, with appeals addressed in the spring semester. About three-quarters of students residing in a zone attend a ZOC school. The most popular options for students who

opt out of ZOC are LAUSD magnet schools and out-of-district options, presumably charter or private schools.

2 Conceptual Framework

We begin with a stylized model for the status quo, neighborhood monopolies competing with a charter sector, and then introduce Zones of Choice, highlighting how the program altered school incentives and discuss the potential benefits.[1] We use j interchangeably to denote schools and neighborhoods, indicating there is one school per neighborhood. Let students indexed by i reside in neighborhood $j(i) \in \{1, \cdots, J\}$ that contains one school also indexed by j. Each school j operates as a monopoly over their neighborhood but faces competition from an outside option in the district indexed by 0.[2]

Students can enroll in either their neighborhood school $j(i)$ or the outside option. Student i's utility of 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 defined in the achievement model discussed in Section 4.2.1, d_{ij} is distance 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.

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

Lastly, we normalize charter school utility to zero. With a logit error structure for the unobserved preference heterogeneity, neighborhood-specific school market share is

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

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

[2] One motivation for starting with one-sided neighborhood monopoly competition with charter schools is a pre-ZOC equilibrium with heterogeneous quality. Another is that this formulation will also suggest that the introduction of ZOC will lead to decreases in the charter school market share, an important aim the district had in establishing the Zones of Choice program.

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

and the charter school share of all students in the district 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).[4] Principal 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.[5] Lastly, we assume that school quality is an increasing concave function of the level of effort e_j

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

Due to cross-neighborhood enrollment restrictions before the ZOC program, each principal sets school effectiveness α_j independently of other school district principals. Therefore, each principals set their 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

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

The ZOC program 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 $\mathcal{J}^+ = \mathcal{J} \cup 0$. Due to the spatial differentiation of schools and student heterogeneity, the value from each additional schooling option varies across

[4] 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 time period, 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 shares 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.

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

students.

We define a student's option value gain (OVG) as the difference in expected max utility under the new choice set $\mathcal{J}^+$ and 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*

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

and with iid Extreme Value Type I errors,

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

OVG is a measure a student's expected welfare gain measured in terms of distance, under the assumption that every option is equally accessible. Intuitively, a student with high OVG gains access to relatively popular schools and values them highly after netting out distance costs; these students are likely to access new schools. A household with low OVG either gains access to schools that are less popular than its local school, or cost factors make the new schools unattractive; in either case, these households are less willing to access their new schools.

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

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

where $OVG_{i0} = \frac{1}{\lambda}\Big(\ln(1 + e^{V_{ij(i)}}) - V_{ij(i)}\Big)$ is student i's fixed charter school option value gain, while OVG_i is the option value gain from expanding the choice set from J_i to $\mathcal{J}^+$. The P_{ij} are decreasing in OVG, indicating that students with high OVG_i gained access to more preferable schools and are more likely to enroll in schools other than their neighborhood school. We can also express school market shares in terms of student OVG

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

The introduction of ZOC also introduces a strategic effort game between principals in $\mathcal{J}$. Whereas principals $j \notin \mathcal{J}$ still independently maximizes their utility subject to the draw of students in their zones, principals $j \in \mathcal{J}$ choose a best response level of effort in anticipation of other principals' $j \in \mathcal{J}$ best responses. The following proposition demonstrates that there is

an equilibrium to the principal effort game ZOC introduces.

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

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

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

Proof. See Appendix A.1. □

2.1 Empirical Map

This framework, discussed more thoroughly in Appendix A, 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), indicating that schools exposed to more competition should improve to sustain their demand. Since ZOC schools start as local neighborhood monopolies, they had a majority of the neighborhood's market share.[6]

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

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

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

$$\Delta\alpha_j = 0$$

We use a difference-in-differences design comparing changes in achievement between ZOC students and other 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 school value-added and treatment effects on student-school match quality. Changes in match quality would imply students sorted more effectively into schools that suited their particular needs. On the other hand, competitive effects would imply differential changes in α_j. Differentiating between these two conditions is important empirically, as they provide additional information about the source of the gains.[7]

The next implication suggests that there is a reduction in the between-school quality gap within ZOCs, 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:*

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

[6] This assumption is necessary due to the assumptions on the unobserved preference heterogeneity and may not be necessary in models without that error structure.

[7] 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.

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 provides an empirical test 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 alone is an index that summarizes the expected welfare gain students receive from their expanded choice sets. But from the school's perspective, the relative popularity of other schools at the onset of the program—captured by OVG—will induce differential responses to the program. For example, among two identical schools the one exposed to more popular schools–and thus exposed to students with relatively higher OVG—will experience larger improvements in their quality. These observations allows us to interpret OVG as an index for competition. We leverage student- and school-level variation in OVG to construct empirical tests for the presence of competitive effects. [8]

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 Zones of Choice data provided by the Zones of Choice office, consisting of rank-ordered preference submissions from all applicants and centralized assignments between 2013 and 2020. Lastly, 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.

The market-level sample covers 2008-2019. To construct the market-level sample, we select all high school students that appear in a LAUSD high school in eleventh grade. We focus on eleventh grade because of the availability of test scores throughout our sample period.[9] Column 1 and Column 2 of Table 1 report mean characteristics for ZOC and non-ZOC cohorts. ZOC students enter high school performing approximately 20-22 percent of a standard deviation more poorly than non-ZOC students in both ELA and math.[10] Most ZOC students are Hispanic, roughly 88 percent or 20 percentage points higher than non-ZOC students. ZOC students are

[8] 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 $\sigma_\varepsilon^2 \to \infty$, then $OVG_i \approx OVG = \frac{\ln|\mathcal{J}_z|}{\lambda}$ for all i, so OVG is closely approximated by the log number of options and differences in school quality or distance 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 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.

[9] A potential concern is differential attrition rates out of the sample that could introduce bias in our analysis. In Appendix Figure E.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.

[10] text

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.

To address the unbalanced nature of the two groups, we created a matched market-level sample. We match each school to a non-Zone of Choice school in the same poverty share and Hispanic share deciles, breaking ties with a propensity score discussed in Appendix C. We report matched non-ZOC mean characteristics in Column 4 of Table 1. The matching strategy effectively balances most covariates, except for achievement, where an achievement gap of 15-16 percent of a standard deviation remains as students enter high school. This achievement gap serves as a benchmark for our market-level estimates.

The lottery sample restricts to students applying to oversubscribed schools within each zone. Column 6 of Table 1 reports lottery sample characteristics. We find that students with stronger preferences for popular schools tend to have higher incoming achievement entering high school. Otherwise, the typical student in the lottery sample is mostly similar to other Zones of Choice students. Appendix H provides additional details pertaining to the lottery sample.

4 Empirical Analysis

4.1 Achievement and College Enrollment Effects

We use a matched difference-in-differences strategy to estimate market-level effects, comparing changes in outcomes between ZOC students and students enrolled at other comparable schools. First, we match each ZOC school to a school in the same poverty and Hispanic share ventiles and break ties using a propensity score estimated in an earlier step (Arnold, 2019, **?**).[11] For a student outcome Y_i, such as achievement or four-year college enrollment, we

[11] Propensity scores are estimated using cross-sectional data of schools the year before the program expansion. Propensity scores come from logistic regressions of ZOC indicators on school average ELA and Math scores, racial, sex, and SES shares. Appendix C discusses the matching strategy and results in further detail.

Table 1: Descriptive Statistics for LAUSD Eighth Graders, 2013-2019

	(1) ZOC	(2) Non-ZOC	(3) Difference	(4) Matched Non-ZOC	(5) Diference	(6) Lottery Sample
8th Grade ELA Scores	-.053	.162	-.215***	.094	-.148**	.006
			(.049)		(.073)	
8th Grade Math Scores	-.037	.164	-.202***	.123	-.16**	.031
			(.047)		(.072)	
Black Share	.041	.106	-.065***	.058	-.017	.017
			(.022)		(.019)	
Hispanic	.877	.678	.2***	.803	.075	.901
			(.042)		(.047)	
White	.018	.111	-.092***	.061	-.042**	.011
			(.017)		(.017)	
English Learner	.102	.076	.026**	.095	.007	.065
			(.011)		(.014)	
Special Education	.033	.03	.003	.03	.003	.044
			(.002)		(.003)	
Female	.506	.504	.002	.504	.002	.501
			(.01)		(.012)	
Migrant	.154	.16	-.006	.174	-.02	.141
			(.012)		(.014)	
Spanish at home	.739	.552	.187***	.691	.048	.778
			(.044)		(.048)	
Poverty	.852	.775	.076***	.833	.019	.895
			(.023)		(.028)	
Parents College +	.029	.061	-.032***	.041	-.013	.021
			(.008)		(.008)	
Students	52665	95331		43547		7756

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 school level.

estimate

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

where $\mu_{j(i)}$ and μ_t 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. Assuming both groups outcomes were trending similarly, the coefficients β_k are period k-specific difference-in-differences estimates capturing the causal impact of ZOC.[12] This design builds in placebo tests that help identify violations of the parallel trends assumption: for $k < 0$, non-zero β_k would be an indi-

[12]In Appendix D, we provide event-study estimates from a parameterized model that summarizes the period-

cation of a violation of parallel trends. Throughout, standard errors are two-way clustered by school and year to account for correlation within schools across years and across schools within a given year.

4.1.1 Event-study Results

Figure 2a reports estimates of Equation 2 on student achievement in English and Language Arts (ELA). The achievement trends among ZOC students are similar to non-ZOC students in the years leading into the expansion of the program, providing support for the parallel trends assumption. We find modest achievement effects for the early cohorts of students, those who are partly affected by the program by the time they took achievement exams in eleventh grade. For the first cohort with full exposure, ZOC achievement improves by 0.14σ relative to the improvement among non-ZOC students and continues to improve, leveling out at roughly 0.2σ by the seventh year of the program.[13] Appendix Figure D.2a reports treatment effects on math scores that are nearly identical to ELA treatment effects.[14]

Compared to achievement gaps as students enter high school, these estimates suggest that the achievement gap is eliminated by eleventh grade. We can also benchmark these effects by comparing the treatment effects to the pre-ZOC eleventh-grade achievement gaps which are roughly 0.2σ in the unmatched sample and $0.15 - 0.16\sigma$ in the matched sample. Appendix Figure L.5 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.

Event-study results for four-year college enrollment are reported in Figure 2b. Similar to achievement effects, we do not find evidence that college enrollment rates among ZOC students trended differently in the years before the program expansion. College enrollment effects mirror achievement effects in that students less exposed to the program experienced smaller effects; by the first cohort with full exposure to ZOC we find ZOC college enrollment rates improved by an additional five percentage points compared to non-ZOC change. To benchmark this effect, 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 as shown in Appendix Figure L.3a.

specific β_k coefficients with the model

$$\beta_k = \theta_1 \mathbf{1}\{k < -1\} \times k + \theta_2 \mathbf{1}\{k \geq 0\} + \theta_3 \mathbf{1}\{k \geq 0\} \times k. \tag{3}$$

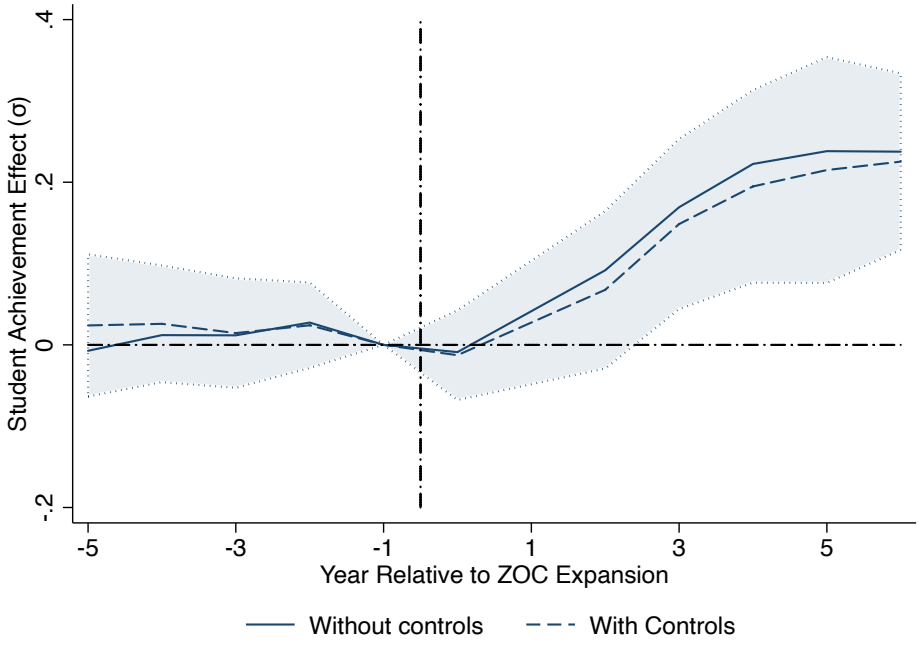
This parameterization concisely summarizes noisy estimates β_k, but further allows for a concise pre-trend test $\theta_1 = 0$, and over-identifying restrictions we use to report goodness of fit test p-values.

[13] Similarly, Appendix Figure D.3 reports estimates of the parametric event-study from Equation 3. The estimated pre-trend slope is nearly zero and indistinguishable from statistical noise. Mimicking the non-parametric event-study we document a clear trend-break following the ZOC expansion—ZOC relative achievement improving by roughly 0.04σ each year.

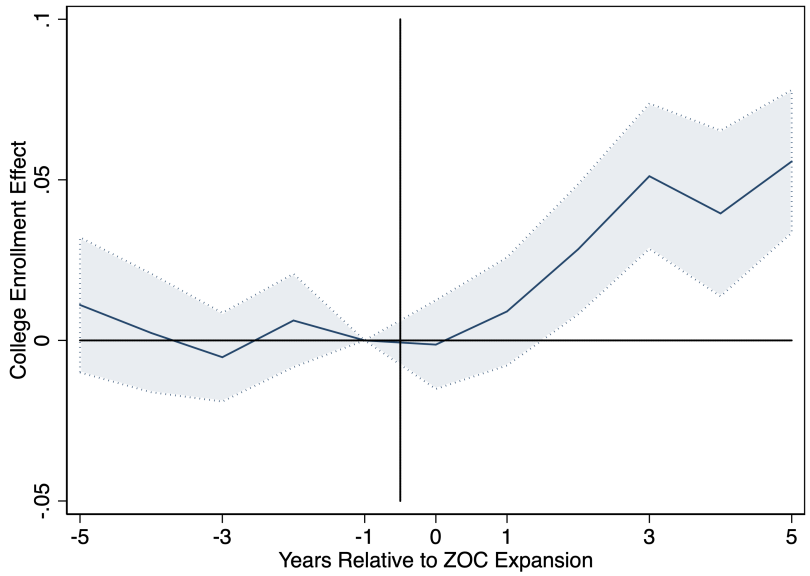
[14] We focus on ELA throughout the rest of the analysis because ELA exams are grade-specific throughout the sample, allowing for more parsimonious value-added estimation in the decomposition exercises that follow. Nonetheless, we find similar results when focusing on math scores and they are reported in Appendix D.

Figure 2: Achievement and College Enrollment Event Studies

(a) Achievement Event Study



(b) 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 ZOC expansion. The coefficient β_k shows difference-in-difference estimates on 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 double clustered at the school and year level and 95 percent confidence intervals are displayed in the shaded regions.

Appendix Figure E.1 reports college destination-specific treatment effects. We find that most of the college treatment effects are due to enrollment in California State University campuses,

with minimal impact on University of California enrollment, and some suggestive evidence of diversion away from private universities. We also do not find evidence of effects on community college enrollment, shown in Appendix Figure D.7. Therefore, the college enrollment event study evidence provides evidence that ZOC was effective in pushing students into college.

4.1.2 Distributional Effects

It could be a concern that most of the benefits reported in the previous section were obtained by high-achieving students or that the gains from some students came at the expense of others. For college outcomes, it is plausible that ZOC affected students who were more likely to enroll in the absence of the program than those students who were less likely to enroll in college. In this section, we study distributional treatment effect heterogeneity to explore these possibilities.

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

$$\mathbf{1}\{A_i \leq a\} = \mu_{j(i)} + \mu_t + \beta_a PostZOC_{it} + \boldsymbol{X}_i'\psi + u_i \tag{4}$$

where β_a is the distributional effect at a. Specifically, β_a measures the effect of ZOC on the probability student achievement is less than a, and differences in β_a inform us about heterogeneous impacts across the distribution of student achievement.

Figure 3 reports distributional estimates across the student achievement distribution. We find most of the gains in the bottom half of the distribution and estimates at the top are centered around zero. These results suggest that most of the treatment effects are concentrated among low-achieving students, noting that these benefits did not come at the expense of high-achieving students. We explore this further in Appendix F, where we estimate counterfactual distributions to provide more details about the distributional effects using various decompositions. Overall, we show that treatment effects were largest among relatively lower-achieving students.

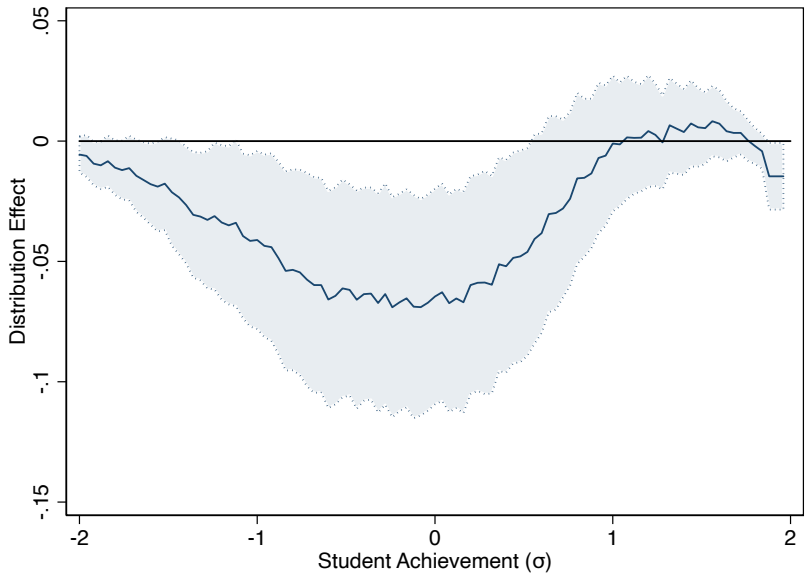
The dichotomous nature of college enrollment outcomes complicates the distributional analysis. To overcome this, we approach the analysis in two steps. First, among students in the pre-period, we predict four-year college enrollment using a logit LASSO for variable selection. Using the estimated parameters 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 4 shows that treatment effects were not just concentrated among students who were more likely to enroll in college, and as in previous results, we find treatment effects to be larger as the 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 for that group as opposed to a roughly 20 percent increase for students in the top two quartile groups.[15]

[15] Appendix Figure L.4 reports trends by different quartile groups.

The heterogeneity analysis provides evidence that ZOC was effective increasing achievement among students who would have otherwise performed poorly and those gains did not come at the expense of other high-achieving students. We also showed that ZOC improved four-year college enrollment outcomes, regardless of students' predicted probabilities of going to college, which suggests that the gains were not just concentrated among relatively low-achieving students as is the case for achievement effects.

Figure 3: Student Achievement Distributional Impacts



Notes: This figure reports estimates of β_a from Equation 4 for 100 equally distanced points between -2 and 2. β_a corresponds to a difference-in-difference estimate on the probability of students scoring below a on their student achievement exams. Standard errors are double clustered at the school and year level and 95 percent confidence regions reported in by the shaded regions.

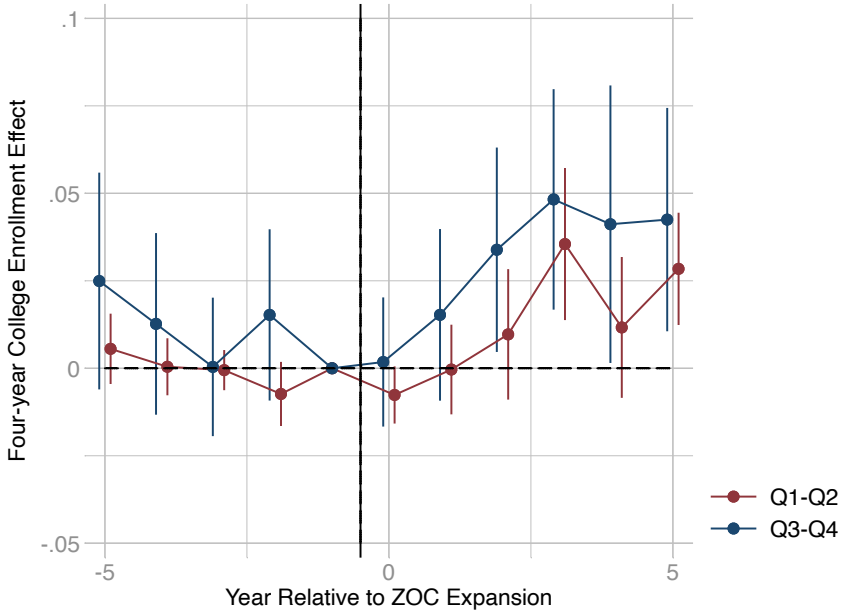
4.1.3 Robustness Checks

We now discuss threats to identification and conduct some robustness exercises. The parallel trends assumption could be violated through a changing composition of students. Changes in the access to certain schools may have induced differential sorting into ZOC neighborhoods, biasing the estimates in Figure 2a and Figure 2b. 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 the household composition (Nechyba, 2000). To assess these potential concerns, Appendix Figure E.1 reports event-studies where the outcomes correspond to different observable student characteristics. The evidence suggests differential changes in observables between the two sectors are not an immediate concern.

It remains possible that some students, similar on observables, strategically sorted into ZOC neighborhoods but differ on unobservables. We partially address this concern by restricting the sample to students that did not move into a ZOC neighborhood during middle school. Estimates are also the same when we restrict to students who did not sort into a ZOC neighborhood during

middle school; Appendix Figure E.2 and Appendix Figure E.3 reports these estimates. Thus, isolating achievement effects for students who did not strategically sort into ZOC does not change the baseline estimates.

Figure 4: Four-year College Enrollment Effects by Predicted Quartile Groups



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

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 grade. The estimates β_k are identified by within-student variation comparing changes in ZOC student gains to changes in non-ZOC student gains before and after the program's expansion. Appendix Figure E.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. 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. The LCFF is an unlikely concern for several reasons. 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 to a 24 percent increase for the top quartile, suggesting ZOC schools did not experience a disproportionate change in funding during our sample period. Lastly, Fejarang-Herrera (2020) further finds no effect of concentration grant money on student outcomes.

Evidence notwithstanding, we conduct a placebo exercise to assess the presence of potential LCFF effects. The intuition behind the placebo exercise comes from the fact 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 due to shared neighborhoods. Therefore, we test whether the program had any impact on lagged middle school test score gains. Appendix Figure E.5 presents estimates of Equation 2 where the outcome is $\Delta A_i = A_i^8 - A_i^7$, students' middle school gain in achievement that predated their ZOC enrollment. The evidence suggests that ZOC did not impact students before they entered high school, showing that differential selection into ZOC or any potential LCFF effect pre-dating ZOC enrollment are not a concern.

4.2 Decomposition of Achievement Effects: Gains in school effectiveness or gains in match quality?

The achievement effects show that ZOC student achievement improved at a remarkable pace compared to improvements of students enrolled at other similar schools. There are two potential sources of such gains. If parents chose schools more suitable 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 also contribute to the gains. We adopt the model of Abdulkadiroğlu et al. (2020) to decompose the achievement effects to provide a more refined reflection on 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 among 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[16]

$$A_{ij} = \alpha_j + \boldsymbol{X}_i'\beta_j + u_{ij} \tag{5}$$

where u_{ij} is mean zero and uncorrelated with $\boldsymbol{X}_i$ by construction. The vector of student characteristics $\boldsymbol{X}_i$ is normalized $E[\boldsymbol{X}_i] = 0$ so then $E[A_{ij}] = \alpha_j$ is the average achievement at

[16] We suppress time indices for notational ease.

school j for 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 5 allows us to express the potential achievement of student i at school j as depending on three factors, ability, the relative effectiveness of school j, and a student-school match quality component 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. Positive M_{ij} could arise if students sorted into schools based returns to their particular attributes their captured by the $\boldsymbol{X}_i'(\beta_j - \bar{\beta})$ or unobserved factors $(u_{ij} - \bar{u}_i)$ that make student i suitable for school j.[17]

4.2.2 Value Added Model Estimation and Bias Tests

For the purposes of the decomposition, we estimate treatment effects on α_j and 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

$$E[A_{ij}|X_i, j(i) = j] = \alpha_j + \boldsymbol{X}_i'\gamma_j; \quad j = 1, \cdots, J. \tag{6}$$

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 the latter shown to be 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 leverage the within-zone lottery variation to test for bias in OLS VAM estimates (Deming et al., 2014). Within-zone lotteries randomly shuffle students across schools and generate unbiased estimates of α_{jt}, which we can use to compare to OLS VAM estimates, similar in spirit to Kane and Staiger (2008). We use the framework developed by Angrist et al. (2017) to test for bias in the value-added estimates and do not find evidence of bias. Appendix H.2 contains

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

additional details regarding the bias tests. We provide details and summary statistics for the achievement model estimates in Appendix G.1.

4.2.3 Event-study Results: Changes in school effectiveness explain most of the gains

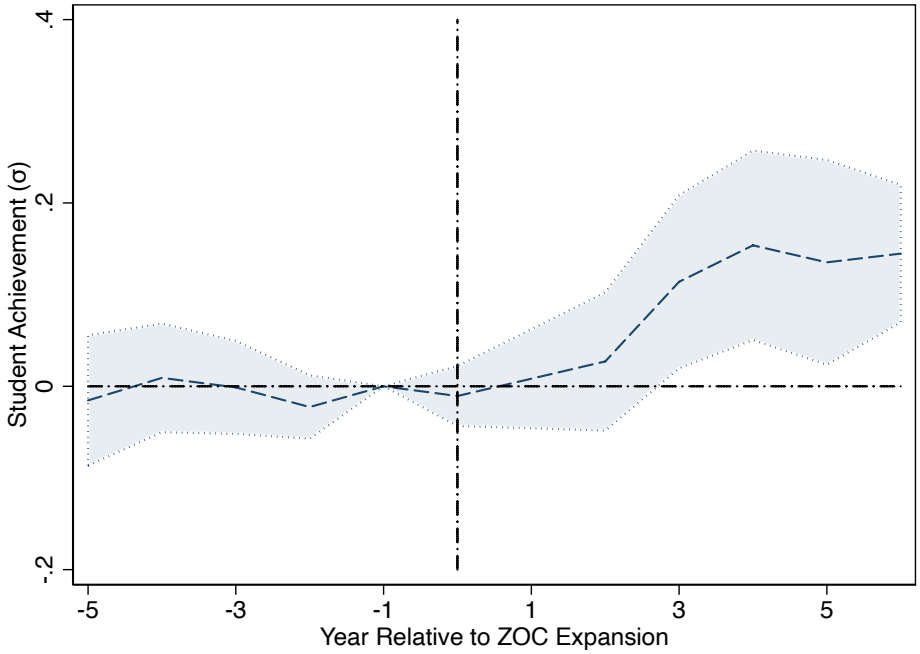
With estimates of the achievement model discussed in the previous section, we can estimate treatment effects on school effectiveness α_j and the observable component of match effects M_{ij}.Viewed through the lens of the model of school competition discussed in Section 2, we should expect to find evidence of improvements in α_j indicating ZOC improvements in causal impacts on student learning. Figure 5a reports event-study estimates for school effectiveness. We do not find evidence of differential trends in the pre-periods. Mimicking the event-study evidence for achievement effects, we find a clear trend-break in the relative improvements in ZOC school effectiveness. The ZOC per-year difference in ATE improvements averaged 0.021σ per year (see Appendix Figure D.4a), accounting for most of the observed achievement effects.

An alternative source of gains arises through the choices parents make. An expanded choice set introduces scope to select schools that more adequately suit students' needs, and as a consequence, the potential of achievement effects in the absence of competitive effects. Figure 5b shows that match effects played 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 expanded, we do not find evidence of large gains on this margin.

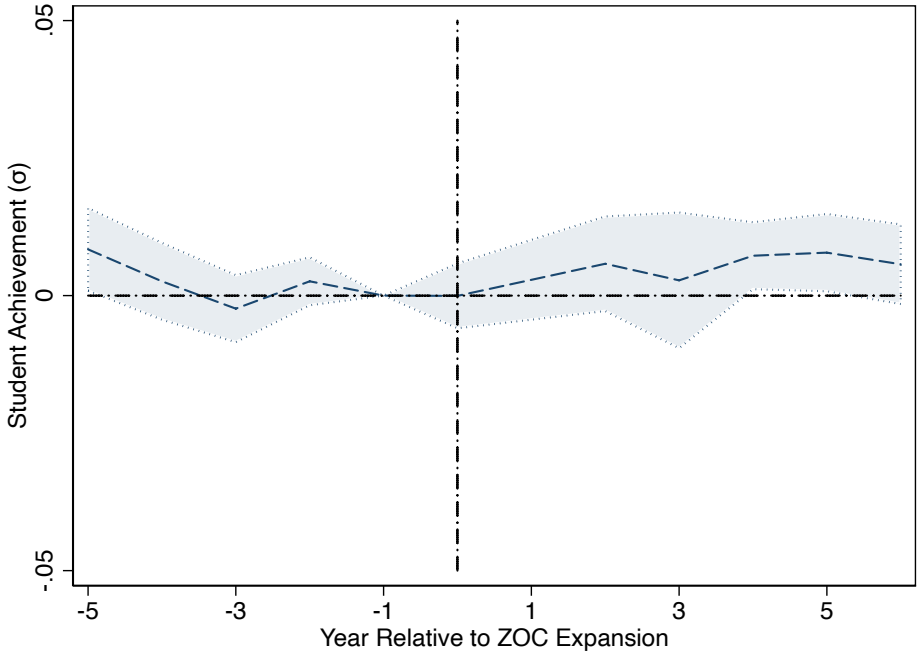
Two comments warrant mentioning about this evidence. First, the α_j effects are consistent with a competitive effects story, but still do not entirely rule out other contemporaneous shocks as a potential explanation. In the following section, we leverage ZOC-specific variation in OVG to test the competitive effects hypothesis. That variation—albeit imperfectly—captures differences in competitive pressures schools faced at the start of the program that should more plausibly uncorrelated with other contemporaneous shocks, providing a more direct test for the competitive effects hypothesis. Second, given the roughly homogeneous population of ZOC students—both within and between zones—suggests that scope for match effects on observables were minimal. The decomposition evidence nonetheless provides evidence consistent with the first implication of the model discussed in Section 2.

Figure 5: Decomposition event studies

(a) ATE event study



(b) Match Effect event study



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

4.3 School effectiveness treatment effect heterogeneity: lower-performing schools improved more

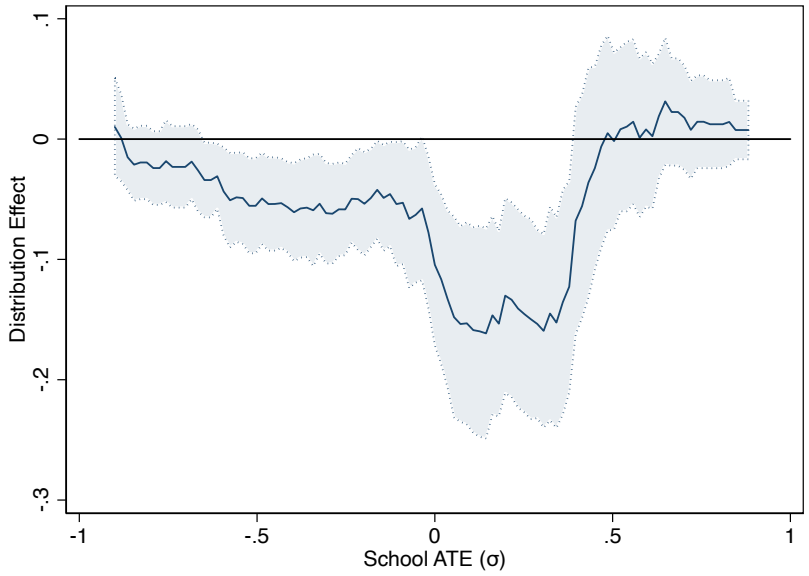
We now turn to Implication 2, which suggests that lower-performing schools should improve more than higher-performing schools, implying a decrease in the within-zone dispersion of school quality. Following the distributional framework used to study distributional effects on student achievement, we assess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

Figure 6a 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 average decreased by roughly 12 percentage points. In contrast, the probability that ZOC value-added is less than 0.5 student standard deviations above average did not change. These pieces of evidence suggest that most of the changes in the school quality distribution were concentrated among lower performing schools, evidence consistent the lowest performing schools improving the most. We provide evidence supporting the parallel trends assumption across the achievement distribution in Appendix Figure F.3, providing reassuring evidence for the strong assumptions required for this design.

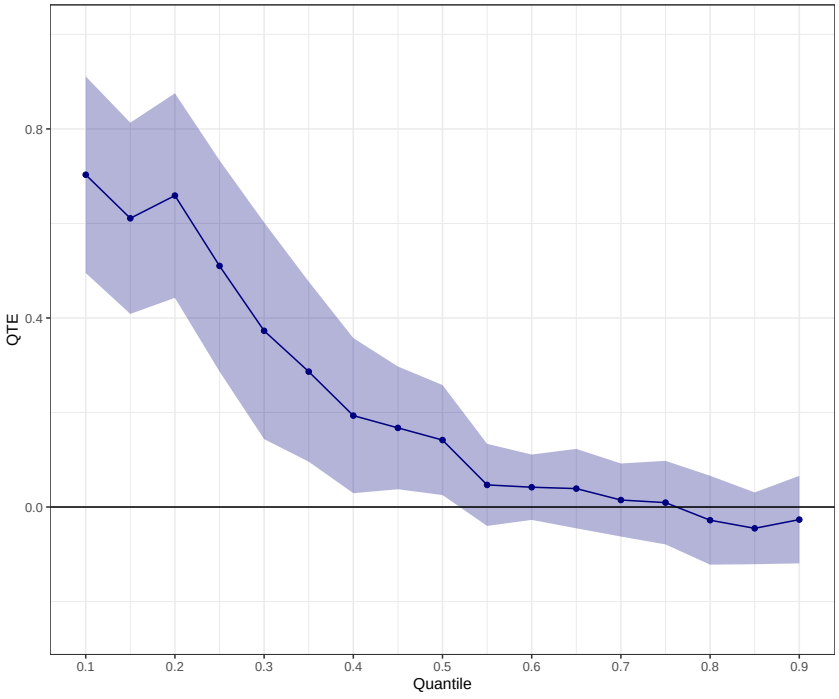
As a second exercise, we estimate unconditional quantile treatment effects shown in Figure 6b using the methods developed in Chernozhukov et al. (2013). These estimates more clearly show that most of the gains are concentrated in the bottom half of the school effectiveness distribution, with modest and potentially negative impacts at the top, although we can't distinguish these from statistical noise. Both of these estimates suggest that the ZOC distribution experienced a compression relative to the non-ZOC distribution. In Appendix I.1, we provide event-study evidence that the change in the within-zone dispersion of value-added decreased relative to the change in the rest of the district, suggesting that the compression is also within zones.

Piecing the evidence from Section 4.2 and Section 4.3 together suggests that schools responded to competition by adjusting their causal impact on student learning, with the schools potentially facing the most pressure to improve, improving the most. We augment the support for the competitive effects story in the following section by assessing treatment effect heterogeneity with respect to OVG. This requires us to pivot to the demand side, where we can also assess parents' choices and provide evidence on the incentives schools faced.

Figure 6: Distribution and Quantile Treatment Effects on ATE



(a) Distribution Effects on School ATE



(b) Quantile Treatment Effects

Notes: Panel A reports point estimates from difference-in-differences regressions of school-level indicators $\mathbf{1}\{\alpha_{jt} <= y\}$ on year indicators, school indicators, school-level student incoming achievement, and post indicators interacted with ZOC indicators for 100 equally-spaced points y between -0.9 and 0.9. Standard errors are double clustered at the school and year level and 95 percent confidence intervals reported by shaded regions. Panel B reports unconditional quantile treatment effects estimated by inverting both the observed ZOC 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 used to construct 95 percent confidence regions.

5 Demand and OVG

Our focus on the supply-side has demonstrated that schools adjusted their quality in response to the program's introduction. We now turn to the demand side to assess if parents choices are consistent with the supply-side response and to further probe the competitive effects interpretation. We can relate estimates of school mean utility—derived from rank-ordered lists parents submit to the program—to measures of school and peer quality to assess the consistency of parents' choices with the supply-side response. Moreoever, we can use variation in OVG at the onset of the program–also derived from rank-ordered lists—to study if schools facing the most pressure to improve did in fact improve the most. Both of these exercises require us to estimate 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).[18] The model in Section 2 allowed school popularity to vary by student characteristics $\boldsymbol{X}_i$, and we incorporate this by categorizing students into three baseline achievement cells and allowing school popularity to vary by achievement cell. Student i's 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 assume $\varepsilon_{ij} \sim EVT1|\delta_{jc}, d_{ij}$, a standard assumption in the discrete choice literature.

For each applicant, we observe a complete ranking over schools in their zone $z(i)$ with varying numbers of schooling options $Z(i)$ across zones, $R_i = (R_{1i}, R_{2i}, \cdots, R_{Z(i)i})$. Assuming applicants reveal their preferences truthfully, the preference profile for each applicant follows

$$R_{ik} = \begin{cases} \arg\max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg\max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}. \tag{7}$$

Truthful preferences 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 have biased beliefs (Kapor et al., 2020). Although this is likely in ZOC neighborhoods, schools observe reported preferences—truthful or not—and respond accordingly to this demand.[19]

[18] The ZOC setting provides an advantageous feature in that students residing within a zone must rank *all* schools within their zone, and are restricted to ranking only schools within their zone. Therefore, we observe complete rankings for all students within each zone, regardless of attendance, and don't face issues arising with endogenous choice sets.

[19] Nonetheless, in future versions of this paper we plan on estimating demand parameters that account for strategic incentives (Agarwal and Somaini, 2018, Larroucau and Rios, 2018). Accounting for strategic reporting amounts to applicants choosing rank-ordered lists that maximize their expected utility, taking as given their beliefs

The likelihood of observing R_i for student i is a product of logits (Hausman and Ruud, 1987). The conditional likelihood of observing list R_i is

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r|U_{ir}<U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \tag{8}$$

We aggregate the log of Equation 8 across individuals to construct the complete likelihood and estimate parameters of the utility specification via maximum likelihood.

We relate estimates of δ_{jct} to time-varying measures of school and peer quality to assess the consistency of parents' choices with the supply-side response. In contrast, to construct measures of OVG, we want to use variation in popularity that is purged of the program's influence. Unfortunately, we do not observe rank-ordered lists before the policy intervention so we use demand parameters corresponding to the first cohort of the program. Our OVG estimates will reflect measures of school popularity at the onset of the program and capture the incentives different schools faced at the start of the program, a measure motivating a potential subsequent supply-side response.

5.2 Parents Value School Effectiveness

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 the student achievement decomposition presented in Section 4.2.1. We estimate

$$\delta_{jct} = \xi_{cz(j)} + \xi_{z(j)t} + \omega_P Q^P_{jt} + \omega_S \alpha_{jt} + \omega_M Q^M_{jct} + u_{jct} \tag{9}$$

where ξ_{cz} are achievement cell by zone fixed effects and ξ_{zt} are zone by year effects capturing zone-specific preference heterogeneity across cohorts. Mean utilities, peer quality, treatment effects, and match effects are scaled in standard deviations of their respective school by year 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.

Table 2 reports estimates of Equation 9. Column 1 and Column 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 precise. In particular, a one standard deviation increase in school effectiveness is associated with a 0.42 standard deviation increase in school popularity, while a one standard deviation increase in peer ability is associated with a 0.17 standard deviation increase in mean utility. In Column 5, 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. In Panel B, we further control for school characteristics such as the type of school and teacher characteristics and find the estimates are essentially unchanged.

The relatively strong preference for school value-added suggests parents effectively distin-

about admissions chances at each school. One approach is to assume applicants have rational expectations, as done in Agarwal and Somaini (2018), or more recent approaches have accounted for belief biases as done in Kapor et al. (2020).

guish between effective and less effective schools, evidence consistent with the market-level evidence. Importantly, these estimates provide suggestive evidence indicating that competitive incentives were not weak as is found in other settings (Abdulkadiroğlu et al., 2020, Hastings et al., 2005, Rothstein, 2006). One notable feature of the ZOC setting is the homogoneity of students within each zone, effectively eliminating selecting schools on income or race. If income and race are characteristics that parents use to proxy for effective schools, this would give rise to selection on levels as found in other settings. Because these channels are effectively eliminated within each zone, then parents may select schools on other characteristics more strongly correlated with value-added. The relative homogeneity of students is one potential reason why the ZOC preference estimates contrast other settings (e.g., Abdulkadiroğlu et al. (2020) and Rothstein (2006)). In Section 7 we provide additional discussion about why features of the ZOC program may have facilitated families acquisition of information.

Table 2: Preferences for school characteristics

	(1)	(2)	(3)	(4)
	Panel A: No Controls			
α	0.420**			0.426**
	(0.200)			(0.194)
Ability		0.169		0.00779
		(0.360)		(0.325)
Match			-0.0411	0.0292
			(0.242)	(0.209)
Observations	459	459	459	459
R-squared	0.502	0.468	0.466	0.503
	Panel B: With School Controls			
α	0.466***			0.486***
	(0.152)			(0.146)
Ability		0.170		0.0163
		(0.329)		(0.300)
Match			-0.0554	0.0623
			(0.198)	(0.159)
Observations	459	459	459	459
R-squared	0.601	0.566	0.565	0.602
Zone X Year FE	X	X	X	X
Cell X Zone 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 ATE, ability and match effects. Both outcomes and school level measures are standardized within each cohort. Panel A does not adjust for other school covariates and Panel B adjust for additional school characteristics such as school type, teacher race and teacher experience. Standard errors are clustered at the school level.

5.3 Option Value Gain

Differences in OVG across students and schools can provide further insights into the effects of competition. Schools exposed to students with higher OVG should face more pressure to improve so that they can sustain their enrollment. Through the lens of the model in Section

A, schools exposed to students with higher OVG should exert additional effort, so we should expect heterogeneous treatment effects with respect to OVG if schools responded to varying incentives. Therefore, evidence of OVG treatment effect heterogeneity provides support for the competitive effects story.

We use preference parameters corresponding to the first cohort of ZOC students to estimate student OVG for the first and every subsequent cohort.[20] Figure B.1 displays the distribution of OVG across students and Table B.1 reports OVG correlates.[21] For the purposes of the analysis here, we categorize students and schools into high and low OVG groups. For students, we categorize students in the top two quartiles of the student OVG distribution as high OVG students; for schools, we do the same but we only use the first year's distribution. The student-level statistic is informative about which students gained access to more popular schools, net of distance costs, and the school-level statistic inform us about which schools had the most pressure to improve.

Figure 7 displays the average student OVG quartile in each Census tract, providing a visual description of where most of the high OVG students are located in. 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 is the zone that offers students the most options, the other two are not necessarily high choice zones. 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 not just accounting for school popularity but also distance costs when estimating the value of introducing new options.

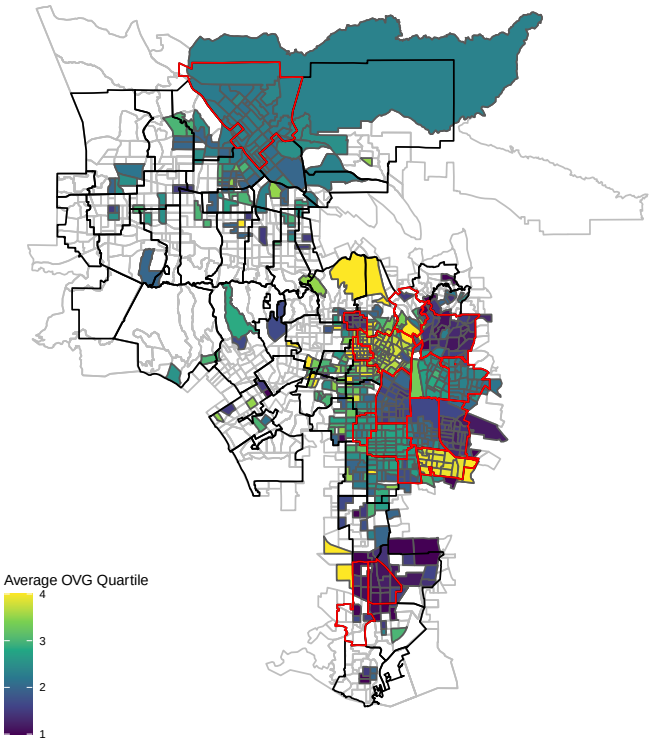
We test for OVG treatment effect heterogeneity by estimating difference-in-differences models that include interactions between $Post \times ZOC$ indicators and school-level high OVG indicators. The school-level OVG metric measures the average OVG of students assigned to that school in the baseline year. A school flagged with high OVG is a school whose students gained access to more desirable schools and were likeliest to enroll elsewhere.

Table 3 reports estimates of OVG treatment effect heterogeneity. Throughout, we include terms to capture school OVG effects and progressively add additional potential sources of treatment effect heterogeneity to assess the stability of our estimates. In Column 1 we reports estimates of models with a $Post \times ZOC$ interaction term and two additional triple interaction terms defined above. The estimates suggest that OVG explains a substantial share of the positive achievement impacts. While students enrolled in schools not flagged as high OVG experienced positive improvements in their achievement, the estimates have wider confidence intervals. The school effects demonstrate that students enrolling in schools with greater pressure to improve experienced additional gains. Columns 2-7 gradually add interaction terms with other observable characteristics to see if they can explain the OVG findings; the OVG interac-

[20] 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.

[21] The average OVG for the first cohort was roughly 18, meaning the typical ZOC household was willing to drive 18 additional miles (36 roundtrip) 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, would imply that the average household was willing to pay $1080 for their new menu of schools.

Figure 7: Census Tract Average Student OVG Quartiles



Notes: This map displays Census Tract student-level OVG quartile averages. That is, for each Census tract with at least two ZOC students, we calculate the average OVG quartile of students in that Census tract and report the resulting average. Grey 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 comprise less than one percent of all ZOC students. The existence of these students is probably due to lags in updating student addresses within the district.

tion terms are remarkably stable across every column. Table **??** explores additional sources of OVG heterogeneity pointing to similar conclusions.

These findings suggest that OVG captures something intrinsic about incentives governing competition that cannot be explained by observable characteristics partly used to determine it. It remains unclear what changes may have occurred to yield these large gains, but we return to this in Section 7 and provide some suggestive evidence.

6 Lottery Analysis

The preceding market-level analysis has demonstrated a remarkable improvement in ZOC student achievement, and these improvements were 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, intra-district 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) answer whether students' academic performance improves if they attend a school they preferred the most. In the Zones of Choice setting, students' choice sets expanded and we ask whether students obtained a premium from attending a most-preferred school, relative to other lower-ranked ZOC schools they may attend in the case that they do not get an offer from their most-preferred school. We relate achievement A_i to indicators of most-preferred enrollment D_i in the following way:

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

where $d_{i\ell}$ 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 also the ZOC program.

If we also assume lottery offers only influence test scores through most-preferred attendance and weakly increase the likelihood of most-preferred enrollment, 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 into attending a most-preferred school through their lottery

Table 3: OVG Treatment Effect Heterogeneity

	(1) ELA	(2) ELA	(3) ELA	(4) ELA	(5) ELA	(6) ELA	(7) ELA
PostZOC	0.045	0.035	0.033	0.020	0.043	-0.048	-0.030
	(0.039)	(0.065)	(0.039)	(0.055)	(0.038)	(0.054)	(0.070)
PostZOC $\times SchoolOVG_{3,4}$	0.123*	0.119*	0.123*	0.117*	0.122*	0.123*	0.117*
	(0.061)	(0.060)	(0.061)	(0.060)	(0.060)	(0.061)	(0.059)
$\times$ Poverty		-0.011					-0.011
		(0.032)					(0.032)
$\times$ College		0.014					0.019
		(0.018)					(0.023)
$\times$ Migrant		-0.091					-0.098*
		(0.052)					(0.052)
$\times$ Spanish at Home		0.045*					0.026
		(0.023)					(0.015)
$\times$ Special Education		0.032					0.010
		(0.036)					(0.045)
$\times$ Female			0.023				0.027
			(0.017)				(0.019)
$\times$ Black				-0.078*			-0.119**
				(0.042)			(0.041)
$\times$ Hispanic				0.034			-0.015
				(0.033)			(0.029)
$\times$ Lagged Achievement					-0.026		-0.031
					(0.017)		(0.020)
$\times$ Income						0.009*	0.008*
						(0.005)	(0.004)
Observations	183,294	183,294	183,294	183,294	183,294	183,294	183,294
R-squared	0.538	0.538	0.538	0.538	0.538	0.538	0.538

Notes: This table reports estimates from difference-in-difference regressions with same controls as event-study models from Equation 2 and an additional interaction terms for OVG heterogeneity. $SchoolOVG_{3,4}$ is an indicator for a school being in the top two quartiles of the school OVG distribution in the baseline year. Additional rows correspond to estimates of coefficients corresponding to triple interactions between post indicators, ZOC indicators, and row-variables. All estimates include main effects for student OVG and lagged test scores. Standard errors are double clustered at the school and year level.

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 H contains additional 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 4 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 offered a seat. Panel A shows that students offered a seat at their most-preferred school experienced a 0.045σ gain on 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 if 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 provided 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 arise due to parents not choosing more effective schools (in terms of value-added), or market-level effects could be causing changes in most-preferred premiums. We explore this in Table 5, with impacts on ELA and Math in Panel A and B, respectively. Column 3 of Table 5 reveals that only the first two cohorts of compliers experienced ELA gains by eleventh grade; the following three cohorts did not experience gains distinguishable from noise. In Columns 4 and 5, we report control complier means to assess how differences in most-preferred premiums changed 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 time period, eliminating the ELA achievement premiums present for earlier cohorts. The pattern is not as salient for math scores, but we do find treatment effects narrowing across cohorts similar to ELA effects.

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

Table 4: 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 most-preferred school attendance 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 don't observe NSC outcomes for the last cohort, so we don't include them in the estimates. Standard errors are clustered at the lottery level for all estimates and reported in parentheses.

Table 5: Lottery estimates by cohort, 2013-2017

	(1) FS	(2) RF	(3) TSLS	(4) CCM	(5) TCM
			Panel A: ELA		
First and Second Cohort	0.467***	0.047*	0.101**	[.071]	[.172]
	(0.063)	(0.024)	(0.048)		
Third and Fourth Cohort	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 Cohort	0.467***	0.052	0.110	[.049]	[.159]
	(0.063)	(0.040)	(0.088)		
Third and Fourth Cohort	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 affected 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 Math and ELA scores, poverty, parental education, and other demographics reported in Table H.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

This paper studies a change in the institutional environment, an increase in school choice, and documents marked improvements in school quality. Changes in inputs—such as teacher quality and class size—could be associated with the changes in school quality we show (Krueger, 1999). Alternatively, differences in management practices have been shown to be 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). Specific institutional features could also facilitate the potential effects of an increase in school choice. In this section, we discuss each of these.

7.1 Changes in School Inputs

Appendix K.1 reports evidence on changes in inputs such as teacher characteristics, teacher quality, and class size.[22] We do not find evidence that these inputs in the production function experienced a differential change. Therefore, we do not find evidence of salient changes in inputs that could explain the improvements in school quality.

7.2 Changes in management practices

We do not have data to correlate changes in management practices, such as the No Excuses approach that has been shown to be associated with effective charter and public schools (Angrist et al., 2013, Fryer Jr, 2014). Therefore, we study changes that albeit indirectly probe at changes in management practices. We focus on classroom assignment policies. We focus on this because it provides insight into within-school changes, partly governed by changes in principals' decisions. Appendix J studies changes in student-teacher racial match and Appendix K studies changes in classroom assignment policies. We find evidence of increases in the 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 practices. While the literature is mixed in terms of the effects of tracking (Betts, 2011, Bui et al., 2014, Card and Giuliano, 2016, Cohodes, 2020, Duflo et al., 2011), these changes suggest other potential organizational changes among ZOC schools.

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.

7.3 Why is Los Angeles different?

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 to 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 due to increased competition, we find that parents exhibited a greater preference for value-added than for other school characteristics, including schools' peer composition. While the latter finding allows us to provide a more consistent narrative, it still stands in contrast with a growing literature that finds parents select schools based on achievement levels instead of gains (Abdulkadiroğlu et al., 2014, 2020, Rothstein, 2006). These differences beg the question: why is Los Angeles different? We highlight particular features of the program that faciliated access to information and note that supply-side constraints prevented

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

schools from adjusting other margins that would attenuate competitive effects.

ZOC administrators devote a considerable amount of resources ensuring each cohort is informed about the application process, knows their schooling options, and administrators also indirectly provide anecdotal information about school or program's defining characteristics. 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). It is also important to emphasize that zones are relatively small compared to the universal high school admissions process in New York, for example. In a setting like New York, where parents must select from a menu of more than 750 schools, parents faced with a complex set of options may resort to using simplified strategies to in selecting schools (Corcoran et al., 2018). The lack of choice overload represents an additional friction that is not present in the ZOC setting. In addition to providing a more personalized approach to providing information about the program, the restricted nature of parents' choice sets implicitly eliminates choice overload concerns present in other school choice settings.[23]

ZOC schools are also constrained in terms of how they can adjust their 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 where parents select schools based on achievement levels as opposed to gains, indicating a stronger preference for peer characteristics, recruitment efforts have lower returns if screening strategies are restricted. The preference estimates suggest ZOC parents do value gains more than 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 relatively homogeneous population of students. Roughly 85-90 percent of ZOC students are classified as Hispanic and poor by the school district. The zones avoided combining catchment areas that were vastly different in terms of 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 isolated schools impact long-run outcomes. In the short-run, Card and Rothstein (2007) find robust evidence that SAT test score gaps are larger in more segregated cities, but that neighborhood segregation effects operate mostly through income. Looking at medium-run outcomes, Billings et al. (2014) find that the end of race-based busing in Charlotte-Mecklenburg schools 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 desgregation orders led to improvements in educational attainment, earnings, and improved adult health for Black individuals. While we find that both short- and medium-run outcomes improved, it remains to be seen if there are potentially adverse consequences from ending K-12 education in

[23] A public disclosure of value-added information by the Los Angeles Times in 2011 studied in more detail by Imberman and Lovenheim (2016), may have also provided parents an adequate baseline signal about school effectiveness and its correlates although this is purely speculation.

racially isolated schools for the affected students. It remains an open question if the impacts of another similar program would be similar if they created zones that integrated students across race and income.

There are several features of the program that contributed to the findings, but some do present apparent tradeoffs. The relatively personalized interactions between ZOC administrators and parents and the relatively small choice sets parents have constitute a setting where acquisition of adequate information about schools is more likely; in particular, ZOC administrator-led information sessions provide a potentially rich setting to learn about differences in school quality within zones. Restricting the scope for screening strategies further incentivizes schools to focus on improving student learning. But most ZOC students ended their K-12 education in racially isolated schools, and future research should pay attention to the potential adverse impacts of this institutional feature.

8 Conclusion

This paper studies a novel expansion of public school choice in Los Angeles known as the Zones of Choice. The unique design and implementation of the program provide a rich setting to study the effects of competition among public schools, and rich data arising from the centralized assignment system permit a more thorough analysis of both parental demand and incentives governing the supply-side response.

We show that the ZOC program led to gains in student achievement and four-year college enrollment rates, both sufficiently large enough 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 improvements in student-school match quality, we decompose achievement effects. Consistent with the competitive effects story, we show that changes in schools' value-added explain most of the achievement effects and that changes in match quality are small. These findings are consistent with demand estimates that suggest parents placed more weight on school effectiveness than on peer quality, suggesting schools under ZOC were incentivized to improve. Using a measure of competition derived from applicant preferences, we show that treatment effects were largest for schools facing the greatest pressure to improve. Therefore, through various avenues, we find evidence supporting the notion that schools improved due to increased competition.

Our market-level analysis helps explain why an analysis using randomized lottery admissions finds that earlier cohorts benefited from accessing in-demand schools, but later cohorts benefited less. This pattern is explained by the competitive incentives facing less-preferred schools, leading to reductions in most-preferred premiums present for early cohorts. Importantly, the two complementary research designs help us show that most of the program's benefits arise through market-level effects and not solely through students accessing the more popular schools.

Our findings reveal that there is scope for public school choice programs to elevate students' educational outcomes but also raise several questions. The Zones of Choice program also presents an inherent tradeoff between improving short-run outcomes through school competition and potentially hurting long-run outcomes through the entrenchment of school segregation

patterns the program generates. While we find empirical evidence supporting multiple predictions from stylized models of school demand and competition, the model does not inform us about the black box that produces the predicted gains or speak to potentially adverse long-run effects induced by the racially and economically segregated nature of students. The mechanisms through which schools adjusted, the factors contributing to parents effectively distinguishing between effective and ineffective schools, and long run effects of the program are important topics for future work.

A A Model of School Choice and School Quality

A.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

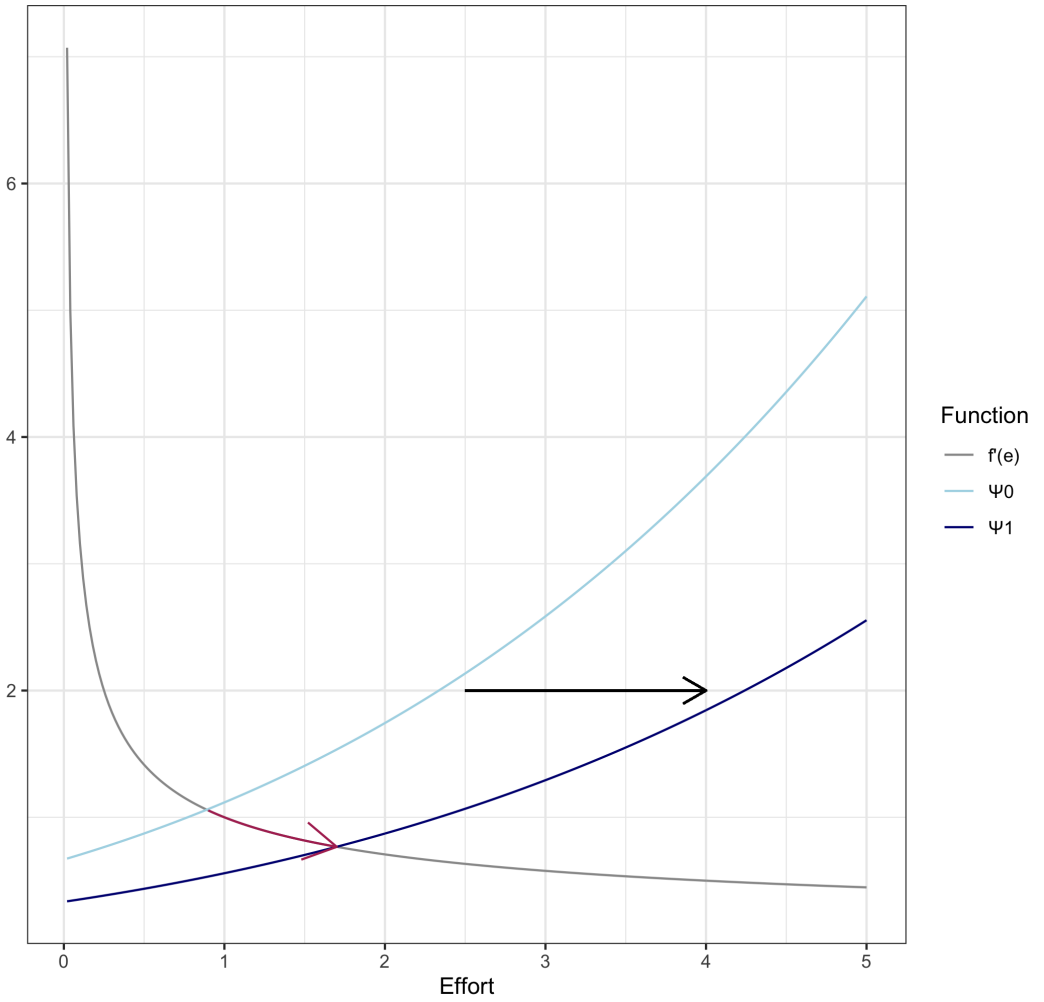
$$\Phi(e_j) = \frac{1}{\theta\omega\frac{1}{N}\sum_i P_{ij}(e_j; d_{ij}, X_i)(1 - P_{ij}(e_j; , d_{ij}, X_i))}$$

and let $\Phi(e_j, e_{-j})$ correspond to the strategic analog of $\Phi(e_j)$ that depends on other principal 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 A.1 depicts this visually.

The transition from the 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 will 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, then principals exert more effort after competition is introduced. Stratetgic 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 initially lowest effort increase their effort the most, leading to a compression in the within-zone quality distribution. Lastly, Proposition 4 provides a comparative static result indicating that an increase in OVG from an equilibrium would lead to further increases in effort. This latter proof again relies on the intuition gained from shifts in Φ.

Figure A.1: Change in Equilibrium



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

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

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

Proof. The existence of equilibria follow 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 $k \neq j$

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

□

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

$$\Delta e_j = 0$$

Proof. Figure A.1 shows that for each school j, their optimal level of effort is determined at the point where Ψ and f' intersect. Therefore, a principal j will find it optimal to increase their effort if Φ 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 would lead to an increase in effort in a non-strategic setting where principals independently maximize their utility, ignoring the actions of others. Then we show that the anticipated increases in effort from other principals leads to further downward shifts in Φ implying an equilibrium where each school j increases their effort.

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

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

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

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

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

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

That shows that the non-strategic 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.

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

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

Alternatively, the strategic complementarities in effort also would point to similar dynamics. Therefore, combining strategic complementarities with the fact that school's exert strictly more effort due to downward shifts in Φ allow 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 Zones of Choice rollout, then the resulting best response for a school j results in their 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 A.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 from a marginal increase in effort Δe is*

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

Proposition A.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 an equilibrium e^*. For a given school j, we have

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

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 where schools all provide 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). □

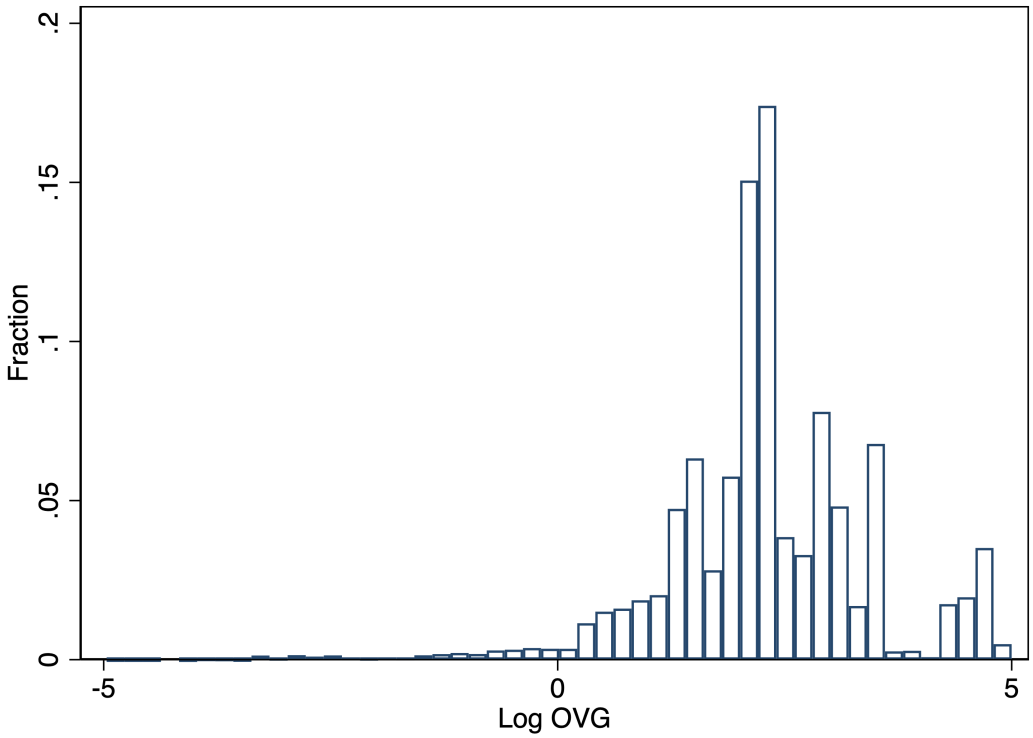
B OVG Details

Table B.1: OVG Correlations

	(1) Log OVG	(2) 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

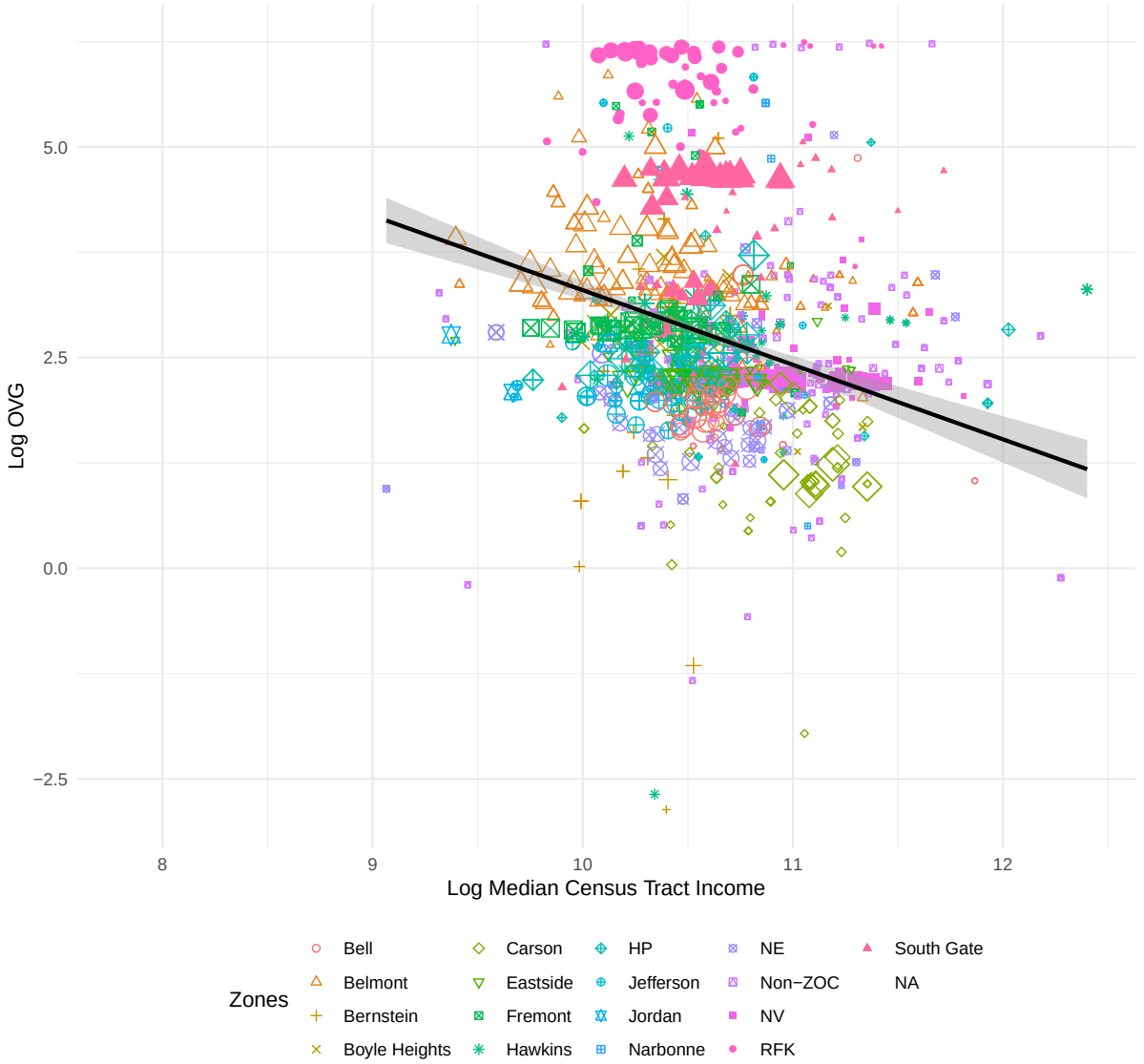
Notes: This table reports coefficients from multivariate regressions of Log OVG on row covariates. The sample is restricted to the initial cohort of Zones of Choice students. Column 1 does not include zone fixed effects, while Column 2 does. Robust standard errors are reported in parentheses.

Figure B.1: Log OVG Distribution



Notes: This figure reports a histogram of estimated Log 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 B.2: OVG-Census Tract Income Correlation



Notes: This map displays a scatter plot of Census Tract average Log OVG and Log Median Census Tract Income in the 2010 Decennial Census. Points are colored and shaped to reflect specific Zones to demonstrate the contributions coming from different zones. Regression line displayed has a slope -0.40615 (Zone Clustered SE=0.1986).

C Propensity Score Estimation

Table C.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	

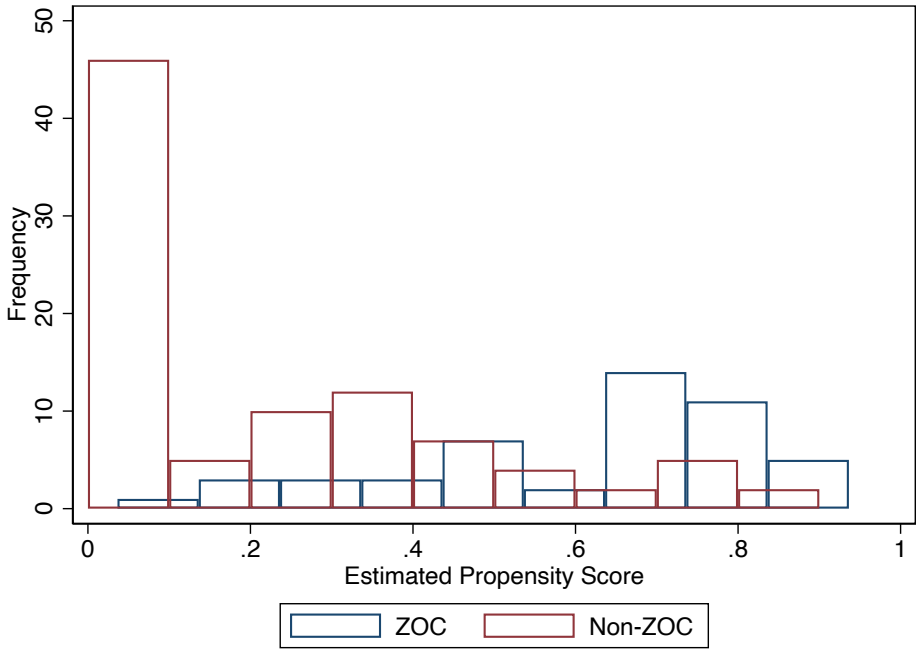
Notes: This table reports estimates from cross-sectional school-level bivariate regressions of the row variable on ZOC school indicators in 2012. All regressions are weighted by school enrollment except for the model where school enrollment is the outcome. Column (1) reports ZOC school means, Column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

Table C.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 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 reported in parentheses.

Figure C.1: Propensity Score Overlap

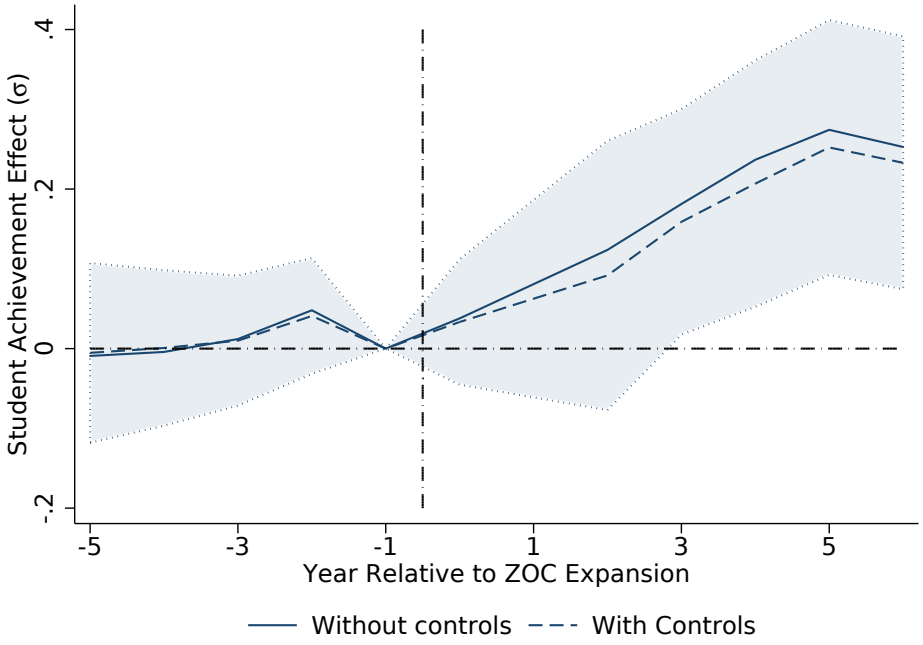


Notes: This figure reports histograms for the estimated school-level propensity scores by treatment status. Bin widths are equal to 0.1.

D Additional Event Study Evidence

D.1 Math Estimates

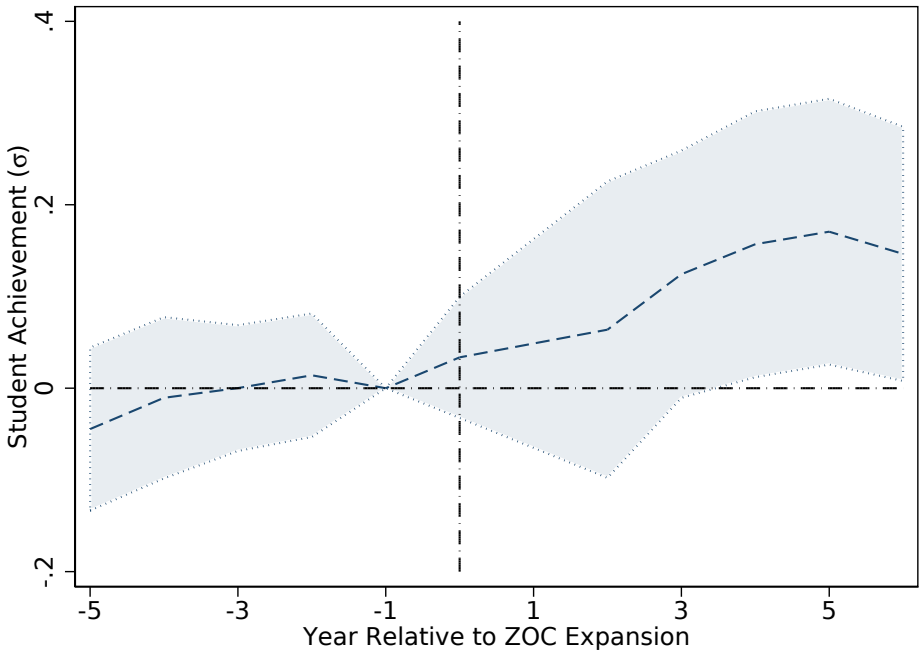
Figure D.1: Math Achievement Event Study



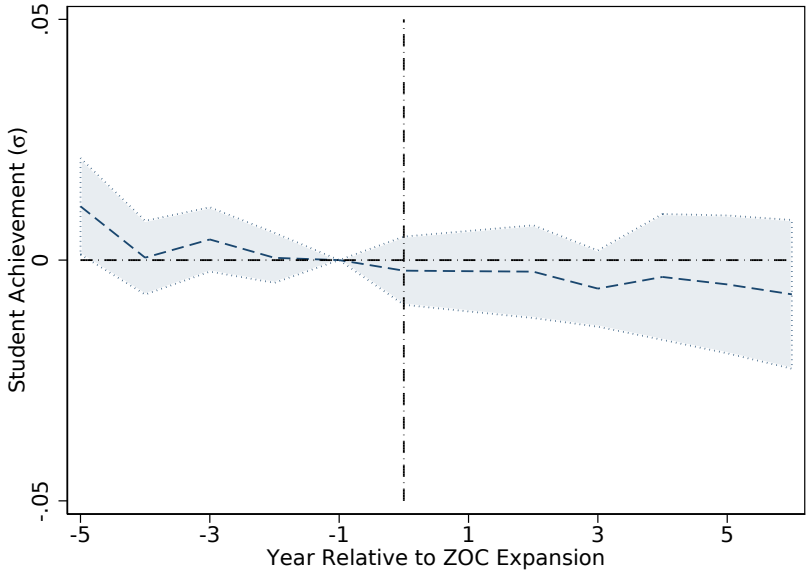
Notes: This figure plots the estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-difference estimates on outcomes relative to the year before the policy. The dashed blue line in Panel A traces out estimates 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 and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure D.2: Math ATE and Match Event Studies

(a) ATE



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

Ø

D.2 Alternate Event-study Parameterization

In this section, we present an alternative parameterization for event-studies. The less parametric models displayed in Figures 2a and 2b are ex-ante noisy, so the parameterization proposed in this section has potential efficiency gains if the model is correctly specified. The parameterization we propose is similar to Lafortune et al. (2018) but instead of directly estimating the parameterized model, we match the the non-parametric moments using the classical minimum distance approach of Ferguson (1958). In particular, we propose

$$\beta_k = \theta_1 \mathbf{1}\{k < -1\} \times k + \theta_2 \mathbf{1}\{k >= 0\} + \theta_3 \mathbf{1}\{k >= 0\} \times k \tag{10}$$

θ_1 captures an estimate of a differential pre-trend, θ_2 captures an immediate mean-shift following the program, and θ_3 captures a trend-break in the post-period. These three parameters are then used to concisely summarize the 10 event-study coefficients.

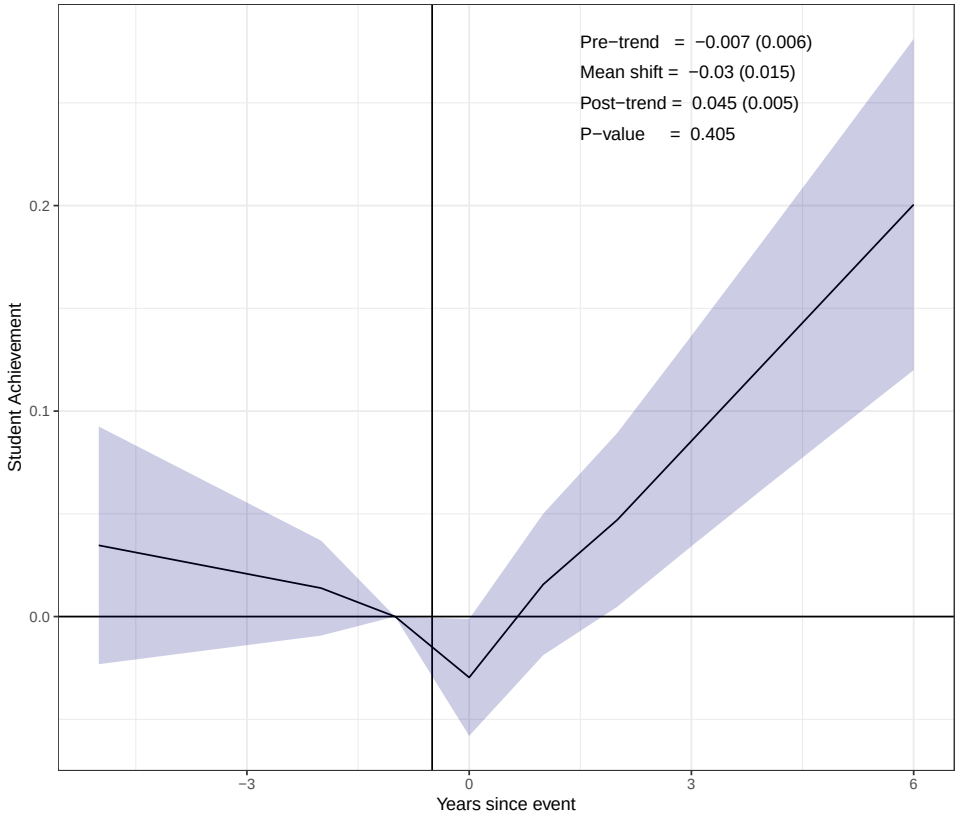
There are several reasons to pursue this approach. First, a test of $\theta_1 = 0$ is a concise test for differential pre-trends and a test of no trend-break $\theta_1 = \theta_3$ concisely summarizes any impact. Second, as discussed above, under correct model specification we gain efficiency. Third, the overidentifying restrictions allow for a goodness of fit test on the parametric restrictions. Under the null, the minimized value of the objective function Q evaluated at the estimator $\hat{\theta}$

$$Q(\hat{\theta}) = [\hat{\boldsymbol{\beta}} - g(\hat{\theta})]' W [\hat{\boldsymbol{\beta}} - g(\hat{\theta})] \sim \chi_{2K-3}$$

where $W = \hat{V}^{-1}$ is the inverse of estimated variance-covariance matrix of $\hat{\boldsymbol{\beta}}$.

Figures D.3, D.4a, and Figures D.4b report the implied event-study estimates from these models. The overarching conclusions are identical to the event-study estimates reported in Figures 2a, 2b, and Figures 5a, 5b. In all models, we fail to reject a differential pre-trend providing support for the parallel trends assumption. As before, we find most of the student achievement treatment effects are due to changes in school effectiveness. Figure D.5 reports event-study estimates where the outcome is student predicted ability implied by the model in Section A. We do not find evidence of differential changes in the predicted ability between ZOC and non-ZOC students.

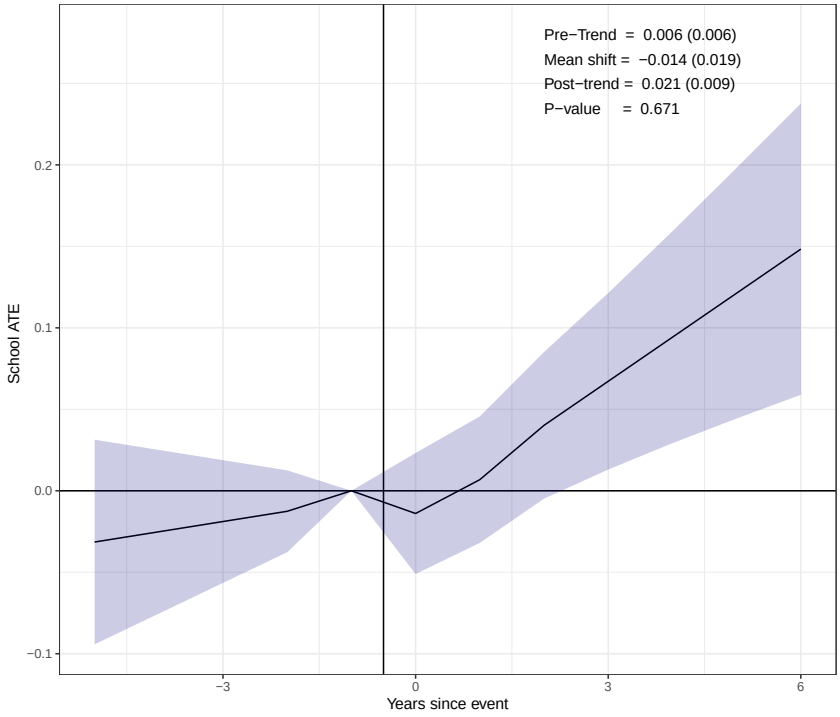
Figure D.3: Parametric Achievement Event Study



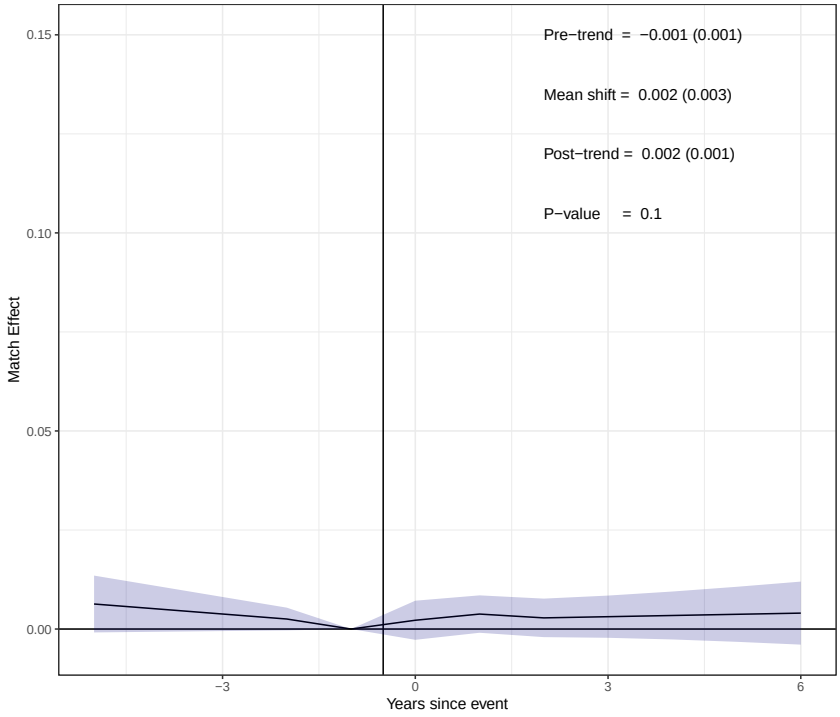
Notes: This figure plots the estimates of $g(\hat{\theta})$ defined in Equation 3. The value of the red line shows the difference in achievement σ between ZOC and non-ZOC students relative to their difference the year before the expansion. Estimates of $\hat{\theta_1}$, $\hat{\theta}_2$, $\hat{\theta}_3$ are denoted by Pre-trend, mean shift, and post-trend, respectively. The p-value from a Chi-squared test with seven degrees of freedom testing the models parametric restrictions is reported. Standard errors were estimated using the delta method using the variance covariance matrix of the non-parametric event-study coefficients $\hat{\boldsymbol{\beta}}$. 95 percent confidence intervals are displayed in the shaded regions.

Figure D.4: Decomposition of treatment effects

(a) Parametric ATE Event Study

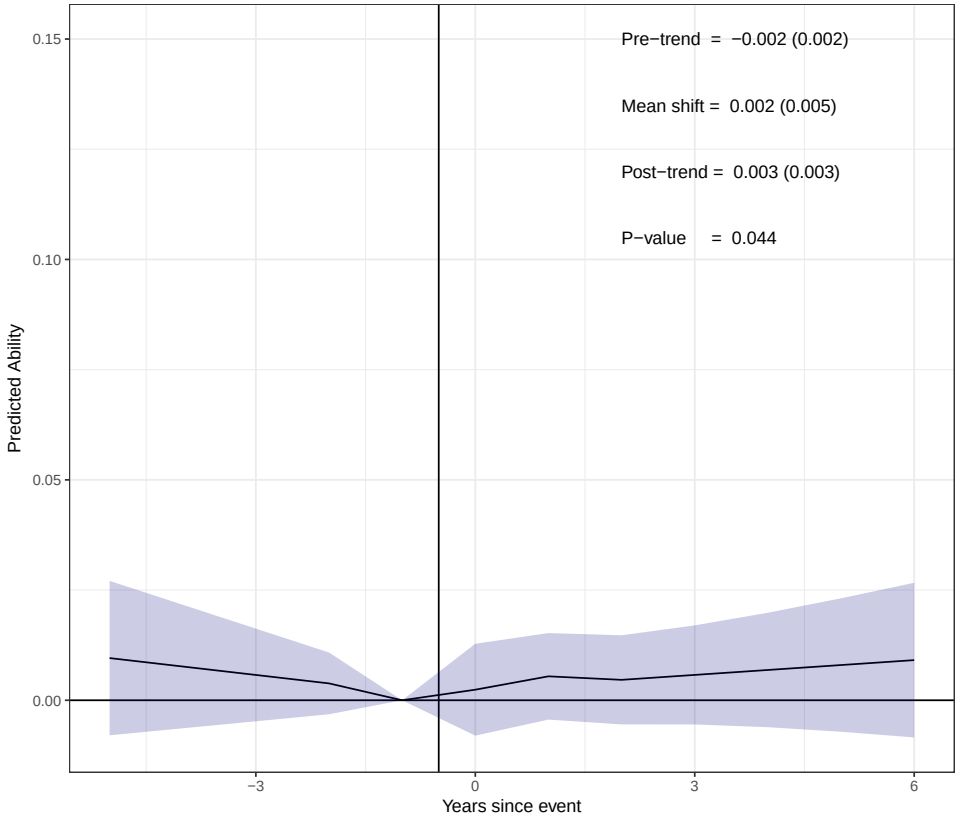


(b) Parametric Match Event Study



Notes: This figure plots the estimates of $g(\hat{\theta})$ defined in Equation 3. The value of the black line shows the difference in the change in match effects (in student σ) between ZOC and non-ZOC students relative to the year before the expansion. Estimates of $\hat{\theta_1}$, $\hat{\theta}_2$, $\hat{\theta}_3$ are denoted by Pre-trend, mean shift, and post-trend, respectively. The p-value from a Chi-squared test with seven degrees of freedom testing the models parametric restrictions is reported. Standard errors were estimated using the delta method using the variance covariance matrix of the non-parametric event-study coefficients $\hat{\beta}$. 95 percent confidence intervals are displayed in the shaded regions.

Figure D.5: Parametric Predicted Ability Event Study

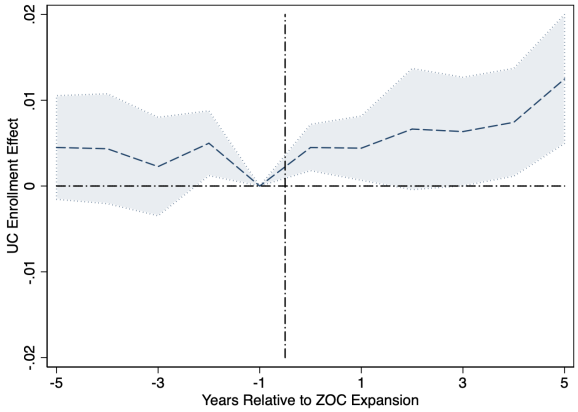


Notes: This figure plots the estimates of $g(\hat{\theta})$ defined in Equation 3. The value of the black line shows the difference in the change in predicted student ability (in student σ) between ZOC and non-ZOC students relative to the year before the expansion. Estimates of $\hat{\theta_1}$, $\hat{\theta}_2$, $\hat{\theta}_3$ are denoted by Pre-trend, mean shift, and post-trend, respectively. The p-value from a Chi-squared test with seven degrees of freedom testing the models parametric restrictions is reported. Standard errors were estimated using the delta method using the variance covariance matrix of the non-parametric event-study coefficients $\hat{\beta}$. 95 percent confidence intervals are displayed in the shaded regions.

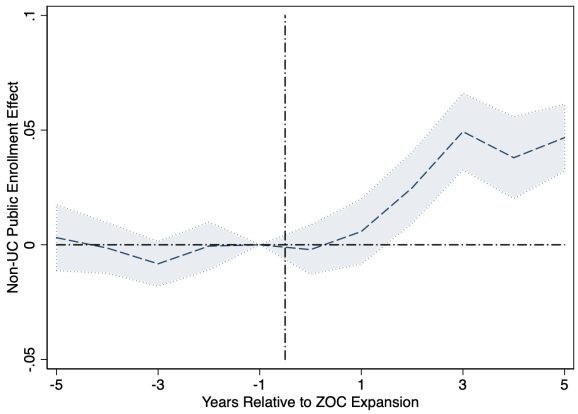
D.3 Additional College Enrollment Estimates

Figure D.6: College Type Event Studies

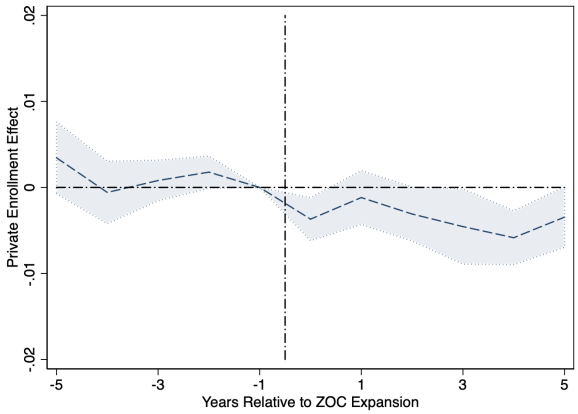
(a) University of California Campuses



(b) California State University Campuses

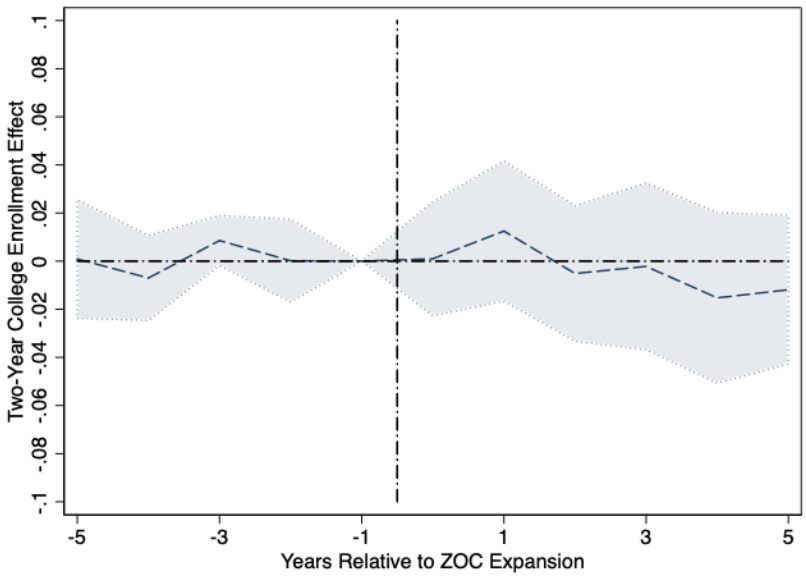


(c) Private Universities



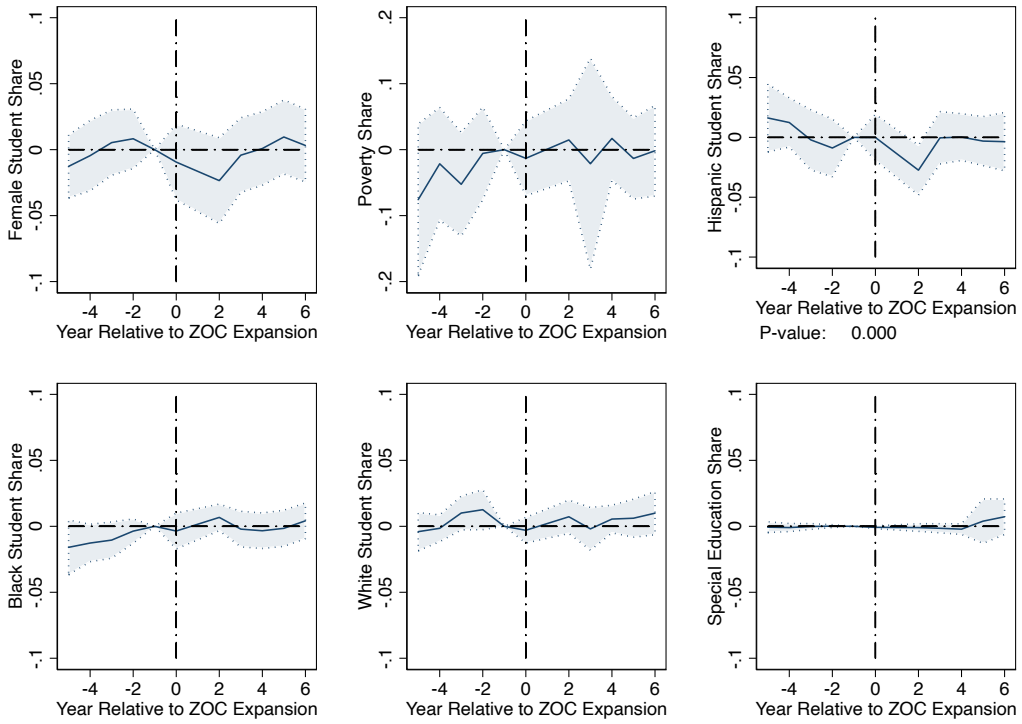
Notes: This figure plots the estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-difference estimates on outcomes relative to the year before the policy. Panel A reports treatment effects on UC college enrollment, Panel B reports estimates on enrollment in CSU campuses, and Panel C reports estimates on private university enrollment. Standard errors are double clustered at the school and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure D.7: Two-year College Enrollment Effects



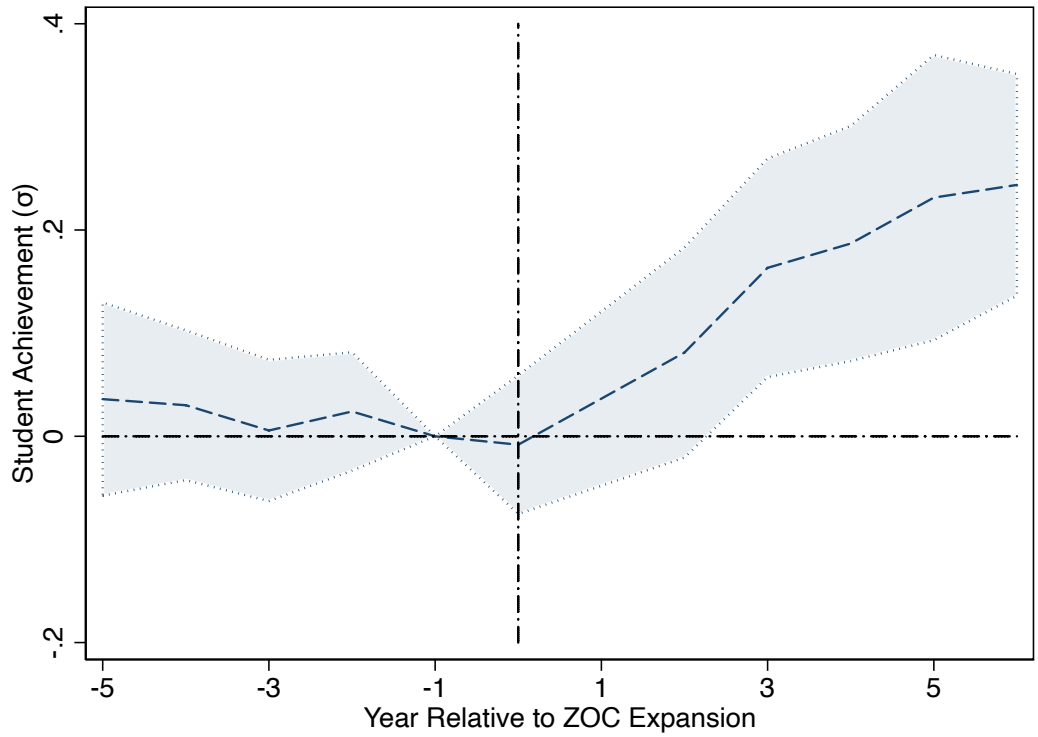
E Robustness Exercises

Figure E.1: Changes in student demographics



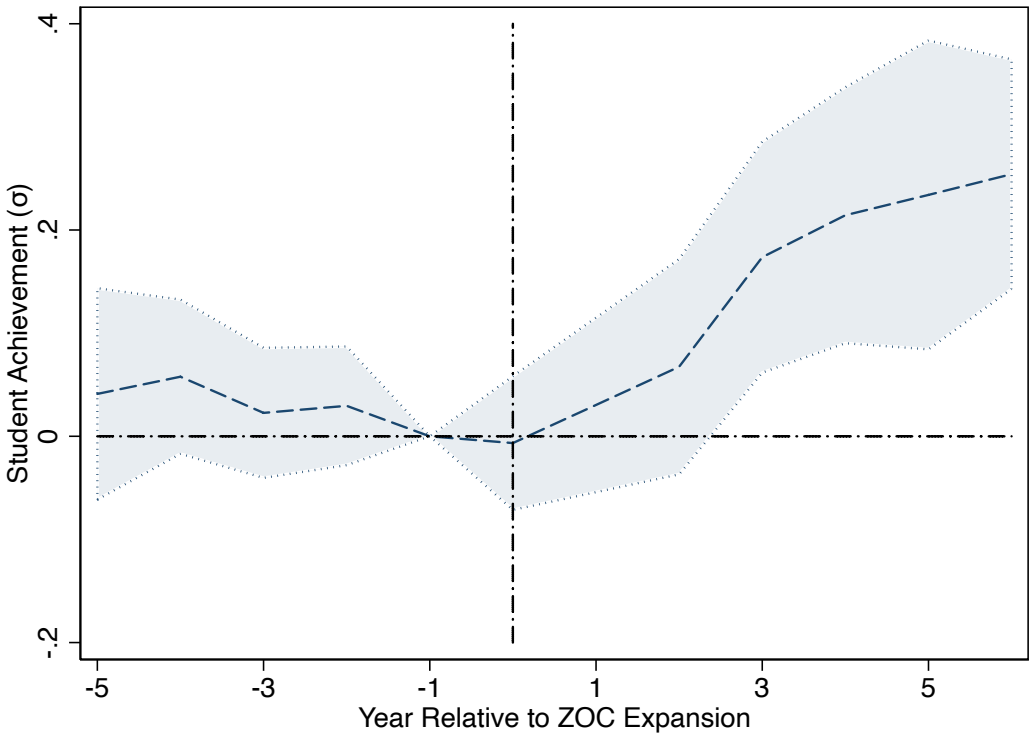
Notes: This figure reports estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in the change of student characteristics, labeled on subfigure vertical axes, between ZOC and non-ZOC students relative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

Figure E.2: Achievement event-study restricted to students who didn't move in eighth grade



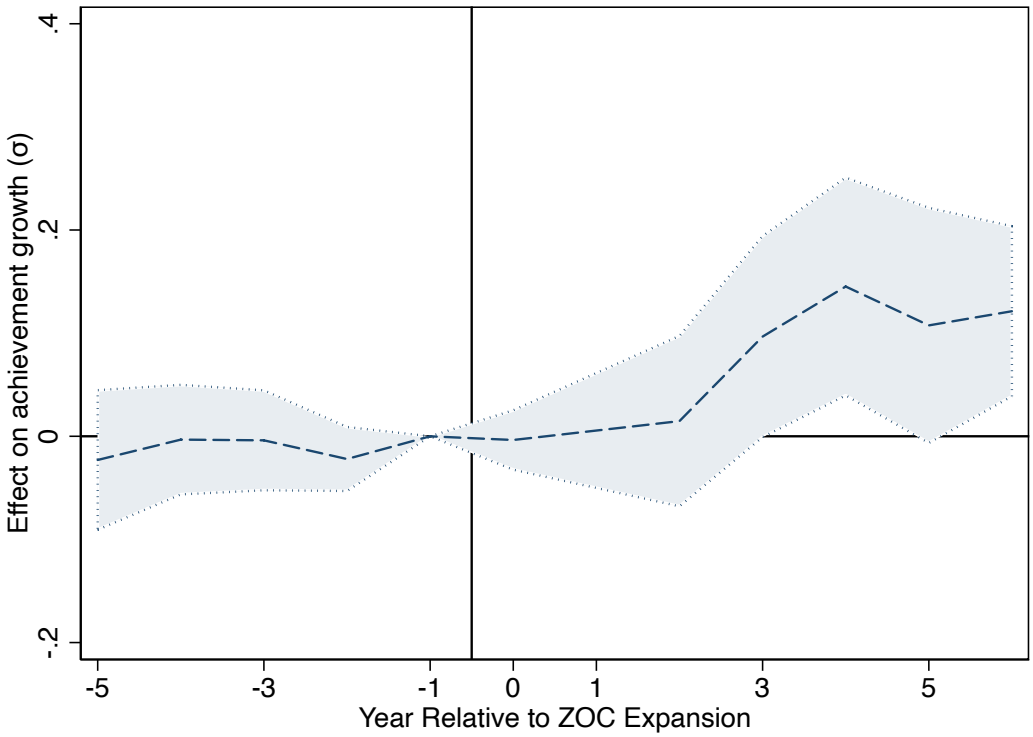
Notes: This figure reports estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The sample is restricted to students that did not move in eighth grade, the year before submit ZOC applications. The coefficient β_k shows the difference in changes 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 and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure E.3: Achievement event study restricted to students who didn't move in middle school



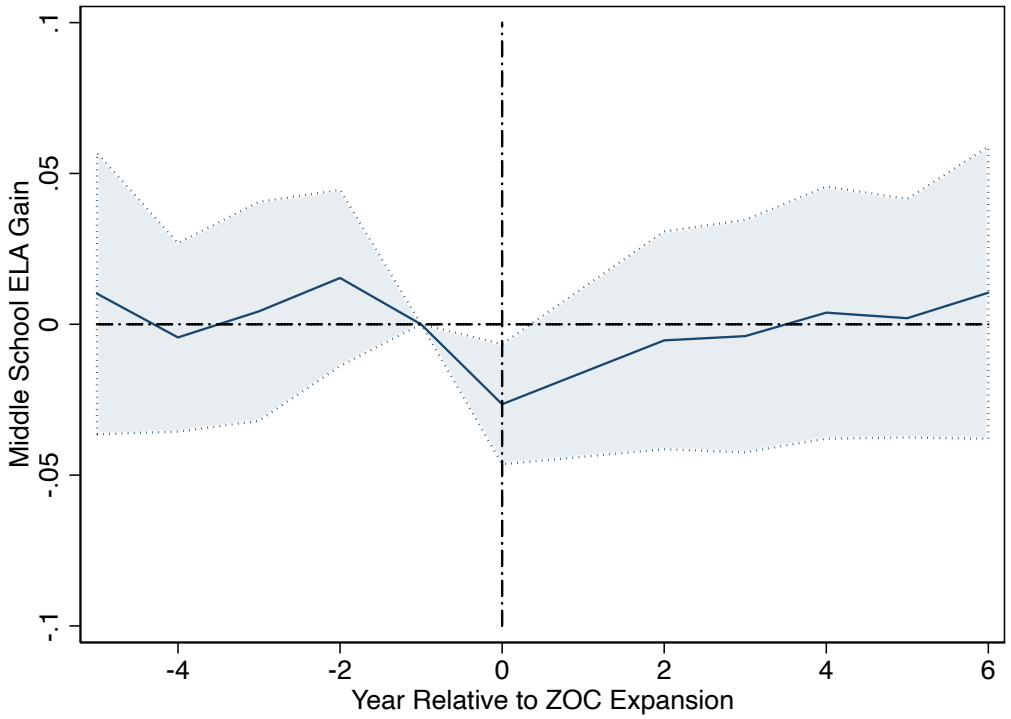
Notes: This figure reports estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The sample is restricted to students that did not move in eighth grade *and* also did not move at anytime during middle school. The coefficient β_k shows the difference 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 and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure E.4: Within-student achievement gain



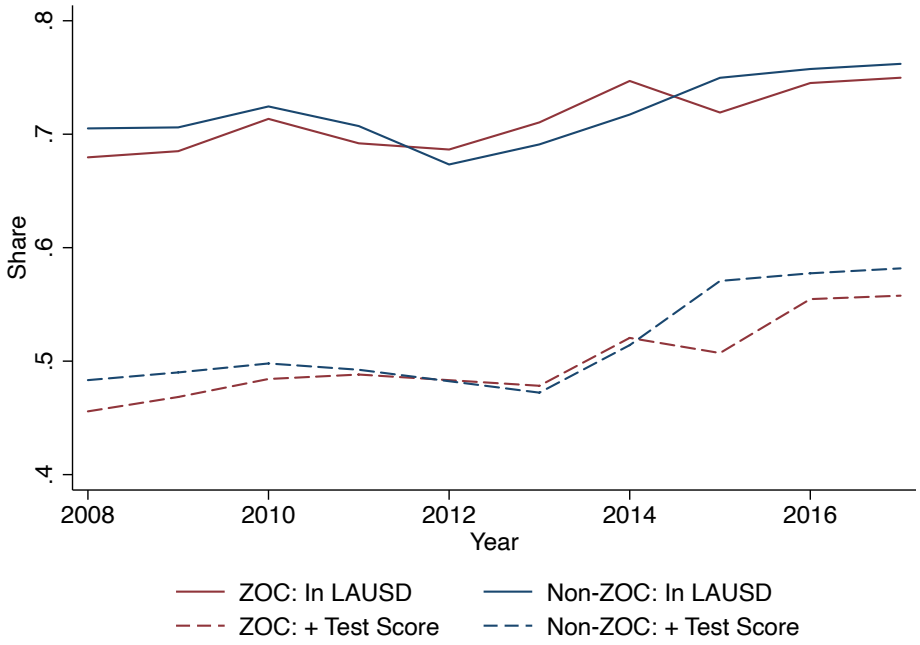
Notes: This figure reports estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The outcome is student-level achievement growth between eighth and eleventh grade, measured in student achievement standard deviations. The coefficient β_k shows 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 and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure E.5: Falsification Test - ZOC Impact on Middle School Gains

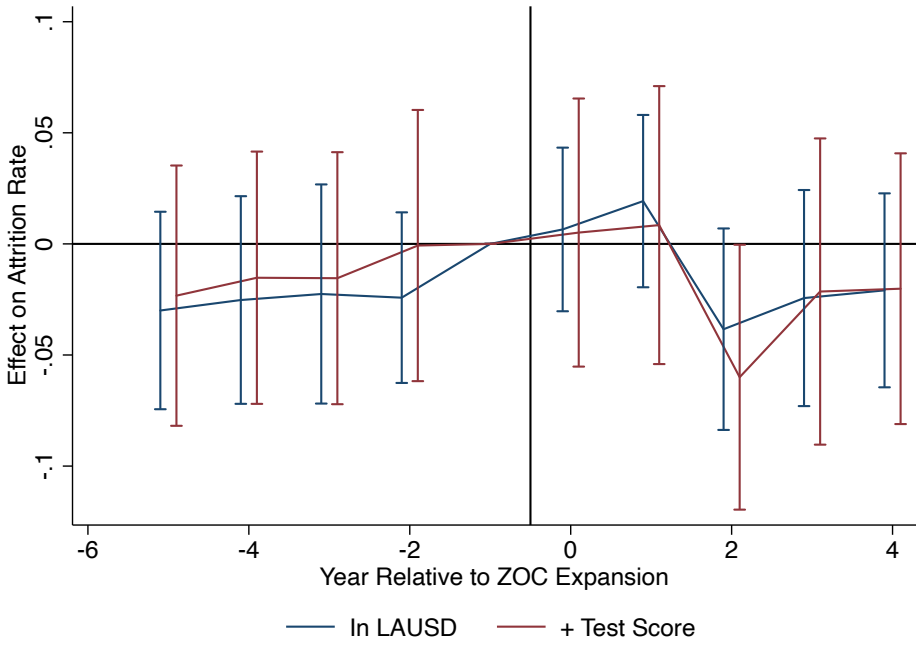


Notes: This figure reports estimates of β_k analogous to those defined in equation 2, where k is the number of years since the ZOC expansion. The outcome is student-level achievement growth between seventh and eighth grade, measured in student achievement standard deviations, and predating their ZOC 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 and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure E.6: Attrition Estimates



(a) Trends in Attrition Rates



(b) Attrition Event-Study Estimates

Notes: This set of figures explores non-random 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) .

F Estimating Counterfactual Distributions

In this section, we discuss the methods used to estimate the counterfactual distributions used to construct quantile treatment effects in Figure 6b. These methods come from Chernozhukov et al. (2013) and Chernozhukov et al. (2020), and the decompositions are in spirit of Oaxaca (1973) and Blinder (1973).

First, we outline the notation we use to construct counterfactual distributions that guide the rest of the empirical analysis. Let $F_{kkt}(a)$ to 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 correspond 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 they 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 CDF at different quantiles and taking the difference.

Below, we consider a few additional exercises that take a deeper dive into student-level achievement effects. Figure F.1 displays the observed ZOC achievement cumulative distribution function (CDF) and the counterfactual that assigns ZOC students to the non-ZOC conditional distribution. We find a rightward shift in the distribution at most points of support below one standard deviation, indicating positive treatment effects at these points of support. In other words, the probability of ZOC students scoring on or below these points decreased.

To further explore these changes, we next consider an exercise analogous to a difference-in-differences design but using the estimated counterfactual distributions. Specifically, we ask: what is the effect of changing the conditional distribution in the pre-period, and similarly, in the post-period? The former checks whether we detect treatment effects in the pre-period, while the latter checks for treatment effects in the post-period. In particular, we can decompose the observed change into these components in the following way:

$$\Delta^F = F_{zz1} - F_{zz0} \tag{11}$$

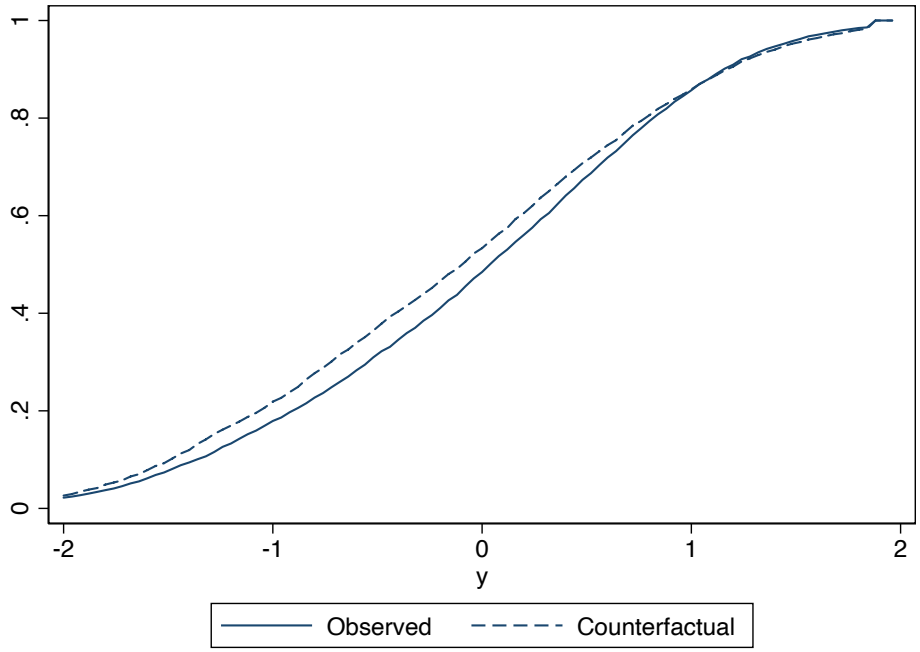
$$= \underbrace{(F_{zz1} - F_{nz1}) - (F_{zz0} - F_{nz0})}_{\Delta_F^{DD}} + (F_{nz1} - F_{nz0}). \tag{12}$$

Equation 12 shows that we can express the change in the ZOC student achievement distribution as an effect analogous to a distributional difference-in-differences Δ_F^{DD}, differencing post-period differences with pre-period differences and an additional term capturing counterfactual changes

in ZOC achievement.

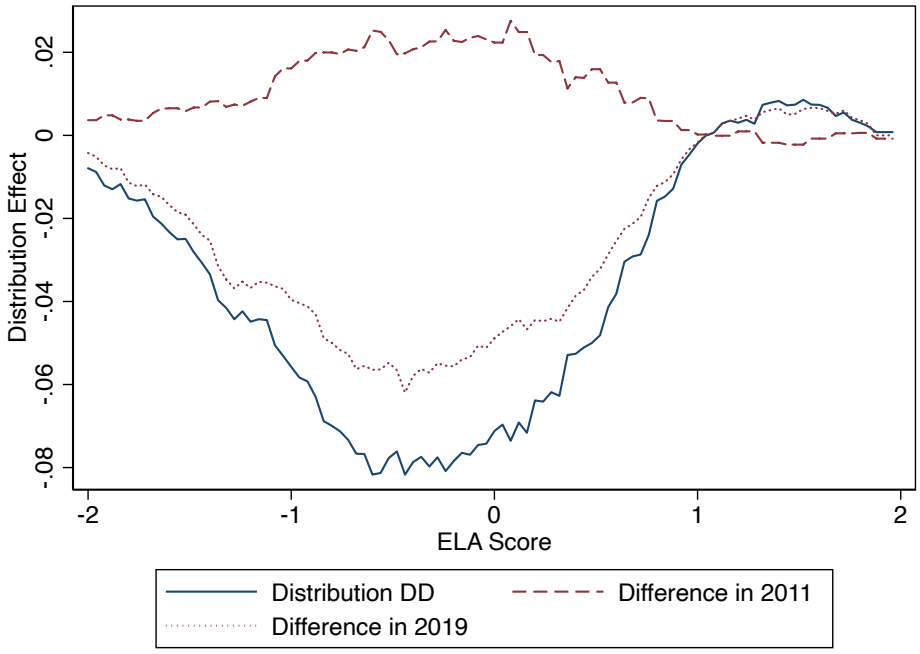
Figure F.2 reports distribution effects at each point of the distribution's support. The dotted line shows distributional effects in 2019—the difference in CDFs in Figure F.1—and the dashed line reports distribution effects in 2011. The distribution effects in the pre-period hover near zero across most points of support as would be expected before the policy. In the post-period, we observe negative distribution effects at pointes below one standard deviation. The solid line plots the implied distributional difference-in-differences estimate at each point. For example, the distribution effect at 0 is roughly -0.07 indicating the probability that student achievement was less than one standard deviation decreased by 7 percentage points; such a decrease indicates moving a mass of students scoring below average to above average. Importantly, we do not find evidence of treatment effects in the upper end of the distribution indicating the gains in the bottom did not come at the expense of high-achieving students.

Figure F.1: Empirical and Counterfactual CDF for ZOC students in 2019



Notes: This figure reports the observed and counterfactual student achievement distribution for 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 F.2: Distribution Effects



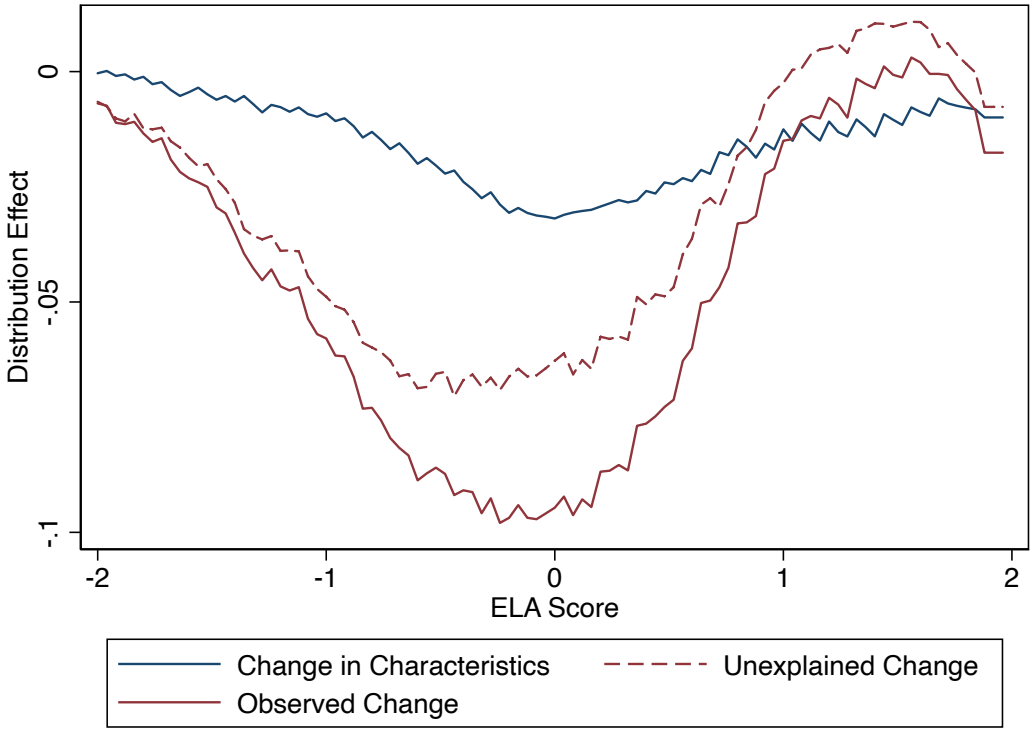
Notes: This figure reports various components of the change in the observed ZOC student achievement distribution between 2011 and 2019. The dashed maroon line reports the difference between the observed and counterfactual ZOC student achievement distribution before the ZOC expansion in 2011, while the dotted maroon line reports the difference after the program's expansion in 2019. The solid blue line is the difference between the dotted and dashed maroon lines and corresponds to a distributional difference-in-differences estimate at each point of the distribution's support. Counterfactual distributions are 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).

An alternative approach is to decompose the observed change in the sectoral achievement gap—ZOC sector versus non-ZOC—as

$$(F_{zz1} - F_{nn1}) - (F_{zz0} - F_{nn0}) = \underbrace{(F_{zz1} - F_{zn1}) - (F_{zz0} - F_{zn1})}_{Changes\ in\ student\ characteristics} + \underbrace{(F_{zn1} - F_{nn1}) - (F_{zn0} - F_{nn10})}_{Unexplained\ change}.$$

In this decomposition, one portion of the change in the gap is due to changes in student characteristics while the unexplained changes would be attributable to the ZOC program. Figure F.3 reports these estimates. Although we find some evidence that changes in student characteristics contributed somewhat to changes in the upper regions of the distribution, the overwhelming share of the changes are due to the ZOC program.

Figure F.3: Decomposition of the change in the sectoral achievement gap



G Model estimates

G.1 Achievement model estimates

To estimate the parameters of the decomposition, we rely on a selection on observables assumption and estimate Equation 5 via OLS. Table G.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 $.34\sigma$ and 0.6σ at other schools. We don't find meaningful mean differences between ZOC and non-ZOC schools in the β_j. The standard deviation 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's 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-period. Appendix Table G.2 reports the estimates, but we do not find evidence that there were meaningful changes induced by the policy for most characteristics. Figure L.6 displays the estimated value-added distributions for both ZOC and other schools in both the pre- and post-period. The estimated distributions provide suggestive evidence that ZOC value-added improved relative to non-ZOC value-added.

Table G.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 school estimates and Columns 3 and 4 show other LAUSD high school estimates; Column 5 reports their difference. Standard errors reported in parentheses.

Table G.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)

G.2 Utility Model Estimates

Table G.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.

H 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.

H.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 H.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 fail to reject the null hypothesis that all differences are jointly zero.

Another threat to internal validity is non-random 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 would be biased due to non-random 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 H.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 H.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 columns 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 H.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. Column 2 and 3 report subject-specific shares of applicants with Math and ELA scores in eleventh grade, respectively. Column 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 H.1. Standard errors, reported in parentheses, are clustered at the lottery level.

H.2 Test for VAM Bias

We use the procedure outlined by Angrist et al. (2017) to test for bias in our value-added 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 $Z_{i\ell}$ we collect across zones and cohorts

$$A_i = \xi + \phi \widehat{A}_i + \sum_{\ell} \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i' \delta + \varepsilon_i \tag{13}$$

$$\widehat{A}_i = \psi + \sum_{\ell} \pi_\ell Z_{i\ell} + \boldsymbol{X}_i' \xi + e_i. \tag{14}$$

If lotteries shift VAM predictions in proportion to their 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 if this applies to each lottery, testing the predictive validity of each lottery.

Table H.3 reports results for three value-added models. Column 1 reports results for a model that omits any additional covariates beyond school by year dummies, the uncontrolled model. As discussed in Angrist et al. (2017), Chetty et al. (2014a), Deming et al. (2014) models that don't adjust for lagged achievement tend to perform poorly in terms of 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 where school effectiveness did 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 5. The forecast coefficient is essentially one and the p-value on the overidentification test fails to reject. One remaining concern is many weak instrument bias that would bias the forecast coefficient to the corresponding OLS estimates. The first-stage F-statistic is roughly 12, passing the rule of thumb test. Evidence notwithstanding, we report the reduced form estimates and first stage estimates in Appendix Figure H.1 corresponding to the overidentifcation test. While the results in Table H.3 don't imply the OLS value-added estimates are free from bias, they are reassuring moving forward.

Table H.3: Forecast Bias and Overidentification Tests: 2013-2017 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 zone of choice. 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 5. 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.

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

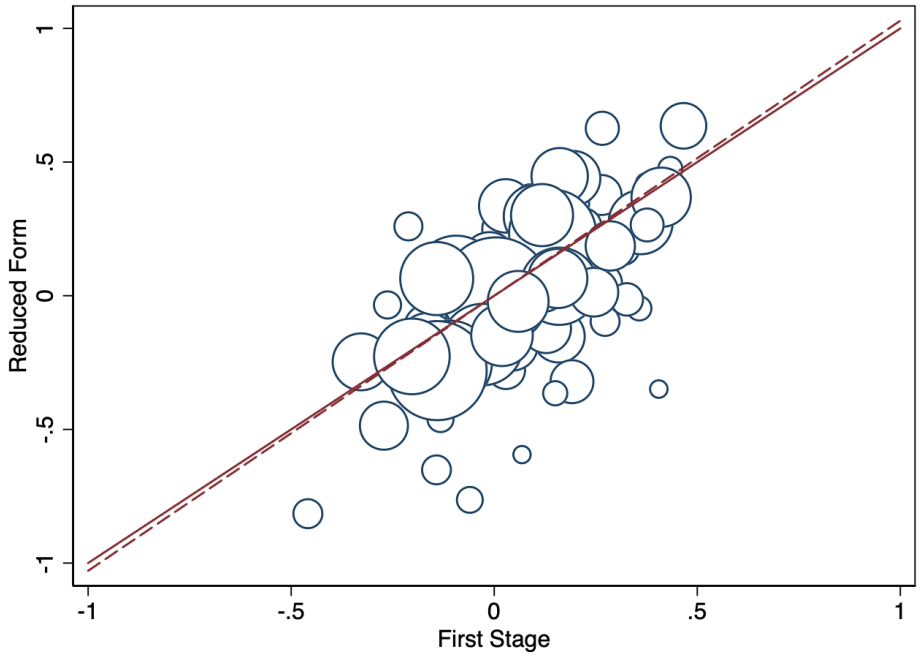


Table H.5: Complier characteristics by cohort

	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 H.4: 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

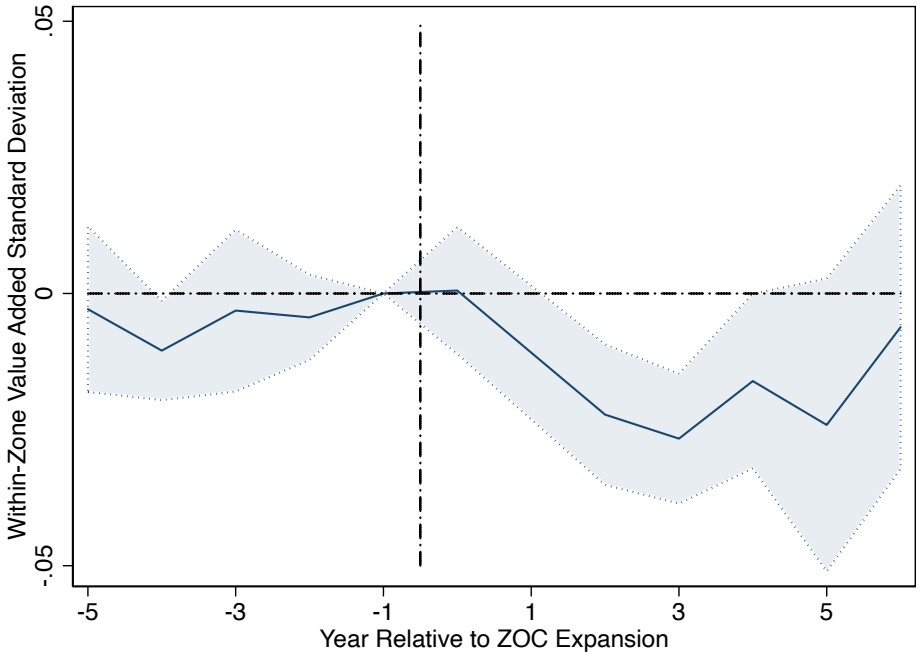
Notes: This table lists all the schools appearing in the lottery sample and the number of lotteries.

I Additional Empirical Results

I.1 More on the compression of within-zone school quality

To further investigate the distributional changes, Figure I.1 estimates treatment effects on the the within-zone standard deviation of school effectiveness.[24] We find that the standard deviation of ZOC school effectiveness across zones decreased by roughly 0.02 student achievement σ. To put this treatment into context of the literature and our setting, many papers find a standard deviation in teacher or school effectiveness of roughly 0.1 student achievement σ (Chetty et al., 2014a, Deming et al., 2014, Rivkin et al., 2005), and in some settings roughly 0.2 (Angrist et al., 2017, Walters, 2015). In our estimates, one standard deviation of school effectiveness amounts to 0.15σ. The treatment effects on the within-zone standard deviation are thus approximately 13 percent of a standard deviation in the school effectiveness distribution. The relative decrease in school inequality within zones compared to the rest of the district is large and a point we explore further in the lottery analysis.

Figure I.1: Within-zone value-added dispersion event-study



Notes: This figure reports event-study coefficients for models regressing estimated within-zone ATE standard deviations as the outcome on event-time indicators. Non-ZOC schools are grouped into a single zone. Zones are weighted by the number of students in the zone. Standard errors are clustered at the zone level and 95 percent confidence intervals displayed as shaded regions.

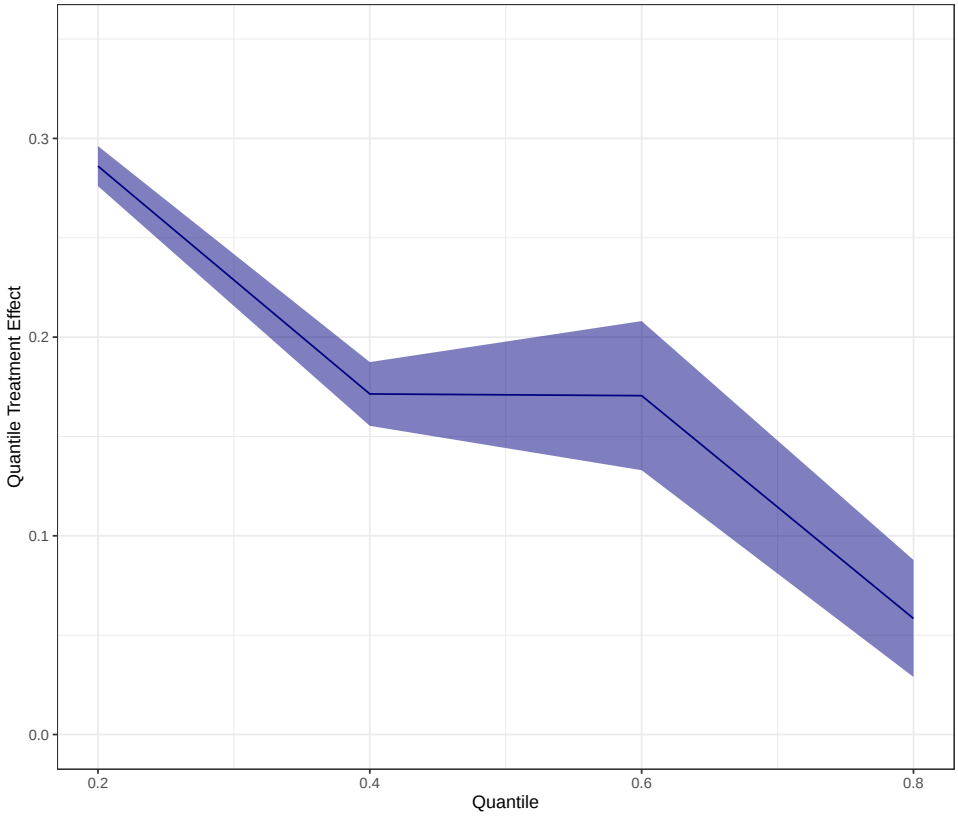
[24] For a given zone z in year t, an estimator of the variance of α_{jt} is given by

$$\sigma^2_{\alpha zt} = \frac{1}{J_z} \sum_{j \in z} \left((\hat{\alpha}_{jt} - \bar{\alpha}_{zt})^2 - SE(\hat{\alpha}_{jt})^2 \right).$$

I.2 Alternate Quantile Treatment Effects

An alternate approach to estimating quantile treatment effects is proposed by Athey and Imbens (2006) and has been used to estimate non-linear difference-in-difference quantile treatment effects in the literature (Finkelstein and McKnight, 2008, Havnes and Mogstad, 2015). Figure I.2 reports changes-in-changes estimates of school effectiveness at various quantiles. We find a weakly decreasing treatment effect in the quantile location of school effectiveness, indicating the lower tail of the school effectiveness distribution improved more than the upper tails. Schools at the 20th percentile of the school effectiveness improved by roughly 30 percent of a standard deviation relative to the improvement of non-ZOC schools, while schools at the 80th percentile improved by roughly 6 percent of a standard deviation. These results are qualitatively similar to the treatment effects we report in Figure 6b using the methods proposed by Chernozhukov et al. (2013).

Figure I.2: Quantile Treatment Effects



Notes: This figure reports quantile treatment effects on school ATE. Counterfactual distributions and standard errors are derived as in Athey and Imbens (2006). 95 percent confidence intervals reported in shaded regions.

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.[25] 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 one if a student is exposed to a same-race teacher in each core ELA course in each year between ninth and eleventh grade.[26] 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 not due through an increased pool of same-race teachers, but rather, a potential within-school change in the way students were assigned to teachers.

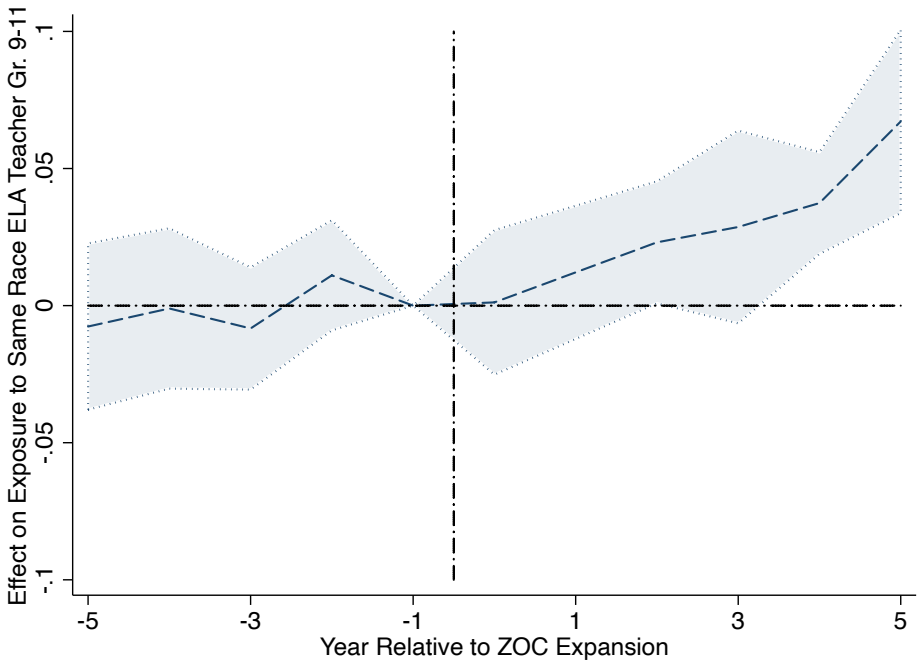
Impacts of same-race teachers have been shown to produce both short- and long-run improvements in outcomes for underrepresented racial minorities (Dee, 2004, Fairlie et al., 2014, Gershenson et al., 2018). In particular, Gershenson et al. (2018) find that Black students randomly assigned a Black teacher in the STAR experiment were four 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 is discussed 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.

[25] We have course level data for one less year so our analysis dependent on these data cover one less year.

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

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 ZOC expansion. The outcome variable is an indicator equal to one if a student is exposed to a same-race teacher in a core ELA course in each year between grades 9 to 11. Standard errors are double clustered at the school and 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 it 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).[27] 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

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

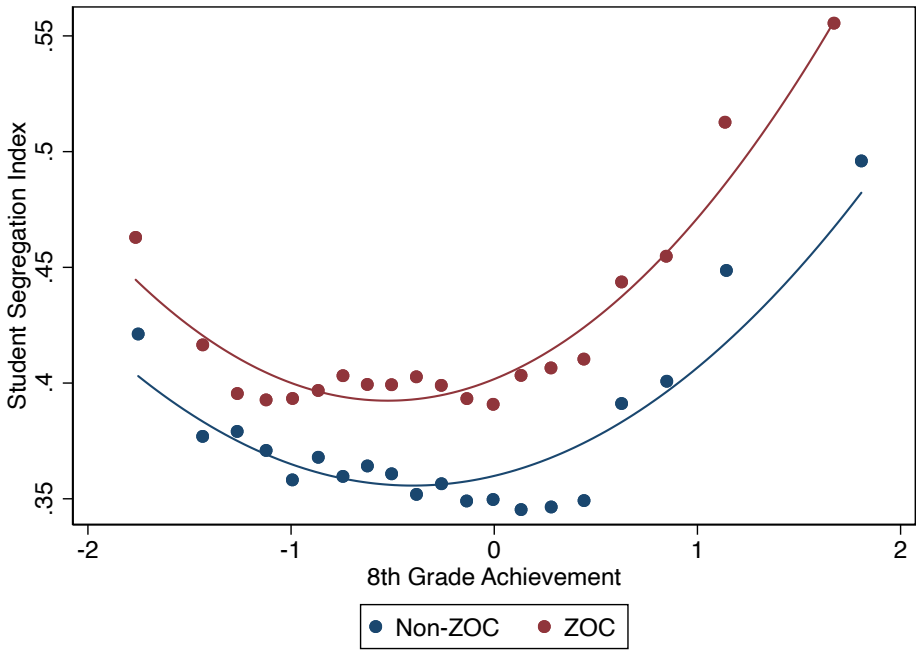
where $f(A^8_{it})$ is a polynomial in students' incoming achievement and μ_{jt} are school by year effects indicating this model is identified from changes in the *within-school-cohort* segregation gap between students with incoming achievement A_{it} and those with $A_{it} = 0$. Therefore, $\beta'_A \times f(A^8_{it})$ captures the causal impact of ZOC on the within-school segregation gap between students with incoming achievement A^8_{it} 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. In the first few post-periods, we also do not not detect any differen-

[27] 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.

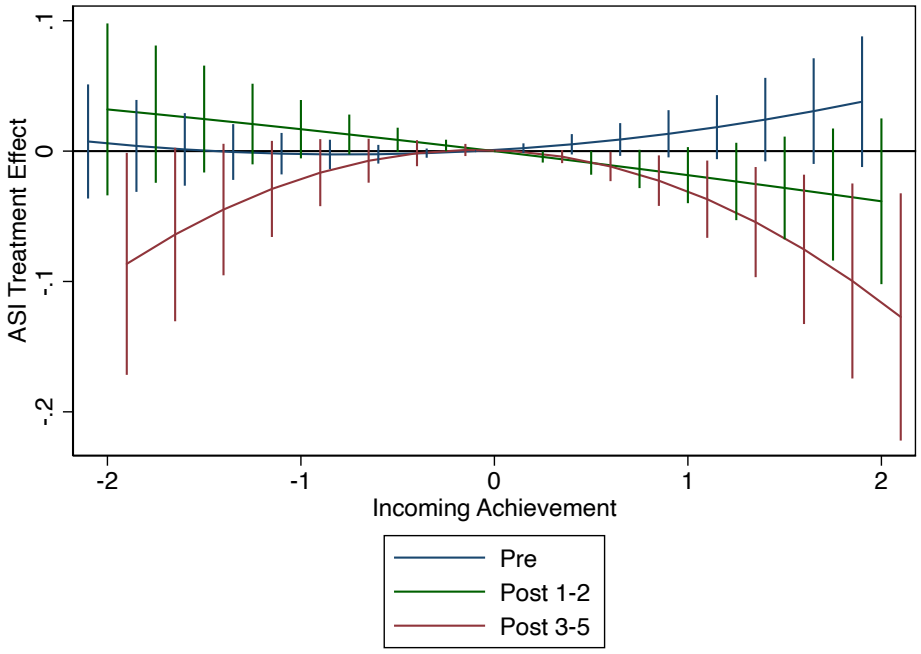
tial changes in within-school segregation gaps but do observe them in the later post-periods. In particular, we find that segregation gaps decreased for both high and low-achieving students, suggesting ninth-grade classrooms became more integrated in terms of students' incoming achievement. The literature 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 finds we don't 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 both 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 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 ZOC dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the school level.

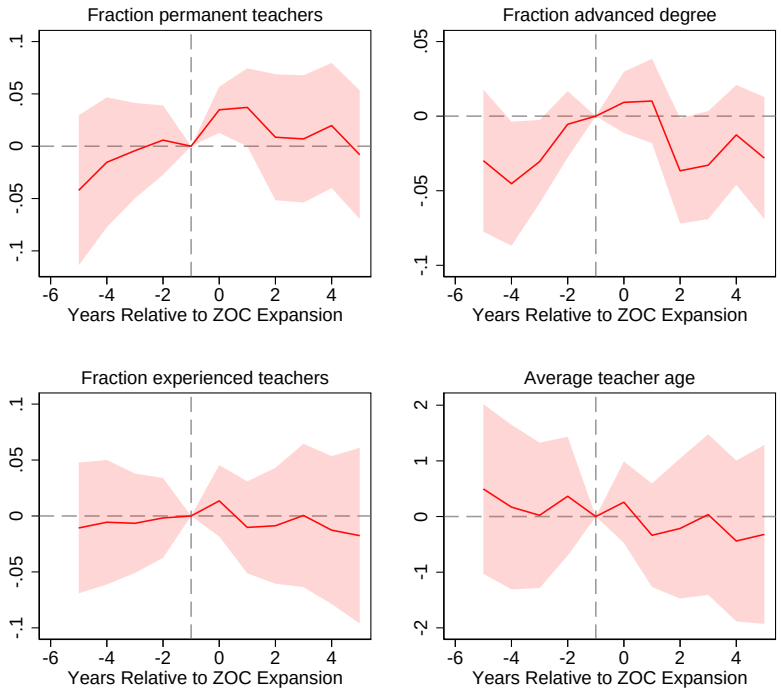
K.1 Changes in school inputs

Variation in schooling inputs and practices explain 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 that 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 or quality of teachers did not change between the two sectors.[28] 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.

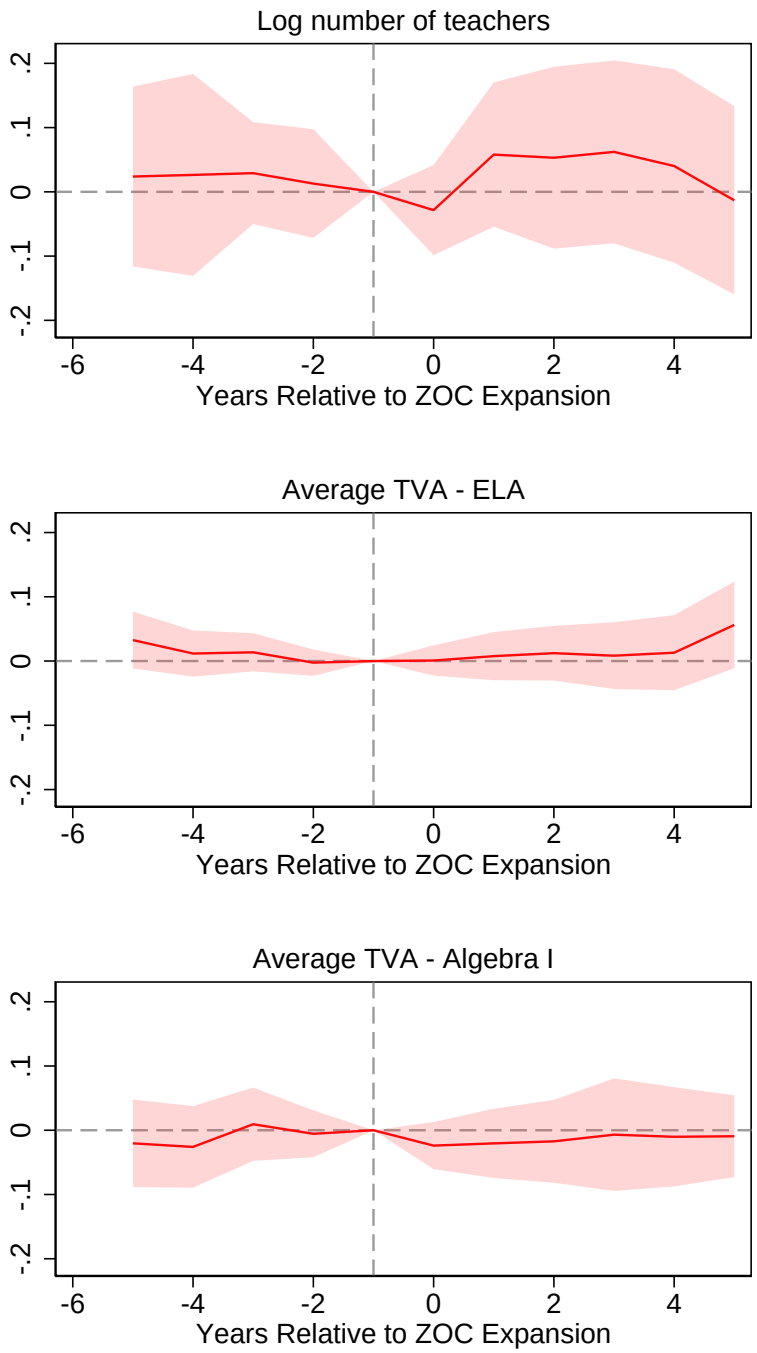
[28]We estimate within-school teacher value-added in the pre-period and track changes in teacher quality with respect to the baseline estimate teacher value-added.

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

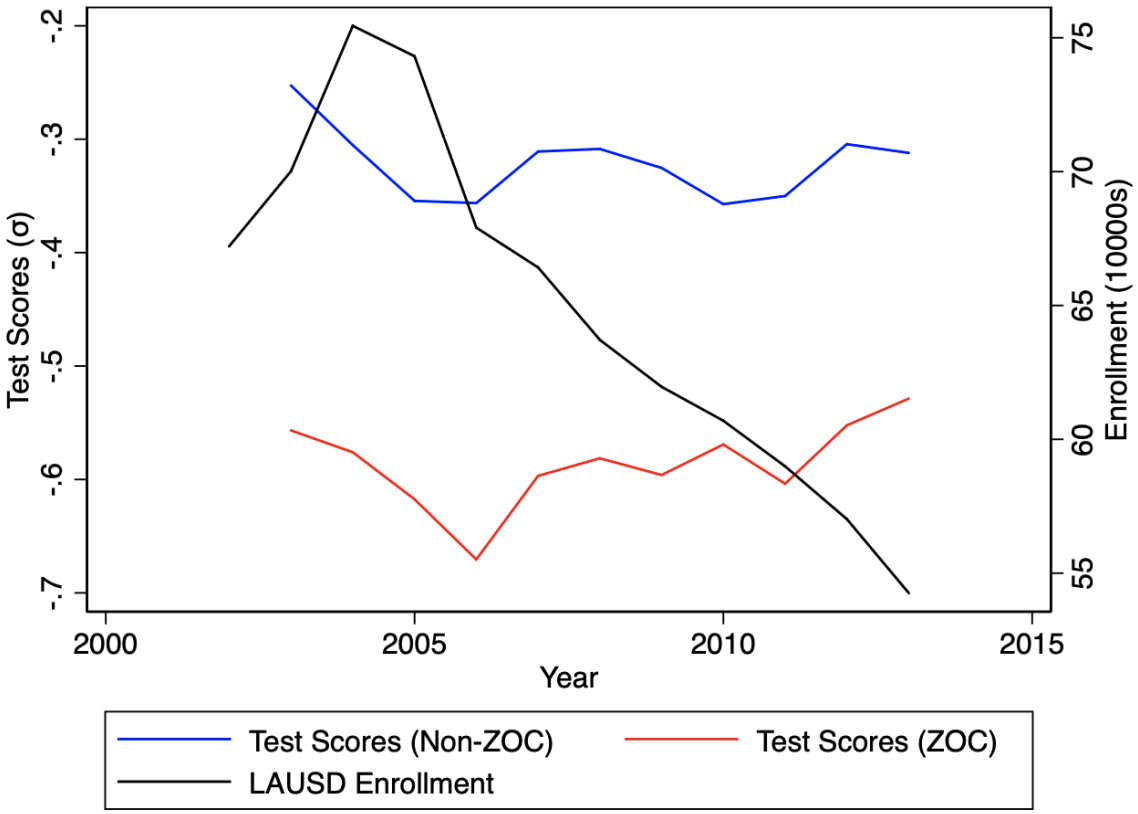
Figure K.4: Teacher Quantity and Quality Event Studies



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

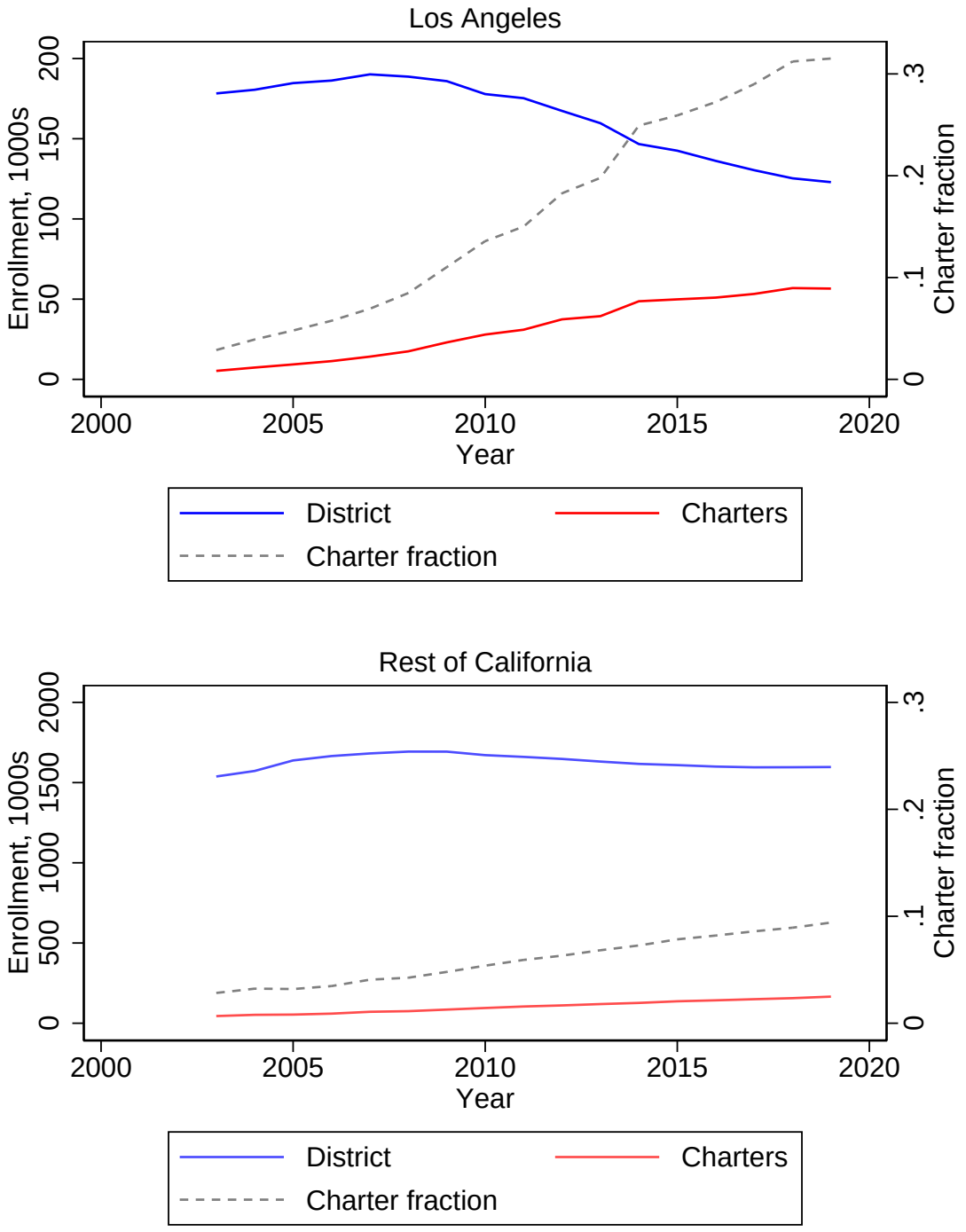
L Additional Empirical Results

Figure L.1: LAUSD: 2002-2013



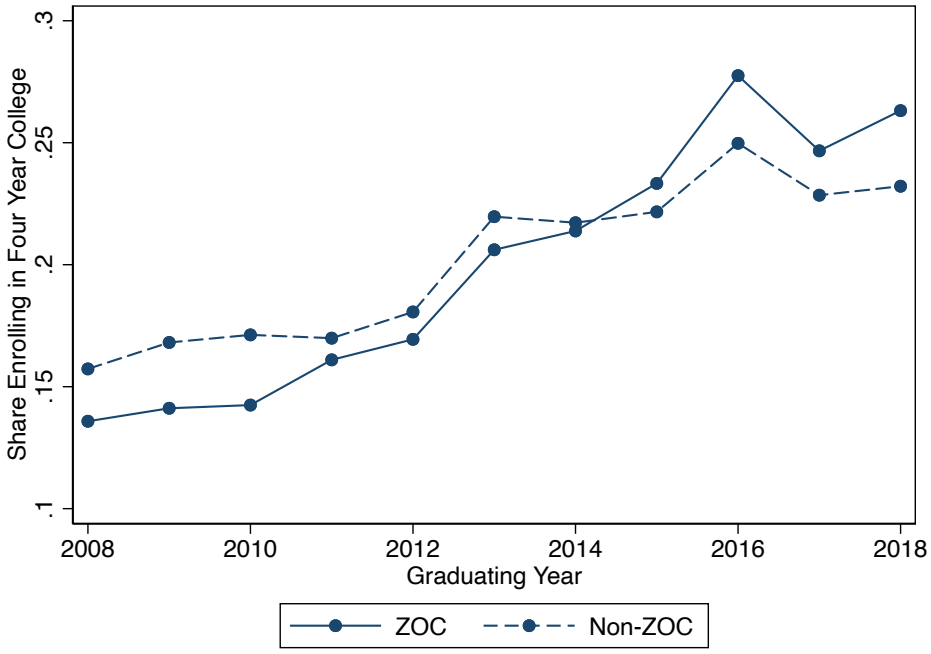
Notes: Enrollment numbers come from admistrative data provided by LAUSD. The California Department of Education provides California Standards Test (CST) statewide means and standard deviations which we use to standardize test scores in this figure. Test scores are ninth grade ELA scores, an exam that is uniform across schools and students.

Figure L.2: Los Angeles and California enrollment

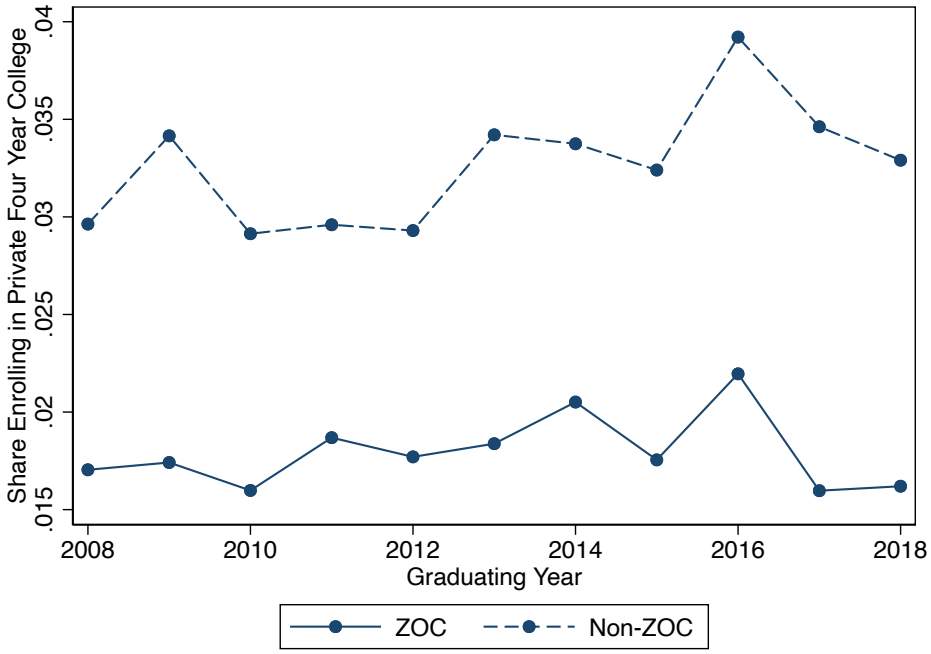


Notes: This figure shows enrollment in thousands for grades 9 through 12, separately for district and charter schools. Enrollment data is from the California Department of Education.

Figure L.3: College Outcomes



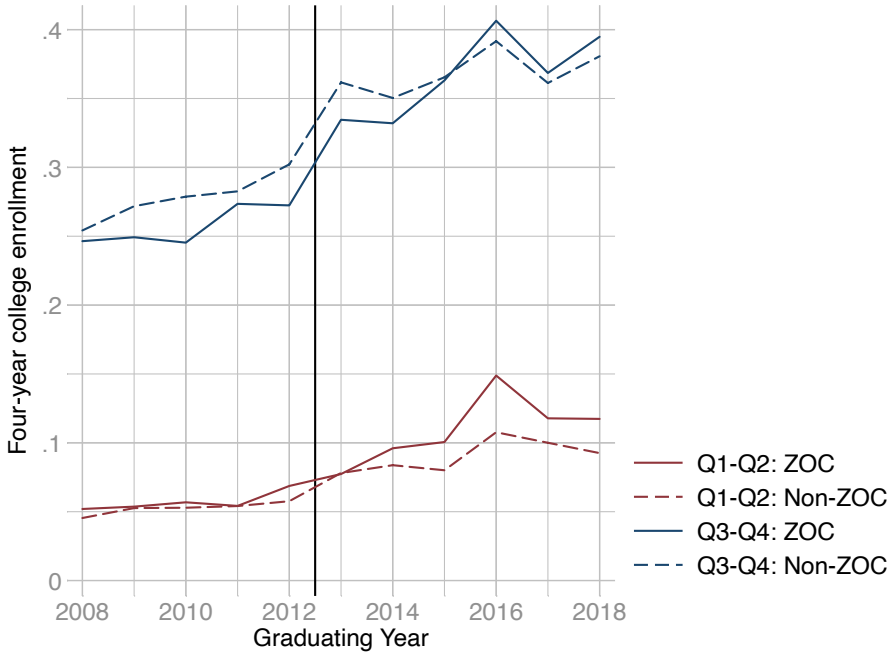
(a) Four-year college enrollment



(b) Private college enrollment

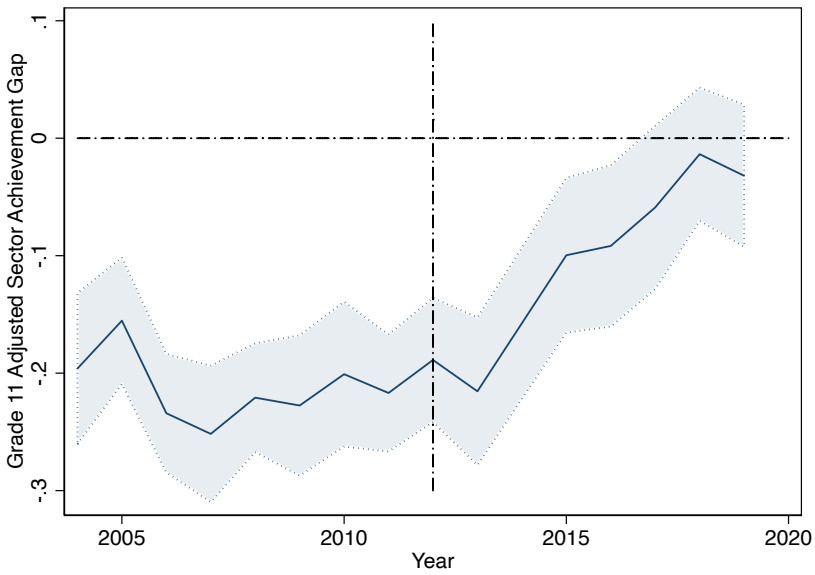
Notes: These figures report four-year college enrollment rates reported to the NSC for multiple graduating cohorts. Panel A reports college enrollment rates at any four-year college and Panel B reports enrollment rates at private colleges.

Figure L.4: Four-year college enrollment rates by predicted quartile group



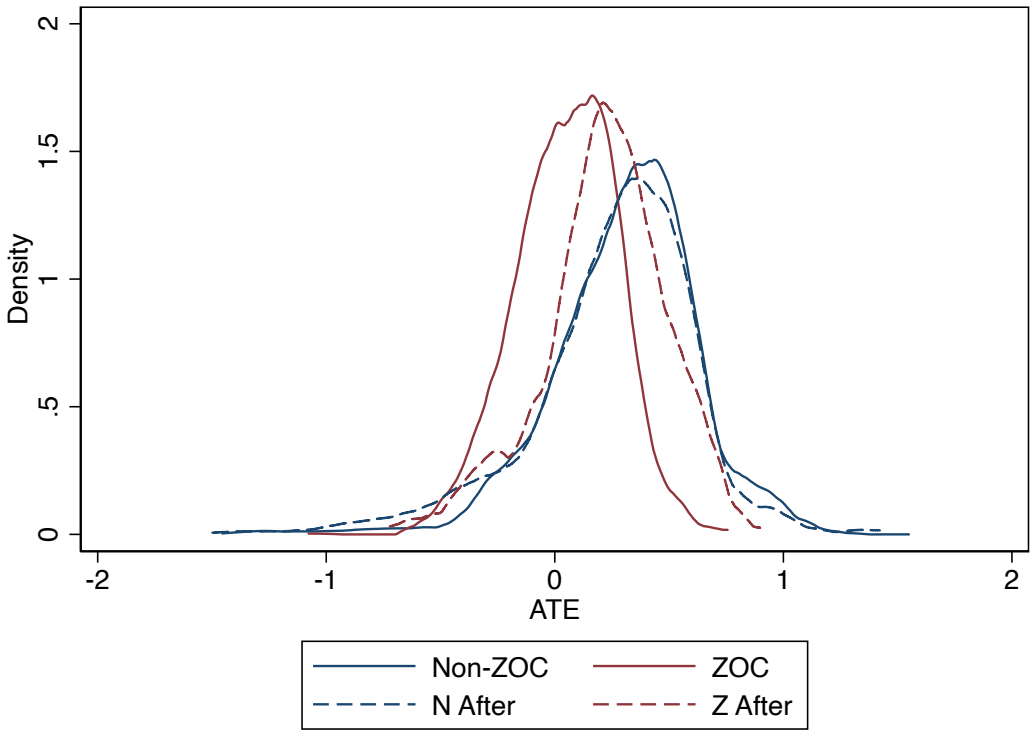
Notes: This figure reports college enrollment rates for students in different quartile groups by ZOC and non-ZOC student status. Solid lines correspond to ZOC students and dashed lines correspond to non-ZOC students. Red lines correspond to students in the bottom two quartiles of the predicted college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

Figure L.5: Eleventh-grade ZOC achievement gaps



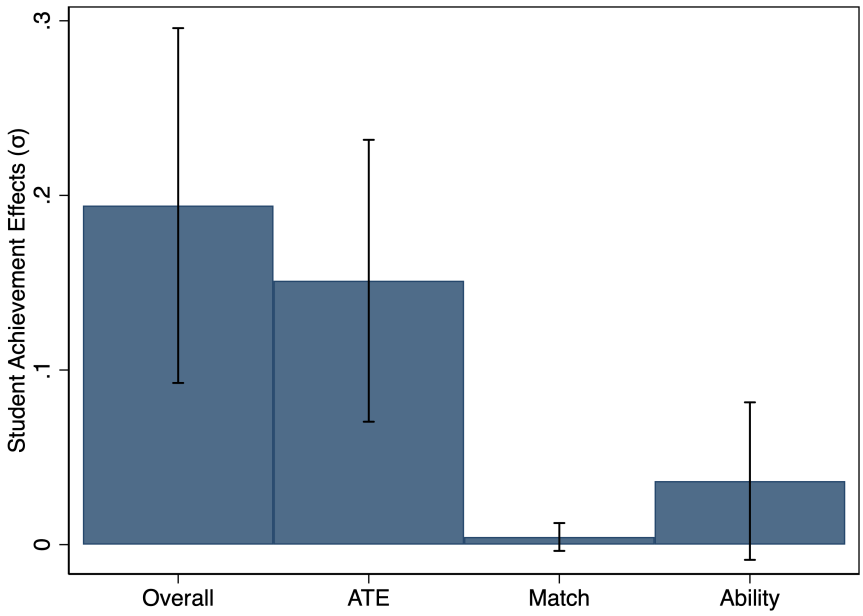
Notes: This figure reports estimates from regressions of student achievement on ZOC indicators interacted with year dummies, adjusting for student characteristics. We report estimates of achievement gaps in the solid lines with 95 perfect confidence intervals reported by shaded regions.

Figure L.6: ATE Distributions before and after ZOC expansion



Notes: This figure reports the ZOC and non-ZOC school ATE distributions before and after the policy comes into place.

Figure L.7: Achievement Effect Decomposition at Year 6



Notes: This figure reports estimates of β_6 defined in equation 2 for four outcomes. Overall, corresponds to a model using student achievement as an outcome not adjusting for lagged test scores; ATE corresponds to a model using estimated ATE as the outcome; match corresponds to a model using estimated match effects as the outcome; and ability corresponds to a model using student predicted ability as the outcome. Each model adjusts for the same covariate of characteristics in all event studies. The coefficient β_6 shows the difference in the change in achievement between between ZOC and non-ZOC students relative to the year before the expansion. Standard errors are double clustered at the school and year level and reported by black lines.

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