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

Christopher Campos
Caitlin Kearns

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ABSTRACT

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

Christopher Campos
Booth School of Business
University of Chicago
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
Christopher.Campos@chicagobooth.edu

Caitlin Kearns
ckearns@berkeley.edu

A data appendix is available at http://www.nber.org/data-appendix/w31553

I Introduction

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

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

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

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

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

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

We address a variety of concerns related to our empirical approach. We find that alternative sources of competition from charter and magnet schools do not differentially affect ZOC neighborhoods, alleviating concerns that our results are driven by these alternative schooling models. We also find that the composition of students did not differentially change after the program expansion. Last, we conduct an intent-to-treat-like analysis and find qualitatively similar results.

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

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

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

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

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

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

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

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

II Institutional Details

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

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

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

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

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

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

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

II.B Program Features and Incentives

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

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

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

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

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

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

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

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

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

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

III Conceptual Framework

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Differences in student characteristics and in distance 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, $\boldsymbol{\alpha}_0 = (\alpha_{10}, \cdots, \alpha_{J0})$.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Proof. See Online Appendix B. □

III.A Empirical Map

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

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

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

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

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

$$\Delta\alpha_j = 0.$$

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

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

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

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

IV Data

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

IV.A Analysis Samples

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

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

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

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

IV.B Outcome Data

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

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

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

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

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

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

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

IV.C Descriptive Statistics

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

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

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

V Empirical Analysis

V.A Achievement and College Enrollment Effects

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

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

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

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

V.A.I Event-Study Results

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

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

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

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

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

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

V.A.II Robustness Checks

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

While the policy aims to increase within-zone choice, students may be self-selecting into the ZOC sector, introducing additional sorting concerns. An alternative approach to address these concerns is to define treatment at students' eighth-grade neighborhood level, ignoring the decision to enroll in a ZOC school or not. This mirrors the empirical strategies of other school choice reforms (Billings, Deming and Rockoff 2014; Fryer 2014). In particular, we define treatment at the level of students' eighth-grade neighborhood and remain agnostic about the school that students eventually sort into, an approach that generates intent-to-treat effects. Because we ignore the enrollment decision, this approach is less stringent in the sample selection criteria and includes schools that open post-reform and a wider swath of magnet programs. Online Appendix E.4 discusses additional details about this empirical approach.

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

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

V.B Probing the Role of Competition

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

V.B.I Decomposition of Achievement Effects

Online Appendix C discusses the achievement model we estimate that allows for a decomposition of effects into school and match quality (Abdulkadiroğlu et al. 2020). To start, we focus on treatment effects explained by changes in school quality, commonly referred to as school value-added. Online Appendix Figure E.15a reports event-study estimates isolating that component of achievement. We do not find evidence of differential trends in the pre-period, and in line with the event-study evidence on achievement, we find a clear trend break in ZOC student school effectiveness, accounting for most of the observed achievement effects. The treatment effects displayed in Online Appendix Figure E.15a capture both relative improvements in school quality over time and allocative changes of students to higher quality schools. We find that most of the effects are captured by improvements in school quality, although we do observe that allocative changes also play a small role.[12]

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

V.B.II School Effectiveness Treatment Effect Heterogeneity

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

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

VI Demand and OVG

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

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

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

VI.A Estimating Demand Parameters

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

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

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

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

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

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

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

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

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

While this approach allows for relative ease in estimation, a key limitation is the assumption that applicants do not act strategically in stating their preferences. Truthful statements are unlikely if applicants are strategic under an immediate acceptance mechanism (Agarwal and Somaini 2018, 2020) or if they do not understand the mechanism's rules or do have biased beliefs (Kapor, Neilson and Zimmerman 2020). Although strategic behavior is likely in ZOC neighborhoods, we emphasize that schools observe reported preferences—truthful or not—and respond to this demand accordingly. Nonetheless, demand estimates that account for strategic incentives are informative about the potential incentives schools may face under alternative centralized assignment policies, such as the increasingly popular deferred acceptance mechanism. We estimate an alternate model of demand in Online Appendix F and find qualitatively similar results, so we proceed with the simple model that assumes families do not behave strategically in their reports.

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

VI.B Parents' Valuation of School Effectiveness

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

$$\delta_{jct} = \xi_{cz(j)t} + \omega_P Q^P_{jt} + \omega_S \alpha_{jt} + \omega_M Q^M_{jct} + u_{jct}, \tag{5}$$

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

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

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

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

VI.C Option Value Gain

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

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

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

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

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

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

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

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

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

VII Discussion

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

VII.A Additional Mechanisms and Intermediate Outcomes

Prior work suggests that discipline is a significant factor in the no-excuses approach. We observe an increase in suspension incidents, indicating a change in disciplinary practices and a possible shift in school philosophy. Panel A of Table IV reports effects on student-level suspension incidents. Column 3 demonstrates that ZOC and non-ZOC suspension rates were on similar trends before the policy expansion, and Column 4 reports difference-in-differences estimates. In terms of the extensive margin, suspension incidents increase by roughly 5 percentage points, amounting to a 31 percent increase from the baseline mean. Looking at the intensive margin reveals a qualitatively similar pattern; an increase of 0.06 suspension days per student, amounting to a 28 percent increase from the baseline mean. Consistent with the notion of increased expectations—also correlated with no-excuses practices—we find reductions in absenteeism, also documented by Imberman (2011) for start-up charter schools. These findings mirror Angrist, Pathak and Walters (2013) in that effective urban charter schools impact achievement, disciplinary incidents, and attendance. This evidence suggests that teaching practices sharply changed between ZOC and non-ZOC schools. [16]

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

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

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

VII.B Institutional Features of ZOC

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

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

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

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

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

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

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

VIII Conclusion

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

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

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

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

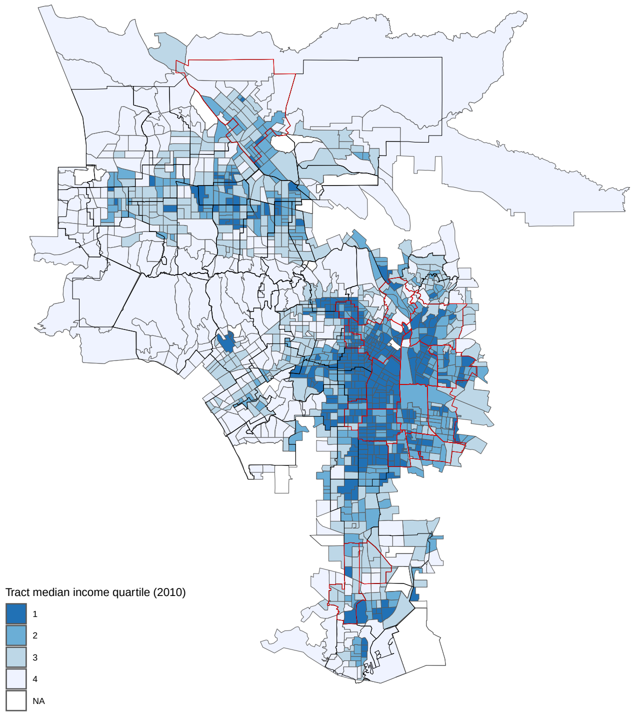


Figure I

ZOC and 2010 Census Tract Income

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

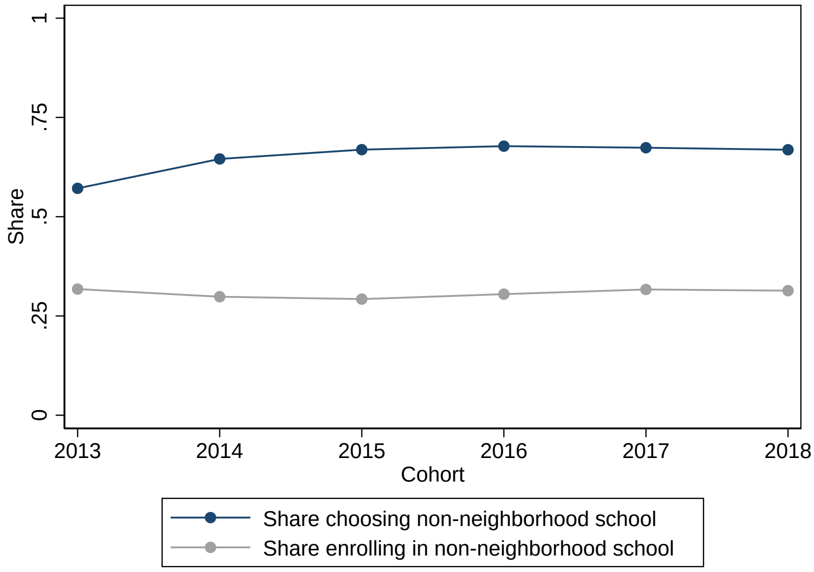
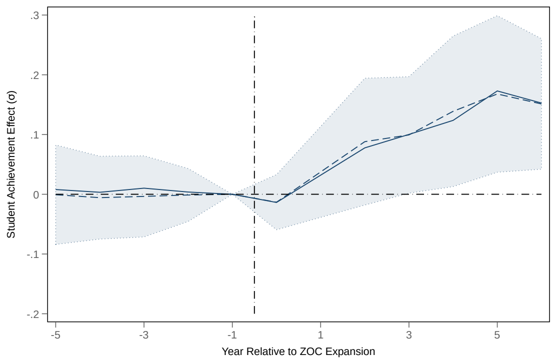


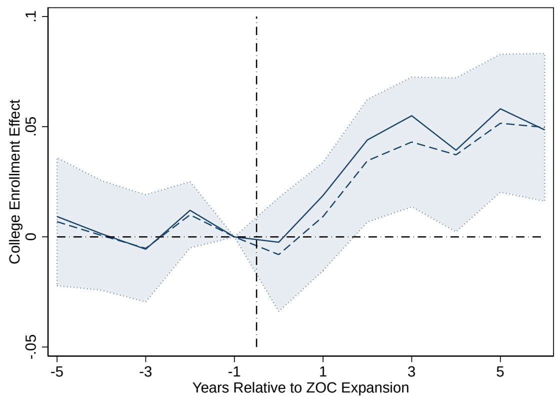
Figure II

Demand and Enrollment for Non-Neighborhood Schools

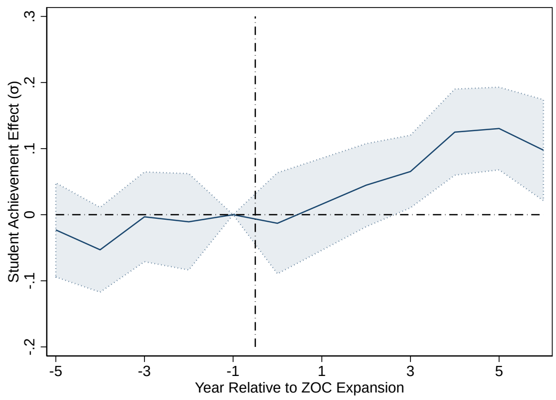
Notes: This figure reports statistics concerning application behavior of ZOC applicants. If we observe a ZOC applicant enroll in an LAUSD high school in ninth grade, we classify them as staying in the district. If we observe a ZOC applicant rank a school other than their neighborhood school as their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



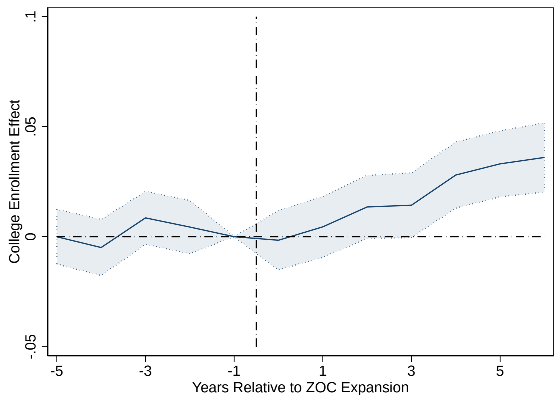
(a) Achievement



(b) Four-Year College Enrollment



(c) ITT: Achievement



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

Figure III

Market-Level Effects

Notes: Panel A and Panel B of this figure plots the estimates of β_k analogous to those defined in Equation 2, where k is the number of years since the ZOC expansion. The coefficient β_k shows difference-in-differences estimates for outcomes relative to the year before the policy. The dashed blue line in Panel A traces out estimates in the matched sample, and the solid line corresponds to estimates from the unmatched sample. Panel A reports treatment effects on student achievement and Panel B reports treatment effects on four-year college enrollment. Standard errors are clustered at the school level, and 95 percent confidence intervals are displayed by the shaded regions. Panel C and Panel D report intent-to-treat estimates where the treatment is assigned at the neighborhood level as opposed to the school level. The neighborhood is determined by a students' middle school address. This is discussed in detail in Online Appendix E.4. For Panel C and Panel D, standard errors are clustered at the attendance zone level, and 95 percent confidence intervals are displayed by the shaded regions.

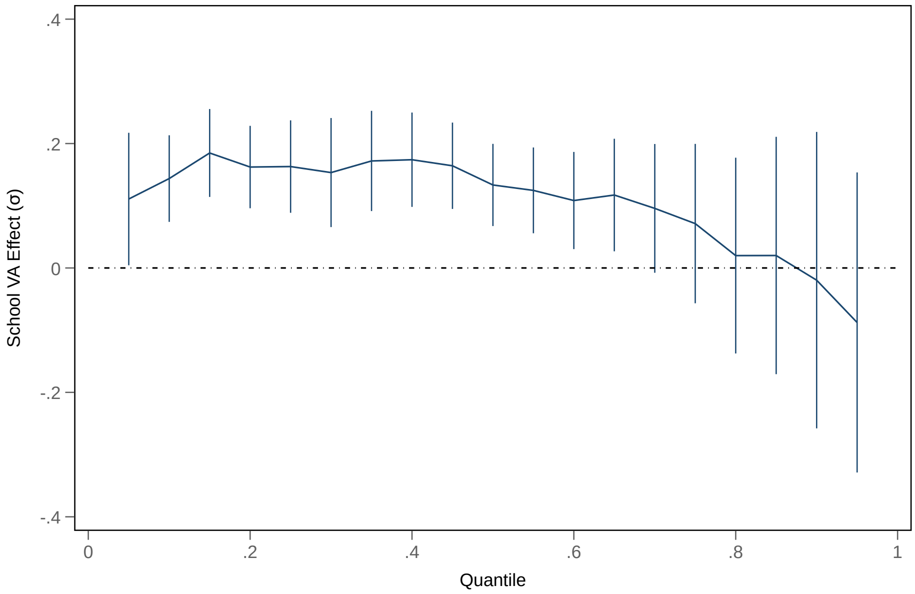


Figure IV

Quantile Treatment Effects on School Effectiveness

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

Table I

ZOC and Non-ZOC Student Characteristics, 2013–2019

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

Notes: Columns (1) and (2) report group means corresponding to row variables. Column (3) reports the difference between Column (1) and Column (2) and reports a standard error in parentheses below the mean difference. Column (4) reports group means for the set of students enrolled in matched schools and thus consists of the control group in the empirical analysis. Column (5) reports the difference between Column (1) and Column (4), with a standard error in parentheses below the mean difference. Eighth-grade Math and ELA scores correspond to CST scores before 2014 and to SBAC after 2014. English Learner is defined to be one if a student is flagged as having any English learner status. Special Education is defined to be one if a student has any special education status. Migrant is defined to be one if the student is flagged as having a birth country other than the United States; it is self-reported. Spanish at home is defined to be one if a family reports speaking Spanish at home as the primary language. Poverty is defined to be one if a student is enrolled in a Community Eligibility (CEP) school, and if they are not, it is defined to be one if the student is a free or reduced-price lunch student. Parents College + is defined to be one if at least one parent reports having earned a bachelor's degree or higher. All standard errors are robust and clustered at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table II

Preferences for School Attributes

	(1)	(2)	(3)	(4)
	Panel A: Baseline Rank-ordered Logit Estimates			
School Quality	0.137***			0.129***
	(0.0365) [0.035]			(0.0358) [0.071]
Peer Quality		0.116		0.0393
		(0.135) [0.645]		(0.139) [0.967]
Match Quality			0.118	0.0495
			(0.108) [0.211]	(0.0699) [0.233]
R-squared	0.440	0.429	0.437	0.431
	Panel B: Rank-ordered Logit + School Controls			
School Quality	0.138***			0.151***
	(0.0385) [0.057]			(0.0412) [0.056]
Peer Quality		-0.0522		-0.129
		(0.100) [0.880]		(0.0904) [0.489]
Match Quality			0.0678	0.0564
			(0.0865) [0.378]	(0.0682) [0.128]
R-squared	0.660	0.651	0.653	0.647
	Panel C: Rank-ordered Logit + School Controls + Quadratic Distance			
School Quality	0.134***			0.147***
	(0.0375) [0.057]			(0.0402) [0.073]
Peer Quality		-0.0652		-0.134
		(0.100) [0.815]		(0.0914) [0.513]
Match Quality			0.0665	0.0524
			(0.0864)[0.369]	(0.0682) [0.1331]
Observations	596	596	596	596
Zone X Cell X Year FE	X	X	X	X

Notes: This table reports estimates from regressions of school popularity measures δ_{jct} for each school among students in achievement cell c in cohort t on estimated school average treatment effect, ability, and match effects all scaled in standard deviation units. Panel A uses δ_{jct} estimates from rank-ordered logit models, and Panel B augments the regression models with time-varying school attributes and characteristics. Panel C uses mean utilities estimated from models with quadratic distance costs and also includes time-varying school attributes as controls. The school attributes and characteristics include STEM, social justice, college academy, art, and business program indicators, along with teacher attributes and school-level course offering attributes. Each observation is weighed by the inverse of the squared standard error of the mean utility estimate. Standard errors are clustered at the cell-by-zone level and are reported in parentheses. Numbers in brackets report p-values from wild bootstrap iterations for models that cluster errors at the zone level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table III

Option Value Gain and Treatment Effect Heterogeneity

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

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

Table IV

Additional Mechanisms and Intermediate Outcomes

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

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

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