Essays on the School Choice, the Distribution of School Quality, and Preferences

by

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Abstract

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In the United States, there exist both overt and covert barriers limiting some students' access to high-quality schools, creating systematic inequities that affect labor market outcomes. Insurmountable challenges in dismantling these barriers have led policymakers to explore the capacity for school choice reforms as a means to improve student outcomes. The decentralized nature of public education in the United States has allowed for wide experimentation across states and districts, but the heterogeneous implementation of policies has also generated mixed results and many open questions. This dissertation contains three essays that aim to tackle some of these open questions.

In chapter 2, I outline a conceptual framework that provides some structure for subsequent chapters. The purpose of this chapter is present a benchmark model where the typical benefits that motivate contemporary school choice reforms materialize. The contributions of this chapter are to generate an empirically-oriented model at the cost of several simplifying assumptions. Other models in the literature take an opposite approach, but the primary contributions of this dissertation are empirical so I opt for an empirically-oriented theoretical framework. I link this model to the subsequent chapters in two ways. First, a key statistic that summarizes families' expected welfare gains from the program is an important covariate for the empirical analysis in Chapter 3. Second, some of the simplifying assumptions regarding perfect information and preferences receive cursory treatment in Chapter 3 and are the focal point of a field experiment discussed in Chapter 4.

In chapter 3, co-authored work with Caitlin Kearns, I study how the demand and supply for school quality can evolve in response to a policy—the Zones of Choice (ZOC) program–that changed the underlying choice and competitive structure that families and schools face. The contributions of this chapter are to provide empirical evidence tackling the questions surrounding access, competition and student-school match quality. I find that the achievement among students enrolled in ZOC schools increases, with gains sufficiently large to eliminate eleventh-grade achievement gaps that existed in the years before the policy expansion. I distinguish between effects driven by parents choosing schools that best suit their children's needs and competitive effects induced by the policy, and find most of the student gains are

driven by general improvements in school quality as measured by school value-added. The lack of improvements in student-school match quality are in part due to the homogeneous set of students within each zone, eliminating the scope for improvements in student-school match quality based on observable student characteristics. I find evidence that schools exposed to more competition (as captured by a statistic derived in Chapter 2) improved more, providing suggestive evidence for a competitive effects story. I then provide suggestive evidence that parent's selected schools in a way that incentivized schools to compete on quality, a finding that contrasts a growing body of evidence suggesting otherwise. The highly segregated nature of each zone of choice can maybe explain differences with the past literature. In settings where parents can use race or income to proxy for school quality, they will do so, but in segregated neighborhoods parents will use other proxies that are correlated with school effectiveness. This presents a tradeoff present in the ZOC setting: short-run gains in terms of reductions in within-district inequality in exchange for potentially negative long-run effects through the entrenchment of school segregation in the district.

Information and preferences are the focal point of Chapter 4, where I report the findings from a field experiment I conducted in Fall 2019. My contribution is to study parental preferences through an information provision experiment, linking the information provision literature with another growing body of research studying parents' preferences for schools. By distributing information to students' households I address the information channel, and in addition provide settings where some parents are perfectly informed about both school and peer quality, allowing me to study relative preferences for peer and school quality. In addition, I explore the role that social interactions play in determining schooling decisions. I corroborate my findings from Chapter 3 by showing that parents not receiving any information tend to place higher weight on school quality than peer quality. Among parents receiving information, I first document evidence that their most-preferred schools changed in terms of quality and peer composition. I then provide evidence showing that the information campaign led to increases in relative preferences for school value-added, suggesting information campaigns can produce changes in choices that affect school quality. Through various avenues, I provide evidence of large and salient treatment spillovers, indicating substantial interactions between parents. The prevalent role of social interactions could introduce disadvantages to groups with less informed networks in this and other settings.

To my mother, Jeene.

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Contents

List of Figures

List of Tables

Chapter 1

Introduction

In the United States, there exist both overt and covert barriers limiting some students' access to high-quality schools, creating systematic inequities that affect labor market outcomes (Neal and Johnson, 1996; Card and Krueger, 1992). Insurmountable challenges to dismantle these barriers have led policymakers to explore the capacity for school choice reforms as a means to improve student outcomes. The decentralized nature of public education in the United States has allowed for wide experimentation across states and districts, but the heterogeneous implementation of policies has also generated mixed results and many open questions. This dissertation aims to tackle some of these open questions.

Many choice reforms today are motivated by three potential benefits. First, expanding parents' options provides an escape hatch to students trapped in struggling schools. Second, providing parents the ability to choose from several schools allows them to sort their student in a school that best suits their needs, so choice alone can generate improvements in student-school match quality. Third, in settings where schools are responsive to enrollment declines, schools will compete with one another generating improvements among all schools, a tide that lifts all boats (Hoxby, 2003).

In practice, however, it remains unclear if students are equally impacted by these potential benefits. Choice alone is typically insufficient to generate positive impacts on student outcomes. For example, if parents lack the adequate information to make informed decisions (Hastings and Weinstein, 2008), or if certain parents face additional barriers or costs in acquiring information, then not all students will equally gain access to additional schools. The unequal access will in turn dampen the potential improvements in student-school match quality and also weaken the incentives schools have to improve. Another factor that can attenuate the benefits offered by choice are preferences. If parents have strong preferences for nearby schools, then expanding choice alone is not an adequate escape hatch. Or if parents prefer to enroll their children in schools with high-income or high-achieving peers, then both student-match quality impacts and competitive incentives are dampened (Abdulkadiroğlu et al., 2020; Rothstein, 2006). The instability of different potential choice reform impacts makes it unsurprising why existing research reaches a diverse set of conclusions, introducing more open questions instead of answers.

To address several of the open questions, I use a large choice program in Los Angeles, the Zones of Choice (ZOC) program, as my laboratory. I leverage multiple features of the program's design to tackle different questions. The program's design is implicitly influenced by the multitude of approaches to school choice that precede it. The program's idea to expand parents' attendance zone boundaries and create zones with multiple nearby schooling options is in part influenced by past approaches of controlled choice such as in Berkeley, Tampa, and Massachusetts (Alves and Willie, 1987; Chavez and Frankenberg, 2009). A key difference is that zones created as part of controlled choice programs had a primary goal to integrate schools, while in the ZOC setting, integration is not a policy goal and in some ways, the design of the zones further entrenches the district's segregation patterns. Similar to other school districts , Zones of Choice administrators use a centralized assignment system to determine assignments within each zone of choice, similar to how enrollment is determined in large school districts with single-offer enrollment systems (Abdulkadiroğlu et al., 2017, 2020; Barrow and Sartain, 2017). The combination of these and other features discussed in more detail in subsequent chapters provide a laboratory that allow me to study questions surrounding the demand and supply of school quality: the impacts of competition (Chapter 2 and 3), the impacts of choice on student-school match quality (Chapter 3), parents' preferences (Chapter 3 and 4), and the role that information plays in affecting schooling decisions (Chapter 4).

In chapter 2, I outline a conceptual framework that provides some structure for subsequent chapters. The purpose of this chapter is present a benchmark model where the typical benefits that motivate contemporary school choice reforms materialize. The contributions of this chapter are to generate an empirically-oriented model at the cost of several simplifying assumptions. Other models in the literature take an opposite approach(e.g., Avery and Pathak (2021); Nechyba (2000)), but the primary contributions of this dissertation are empirical so I opt for an empirically-oriented theoretical framework. I link this model to the subsequent chapters in two ways. First, a key statistic that summarizes families' expected welfare gains from the program is an important covariate for the empirical analysis in Chapter 3. Second, some of the simplifying assumptions regarding perfect information and preferences receive cursory treatment in Chapter 3 and are the focal point of a field experiment discussed in Chapter 4.

In chapter 3, I study how the demand and supply for school quality can evolve in response to policies that change the underlying choice and competitive structure that families and schools face. The contributions of this chapter are to provide empirical evidence tackling the questions surrounding access, competition and student-school match quality. I find that the achievement among students enrolled in ZOC schools increases, with gains sufficiently large to eliminate eleventh-grade achievement gaps that existed in the years before the policy expansion. I distinguish between effects driven by parents choosing schools that best suit their children's needs and competitive effects induced by the policy, and find most of the student gains are driven by general improvements in school quality as measured by school value-added. The lack of improvements in student-school match quality are in part due to the homogeneous set of students within each zone, eliminating the scope for improvements in student-school match quality based on observable student characteristics. I find evidence that schools exposed to more competition (as captured by a statistic derived in Chapter 2) improved more, providing suggestive evidence for a competitive effects story. I then provide suggestive evidence that parent's selected schools in a way that incentivized schools to compete on quality, a finding that contrasts a growing body of evidence suggesting otherwise (Rothstein, 2006; Abdulkadiroğlu et al., 2020). The highly segregated nature of each zone of choice can maybe explain differences with the past literature. In settings where parents can use race or income to proxy for school quality, they will do so, but in segregated neighborhoods parents will use other proxies that are correlated with school effectiveness. This posits an inherent tradeoff present in my setting: short-run gains in terms of reductions in within-district inequality coupled with potentially negative more long-run effects through the entrenchment of school segregation in the district (Johnson, 2011).

Information and preferences are the focal point of Chapter 4, where I report the findings from a field experiment I conducted in Fall 2019. The motivation from this experiment stems from a common empirical finding that parents tend to select schools in a way that places more weight on peer characteristics than school quality. One often advanced hypothesis is that parents lack the adequate information to observe school quality, so they instead resort to proxies such as peer characteristics including race and income. My contribution is to study parental preferences through an information provision experiment, linking the information provision literature with another growing body of research studying parents' preferences for schools. By distributing information to students' households I address the information channel, and in addition provide settings where some parents are perfectly informed about both school and peer quality, allowing me to study relative preferences for peer and school quality. In addition, I explore the role that social interactions play in determining schooling decisions. I corroborate my findings from Chapter 3 by showing that parents not receiving any information tend to place higher weight on school quality than peer quality. Among parents receiving information, I first document evidence that their most-preferred schools changed in terms of quality and peer composition. Through various avenues, I provide evidence of large and salient spillovers in treatment, indicating substantial interactions between parents. I then provide evidence showing that the information campaign led to increases in relative preferences for school value-added. The prevalent role for social interactions could introduce disadvantages to groups with less informed networks in this and other settings.

Chapter 2

A Model of School Choice and Competition

The Zones of Choice setting provides a setting where some schools were exposed to competition while the status quo remained elsewhere in the district. In this chapter, I motivate the empirical analysis provided in Chapter 3 with a model. Under certain assumptions, the model generates key predictions which are the focal point of my empirical analysis.

2.1 A Model of School Choice and School Quality

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 school in the district indexed by 0.[2]

Therefore, students can enroll in either their neighborhood school $j(i)$ or the charter sector. Student i's utility of attending school $j \in \{0, j(i)\}$ is

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

where α_j is school quality defined in the achievement model, d_{ij} is distance to school j, $\boldsymbol{X}_i$ captures preference heterogeneity with respect to student characteristics, and ε_{ij} captures

[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.

unobserved school attributes and idiosyncratic preference heterogeneity not captured by student characteristics $\boldsymbol{X}_i$.

We assume the idiosyncratic preference heterogeneity ε_{ij} is additively separable and extreme value type 1 conditional on d_{ij} and $\boldsymbol{X}_i$

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

We can further decompose V_{ij} into a heterogeneous component determining school j's popularity $\delta_j(\alpha_j, \alpha_0, \boldsymbol{X}_i)$ and another component capturing linear distance costs λd_{ij}[3]

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

Preferences for school quality α_j are constant across students and additively separable from $\mu_j(\boldsymbol{X}_i)$ that captures any remaining heterogeneity governing school popularity

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

Lastly, we normalize charter school utility to zero.

With a logit error structure, neighborhood-specific district school market share is

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

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 supply-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

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

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

[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.

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. 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 schools $\mathcal{J}$. Therefore, the choice set of a student residing in one of these neighborhoods expands from $J_i = \{0, j(i)\}$ to $\mathcal{J}^+ = \mathcal{J} \cup 0$. Each additional option has varying popularity $\delta_j(\alpha_j, X_i)$ and students face varying distance costs to access each new option, implying differences in the net value of each new option. We define a student's option value gain (OVG) as the difference in expected max utility between 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*

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

and with iid Extreme Value Type I errors,

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

OVG is a measure a student's expected welfare gain measured in terms of distance. Intuitively, a student with high OVG gained access to relatively popular schools and valued them highly, net of 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 $OVG_{i0} = \frac{1}{\lambda}\left(\ln(1 + e^{V_{ij(i)}}) - V_{ij(i)}\right)$ 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

$$S_j = \frac{1}{N}\Bigg(\underbrace{\sum_{j(i)=j} e^{-\lambda OVG_i - \lambda OVG_{i0}}}_{Neighborhood\ j\ students} + \underbrace{\sum_{k \neq j} \sum_{j(i)=k} e^{\Delta_{ijk} - \lambda OVG_i - \lambda OVG_{i0}}}_{Other\ students\ in\ \mathcal{J}}\Bigg). \tag{2.1}$$

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.

2.2 Implications and Empirical Map

In this section we map the model's predictions to empirical exercises. Before introducing the empirically oriented propositions, we establish that there is an equilibrium to the effort game introduced above. 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 Section 2.4.1. □

The next proposition 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. This requires an additional, but plausible assumption: ZOC schools start as local neighborhood monopolies with a majority of their neighborhood market share.[5]

Proposition 2. *If each school* $j \in \mathcal{J}$ *has at least 50 percent of its market share* ($P_j^0 > 0.5$) *before the ZOC expansion with sufficiently high quality elasticity of demand, then 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*

$$\Delta\alpha_j = 0$$

Proof. See Section 2.4.1. □

The model suggests a difference-in-differences design comparing changes in achievement between ZOC students and other non-ZOC students. 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.

Proposition 3 shows that there is a reduction in the between-school quality gap within ZOCs, indicating a compression in the school effectiveness distribution.

Proposition 3. *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:*

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

Proof. See Section 2.4.1. □

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

There is not a complete convergence in quality due to heterogeneous preferences and distance costs. To test this empirically, we estimate distributional and unconditional quantile treatment effects on school effectiveness.

Proposition 4 introduces OVG into the empirical analysis.

Proposition 4. *School quality* $\alpha_j = f(e_j^{BR}(e_{-j}, e))$ *is increasing in OVG for each school* j

Proof. See Section 2.4.1. □

OVG is introduced as an index that summarizes the expected welfare gain students receive from their expanded choice sets, but from the school's perspective, exposure to students with high OVG requires additional effort to attract them since their outside options are better. This observation allows us to interpret OVG as an index for competition. The student- and school-level variation in OVG provides a useful source of heterogeneity to test for competitive effects.[6]

2.3 Conclusion

The limited rollout of the Zones of Choice program provides a setting in which some schools were exposed to competition but others were not. I model this as an expansion of parents' choices sets from one neighborhood school to several nearby options. Then if we assume that principals (or school administrators) are rewarded for higher enrollment shares, that generates incentives for schools within each zone of choice to compete with one another. Combined with standard discrete choice assumptions, the competitive effects generate weakly positive improvements in school quality among all affected schools and a summary statistic that aims to capture the competitive pressure schools faced at the onset of the program. In the next chapter, I empirically assess these predictions.

[6] 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.

2.4 Additional Results

2.4.1 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'. OGV 2.1 depicts this visually.

The transition from the 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 establishes that the effort game exhibits strategic complementarities, and as a consequence, a Nash equilibrium exists Vives (1990, 2005). A caveat is that this proof does not guarantee uniqueness. 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. The functional monopoly assumption requires pre-ZOC market shares to be at least 50 percent, and is mostly necessary due to the logit error structure, and may not be necessary under other error structures. Strategic complementarities play a role in ensuring the post-ZOC equilibrium levels are strictly greater than pre-ZOC equilibrium effort levels for all schools $j \in \mathcal{J}$. Proposition 3 further shows that schools with 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.

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 marginal payoff of principal j is increasing in the effort of another principal $k \neq j$

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

To gain some intuition, note that at a given level of e_j, a ceteris paribus increase in e_k reduces j's market share, increasing the marginal utility in e_j. Principal j then increases their effort e_j in response to the increase in e_k, and these dynamics play out at all levels of e_j, making the marginal utility of j increasing in the effort of k, the definition of strategic complements. □

Proposition 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 $\varepsilon^1_{ij} > \frac{P^0_j}{P^1_j}\varepsilon^0_j$, then for each $j \in \mathcal{J}$, the change in principal effort is*

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

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

$$\Delta e_j = 0$$

Proof. Figure 2.1 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

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

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

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

Alternatively, the strategic complementarities in effort also would point to similar dynamics. Therefore, combining strategic complementarities with the fact that 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 $\Phi_j(e^{BR}_j(e_{-j}, e), e_{-j})$ and $f'(e^{BR}_j(e_{-j}, e))$ where $e^{BR}_j > e_{j0}$.

$\square$

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

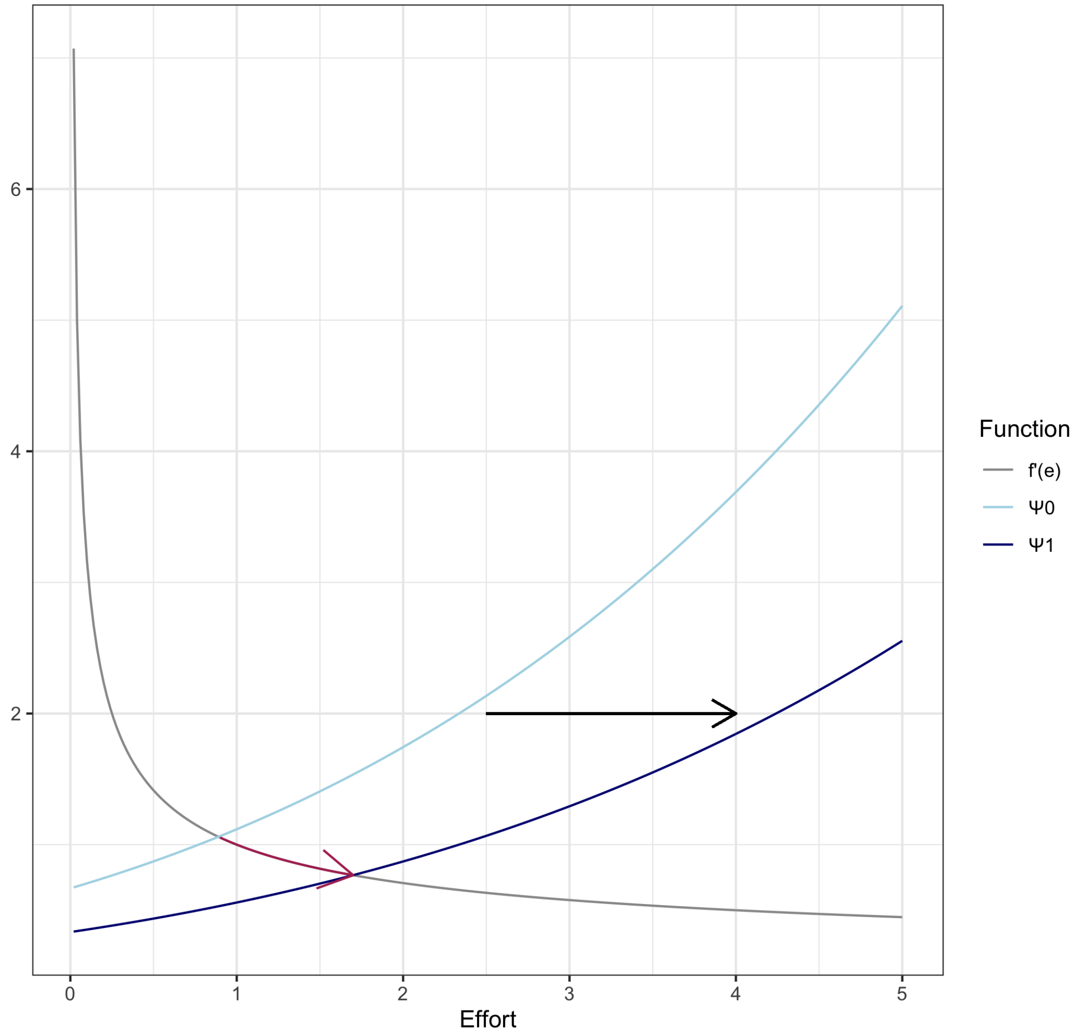
Proof. Let $\boldsymbol{OVG} = (OVG_1, \cdots, OVG_N)$ be a vector of student-level OVG. Suppose we depart from an equilibrium e^*. For a given school j, we have

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

Therefore, for a marginal increase in $\boldsymbol{OVG}$, Φ shifts further downward leading to increases in effort and strategic complementarities in Proposition 2 imply a new equilibrium 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). □

Figure 2.1: Change in Equilibrium



Chapter 3

The Impacts of Neighborhood School Choice: Evidence from Los Angeles' Zones of Choice

3.1 Introduction

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 (Friedman, 1955; Hoxby, 2003; Chubb and Moe, 1990). However, empirical studies of school choice experiments have generated mixed results regarding the effects and efficacy of school choice (Abdulkadiroğlu et al., 2018; Muralidharan and Sundararaman, 2015; Neilson, 2013; Rouse, 1998; Lavy, 2010). 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; Rouse, 1998; Hoxby et al., 2009; Tuttle et al., 2012; Cullen et al., 2006; Deming et al., 2014; Angrist et al., 2002; Krueger and Zhu, 2004). 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 (Muralidharan and Sundararaman, 2015; Card and Lemieux, 2001; Hsieh and Urquiola, 2006; Figlio and Hart, 2014; Figlio et al., 2020; Gilraine et al., 2019;

Neilson, 2013; Allende, 2019a). 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 (Rouse, 1998; Abdulkadiroğlu et al., 2011; Deming et al., 2014; Cullen et al., 2006). 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 (Hastings et al., 2005; Burgess et al., 2015). ZOC parents' choices reflecting stronger preferences for school effectiveness, as opposed to student composition, are consistent with the improvements in school quality we find. A potential reason for this finding is the limited scope for parents to select schools based on peer composition because every zone of choice is roughly homogeneous in terms of race and income. Effectively eliminating sorting on race and income could compel parents to select schools on other measures more correlated with school effectiveness, but these results present a potential inherent tradeoff: economically and racially segregated neighborhood choice settings can produce stronger competitive incentives for schools producing short-run gains, but can at the cost of negative long-run effects through increased school segregation (Johnson, 2011, 2019).

There are 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; Hsieh and Urquiola, 2006; Rothstein, 2007; Hoxby, 2003), 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 (Neilson, 2013; Allende, 2019a,b). We contribute to this literature by studying an intra-district program that changed the market structure locally, and thus allows us to estimate local market-level effects. Unlike previous papers, we differentiate between changes in match quality and changes in school quality.

We also contribute a preference-based approach to measuring competitive pressures. 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. Our approach contributes to this literature by using a measure of competition derived from preferences.

Our demand analysis contributes 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 (Fack et al., 2019; Agarwal and Somaini, 2019; Kapor et al., 2020). Importantly, the preference estimates we provide can be directly related to the market-level effects of the program helping us paint a consistent story providing a suggestive link between the demand-side and the 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 (Rouse, 1998; Abdulkadiroğlu et al., 2018; Angrist et al., 2002; Howell et al., 2002; Krueger and Zhu, 2004), charter schools (Abdulkadiroğlu et al., 2011; Tuttle et al., 2012; Hoxby et al., 2009; Angrist et al., 2016), or exercising choice in district open-enrollment programs (Deming et al., 2014; Cullen et al., 2006). 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 3.2 we outline the features of the policy and the data sources; Section 3.3 discusses the data; Section 3.4 presents the market-level analysis; Section 3.5 estimates demand and OVG; Section 3.6 presents lottery estimates; Section 3.7 presents evidence on changes occurring within schools and provides a discussion of key differences between ZOC and other school choice reforms; and lastly, Section 3.8 concludes.

3.2 Institutional Details

3.2.1 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 Figure 3.36 and Figure 3.37). 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 3.1 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..."*

3.2.2 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. 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.

3.3 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.[1] Column 1 and Column 2 of Table 3.1 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.[2] Most ZOC students are Hispanic, roughly 88 percent or 20 percentage points higher than non-ZOC students. ZOC students are also more socioeconomically disadvantaged than other students in the district. 85 percent are classified as poor by the district and only 3 percent of students have parents who graduated from college, 50 percent less than non-ZOC students.

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 Section 3.9.2. We report matched non-ZOC mean characteristics in Column 4 of Table 3.1. The matching

[1] A potential concern is differential attrition rates out of the sample that could introduce bias in our analysis. In Figure 3.24 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.

[2] text

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 3.1 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. Section 3.9.7 provides additional details pertaining to the lottery sample.

3.4 Empirical Analysis

3.4.1 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; Smith et al., 2017).[3] For a student outcome Y_i, such as achievement or four-year college enrollment, we
estimate

$$Y_i = \mu_{j(i)} + \mu_t + \sum_{k \neq -1} \beta_k ZOC_{j(i)} \times \mathbf{1}\{t - 2013 = k\} + \boldsymbol{X}_i' \psi + u_i \tag{3.1}$$

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.[4] 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 indication 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.

[3] 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. Section 3.9.2 discusses the matching strategy and results in further detail.

[4] In Section 3.9.3, we provide event-study estimates from a parameterized model that summarizes the period-specific β_k coefficients with the model

$$\beta_k = \theta_1 \mathbf{1}\{k < -1\} \times k + \theta_2 \mathbf{1}\{k \geq 0\} + \theta_3 \mathbf{1}\{k \geq 0\} \times k. \tag{3.2}$$

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.

Event-study Results

Figure 3.2a reports estimates of Equation 4.2 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.[5] Figure 3.12 reports treatment effects on math scores that are nearly identical to ELA treatment effects.[6]

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. Figure 3.40 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 3.2b. 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 Figure 3.38a.

Figure 3.19 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 Figure 3.18. Therefore, the college enrollment event study evidence provides evidence that ZOC was effective in pushing students into college.

[5] Similarly, Figure 3.14 reports estimates of the parametric event-study from Equation 3.2. 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.

[6] 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 Section 3.9.3.

Distributional Effects

A concern is 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

$$\mathbf{1}\{A_i \leq a\} = \mu_{j(i)} + \mu_t + \beta_a PostZOC_{it} + \boldsymbol{X}_i'\psi + u_i \tag{3.3}$$

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 3.3 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 hover 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 Section 3.9.5, 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 3.4 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.[7]

The heterogeneity analysis provides evidence that ZOC was effective increasing achievement among students who would have otherwise performed poorly and those gains did not

[7]Figure 3.39 reports trends by different quartile groups.

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.

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 3.2a and Figure 3.2b. 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 (Black, 1999; Bayer et al., 2007) and changes in the household composition (Nechyba, 2000). To assess these potential concerns, Figure 3.19 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; Figure 3.20 and Figure 3.21 reports these estimates. Thus, isolating achievement effects for students who did not strategically sort into ZOC does not change the baseline 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. Figure 3.22 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. Figure 3.23 presents estimates of Equation 4.2 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.

3.4.2 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.

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[8]

$$A_{ij} = \alpha_j + \boldsymbol{X}_i'\beta_j + u_{ij} \tag{3.4}$$

[8]We suppress time indices for notational ease.

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 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 3.4 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.[9]

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

$$E[A_{ij}|X_i, j(i) = j] = \alpha_j + \boldsymbol{X}_i'\gamma_j; \quad j = 1, \cdots, J. \tag{3.5}$$

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 (Deming et al., 2014; Chetty et al., 2014a). 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).

[9] 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.

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. Section 3.9.7 contains additional details regarding the bias tests. We provide details and summary statistics for the achievement model estimates in Section 3.9.6.

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

Treatment effects on school effectiveness are expected if viewed through the lens of the model of school competition. Figure 3.5a 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 Figure 3.15a), accounting for most of the observed achievement effects.

An alternative source of gains arises through the choices parents make. If parents select schools that are more suitable for their children, we would expect to find gains in achievement through gains in match effects. Figure 3.5b 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. Therefore, although parents scope for choosing more suitable schools expanded, we do not find evidence of large gains on this margin.

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

We now turn to Proposition 3, 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 3.6a 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. We then estimate unconditional quantile treatment effects shown in Figure 3.6b using the methods developed in Chernozhukov et al. (2013). The estimates reveal that most of the improvements came from schools in the bottom half of the school effectiveness distribution. Both of these estimates suggest that the ZOC distribution experienced a compression relative to the non-ZOC distribution. In Section 3.9.8, 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.

In summary, our findings thus far suggest that schools responded to competition by adjusting their causal impact on student test scores and not by investing in screening strategies to improve their average achievement. Our first piece of suggestive evidence supporting this comes from the decomposition of achievement effects showing that changes in school effectiveness contributed the most to changes in achievement. In addition, the heterogeneous school effectiveness treatment effects we find in this section are consistent with the model of school competition suggesting larger improvements among lower-performing schools. These findings seem to contrast modest effects found in other settings (Muralidharan and Sundararaman, 2015; Hsieh and Urquiola, 2006; Card et al., 2010). In Section 3.7.3, we provide some comments on why Los Angeles may have provided the adequate setting for these large competitive effects.

3.5 Demand and OVG

Estimates of demand using rank-ordered lists parents submit to the district allows us to asses both the strength of competitive inventives among ZOC schools and to further test for competitive effects. The strength of competitive incentives are governed by how parents select schools, so estimates of average preferences help assess these incentives. Empirical estimates of OVG, calculated using demand parameters, allow us to test for OVG treatment effect heterogeneity and a test for competitive effects.

3.5.1 Estimating Preferences

We use rank-ordered preference data submitted by ZOC applicants to estimate demand parameters (Agarwal and Somaini, 2019; Beuermann et al., 2018; Abdulkadiroğlu et al., 2020; Hastings et al., 2005).[10] The model in Section 2.1 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

[10] 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.

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

$$R_{ik} = \begin{cases} \arg\max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg\max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}. \tag{3.6}$$

Truthful preferences are unlikely if applicants are strategic under an immediate acceptance mechanism (Agarwal and Somaini, 2019; Pathak and Sönmez, 2013), or if applicants do not understand the mechanism's rules or have biased beliefs (Kapor et al., 2020). Nonetheless, schools observe reported preferences—truthful or not—and respond accordingly to this demand.

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

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r | U_{ir} < U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \tag{3.7}$$

We aggregate the log of Equation 4.8 across individuals to construct the complete likelihood and estimate parameters of the utility specification via maximum likelihood.

3.5.2 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 3.4.2. We estimate

$$\delta_{jct} = \xi_{cz(j)} + \xi_{z(j)t} + \omega_P Q^P_{jt} + \omega_S \alpha_{jt} + \omega_M Q^M_{jct} + u_{jct} \tag{3.8}$$

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 3.2 reports estimates of Equation 3.8. 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 distinguish between effective and less effective schools. Importantly, these estimates provide suggestive evidence indicating that competitive incentives were not weak as is found in other settings (Rothstein, 2006; Abdulkadiroğlu et al., 2020; Hastings et al., 2005); this evidence is consistent with the school effectiveness event-study evidence. 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 3.7 we provide additional discussion about why features of the ZOC program may have facilitated families acquisition of information.

3.5.3 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 2.1, 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.[11] Figure 3.8 displays the distribution of OVG across students and Table 3.6 reports OVG correlates.[12] 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

[11]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.

[12]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.

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 3.7 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 3.3 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 interaction 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 3.7 and provide some suggestive evidence.

3.6 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., 2018, 2011; Chabrier et al., 2016; Rouse, 1998; Cullen et al., 2006). We complement the market-level analysis with this alternative design and show that the majority of the ZOC benefits stem from market-level effects.

3.6.1 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 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.

Section 3.9.7 contains additional additional lottery details. We report balance tests to show the conditional randomness of lottery offers. The additional tables also report attrition differentials to ensure our lottery estimates are not driven by selective attrition out of the sample.

3.6.2 Results

Table 3.4 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 3.5, with impacts on ELA and Math in Panel A and B, respectively. Column 3 of Table 3.5 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 presented here suggests an initial premium of attending a most-preferred school, but market-level effects diminished this benefit with the lower-performing schools catching up with the initially higher-performing schools. This evidence is also consistent with both the demand estimates and OVG analysis. Therefore, the majority of program's impacts come from the market-level effects which resulted in compressions of school quality within zones, eliminating most-preferred premiums.

3.7 Mechanisms and 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 (Bloom et al., 2015; Angrist et al., 2013; 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.

3.7.1 Changes in School Inputs

Section 3.9.10 reports evidence on changes in inputs such as teacher characteristics, teacher quality, and class size.[13] 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.

3.7.2 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. Section 3.9.9 studies changes in student-teacher racial match and Section 3.9.10 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; Gershenson et al., 2018; Fairlie et al., 2014). We also find evidence of reductions in tracking practices. While the literature is mixed in terms of the effects of tracking (Betts, 2011; Duflo et al., 2011; Cohodes, 2020; Card and Giuliano, 2016; Bui et al., 2014), 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.

3.7.3 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. Furthermore, we find that student achievement effects are mostly explained by improvements in school effectiveness, or school value-added, improvements that essentially eliminated ZOC achievement gaps. These treatment effects are large in comparison to more modest effects of competition in public schools estimated in the literature (Ridley and Terrier, 2018; Figlio and Hart, 2014; Gilraine et al., 2019; Figlio et al., 2020; Card et al., 2010). 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

[13] 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.

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 (Rothstein, 2006; Abdulkadiroğlu et al., 2020, 2014). These differences beg the question: why is Los Angeles different? We argue that access to information and supply-side constraints are important factors that are notably different in the ZOC program.

In terms of information, 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. Therefore, 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.[14]

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.

In summary, 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. Furthermore, the centralized assignment mechanism that does not allow for priorities based on screening strategies reduces the benefits to recruitment efforts, which could be important for how schools responded to their changed incentives.

[14] 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.

3.8 Conclusion

This paper studies a novel expansion of public school choice in Los Angeles. The unique design and implementation of the Zones of Choice 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. One explanation for this finding is the highly segregated nature of each zone of choice, effectively eliminating the scope for parents to select on peer quality due to the limited variability. Then, using a measure of competition derived from applicant preferences, we show that treatment effects were largest for schools facing the greatest pressure to improve. 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.

Figure 3.1: Zones of Choice and 2010 Census Tract Income

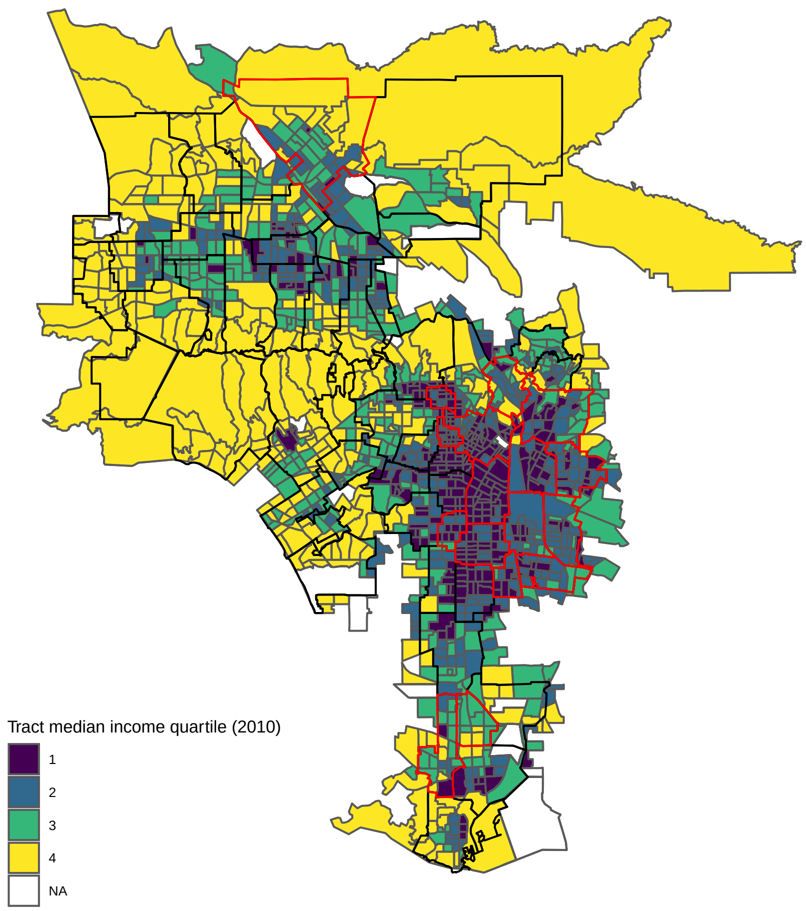
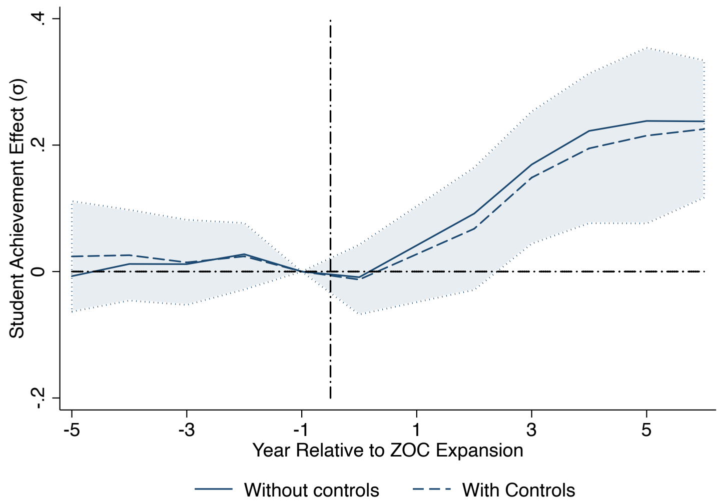
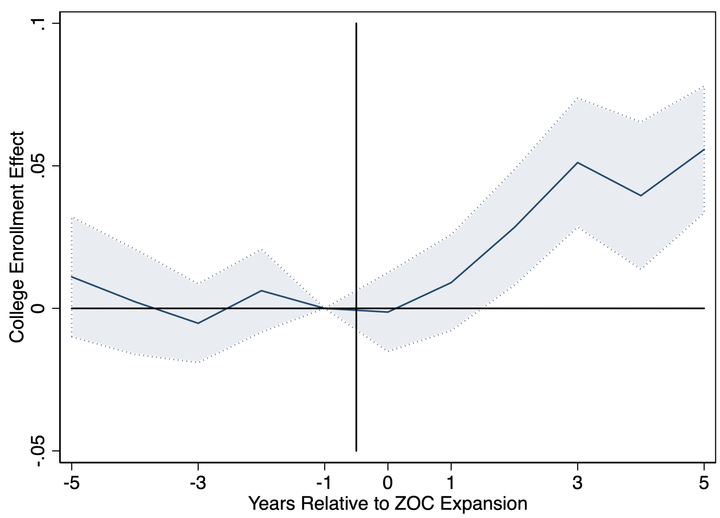


Figure 3.2: Achievement and College Enrollment Event Studies

(a) Achievement Event Study

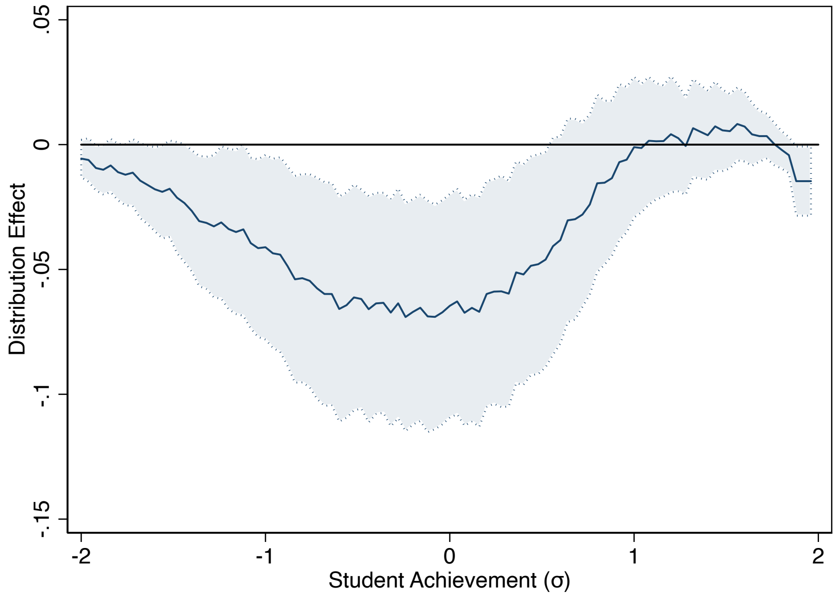


(b) College Enrollment Event Study



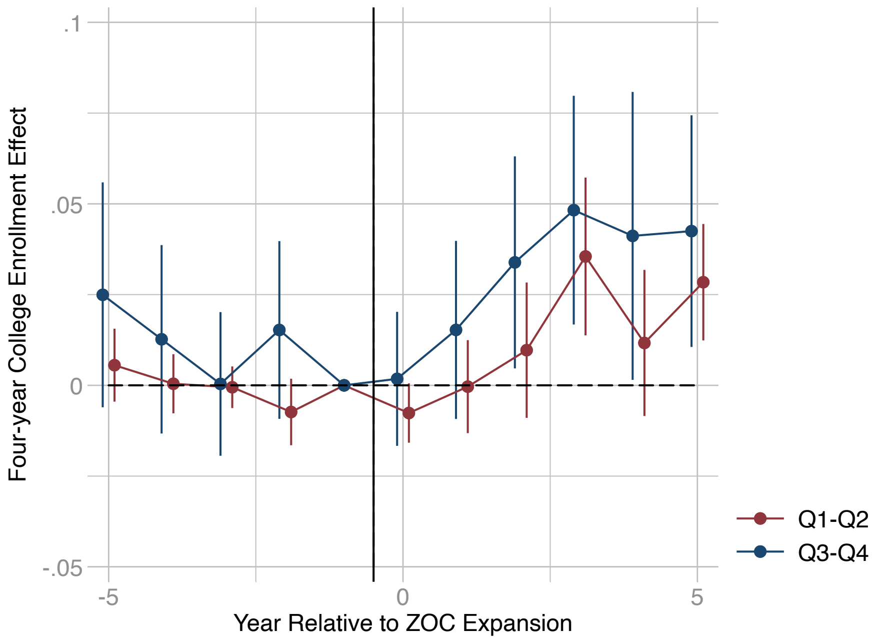
Notes: This figure plots the estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-difference estimates on outcomes relative to the year before the policy. The dashed blue line in Panel A traces out 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.

Figure 3.3: Student Achievement Distributional Impacts



Notes: This figure reports estimates of β_a from Equation 3.3 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.

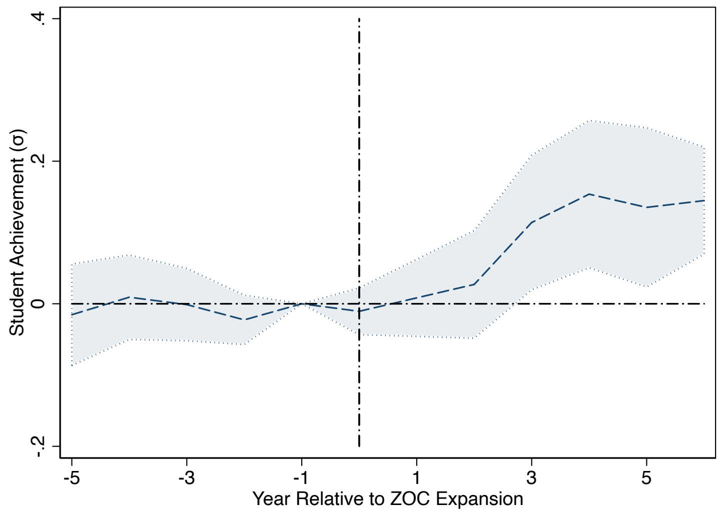
Figure 3.4: Four-year College Enrollment Effects by Predicted Quartile Groups



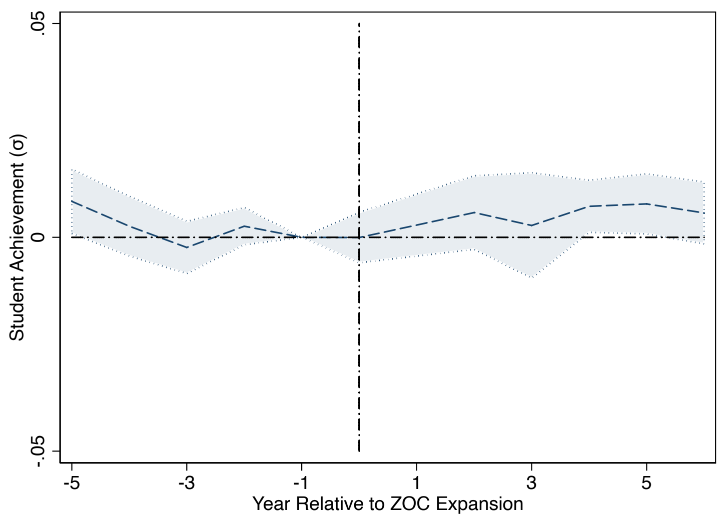
Notes: This figure plots the estimates of β_k analogous to those defined in Equation 4.2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-difference estimates on four-year college enrollment rates relative to the year before the policy. Estimates in blue 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.

Figure 3.5: Decomposition event studies

(a) ATE event study

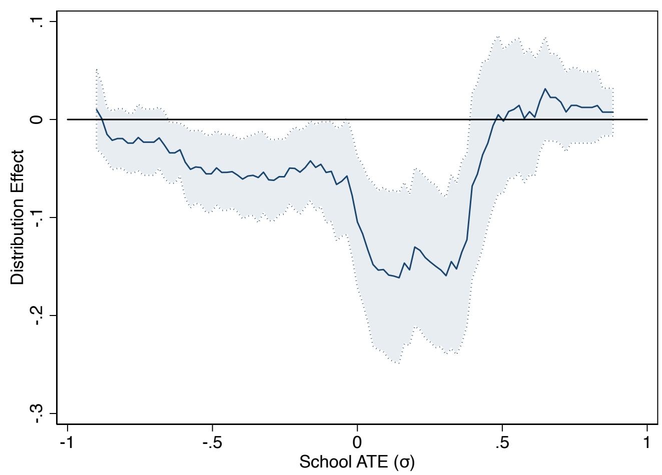


(b) Match Effect event study

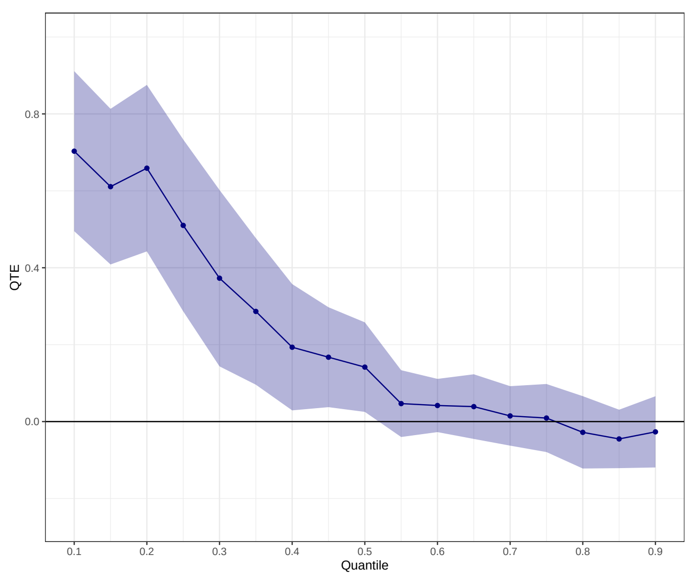


Notes: This figure plots the estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in achievement σ between changes in ZOC and non-ZOC student changes relative to their difference the year before the expansion. Standard errors are double clustered at the school and year level and 95 percent confidence intervals are displayed in the shaded regions.

Figure 3.6: Distribution and Quantile Treatment Effects on ATE



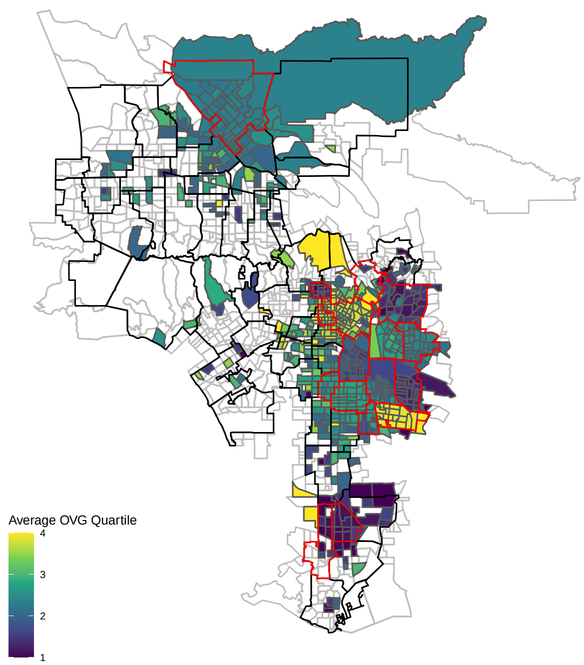
(a) Distribution Effects on School ATE



(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.

Figure 3.7: 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.

Table 3.1: 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.

Table 3.2: 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.

Table 3.3: 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)
× Poverty		-0.011					-0.011
		(0.032)					(0.032)
× College		0.014					0.019
		(0.018)					(0.023)
× Migrant		-0.091					-0.098*
		(0.052)					(0.052)
× Spanish at Home		0.045*					0.026
		(0.023)					(0.015)
× Special Education		0.032					0.010
		(0.036)					(0.045)
× Female			0.023				0.027
			(0.017)				(0.019)
× Black				-0.078*			-0.119**
				(0.042)			(0.041)
× Hispanic				0.034			-0.015
				(0.033)			(0.029)
× Lagged Achievement					-0.026		-0.031
					(0.017)		(0.020)
× 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 4.2 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.

Table 3.4: Lottery Estimates

		FS (1)	RF (2)	TSLS (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 3.5: Lottery estimates by cohort, 2013-2017

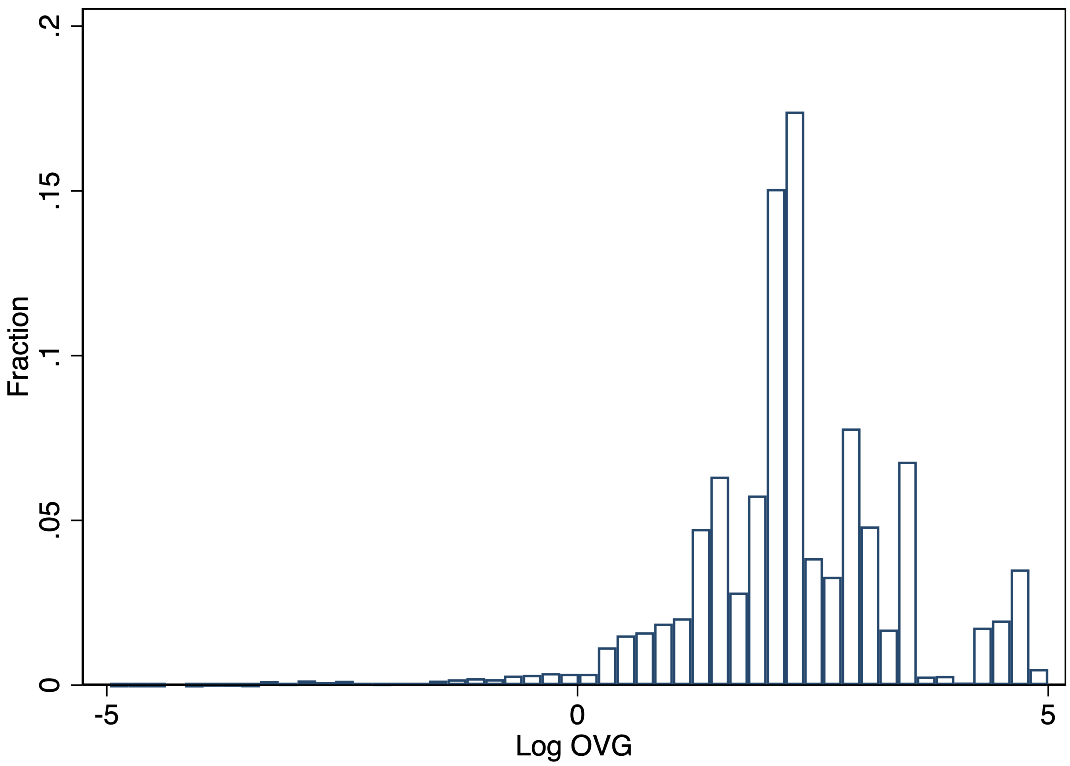
	(1) FS	(2) RF	(3) TSLS	(4) CCM	(5) TCM
	Panel A: ELA				
First and Second Cohort	0.467***	0.047*	0.101**	[.071]	[.172]
	(0.063)	(0.024)	(0.048)		
Third and Fourth Cohort	0.492***	-0.022	-0.045	[.201]	[.157]
	(0.053)	(0.029)	(0.058)		
Fifth Cohort	0.444***	0.002	0.005	[.244]	[.249]
	(0.089)	(0.047)	(0.105)		
	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 3.13. 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.

3.9 Additional Results

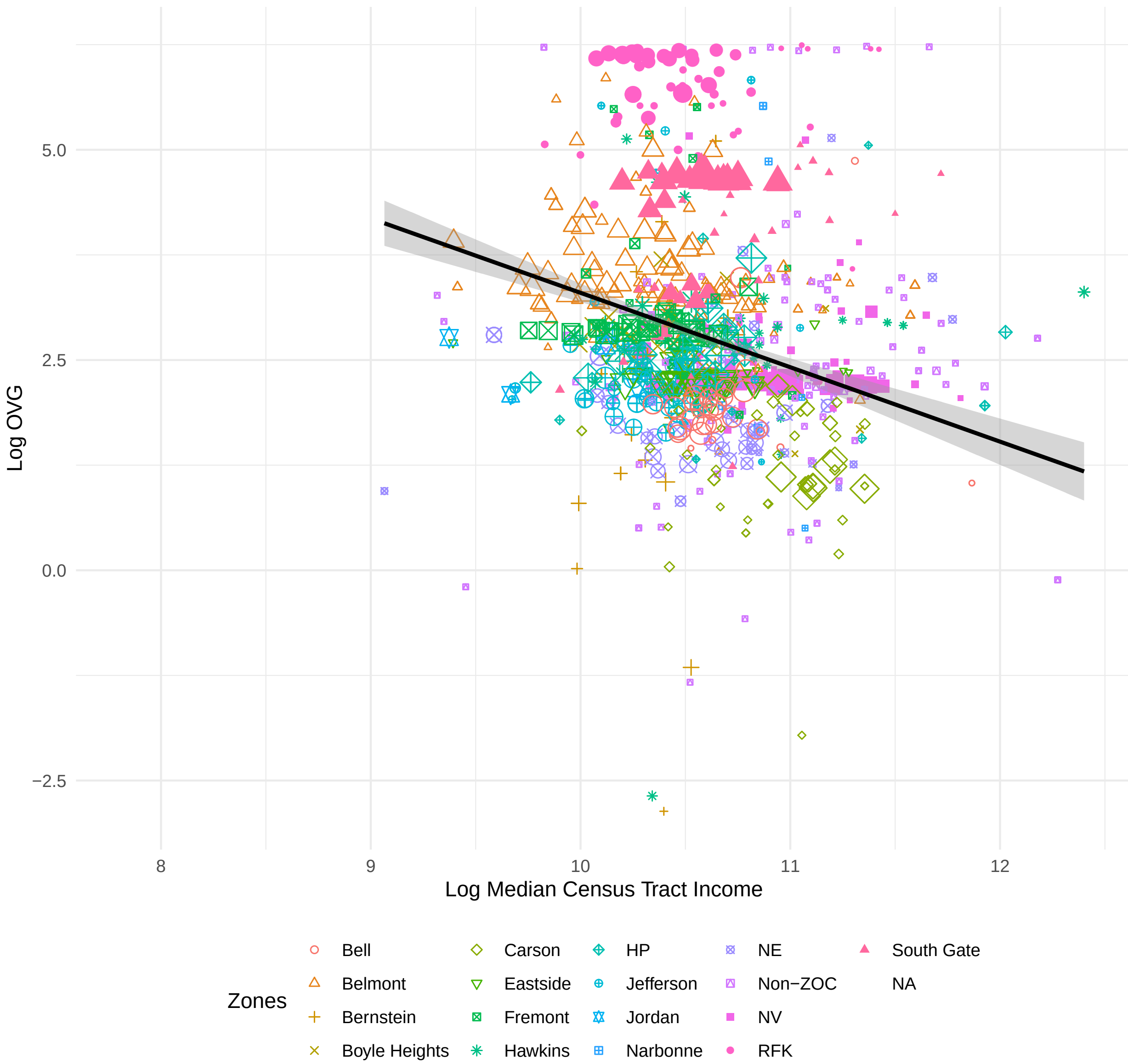
3.9.1 OVG Details

Figure 3.8: 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 3.9: 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).

Table 3.6: 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.

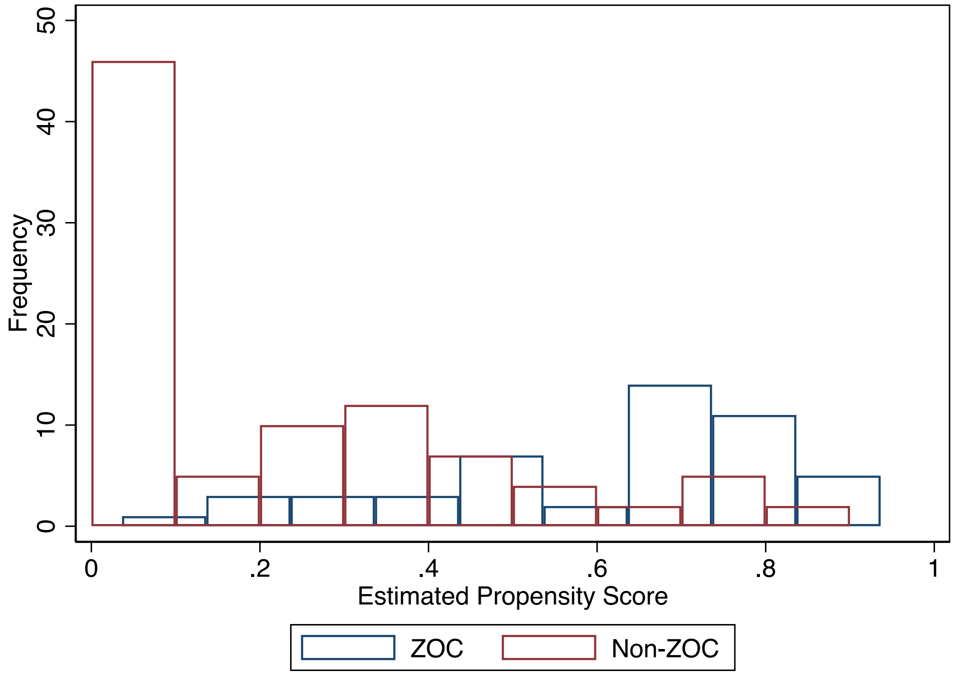
Table 3.7: OVG Treatment Effect Heterogeneity

	(1) ELA	(3) ELA	(4) ELA	(5) ELA	(6) ELA	(7) ELA	(8) ELA	(9) ELA	(10) ELA	(11) ELA	(12) ELA
PostZOC	0.021	0.027	-0.020	0.030	0.021	0.035	-0.000	0.020	0.019	-0.059	
	(0.038)	(0.039)	(0.059)	(0.054)	(0.038)	(0.040)	(0.048)	(0.038)	(0.038)	(0.054)	(0.071)
PostZOC $\times StudentOVG_{3,4}$	0.119***	0.117***	0.116***	0.119***	0.119***	0.116***	0.116***	0.118***	0.117***	0.117***	0.110***
	(0.030)	(0.030)	(0.030)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
(0.029)											
PostZOC $\times SchoolOVG_{3,4}$	0.118*	0.114*	0.114*	0.119*	0.118*	0.118*	0.116*	0.118*	0.118*	0.119*	0.114*
	(0.059)	(0.059)	(0.058)	(0.059)	(0.059)	(0.059)	(0.058)	(0.059)	(0.059)	(0.059)	(0.058)
PostZOC											
× Black		-0.096*									-0.115**
		(0.046)									(0.042)
× Hispanic				0.048							
-0.020											
			(0.035)								(0.029)
× Poverty					-0.012						
-0.016											
				(0.031)							(0.029)
× College						0.004					
0.019											
					(0.016)						(0.023)
× Migrant							-0.085				
-0.094*											
						(0.050)					(0.050)
× Spanish								0.030			
0.021											
							(0.023)				(0.015)
× Sped. Ed.									0.028		
0.002											
								(0.033)			(0.043)
× Lag ELA										-0.024	
-0.030											
									(0.017)		(0.020)
× Income										0.008	0.007*
										(0.004)	(0.004)
OVG Main Effect	X	X	X	X	X	X	X	X	X	X	X
Lagged Test Scores	X	X	X	X	X	X	X	X	X	X	X
Observations	183,294	183,294	183,294	183,294	183,294	183,294	183,294	183,294	183,294	183,294	183,294
R-squared	0.538	0.538	0.539	0.538	0.538	0.539	0.538	0.538	0.539	0.539	0.539

tiny *Notes:* This table reports estimates from difference-in-difference regressions with same controls as event-study models from Equation 4.2 but additional interaction terms for OVG heterogeneity. $StudentOVG_{3,4}$ is an indicator for a student having estimated OVG in the top two quartiles of the student OVG distribution. $SchoolOVG_{3,4}$ 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 OVG and lagged test scores. Standard errors are double clustered at the school and year level.

3.9.2 Propensity Score Estimation

Figure 3.10: 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.

Table 3.8: 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 3.9: 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.

3.9.3 Additional Event Study Evidence

Math Estimates

Alternate Event-study Parameterization

In this section, we present an alternative parameterization for event-studies. The less parametric models displayed in Figures 3.2a and 3.2b 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

$$\beta_k = \theta_1 \mathbf{1}\{k < -1\} \times k + \theta_2 \mathbf{1}\{k >= 0\} + \theta_3 \mathbf{1}\{k >= 0\} \times k \tag{3.9}$$

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.

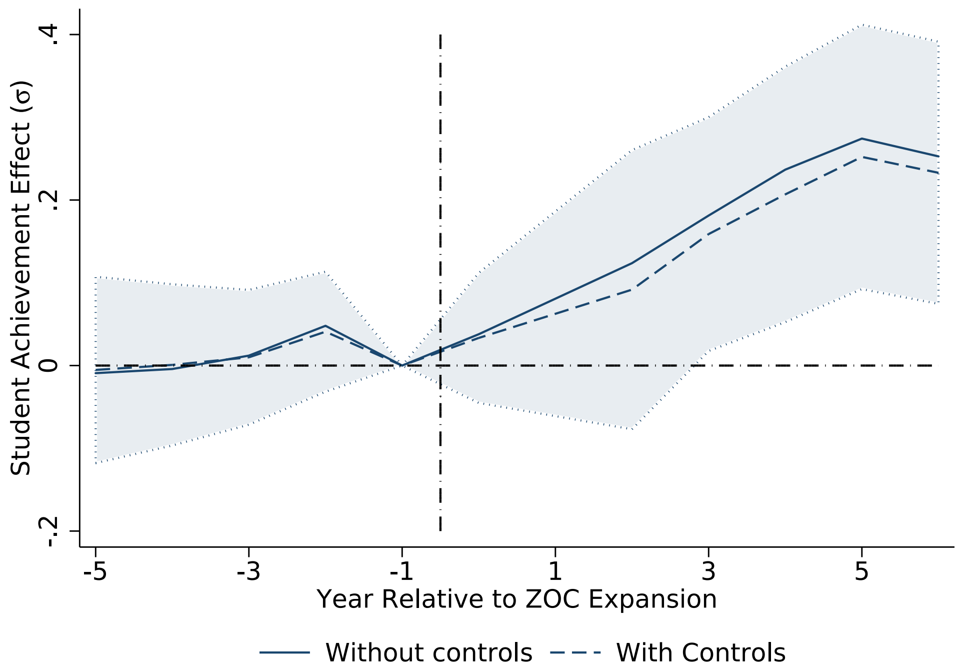
There are several reasons to pursue this approach. First, a test of $\theta_1 = 0$ is a concise test for differential pre-trends and a test of no trend-break $\theta_1 = \theta_3$ concisely summarizes any impact. Second, as discussed above, under correct model specification we gain efficiency. Third, 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 $\hat{\boldsymbol{\beta}}$.

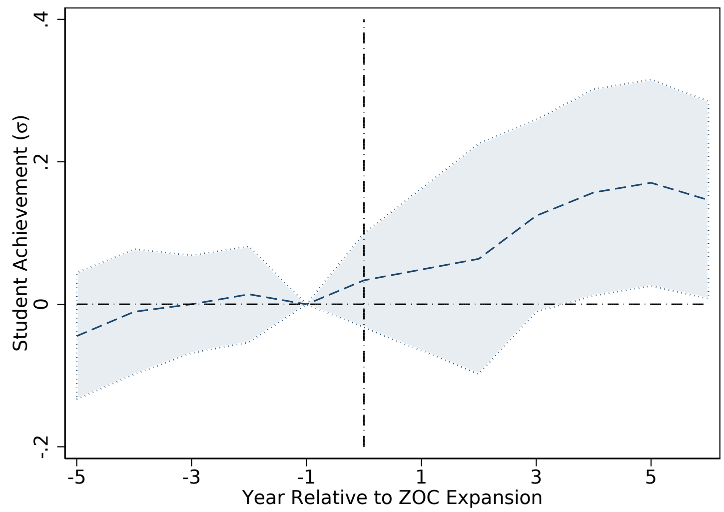
Figures 3.14, 3.15a, and Figures 3.15b report the implied event-study estimates from these models. The overarching conclusions are identical to the event-study estimates reported in Figures 3.2a, 3.2b, and Figures 3.5a, 3.5b. In all models, we fail to reject a differential pre-trend providing support for the parallel trends assumption. As before, we find most of the student achievement treatment effects are due to changes in school effectiveness. Figure 3.16 reports event-study estimates where the outcome is student predicted ability implied by the model in Section 2.1. We do not find evidence of differential changes in the predicted ability between ZOC and non-ZOC students.

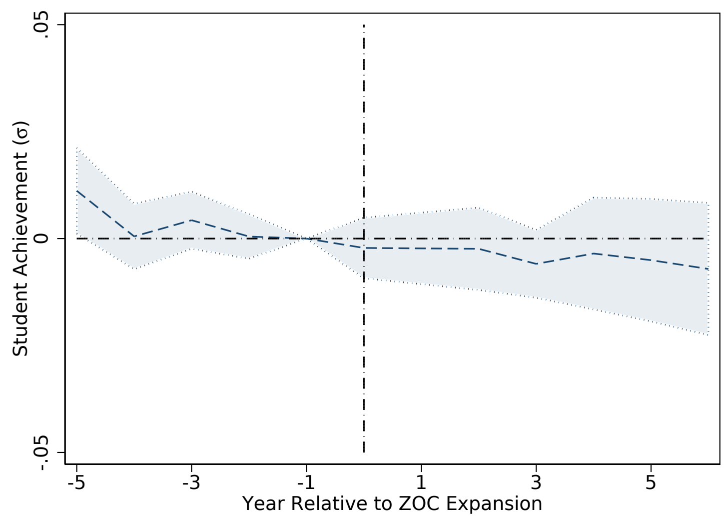
Figure 3.11: Math Achievement Event Study



Notes: This figure plots the estimates of β_k analogous to those defined in equation 4.2, 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.

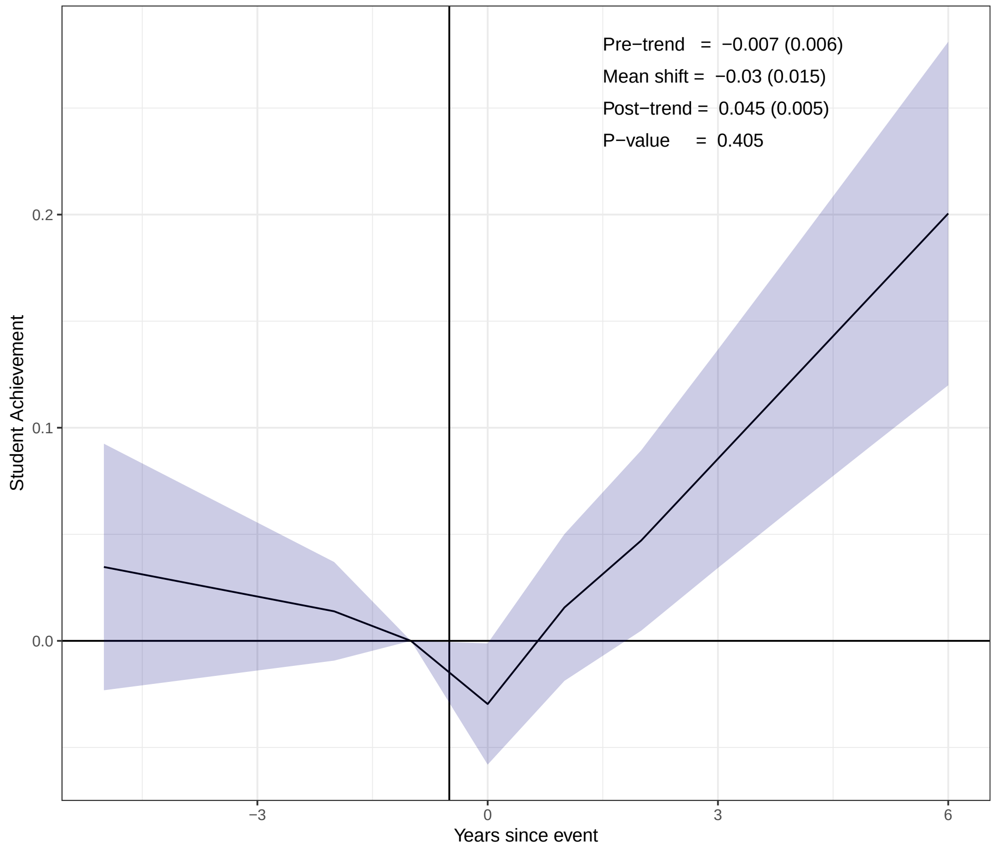
Figure 3.12: Math ATE and Match Event Studies





Notes: This figure plots the estimates of β_k analogous to those defined in equation 4.2, 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.

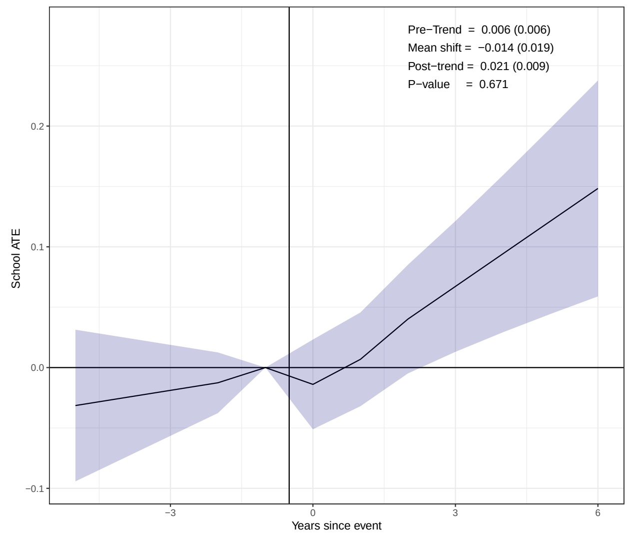
Figure 3.14: Parametric Achievement Event Study



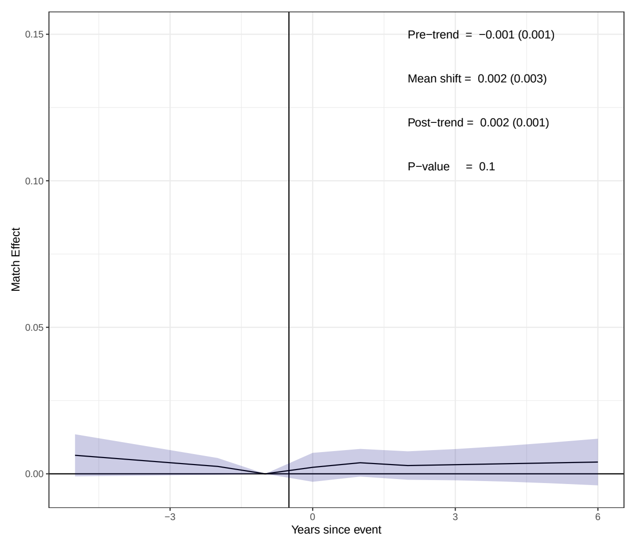
Notes: This figure plots the estimates of $g(\hat{\theta})$ defined in Equation 3.2. The value of the red line shows the difference in achievement σ between ZOC and non-ZOC students relative to their difference the year before the expansion. Estimates of $\hat{\theta}_1$, $\hat{\theta}_2$, $\hat{\theta}_3$ 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 3.15: Decomposition of treatment effects

(a) Parametric ATE Event Study

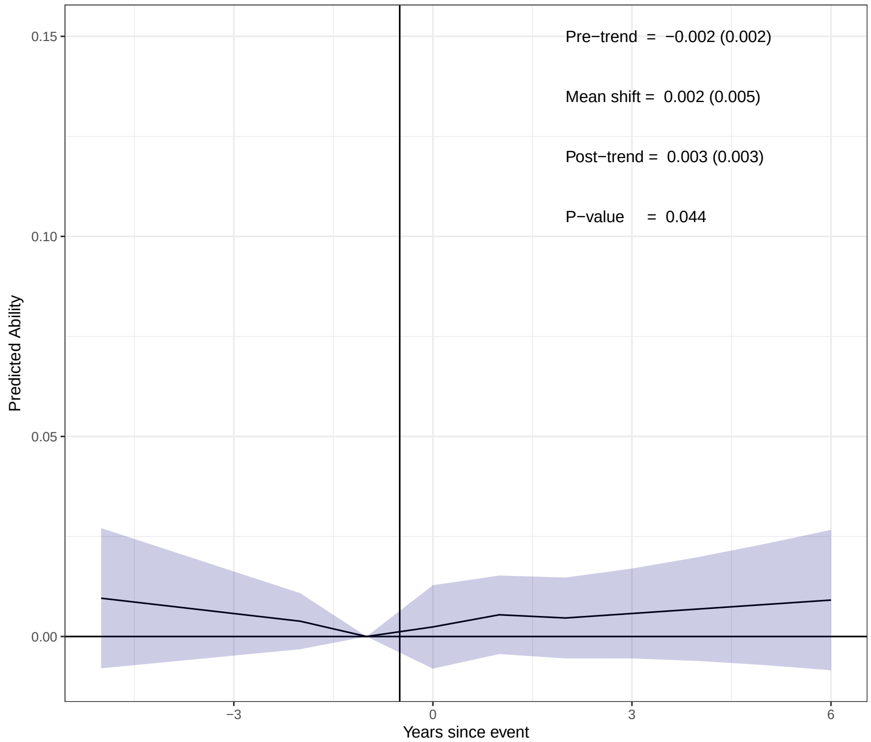


(b) Parametric Match Event Study



Notes: This figure plots the estimates of $g(\hat{\theta})$ defined in Equation 3.2. 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 $\hat{\boldsymbol{\beta}}$. 95 percent confidence intervals are displayed in the shaded regions.

Figure 3.16: Parametric Predicted Ability Event Study

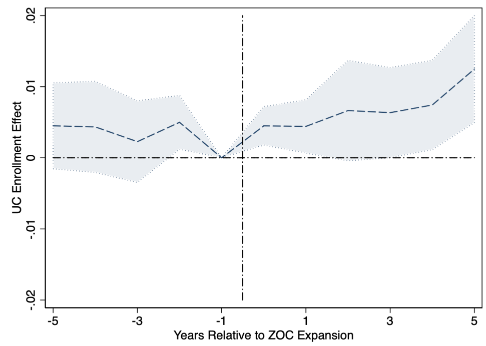


Notes: This figure plots the estimates of $g(\hat{\theta})$ defined in Equation 3.2. The value of the black line shows the difference in the change in predicted student ability (in student σ) between ZOC and non-ZOC students relative to the year before the expansion. Estimates of $\hat{\theta}_1$, $\hat{\theta}_2$, $\hat{\theta}_3$ 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 $\hat{\boldsymbol{\beta}}$. 95 percent confidence intervals are displayed in the shaded regions.

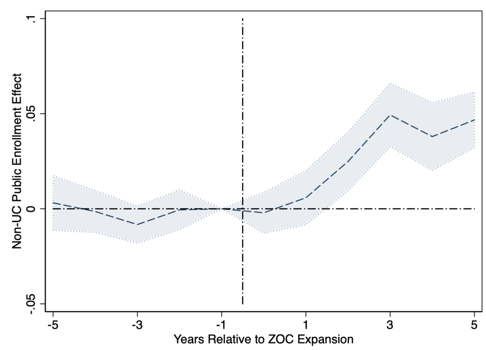
Additional College Enrollment Estimates

Figure 3.17: College Type Event Studies

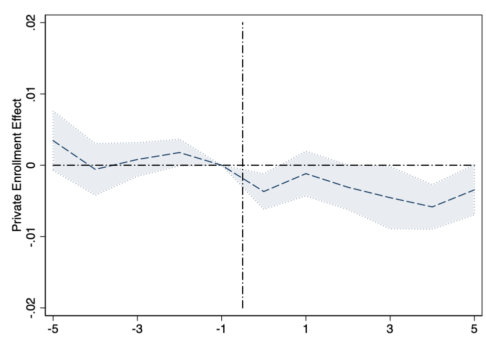
(a) University of California Campuses



(b) California State University Campuses

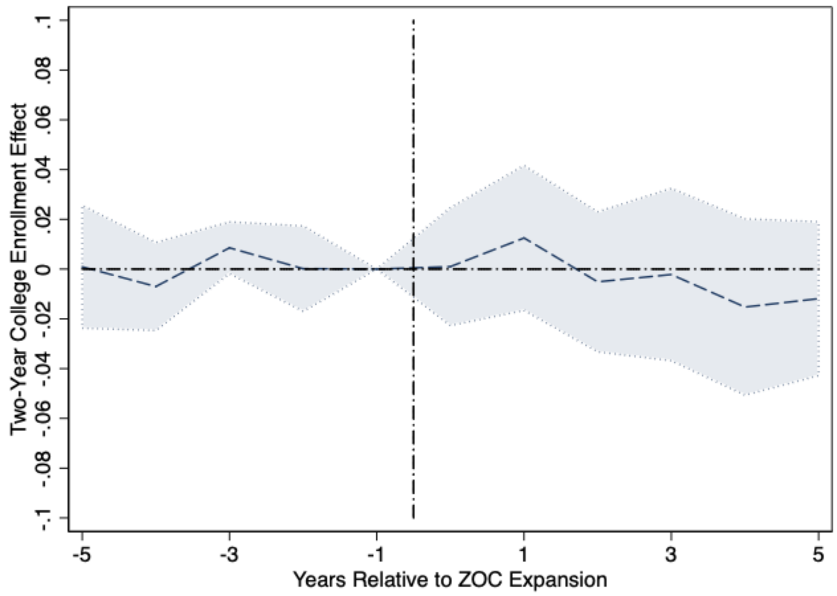


(c) Private Universities



Notes: This figure plots the estimates of β_k analogous to those defined in equation 4.2, 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 3.18: Two-year College Enrollment Effects



3.9.4 Robustness Exercises

3.9.5 Estimating Counterfactual Distributions

In this section, we discuss the methods used to estimate the counterfactual distributions used to construct quantile treatment effects in Figure 3.6b. 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 3.25 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:

$$\Delta^F = F_{zz1} - F_{zz0} \tag{3.10}$$

$$= \underbrace{(F_{zz1} - F_{nz1}) - (F_{zz0} - F_{nz0})}_{\Delta_F^{DD}} + (F_{nz1} - F_{nz0}). \tag{3.11}$$

Equation 3.11 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.

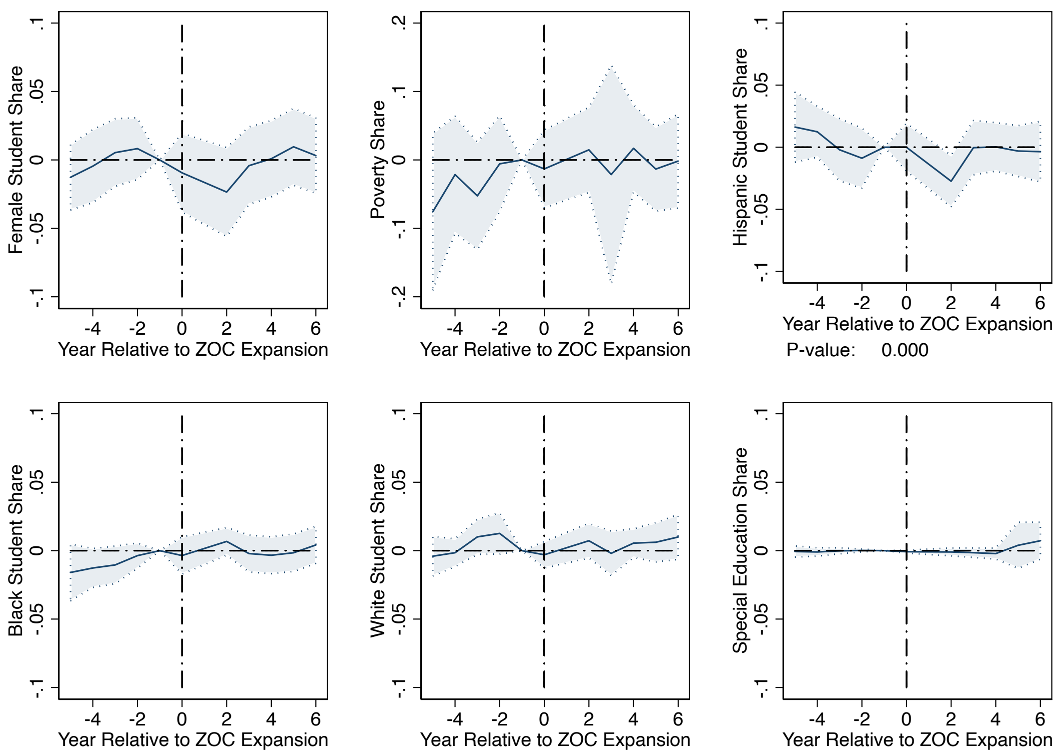
Figure 3.26 reports distribution effects at each point of the distribution's support. The dotted line shows distributional effects in 2019—the difference in CDFs in Figure 3.25—and the dashed line reports distribution effects in 2011. The distribution effects in the pre-period hover near zero across 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.

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

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

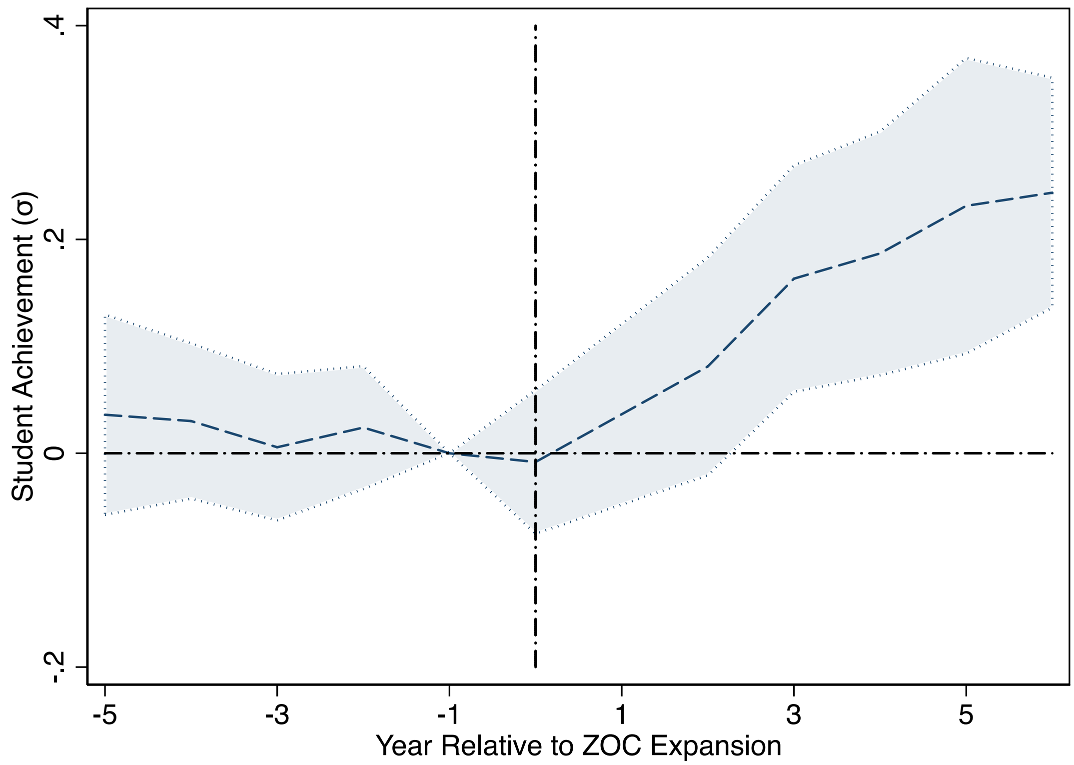
In this decomposition, one portion of the change in the gap is due to changes in student characteristics while the unexplained changes would be attributable to the ZOC program. Figure 3.27 reports these estimates. Although we find some evidence that changes in student characteristics contributed somewhat to changes in the upper regions of the distribution, the overwhelming share of the changes are due to the ZOC program.

Figure 3.19: Changes in student demographics



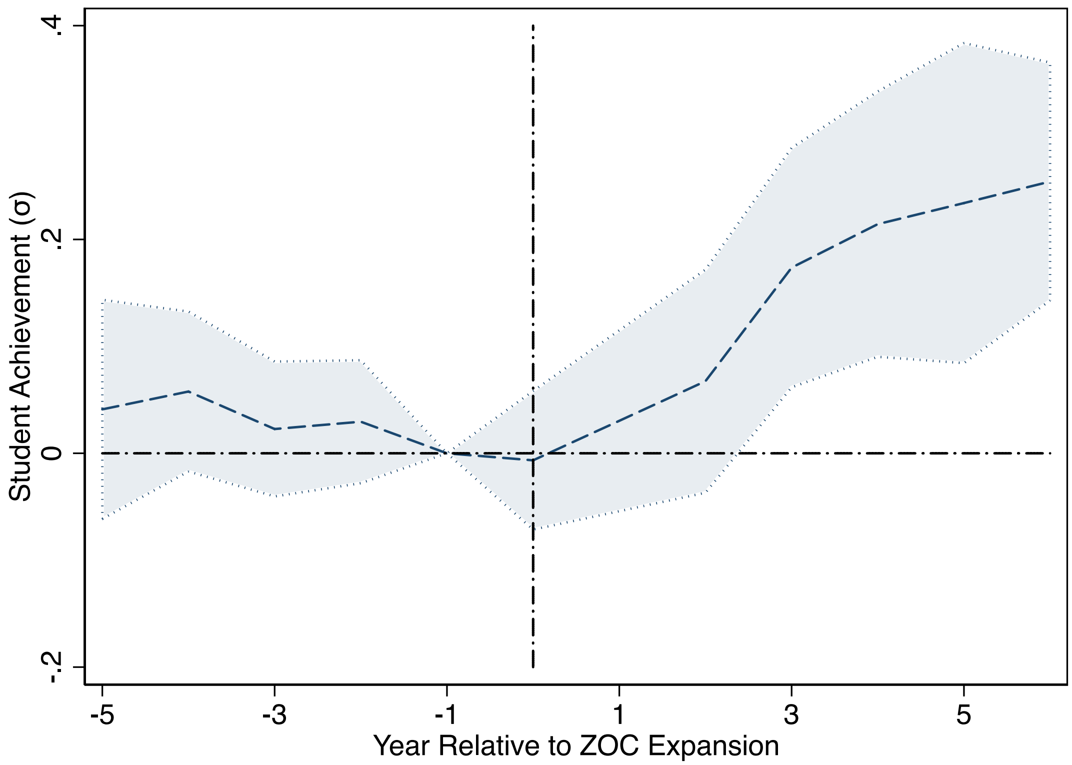
Notes: This figure reports estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The coefficient β_k shows the difference in the change of student characteristics, labeled on subfigure vertical axes, between ZOC and non-ZOC students relative 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 3.20: Achievement event-study restricted to students who didn't move in eighth grade



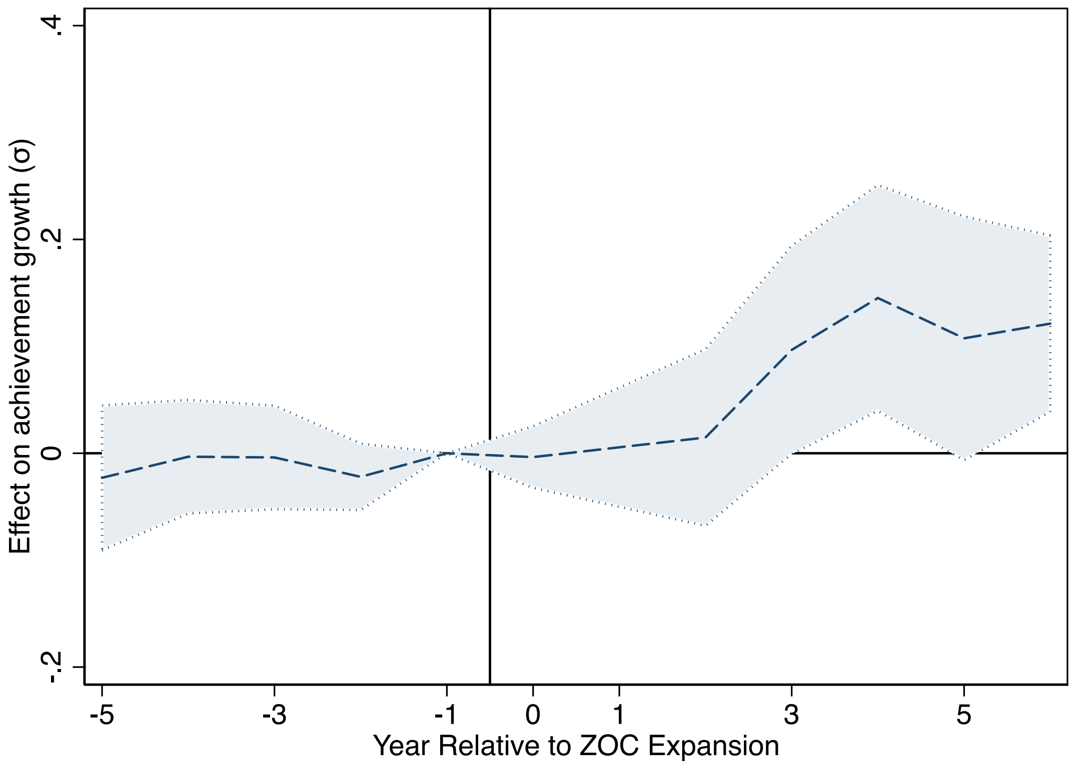
Notes: This figure reports estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The sample is restricted to students that did not move in eighth grade, the year before submit ZOC applications. The coefficient β_k shows the difference in 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 3.21: Achievement event study restricted to students who didn't move in middle school



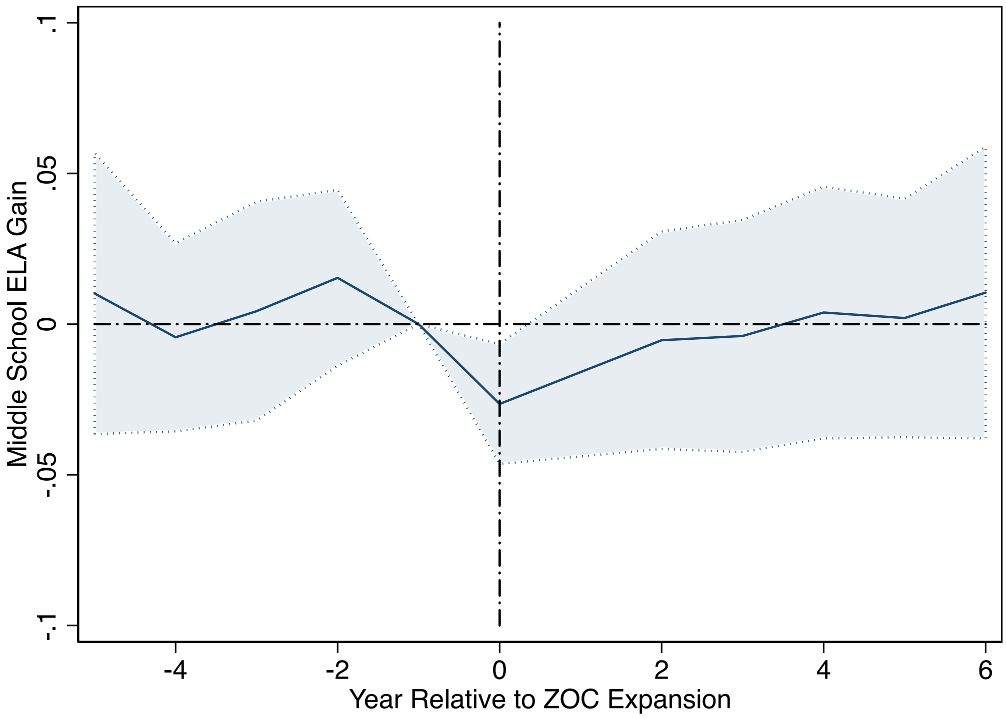
Notes: This figure reports estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The sample is restricted to students that did not move in eighth grade *and* also did not move at anytime during middle school. The coefficient β_k shows the 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 3.22: Within-student achievement gain



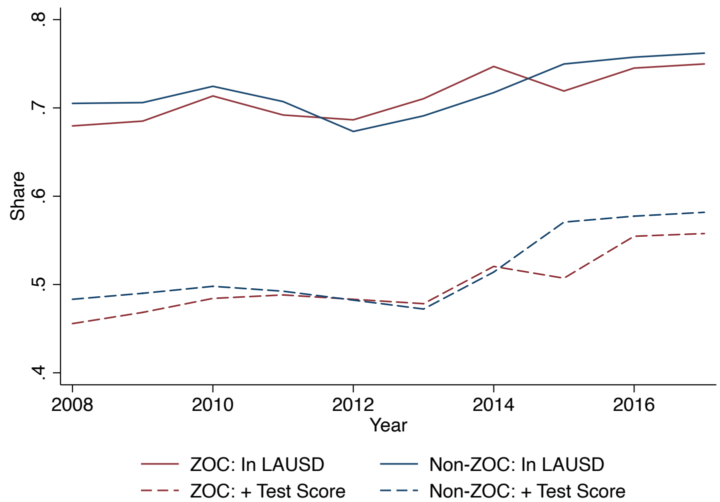
Notes: This figure reports estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The outcome is student-level achievement growth between eighth and eleventh grade, measured in student achievement standard deviations. The coefficient β_k shows 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 3.23: Falsification Test - ZOC Impact on Middle School Gains

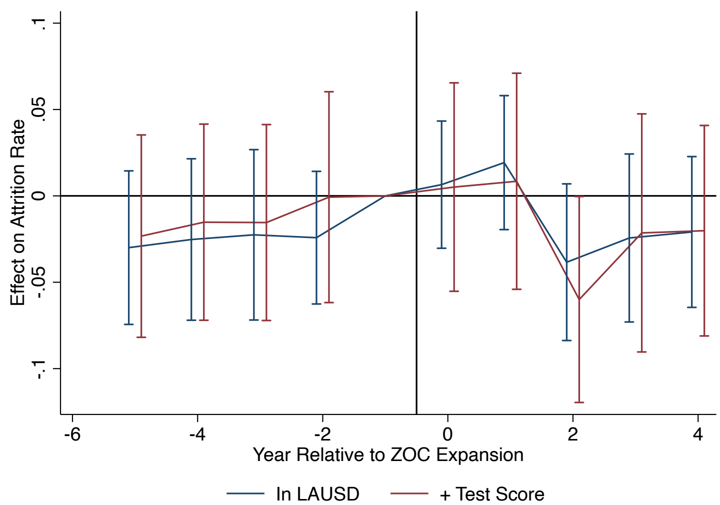


Notes: This figure reports estimates of β_k analogous to those defined in equation 4.2, where k is the number of years since the ZOC expansion. The outcome is student-level achievement growth between seventh and eighth grade, measured in student achievement standard deviations, and predating their ZOC 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 3.24: Attrition Estimates



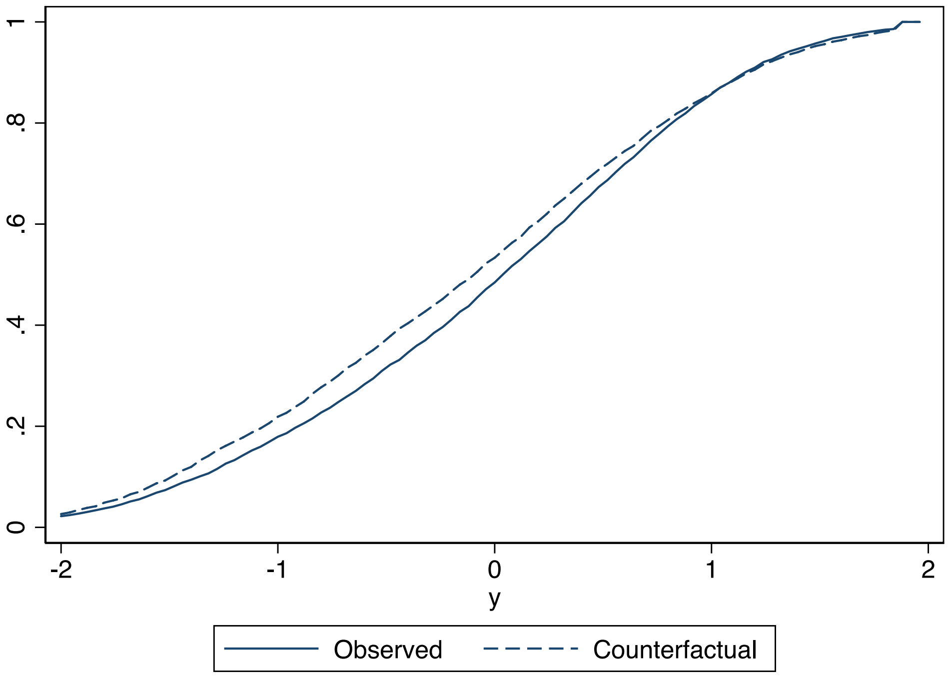
(a) Trends in Attrition Rates



(b) Attrition Event-Study Estimates

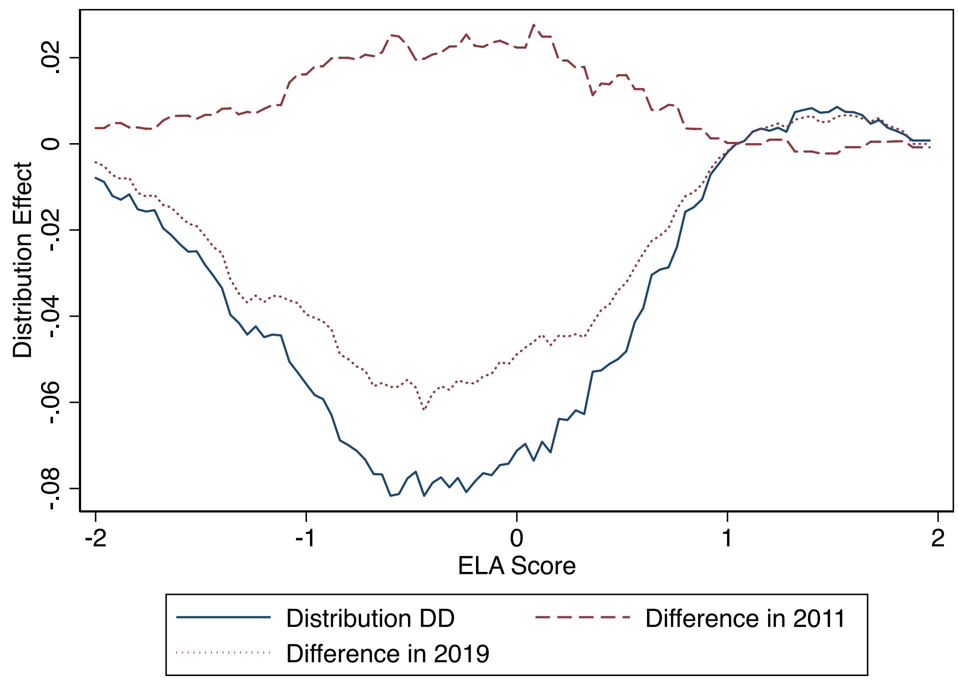
Notes: This set of figures explores non-random attrition out of the sample. Panel (a) reports the share of students enrolled in a high school in ninth grade that are present in eleventh grade and also the share of students in eleventh grade with test scores. Panel (b) reports unadjusted event-study analogs of Panel (a) .

Figure 3.25: Empirical and Counterfactual CDF for ZOC students in 2019



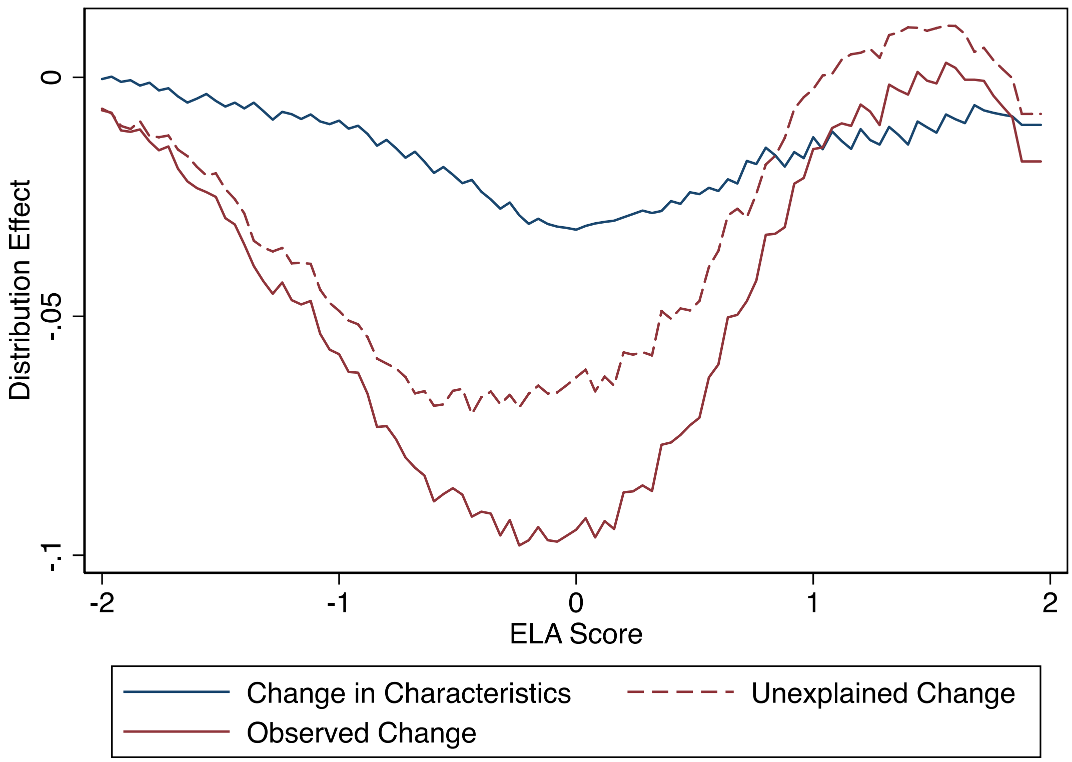
Notes: This figure reports the observed and counterfactual student achievement distribution for ZOC students in 2019. The counterfactual distribution is calculated by integrating the estimated non-ZOC conditional achievement distribution with respect to ZOC student characteristics at each point of support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

Figure 3.26: 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).

Figure 3.27: Decomposition of the change in the sectoral achievement gap



3.9.6 Model estimates

Achievement model estimates

To estimate the parameters of the decomposition, we rely on a selection on observables assumption and estimate Equation 3.4 via OLS. Table 3.10 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. Table 3.11 reports the estimates, but we do not find evidence that there were meaningful changes induced by the policy for most characteristics. Figure 3.41 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.

Utility Model Estimates

Table 3.10: 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 3.11: Summary statistics of time-varying match effects

	Before					Change		
	ZOC		Non-ZOC			ZOC	Non-ZOC	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Difference (5)	Mean (6)	Mean (7)	Diff-in-diff (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)

Table 3.12: 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.

3.9.7 Lottery Details

In this section, we present additional details related to the lottery analysis presented in Section 3.6. 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.

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 3.13 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 3.14 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.

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

$$A_i = \xi + \phi\widehat{A}_i + \sum_{\ell} \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i'\delta + \varepsilon_i \tag{3.12}$$

$$\widehat{A}_i = \psi + \sum_{\ell} \pi_\ell Z_{i\ell} + \boldsymbol{X}_i'\xi + e_i. \tag{3.13}$$

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 3.15 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 Deming et al. (2014); Chetty et al. (2014a); Angrist et al. (2017) 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 3.4. 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 Figure 3.28 corresponding to the overidentifcation test. While the results in Table 3.15 don't imply the OLS value-added estimates are free from bias, they are reassuring moving forward.

Figure 3.28: Reduced Form Effects on First Stage by Lottery

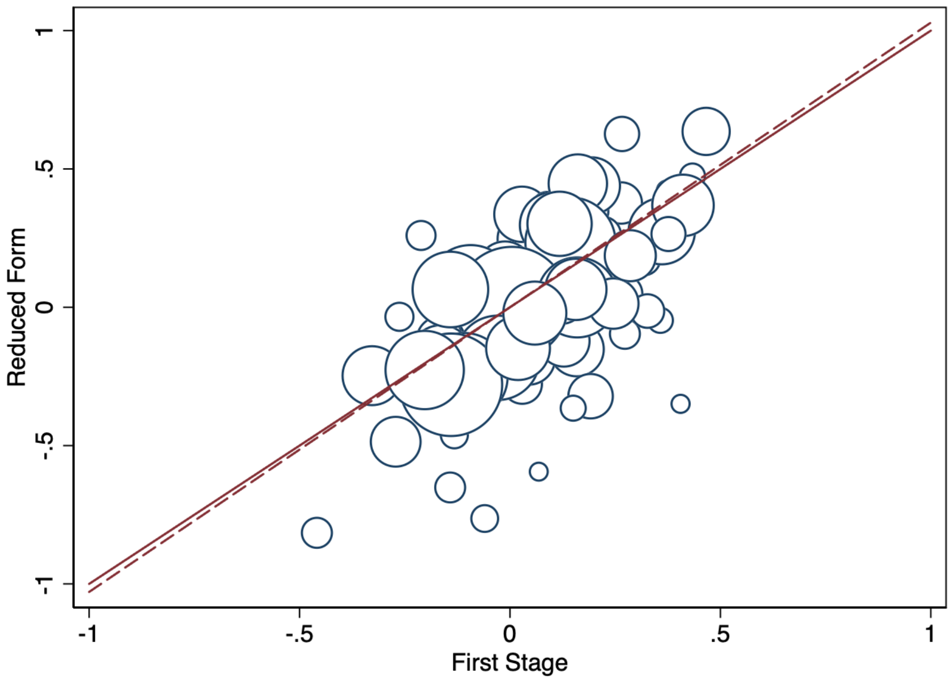


Table 3.13: Lottery Balance

	Not Offered (1)	Offered (2)	Difference (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 3.14: 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 3.13. Standard errors, reported in parentheses, are clustered at the lottery level.

Table 3.15: 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 3.4. Forecast coefficients and overidentification tests reported in Columns 1-3 come from two-stage least squares regressions of test scores on OLS fitted values estimated separately, instrumenting OLS fitted values with school-cohort-specific lottery offer indicators, controlling for baseline characteristics.

Table 3.16: 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.

Table 3.17: Complier characteristics by cohort

	2013 (1)	2014 (2)	2015 (3)	2016 (4)	2017 (5)	P-value (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)	

3.9.8 Additional Empirical Results

More on the compression of within-zone school quality

To further investigate the distributional changes, Figure 3.29 estimates treatment effects on the the within-zone standard deviation of school effectiveness.[15] 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 (Deming et al., 2014; Chetty et al., 2014a; Rivkin et al., 2005), and in some settings roughly 0.2 (Walters, 2015; Angrist et al., 2017). 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.

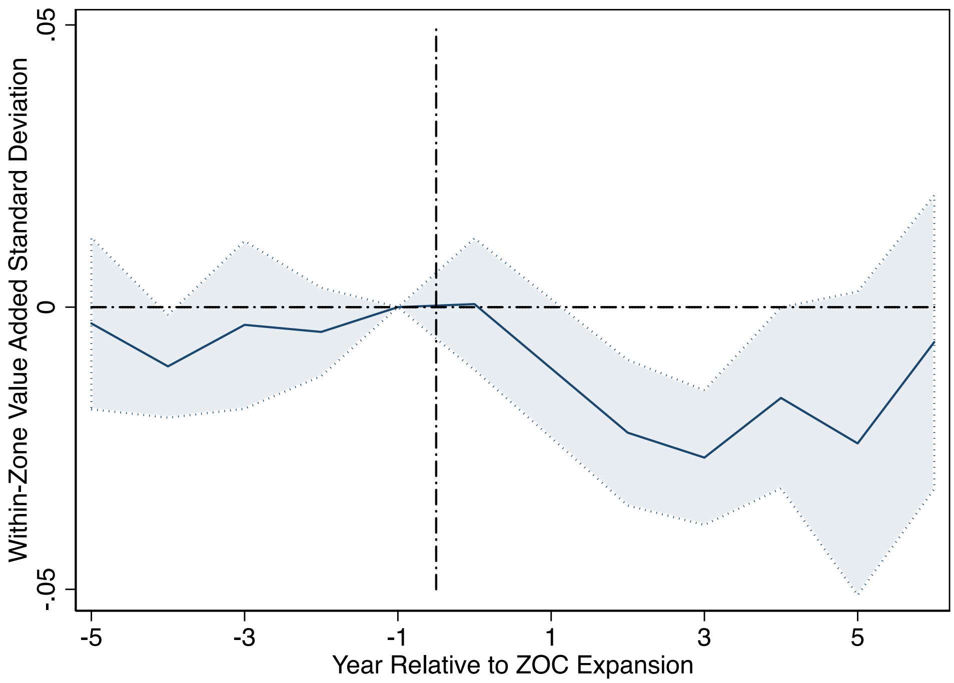
Alternate Quantile Treatment Effects

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 3.30 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 3.6b using the methods proposed by Chernozhukov et al. (2013).

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

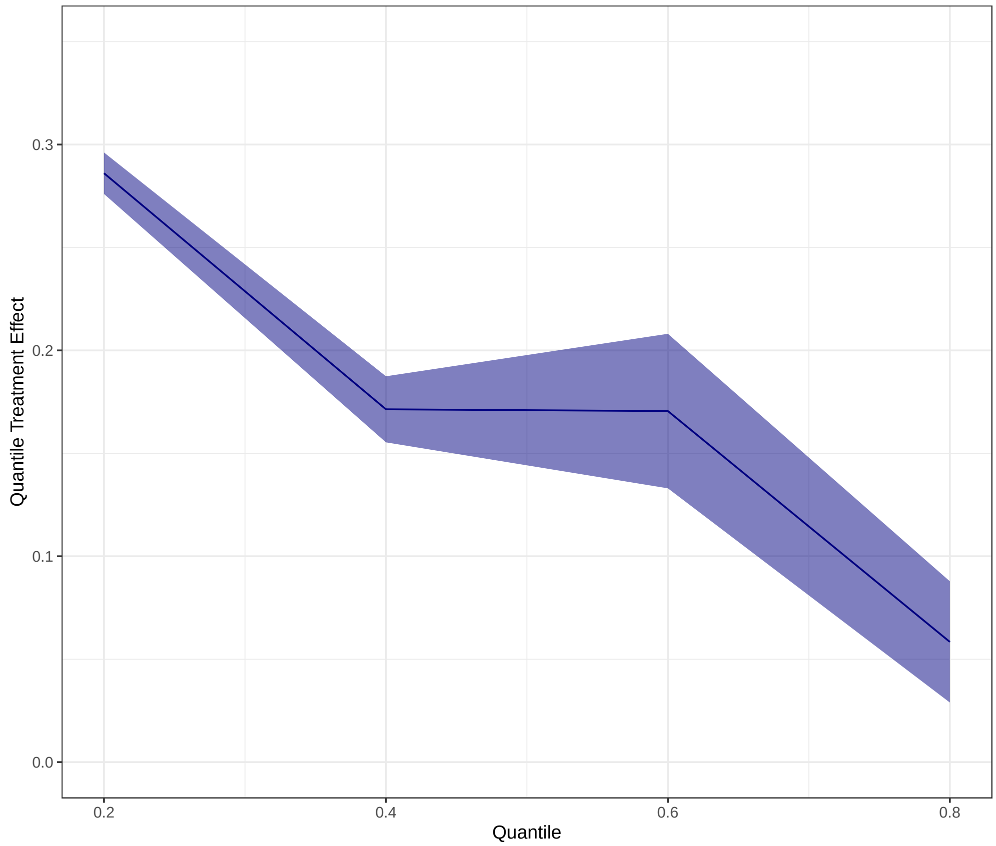
$$\sigma^2_{\alpha zt} = \frac{1}{J_z} \sum_{j \in z} \left((\hat{\alpha}_{jt} - \bar{\alpha}_{zt})^2 - SE(\hat{\alpha}_{jt})^2 \right).$$

Figure 3.29: 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.

Figure 3.30: 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.

3.9.9 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; Gershenson et al., 2018; Fairlie et al., 2014). 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.[16] We track the number of same-race teachers students are exposed to and study ZOC impacts on racial match propensity. Figure 3.31 reports event-study estimates analogous to Equation 4.2 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.[17] 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.

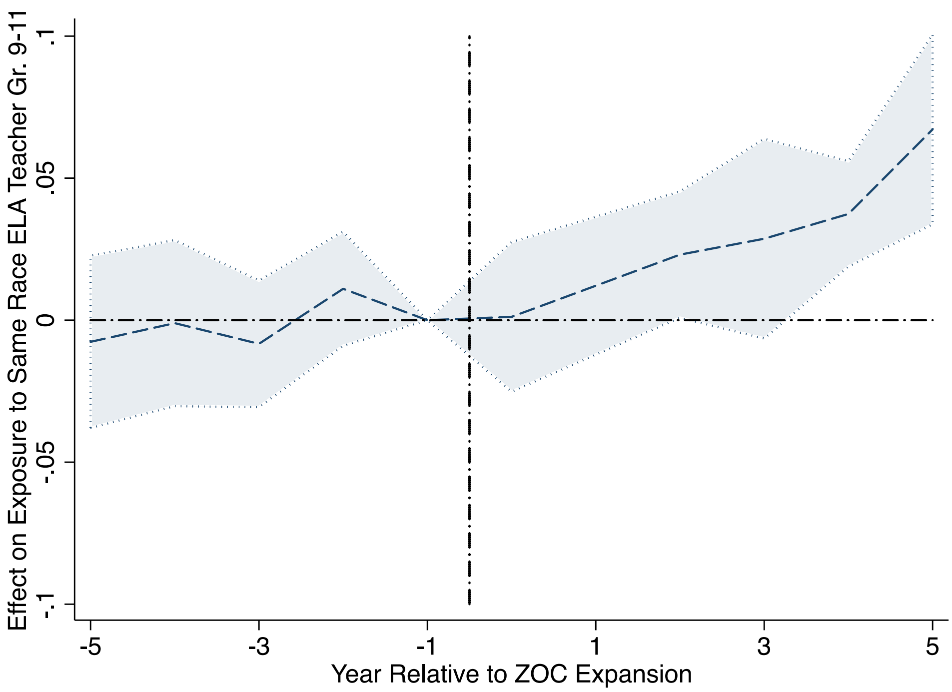
Impacts of same-race teachers have been shown to produce both short- and long-run improvements in outcomes for underrepresented racial minorities (Dee, 2004; Gershenson et al., 2018; Fairlie et al., 2014). In particular, Gershenson et al. (2018) find that Black students randomly assigned a Black teacher 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 Section 3.9.10.

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.

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

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

Figure 3.31: Same-race Teacher Event-Study



Notes: This figure plots the estimates of β_k analogous to those defined in equation 4.2, 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.

3.9.10 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).[18] Figure 3.32 reports ZOC and non-ZOC ASI averages at multiple incoming achievement cells. Even though there are level differences in ASI between ZOC and non-ZOC students, both share a common feature that students at the tails of the achievement distribution have higher average ASI. This observation is indicative of tracking practices existing in both ZOC and non-ZOC schools, with tracking practices being more pronounced for high-achieving students.

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

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

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

Figure 3.33 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

[18] Section 3.9.11 provides estimation details and statistics. We also provide results using classroom incoming achievement standard deviations and school-level between-classroom shares of variance.

trends assumptions. In the first few post-periods, we also do not not detect any differential 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; Duflo et al., 2011; Cohodes, 2020; Card and Giuliano, 2016; Bui et al., 2014). 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.

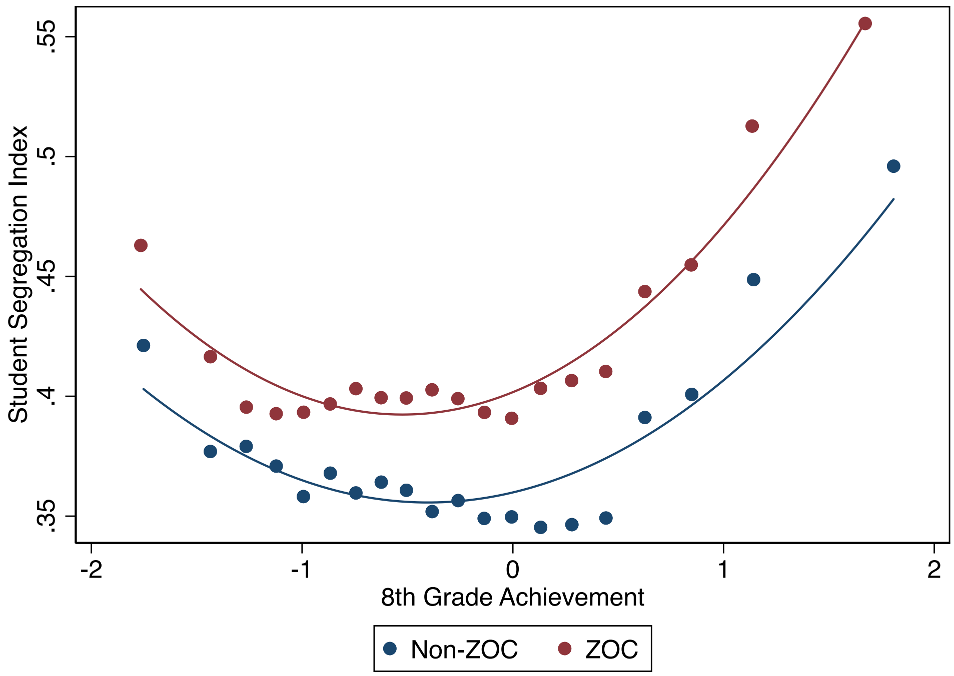
Changes in school inputs

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 3.34. Similarly, Figure 3.35 shows that both the quantity or quality of teachers did not change between the two sectors.[19] 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.

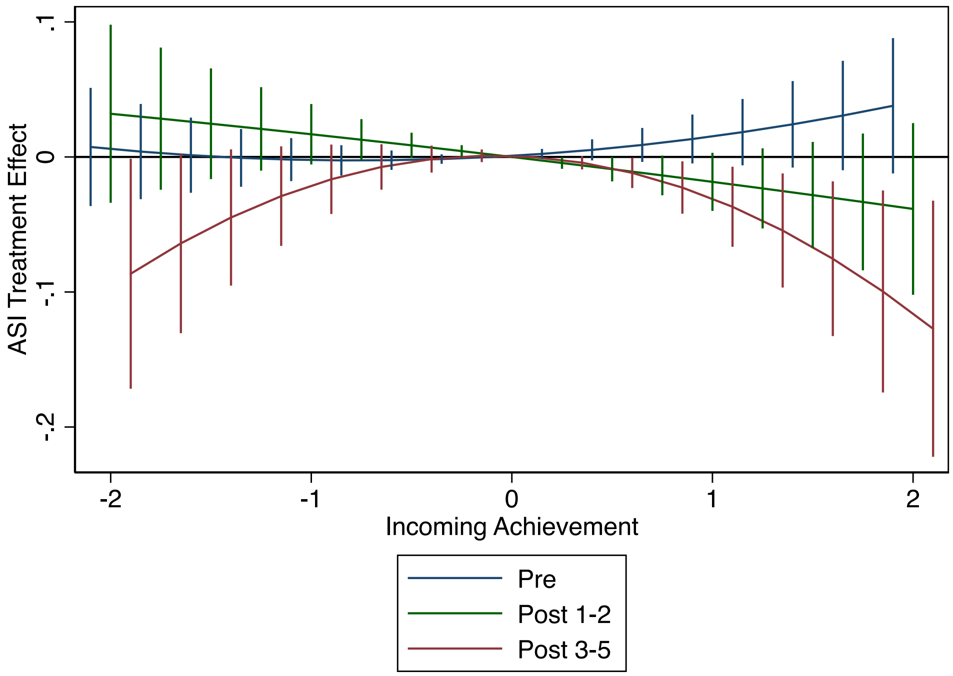
[19] We estimate within-school teacher value-added in the pre-period and track changes in teacher quality with respect to the baseline estimate teacher value-added.

Figure 3.32: Estimated ASI Averages by Incoming Achievement



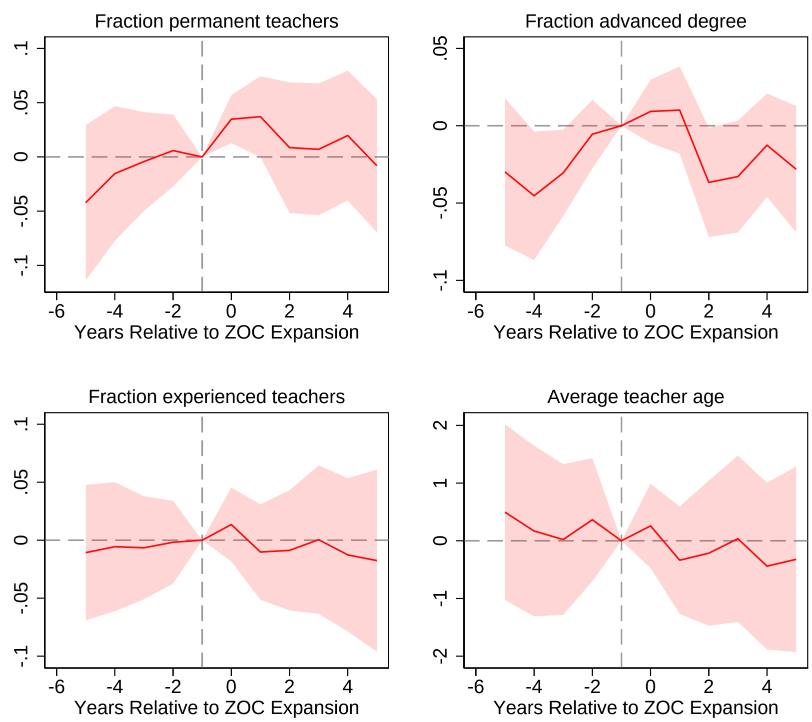
Notes: This figure reports school-level event-study estimates from regressions of an outcome on school fixed effects, year fixed effects, and event-time indicators interacted with ZOC dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the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

Figure 3.33: ASI Treatment Effects by Incoming Achievement



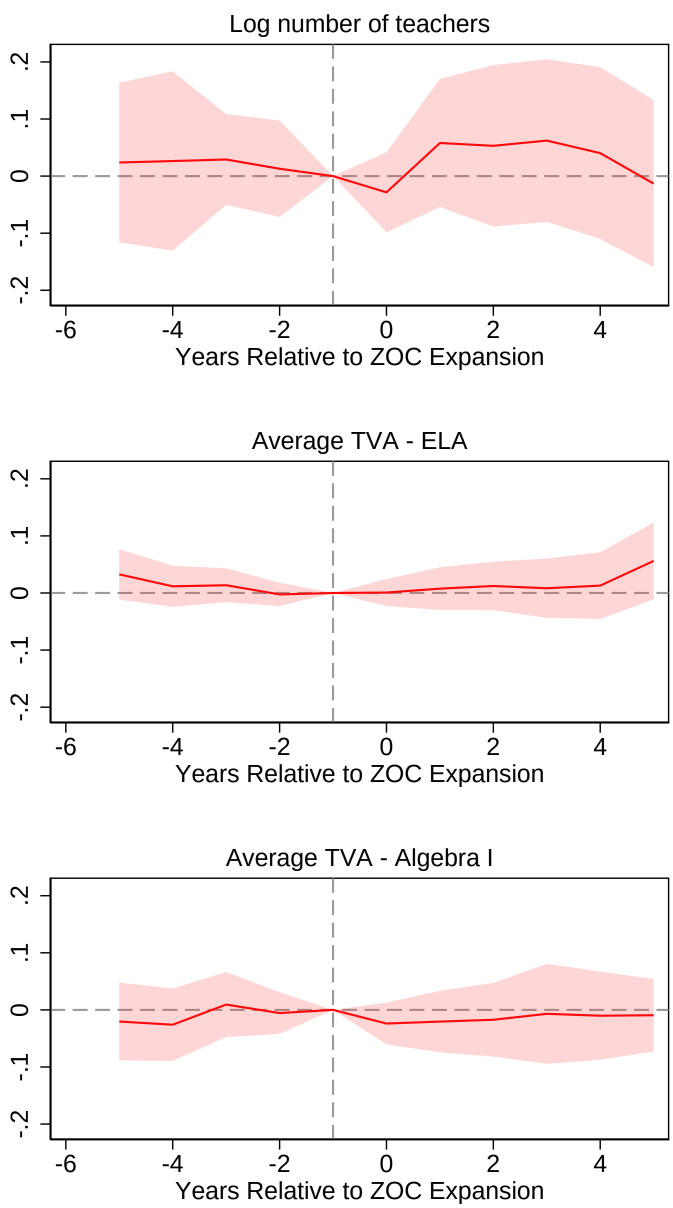
Notes: This figure reports school-level event-study estimates from regressions of an outcome on school fixed effects, year fixed effects, and event-time indicators interacted with ZOC dummies. Outcomes are school-level averages for various teacher characteristics. Standard errors are clustered at the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

Figure 3.34: 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.

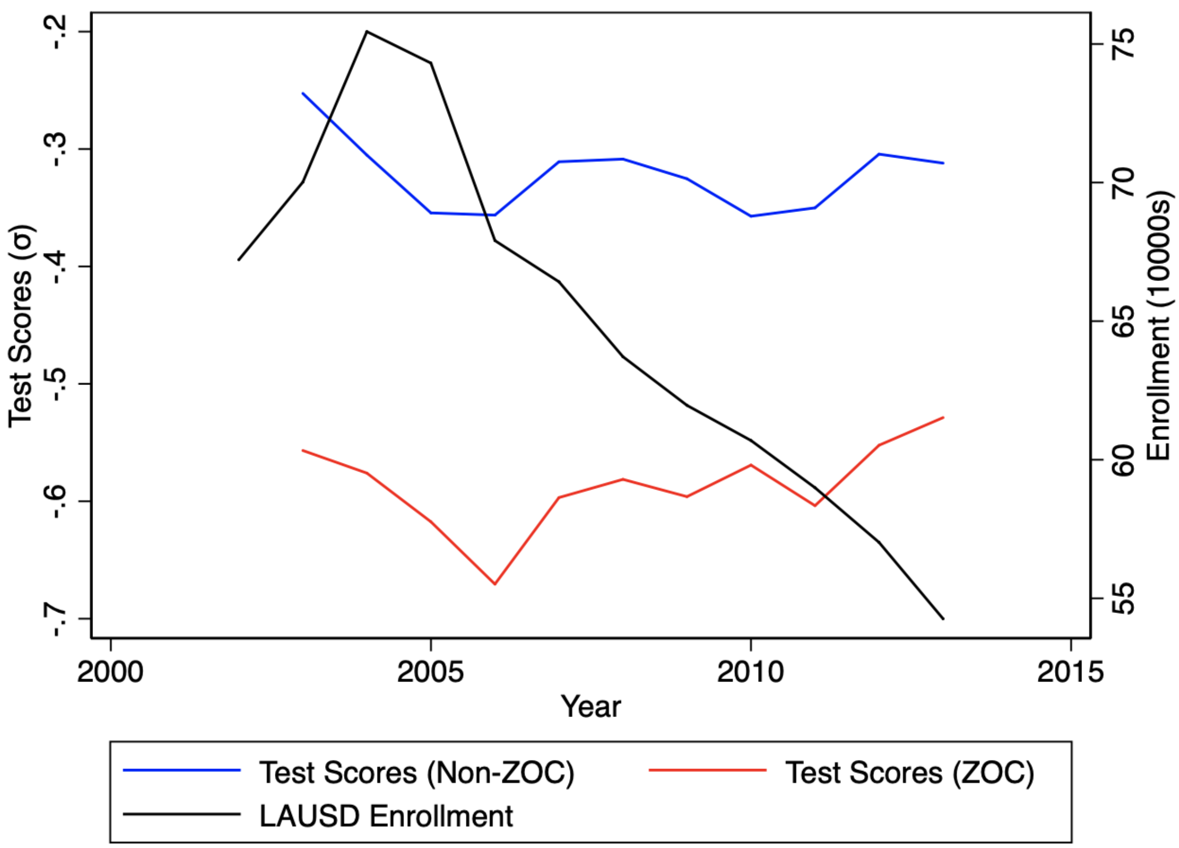
Figure 3.35: Teacher Quantity and Quality Event Studies



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.

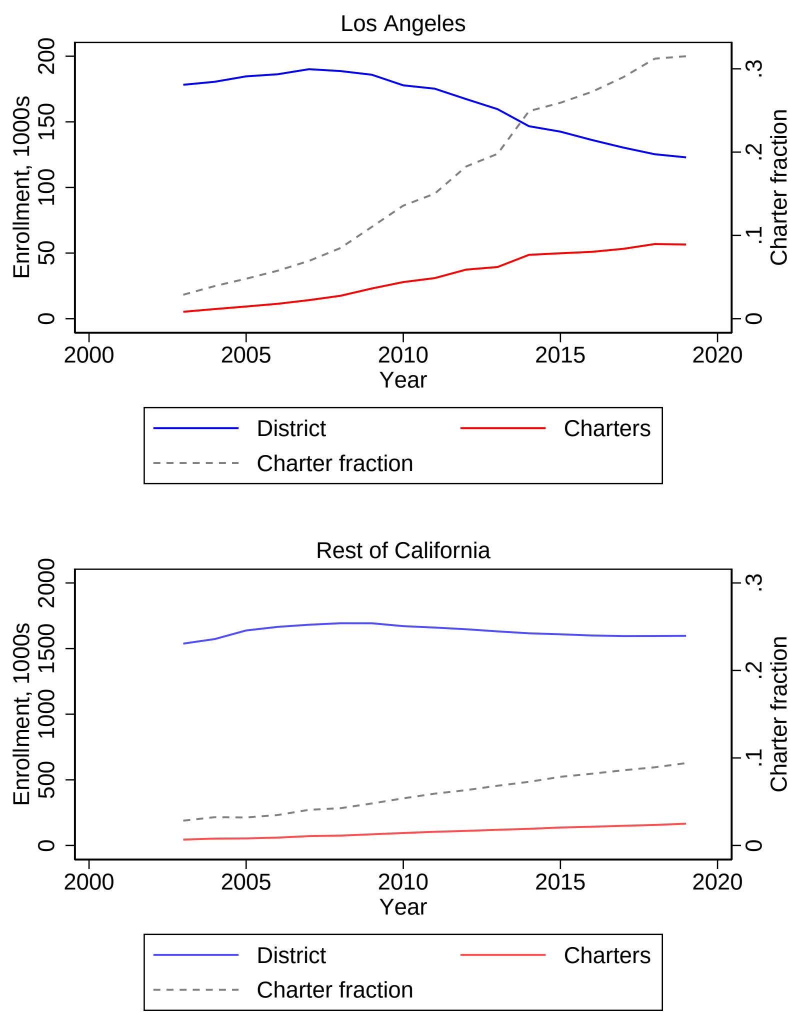
3.9.11 Additional Empirical Results

Figure 3.36: LAUSD: 2002-2013



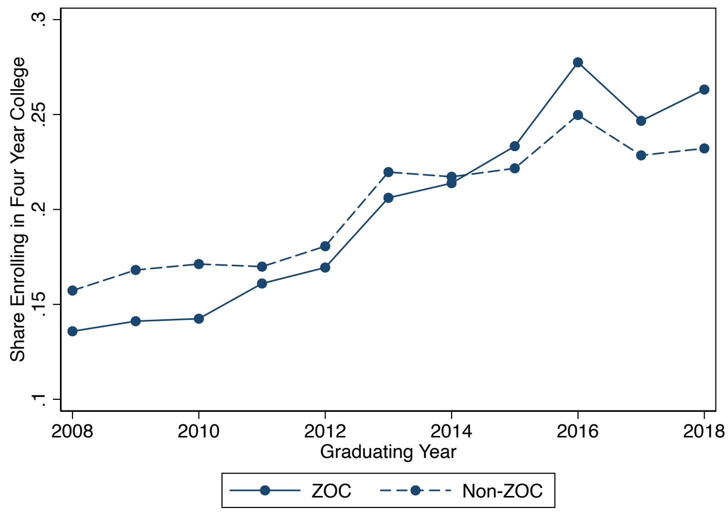
Notes: Enrollment numbers come from admistrative data provided by LAUSD. The California Department of Education provides California Standards Test (CST) statewide means and standard deviations which we use to standardize test scores in this figure. Test scores are ninth grade ELA scores, an exam that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

Figure 3.37: Los Angeles and California enrollment

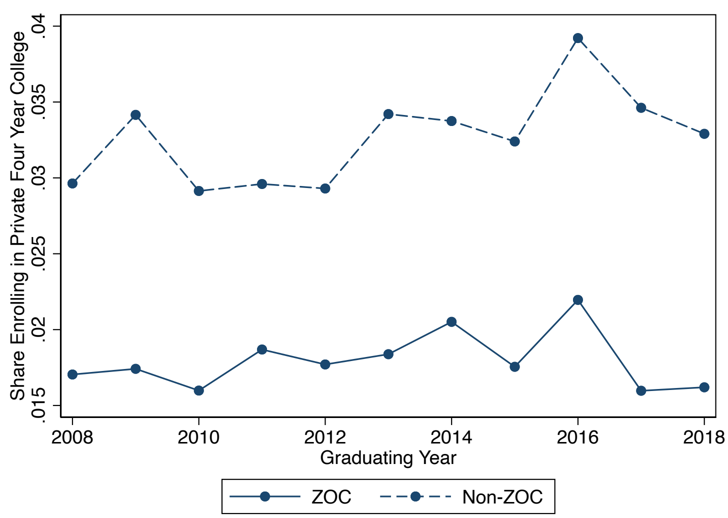


Notes: This figure shows enrollment in thousands for grades 9 through 12, separately for district and charter schools. Enrollment data is from the California Department of Education.

Figure 3.38: College Outcomes



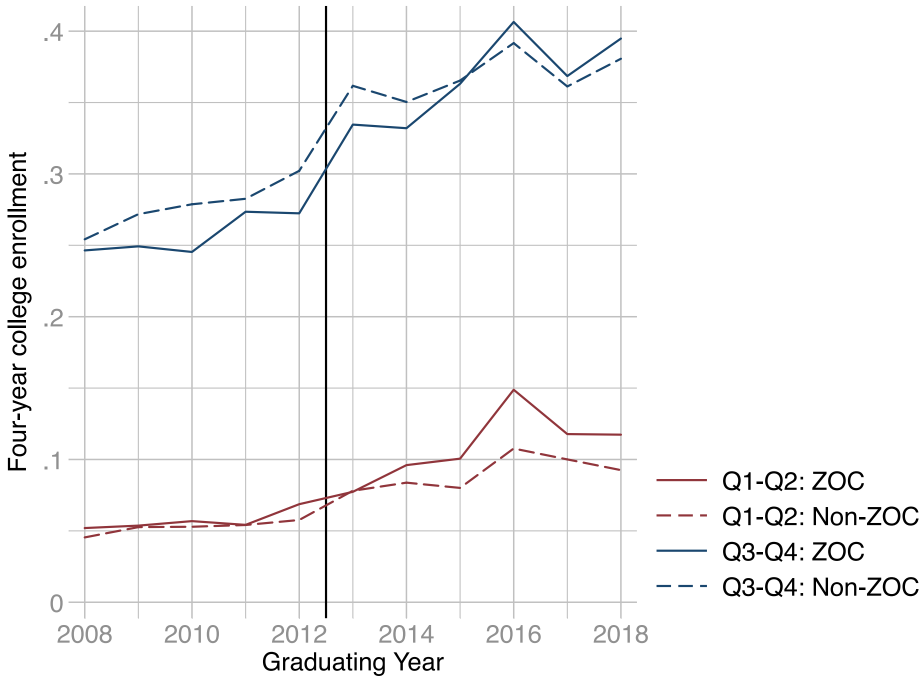
(a) Four-year college enrollment



(b) Private college enrollment

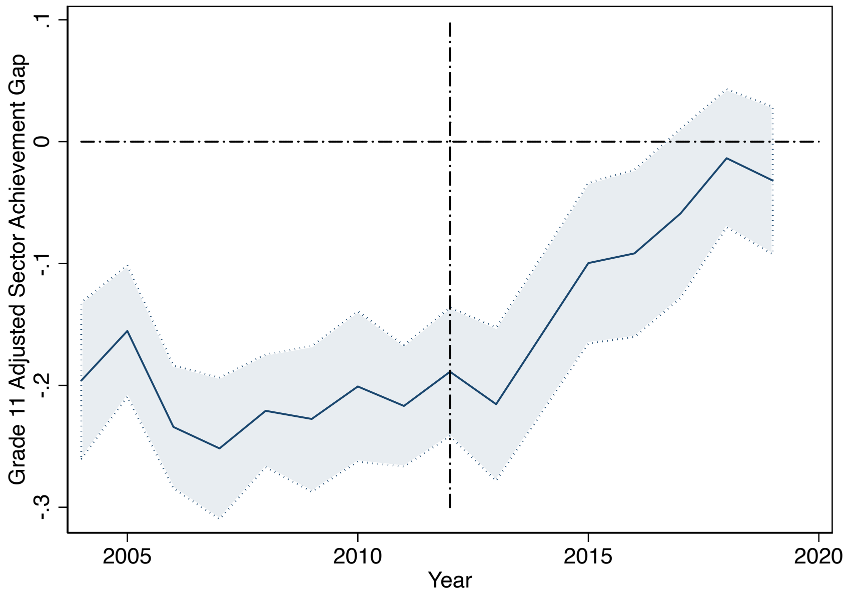
Notes: These figures report four-year college enrollment rates reported to the NSC for multiple graduating cohorts. Panel A reports college enrollment rates at any four-year college and Panel B reports enrollment rates at private colleges.

Figure 3.39: 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.

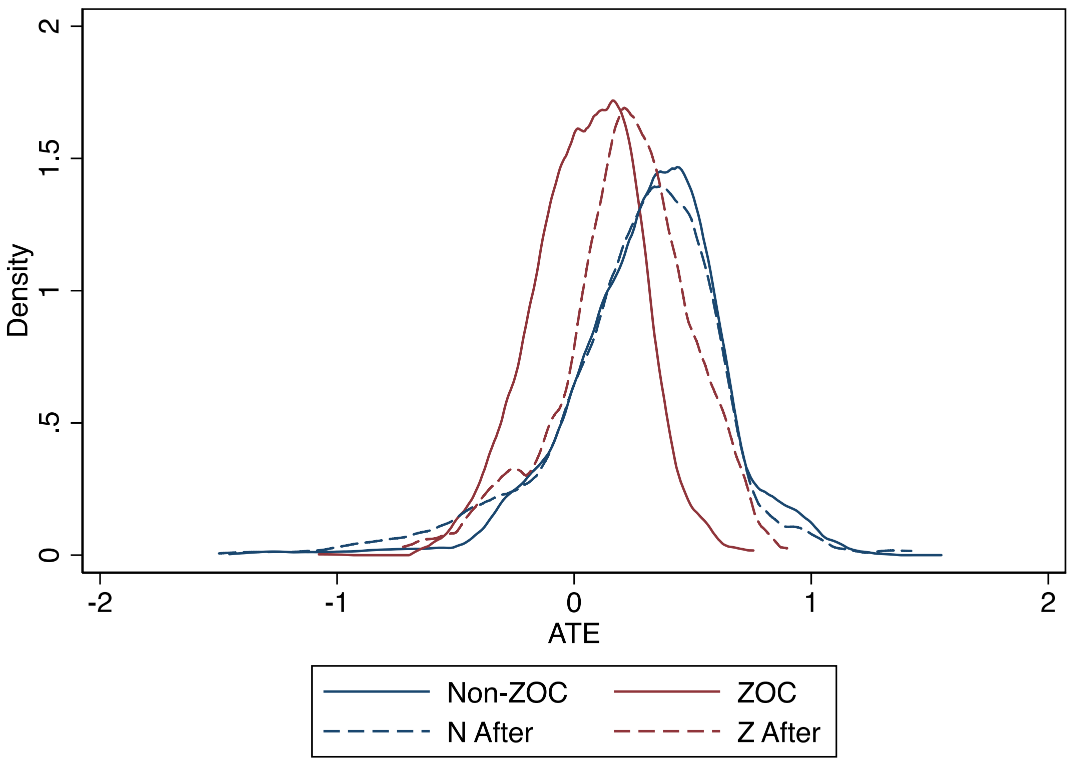
Figure 3.40: Eleventh-grade ZOC achievement gaps



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.

OGV

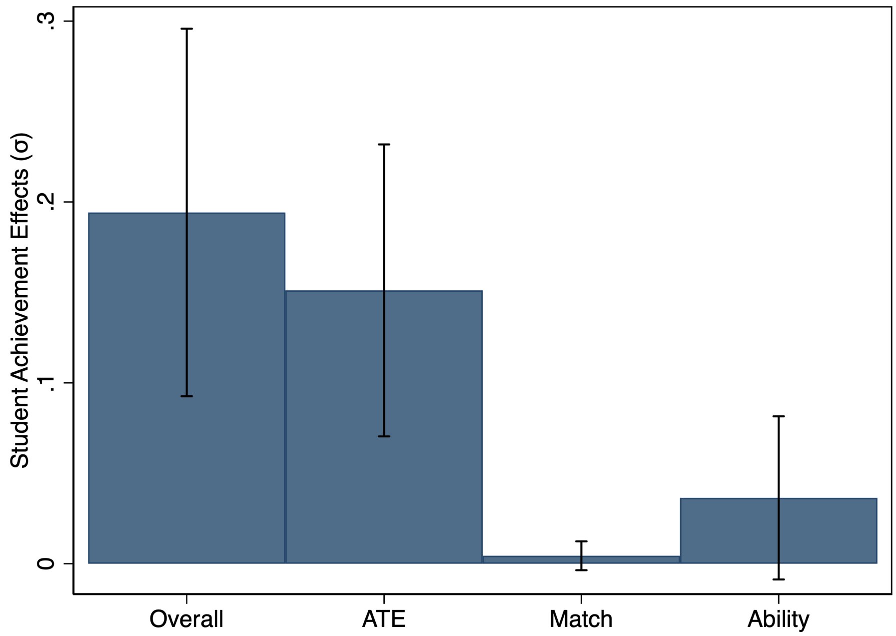
Figure 3.41: ATE Distributions before and after ZOC expansion



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

OGV

Figure 3.42: Achievement Effect Decomposition at Year 6



Notes: This figure reports estimates of β_6 defined in equation 4.2 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.

Chapter 4

Preferences for Schools and Social Interactions: Experimental Evidence from Los Angeles

4.1 Introduction

In education markets, school incentives are shaped by parents' preferences for school quality. But school quality in education markets is ambiguous and challenging to observe, potentially producing settings where parents provide schools weak incentives to compete (Abdulkadiroğlu et al., 2020; Rothstein, 2006; Burgess et al., 2015). An often advanced hypothesis is that difficulties in information acquisition complicate how parents make decisions and can affect the manner in which families select schools. In light of imperfect information, parents may resort to their social networks to obtain relevant information. This suggests that the role social interactions play in generating choices (e.g., Banerjee (1992); Manski (2000)) may further amplify biases and shape preferences if some social networks are less informed. Understanding the role that information imperfections, preferences, and social interactions play in shaping schooling decisions remains paramount but not fully understood.

One strand of literature uses information provision experiments to document how parents' schooling choices change in response to receiving information (Hastings and Weinstein, 2008; Corcoran et al., 2018; Andrabi et al., 2017; Ainsworth et al., 2020). While these papers provide compelling evidence of imperfect information and show that information provision can be effective, they are agnostic about the different quality margins that parents may value and how that affects schools' incentives. To date, most of these papers distribute information on either a school's performance level (Hastings and Weinstein, 2008; Andrabi et al., 2017), or a school's performance growth (Ainsworth et al., 2020), but not both, and are less capable of studying the relative preference channel. A growing number of papers tackle the more empirically challenging preference channel but arrive at mixed conclusions (Abdulkadiroğlu et al., 2020; Rothstein, 2006; Beuermann et al., 2018; Campos and Kearns, 2021). A third strand of papers study the how preference interactions can general choices, segregation patterns, and schooling decisions (Pollak, 1976; Schelling, 1971; Schneider et al., 1997), but empirical evidence is limited. There remains scope to use information interventions to learn about both preferences and social interactions, but that avenue is yet to be explored in the literature.

This paper fills that gap by using an information provision experiment in a setting where parents are required to participate in a school choice program. I use this experiment to answer four questions: How imperfectly informed are parents about school and peer quality? What are the sign and direction of any biases, if any? Do social interactions matter for schooling decisions? When perfectly informed about school and peer quality, do parents prefer higher quality (value-added) schools or do they prefer schools with higher achieving peers?

The setting is the Zones of Choice program, covering 30-40 percent of high school students in the Los Angeles Unified School District (LAUSD). The program offers parents constrained neighborhood choice, where their choice set consists of multiple nearby options but remains constrained by expanded attendance zone boundaries. The intervention takes place during the 2019 cohort's application cycle that occurs while they are enrolled in middle school.

To study biases and discrepancies in information, a survey is distributed to students middle school classrooms. The purpose of the survey is to elicit beliefs, preferences, and obtain a deeper understanding of how informed parents are about the decision-making process they have to participate in. The survey is distributed and collected by middle school homeroom teachers, and students are incentivized to participate.

The information intervention studies social interactions and relative preferences. To study parents' interactions, the experiment employs a two-stage randomization procedure that allows us to detect spillover effects from information treatments (Crépon et al., 2013). In the first stage, students' middle schools are assigned to treatment groups, with one middle school in each zone assigned as a pure-control school indicating no parents in that school receive any treatment. Conditional on middle school treatment status, parents are randomly assigned treatment. Spillover effects are identified by comparing the outcomes of untreated parents in treated schools to that of untreated parents in untreated schools. Because untreated parents don't directly receive any information, any differences are due to information *spillovers*, which I interpret as due to parents interacting.

To study relative preferences, I cross-randomize two different treatments. This addition allows us to compare differences in behavior by treatment status and study how the interaction of the two quality margins affects choice behavior. Intuitively, I present some parents with settings where they are perfectly informed about the different quality metrics, allowing me to partly address the imperfect information channel and study relative preferences for peer and school quality. Changes in the implicit utility weights they place on peer and school quality allow me identify changes in relative preferences under a few additional assumptions.

Using only the first-level of the randomization—the school-level—I find that information on either quality margin changes the decisions parents make. Parents in treated schools list top-ranked schools with different peer compositions, although the differences are small in magnitude, partly due to the mostly homogeneous nature of students within each zone of choice. Using past cohorts' application data, I conduct placebo-like tests in a difference-in-differences framework to see if my reduced-form effects are partly driven by persistent preference heterogeneity or some unobservable imbalance. By comparing changes in choice behavior among parents in treated and untreated schools before and after the intervention, I can difference out any unobservable and persistent characteristic. I fail to reject no impacts among untreated cohorts, but find strong impacts in the treated cohort. The difference-in-difference estimates show that the value-added of parents' top-ranked schools increased, while the incoming achievement (peer quality) of parents' top-ranked schools decreased.

I then use the experiment's complete design to demonstrate that social interactions are prevalent, generating treatment effects for untreated parents in treated schools that are similar to that of treated parents. I also find that treatment effects are mostly homogeneous across different treatment arms, suggesting that social interactions are sufficiently large to generate market-level consensuses regarding preferences and schooling decisions. The consensus shifts demand toward higher value-added schools coupled with a systematic shift towards schools with lower peer incoming achievement. The evidence suggests that actively distributing information about both schools' peer composition and their causal impact on student learning, can change demand in a way consistent with demand-side behaviors inducing competitive pressures to schools to compete on value-added.

I complement the reduced-form evidence by estimating impacts on the implicit weights that parents place on the two quality metrics. All else equal, compared to parents without access to any information, parents that either directly or indirectly received information were willing to drive an additional 0.6 miles to access a school that was 10 percentile rank points higher in terms of value-added. In contrast, holding all else constant parents directly or indirectly exposed to information were willing to drive 2.5 miles less than unexposed parents for ten additional incoming achievement percentile rank points. Again, I find that treatment effects on implicit utility weights are similar across the various treatment arms, providing suggestive evidence that parents reached market-level consensuses. I find suggestive evidence of substantial parental interactions sufficiently strong to address difficulties surrounding interpreting differences between peer and school quality.

I then leverage neighborhood-level variation in exposure to treated parents to assess if proximity to neighbors treated neighbors produced differential impacts. This allows me to explore the empirical relevance of school-level interactions, where cursory discussions are more likely, and neighborhood-level interactions, where there is scope for detailed discussions. I find evidence suggesting that proximity to treated neighbors produced an additional layer of interactions. Treatment effect gradients were larger among parents with more nearby neighbors. The neighborhood-level evidence highlights the importance of local social networks for schooling decisions, and the potential for information barriers to inhibit benefits for some groups where information is less readily available.

Related Literature

This paper contributes to an extensive information provision literature studying various settings. In the K-12 education literature, most papers distribute information on school performance levels. Hastings and Weinstein (2008) examines a natural experiment and field experiment that provided information on school performance levels to low-income families in Charlotte-Mecklenberg, highlighting the importance of information provision. In New York, Corcoran et al. (2018) study the impact of personalized information treatments and found both disadvantaged and comparatively advantaged students both used the information. Outside of the United States, Andrabi et al. (2017) study the impact of providing school report cards (school performance levels) on test scores, prices, and demand, finding that private schools in Pakistan exploit information asymmetries in pricing and quality determination. Ainsworth et al. (2020) study the impacts of distributing value-added information to parents in Romania, finding parents are responsive to information but don't always select the highest value-added school in their feasible choice sets. In contrast to these papers that focus on one quality measure, this paper studies the impact of simulataneously providing two measures of quality, contributing to this literature and to a broader discussion around the availability of value-added information.[1]

By distributing information on multiple measures of quality, this paper can study changes in relative preferences. In that spirit, this paper is related to a growing body of evidence studying parental preferences. Parental preferences ultimately determine the incentives that schools face in settings with expanded scope for choice, making them paramount in the broader school choice discussion. Rothstein (2006) studied preferences indirectly and found that households do not seem to place significant weight on school effectiveness. More recent papers leverage information contained in rank-ordered lists that parents submit as part of school choice programs to estimate demand parameters (Agarwal and Somaini, 2019). The evidence in these papers is more mixed, with Abdulkadiroğlu et al. (2020) finding evidence that corroborates Rothstein (2006), while evidence outside the country or in segregated neighborhoods in Los Angeles find suggestive evidence to the contrary (Beuermann et al., 2018; Campos and Kearns, 2021). The approach taken in this paper combines both reduced form and choice modeling to study how information can change families choices and to understand how relative preferences can be impacted by information campaigns.

A third related literature studies preference interactions, and how preference interactions affect general consumer behavior, segregation patterns, and schooling decisions. Pollak (1976) introduced a simple modeling framework that allowed the utility of consumers to depend on the consumption bundles of others. In the context of this paper, parents may be similarly influenced by the opinions or information of other parents. But in a similar way that Schelling (1971) demonstrated how extreme segregation can arise from social interac-

[1] GreatSchools.org provides an abundance of information about school quality but was criticized for penalizing schools in disadvantaged communities by only highlighting school performance levels and not performance growth metrics. There remain differences across states in the availability of growth metrics—typically referred to as school value-added.

tions, school segregation can arise in school choice settings. As Schneider et al. (1997) points out, the importance of social networks in schooling decisions can introduce vast inequalities if there are disparities in how different groups acquire information that they ultimately share with each other. In this respect, this paper is the first to empirically document the importance of social interactions in a real school choice setting.

The rest of this paper is structured as follows: in Section 4.2, I present a simple conceptual framework that motivates the experimental design and the interpretation of treatment effects; in Section 4.3, I describe the setting, the design of the experiment, and the data; in Section 4.4, I report market-level results and evidence that parents use the information; in Section 4.5, I provide evidence of parental interactions in both reduced form and by estimating impacts on utility weights; in Section 4.7, I take a closer look at the level of interactions; and in Section 4.8, I provide some concluding remarks.

4.2 What can information interventions detect?

While there is scope to use information interventions to learn about preferences, there are limitations due to the prevalence of imperfect information, correlated quality measures, and correlated biases. This section uses a stylized framework to demonstrate that information interventions impact choices through an information, preference, and correlated quality channel. To see this explicitly, index households by $i \in \mathcal{I}$, schooling options by $j \in \mathcal{J}$, and residential neighborhoods by $k \in \mathcal{K}$. The indirect utility of household i residing in neighborhood k and enrolling their child in school j is

$$U_{ij} = \delta_{jk(i)} - \lambda d_{ij} + \varepsilon_{ij}$$

where δ_{jk} captures mean utility of school j for neighborhood k residents, d_{ij} measures the distance between household i and school j, and ε_{ij} are idiosyncratic tastes assumed to be independent of d_{ij} and δ_{jk}. Mean utility (or school popularity) is influenced by school and peer quality, Q_j^S and Q_j^P, respectively,:

$$\delta_{jk} = \alpha_k + \gamma_P Q_j^P + \gamma_S Q_j^S + e_{jk}$$

Because schools with higher quality Q_j^S tend to attract higher-achieving peers, the two quality metrics are positively correlated. For simplicity, assume that the two quality metrics are bivariate standard normal:

$$\begin{pmatrix} Q_j^P \\ Q_j^S \end{pmatrix} = \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho_Q \\ \rho_Q & 1 \end{pmatrix} \right).$$

Families are imperfectly informed about the two quality metrics and form beliefs with quality-specific proportional biases $\tilde{Q}_{ji}^P = (1 + b_{Pi})Q_j^p$ and $\tilde{Q}_{ji}^S = (1 + b_{Si})Q_j^S$. Assume that beliefs are similar within but differ between neighborhoods and are also bivariate normal:

$$\begin{pmatrix} b_{Pk} \\ b_{Sk} \end{pmatrix} = \mathcal{N}\left(\begin{pmatrix} \mu_P \\ \mu_S \end{pmatrix}, \begin{pmatrix} 1 & \rho_b \\ \rho_b & 1 \end{pmatrix} \right).$$

To address information disparities, the school district makes information available to families, so that families that receive a signal weigh the real quality metric when making decisions, while families that don't receive a signal use their beliefs. However, receiving a signal about one quality measure but not the other still induces families to update their beliefs about the unobserved quality measure. To overcome self-selection issues in who seeks the information, the school district distributes school quality signals to families living in neighborhoods $\mathcal{K}_T^S \subset \mathcal{K}$ and peer quality signals to neighborhoods $\mathcal{K}_T^P \subset \mathcal{K}$.

After randomly assigning treatments to neighborhoods $k \in \mathcal{K}_T$, school mean utilities can be expressed

$$\delta_{jk} = \alpha_k + \gamma_P Q_j^P + \gamma_S Q_j^S + \beta_P Q_j^P \times \mathbf{1}\{k \in \mathcal{K}_T^P\} + \beta_S Q_j^S \times \mathbf{1}\{k \in \mathcal{K}_T^S\} + e_{jk},$$

where β_P/β_S captures the average change in relative preferences induced by the information in the case all households are approximately perfectly informed, $\mu_P = \mu_S \approx 0$.

Some families receive one treatment, others receive both, while others don't receive anything. Families that receive at least one treatment update their beliefs accordingly. The correlations ρ_Q and ρ_b imply that changes in mean utility operating through the quality measures capture a multitude of factors. For example,

$$\begin{aligned} E[\delta_{jk}|Q_P, Q_j^S, T_P = 1, T_S = 0] &= (\gamma_P + \beta_P)Q_j^P + \gamma_S\rho_Q(1 + \mu_S - \rho_b\mu_P)Q_j^P \\ E[\delta_{jk}|Q_P, Q_j^S, T_P = 0, T_S = 0] &= \gamma_P(1 + \mu_P)Q_j^P + \gamma_S\rho_Q(1 + \mu_S)Q_j^P \end{aligned}$$

so that

$$\beta_P^* = (\underbrace{\beta_P}_{\Delta Preferences} - \underbrace{\gamma_P\mu_P}_{\Delta Information} - \underbrace{\rho_b\mu_P\rho_Q}_{\Delta\ Beliefs}).$$

The parameter $\beta_P^* = \beta_P$ when biases are not present, and either biases or quality measures are uncorrelated. In general, the observed change in relative preferences induced by information provision will also capture updates in information sets $\gamma_P\mu_P$ and a factor driven by correlated beliefs and quality, $\rho_b\mu_P\rho_Q$.

A change in preferences could arise due to households becoming aware of the quality measures of interest. Alternatively, families could update their information sets and reassess the weights they place on each quality metric. In some cases, families may not update their preferences, so $\beta_P = \beta_S = 0$, and only update their information sets. In these cases, the direction of the biases determine the sign in the change in relative preferences. If families had upward-biased beliefs of a metric, then the observed change in weights will be negative and the opposite will be true if families have downward-biased beliefs.

The treatment of spillovers is similar but omitted to avoid complicating the notation. The experiment discussed in the next section is designed to detect spillovers or parental interactions, but the spillover effects will also be a combination of preference and information updating. These factors are implicit in most information interventions.

4.3 Experimental Design

Setting and Timeline

The Zones of Choice program is one of several public choice alternatives provided by the Los Angeles Unified School District (LAUSD) in addition to charter schools. See Chapter 3 for a more detailed description of the history and expansion of the program in 2012. Before students enroll in ZOC schools, they participate in an application process during the fall semester of their eighth grade.[2] Eligible students must submit applications ranking all schools located in their neighborhood-based zone of choice. Failure to submit an application may result in assignment to an undesirable school that is not a students' neighborhood school.

In addition to application submission incentives, district and high school administrators devote a considerable amount of time and resources to inform parents about the program and their options. District administrators meet with middle schools to help facilitate application submissions. They also hold information sessions where they inform parents about the program, their options, and how to submit applications. Open houses are also hosted by high schools to help recruit students. For past cohorts, the district has experimented with sending mailers to families informing them about the program and their options.

We incorporate a survey and information provision into a typical application cycle. The three phases that summarize the experiment are: (i) baseline survey, (ii) the information intervention, (iii) deliberation, (iv) application submission. The survey is distributed before the application cycle begins to learn about parents beliefs and preferences before the intervention. Information is distributed before applications are collected and well before the deadline. The wide interval of time between information and submission allows parents to internalize the information and deliberate among themselves. After the deliberation process, parents submit applications and the intervention is complete.

The Baseline Survey

The survey serves two purposes. The first is to gain general insight about parents' awareness of the program. Although the program has existed for nearly ten years and is neighborhood-based, parents may still not be aware of their upcoming participation. Second, eliciting baseline beliefs and preferences are informative for the empirical analysis. In Section 4.2, I showed how treatment effects on utility weights will consist of a mixture of preference impacts and information updating. With measures of bias, we can correct the treatment effects to isolate the preference impact channel.

Following the advice of LAUSD administrators, we distributed the survey in paper form.[3]

[2]The application process used to take place in the Spring semester but was changed to Fall in 2018.

[3]Every year, LAUSD administers the School Experience Survey to every student and parent in the district. Low-income households tend to participate more in paper format than online, so I followed administrator's advice in only offering the paper survey.

Surveys were distributed in homeroom classes and returned to homeroom within a week of being distributed with incentives provided to both teachers and students to boost participation.[4] Section 4.9.1 displays example surveys for one zone. At the time of this writing, the surveys are awaiting to be digitized so the subsequent analysis omits any discussion about surveys.

Randomization

The Zones of Choice setting maps to the framework from the conceptual framework, where neighborhoods k correspond to *feeder* middle-schools that feed into separate markets. The school-level randomization is in the same spirit as Hastings and Weinstein (2008) and Corcoran et al. (2018), with a key distinction that we cross-randomize multiple treatments. To study spillover effects, we use a design that is similar to how Crépon et al. (2013) study displacement effects of labor market policies in France and how Andrabi et al. (2020) study equilibrium effects of private school grant provision in Pakistan. This design allows us to estimate intent-to-treat impacts of directly receiving information or indirectly receiving information through group exposure. I now describe the design and randomization procedure in more detail.

Each zone is considered a separate market and has different middle schools that feed into the zone.[5] Students from a set of schools that uniquely feed into a zone have the same effective market of schools to choose from, so each block of schools is a different experiment. Within each block, one school is assigned treatment H, another is assigned treatment L, and another is assigned as the pure-control school.[6] In this respect, there are market-specific school-level experiments with two treatments, H and L. Within each treated school, I nest a household-level experiment where I cross-randomize information on school and peer quality. The household-level randomization coupled with the school-level experiment helps identify intent-to-treat effects for households directly receiving a signal and for households indirectly receiving a signal (a spillover effect) by comparing treated households (direct and indirect) to households in the pure-control school, where no one received any information.[7]

Figure 4.1 provides a visual representation for the experiment in the Bell Zone of Choice. Elizabeth MS is randomly assigned to high saturation (treatment H) where π^h share of

[4] Amazon gift cards of varying amounts were raffled to participating teachers administrators, and iPads were raffled to students who return the survey. Student and teacher eligibility is conditional on school-level participation rates of 80 percent.

[5] Cross-zone enrollment is negligible, so each zone is effectively a separate market.

[6] Not all zones have three feeder middle schools, so I create blocks based on proximity and size of the feeder middle schools. This occurs for a total of four zones for which I create two additional blocks. Also, the number of feeder middle schools in a zone is not always divisible by three. Any residual feeder middle schools remain as pure control middle schools, therefore the control group is larger than the treatment groups by design.

[7] Feeder school enrollment is mostly neighborhood-based, so contamination of treatments within a zone to the pure-control school is not likely. Because treatment is at the school level, it mostly ensures that any neighborhood interactions occur between middle school parents with children enrolled in the same school.

households receive each treatment, Ochoa MS is assigned to low saturation, and Nimitz is the pure-control school; this is highlighted by the red arrows. Among treated schools, the two information treatments are cross-randomized with the share receiving each determined by the school-level saturation levels. This design has a total of eight treatment statuses, one for each information- and saturation-specific treatment, and each identified relative to households in the pure-control school.

4.3.1 Treatment Letters

Families with children enrolled in either high or low saturation treatment schools can potentially receive treatment letters. A challenge common in all information interventions is communicating the researcher's message in a succinct and effective manner. Because this intervention involves two treatments, effectively describing differences between peer and school quality is even more challenging. To address these challenges, focus groups with LAUSD parents were conducted along with piloting different messages on Amazon Mturk.

Through the combination of focus groups and piloting, I decided to refer to a school's value-added as school *achievement growth* and a school's peer quality is referred to as school *incoming achievement*. Section 4.9.2 reports summary statistics from the piloting. Participants were restricted to those identifying as parents between age 25 and 50 without college degrees. The college degree restriction aimed to mimic the characteristics of Zones of Choice parents. Pilot participants were able to report the accurate incoming achievement of a hypothetical school while holding achievement growth constant roughly 83 to 92 percent of the time, and reported the accurate achievement growth of a hypothetical school 91 to 94 percent of the time. During testing, participants were asked to write a one to two sentence description of the difference between the two metrics, and roughly 67 percent of participants signaled some understanding of the difference. Combined with a brief description, incoming achievement and achievement growth adequately communicated the intended message to pilot participants with similar demographic characteristics as Zones of Choice parents. The results from the pilot were mimicked during focus group discussions.

Incoming achievement metrics are constructed by first calculating past cohorts' average Reading scores for each school. Treatment letters report a school's incoming achievement percentile rank across all other high schools in LAUSD. A school's achievement growth metric comes from school value-added estimated using ordinary least squares. School achievement growth measures are sourced from value-added estimates from a model regressing students' eleventh grade test scores on lagged middle school achievement, demographic characteristics, and school by year effects. This model is similar to the one estimated and validated in Chapter 3. The within-zone correlation between the two quality measures is weakly positive 0.12. Treatment letters report a school's district wide percentile rank in the estimated school-value added distribution.

Figure 4.2 displays example treatment letters for the Bell Zone of Choice and Figure 4.11 displays treatment letters in Spanish. The design of the letters is similar to other studies (Hastings and Weinstein, 2008; Corcoran et al., 2018). There is a brief description of what the letter contains at the top of each letter, followed by a list of schools corresponding to a recipient's particular zone. A key difference in these treatment letters from past literature is the randomized order of schools in the list. The motivation for the randomization is to detect potential order biases, an issue that may affect treatment effect estimates of past studies.

4.3.2 Data

The set of students part of this study reside within Zones of Choice bondaries and attend a feeder middle school during eighth grade. There are 13,015 students meeting this requirement.[8] The starting point for the analysis is administrative data that LAUSD collects for all students in the district, including demographics, achievement records, addresses, and other outcomes. These data are linked to data collected by the Zones of Choice. For the purposes of this study, we are interested in the rank-ordered lists that parents submit.

Table 4.1 reports descriptive statistics of eighth grade students enrolled in LAUSD schools in Fall 2019. The typical ZOC student is noticeably different from the typical eighth grade student elsewhere in the district. ZOC students are entering high school performing roughly 21-25 percent of a standard deviation more poorly on Math and Reading scores than the typical non-ZOC student. Roughly 6 percent of ZOC parents have earned a four-year degree, and 97 percent of ZOC students are classified as poor. ZOC students are also more likely to be classified as English Learners. In addition to these socioeconomic differences, there are vast racial and ethnic differences. 86 percent of rising ZOC students are classified as Hispanic compared to 68 percent elsewhere in the district. The approximate racial and socioeconomic homogeneity of ZOC students was similar for past cohorts studied in Chapter 3.

4.3.3 Balance

Tables 4.2 and Table 4.3 contain balance tables for each stage. For the school-level randomization, there are 52 feeder middle schools contributing to sixteen zones. Among these middle schools, sixteen are assigned to low saturation, sixteen are assigned to high saturation, and twenty are assigned as pure control schools. As mentioned earlier, blocks of at least three feeder schools were created and treatments were randomized within each block. Table 2 demonstrates that saturation treatments are balanced across a wide array of observable school-level mean characteristics. Schools assigned to either treatment exhibit no meaningful differences in terms of their students' achievement, racial composition, socioeconomic characteristics, gender, and size. Within schools assigned to low saturation, 2633 students received some treatment, and in schools assigned to high saturation, 3780 students received at least one treatment.

Table 4.3 checks for imbalances in the within-school randomization. Column 1 reports mean characteristics for students not receiving any treatment letters within treated schools.

[8] These counts correspond to assignments made before the semester starts. In practice, many students switch schools during the summer and many leave the district.

While these students don't directly receive treatment, they could be exposed to treatment through interactions with others enrolled in their school that did receive treatment. Columns 2-4 report group differentials for different treatment groups, and Column 5 reports a p-value corresponding to the null hypothesis that the three differentials are jointly zero. Overall, we don't detect any statistically meaningful differences. Overall, we find evidence supporting the success of the randomization strategy.

4.3.4 Attrition

Although my analysis sample begins with 13,015 eligible students, not all of these students subsequently remain in the feeder middle school by the time treatment letters are distributed. I observe 12,527 of the 13,015 students (96 percent) that were part of the randomization. This could introduce challenges in the empirical analysis of there is differential attrition by treatment status. Table 4.4 reports attrition differentials for different treatment groups. The follow-up rate among students in pure control schools is 97 percent and I do not find any meaningful differentials for treated students, both direct and indirect. Therefore, there is minimal concern of differential attrition out of the sample.

4.4 School-level Experiment

The experimental design nests a student-level experiment within a school-level experiment. I leverage the school-level treatment with temporal variation to provide evidence of parents using the information. This circumvents the need to randomize at the student-level and has the advantage that we can compare cohorts different cohorts across time within treatment groups in a difference-in-differences framework. This approach implicitly controls for persistent preference heterogeneity across treatment groups and provides additional power to identify treatment effects. I estimate the following difference-in-differences model

$$Y_i = \alpha_{z(i)t(i)} + \alpha_{d(i)} + \sum_{d \in \{High, Low\}} \beta_d Post_{t(i)} \times D_i^d + u_i \tag{4.1}$$

where α_{zt} are zone-by-year effects, α_d are treatment group effects, D_i^d is an indicator for assignment to treatment group $d \in \{High, Low\}$. The coefficients β_{High} and β_{Low} capture saturation-specific treatment effects. To provide additional evidence on the success of the randomization strategy, we estimate event-study models

$$Y_i = \alpha_{z(i)t(i)} + \alpha_{d(i)} + \sum_{t \neq 2018} \sum_{d \in \{High, Low\}} \beta_{dt} Post_{t(i)} \times D_i^d + u_i \tag{4.2}$$

that visually inspects for differential pre-trends. These models treat the school as the treatment unit and do not identify spillover effects or differentiate between the different student-level treatments, but they do provide evidence on how treated cohorts in treated schools differentially responded to receiving information.

Table 4.5 reports estimates of Equation 4.1. The first two rows indicate that parents in treated markets (feeder middle schools) listed most-preferred schools with achievement growth percentiles that were between 2-6 percent higher than parents in untreated schools. Although achievement growth and incoming achievement are positively correlated, parents in treated markets listed most-preferred schools whose incoming achievement percentile was roughly 8 percent lower than most-preferred schools in untreated markets. The remaining rows show that the composition of parents' most-preferred schools changed, although most changes are small.

Figure 4.3 reports difference-in-difference estimates from Equation 4.2 on the two quality metrics for cohorts preceding the experiment and the experimental cohort. As expected, we do not find evidence of differential pre-trends between treated and untreated schools, and find quantitatively similar estimates as in Table 4.5. The evidence in Table 4.5 and Figure 4.3 are in the same vein as Corcoran et al. (2018) and Hastings and Weinstein (2008) who conduct school-level experiments to identify intent-to-treat impacts of information provision. Similar to Hastings and Weinstein (2008), these findings support the notion that households are imperfectly informed about schooling options and providing them information allows them to update both their information and preferences, subsequently affecting their choices.

The school-level experiment reports several new findings in comparison to the previous literature. This initial evidence suggests that parents in treated markets systematically chose schools with higher achievement growth rankings, even if it came at the cost of enrolling their children in schools with lower achieving peers. Therefore, in settings where parents had access to both quality metrics, there appears to be a consensus about the new best school, one with higher value-added, providing suggestive evidence that parents place more weight on value-added when making decisions. The market-level evidence provides the first piece of evidence for findings that will be supported throughout the rest of this paper, but it does not take advantage of the full experimental design.

4.5 Evidence of Parental Interactions

The experimental design highlighted parental interactions as a potential source to amplify the availability of information, or in the case of imperfect information, amplify biases. The cross-randomization of different treatments allow for differential responses to different treatments. In this section we leverage the experimental design to isolate information spillover effects and treatment-specific effects.

Within each experiment, there are eight treatment groups: six saturation-specific treatment groups (two for each treatment type) and two saturation-specific spillover groups (see Figure 4.1 for details). To that end, we estimate

$$\begin{aligned} Y_{j(i)} = \alpha_{z(i)} &+ \underbrace{\beta_{Ph} T_i^P \times D_{s(i)}^h + \beta_{Sh} T_i^S \times D_{s(i)}^h + \beta_{Bh} T_i^S \times T_i^P \times D_{s(i)}^h}_{Treated\ in\ High\ Saturation\ Schools} \\ &+ \underbrace{\beta_{P\ell} T_i^P \times D_{s(i)}^\ell + \beta_{S\ell} T_i^S \times D_{s(i)}^\ell + \beta_{B\ell} T_i^S \times T_i^P \times D_{s(i)}^\ell}_{Treated\ in\ Low\ Saturation\ Schools} \\ &+ \underbrace{\beta_h C_i \times D_{s(i)}^h + \beta_\ell C_i \times D_{s(i)}^\ell}_{Untreated\ in\ Treated\ Schools:\ Spillover\ Effects} + u_i \end{aligned} \tag{4.3}$$

where $Y_{j(i)}$ is a characteristic of parent i's top-ranked school, α_z are zone fixed effects, T_i^x are treatment x indicators for $x \in \{P, S, B\}$, D_j^y are school saturation indicators for $y \in \{High, Low\}$, and C_i is an indicator for not receiving any treatment. The model produces eight parameters, all identified by comparisons with the pure-control group school parents.

Table 4.6 reports estimates of Equation 4.3. Column 1 reports impacts on the achievement growth ranking of most-preferred schools separately for the eight treatment groups, and Column 2 is similar but for incoming achievement rank. The point estimates are qualitatively and quantitatively similar to the market-level results, but estimated with far less precision. For the most part, I do not find that effects vary by treatment type or by saturation levels, indicating market-level consensuses. I find evidence of large and salient spillovers that mimic the treatment effects among the treated, providing suggestive evidence of market-level consensuses driving the estimates across treatment groups.

While the estimates in Table 4.6 fully decompose the treatment effects, there is little evidence of heterogeneity across treatment groups. The mean impacts could potentially mask heterogeneity across the distribution, so I next estimate distributional impacts. To study distributional impacts, I estimate

$$\mathbf{1}\{Y_{j(i)} \leq y\} = \alpha_z + \beta_P T_i^P + \beta_S T_i^S + \beta_B T_i^B + \beta_C C_i \times D_{s(i)} + u_i \tag{4.4}$$

where T_i^x are treatment-specific indicators, C_i is a control group indicator interacted with $D_{s)i}$ that are school-level treatment indicators. This aggregated model pools saturation-specific treatments into omnibus treatment indicators for each treatment type. The pooled estimates are motivated by the apparent lack of differences displayed in Table 4.6, providing a boost in precision when pooled.

Figure 4.4 reports distributional impacts on achievement growth and incoming achievement percentiline ranks, respectively. Figure 4.4a shows that a uniform shift toward higher value-added schools that did not vary by the type of information families received or whether they directly received the information or not. In contrast, Figure 4.4b displays a uniform shift toward schools with lower incoming achievement ranks. The combination of these two salient and uniform patterns indicates parents systematically traded off the quality metrics, and responded more positively to value-added. For example, the estimates suggest that the probability parents ranked most-preferred schools with value-added in the bottom half of the district-wide distribution decreased by roughly 10 percentage points, and the probability that parents ranked most-preferred schools in the bottom half of the district-wide distribution increased by roughly 8 percentage points. With a weakly positive correlation between the two quality metrics, the uniform shifts are particularly striking and provide suggestive evidence of market-level consensuses guiding the decisions that parents make. Parental interactions matter in the sense that they generate the market-level consensuses leading to these patterns.

These estimates provide strong evidence of informational spillovers in educational choice settings. Spillovers are an important consideration as they facilitate the transmission of information the district provides, alleviating potential budgetary constraints restricting information campaigns. On the other hand, evidence of parental interactions also implies the amplification of certain biases in settings where information is not readily accessible to all parents, or the potential for groups with less advantaged information networks to face further disadvantage in selecting schools.

4.6 Impacts on Preferences

The reduced form evidence suggests parents systematically deviated towards schools with higher achievement growth rankings, and that was coupled with deviations toward schools with lower-achieving peers. That analysis focused on schools ranked at the top of parents' lists, but the information could have impacted other parts of the rank-ordered list as well. Another shortcoming of the reduced form approach is the inability to isolate impacts on preferences for one characteristic, while holding the other constant. This section attempts to overcome these limitations, although it admittedly can't solve other issues that arise with interpretation highlighted in Section 4.2.

I estimate a choice model leveraging the full-suite of preferences reported in parents' rank-ordered lists. The basis for the analysis starts with a model assuming parents select schools based on peer quality, school quality, and proximity. The information treatments can alter the implied weight that households place on the different school attributes. But as highlighted in Section 4.2, in settings with imperfect information, the treatment affects choices through an information channel and a preference channel. As a consequence, any treatment effects on utility weights discussed in this section will nest these two channels.

Building on the reduced-form analysis finding mostly homogeneous effects across saturation or treatment types, let there be one omnibus direct treatment group indicator T_i and an omnibus indirect treatment indicator C_i i.e., untreated parents in treated schools. The utility that household i obtains from enrolling their child in school j is

$$U_{ij} = \delta_j + \beta_P Q_j^P \times T_i + \beta_S Q_j^S \times T_i + \kappa_P \times Q_j^P \times C_i + \kappa_S \times Q_j^S \times C_i - \lambda d_{ij} + \varepsilon_{ij} \qquad (4.5)$$

so that $(\beta_P, \beta_S, \kappa_P, \kappa_S)$ are quality-specific mean utility deviations induced by treatment, λ captures distance distastes, and ε_{ij} captures unobserved preference heterogeneity. The

δ_j represent school mean utility capture all school-specific unobserved heterogeneity. An alternative representation is

$$\delta_j = \gamma_P Q_j^P + \gamma_S Q_j^S \tag{4.6}$$

where the quality measures entirely explain differences in school mean utilities. From this perspective, treatment induces changes in the marginal utility of each quality measure, but the parameterization is restrictive as there are potentially other school-specific attributes that induce meaningful variation in school mean utility.

To estimate the parameters, I each household's rank-ordered list. For each applicant, I 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

$$R_{ik} = \begin{cases} \arg\max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg\max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}. \tag{4.7}$$

Assuming $\varepsilon_{ij} \sim EVT1|(\delta_j, T_i, C_i, Q_j^P, Q_j^S, d_{ij})$, then the likelihood contribution of household i, 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

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r | U_{ir} < U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \tag{4.8}$$

I can therefore estimate the parameters via maximum likelihood.[9] Table 4.7 reports estimates for the restricted and less restrictive model. Both models produce qualitatively and quantitatively similar results. Column 1 and Column 2 report estimates for the restricted model and Columns 3 and 4 report unrestricted model estimates. Panel A pools treatments into omnibus saturation-specific treatment indicators and Panel B pools treatments into omnibus information-specific treatments, where we corroborate the reduced-form evidence.

One advantage of the restricted model is the ability to estimate relative preferences for each quality measure among parents in control group schools. As found in Chapter 3, parents seem to place higher weight on school value-added (achievement growth) than peer quality (incoming achievement) and dislike distance. This evidence contrasts that in other settings

[9]It is plausible that parents weigh their assignment chances when submitting applications because of the immediate acceptance mechanism that each zone employs. Parents may therefore strategically misreport preferences for a highly desired school with low chance of admission to ensure admission into a less-desired school. Indeed, Abdulkadiroglu et al. (2006) find evidence of strategic reporting among Boston applicants. While this may point to a preference for strategy-proof mechanisms when estimating demand from a researcher's perspective, costly search limits this advantage empirically (Arteaga et al., 2021). There remain limitations in estimating demand using data from centralized assignment systems (Agarwal and Somaini, 2019).

in the United States (e.g, Rothstein (2006); Abdulkadiroğlu et al. (2020)) but is consistent with the evidence found in Chapter 3.[10]

Column 1 of Panel A further shows that receiving any information led to decreases in parents' estimated marginal utility of school incoming achievement. In contrast, Column 2 shows that receiving any information led to increases in the estimated marginal utility of school achievement growth.[11] The impacts on preferences for achievement growth are more homogeneous across treatment groups, while the impacts on preferences for peers are more variable. Like the reduced-form evidence, I find evidence of large spillovers, mimicking that of parents that were directly treated. This again provides suggestive evidence that parents at different feeder middle schools reached consensuses that moved demand similarly across treatment groups. Scaling any of the coefficients by the estimated linear distaste for distance provides willingness to travel estimates. Families that received any treatment (direct or indirect) were willing to travel approximately 0.4 miles more to attend a school whose growth percentile rank was 10 points higher compared to families that did no receive any treatment. In contrast, families that received any treatment were willing to travel 0.37 to 2.29 miles less to enroll in a school whose incoming achievement rank was 10 points higher, all else equal.

Panel B reports qualitatively similar results, but in these estimates we group treatments across saturation levels. The estimated impacts on the two quality metrics are similar, both in levels and variability across treatment groups. The broadly similar treatment effects again suggest that parents within treated schools reached consensuses about desirable schools and updated their choices accordingly.

A peculiarity is that treatment effects on incoming achievement weights are sufficiently large to make it appear as if parents marginal utility for incoming achievement is negative. This is likely due to the fact that the estimated treatment effects are a mixture of preference and information impacts. Recall from Section 4.2 that

$$\hat{\beta}_S = \beta_S - \gamma_S \mu_S - \rho_Q \rho_b \mu_S$$

where μ_S measures mean achievement growth bias, ρ_b measures the correlation between biases, and ρ_Q measures the correlation between the two quality metrics. In our sample, $\rho_Q = 0.12$, so $\rho_Q \rho_b \mu_S \approx 0$. Therefore, if parents have downward-biased beliefs, then the estimated parameters will be upward-biased and if parents have upward-biased beliefs, then the estimated parameters will be downward-biased. With survey data digitized we will at the

[10]One potential explanation is the relative homogeneity of students within each zone which effectively eliminates selecting schools based on easily observable peer characteristics such as race or income. The lack of variation along those school attributes may require parents to differentiate schools based on other characteristics more strongly correlated with school effectiveness. While this feature of the institutional setting may facilitate competitive forces, it further entrenches the highly racially segregated nature of schools in urban school districts.

[11]Actually, each utility weight is scaled by a measure of the dispersion in the preference heterogeneity so marginal utilities are not exactly identified. Because the scaling factor is similar for each parameter, the relative impacts are identified, pointing to a similar conclusion.

minimum be able to sign the direction of the biases. Regardless, the information treatments resulted in parents updating their choices in a way that would in practice reflect a change in demand toward more effective schools.

To probe the direction and magnitude of the estimated biases, I simulate bias adjustments under a few additional stringent assumptions. With known mean biases (μ_S, μ_P), the estimated parameters produce a system of equations

$$\begin{aligned}
\hat{\gamma}_P &= (1+\mu_P)\gamma_P \\
\hat{\gamma}_S &= (1+\mu_S)\gamma_S \\
\hat{\beta}_P &= \beta_P - \gamma_P\mu_P \\
\hat{\beta}_S &= \beta_S - \gamma_S\mu_S \\
\hat{\kappa}_P &= \kappa_P - \gamma_P\mu_P \\
\hat{\kappa}_S &= \kappa_S - \gamma_S\mu_S
\end{aligned}$$

with an equal number of unknowns.[12] These restrictive assumptions allow me to simulate belief bias-adjusted treatment effects. This allows us to assess the variability of the potential impacts under these assumptions. Figure 4.6 and Figure 4.7 report belief bias adjusted treatment effect estimates where the biases are simulated. For example, Figure 4.6a reports adjusted parameter estimates where incoming achievement beliefs are very optimistic ($\mu_P = 0.5$) and I vary the achievement growth belief bias μ_S. The other panels of Figure 4.6 are similar but vary the optimism of incoming achievement beliefs which are held constant in each panel.

The takeaway from Figure 4.6 is that increasing optimism over achievement growth results in upward adjustments to the estimated achievement growth-related coefficients, and does not produce a notable impact incoming achievement-related coefficients. Interestingly, varying the optimism across panels does not seem to substantially alter the adjustments to any of the estimated coefficients. Under the stringent assumptions this exercise imposes, any adjustments to the estimates from Table 4.7 will mostly be due to achievement growth biases and not incoming achievement biases. Figure 4.7 corroborates that intuition, showing the varying incoming achievement optimism while holding achievement growth beliefs constant does not substantially alter the parameter estimates.

While these exercises impose strong assumptions on the structure of beliefs, they provide some insight into how belief biases are affecting the estimated treatment effects. For most reasonable beliefs about achievement growth, the bias-adjustments suggest positive impacts on preferences for achievement growth. Throughout all simulations, the results seem to suggest families decreased their taste for incoming achievement. These results suggest that relative preferences for achievement growth increased.

[12]Note that we can disaggregate treatment effects or aggregate them by saturation level or treatment type and have a similar set of equations. In the exercises that follow, I aggregate by saturation level.

Alternative Explanations

The interpretation of the results hinge on the treatment affecting preferences through the two measures of quality. The peculiar results related to incoming achievement may be operating through a change in tastes for some other unobserved factor negatively correlated with incoming achievement. To explore this possibility I assess what share of the variation in estimated mean utilities can be explained by the treatments operating through the two quality measures.

A regression of estimated changes in δ_j on school effects and the two quality measures interacted with treatment group dummies has an R^2 of roughly 0.8. The R^2 suggests that changes in mean utility induced by treatment through the two quality metrics captures a sizable share of the variation in the changes in school popularity. This alleviates the concern but does not rule out other potential factors explaining the changes in school popularity induced by the treatment. However, it is hard to imagine an attribute that families prefer but is negatively correlated with incoming achievement. For example, if treatment induces parents to have a stronger preference for newer facilities, it is unlikely that schools with newer facilities attract students with lower incoming achievement compared to others in the same zone of choice.

Next, the interpretation of spillover effects implies that changes in mean utility should be similar across treatment arms. In other words, a regression of changes in school mean utilities for directly treated individuals on changes in school mean utilities for indirectly treated individuals should have a coefficient equal to one. Figure 4.18 shows this is the case.

While these checks provide suggestive evidence supporting the interpretation of preference and spillover impacts, they fall short of confirming them. However, it remains difficult to find alternative school attributes that could rationalize the observed patterns.

4.7 Interactions at schools or in the neighborhood?

The spillover effects reported so far are agnostic about the level of parental interactions. Some treated households may not have nearby neighbors participating in the application cycle, limiting their potential interactions to school sites. Other treated households may have the ability to discuss the information at schools and back at home because they have neighbors nearby. Among untreated households, disentangling between neighborhood-based and school-based interactions provides more insight about how to maximize the dissemination of information in settings where there are constraints on distribution. This section leverages neighborhood heterogeneity in exposure to assess the role of neighborhood-level interactions.

Figure 4.8 displays census tract counts of treated households with overlaid middle school attendance zone boundaries. The neighborhood-level variation can be seen by the differences between census tract intensities within attendance zone boundaries, where the attendance zone boundary reflects feeder middle schools.[13]

[13]The analysis leverages a more granular level of neighborhood exposure, Census block, but due to many

As a first pass and to visualize neighborhood-specific effects, Figure 4.9 reports mean most-preferred characteristics for different treatment groups by the number of households treated in their neighborhood. Panel A displays a positive gradient in terms of households most-preferred school's achievement growth and their neighborhood exposure. Importantly, there is not a gradient among parents with students enrolled in pure control schools, as would be expected.[14] Panel B is an analogous figure for incoming achievement. The negative mean shift suggests that parents in isolated neighborhoods were more likely to deviate to schools with lower incoming achievement, but parents in less-isolated neighborhoods were less likely to have different preferences for incoming achievement compared to parents in pure control schools. Figure 4.14 displays the distribution of exposure across treatment groups. Most households have minimal exposure to other LAUSD households with children in feeder middle schools; nearly 40 percent have 2 or fewer neighbors that are eligible to receive information.

The evidence in Figure 4.9 is suggestive and evidently noisy. I now test for neighborhood exposure heterogeneity in treatment effects. Let T_i be an omnibus treatment indicator, S_i be an indicator for untreated households enrolled in treated schools, and let O_i correspond to the number LAUSD households in i's neighborhood, excluding i. I then estimate

$$Y_i = \alpha_{z(i)} + \beta_T T_i + \beta_S S_i + \pi_T T_i \times O_i + \pi_S S_i \times O_i + \eta O_i + u_i \tag{4.9}$$

where β_T, β_S capture treatment effects for spillover and treated groups that were not exposed to treated neighbors, and π_T, π_S capture differential effects increasing with the number of treated neighbors. As previous reduced-form analyses, standard errors are clustered at the feeder school level.

Table 4.8 reports treatment effects that vary with neighborhood exposure. Column 1 and Column 3 report the baseline model assuming no heterogeneity, and Column 2 and Column 4 report estimated heterogeneous treatment effects. The estimates mirror Figure 4.9 in that we find positive gradients for both quality metrics, and evidence of a negative mean shift in most-preferred schools' incoming achievement.

Table 4.8 suggests there are multiple layers of interactions driving the results. Parents can have cursory discussions about treatment letters at schools, and can have potentially more detailed discussions among neighbors near home. Taken at face value, the evidence suggests that parents less exposed to more detailed discussions about the treatment letters responded by shifting demand toward schools with higher value-added and schools with lower incoming achievement. For parents more exposed to other parents of similarly aged students enrolled in similar schools, the evidence suggests that they shifted demand toward schools with higher value-added but were more careful about the tradeoff between achievement growth and incoming achievement. For either, there appeared to be consensus in preferring

empty cells the visualization is more appealing when aggregated to Census tract.

[14]The flat gradient among parents in pure control schools also provides evidence against information leaking between treated and untreated schools. Figure 4.8 displays some treated households residing in neighborhoods within untreated school attendance zone boundaries, potentially contaminating the information sets of parents in pure control schools. Figure 4.9 suggests that is not a serious concern.

schools with higher achievement growth ranking instead of schools with higher incoming achievement ranking.

The evidence reported in this section suggests that parental interactions matter in the schooling decision process, but multiple layers of interaction matter differently. While the private signals of others are important determinants of parents' decisions, parents have different capacities to internalize those signals. The dissemination of information a school district provides is best internalized when interactions are substantial, pointing to the importance of interactions with neighbors. In addition, interactions with neighbors can facilitate understanding difficult information, such as understanding the difference between a school's performance level and a school's performance growth. Overall, these results suggest that parents more exposed to other parents had stronger value-added taste impacts, suggesting the importance of more nuanced discussions with neighbors.

While spillover effects highlight the importance of social networks for school choice, they could also facilitate the exacerbating inequality in exercising choice. If families resort to their social network for information, then being connected to a less informed network could generate schooling decisions that only benefit those with more informed networks (Schneider et al., 1997). If information availability is correlated with socioeconomic status, then successful school choice reforms without adequate information campaigns targeting the least-informed networks are unlikely.

4.8 Conclusion

Parents often have to decide between multiple schooling options and are imperfectly informed about school and peer quality, school attributes they may or may not care about. At the same time, the weights that parents place on school and peer quality when making decisions affects school incentives, making preferences an important determinant of successful education reforms. In addition to considering different school attributes, parents may also weigh the information and opinions of other parents, suggesting a potential role for parental interactions. This paper uses an information provision experiment to address information imperfections, to assess the empirical relevance of parental interactions, and to study relative preferences for peer and school quality.

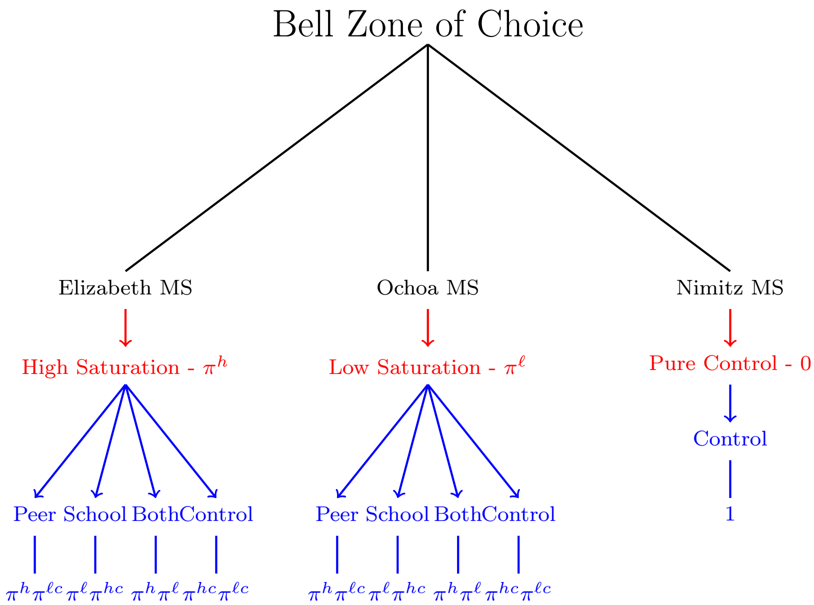
The evidence reported throughout this paper shows families are imperfectly informed about different quality measures that are important determinants of later life outcomes. The information provision addressed these imperfections but information provision affects choices through parents' updated information sets and changed preferences.

To disentangle between the two, I provide two sets of results that point to the same conclusion. First, I estimate reduced form impacts on the attributes of parents' most-preferred schools. This shows that treated parents most-preferred schools had higher value-added and lower achieving peers than control group parents. I then estimate a choice model that allows me to estimate impacts on utility weights. Under a few additional assumptions, I can account for biases and recover a range of preference impacts. I find that beliefs on value-added play a more important role than incoming achievement beliefs in producing biases in the estimated utility weight impacts. With additional survey data containing parents' pre-intervention beliefs, I can calculate the implied preference impacts, but for most plausible biases, I find positive impacts on achievement growth preferences and negative impacts on incoming achievement preferences. This suggests that the information campaign led to an increase in parents' relative preference for value-added.

I also demonstrate the importance of social networks for schooling decisions. Parents who did not directly receive any treatment but were exposed to parents that received treatment changed their behavior similarly. Some evidence suggests interactions occurring both at school sites and among neighbors with similarly aged children. The importance of social networks can amplify inequalities in who benefits from school choice reforms, with those belonging to less informed networks benefiting less.

The evidence in this paper points to potential benefits from distributing information on both schools' performance and growth metrics. The precise manner in which parental interactions translate into updated preferences, the capacity for other information interventions to boost competitive incentives, more progress on disentangling preference impacts from information impacts in information provision experiments, and how social networks can amplify disparities in school choice settings are important avenues for future research.

Figure 4.1: Assignment to treatment



Notes: This figure describes the randomization if the block size was three. There are certain zones with more than 3 feeder schools but less than six, so the block sizes were either three or four schools. π_h is the saturation level of high saturation schools, and π^ℓ is the saturation level for low-saturation schools. π^{hc} and $\pi^{\ell c}$ are 1 minus the π^h and π^ℓ, respectively.

Figure 4.2: Treatment Letter Example: Bell Zone of Choice

Bell Zone of Choice

To ensure you have the best information available prior to your upcoming decision, we are providing information about schools within the Bell Zone of Choice. We determine the quality of a school based on students' average scores on state exams. One determinant measures a school's ability to attract high-achieving students by measuring the average test scores of its incoming students (**incoming achievement**). The table below provides performance levels for each school in your zone.

School	Incoming Achievement*	Campus Location	Type of School
Science, Technology, Engineering, Arts & Math (STEAM)	65	Legacy	Small School
Visual & Performing Arts (VAPA)	58	Legacy	Small School
Health Academy	37	Elizabeth	Small Learning Community
Multilingual Teacher Career Academy (MTCA)	47	Bell	Linked Learning Community
Information Technology Academy	33	Elizabeth	Small Learning Community
Arts Language & Performance Humanities Academy (ALPHA)	47	Bell	Linked Learning Community
9th Grade Academy	16	Maywood	Small Learning Community
Bell Global Studies	47	Bell	Comprehensive School

*Schools' Incoming Achievement are provided in percentiles. For example, if a school has a incoming achievement of 55, this means that the average test scores of its incoming students are better than 55 percent of other high schools in LAUSD.

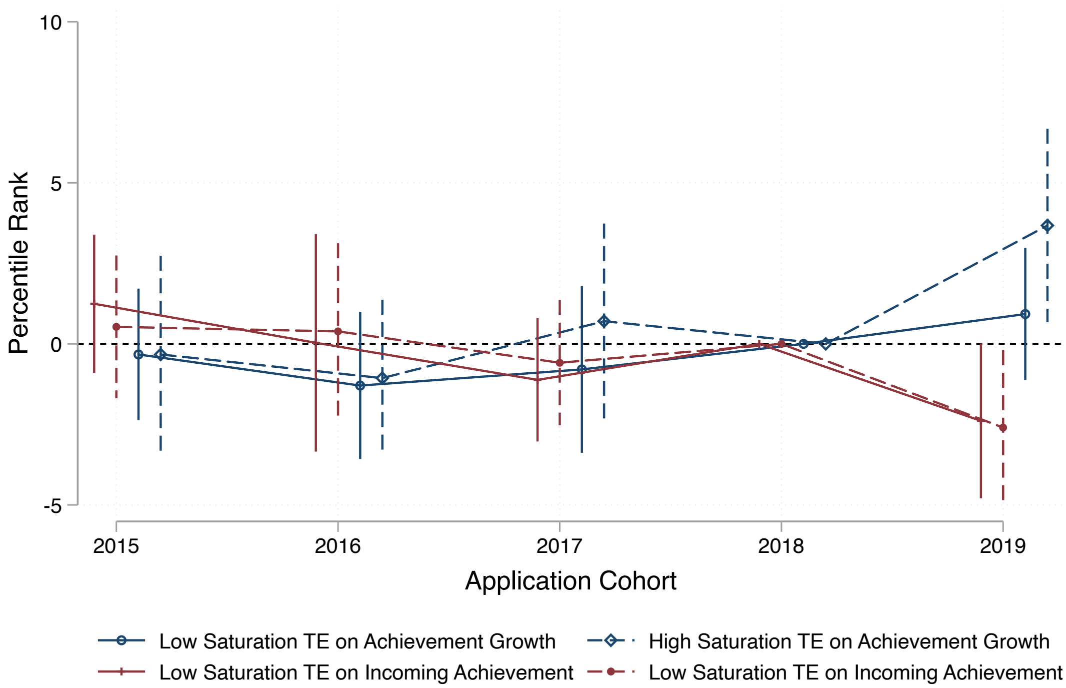
Bell Zone of Choice

To ensure you have the best information available prior to your upcoming decision, we are providing information about schools with-in the Bell Zone of Choice. We determine the quality of a school based on students' average scores on state exams. One determinant measures a school's ability to improve test scores by measuring the average growth of student's test scores between entry into the school and some later date (**achievement growth**). The table below provides performance levels for each school in your zone.

School	Achievement Growth*	Campus Location	Type of School
Science, Technology, Engineering, Arts & Math (STEAM)	78	Legacy	Small School
Visual & Performing Arts (VAPA)	43	Legacy	Small School
Health Academy	49	Elizabeth	Small Learning Community
Multilingual Teacher Career Academy (MTCA)	61	Bell	Linked Learning Community
Information Technology Academy	47	Elizabeth	Small Learning Community
Arts Language & Performance Humanities Academy (ALPHA)	61	Bell	Linked Learning Community
9th Grade Academy	24	Maywood	Small Learning Community
Bell Global Studies	61	Bell	Comprehensive School

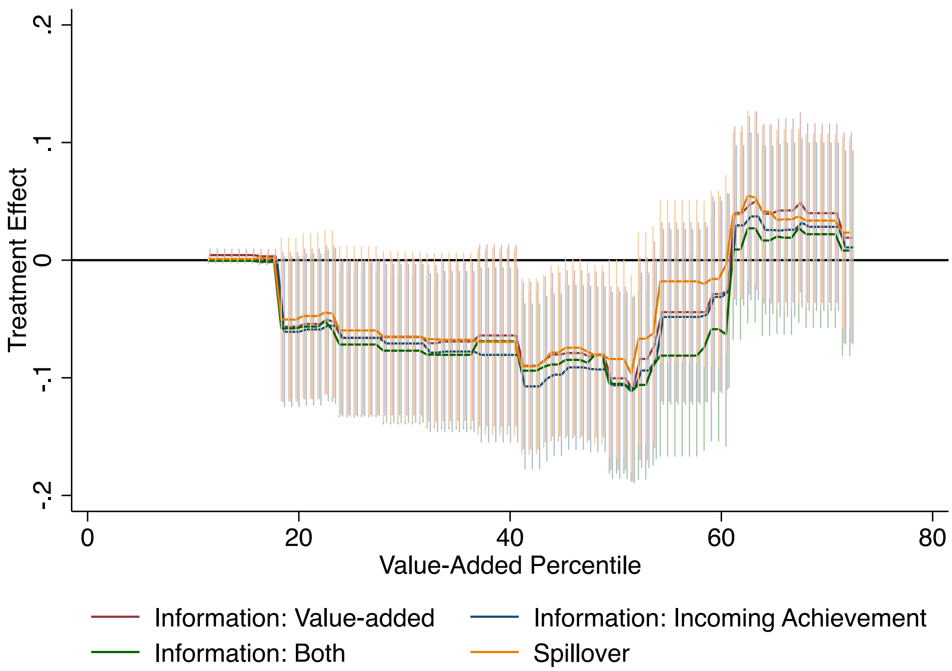
*Schools' Achievement Growth are provided in percentiles. For example, if achievement growth is 75, then the school's ability to improve test scores is better than 75 percent of high schools in LAUSD.

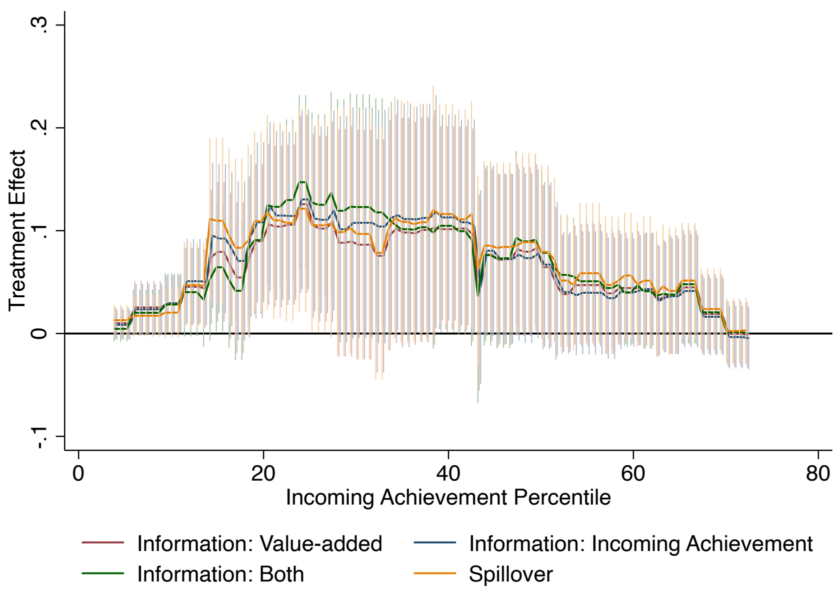
Figure 4.3: Difference-in-difference estimates for multiple cohorts



Notes: This table reports difference-in-difference estimates relative to the year before the field of experiment. The estimates come from a student-level regression of most-preferred incoming achievement and achievememt growth on high saturation and low saturation treatment indicators. Treatments are at the feeder school level allowing us to identify placebo treatment groups without the need to do a placebo student-level randomization. Application cohorts 2015-2018 are placebo cohorts and 2019 is the treated cohort. Standard errors are clustered at the feeder middle school level. Standard errors are clustered at the feeder middle school level.

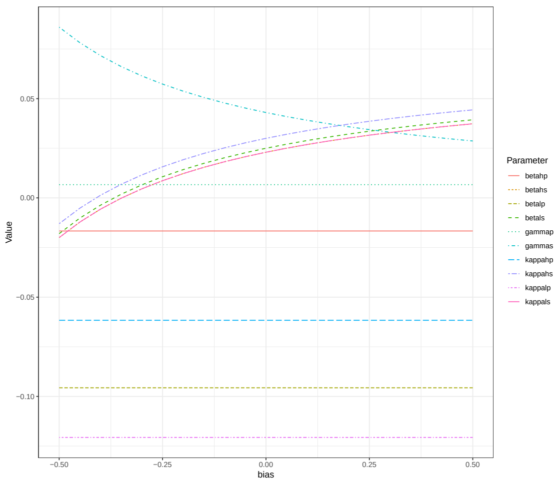
Figure 4.4: Distributional Analysis by Treatment Type



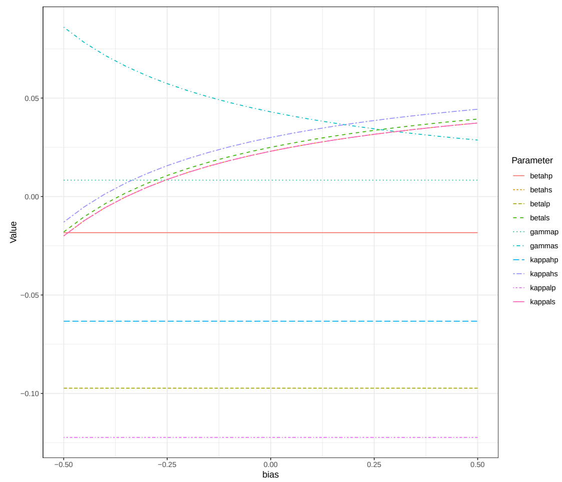


Notes: These figures report treatment effects estimated from Equation 4.4. Panel a corresponds to estimates where the outcome variable is the value-added percentile rank of students' most-preferred school, while Panel b is similar but for incoming achievement. Treatments are aggregated to treatment-type for boost in precision. Standard errors are clustered at the feeder school in both panels.

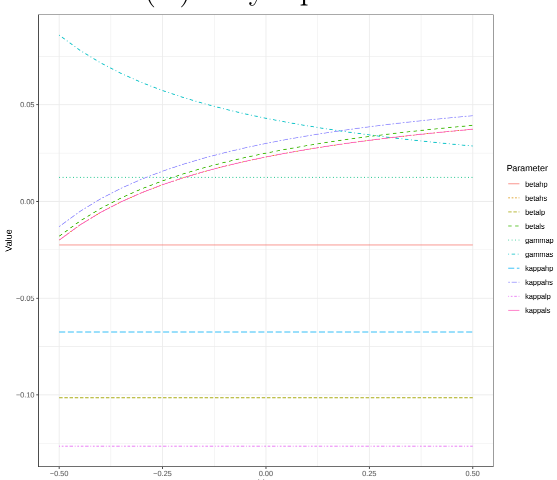
Figure 4.6: Simulated Belief Bias Adjusted Parameters Holding Incoming Achievement Beliefs Constant



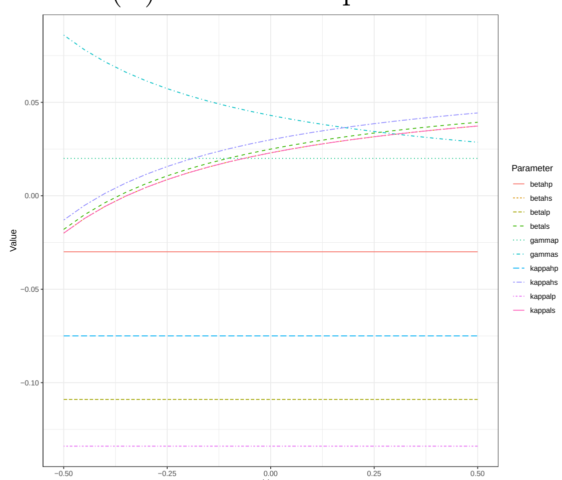
(A) Very Optimistic



(B) Somewhat Optimistic



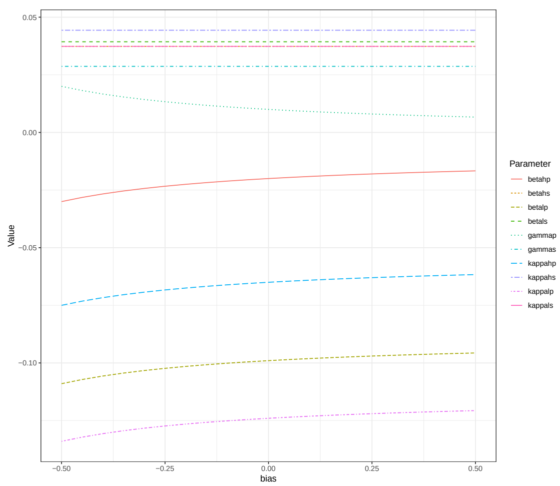
(C) Somewhat Pessimistic



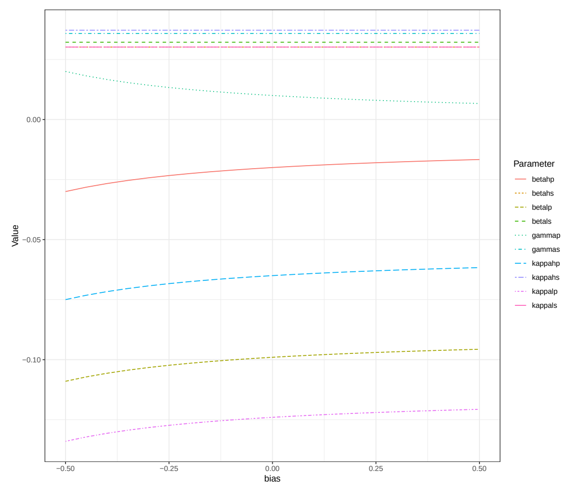
(D) Very Pessimistic

Notes: This figure reports simulated belief-bias adjusted parameter estimates resulting from solving the system of equations outlined in Section 4.6. Panel A holds incoming achievement mean bias constant at $\mu_P = 0.5$ and varies achievement growth mean bias on the horizontal axis between $\mu_S \in [-0.5, 0.5]$. Each line traces the value of the estimated parameter as we vary μ_S. Panel A replicates this excercise but changes $\mu_P = 0.2$, Panel C changes to $\mu_P = -0.2$, and Panel D changes to $\mu_P = -0.5$. Throughout we assume that $\rho_Q \rho_b \mu_P$ is sufficiently small that we can ignore, noting that $\rho_Q = 0.12$ and thus suggesting the assumption is somewhat plausible.

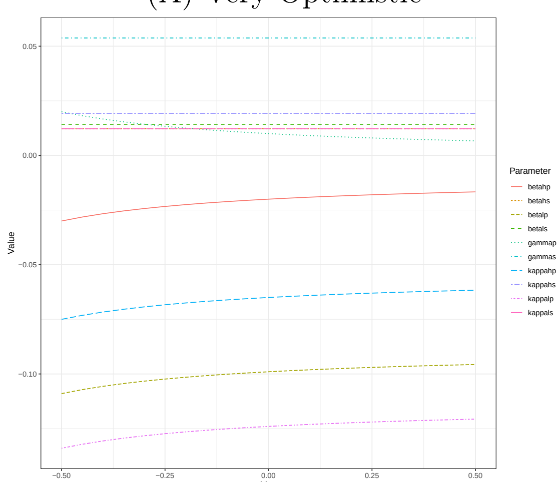
Figure 4.7: Simulated Belief Bias Adjusted Parameter Estimates Holding Achievement Growth Beliefs Constant



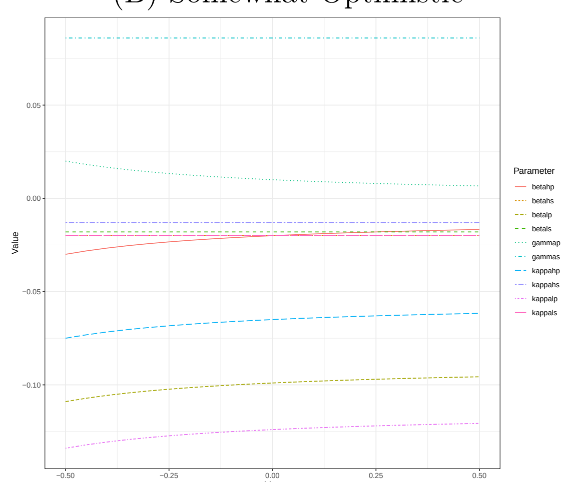
(A) Very Optimistic



(B) Somewhat Optimistic



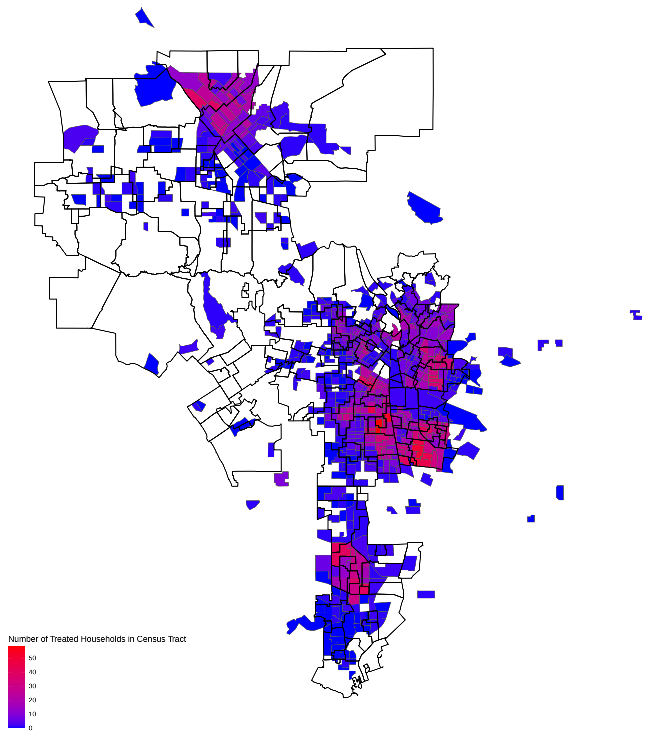
(C) Somewhat Pessimistic



(D) Very Pessimistic

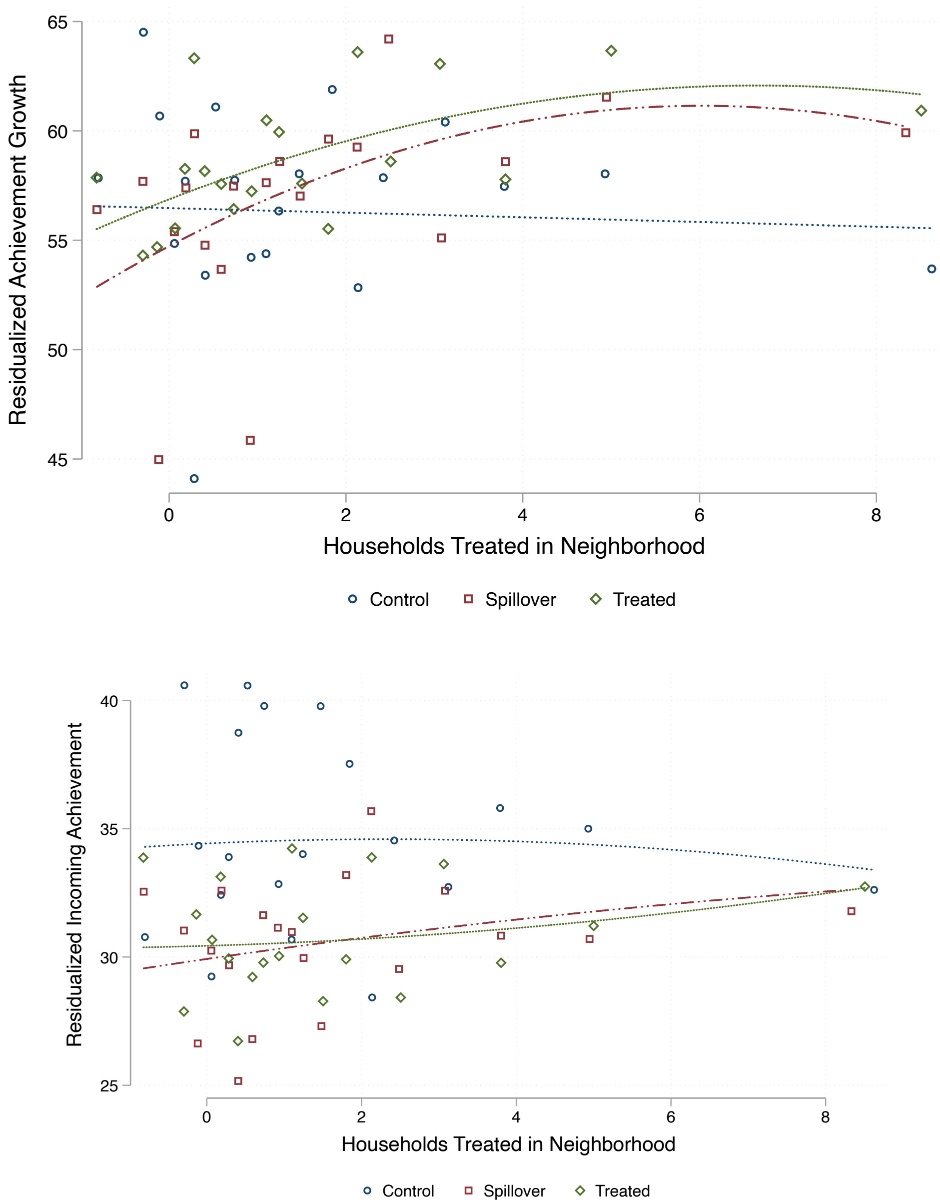
Notes: This figure reports simulated belief-bias adjusted parameter estimates resulting from solving the system of equations outlined in Section 4.6. Panel A holds achievement growth mean bias constant at $\mu_S = 0.5$ and varies incoming achievement mean bias on the horizontal axis between $\mu_P \in [-0.5, 0.5]$. Each line traces the value of the estimated parameter as we vary μ_P. Panel A replicates this excercise but changes $\mu_S = 0.2$, Panel C changes to $\mu_S = -0.2$, and Panel D changes to $\mu_S = -0.5$. Throughout we assume that $\rho_Q \rho_b \mu_S$ is sufficiently small that we can ignore, noting that $\rho_Q = 0.12$ and thus suggesting the assumption is somewhat plausible.

Figure 4.8: Number of Treated Households by Census Tract



Notes: This figure displays a map of the portion of Los Angeles County that LAUSD covers. Black bordered empty polygons correspond to middle school attendance boundaries. The filled in smaller polygons are Census tracts with at least one treated parent residing in that tract.

Figure 4.9: Heterogeneity by Neighborhood-level Exposure



Notes: This figure displays mean most-preferred school attributes among parents residing in neighborhoods with varying number of treated parents nearby. A quadratic fit is overlaid the bin means. Panel A corresponds to value-added percentile ranks and Panel B corresponds to incoming achievement percentile ranks.

Table 4.1: ZOC and non-ZOC Differences

	Non-ZOC	ZOC	Difference
	(1)	(2)	(3)
Reading Scores	.135	-.117	-.252
			(.081)
Math Scores	.099	-.114	-.213
			(.081)
College	.1	.065	-.036
			(.017)
Migrant	.036	.054	.018
			(.007)
Female	.513	.481	-.032
			(.016)
Poverty	.909	.967	.058
			(.024)
Special Education	.148	.141	-.007
			(.022)
English Learners	.076	.134	.058
			(.017)
Black	.107	.03	-.077
			(.027)
Hispanic	.683	.862	.179
			(.075)
White	.038	.015	-.024
			(.009)
N	26517	13015	

Notes. This table consists of the 2019-2020 cohort of eighth grade students in LAUSD observed in sixth grade. Column (1) contains sample means for non-ZOC students, column (2) contains sample means for ZOC students, and column (3) contains the difference with a robust standard error in parentheses underneath. College is an indicator equal to one if parents self-reported being college graduates. Migrant is an indicator equal to one if a student's birth country is not the United States. Poverty is an indicator equal to one if LAUSD flags the student as living in poverty. ELA and Math test scores are normalized within grade and year.

Table 4.2: Saturation Level Balance

	Control (1)	Low - Control (2)	High - Control (3)
ELA	-.116	.021	.028
		(.102)	(.103)
Math	-.109	-.005	.029
		(.1)	(.116)
College	.081	.006	-.005
		(.022)	(.024)
Migrants	.063	-.009	-.005
		(.008)	(.008)
Female	.486	0	.015
		(.014)	(.01)
Poverty	.947	.011	.005
		(.026)	(.027)
Special Education	.126	.016	.008
		(.011)	(.009)
English Learner	.121	.005	.022
		(.015)	(.02)
Black	.04	-.009	-.011
		(.015)	(.014)
Hispanic	.846	.008	-.014
		(.037)	(.024)
White	.017	0	-.002
		(.007)	(.008)
Size of Cohort	239.639	16.212	18.399
		(44.856)	(42.92)
Number of Schools	20	16	16
Number Treated Students	0	2633	3780

Notes. This table consists of the 2019-2020 cohort of eighth grade students in LAUSD observed in sixth grade. Column (1) contains sample means for non-ZOC students, column (2) contains sample means for ZOC students, and column (3) contains the difference with a robust standard error in parentheses underneath. College is an indicator equal to one if parents self-reported being college graduates. Migrant is an indicator equal to one if a student's birth country is not the United States. Poverty is an indicator equal to one if LAUSD flags the student as living in poverty. ELA and Math test scores are normalized within grade and year.

Table 4.3: Within-school balance

	Control (1)	Peer - Control (2)	School - Control (3)	Both - Control (4)	P-value (5)
ELA Scores	-.101	.016	-.05	0	.144
		(.039)	(.021)	(.038)	
Math Scores	-.114	.027	-.004	-.025	.794
		(.031)	(.024)	(.037)	
Parents College	.065	.002	-.005	0	.856
		(.011)	(.008)	(.014)	
Migrant	.047	.01	0	.004	.156
		(.007)	(.008)	(.01)	
Female	.477	.001	.003	-.002	.998
		(.017)	(.018)	(.025)	
Poverty	.968	.006	.003	-.01	.263
		(.004)	(.006)	(.006)	
Special Education	.135	.007	.018	-.012	.35
		(.011)	(.01)	(.013)	
English Learners	.128	.007	.009	.001	.5
		(.01)	(.009)	(.013)	
Black	.024	.006	.002	-.007	.646
		(.005)	(.005)	(.007)	
Hispanic	.864	-.012	.007	.003	.121
		(.009)	(.011)	(.014)	
White	.014	.001	.001	-.002	.949
		(.004)	(.004)	(.005)	
Joint Test P-value		.757	.607	.905	
N	1836	1906	1906	2641	

Notes: Column (1) reports within-school control group means and columns (2)-(4) contain mean differences between treated and control group individuals. Column (5) reports the p-value of a joint test of equality of means across groups for that given row. The p-value on a test of treatment-control comparisons for all characteristics. Note that the population in this table is those assigned to non-pure control schools. Standard errors are clustered at the school level for all tests.

Table 4.4: Attrition Differentials

	Follow-up Differentials			
Follow-up Rate	Treated High	Treated Low	Control High	Control Low
0.97	-0.015	-0.012	-0.012	-0.009
	(0.008)	(0.012)	(0.016)	(0.010)
P-value: 0.357				
Follow-up Total: 12527				

Notes: This table reports coefficient estimates from a regression of a follow-up indicator on treatment indicators. The first column reports the follow-up rate for pure control students. The subsequent columns report follow-up differentials for each treatment group. Standard errors are clustered at the feeder middle school level.

Table 4.5: Difference-in-difference estimates on most-preferred school characteristics

	(1) Pure Control Mean	(2) High Saturation	(3) Low Saturation
Achievement Growth Percentile	66.205	3.850***	1.535***
		(0.518)	(0.300)
Incoming Achievement Percentile	33.589	-2.688***	-2.436***
		(0.332)	(0.262)
Special Education Share	0.059	0.002***	0.007***
		(0.000)	(0.001)
Female Share	0.470	-0.003***	-0.007***
		(0.001)	(0.001)
Poverty Share	0.896	0.022***	0.015***
		(0.002)	(0.001)
Migrant Share	0.180	0.011***	0.003***
		(0.002)	(0.001)
Black Share	0.040	-0.016***	-0.019***
		(0.002)	(0.002)
Hispanic Share	0.905	0.009***	0.014***
		(0.003)	(0.003)
White Share	0.016	0.000	-0.002***
		(0.000)	(0.000)
College Share	0.020	-0.004***	-0.003***
		(0.001)	(0.001)
N		56521	

Notes: This table reports difference-in-difference estimates from a student-level regression of the row variable on high saturation and low saturation treatment indicators. Treatments are at the feeder school level allowing us to identify placebo treatment groups without the need to do a placebo student-level randomization. Standard errors are clustered at the feeder middle school level.

Table 4.6: Saturation- and treatment-specific effects

	(1) Achievement Growth	(2) Incoming Achievement
High Saturation Treatment		
Peer Quality	3.966	-5.222**
	(3.259)	(2.462)
School Quality	3.117	-5.317**
	(3.164)	(2.373)
Both	3.123	-4.99**
	(3.217)	(2.396)
Low Saturation Treatment		
Peer Quality	1.885	-5.294*
	(2.803)	(2.821)
School Quality	.495	-4.719*
	(2.997)	(2.806)
Both	3.376	-5.213*
	(2.805)	(2.807)
Spillover Treatment		
High Saturation	2.322	-5.867**
	(2.843)	(2.444)
Low Saturation	1.519	-5.267*
	(2.814)	(2.839)
Pure Control Mean	65.739	45.749
R2	0.240	0.400
N	11541	11541

Notes: This table reports coefficient estimates from Equation 4.3. There are a total of eight parameters, 6 saturation-specific direct treatments and 2 saturation-specific indirect treatments, all identified relative to the pure-control group. Standard errors are clustered at the feeder middle school level.

Table 4.7: Rank-ordered logit estimates

	Model 1		Model 2	
	(1) Achievement	(2) VA	(3) Achievement	(4) VA
	Panel A: Saturation Type			
Treatment				
Untreated	0.010	0.043***		
	(0.009)	(0.005)		
Information: High	-0.020	0.023***	0.027**	0.032***
	(0.013)	(0.007)	(0.013)	(0.007)
Information: Low	-0.099***	0.025***	-0.065***	0.029***
	(0.013)	(0.007)	(0.015)	(0.008)
Spillover: High	-0.065*	0.030*	-0.028	0.040**
	(0.034)	(0.016)	(0.033)	(0.017)
Spillover: Low	-0.124***	0.023**	-0.092***	0.032***
	(0.016)	(0.009)	(0.018)	(0.010)
Distance	-0.054***		-0.043***	
	(0.009)		(0.009)	
	Panel B: Information Type			
Treatment				
Untreated	0.010	0.043***		
	(0.009)	(0.005)		
Information: Achievement	-0.081***	0.032***	-0.046***	0.039***
	(0.016)	(0.008)	(0.016)	(0.009)
Information: VA	-0.062***	0.020**	-0.019	0.024***
	(0.015)	(0.008)	(0.016)	(0.009)
Spillover: Both	0.114***	-0.031***	0.081***	-0.034***
	(0.023)	(0.012)	(0.023)	(0.013)
Spillover	-0.114***	0.024***	-0.079***	0.034***
	(0.015)	(0.008)	(0.016)	(0.009)
Distance	-0.054***		-0.044***	
	(0.009)		(0.009)	
Number of Choices	75922			

Notes: This table reports estimates from Equation 4.5 and Equation 4.6. Panel A pools treatments into saturation-specific treatments and Panel B pools treatments into information-specific treatments. Column 1 and 2 report coefficient estimates from Equation 4.6, while Column 3 and 4 report coefficient estimates from Equation 4.5. Each column reports coefficients corresponding to the labeled attribute and their interactions with the row variable. Standard errors are double clustered at the feeder school and neighborhood level.

Table 4.8: Neighborhood Exposure Heterogeneity

	(1) Incoming Achievement	(2) Incoming Achievement	(3) Growth	(4) Growth
Treated	-3.947*	-5.130**	2.863	1.934
	(2.051)	(2.071)	(3.119)	(3.384)
Treated × Block Exposure		0.261***		0.204*
		(0.0822)		(0.119)
Spillover	-4.117*	-6.227***	1.231	-0.545
	(2.274)	(2.206)	(3.593)	(4.083)
Spillover × Block Exposure		0.478***		0.403*
		(0.105)		(0.214)
Observations	11,041	11,041	11,041	11,041
R-squared	0.282	0.284	0.150	0.150

4.9 Additional Results

4.9.1 Survey Examples

2019-2020 Zones of Choice Participation Survey

Congratulations! You live in the North Valley Zone of Choice which means you have many options when choosing your student's high school. Part of the application process this year involves you answering a few questions that will inform Zones of Choice administrators moving forward. The purpose of this survey is to learn about what factors matter most to you when picking schools. Accurately answering these questions is vital for the success of this year's application process and that of future years to come. The survey takes less than 10 minutes to complete. Thank you for participating.

StudentName LAUSDID

Section A - The following questions are useful to help the district better communicate the program to families.

1. What is your relationship to the student?
   Father ☐ Mother ☐ Grandparent ☐ Legal Guardian ☐
2. Do you have any other children attending Zones of Choice schools?
   Yes ☐ No ☐
3. Has anyone mentioned Zones of Choice to you before?
   Yes ☐ No ☐
4. Has your child ever attended a charter school within LAUSD?
   Yes ☐ No ☐
5. Do you plan on enrolling your child in a Zone of Choice school next year?
   Yes ☐ No ☐

Section B - The following questions are to assess your planned participation in the application cycle and for us to learn what to emphasize in future years.

6. How many hours do you anticipate you will spend researching schools?
   - Less than 2 hours ☐
   - 2-5 hours ☐
   - 6-10 hours ☐
   - 11-15 hours ☐
   - More than 15 hours ☐
7. Do you anticipate doing any of the following (check all that apply):
   - Visit School Fair ☐
   - Watch school promotional videos ☐
   - Online research ☐
   - Talk to teachers ☐
   - Talk to other parents ☐
   - Consider you student's input ☐
8. Rank the following school characteristics in terms of importance (1-7), where 1 is most important.

Improve test scores	___________	Teachers	___________
Performance of other students	___________	Distance	___________
College enrollment success	___________	Sports	___________
Safety	___________		

9. How important are a school's students when choosing a school?
   Not important ☐ Somewhat important ☐ Important ☐ Very Important
10. How important are a school's test scores when choosing a school?
   Not important ☐ Somewhat important ☐ Important ☐ Very Important
11. Do you think schools that attract the highest performing students are also the most effective facilitating test scores?
   Yes ☐ No ☐

Section C- This is the most important section of the survey. We are going to ask you questions about your preferences and beliefs about two important characteristics of schools. First, we provide you with a brief background on the two characteristics.

We determine the quality of a school based on students' average scores on state exams This measure has two parts you should consider, one which measures the school's ability of attracting high scoring students, and the second is the school's impact on test score growth. For example, one school may have high average test scores because they attract high-achieving students who do well regardless of the school they attend, but not improve the test scores of their students by much. Another school may perform well because they significantly improve the test scores of students who would have otherwise performed poorly.

We can measure a school's ability to attract high-achieving students by measuring the average test scores of its incoming students (**incoming achievement**). Similarly, we can measure the school's ability to improve test scores using the growth of the same student's test scores between entry into the school and some later date (**achievement growth**). Therefore, a school's observed quality is a combination of both their students' **incoming achievement** and the **achievement growth** they obtain while at the school. Some parents may prefer schools with high incoming achievement, and others may prefer schools with high achievement growth.

12. With the discussion above in mind, to the best of your ability, report your belief about each school's ability in drawing in high achieving students (**incoming achievement**) and the school's ability improving test scores (**achievement growth**). Please provide a score ranging 1-100, where 100 would mean you believe the school is the best in the district and 1 would mean it is the worst in the district.

School Name	**Achievement Growth**	**Incoming Achievement**
Academy of College and Career Readiness (ACCR)		
Academy of Scientific Exploration		
Humanitas Futures Academy		
Social Justice Humanitas Academy		
Technology Preparatory Academy		

13. With the discussion above in mind, please rank the schools as if you were submitting the application today. Note there are 5 schools you can choose from, so rank your most preferred as 1 and the least preferred as 5.

School Name	**Campus**	**Rank**
Academy of College and Career Readiness (ACCR)	Sylmar	
Academy of Scientific Exploration	Chavez	
Humanitas Futures Academy	San Fernando	
Social Justice Humanitas Academy	Chavez	
Technology Preparatory Academy	Chavez	

Thank you for your participation. We look forward to the success of your student!

___________________________ _______________________________

Legal Guardian Signature Date

Encuesta de Participación de Zonas de Opción 2019-2020

¡Felicidades! Usted vive en la Zona de Opción de North Valley, lo que significa que tiene la oportunidad de inscribir a su hijo en cualquier escuela dentro de su zona de opción. Parte del proceso de solicitud de este año incluye que responda algunas preguntas que informarán a los administradores de Zonas de Opción. El propósito de esta encuesta es aprender qué factores son los más importantes para usted cuando elige escuelas. Responder con precisión a estas preguntas es vital para el éxito del proceso de solicitud de este año y el de los próximos años. La encuesta no toma mucho tiempo. Le agradecemos por su participación.

StudentName LAUSDID

Sección A – Las preguntas que siguen son para ayudar al distrito escolar mejorar en su comunicación del programa.

1. ¿Cuál es su relación con el estudiante?
   Padre ☐ Madre ☐ Abuelo/Abuela ☐ Guardián legal ☐

2. ¿Tiene otros niños en las escuelas de Zonas de Opción?
   Si ☐ No ☐

3. ¿Alguien te ha mencionado Zonas de Opción antes?
   Si ☐ No ☐

4. ¿Ha asistido su hijo alguna vez a una escuela charter en el LAUSD?
   Si ☐ No ☐

5. ¿Planea inscribir a su hijo en una escuela de Zonas de Opción el próximo año?
   Si ☐ No ☐

Sección B - Las siguientes preguntas son para evaluar su participación planificada en el ciclo de solicitud y para que aprendamos qué enfatizar en años futuros.

6. ¿Cuanto tiempo anticipa investigar sobre las escuelas que puede elegir?
   - Menos de 2 horas ☐
   - 2-10 horas ☐
   - 6-10 horas ☐
   - 11-15 horas ☐
   - Mas de 15 horas ☐

7. Anticipa hacer cualquier de lo de siguiente (seleccione todos que apliquen):
   - Visitar ferias de escuela ☐
   - Mirar videos ☐
   - Investigacion en la red ☐
   - Hablar con profesores/profesoras ☐
   - Hablar con otros padres ☐
   - Hablar con tu estudiante ☐

8. Clasifique las siguientes características en orden de importancia (1-7), en donde 1 es el mas importante.

Mejorar resultados de exámenes	___________	Maestros/Maestras	___________
Puntajes de otros estudiantes	___________	Distancia a casa	___________
Logro en matrícula universitaria	___________	Deportes	___________
Tasa de crimen	___________		

9. ¿Qué tan importante son los compañeros de su estudiante cuando usted elige una escuela?
   No importante ☐ Un poco importante ☐ Importante ☐ Muy importante

10. ¿Qué tan importantes son los puntajes de las pruebas de una escuela al elegir una escuela?
    No importante ☐ Un poco importante ☐ Importante ☐ Muy importante

11. ¿Crees que las escuelas que atraen a los estudiantes con alto rendimiento también son las más efectivas para mejorar puntajes en exámenes estatales?
    Si ☐ No ☐

Sección C- Esta es la sección más importante de la encuesta. Ahora le haremos unas preguntas sobre sus preferencias y creencias acerca de dos características importantes de las escuelas. Primero, le proporcionamos un breve resumen de las dos características.

Determinamos la calidad de una escuela según su puntaje promedio en los exámenes estatales. Sin embargo, los puntajes tienen dos partes, uno que mide la capacidad de la escuela para atraer a estudiantes de alto rendimiento, y el segundo es el impacto de la escuela en el crecimiento de los puntajes de las pruebas. Por ejemplo, una escuela puede tener puntuaciones promedio altas en los exámenes porque atrae a estudiantes de alto rendimiento que se desempeñan bien independientemente de la escuela a la que asisten, pero esa escuela puede tener un impacto limitado en el crecimiento de los puntajes sus estudiantes. Otra escuela puede tener calificaciones de exámenes promedio más bajas porque educa a los estudiantes con mas desventaja, pero tiene un fuerte impacto en el crecimiento de las calificaciones de exámenes.

Podemos medir la capacidad de una escuela para atraer a buenos estudiantes al medir los puntajes promedio de los estudiantes que ingresan (**rendimiento entrante**). De manera similar, podemos medir la capacidad de la escuela para mejorar los puntajes de los exámenes al observar los puntajes de los mismos estudiantes unos años después (**crecimiento de logros**). Por lo tanto, la calidad observada de una escuela es una combinación del rendimiento entrante de sus alumnos y el crecimiento de rendimiento que obtienen mientras están en la escuela. Algunos padres pueden preferir escuelas con alto rendimiento entrante, y otros pueden preferir escuelas con alto crecimiento de rendimiento.

12. Con la discusión anterior en mente, lo mejor que pueda, informe su creencia sobre la capacidad de cada escuela para atraer estudiantes de alto rendimiento (**rendimiento entrante**) y la capacidad de la escuela para mejorar los puntajes de las pruebas (**crecimiento de logros**). Proporcione un puntaje de 1-100, donde 100 significaría que cree que la escuela es la mejor en el distrito y 1 significaría que es la peor en el distrito.

Nombre de escuela	**Rendimiento entrante**	**Crecimiento de logros**
Academia para el Avance Colegial y Profesional (ACCR)		
Academia de Exploración Científica		
Academia de Futuros Humanitas		
Academia de Justicia Social Humanitas		
Academia de Preparación Tecnológica		

13. Clasifique las escuelas como si estuviera presentando la solicitud hoy. Tenga en cuenta que hay 5 escuelas entre las que puede elegir, así que clasifique las más preferidas como 1 y las menos preferida como 5.

Nombre de escuela	**Plantel de ubicación**	**Rango**
Academia para el Avance Colegial y Profesional (ACCR)	Sylmar	
Academia de Exploración Científica	Chavez	
Academia de Futuros Humanitas	San Fernando	
Academia de Justicia Social Humanitas	Chavez	
Academia de Preparación Tecnológica	Chavez	

Gracias por su participación. ¡Esperamos el éxito de su estudiante!

_____________________________ _________________________________

Firma de guardián legal Fecha

4.9.2 Pilot Details

During piloting, a subset of the survey took five minutes to complete, on average. The survey questions borrow from **?** in their style and both English and Spanish versions are provided below. Since this project is inherently interested in the selection criteria parents employ when presented with choosing between incoming achievement or achievement growth, it is paramount that parents effectively understand the difference. Therefore, the explanation contained in the survey was piloted on Mturk, a marketplace where researchers are increasingly hiring workers for piloting purposes.

The purpose of the piloting was to assess how effective proposed statements were to individuals from similar backgrounds as parents within Zones of Choice neighborhoods. Therefore, respondents were restricted to be under the age of 60 and have at most a high school degree. I included two questions to test a respondent's understanding of the difference between school-level achievement growth and incoming achievement after they read our statement. In each, either incoming achievement or achievement growth were held constant and the respondent had to infer differences between hypothetical schools based on the other measure. A question asking them to explain the difference between the two was also included. These restrictions were made to mirror the demographics of students' parents. Table 4.9 presents results. Roughly 90 percent of participants were able to correctly infer incoming achievement and achievement growth. Hispanic respondents responded correctly at a modestly lower rate that was not statistically significant. Looking at respondents written responses, around 70 percent wrote something that indicated they understood the difference between incoming achievement and achievement growth. In contrast to the other questions, Hispanic respondents wrote correct responses at a modestly higher rate but also statistically insignificant. Other pilots were run on samples that did not restrict to high school graduates and we observed higher averages.

4.9.3 Treatment Letters

4.9.4 Additional Figures and Tables

Figure 4.11: Treatment Letters in Spanish

Zona de Opción Bell

Para garantizar que tenga la mejor información disponible antes de su próxima decisión, proporcionamos información sobre las escuelas dentro de la Zona de Opción de Bell. Determinamos la calidad de una escuela en función de los puntajes promedio de los estudiantes en los exámenes estatales. Podemos medir la capacidad de una escuela para atraer estudiantes de alto rendimiento midiendo los puntajes promedio de las pruebas de sus estudiantes entrantes (**rendimiento entrante**). Ofrecemos la clasificación de cada escuela en todo el distrito a continuación.

Escuela	Rendimiento Entrante*	Ubicación del campus	Tipo de escuela
Preparatoria de Ciencia, Tecnología, Ingeniería, Artes y Matemáticas (STEAM)	65	Legacy	Escuela Pequeña
Preparatoria de Artes Visuales y Técnicas (VAPA)	58	Legacy	Escuela Pequeña
Academia de Salud	37	Elizabeth	Comunidad Educativa Pequeña
Academia de Aprendizaje Enlazado/Carrera de Profesores Multilingües	47	Bell	Academia de Aprendizaje Enlazado
Academia de Información Tecnológica	33	Elizabeth	Comunidad Educativa Pequeña
Academia de Artes, Idiomas, Artes Escénicas y Humanidades	47	Bell	Academia de Aprendizaje Enlazado
Academia del 9º Grado	16	Maywood	Comunidad Educativa Pequeña
Bell Estudios Globales	47	Bell	Escuela Comprensiva

* El rendimiento entrante de las escuelas se proporciona en percentiles. Por ejemplo, si una escuela tiene un rendimiento inicial de 55, esto significa que los puntajes promedio de las pruebas de sus estudiantes entrantes son mejores que el 55 por ciento de otras escuelas secundarias en el LAUSD.

Zona de Opción Bell

Para garantizar que tenga la mejor información disponible antes de su próxima decisión, proporcionamos información sobre las escuelas dentro de la Zona de Opción de Bell. Determinamos la calidad de una escuela en función de los puntajes promedio de los estudiantes en los exámenes estatales. Podemos medir la capacidad de la escuela para mejorar los puntajes de los exámenes usando el crecimiento de los puntajes de los mismos estudiantes entre el ingreso a la escuela y una fecha posterior (**crecimiento de logros**). Proporcionamos información sobre la capacidad de cada escuela para mejorar los puntajes de las pruebas de los estudiantes a continuación.

Escuela	Crecimiento de logros*	Ubicación del campus	Tipo de escuela
Preparatoria de Ciencia, Tecnología, Ingeniería, Artes y Matemáticas (STEAM)	78	Legacy	Escuela Pequeña
Preparatoria de Artes Visuales y Técnicas (VAPA)	43	Legacy	Escuela Pequeña
Academia de Salud	49	Elizabeth	Comunidad Educativa Pequeña
Academia de Aprendizaje Enlazado/Carrera de Profesores Multilingües	61	Bell	Academia de Aprendizaje Enlazado
Academia de Información Tecnológica	47	Elizabeth	Comunidad Educativa Pequeña
Academia de Artes, Idiomas, Artes Escénicas y Humanidades	61	Bell	Academia de Aprendizaje Enlazado
Academia del 9º Grado	24	Maywood	Comunidad Educativa Pequeña
Bell Estudios Globales	61	Bell	Escuela Comprensiva

* El crecimiento de logros de las escuelas se proporciona en percentiles. Por ejemplo, si una escuela tiene un crecimiento de logros de 55, esto significa que lel crecimiento de logros de su estudiante promedio es mejor que 55 por ciento de otras escuelas secundarias en el LAUSD.

Figure 4.12: Within-zone Quality Measure Correlation

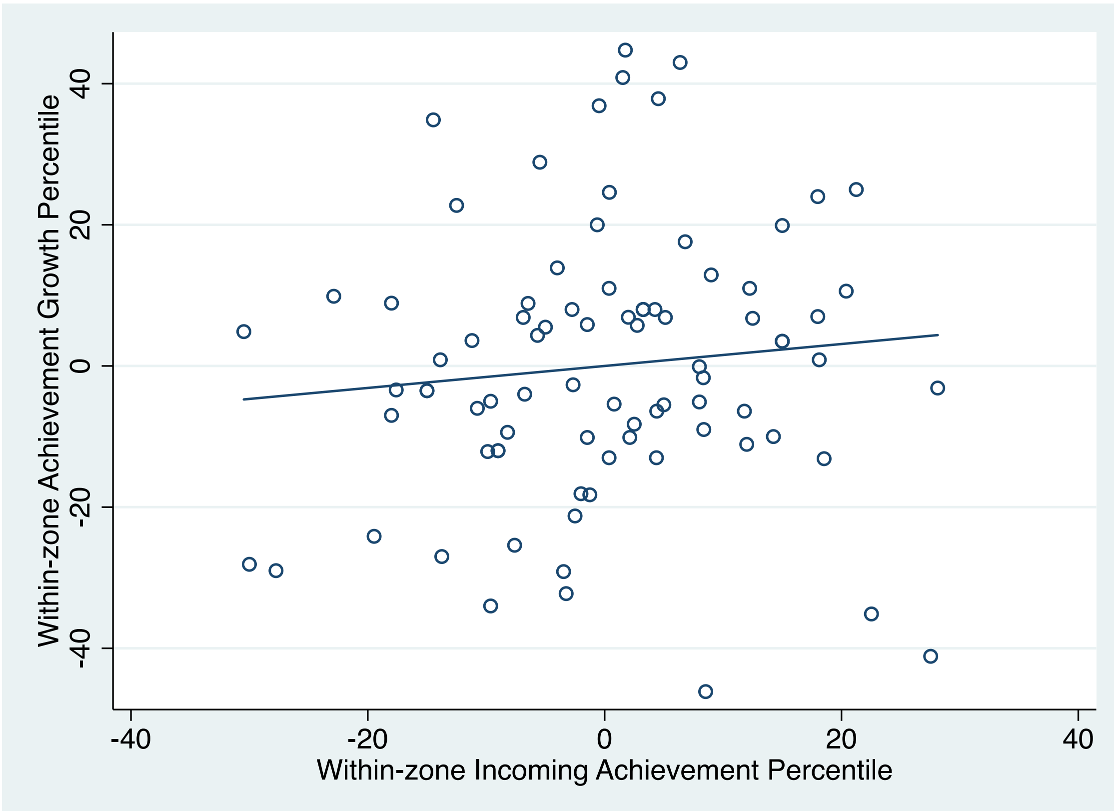


Figure 4.13: Choices

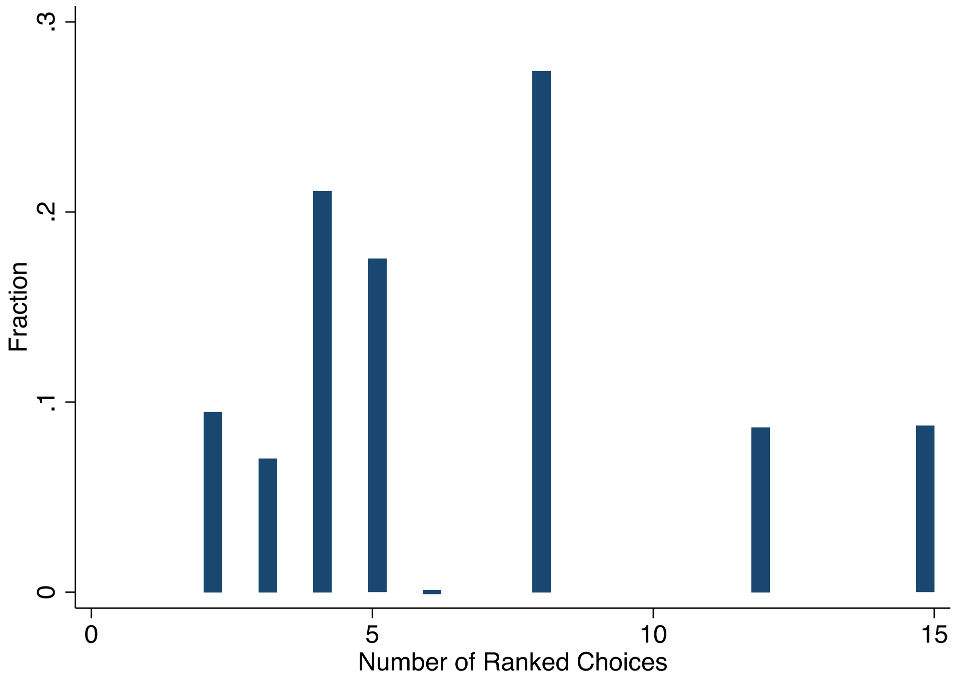


Figure 4.14: Neighborhood Exposure Distribution and Overlap

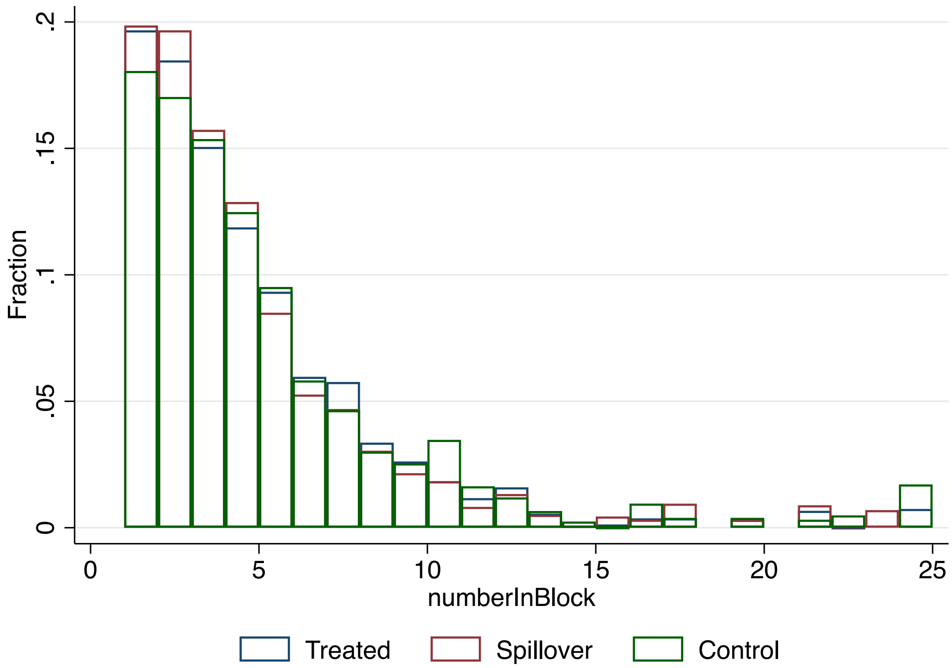


Figure 4.15: 2018 Cohort Placebo Estimates

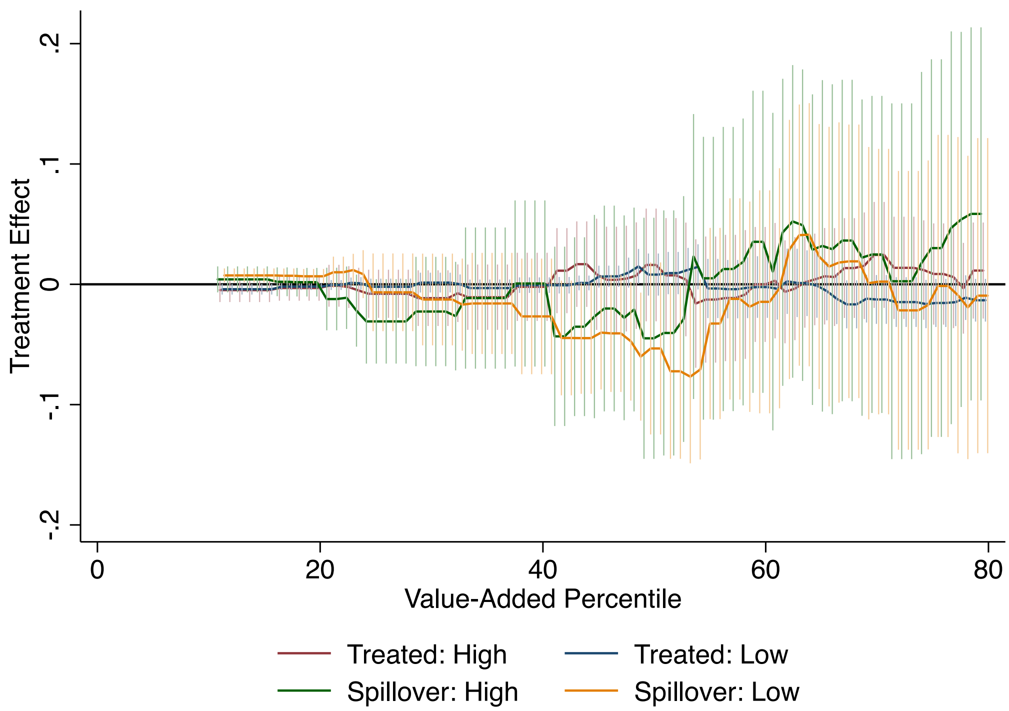


Figure 4.16: 2017 Cohort Placebo Estimates

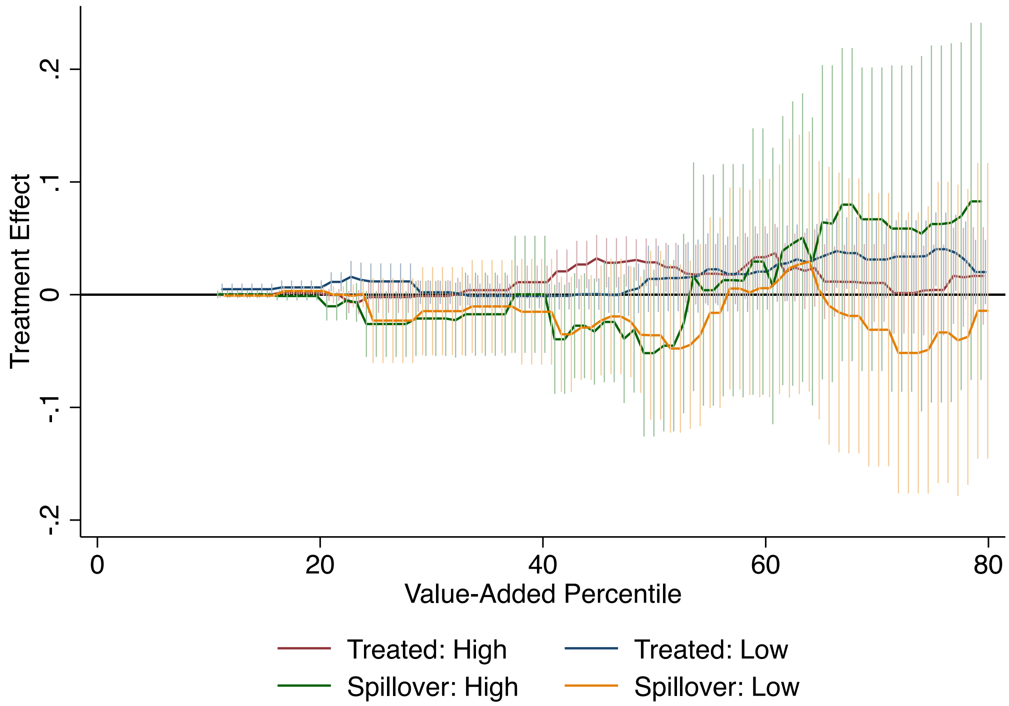


Figure 4.17: 2016 Cohort Placebo Estimates

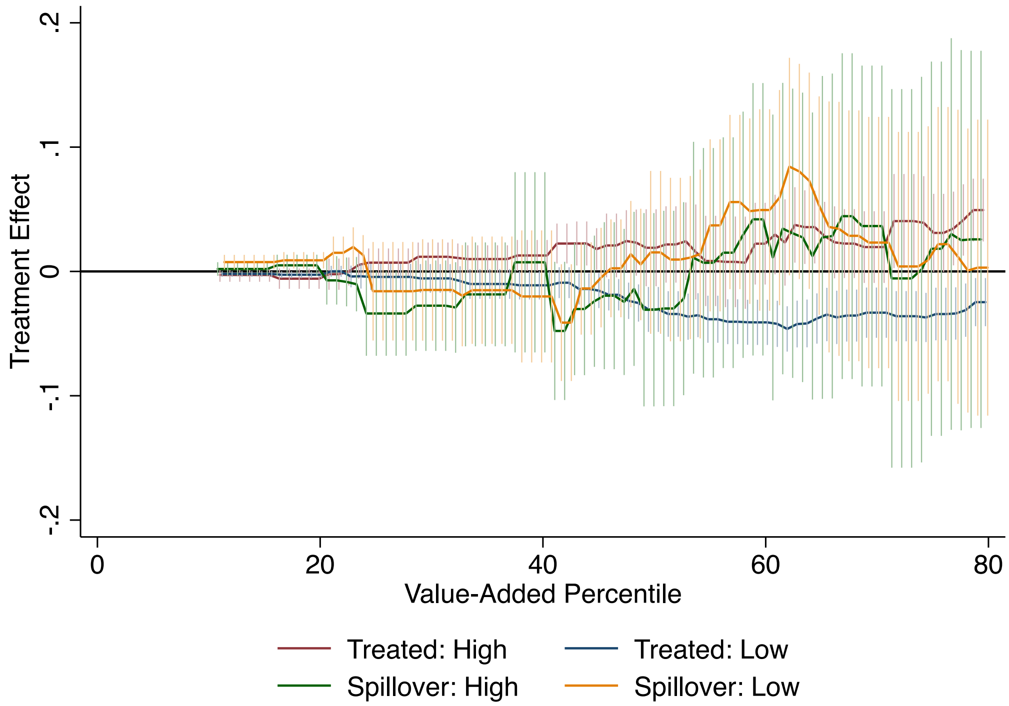


Figure 4.18: Spillover Validity

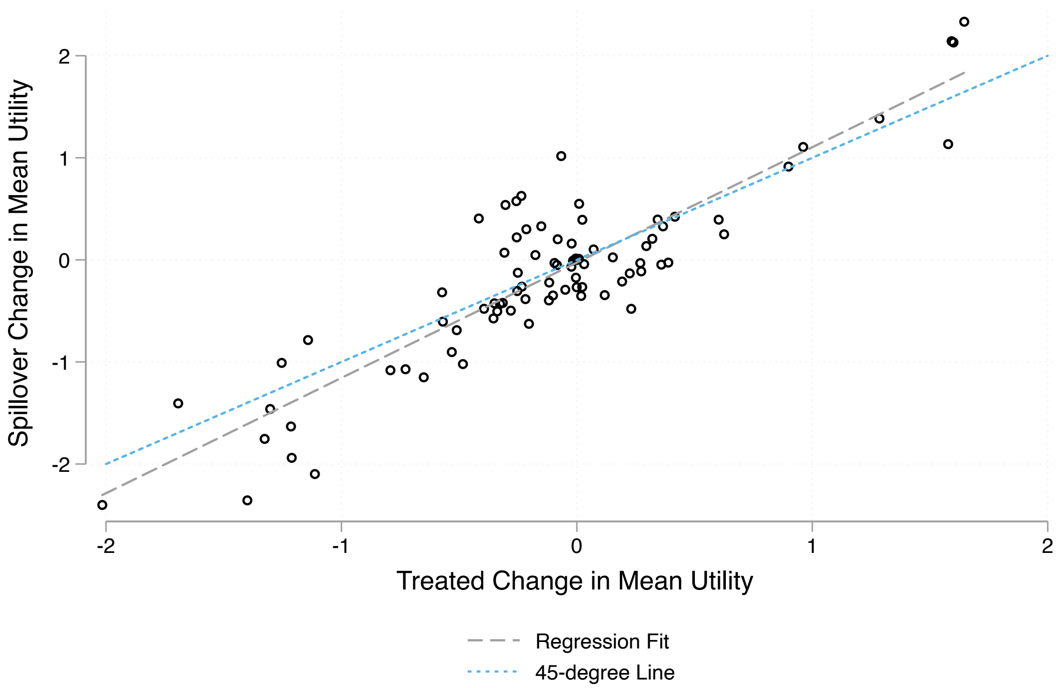


Table 4.9: Mturk Piloting Results

	Non-Hispanic	Hispanic	Difference
	(1)	(2)	(3)
Incoming Achievement	0.926	0.833	-0.092
			(0.058)
Achievement Growth	0.946	0.917	-0.029
			(0.044)
Both	0.892	0.792	-0.101
			(0.064)
Understood	0.671	0.687	0.0163
			(0.078)
Time to completion	290	320	30.1
			27.8
N	149	48	

Notes. Incoming achievement results come from a question holding achievement growth constant for two hypothetical schools and asking respondents which school had the highest incoming achievement. Achievement growth results similarly come from a question holding incoming achievement constant and asking respondents to infer hypothetical schools' achievement growth. Both corresponds to respondents that got both questions right. Understood presents results from a subjective evaluation of responses explaining the difference between achievement growth and incoming achievement. Time to completion corresponds to response times (in seconds)

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