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

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Abstract

This paper studies how parents' preferences for schools are affected by information about school and peer quality and how social interactions mediate changes in demand. I design an information intervention that cross-randomizes whether parents receive information about school quality (school value-added) and peer quality. Using a spillover design that varies the saturation of information across schools, I also randomize parents' proximity to other parents with similar information. I find that the information leads to changes in parental preferences toward higher value-added schools, and this occurs when both parents and their neighbors receive information. These results imply substantial information spillovers. I complement this evidence with survey data on the distribution of beliefs over school and peer quality and conclude that the direct and spillover effects of my experiment come primarily from changes in parental preferences rather than an updating of parental beliefs in response to information. These findings show that when parents are informed about school and peer quality, their social interactions lead to changes in preferences in a way that rewards more effective schools.

Keywords: school choice, school quality, preferences, information
JEL Classification: I21, I24

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1 Introduction

Parents' valuation of effective schools govern the success of school choice policies, but many open questions remain as to what they prioritize. Some studies suggest that parents prioritize schools that improve student learning (Beuermann et al., 2022, Campos and Kearns, 2022), while others find that they tend to prioritize schools based on peer attributes regardless of the quality of the school itself (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020, Rothstein, 2006). The existing evidence, however, tends to rely on revealed preference arguments that are complicated by the presence of imperfect information. As a consequence, the existing evidence encounters challenges isolating preferences in settings where choices are made with imperfect information. In addition to uncertainty about parents' valuations, open questions remain about what parents know when making decisions and what factors mediate their choices.

This paper reports evidence from an experiment that sheds light on these open questions. To understand the severity of information frictions, I elicit parents' beliefs about school and peer quality in a baseline survey. Both measures have been extensively studied in prior work (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020, Beuermann et al., 2022, Hastings and Weinstein, 2008, Rothstein, 2006), but to date, we have a limited understanding of what parents actually know. Belief elicitation is followed by information provision that distributes information on both quality measures and allows me to study how relative preferences evolve in a laboratory-like setting. Combining information about beliefs with the information provision allows for a novel decomposition of treatment effects into factors driven by salience and information updating. Last, to gain insight into factors mediating choices, I introduce a component into the design that allows me to measure the importance of social learning (Golub and Sadler, 2017).

The setting is a subset of high schools in Los Angeles neighborhoods referred to as Zones of Choice (ZOC) neighborhoods (Campos and Kearns, 2022)[1] Families residing within ZOC neighborhoods have several nearby schools they can apply to as opposed to a single neighborhood school. Each market is unique in its offerings, size, and location, which provides a rich setting to experimentally study behavior in many markets with pre-determined, market-specific enrollment flows. Applications and assignments are centralized, allowing insight into rich demand-side behavior to probe and understand how information interventions affect the ways families systematically trade off different school attributes. The experimental sample consists of roughly 20,000 students in 32 unique markets across two experimental waves.

The design of the experiment is informed by prior work (Ainsworth et al., 2020, Cohodes et al., 2022, Corcoran et al., 2018, Hastings and Weinstein, 2008) but is differentiated in important ways. As in previous work, one layer of randomization is at the middle school level, where in this setting high-school applicants enrolled in different middle schools are assigned different information treatments that vary the saturation levels of each school. One differentiation from prior work is that two information treatments are cross-randomized within schools, conditional on the saturation level. This mirrors the canonical spillover design employed in prior work (Crépon et al., 2013). In the ZOC setting, this design choice allows one to isolate how relative

[1] The ZOC program is a form of controlled choice, similar to past controlled choice programs in Seattle and Charlotte-Mecklenburg but with different goals motivating the controlled choice scheme.

preferences change in response to various information treatments and to isolate the role of social interactions or spillovers. I then complement the information intervention with novel survey data I collect about parents' beliefs about both measures of quality.[2] Combining novel survey data with information provision and a canonical school choice model allows for a novel decomposition of treatment effects that nest a combination of preference (salience) impacts, information-updating effects, and correlated beliefs effects. The decomposition relies on first and second moments of the beliefs distribution, which I collect in the baseline survey. Therefore, the baseline survey is intrinsically linked to the subsequent analysis.

I start with a reduced-form analysis that does not rely on the survey results; I divide these analyses into two parts, starting with an analysis of the school-level experiment. This analysis aggregates across the various within-school information treatments and is informative about how effects vary by the degree of information availability. The treatment aggregation also allows for natural falsification tests of effects in pre-intervention periods, providing a distinct and more robust perspective of baseline balance. I find that families receiving any treatment rank higher AG (value-added) schools as their most-preferred options and the effects are sensitive to saturation status. I only find effects in schools with high-saturation levels, the paper's first piece of evidence pointing toward the importance of social interactions.[3] The impacts on most-preferred AG suggest there is scope to increase the competitive pressure schools face in public education markets and where competition may be lacking due to apparent preferences for peer quality (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020).

I then turn to provide evidence of social interactions in the school choice process. As in other spillover designs, I compare untreated families in treated schools to families in untreated schools. Any treatment effects on this subset of families follows from "spillovers," or what I refer to as social interactions. Distributional estimates reveal that families systematically choose schools with higher AG scores with a coinciding shift toward schools with lower IA scores. The effects are pronounced across most of the distribution, with minimal impacts at the top. Most importantly, changes in demand are identical across the various information-specific treatments, suggesting a community-level consensus reflected in their rank-ordered lists. The spillover effects I document point to the importance of social networks in interpreting and using information, a measure that may contribute to racial differences in preferences for academic quality (Hastings et al., 2006, Laverde et al., 2022).[4]. After documenting the empirical relevance of social interactions, I

[2]The beliefs data shed light on the joint distribution of families' beliefs, but eliciting beliefs about two different measures of quality presents some challenges in conveying messaging to parents that are addressed in various ways. I address this challenge in two ways. First, through extensive piloting, the choice of messaging succinctly signals the difference between peer quality and school quality. Peer quality is referred to as IA to emphasize the invariance to school quality, and school quality is referred to as AG to emphasize a change in achievement partly attributable to the school. Second, throughout the intervention, I deploy videos to teach and aid families' understanding about the differences between IA and AG. The videos serve an instrumental role in improving families' understanding of the content, working in tandem with the social learning the experiment is designed to measure.

[3]I also find qualitatively similar patterns across the entire rank-ordered list, demonstrating that increases in demand for higher value-added schools are not local to most-preferred options.

[4]More generally, the peer effect I document is distinct from those considered in the literature generating strategic complementarities (Allende, 2019) or studying the existence of stable matchings in the presence of externalities (Cox et al., 2021, Leshno, 2021, Sasaki and Toda, 1996). In contrast to the theoretical importance of peer externalities in those papers, I find weak evidence that parents necessarily care about a school's peer composition, but they do care about neighboring parents' opinions and information sets.

complement the experimental evidence with survey results to shed further light on the sources of changes in parents' choices.

Four facts arise from the survey data. First, families tend to be pessimistic about AG and optimistic about IA. These differences hold across the rank-ordered list, with modest gradients indicating that families are more pessimistic about the schooling options that they prefer less. Second, I find modest bias gradients with respect to student baseline achievement. Families' mean AG pessimism holds across the baseline achievement distribution at roughly half of a decile. In contrast, their IA optimism is less pronounced and approaches researcher-generated values among families with higher-achieving students. Third, I do not find salient student-level attributes that correlate with either IA or AG biases. This finding mirrors evidence that value-added measures tend to weakly correlate with observables, with a key distinction being that I focus on beliefs about value-added. Fourth, conclusions about preferences are highly sensitive to using researcher-generated measures or elicited measures. Using elicited measures instead of researcher-generated ones implies vastly different preference estimates and serves as caution when interpreting estimates in a variety of studies (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020, Beuermann et al., 2022, Campos and Kearns, 2022).

With the survey data, I turn to estimating treatment effects on utility weights assigned to school and peer quality. This analysis features a few key advantages. First, it leverages information from the entire rank-ordered list, providing a comprehensive summary of how families trade off school and peer quality. Second, the reduced-form analysis studies effects on IA and AG in isolation, while this analysis can hold constant preferences for one quality measure while studying preferences for the other. Third, with information about first and second moments of the belief distribution, I can decompose utility weight impacts into various sources. Therefore, treatment effects on utility weights overcome the reduced-form limitations and provide another corroborating perspective about how the intervention affects school choices.

I find that families increase their willingness to travel for AG; similarly, I find that their willingness to travel for IA decreases. The increases in willingness to travel for AG range between 0 and 0.7 kilometers for a school that has AG scores that are 10 percentile points higher. The decreases in willingness to travel for IA range between 0.4 and 1.4 kilometers. The findings are mostly consistent with the reduced-form results, with magnitudes that are quantifiable in terms of willingness to travel. Spillover effects remain mostly identical to the treatment effects within saturation clusters, a third and final piece of evidence highlighting the importance of social interactions. Last, decompositions demonstrate that most of the changes are due to changes in preferences, also interpreted as salience effects. Taken at face value, this finding suggests that while families do update their information in response to the intervention, the observed changes in choices reflect a reorientation of their preferences toward higher value-added schools. Overall, the experiment provides robust evidence that when properly informed, families make choices in a way that is consistent with them rewarding effective schools and that social interactions are important mediators governing changes in demand.

The findings in this paper contribute to three strands of literature, with the most immediate related to parents' valuation of effective schools. Early findings focus on implications from school choice experiments where some students are lotteried into their most-preferred schools, while others fail to receive offers (Abdulkadiroğlu et al., 2014, Cullen et al., 2006, Deming et al., 2014, Lucas and Mbiti, 2014). The findings in these papers more or less conclude that there are minimal impacts from enrolling in a most-preferred school, indicating that parents do not systematically sort into schools with higher value-added or school quality differences within local markets are minimal. More recently, a growing body of evidence has turned to estimating preferences leveraging the full suite of information contained in rank-ordered lists submitted to centralized assignment systems. Some find that parents value effective schools (Beuermann et al., 2022, Campos and Kearns, 2022), and others find that families care mostly about schools' peer composition (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020).

While these recent papers are a step forward in understanding parents' preferences, they all invariably rely on revealed preference arguments in settings where it is plausible that imperfect information looms large. The presence of imperfect information muddles the interpretation as estimates could reflect families misoptimizing as opposed to expressing genuine preferences. This paper contributes to the literature in two ways. It is the first to show evidence on the joint distribution of families beliefs on peer and school quality. Existing papers have alluded to the potential presence and importance of biases, while this paper measures them. The paper also provides experimental evidence of how families' choices systematically change under various information scenarios, alleviating concerns about interpreting estimates due to information frictions.

An extensive literature has deployed information interventions to answer and address a variety of policy-relevant questions (Haaland et al., 2020). In education, the seminal work of Hastings and Weinstein (2008) highlights the importance of information frictions in school choice settings and the potential for information to change both choices and outcomes. Follow-up work has emphasized the importance of easily accessible information and potential inequities in who takes up the information (Cohodes et al., 2022, Corcoran et al., 2018). More recently, a turn to the potential equilibrium effects of large-scale policies has further motivated the usefulness of these interventions in affecting outcomes (Allende et al., 2019, Andrabi et al., 2017).

The existing papers, however, tend to focus on measures that are similar to what I refer to as peer quality and do not distinguish between preferences for peer or school quality. Ainsworth et al. (2020) is the only paper to consider a school-quality-based intervention but omits differentiating between school and peer quality. This paper builds on the existing work that uses information interventions to address school choice inequities, and it contributes by shedding light on families' preferences and decomposing treatment effects to estimate policy-relevant effects.

A third and nascent literature has focused on the implications of peer effects in the school choice process. Existing papers have primarily focused on how externalities permeate through demand systems, with Allende et al. (2019) studying how preferences for peers distort school incentives in a structural model based on insights from Rothstein (2006). Another strand of papers in the market design space have highlighted that stable matchings may not exist if preferences are interdependent (Sasaki and Toda, 1996). A recent strand of papers have tackled studying the existence of stable matchings, allowing market participants to express preferences for peer attributes (Cox et al., 2021, Leshno, 2021). This paper provides empirical evidence that such peer preferences may not matter in some markets and is consistent with findings for prior ZOC cohorts (Campos and Kearns, 2022). My findings also pivot the peer effect discussion from externalities that do not generate interdependent preferences as captured by preferences for peer composition to externalities operating through information and social networks (Golub and Sadler, 2017). The evidence of social interactions in the school choice process gives rise to potential network-based inequalities that have received less empirical attention.

The rest of the paper is organized as follows. Section 2 provides a description of the setting in which the intervention takes place. Section 3 presents a simple school choice framework that aids the interpretation of effects and motivates the final decomposition. Section 4 discusses the experiment's design in detail as well as the data and standard checks in the randomized control trials. Section 6 reports the survey results, and Section 5 reports results from the school-level experiment, direct evidence about the importance and magnitude of social interactions in the school choice process, and utility weight impacts along with their decomposition. Section 7 discusses the implications of the findings for future research, and Section 8 concludes.

2 Institutional Details

The ZOC program is one of several public choice alternatives provided by the Los Angeles Unified School District (LAUSD) in addition to charter schools, magnet programs, and other choice options. It is a neighborhood-based school choice program that organizes clusters of schools and programs into local markets and offers families several nearby options as opposed to a single neighborhood program. ZOC markets operate independently, with their student population determined by geographic boundaries drawn by the district.[5] The markets vary in size and programs' spatial differentiation. Some markets contain as few as two schools (2 programs) to as many as five schools (15 programs), and families apply to programs in their market the year before enrollment. Campos and Kearns (2022) provide a more detailed description of the program's history and expansion in 2012.

ZOC does not cover the entire school district. Most of the zones are concentrated in Central, South, and East Los Angeles, with some zones as far south as Narbonne and others as far north as Sylmar in the San Fernando Valley. Although LAUSD is composed of primarily Hispanic students (68%), the Hispanic share within ZOC neighborhoods is 86%. Nearly all (90%) of ZOC students are classified as poor, and their parents are less likely to have college degrees. The relative homogeneity of students within ZOC markets is an important and distinguishing feature of this program compared to other controlled choice programs (Orfield and Frankenberg, 2013).

Families residing within ZOC boundaries apply to high schools during the fall semester of their students' eighth-grade year. During this time, ZOC administrators and guidance counselors make the application a salient aspect of this semester. It is during this time period where most families learn about the program's existence and start researching their options.[6] Failure

[5]Not all families residing within a Zone of Choice enroll in a program school. Some opt for a charter sector, some opt for a private schools, and some enroll in another district magnet program through another centralized choice system.

[6]The survey results discussed in Section 6 show that roughly 70% of families in the 2021 application cohort

to submit an application may result in being assigned 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, and they also hold information sessions to inform parents about the program, their options, and how to submit applications. Open houses are hosted by high schools to help recruit students. In past years, the district experimented with sending mailers to families informing them about the program and their options.

School assignments are made centrally by the ZOC office through the use of an immediate acceptance mechanism, also referred to as the Boston mechanism (Abdulkadiroğlu and Sönmez, 2003) or the first preference first mechanism (Terrier et al., 2021). There are neighborhood and sibling priorities that are taken into consideration during the assignment process, but no other priorities or screening strategies are in place as is common in New York City (Cohodes et al., 2022, Corcoran et al., 2018). Although the length of the list is not capped, avoiding theoretical and empirical issues highlighted in the literature (Calsamiglia et al., 2010, Haeringer and Klijn, 2009), the mechanism is not strategy proof as it incentivizes families to misreport their ordinal preferences to avoid being assigned to a school far down their preference list (Abdulkadiroğlu and Sönmez, 2003).

In general, there is mixed evidence about the degree of sophistication and incentives to misreport preferences under immediate acceptance mechanisms. One body of evidence from various cities shows that low socioeconomic status families are more prone to misunderstand the rules and are less likely to strategize (Abdulkadiroglu et al., 2006, Agarwal and Somaini, 2018, Calsamiglia and Güell, 2018, Terrier et al., 2021), while other research finds weaker socioeconomic status gradients with respect to strategizing (Calsamiglia et al., 2020) or predicts an opposite gradient if there are unequal outside options (Akbarpour et al., 2022). Anecdotes suggest that the rules of the mechanism are not too salient during information sessions. Therefore, it is likely that strategizing is not a first order concern given the disproportionate share of low socioeconomic status families and the low importance assigned to the mechanisms' technical rules beyond priorities.

Information gaps are likely prevalent in ZOC markets. To begin, many families are unaware of their eligibility and necessity to participate in the program at the start of the application cycle (see Appendix B). In addition, many "low-touch" information interventions have been shown to influence K-12 choices across the United States (Cohodes et al., 2022, Corcoran et al., 2018, Hastings and Weinstein, 2008, Valant, 2014, Weixler et al., 2020) and around the world (Ajayi and Sidibe, 2020, Ajayi et al., 2020, Allende et al., 2019, Andrabi et al., 2017, Arteaga et al., 2022). The findings from low-touch interventions argue that treatment effects imply the presence of imperfect information as perfectly informed families would not change their choices in response to researcher-provided information.

These implications are limited as a combination of factors influence changes in K-12 choices in response to information interventions. For example, simply showing families information about any attribute will make them rethink the importance of that attribute, effectively "chang-

had not heard of the program at the start of the application cycle.

ing" their preferences. Without additional data about families beliefs, however, it is impossible to distinguish between information-updating and salience (or preference) effects. Perhaps surprisingly, the existing literature is thin in terms of collecting families' beliefs (Ainsworth et al. (2020) is a notable exception) and thus cannot distinguish between the confluence of factors contributing to changes in K-12 choices. The following section bridges this gap with a simple model that motivates the survey collection and intervention.

3 Conceptual Framework

Canonical school choice models assume families have accurate information at the time they make decisions, yet a growing a body of evidence suggests this assumption is far from true (Ainsworth et al., 2020, Andrabi et al., 2017, Arteaga et al., 2022, Hastings and Weinstein, 2008). The presence of biases will distort choices and introduce allocative inefficiencies (Ainsworth et al., 2020). In this section, I outline a school choice model that allows families to choose schools based on their beliefs and models the potential effects of information interventions.

Families are indexed by $i \in \mathcal{I}$ and schooling options by $j \in \mathcal{J}$. The indirect utility of family i enrolling their child in school j is

$$U_{ij} = \delta_j - \lambda d_{ij} + \varepsilon_{ij}, \tag{1}$$

where δ_j captures mean utility of school j, d_{ij} measures the distance between household i and school j, and ε_{ij} is unobserved preference heterogeneity. I assume that mean utility is summarized by school and peer quality, Q_j^S and Q_j^P, respectively:

$$\delta_j = \gamma_P Q_j^P + \gamma_S Q_j^S.$$

I also assume that the Q_j^P and Q_j^S are bivariate normal:

$$\begin{pmatrix} Q_j^P \\ Q_j^S \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_P^2 & \rho_Q \sigma_P \sigma_S \\ \rho_Q \sigma_P \sigma_S & \sigma_S^2 \end{pmatrix} \right),$$

where ρ_Q governs the correlation between Q_j^P and Q_j^S and σ_P and σ_S govern the respective standard deviations.

Beliefs are introduced with biases that govern how families are imperfectly informed about Q_j^P and Q_j^S. Individual-level beliefs have idiosyncratic quality- and school-specific biases $\tilde{Q}_{ji}^P = (1 + b_{Pji})Q_j^p$ and $\tilde{Q}_{ji}^S = (1 + b_{Sji})Q_j^S$. I assume that beliefs are also bivariate normal:

$$\begin{pmatrix} b_{Pji} \\ b_{Sji} \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} \mu_P \\ \mu_S \end{pmatrix}, \begin{pmatrix} \sigma_{Pb}^2 & \rho_b \sigma_{Pb} \sigma_{Sb} \\ \rho_b \sigma_{Pb} \sigma_{Sb} & \sigma_{Sb}^2 \end{pmatrix} \right),$$

with ρ_b governing the correlation of biases and σ_{Pb} and σ_{Sb} the respective standard deviations.

In the absence of additional information, families' choices reflect decisions made using their beliefs about Q_j^P and Q_j^S. Therefore, families' perceived indirect utility is

$$\tilde{U}_{ij} = \gamma_P \tilde{Q}_{ji}^P + \gamma_S \tilde{Q}_{ji}^S - \lambda d_{ij} + \tilde{\varepsilon}_{ij}. \tag{2}$$

To address information gaps, a school district distributes information to a subset of families. The school district sends families in transitioning grades information and randomizes the schools whose families receive information (see Section 4 for intervention details). Families at treated schools that receive information make choices using the information. In other words, their $b_{Pji} = 0$ or $b_{Sji} = 0$, depending on the information they receive.[7] Families that do not directly receive information can obtain it by interacting with other directly treated families nearby if their school was treated; families at untreated schools do not receive any information and cannot indirectly receive any information from others nearby. Let $\mathcal{I}_P$ and $\mathcal{I}_S$ be the set of families receiving peer quality and school quality information, respectively, and let $\mathcal{I}_C$ be the subset of families in treated schools that do not directly receive any information. Last, information is randomly distributed.

Receiving information—directly or indirectly—potentially causes families to change the weights they assign to Q_j^P and Q_j^S. Equation 2 is augmented to capture the changes

$$\begin{aligned} \tilde{U}_{ij} = \gamma_P \tilde{Q}_{ji}^P &+ \gamma_S \tilde{Q}_{ji}^S - \lambda d_{ij} \\ &+ \beta_P Q_j^P \times \mathbf{1}\{i \in \mathcal{I}_P\} + \beta_S Q_j^S \times \mathbf{1}\{i \in \mathcal{I}_S\} \\ &+ \psi_P Q_j^P \times \mathbf{1}\{i \in \mathcal{I}_C\} + \psi_S Q_j^S \times \mathbf{1}\{i \in \mathcal{I}_C\} + \tilde{\varepsilon}_{ij} \end{aligned} \tag{3}$$

where $\mathbf{1}\{i \in \mathcal{I}_P\}$, $\mathbf{1}\{i \in \mathcal{I}_S\}$, and $\mathbf{1}\{i \in \mathcal{I}_C\}$ are indicators for belonging to the treatment P, treatment S, or spillover groups, respectively. The β_S, β_P, ψ_S, and ψ_P capture changes in utility weights for the various treatment groups. The utility weight impacts are all identified relative to families in untreated schools who cannot obtain any information.

Equation 3 implicitly assumes that treatments only affect the relevant weights; for example, peer quality information only affects peer quality weights. The spillover group can learn about both quality measures through social interactions with a probability equal to the saturation level. Equation 3 also assumes constant treatment effects, ruling out heterogeneity with respect to initial biases. Another implicit assumption is that the information does not affect distance cost parameters.

The first assumption warrants further discussion. Information about Q_j^P should induce changes in choices about Q_j^S through either ρ_Q or ρ_B. Therefore, the current parameterization implies that β_S and β_P will be a confluence of changes due to changed information sets, actual changes in preferences, and changes due to the correlated nature of the quality measures. This intuition motivates a simple decomposition that captures these various factors.

To see this directly, notice that

$$E[U_{ij}|T_P = 1, T_S = 1, Q_j^P, Q_j^S] = (\gamma_P + \beta_P)Q_j^P + (\gamma_S + \beta_S)Q_j^S, \tag{4}$$

$$E[U_{ij}|T_P = 1, T_S = 0, Q_j^P, Q_j^S] = (\gamma_P + \beta_P)Q_j^P + \gamma_S(1 + \mu_S - \rho_b \frac{\sigma_{Sb}}{\sigma_{Pb}} \mu_P)Q_j^S, \tag{5}$$

[7] This implicitly assumes perfect compliance in using the information. In general, it is challenging to detect compliance in information interventions without additional data.

$$E[U_{ij}|T_P = 0, T_S = 0, Q_j^P, Q_j^S] = \gamma_P(1 + \mu_P)Q_j^P + \gamma_S(1 + \mu_S)Q_j^S. \tag{6}$$

Denoting the difference of Equations 5 and 6 as Δ^P, we have

$$\Delta^P = (\beta_P - \mu_P\gamma_P)Q_j^P - \rho_B\frac{\sigma_{Sb}}{\sigma_{Pb}}\mu_S\gamma_S Q_j^S. \tag{7}$$

Last, notice that the average change in utility is

$$E[\Delta^P|Q_j^P] = (\beta_P - \mu_P\gamma_P - \rho_B\frac{\sigma_{Sb}}{\sigma_{Pb}}\mu_S\rho_Q\frac{\sigma_S}{\sigma_P}\gamma_S)Q_j^P, \tag{8}$$

which follows from the distributional assumptions on Q_J^P and Q_j^S. Equation 8 demonstrates that the average change in estimated utility weights for peer quality reflects a salience channel, β_P; a portion driven by families updating their choices, $\gamma_P\mu_P$; and a factor driven by how their choices for school quality changed, a correlated beliefs channel, $\rho_B\frac{\sigma_{Sb}}{\sigma_{Pb}}\mu_S\rho_Q\frac{\sigma_S}{\sigma_P}\gamma_S$. One can similarly show that

$$E[\Delta^S|Q_j^S] = (\beta_S - \mu_S\gamma_S - \rho_B\frac{\sigma_{Pb}}{\sigma_{Sb}}\mu_P\rho_Q\frac{\sigma_P}{\sigma_S}\gamma_S)Q_j^S. \tag{9}$$

The decompositions demonstrated in Equations 8 and 9 have intuitive interpretations. Assume that families have a taste for both peer and school quality, $\gamma_P > 0$ and $\gamma_S > 0$, both of which are verified in the data. In the absence of any salience or correlated beliefs effects ($\beta_P = 0$ and $\rho_B\mu_P\gamma_P = 0$), families with systematic positive biases $\mu_P > 0$ and receiving peer quality information will revise downward and be reflected as an apparent decrease in their taste for peer quality. Similarly, in the absence of information-updating and salience effects, the direction of bias induced by correlated beliefs is governed by μ_S if $\rho_B > 0$ and $\rho_Q > 0$, both of which are true in the data. If $\mu_S > 0$, then receiving information about peer quality makes it so that the degree of relative overestimation of school quality relative to peer quality is larger, making it appear that choices deviate away from peer quality. In contrast, if $\mu_S < 0$, then the opposite is true, making it appear that choices deviate toward peer quality. The correlated beliefs channel is present in treatment arms with only one quality measured, while the information-updating channel is present in all treatment arms.

To see this, if we denote the difference between Equations 4 and 6 as Δ^B, we get

$$E[\Delta^B|Q_j^S, Q_j^S] = (\beta_S - \mu_S\gamma_S)Q_j^S - (\beta_P - \mu_P\gamma_P)Q_j^P. \tag{10}$$

Receiving information about both shuts down the correlated beliefs channel as families make choices using known Q_j^P and Q_j^S.

Last, the spillover group indirectly receives information with a probability equal to the share of families receiving information at their school, π^C. This implies that some families act as if they receive both treatments and other families act as if they received neither. It follows that the probability of contact attenuates changes in expected utility,

$$E[\Delta^C|C = 1, Q_j^P, Q_j^S] = (\pi^C(\psi_S - \mu_S\gamma_S))Q_j^S - (\pi^C(\psi_P - \mu_P\gamma_P))Q_j^P. \tag{11}$$

If $\pi^C = 1$, the situation where everyone at a treated school receives information, then $E[\Delta^C | C = 1, Q_j^P, Q_j^S] = E[\Delta^B | T_P = 1, T_S = 1, Q_j^P, Q_j^S]$. Otherwise, the implied weight impact will be an average across those that accessed information and those that did not.

Putting together Equations 8, 9, 10, and 11 shows that utility weight impacts are an amalgam of factors that are typically unobserved. In particular, the decomposition above depends on moments of the beliefs distribution. The survey will contain information about the relevant moments necessary for the decomposition, which complements the reduced-form results that do not depend on survey moments. More generally, I design an experiment oriented around this conceptual framework to identify preference impacts and report the implied decomposition.

4 Experimental Design

Timeline

I incorporate a survey and information provision into a typical application cycle discussed in Section 2. The four phases that summarize the experiment are (i) the baseline survey, (ii) the information intervention, (iii) deliberation, and (iv) application submission. The survey distribution happens before the application cycle begins so that it can document 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.

Baseline Survey

The survey serves two purposes. The first is to gain general insight about parents' awareness of the program, their options, and factors that matter to them in the school choice process. Although the program has existed for nearly 10 years and is neighborhood based, parents may still be unaware of the options it provides. Second, elicited baseline beliefs and preferences are informative for the empirical analysis. In Section 3, I showed that utility weight treatment effects consist of a mixture of preference/salience impacts and information updating. With beliefs data, we can decompose treatment effects to shed light on the factors contributing to changes in choices.

The survey distribution changed in each wave. In the first year, only a paper survey was issued to students in their eighth-grade homeroom classrooms,[8] in the second wave, both the paper and digital surveys were distributed. The digital survey was messaged to families via internal district messaging services. While the survey distribution methods changed across waves, the questions remained constant. Efforts to digitize the paper surveys produced few surveys with enough signal to use in the paper, so the survey results in this paper consist of survey evidence from the second wave in digital format.

[8]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 the administrator's advice in only offering the paper survey.

While there is precedent eliciting beliefs about peer or school quality in isolation (Ainsworth et al., 2020), there are substantial hurdles in eliciting beliefs about each jointly. Effective messaging that succinctly explains the differences between peer and school quality is challenging to produce. I addressed these belief elicitation challenges in two ways. First, focus groups with LAUSD parents were conducted along with piloting different messages on Amazon MTurk (see Appendix Section A for summary statistics from the piloting). The results from the pilot were mimicked during focus group discussions. Extensive piloting suggested IA as the most effective term for peer quality and AG as the most effective term for school quality. The term IA aims to signal that it is a measure of peer quality that is less associated with incumbent school inputs as it is captured as students enter the school. In contrast, AG clearly signals that it is a measure of students' academic progress occurring during their tenure at the school. This choice of messaging avoids having to use terminology such as value-added, which is arguably more challenging to describe, but still conveys the message that one measure is about growth and another is about a level.

Second, I complement the messaging decision with instructional videos that further aid families' understanding of the quality measures in the intervention and in the survey. The videos aim to provide visual descriptions of the differences between IA and AG and thus more clearly delineate the differences. The paper surveys contained a QR code linking respondents to the video, while the digital version contained an embedded version right before respondents were asked about beliefs. Figure 1 displays some relevant frames from the two-minute video.

Frame (a) conveys that the video was produced in collaboration with the ZOC and the LAUSD, and frame (b) introduces the two terms IA and AG. Frame (c) associates IA with a measure that captures achievement as students enter school and is aided by a graphic showing students entering a school. Frame (d) associates AG with a dynamic measure happening during a students' tenure at the school and is aided by a graphic depicting student progress. Frame (e) succinctly highlights the differences between each and is agnostic about nudging families in any direction, and frame (f) highlights that families should also consider other non-test-score-based school attributes. The combination of the messaging and the instructional video helps families understand the objectively different measures of peer and school quality that the survey aims to elicit beliefs about.

Defining School and Peer Quality

Our measures of school and peer quality are conceptually tied to a constant effects potential outcome model of achievement. IA is calculated as the implied peer quality estimates derived from a model described in Appendix C, and AG is the estimated school value-added from the same model. Given the lack of quasi-experimental variation in school assignment, the model is estimated via ordinary least squares. Campos and Kearns (2022) find that school quality is forecast unbiased in Los Angeles, and I report similar findings in Appendix C. I convert each quality measure to its percentile rank among all other LAUSD schools. With these measures, I can construct the various versions of the zone-specific treatment letters and serve as a benchmark to the beliefs elicited in the baseline survey.

Randomization

The randomization strategy is designed to answer two questions: how responsive parents' choices are to different measures of school quality, and how important social interactions are in the school choice process. To answer the latter question, I employ a two-stage randomization procedure used to study spillovers (Andrabi et al., 2020, Crépon et al., 2013). The key feature of spillover designs is that there are control group participants in close proximity to other treated participants, who researchers can compare to control group participants without potential exposure to other treated participants. Any treatment effects are due to treatment effect spillovers, which in this setting amounts to social interactions generating a diffusion of information to untreated parents. To answer the first question, I cross-randomize information about peer and school quality.

The randomization process occurs within separate ZOC markets or zones, with the first randomization layer occurring at the school level and the second at the individual level. Each zone is considered a separate market and has different middle schools that feed into the zone.[9] 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.[10]

The first stage of the randomization assigns each group of feeder middle schools into either a high-saturation, low-saturation, or pure control school. The saturation level indicates the share of parents receiving information about a given measure of information, where high corresponds to 50% and low corresponds to 30%. In this respect, there are market-specific school-level experiments with two treatments, H and L.

Within each treated school and conditional on their assigned saturation level, the second randomization layer cross-randomizes the different information treatments. The individual-level randomization coupled with the school-level experiment helps to identify intent-to-treat effects for households directly receiving information and for households indirectly receiving information (a spillover effect) by comparing treated households (direct and indirect) to households in the pure control school, where no one received any information.[11]

Figure 2 provides a visual representation for the experiment in the Bell Zone of Choice. Elizabeth Middle School (MS) is randomly assigned to high saturation (treatment H), where π^h share of households receive each treatment, and Ochoa MS is assigned to low saturation. Nimitz is the pure control school, 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 treatment status is identified relative to households in the pure control school.

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

[10] Not all zones have three feeder middle schools, so I create blocks based on the 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, and therefore the control group is larger than the treatment groups by design.

[11] Feeder school enrollment is mostly neighborhood based, so it is unlikely that treatments within a zone to the pure control school are contaminated. Treatment being at the school level mostly ensures that any neighborhood interactions occur between middle school parents with children enrolled in the same school.

Treatment Letters

Families with children enrolled in either high- or low-saturation treatment schools can potentially receive treatment letters. Following decisions determining terms in the survey, I refer to peer quality as IA and value-added as AG. Some treated families receive information about IA, others receive AG, and some receive both.

Figure 3 displays example treatment letters for the Bell Zone of Choice in both English and Spanish. The design of the letters is similar to other studies (Corcoran et al., 2018, Hastings and Weinstein, 2008). At the top of each letter is a brief description of what it contains, followed by a list of schools corresponding to a recipient's particular zone. A key difference in these treatment letters from the 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. There are two other versions of the letters not displayed in Figure 3 that are identical but just report information about one measure of quality.

Data

The data for this paper come from the LAUSD and the ZOC office. 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 ZOC, including student rank-ordered lists and applications. The experimental sample contains students residing within ZOC boundaries and attending a feeder middle school during the eighth grade. In 2019, there are 13,015 students meeting this requirement and slightly fewer in 2021.[12] These students are not a random sample from the LAUSD population.

Table 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. This student is entering high school performing roughly 21%–25% of a standard deviation more poorly on math and reading scores than the typical non-ZOC student. Roughly 6% of ZOC parents have earned a four-year degree, and 97% of ZOC students are classified as poor. They are also more likely to be classified as English learners. In addition to these socioeconomic differences, there are vast racial and ethnic differences. Eighty-six percent of rising ZOC students are classified as Hispanic compared to 68% elsewhere in the district. The approximate racial and socioeconomic homogeneity of ZOC students was similar for past cohorts studied in Campos and Kearns (2022). While these students are notably different from the LAUSD population, treatment assignment occurs within the experimental sample.

Balance

Table 2 reports balance for the school-level randomization. Across 52 feeder middle schools, 16 get randomly assigned to the low-saturation treatment, 16 get randomly assigned to the high-saturation treatment, and 20 remain as pure control schools. There are minimal differences between treated and pure control schools across an array of school attributes, including

[12] These counts correspond to assignments made just before the semester starts. Some students may switch schools after that, but attrition or changes are minimal.

achievement and various demographic characteristics. Special education status is a notable omission that is not balanced, but joint tests fail to reject the null hypothesis pointing to an imbalance by chance.

Table 3 reports balance for the student-level randomization conditional on saturation status. These balance checks are limited to the sample of low- and high-saturation status schools as pure control schools do not contain any treated families. Mirroring the school-level balance checks, the randomization procedure produces a balanced sample across an array of student baseline outcomes and characteristics, including achievement and demographic characteristics.

Both tables point to the success of the randomization process. Throughout the analysis, however, I still control for the reported baseline covariates to increase precision in the estimates.

5 Reduced-Form Results

The experiment has two layers of randomization. I first focus on the school-level experiment, documenting the first piece of indirect evidence of spillovers. I then directly study social interactions, leveraging the experiment's full design. The reduced-form results do not take into account the survey results and are in similar spirit to previous information interventions (Corcoran et al., 2018, Hastings and Weinstein, 2008). The survey results discussed in Section 6 provide a different and corroborating perspective on the treatment effects discussed in this section.

5.1 School-Level Experiment

The first layer of the experiment randomizes schools to treatment statuses that vary by saturation level. School-level experiments are common in the literature as concerns over spillovers within schools are plausible. The analysis starts with the initial layer, aggregating treatments within schools to a single school-level treatment.

An advantage of the school-level experiment is that there is scope for natural, and convenient, falsification tests that implicitly test for balance on pre-intervention trends in outcomes of interest. To do this, I organize the empirical analysis in a difference-in-differences model that compares changes in student outcomes in treated and untreated schools over time. For a given outcome, the specification is

$$Y_i = \alpha_{z(i)t(i)} + \alpha_{g(i)} + \sum_{k\neq -1} \left(\beta_{Lk} D_{L(i)} \times Post_{k(i)} + \beta_{Hk} D_{H(i)} \times Post_{k(i)} \right) + u_i, \tag{12}$$

where α_{zt} are zone-by-year effects, α_g are treatment group fixed effects, $D_{L(i)}$ and $D_{H(i)}$ are low- and high-saturation treatment indicators, and $Post_{k(i)} = \mathbf{1}\{t(i) - 2019 = k\}$. The β_{Lk} and β_{Hk} terms capture difference-in-difference estimates relative to the year before the first wave (2019) for low- and high-saturation groups, respectively. Both are identified by comparing changes in application behavior between applicants in the respective groups and applicants in pure control schools. This approach provides improvements in precision relative to the static experimental specification.[13] Standard errors are robust and clustered at the school level.

[13]The static results are reported in Appendix D and are similar but less precise.

Figure 4 reports estimates of Equation 12, considering top-ranked school IA and AG as outcomes. The gray lines correspond to estimates for impacts on the AG of top-ranked schools, and the blue lines correspond to impact estimates on the IA of top-ranked schools. The solid lines correspond to low-saturation treatment effects, i.e., β_{Lk}, and dashed lines correspond to high-saturation treatment effects, i.e., β_{Hk}, all identified from comparisons with pure control schools.

To begin, Figure 4 reports null treatment effects during pre-intervention periods, a reassuring finding and further emphasizing the success of the randomization strategy. The blue lines reveal that the treatment effects on top-ranked school IA are minimal and indistinguishable from statistical noise for both low- and high-saturation treatment schools. In contrast, the gray lines show that the treatment effects on top-ranked school AG are positive, ranging between 4.8 and 8.8 percentile rank points. The positive treatment effects are only present for high-saturation schools; this is the first finding alluding to the importance of social interactions, which I provide direct evidence for in the next section. The findings in Figure 4 are limited to only top-ranked options, and the effects may vary at different points of the rank-ordered list.

Figure 5 reports effects across the entire rank-ordered list. Panel (a) reports 2019 effects, and Panel (b) reports 2021 effects. I aggregate all options at the fifth position of the list or greater into a single group but otherwise report rank-specific effects. Treatment effects for the first option replicate the findings reported in Figure 4. Common to both years is the finding of effects mostly among the highly saturated group, limited to just effects on school AG, and they are more pronounced at the top of the list. The effects are larger and more persistent for the 2021 wave, with measured and significant effects through the fourth-ranked option ranging between 5 and 9 AG percentile points. This finding reveals that families in highly saturated areas systematically choose schools with higher AG scores while not compromising their school IA scores.

It is worth emphasizing a few points about these findings. First, the average response to being in an environment with information, regardless of the individual treatment status, is to choose schools with higher AG measures (higher value-added). These average effects include a mixture of treatment effects, including quality-specific treatments and spillover effects the experiment is designed to detect. Despite this, the findings suggest that when information about both school and peer quality is distributed in a market, demand will change in a way that rewards effective schools. Thus, one of the prevailing hypotheses about information frictions producing a relative preference for peer over school quality holds some merit (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020, Rothstein, 2006), as this market-level experiment addresses the information friction and finds relative demand for school quality increases.

Second, and perhaps not surprisingly, treatment effects depend on the information's prevalence. In markets where fewer parents have direct access to any information, changes in demand were minimal, but the opposite was true in markets where most parents have access to some information. This finding is consistent with anecdotes and mounting qualitative evidence about the importance of word-of-mouth and social networks in school choice environments (CITE). The saturation-specific effects provide the first finding alluding to the importance of social interactions in generating meaningful changes in demand, a finding I complement in the next section by estimating spillover effects directly and that also vary by saturation status.

Table 4 reports treatment effects on top-ranked school attributes that are potentially correlated with school IA and AG. The first two rows report the estimates displayed in Figure 4, and the subsequent rows consider treatment effects on an array of most-preferred school characteristics. Perhaps not surprisingly, the table finds minimal evidence that changes in demand for AG substantially alter other attributes of most-preferred schools. This finding is consistent with minimal impacts on school IA since it, to a close approximation, serves as a summary index for all other row variables and other evidence finding that school value-added tends to be weakly correlated with school observable characteristics.

5.2 Social Interactions

The findings reported in Figure 4 and Table 4 aggregate treatment to the school level, masking the fact that families were treated with different measures of school quality within schools and some did not receive anything. This design feature allows one to directly estimate spillover effects by comparing untreated parents in treated schools to untreated parents in untreated schools. It also allows one to compare how sensitive choices are to different measures of quality and, in particular, to provide evidence about how relative demand for school quality changes in response to various treatments.

The complete specification is

$$\begin{aligned} Y_i = \alpha_z &+ \underbrace{\beta_{Ph} T_i^P \times D_{s(i)}^h + \beta_{Sh} T_i^S \times D_{s(i)}^h + \beta_{Bh} T_i^B \times D_{s(i)}^h}_{High\ Saturation\ Effects} \\ &+ \underbrace{\beta_{P\ell} T_i^P \times D_{s(i)}^\ell + \beta_{S\ell} T_i^S \times D_{s(i)}^\ell + \beta_{B\ell} T_i^B \times D_{s(i)}^\ell}_{Low\ Saturation\ Effects} \\ &+ \underbrace{\beta_h C_i \times D_{s(i)}^h + \beta_\ell C_i \times D_{s(i)}^\ell}_{Spillover\ Effects} + u_i, \end{aligned} \tag{13}$$

where α_z is a zone fixed-effect (or randomization block), T_i^x are individual-level treatment x indicators for $x \in \{P, S, B\}$, $D_{s(i)}^x$ are school-level treatment indicators, and C_i are individual-level indicators for untreated parents. The specification contains a total of eight saturation-specific parameters of interest. β_{xh} and $\beta_{x\ell}$ are treatment $x \in \{P, S, B\}$ effects for high- and low-saturation groups, respectively, and β_h and β_ℓ are saturation-specific spillover effects. All parameters are identified with comparisons to families in pure control schools. This design is an multiple treatment extension of other work studying spillover effects across a variety of domains (Andrabi et al., 2020, Crépon et al., 2013). Standard errors are robust and clustered at the school level.

Appendix Table D.2 reports estimates for the 2021 wave, and Appendix D reports additional estimates for the 2019 wave. Column 1 reports effects on most-preferred school AG, and Column 2 reports effects on most-preferred IA. Each column reports estimates for the eight parameters from the full specification. Effect sizes tend to be similar within saturation group. For example, I cannot reject that most-preferred AG impacts are the same for those in the high-saturation treatment arm regardless of being directly treated or in the spillover group. The same is true for most-preferred IA. The evidence in Appendix Table AD.2, albeit noisy and imprecise, suggests that families systematically chose schools with higher AG scores and without a salient or corresponding change in their most-preferred school IA scores.

The mean impacts mask considerable heterogeneity in impacts across the numerous distinct markets with different choice sets families face. For example, parents have more scope to change their preferences for AG in markets with higher cross-school variance in AG. Alternatively, in markets with schools with high AG and IA scores, there is less scope to find treatment effects due to ceiling effects. Figure 6 reports a series of distributional impacts. Based on the evidence in Appendix Table D.2, Panels (a) and (b) aggregate treatments by type of information received, eliminating the saturation-level variation in treatment. Based on the previous evidence, Panels (c) and (d) aggregate treatment by saturation status. The aggregation provides some efficiency gains and distinct perspectives on how demand for most-preferred schools changed in response to the various treatments.

Panel (a) begins by demonstrating impacts across the most-preferred school IA. A point reveals the direction and magnitude the cumulative distribution function shifted, so for example, at 40, the probability that a most-preferred school IA ranking was below the 40th percentile decreased by approximately 8 percentage points for the families receiving AG. Treatment effects are remarkably similar across the various treatment groups, including the spillover group. Overall, families tended to shift their most-preferred school choices to schools with lower IA, with much less pronounced changes in markets with high IA schools. While Panel (a) detects that families shifted their choices toward schools with lower IA, these changes are complemented with increased demand for higher AG schools as Panel (b) demonstrates. Similar to impacts on most-preferred IA, treatment effects of untreated parents in treated schools mirror the effects of treated parents. The striking visual evidence in Panels (a) and (b) suggest a community-level convergence moving average demand in a way that rewarded effective schools.

Panels (c) and (d) report saturation-specific effects, mirroring the evidence for the school-level experiments reported in Section 5.1. Treatment effects for families in highly saturated schools are more pronounced than those in less saturated schools. The saturation heterogeneity in treatment effects further corroborates the role of social interactions. First, spillovers effects are identical to that of treated parents. Second, the saturation heterogeneity indicates that a sufficiently large mass of parents need to discuss the information to produce more salient and pronounced changes in demand. So while spillovers may be present, a community-level effort is needed to produce more meaningful changes in demand for effective schools regardless of the saturation level.

The assemblage of evidence reported in the previous sections can be summarized with a few key points. First, imperfect information about school effectiveness is empirically relevant as families changed their choices in response to the intervention. This has been highlighted in Ainsworth et al. (2020) and alluded to in prior research (Abdulkadiroğlu et al., 2020, Beuermann et al., 2022, Rothstein, 2006). Second, and in contrast to previous research, I show that when information about both peer and school quality are prevalent, families systematically choose more effective schools without meaningful average changes on their most-preferred school peer quality. This evidence suggests that effectiveness-oriented campaigns can orient demand in a way that parents reward effective schools, with implications for school competition and student outcomes in the long run (Abdulkadiroğlu et al., 2020, Campos and Kearns, 2022). Third and last, the reduced-form results reveal that social interactions, that is, parents discussing the information among themselves, are important determinants for own-school choices. This evidence is compounded by the finding that in settings where information was not as widely available, changes in demand were less pronounced. The existing literature thus far has provided anecdotes and qualitative evidence about the importance of networks (Fong, 2019, Kosunen and Rivière, 2018); this is the first evidence documenting social interactions matter for individual choices.[14]

The reduced-form results thus far show how choices changed on average and cannot speak to the factors influencing the changes in choices. As discussed in the conceptual framework, some of the changes are due to information updating, while others are due to actual preference changes or salience impacts. In the next section, I add some structure to the choice process and combine that with survey data to shed light on the various factors governing the treatment effects.

6 Survey Results and Decomposition

The baseline survey elicited baseline preferences and beliefs about school and peer quality. In addition, there were other questions that revealed information about parents' intentions during the school choice process, which are discussed in detail in Appendix B. In this section, I first focus on descriptive evidence on elicited preferences and beliefs. I then return to the experiment, combining the survey results with a slightly more structural approach to shed light on the various factors contributing to the treatment effects.

Throughout, biases are defined in terms of pessimism. Let Q_j^x be the measured quality of school j along measure $x \in \{IA, AG\}$, and define parent i's belief as $\tilde{Q}_{ji}^x$. The biases are

$$Bias_{ji}^x \equiv Q_j^x - \tilde{Q}_{ji}^x.$$

6.1 Descriptive Evidence

Figure 7 reports a histogram of elicited pessimism for both IA and AG. On average, parents are pessimistic about school AG but are slightly optimistic about school IA. While roughly 50% of parents are pessimistic about AG, only 34% are pessimistic about IA. These patterns are not a consequence of center guessing; Appendix Figure B.2 reports the overlap in estimated deciles and elicited belief deciles. The figure shows substantial overlap between AG beliefs and measured AG, and to a lesser extent the same is true for IA, with both findings indicating that

[14]It is important to contrast social interactions defined in this paper from preferences for peers studied in previous papers (Allende, 2019). While preferences for peers are a form of social interaction in the sense that my demand for an option depends on the composition of students, the findings in this paper are conceptually different. The evidence in this paper compares how actual choices change in response to the information availability of nearby peers, irrespective of the demand for peers. In fact, I find that preferences for peers tend to not be too important in these markets, which is partly explained by the relatively segregated markets in terms of race and income.

elicited beliefs carry some signal. As this is the first finding in the literature regarding beliefs about both of these measures, it is worth reporting some additional patterns about beliefs.

The pessimism patterns documented in Figure 7 hold across most of the entire rank-ordered list. Figure 8 reports average pessimism across each position of the rank-ordered list. There are four findings that immediately stand out. Throughout the list, parents are more pessimistic about AG than they are about IA. They also get progressively more pessimistic about schools they rank farther down their list, and the patterns is slightly more pronounced for AG. For top-ranked options, parents tend to be optimistic about both IA and AG. They are optimistic about IA across the entire list, while AG optimism shifts toward pessimism at the third-ranked option.

To explore potential differences in beliefs by relative advantage, I use baseline achievement as a summary measure. Figure B.4 reports the relationship between pessimism and students' baseline achievement. Panel (a) reports the relationship for all options, and Panel (b) focuses on the top-ranked option. Perhaps surprisingly, both panels indicate a lack of a relationship between AG pessimism and students' baseline achievement. In contrast, there is a modest achievement gradient for IA, indicating that higher-achieving families have beliefs that are closer to the truth. The latter finding may not be surprising as there are numerous publicly available sources reporting measures similar to IA, and more-resourced families likely access this information at a higher prevalence.

Table 5 reports additional correlations between biases and student baseline covariates.[15] Columns 1 and 3 report estimates from a series of bivariate regressions, while Columns 2 and 4 report estimates from a single multivariate regression. Focusing on IA first, college-educated parents tend to be more pessimistic about IA both unconditionally and conditional on other covariates, mirroring the correlation between pessimism and baseline achievement. Ethnic/racial differences in IA pessimism do not hold conditional on other student covariates. Parents with students classified as impoverished tend to be optimistic about IA. Turning to AG pessimism, few student characteristics correlate with it. Hispanic families tend to be the most pessimistic about AG, and few other covariates stand out with meaningful differences.

6.2 Beliefs versus Researcher-Estimated Quality

With elicited beliefs and baseline rank-ordered lists, one can assess how families trade off IA and AG beliefs in comparison to estimated IA and AG. This exercise is similar to other literatures that estimate preferences using revealed preferences (Abdulkadiroğlu et al., 2020, Beuermann et al., 2022), with a key difference being that I can also relate choices to beliefs about the quality measures (Ainsworth et al., 2020).

In this descriptive exercise, I can relate baseline choices to beliefs and researcher-estimated quality measures. In particular, the indirect utility of school j for student i is

$$U_{ij} = \gamma_P Q_j^P + \gamma_S Q_j^S - \lambda d_{ij} + \varepsilon_{ij},$$

where Q_j^P and Q_j^S are estimated school and peer quality. I can compare this model to one where

[15]The emphasis in Table 5 is on the top-ranked option as that contains the most signal. Appendix B contains a similar table for all options on the rank-ordered list.

families trade off beliefs about Q_j^P and Q_j^S:

$$\tilde{U}_{ij} = \tilde{\gamma}_P \tilde{Q}_{ij}^P + \tilde{\gamma}_S \tilde{Q}_{ji}^S - \tilde{\lambda} d_{ij} + \tilde{\varepsilon}_{ij}.$$

Comparing the estimates of $\frac{\tilde{\gamma}_S}{\tilde{\gamma}_P}$ to $\frac{\gamma_P}{\gamma_S}$ tell us how families differentially trade off school and peer quality when using their actual beliefs and when using researcher-generated measures of quality that are common in the literature.

Appendix Table B.5 reports estimates from these exercises. Columns 1 and 2 correspond to estimates using researcher-estimated IA and AG, while Columns 3 and 4 correspond to estimates using elicited beliefs. Given the discrete nature of the survey-elicited beliefs, each model bins quality deciles, omitting the worse category in the support of a given measure. Columns 3 and 4 show that families trade off IA and AG beliefs at similar rates and prefer larger values of each. Interestingly, many families report not knowing about some schools in their choice set, and they tend to have a preference for those over schools in the bottom decile of the IA and AG distribution. In other words, they are willing to take on some uncertainty over school quality as opposed to enrolling their children in a school they believe to be the worst quality. In comparison to the estimates in Columns 1 and 2, the implied willingness to travel measures are significantly larger in the beliefs model. This suggests that existing estimates in the literature using researcher-estimated quality measures potentially misstate preferences.

It is important to note that the survey results come from a self-selected group of families that chose to participate and share their beliefs. The students in these families who responded to the survey are performing 0.07σ below the district average; in comparison all survey recipients are performing 0.14σ lower than the average student in the district. It is also worth mentioning that the families that responded to the survey are more likely to have parents with a college education. Despite these differences, the differences between the students in these families and other students in the district are not too stark.[16]

6.3 Impacts on Preferences

The beliefs data can now be incorporated to decompose treatment effects on utility weights in random utility models. The pivot to this framework allows me to leverage information contained in the entire rank-ordered list and to zoom in on how families trade off school and peer quality across the entire list.

This analysis allows me to naturally quantify the implied changes in families' willingness to travel for the two quality measures considered. To do so, I depart from Equation 1, where I assume that school mean utility is summarized by schools' IA and AG rankings:

$$\delta_j = \gamma_P Q_j^P + \gamma_S Q_j^S.$$

While this assumption is arguably implausible and also relaxed in the empirical analysis, the focus is on the γ_P and γ_S change in response to the various treatments introduced by the intervention. In particular, families' indirect utility can be expressed as

[16] Appendix Table B.1 reports the differences between survey respondents and non-respondents.

$$U_{ij} = \delta_j - \lambda d_{ij} + \sum_{t \in \{P,S,B,C_h,C_L\}} (\beta_{tP} Q_j^P \times \mathbf{1}\{T_i = t\} + \beta_{tS} Q_j^S \times \mathbf{1}\{T_i = t\}) + \varepsilon_{ij}, \tag{14}$$

where T_i is the treatment group that i is assigned to. β_{tP} and β_{tS} measure how much utility weights assigned to IA and AG, respectively, change for the various treatment groups t. Scaling them by λ measures the change in the willingness to travel. The focal point of this analysis will center around how the treatments change the willingness to travel for the two quality measures.

β_{tP} and β_{tS} reflect changes in utility weights but nest a mixture of influences discussed in Section 3. On the one hand, the presence of biases implies families are incorrectly trading off IA and AG, so information will lead to changes in the weights simply through information updating. The correlated nature of beliefs and quality also contributes to weight changes. Distinguishing between the various factors is important in aiding the interpretation of how the information intervention affects preferences (or salience) vis-à-vis information updating.

The decomposition implied by the distributional assumptions on beliefs and quality implies that for families only receiving the peer quality treatment,

$$\widehat{\beta}_{PP} = \beta_{PP} - \mu_P \gamma_P - \frac{\sigma_{Pb}}{\sigma_{Sb}} \rho_B \mu_S \rho_Q \frac{\sigma_P}{\sigma_S} \gamma_S. \tag{15}$$

The decomposition of depends on the variances σ_P and σ_S, belief and quality correlations ρ_B and ρ_Q, all estimated from the survey, and utility weights of the control group. Families receiving only school quality treatment have an analogous decomposition:

$$\widehat{\beta}_{SS} = \beta_{SS} - \mu_S \gamma_S - \frac{\sigma_{Sb}}{\sigma_{Pb}} \rho_B \mu_P \rho_Q \frac{\sigma_S}{\sigma_P} \gamma_P. \tag{16}$$

Last, for families receiving both treatments,

$$\widehat{\beta}_{tX} = \beta_{BX} - \mu_X \gamma_X, \tag{17}$$

where X corresponds to either P or S. The treatment effects for families in the spillover group will be attenuated by the corresponding saturation share, as depicted in Section 3. For families receiving just one treatment, the change in their choices is a confluence of changes in their preferences γ_{tX} (also interpreted as salience effects), information updating $\mu_X \gamma_X$, and how their choices for the other quality measures change due to the fact that their beliefs are correlated $\frac{\sigma_X}{\sigma_Y} \rho_B \mu_Y \gamma_Y$. In the case families receive both treatments, the correlated beliefs channel is eliminated, and thus their decision changes reflect salience/preference impacts and information updating. The decompositions allow me to provide crude measures of how much each measure contributes to the implied changes in families' willingness to travel for each of the quality measures. Although the decomposition is intended for quality-specific treatments, I also report decomposition estimates for saturation-specific effects. Standard errors are clustered at the school level and estimated via the delta method where appropriate.

Table 6 reports estimates for a variety of models; all are willingness to travel estimates except for distance coefficients.[17] Panel A considers saturation-specific effects, while Panel B considers quality-specific effects. The first two columns, labeled without school effects, report estimates from a model that parameterizes school mean utilities in terms of IA and AG only, and the third and fourth columns report estimates with school effects.

The first two columns of Panel A show that untreated families tend to place positive weight on IA and AG, with a higher weight on AG. This finding mirrors previous findings documented for earlier ZOC cohorts in Campos and Kearns (2022) but stands in contrast to findings in New York found by Abdulkadiroğlu et al. (2020) and in Romania by Ainsworth et al. (2020). It is likely that conditions affecting the school choice process vary across settings and explain the diverse findings. For example, in ZOC markets, there is much less pronounced variation in race and socioeconomic status, potentially reducing the weight families place on peer quality, which is in part a proxy for socioeconomic status.

The subsequent two rows of Panel A show that families receiving information reduce their willingness to travel for IA and increase their willingness to travel for AG regardless of the saturation level. The effects are larger for those in highly saturated areas than those in less saturated areas. Mirroring the reduced-form results, I find appreciable spillover effects, with their magnitudes closely matching saturation-specific treatment effects. Families in treated schools are willing to travel anywhere between 0.25 and 0.62 additional miles to enroll their children in schools with 10 percentile point higher AG scores, all else equal. The magnitudes are sensible given the dense nature of ZOC markets, where families choice sets mostly consider of nearby schools. Columns 3 and 4 report effects for models that include school effects, absorbing weights for untreated parents. The findings are qualitatively similar with one key difference: low-saturation effects on willingness to travel for AG are less pronounced. Overall, the findings demonstrate that families' choices responded in a way that rewards effective schools, a crucial channel governing the success of school choice policies.

Panel B changes the aggregation of treatment to the information level, aggregating across saturation status. Columns 1 and 2 point to similar findings as in Panel A; families increase their willingness to travel for AG and reduce their willingness to travel for IA, with sizable spillover effects. Columns 3 and 4 find qualitatively similar evidence though estimated with more error. Overall, however, the findings across Panels A and B point to the same qualitative conclusion that the information intervention led to choices that increased schools incentives to invest in inputs that contribute to student learning.

Although the findings discussed above reflect policy-relevant effects measuring how choices change on average, there are a variety of forces at play. Some families' measured weights on one quality measure will change due to information updating, causing, as a consequence, their weights on the other quality measure to change through correlated beliefs. On the other hand, some families may simply change their preferences or rethink the weight they assign to the various attributes upon receiving information. More generally, the decomposition is informative about information provision effects and how the channels through which they affect policy and outcomes. If the effects mostly reflect information updating, then that emphasizes current

[17] A willingness to travel parameter is estimated by taking the ratio of an estimated utility weight and the distance coefficient. Standard errors are estimated via the delta method.

allocative inefficiencies due to imperfect information. In contrast, if the effects are mostly due to salience, then this emphasizes the potential role that information campaigns play in affecting school incentives and outcomes, such as achievement and segregation patterns.

Figure 9 reports estimates of various decompositions. Panels (a) and (b) report decompositions of $\beta\gamma_P$ and $\beta\gamma_S$ estimated from models that aggregate treatments at the school saturation level. Panel (a) reports weight treatment effect impacts among those directly treated, and Panel (b) reports weight treatment effect impacts for those indirectly treated. The first three bars in each figure correspond to AG weight treatment effect impacts, while the subsequent three bars correspond to IA weight treatment effect impacts. The information-updating and correlated beliefs channels bias the estimated coefficients, with the magnitudes and directions depending on moments of the belief distribution.

Overall, the takeaway from Figure 9 is that salience explains most of the changes in choices. In other words, the information reoriented how families thought about school and peer quality above and beyond information-updating channels alone. In fact, information updating proves to correspond to a small share of the overall changes. This latter finding is a consequence of families' beliefs not being too far off from the truth on average.

7 Discussion

The assorted set of results have three broad implications. The first relates to our understanding of parents' preferences and what we can and cannot learn from this intervention. The second relates to the implications of social interactions for educational inequality and access to effective schools. The third relates to the role of salience effects in information interventions more broadly. I discuss each now in turn.

The evidence in this paper shows that when both peer and school quality is widely available, any measurable changes in demand are oriented toward higher value-added schools. While the evidence in this paper demonstrates that families may systematically prefer school quality over peer quality, there are a few additional points worth addressing. My findings do not speak to whether or not families "max" out on school effectiveness (Ainsworth et al., 2020). The multidimensional nature of a school's production function makes it plausible that families need not maximize only school effectiveness (Beuermann et al., 2022). The findings suggest that effectiveness-oriented information campaigns can reorient demand in a way that compels schools to invest in inputs that contribute to student learning *and* that parents may prefer this information over other information that mostly reflects student selection. This type of demand-side behavior may motivate active school quality-based information campaigns that can potentially improve student outcomes.

Measurable changes in demand were facilitated through social interactions, which has implications for how we interpret observed demand in general. To begin, the spillover results provide evidence of an externality in school choice that is distinct from a preference for peers that has received much attention in the empirical (Allende, 2019, Rothstein, 2006) and theoretical literature (Cox et al., 2021, Leshno, 2021). Consequently, the prevalence (or lack) of social interactions can mediate observed differences in preferences and contribute to differences in access to effective schools as differentially connected families learn at different rates (Golub and Sadler, 2017).

In particular, my findings demonstrate that the preference for peers may be attenuated in settings with widespread school quality information and that demand externalities may operate through information acquisition *before* centralized matches occur and become less dependent on assignments. This pivots the discussion to the endogenous information acquisition stage (Chen and He, 2021, Harless and Manjunath, 2015, Immorlica et al., 2020, Maxey, 2021) and emphasizes network-based externalities. For example, if parents' information sets are shaped by their networks, then common findings that disadvantaged families have a lower taste for academic quality (Hastings et al., 2006) or less take-up of information (Cohodes et al., 2022, Corcoran et al., 2018, Finkelstein and Notowidigdo, 2019) can be potentially explained by biased networks among the disadvantaged.[18] Information campaigns that further motivate interactions can potentially reduce existing school quality gaps.[19] Incorporating network-based preference externalities is a fruitful avenue for future theoretical and empirical research.

The beliefs data allowed me to shed light on factors influencing treatment effects, something information interventions are typically silent about (Haaland et al., 2020). The crude decomposition I provide demonstrates that information campaigns generate a meaningful share of treatment effects through salience, and perhaps to a lesser degree, information updating. Information interventions, however, are commonly motivated to allow consumers to make more informed decisions and reduce information gaps. The findings suggest that information interventions play a powerful role in shaping families' preferences and choices, above and beyond addressing information gaps that were present in ZOC markets. At one extremity, this suggests that information interventions can be used as tools to reorient demand in a way consistent with policymaker goals. For example, policymakers interested in successful school choice policies can make school-quality information widely available, serving a dual purpose of eliminating information gaps and reorienting demand to potentially improve student outcomes.

8 Conclusion

Parents' choices govern the success of school choice initiatives and it is paramount to understand both their preferences and factors that mediate their choices. This paper provides survey and experimental evidence about parents' beliefs and valuation of effective schools while also providing experimental evidence about a network-based externality in preference formation. The experimental evidence is the first attempt to depart from revealed preference arguments that implicitly assume perfect information (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2020, Beuermann et al., 2022, Campos and Kearns, 2022) and is the first to show the relevance of social interactions for preference formation discussed in a nascent theoretical literature (Harless

[18]In the ZOC setting, there was less scope for income- or race-based inequality in take-up, but there is suggestive evidence that the information intervention decreased achievement-based differences in preferences for effective schools (see Appendix D).

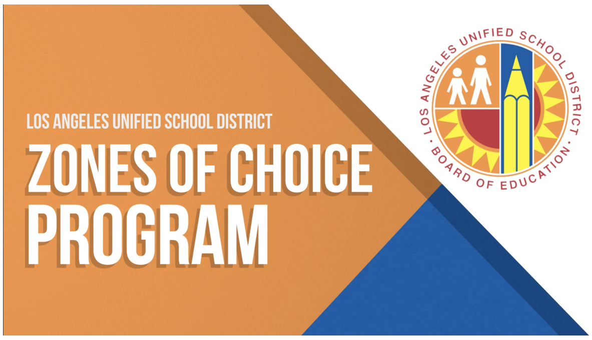
[19]Widespread effectiveness information campaigns potentially introduce some issues, however. For example, they can realign enrollment and have consequential effects on school segregation, as recent laboratory experiments have shown (Houston and Henig, 2021). Recent work by Angrist et al. (2022) also highlights some potential trade-offs.

and Manjunath, 2015, Immorlica et al., 2020, Maxey, 2021).

The survey results reveal that families are pessimistic about school quality and are optimistic about peer quality among schools in their neighborhood-specific choice sets. Information provision in these markets shows that families receiving any information increase their demand for schools of higher quality in a way that is consistent with them rewarding effective schools. Social interactions and spillovers are vital mediators governing new market-level consensus of desirable schools. A decomposition of my estimates suggests that salience or preference impacts account for most of the changes in choices, with information-updating and correlated beliefs playing smaller roles. These findings demonstrate that effectiveness-oriented campaigns can produce meaningful changes in demand and reorient school incentives in ways that generate positive outcomes for students.

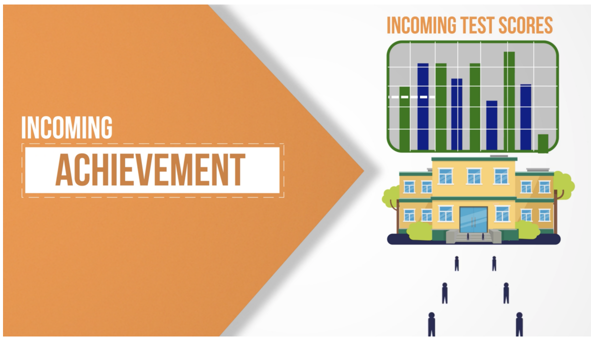
This paper advances what we know about parents' beliefs and preferences but is limited along certain dimensions. The results speak to short-run partial equilibrium effects, providing, at best, suggestive evidence for potential supply-side responses. Moreover, the findings are silent about how changes in demand can affect school segregation patterns, the role of congestion, and the importance of social networks in general equilibrium. These are all fruitful avenues for future research.

Figure 1: Video Frames



(a)

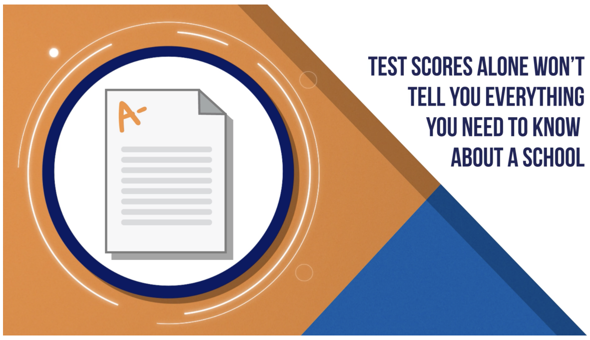
(b)



(c)

(d)

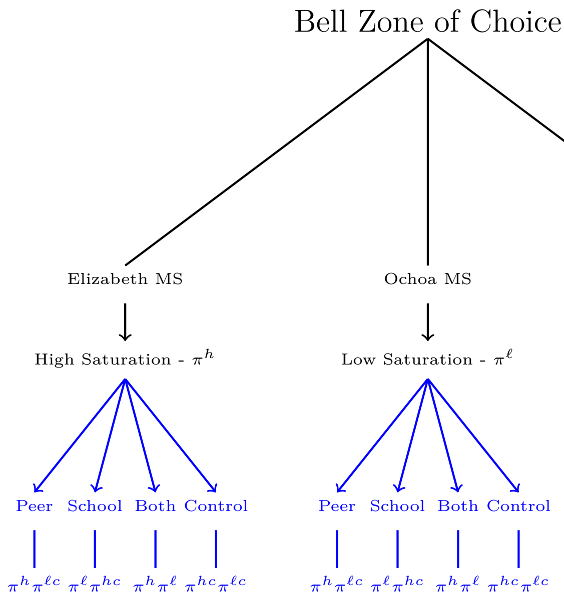
(e)



(f)

Notes: This figure displays six frames from the video distributed alongside the baseline survey. Frame (a) is the introduction slide, indicating that this message comes from the ZOC office and the LAUSD. The second frame introduces the two quality measures and juxtaposes them as distinct objects. Frame (c) provides some visualization indicating that incoming achievement captures student achievement at the time they enter school and thus are less affected by the school's inputs. Frame (d) depicts achievement growth as something dynamic and occurring during the students' tenure at the school. Frame (e) highlights some differences with the aim to be agnostic about which is better, and Frame (f) qualifies the information with a statement nudging families to also consider other non-test-score-based attributes.

Figure 2: Assignment to Treatment



Notes: This figure describes the randomization if the block size was three. There are certain zones with more than three 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 3: Treatment Letter Example: Bell Zone of Choice

We are providing information about schools within your Zone of Choice to ensure you have the best information available prior to your upcoming decision.

Bell Zone of Choice

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.

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. The table below provides each school's district-wide ranking.

Incoming Achievement

Incoming achievement is the average test scores of school's incoming students at the time they enter school.

Achievement Growth

We measure a school's ability improve test scores by measuring the growth of their students' test scores between entry into the school and eleventh grade.

We hope you use this information when choosing the right school for your student.

School	Incoming Achievement*	Achievement Growth*	Campus Location	Type of School
Science, Technology, Engineering, Arts & Math (STEAM) High School	76	94	Legacy HS	Small School
Visual & Performing Arts (VAPA) High School	74	67	Legacy HS	Small School
Health Academy	58	58	Elizabeth LC	Small Learning Community
Multilingual Teacher Academy	63	50	Bell HS	Linked Learning Academy
STEAM	47	82	Maywood Academy	Small Learning Community
Information Technology Academy	49	53	Elizabeth LC	Small Learning Community
Arts Language & Performance Humanities Academy	63	50	Bell HS	Linked Learning Academy
9thGrade Academy	47	82	Maywood Academy	Small Learning Community
Bell Global Studies	63	50	Bell HS	Small Learning Community

*Schools' Incoming Achievement and Achievement Growth 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. Similarly, if achievement growth is 75, then the school's ability to improve test scores is better than 75 percent of high schools in LAUSD.

Estamos proporcionando información sobre las escuelas dentro de su Zona de Opción, para asegurarnos de que tenga la mejor información disponible antes de su próxima decisión.

Zona de Opción Bell

Determinamos la calidad de una escuela en función de los puntajes promedio de los estudiantes en los exámenes estatales

Esta medida tiene dos partes que debe considerar, una que mide la capacidad de la escuela para atraer a estudiantes con altas calificaciones, y la segunda es el impacto de la escuela en el crecimiento de las calificaciones de las pruebas.

Por lo tanto, la calidad observada de una escuela es una combinación tanto del **rendimiento entrante** de sus estudiantes como del **crecimiento de logros** o **crecimiento del 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 logros. A continuación, proporcionamos la clasificación de cada escuela comparado a todas escuelas en el distrito.

Rendimiento Entrante

El rendimiento entrante de una escuela es el puntaje promedio de sus estudiantes cuando ingresan a la escuela.

Crecimiento de logros

Medimos la capacidad de una escuela para mejorar los puntajes de los exámenes midiendo el crecimiento de los puntajes de los exámenes de sus estudiantes entre el ingreso a la escuela y el onceavo grado.

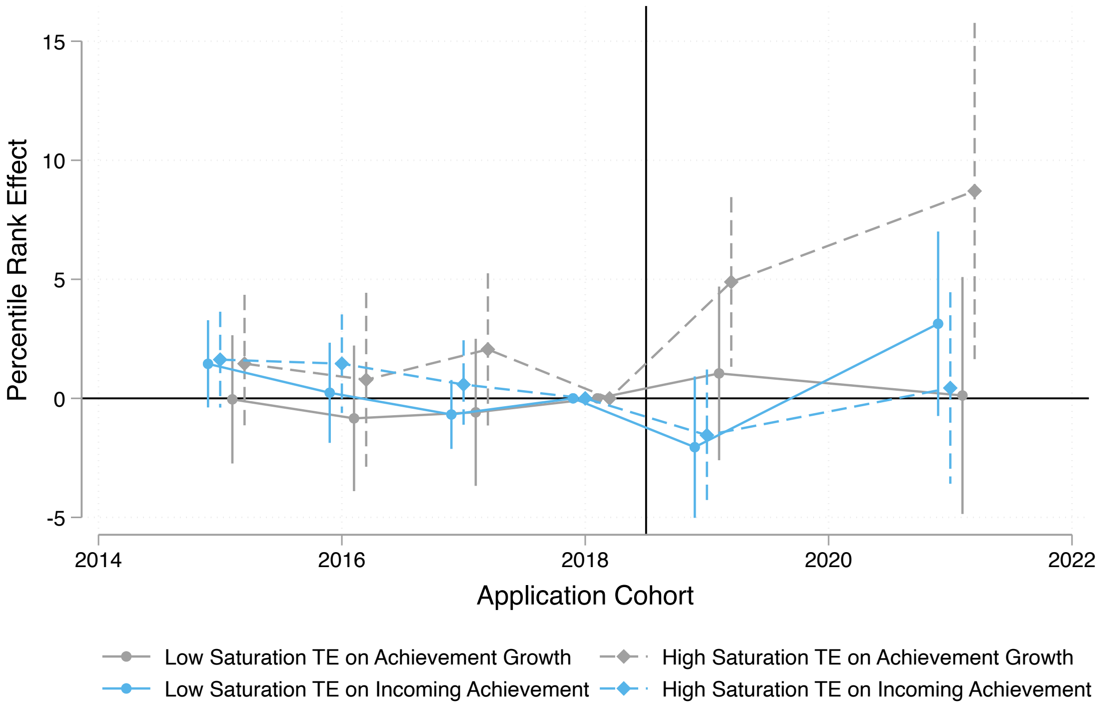
Esperamos que utilice esta información al elegir la escuela adecuada para su estudiante.

Escuela	Rendimiento Entrante*	Crecimiento de logros*	Ubicación del campus	Tipo de escuela
Preparatoria de Ciencia, Tecnología, Ingeniería, Artes y Matemáticas (STEAM)	76	94	Legacy HS	Escuela Pequeña
Preparatoria de Artes Visuales y Técnicas (VAPA)	74	67	Legacy HS	Escuela Pequeña
Academia de Salud	58	58	Elizabeth LC	Comunidad Educativa Pequeña (SLC)
Academia de Aprendizaje Enlazado/ Carrera de Profesores Multilingües	63	50	Bell HS	Academia de Aprendizaje Enlazado
Academia de Ciencia, Tecnología, Ingeniería, Artes y Matemáticas (STEAM)	47	82	Maywood Academy	Comunidad Educativa Pequeña (SLC)
Academia de Información Tecnológica	49	53	Elizabeth LC	Comunidad Educativa Pequeña (SLC)
Academia de Artes, Idiomas, Artes Escénicas y Humanidades	63	50	Bell HS	Academia de Aprendizaje Enlazado
Academia del 9º Grado	47	82	Maywood Academy	Comunidad Educativa Pequeña (SLC)
Estudios Globales	63	50	Bell HS	Comunidad Educativa Pequeña (SLC)

*El rendimiento entrante y el crecimiento del logros de las escuelas se proporcionan en percentiles. Por ejemplo, si una escuela tiene un rendimiento entrante de 55, esto significa que los puntajes promedio de las pruebas de sus estudiantes entrantes son mejores que el 55 porciento de otras escuelas secundarias en LAUSD. Del mismo modo, si el crecimiento de legros es 75, la capacidad de la escuela para mejorar los puntajes de las pruebas es mejor que el 75 porciento de las escuelas secundarias del LAUSD.

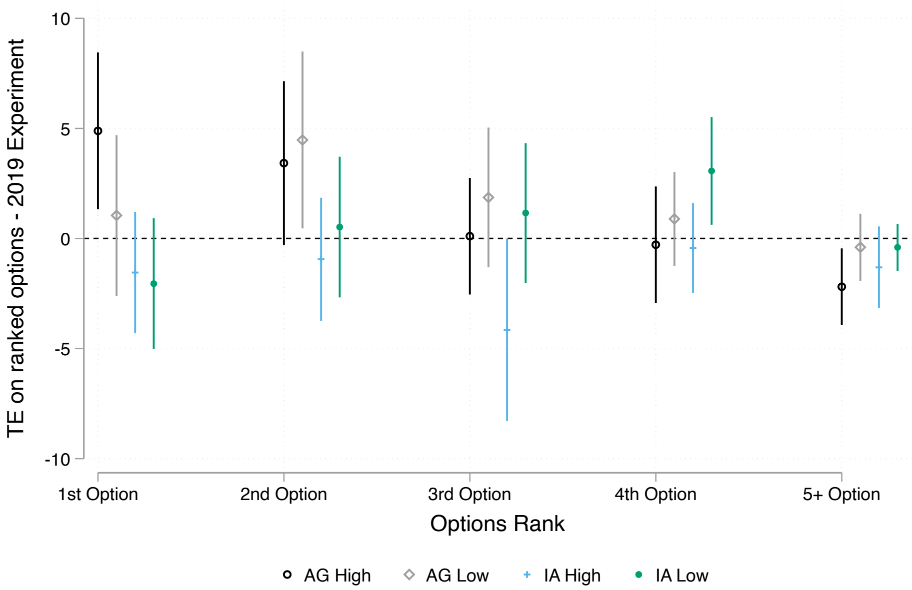
Notes: The front page of the letter is in English, while the second page is in Spanish. The letters list all the relevant schools for the Zone of Choice that the feeder middle school feeds into. The letters provide a simple and short explanation about incoming achievement and achievement growth, along with a quick summary with some graphics that aid in understanding the differences.

Figure 4: Difference-in-Difference Estimates

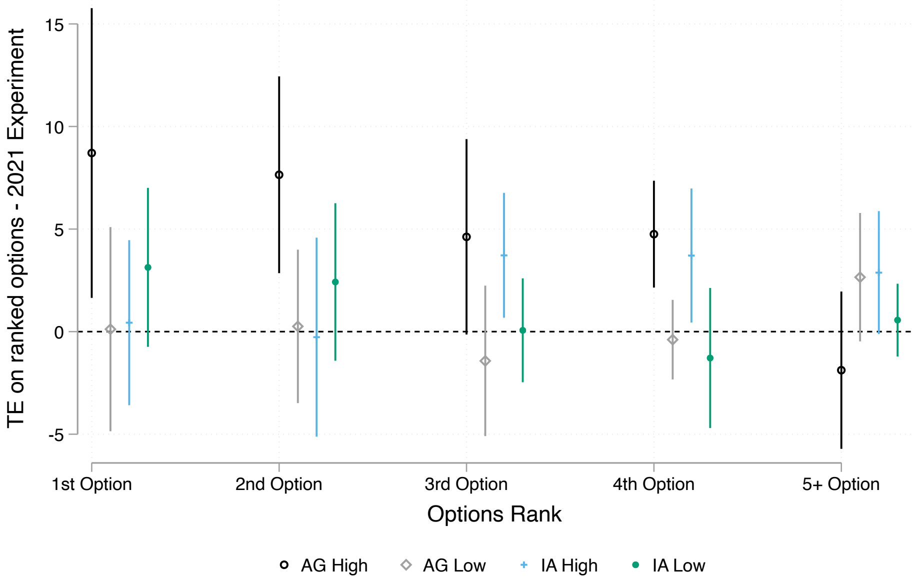


Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. All estimates are identified by comparisons with pure control schools. The omitted year is 2018, the year before the first wave of the intervention. Standard errors are robust and clustered at the school level.

Figure 5: Treatment Effects across the Rank-Ordered List



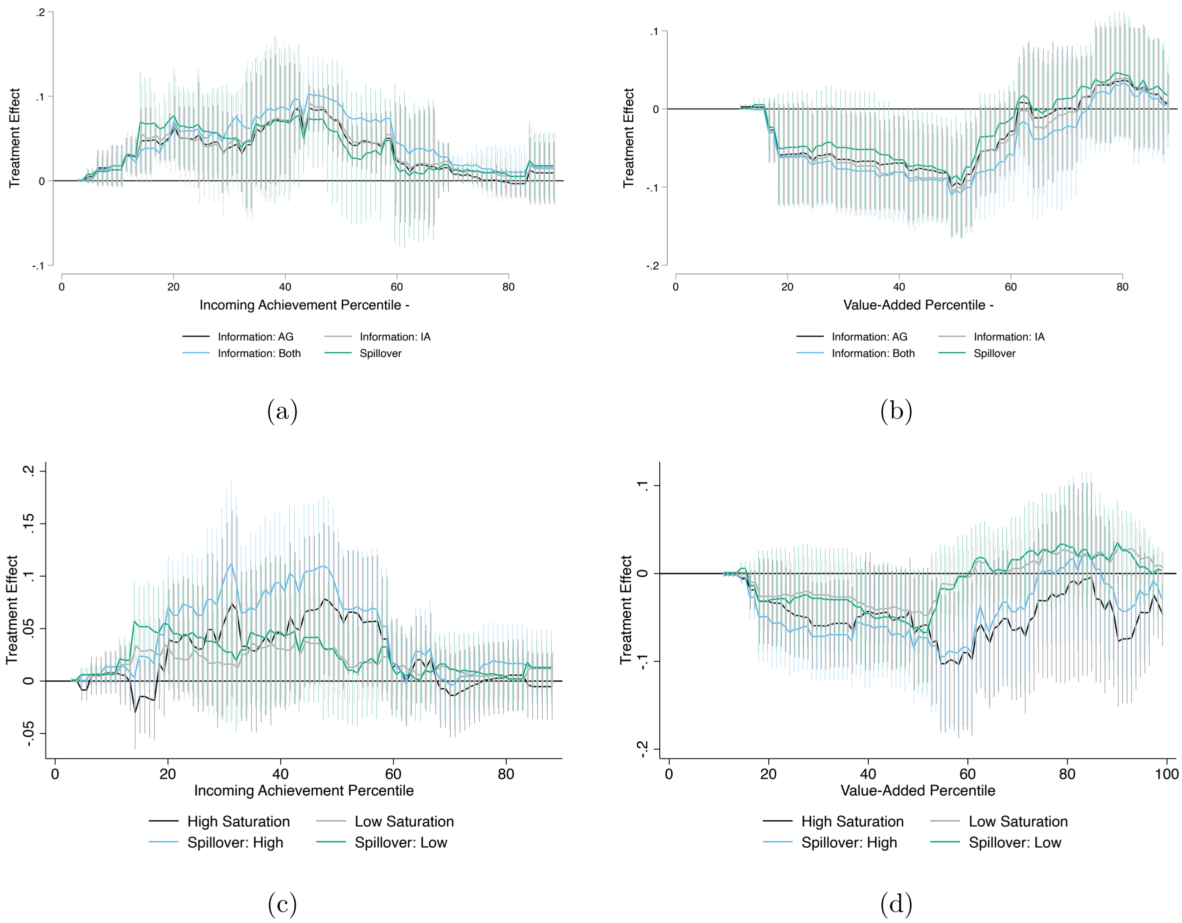
(a) 2019 Effects



(b) 2021 Effects

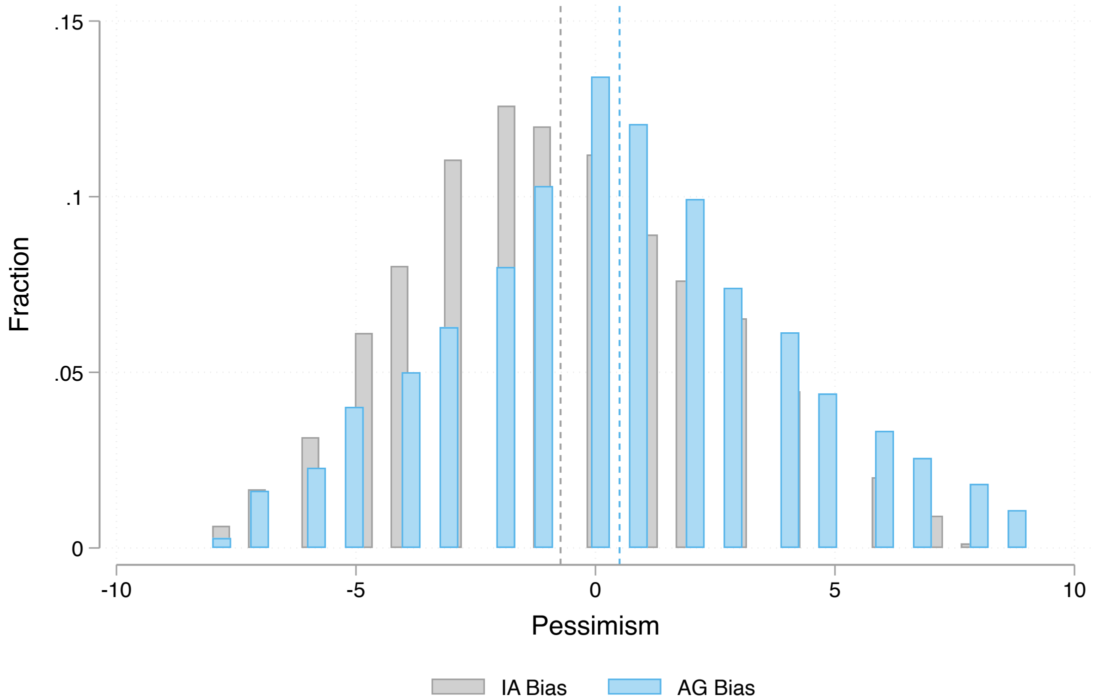
Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of rank-specific school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. In contrast to Figure 4, this figure reports 2019- and 2021-specific effects, omitting other event-time estimates, and instead reports rank-specific estimates. Schools ranked as the fifth option or lower are grouped into a single group. All estimates are identified by comparisons with pure control schools. The omitted year is 2018, the year before the first wave of the intervention. Standard errors are robust and clustered at the school level.

Figure 6: Distributional Estimates



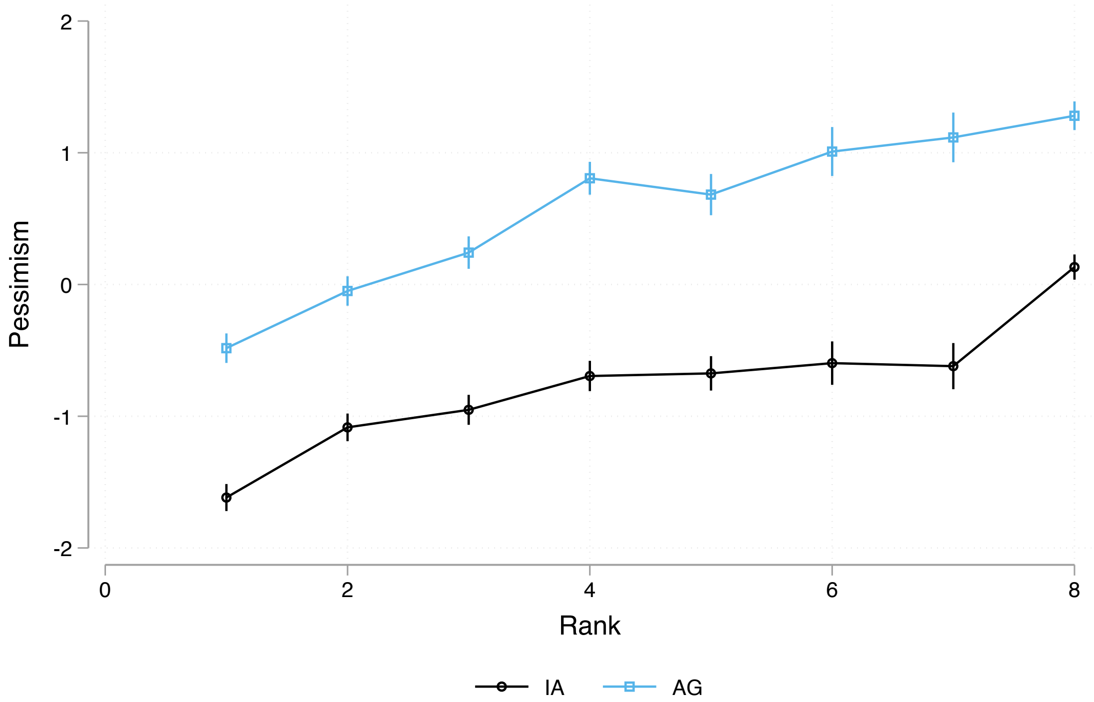
Notes: This figure displays distribution regression estimates across the incoming achievement (achievement growth) distribution. The sample stacks experimental waves and includes experiment-year fixed effects along with student baseline controls included in other estimates throughout the paper. Panels (a) and (b) report treatment effects from models that aggregate treatment at the treatment type level, with types corresponding to IA, AG, both, or spillover. Panels (c) and (d) aggregate treatments to the saturation level, with treatments high, low, spillover high, and spillover low. Throughout, standard errors are clustered at the school level.

Figure 7: IA and AG Pessimism Distribution



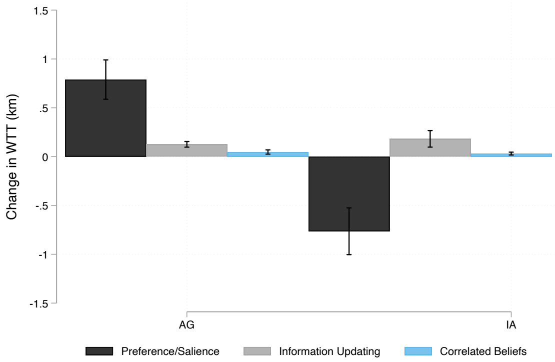
Notes: This figure reports the pessimism distribution for incoming achievement (IA) and achievement growth (AG). Beliefs are collected in terms of deciles, and pessimism is calculated by the difference in between the elicited belief and the estimated belief. Dashed lines correspond to mean pessimism for both quality measures.

Figure 8: Pessimism across the Rank-Ordered List

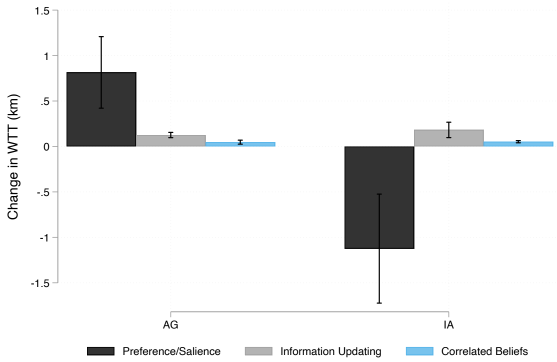


Notes: This figure reports mean pessimism for incoming achievement (IA) and achievement growth (AG) at various points of parents' rank-ordered lists. Points corresponds to means, and 95% confidence intervals are represented by the bars.

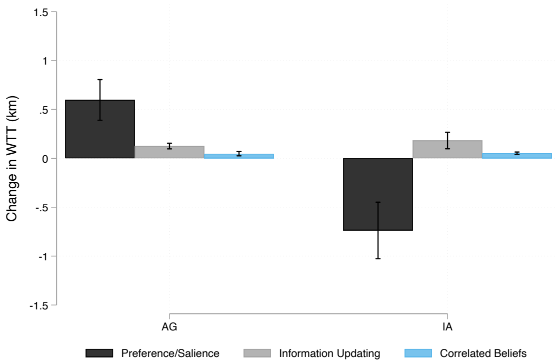
Figure 9: Decomposition of Utility Weight Impacts



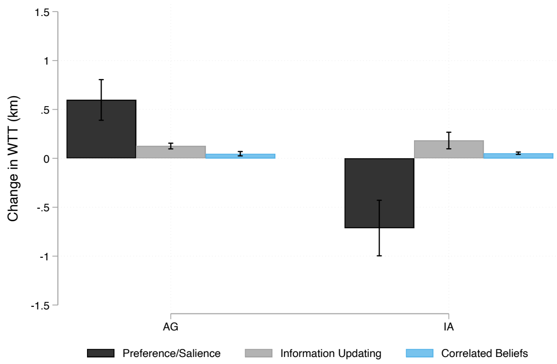
(a) Saturation-Specific Model: Treatment Effects



(b) Saturation-Specific Model: Spillover Effects



(c) Information-Specific Model: Treatment Effects



(d) Information-Specific Model: Spillover Effects

Notes: This figure reports decomposition estimates for two separate models. Panel (a) and Panel (b) report decomposition estimates from a saturation-specific model, and Panel (c) and Panel (d) report decomposition estimates from an information-specific model. Bias variances and correlations are jointly estimated via maximum likelihood, and similarly, quality variances and correlations are jointly estimated via maximum likelihood. These estimates are used in combination with control group utility weight estimates to calculate decomposition factors. Standard errors are estimated via the delta method.

Table 1: ZOC and Non-ZOC Differences

	Non-ZOC (1)	ZOC (2)	Difference (3)
Reading Scores	0.135	-0.117	-0.252
			(0.081)
Math Scores	0.099	-0.114	-0.213
			(0.081)
College	0.1	0.065	-0.036
			(0.017)
Migrant	0.036	0.054	0.018
			(0.007)
Female	0.513	0.481	-0.032
			(0.016)
Poverty	0.909	0.967	0.058
			(0.024)
Special Education	0.148	0.141	-0.007
			(0.022)
English Learners	0.076	0.134	0.058
			(0.017)
Black	0.107	0.03	-0.077
			(0.027)
Hispanic	0.683	0.862	0.179
			(0.075)
White	0.038	0.015	-0.024
			(0.009)
N	26,517	13,015	

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. Reading and math test scores are normalized within grade and year.

Table 2: Saturation School-Level Balance

	Control (1)	Low – Control (2)	High – Control (3)
Reading Scores	-0.094	-0.051	-0.069
		(0.104)	(0.111)
Math Scores	-0.108	-0.054	-0.076
		(0.096)	(0.103)
Parents College +	0.082	0.007	-0.012
		(0.024)	(0.028)
Migrant	0.086	-0.011	0.006
		(0.007)	(0.013)
Female	0.495	-0.016	-0.004
		(0.01)	(0.01)
Poverty	0.9540	-0.024	0.026
		(0.035)	(0.029)
Special Education	0.115	0.015	0.021
		(0.008)	(0.01)
English Learner	0.158	0.014	0.032
		(0.016)	(0.019)
Black	0.051	-0.007	-0.012
		(0.013)	(0.015)
Hispanic	0.863	-0.011	0.013
		(0.043)	(0.033)
White	0.001	0	-0.001
		(0.001)	(0)
Number of Schools	40	32	32

Notes: This table reports estimates from school-level regressions of row variables on saturation-specific indicators and zone fixed effects. The schools are stacked across both years. Column 1 reports the control school means, and Columns 2 and 3 report low- and high-saturation school differentials. Robust standard errors are reported in parentheses.

Table 3: Within-School Randomization Balance: 2019 and 2021 Experiment

	Control (1)	Peer – Control (2)	School – Control (3)	Both – Control (4)	P-Value (5)
Reading Scores	-0.126	0.006	-0.015	-0.006	0.86
		(0.02)	(0.02)	(0.024)	
Math Scores	-0.124	0.013	-0.01	-0.018	0.607
		(0.017)	(0.016)	(0.019)	
Parents College	0.077	-0.001	-0.001	0	0.993
		(0.005)	(0.004)	(0.005)	
Migrant	0.034	0.006	-0.002	0.004	0.182
		(0.004)	(0.004)	(0.003)	
Female	0.485	-0.005	0.001	0.003	0.892
		(0.009)	(0.01)	(0.008)	
Poverty	0.938	0.001	0	-0.005	0.561
		(0.004)	(0.003)	(0.004)	
Special Education	0.138	-0.002	0.008	-0.002	0.597
		(0.006)	(0.007)	(0.006)	
English Learner	0.152	0.002	0.001	0.013	0.324
		(0.005)	(0.006)	(0.007)	
Black	0.031	0.002	-0.004	0.002	0.663
		(0.003)	(0.003)	(0.004)	
Hispanic	0.906	-0.004	0.003	-0.005	0.506
		(0.005)	(0.005)	(0.004)	
White	0.016	-0.002	0	0.001	0.802
		(0.002)	(0.002)	(0.002)	
Joint Test P-Value		0.769	0.951	0.716	
Number Treated	13,954	3,329	3,351	4,636	

Notes. Column 1 reports within-school control group means, and Columns 2–4 contain mean differences between treated and control group individuals. Column 5 contains p-values on a joint test of equality of means across groups for that given row. The p-values reported on the bottom of the table come from a column-wise test of no difference between the treated and control groups. 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: Difference-in-Difference Estimates on School Attributes

	(1) Pure Control Mean	(2) High Saturation 2019	(3) Low Saturation 2019	(4) High Saturation 2021	(5) Low Saturation 2021
Achievement Growth	65.587	4.896**	1.033	8.775**	0.097
		(2.120)	(2.175)	(4.186)	(2.962)
Incoming Achievement	34.517	-1.540	-2.061	0.482	3.122
		(1.646)	(1.774)	(2.397)	(2.313)
Female	0.487	0.003	-0.001	0.006	-0.001
		(0.002)	(0.002)	(0.005)	(0.003)
Migrant	0.082	0.000	0.002*	-0.002	-0.001
		(0.001)	(0.001)	(0.003)	(0.002)
Poverty	0.979	0.000	0.003*	0.005	0.002
		(0.002)	(0.002)	(0.006)	(0.004)
Special Education	0.119	0.003**	0.001	0.004	0.000
		(0.001)	(0.001)	(0.004)	(0.002)
English Learner	0.146	0.002	0.004**	-0.010	0.000
		(0.003)	(0.002)	(0.009)	(0.005)
College	0.054	0.001	-0.002	0.002	0.000
		(0.002)	(0.002)	(0.006)	(0.003)
Black	0.044	0.000	0.000	-0.014	-0.003
		(0.002)	(0.001)	(0.013)	(0.004)
Hispanic	0.908	-0.002	0.002	0.008	0.002
		(0.003)	(0.003)	(0.014)	(0.007)
White	0.019	0.002*	-0.002	0.005	0.001
		(0.001)	(0.001)	(0.004)	(0.002)
Suspension Days	12.310	-0.572	0.162	-1.485	-0.582
		(0.605)	(0.545)	(3.517)	(2.832)
Suspension Incidents	0.007	0.000	0.000	-0.001	0.000
		(0.000)	(0.000)	(0.001)	(0.001)
N			69,054		

Notes: This table reports difference-in-difference estimates on row variables. These estimates come from regressions of most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. All estimates are identified by comparisons of changes between students at treated schools with pure control schools. Standard errors are robust and clustered at the school level.

Table 5: IA and AG Pessimism Correlation with Student Characteristics for Top-Ranked School

	IA Pessimism		AG Pessimism	
	(1) Bivariate	(2) Multivariate	(3) Bivariate	(4) Multivariate
Parents College +	1.085 ***	0.627 ***	-0.009	0.126
	(0.179)	(0.197)	(0.197)	(0.220)
Hispanic	-0.883 ***	-0.243	0.844 ***	1.045 ***
	(0.178)	(0.196)	(0.258)	(0.288)
English Learner	-0.365 **	-0.146	-0.064	-0.247
	(0.152)	(0.167)	(0.189)	(0.210)
Special Education	0.202	0.354 *	0.202	0.211
	(0.157)	(0.171)	(0.182)	(0.201)
Black	0.723 **	0.499	-0.882 **	0.288
	(0.323)	(0.359)	(0.437)	(0.490)
White	0.924 **	0.279	-0.024	0.781
	(0.410)	(0.449)	(0.525)	(0.584)
Female	-0.091	-0.141	-0.094	-0.091
	(0.107)	(0.118)	(0.114)	(0.127)
Poverty	-1.708 ***	-1.572 ***	0.086	-0.154
	(0.171)	(0.190)	(0.197)	(0.220)
Math Z-Score	0.161 ***	-0.043	-0.040	-0.043
	(0.060)	(0.066)	(0.098)	(0.110)
Reading Z-Score	0.194 ***	0.158	-0.026	0.010
	(0.061)	(0.067)	(0.102)	(0.114)
Migrant	-1.265	-1.019	-1.484	-1.533
	(1.026)	(1.123)	(1.006)	(1.118)
Mean	-1.63		-0.52	
SD	3.07		3.36	

Notes: This table reports univariate and multivariate correlations between student-level IA and AG pessimism measures and student-level covariates. Column 1 and Column 2 consider IA pessimism and Column 3 and Column 4 consider AG pessimism. Odd-numbered columns consider bivariate regressions of the pessimism measure on the row variable, and even-numbered columns report estimates from the multivariate analog. Robust standard errors are reported in parentheses.

Table 6: Willingness to travel estimates and impacts

	Without School Effects		With School Effects	
	(1)	(2)	(3)	(4)
	IA	AG	IA	AG
Panel A: Saturation Type				
Treatment				
Untreated	0.391***	0.656***		
	(0.093)	(0.077)		
Information: High	-0.977***	0.616***	-0.617***	0.550***
	(0.154)	(0.095)	(0.158)	(0.121)
Information: Low	-0.743***	0.312***	-0.522***	-0.040
	(0.147)	(0.088)	(0.171)	(0.113)
Spillover: High	-1.358***	0.642***	-1.202***	0.585**
	(0.322)	(0.196)	(0.410)	(0.269)
Spillover: Low	-0.852***	0.255**	-0.571***	-0.039
	(0.175)	(0.105)	(0.214)	(0.140)
Distance	-0.068***		-0.052***	
	(0.006)		(0.007)	
Panel B: Information Type				
Treatment				
Untreated	0.392***	0.658***		
	(0.093)	(0.078)		
Information: IA	-0.972***	0.474***	-0.812***	0.272**
	(0.174)	(0.104)	(0.209)	(0.131)
Information: AG	-0.865	0.424***	-0.594	0.181
	(0.171)	(0.101)	(0.199)	(0.127)
Information: Both	-0.815***	0.565***	-0.393**	0.455***
	(0.154)	(0.100)	(0.160)	(0.126)
Spillover	-0.947***	0.336***	-0.688***	0.097
	(0.172)	(0.100)	(0.204)	(0.129)
Distance	-0.068***		-0.051***	
	(0.006)		(0.007)	
Number of Choices	142,589			
Number of Students	21,774			

Notes: This table reports estimates from four different models. Column (1) and Column (2) of Panel A correspond to estimates from the same model without school fixed effects, while Column (3) and Column (4) of Panel A correspond to estimates from the same model with school fixed effects. Panel A reports estimates from saturation-specific models, while Panel B reports estimates from information-specific models. Untreated rows correspond to utility weight estimates for families in the pure control group. Information: High, Information:Low correspond to families in high and low saturation schools, respectively. Spillover:High and Spillover:Low are defined similarly. Last, Information:IA, Information:AG, and Information:Both correspond to directly receiving IA, AG, or Both types of information, respectively. Each cell, except for distance estimates, report willingness to travel estimates or impacts. These are calculated by dividing the unreported utility weight estimate (or impact) by the corresponding distance disutility estimate. Standard errors are estimated via the delta method.

ONLINE APPENDIX. Not for Publication

A Pilot Details

Months before the intervention, I piloted various messaging strategies on Amazon Mechanical Turk (mTurk). I provided respondents with brief descriptions about each quality measure and then asked them to answer questions that allowed me to infer two things: (i) whether or not they were paying attention and (ii) their level of understanding. To detect inattention, I presented respondents with hypothetical questions that asked them to infer what peer and school quality were like with the available information. In these questions, either incoming achievement (IA) or achievement growth (AG) were held constant, and the respondent had to infer differences between hypothetical schools based on the other measure. To probe at their level of understanding, I asked them to provide a description of the difference between the two measures. Independent researchers subsequently subjectively evaluated the responses.

Given the selected nature of mTurk participants, I imposed a few restrictions on who could respond and to more closely mirror ZOC families. Respondents were restricted to be parents, be under the age of 60, and have at most a high school degree. Too few Hispanic respondents participated at the times I issued the survey to hold that attribute constant across respondents.

Table A.1 presents the results. Roughly 90% of participants could correctly infer IA and AG. Hispanic respondents responded correctly at a modestly lower rate that was statistically insignificant. For respondents' written responses, around 70% wrote something that indicated they understood the difference between IA and AG. In contrast to the other questions, Hispanic respondents wrote correct responses at a modestly higher rate that was also statistically insignificant. Other pilots were run on samples that were not restricted to high school graduates, and I observed higher averages.

Table A.1: 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 who 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)

B Survey Details

B.1 Survey Questions

The survey has a total of 10 questions and in piloting took roughly 5-8 minutes to complete. The questions are reported below.

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
    - Guardian

2. Has anyone mentioned the Zones of choice to you before?
    - 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.

3. How many hours do you anticipate spending researching schools?
    - Less than 2 hours
    - 2-5 hours
    - 6-10 hours
    - 11-15 hours
    - More than 15 hours

4. 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 your student's input

5. Rank the following school characteristics in terms of importance (1-7), where 1 is the most important

- Test score improvement
- Performance of other students
- Safety
- Reputation of teachers
- Distance from home
- Available sport offerings

6. How important are a school's students when choosing a school?
    - Not important
    - Somewhat important
    - Important
    - Very important

7. How important are a school's test scores when choosing a school?
    - Not important
    - Somewhat important
    - Important
    - Very important

8. Do you think schools that attract the highest performing students are also the most effective at facilitating test score growth?
    - Yes, definitely
    - Not necessarily

Section C - We are going to ask you questions about your preferences and beliefs about two important characteristics of schools. We determine the quality of a school based on students' average scores on state exams.

This measure has two parts you should consider: One (1) which measures the school's ability of attracting high scoring students, and the second (2) is the school's impact on test score growth.

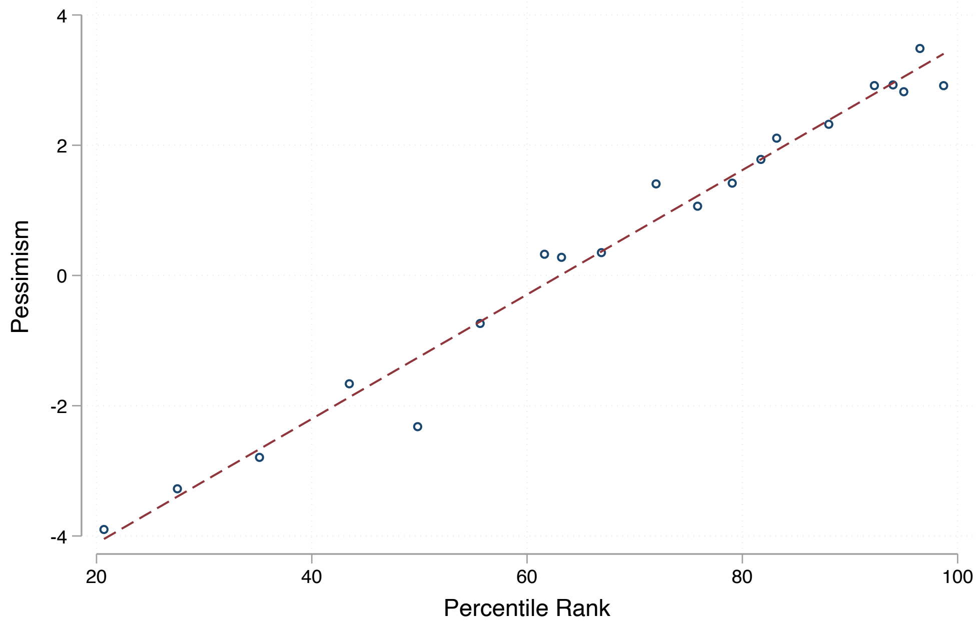
- Incoming Achievement (IA): We can measure a school's ability to attract high-achieving students by measuring the average test scores of its incoming students.
- Achievement Growth (AG): Similarly, we can measure the school's ability to improve test scores using the growth of the same student's test scores between entry into the school and some later date.

9. For the next table, please give each school a rating between 0-10, 10-20, $\cdots$, 90-100 according to your beliefs about their ability in terms of (1) Incoming Achievement and (2) Achievement Growth.

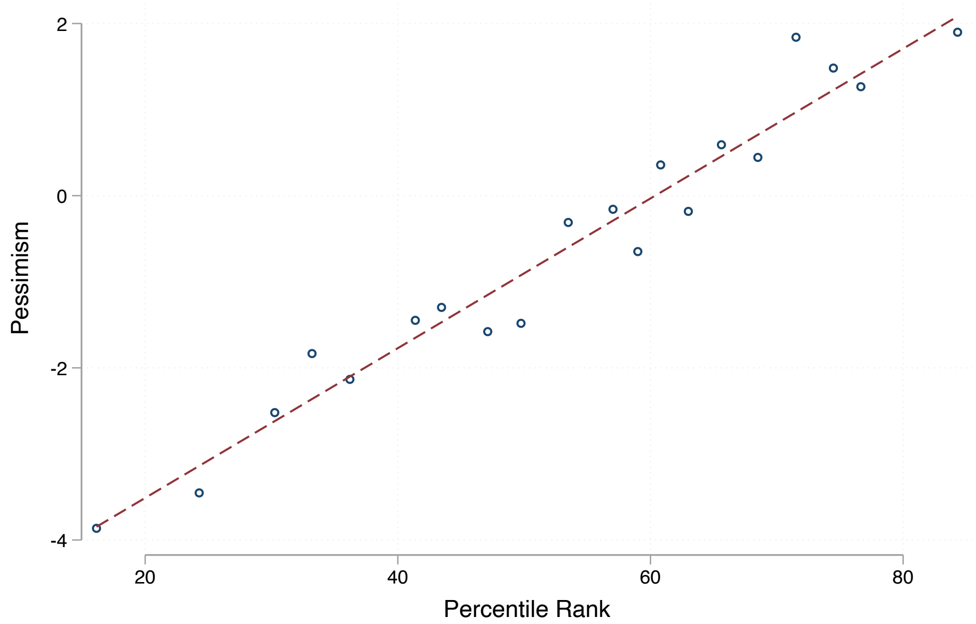
10. Please rank the schools as if you were submitting the application today. Note there are K schools you can choose from, so rank your most preferred as 1 and the least preferred as K.

Figure B.1: AG Bias-Truth Relationship

(a) All Options on Rank-Ordered List



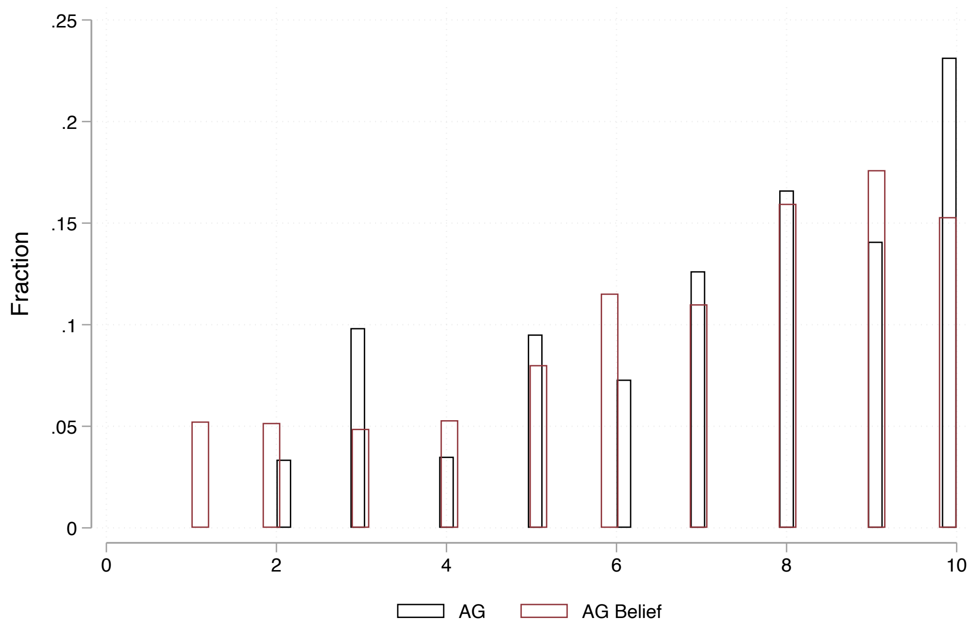
(b) IA Bias-Truth Relationship



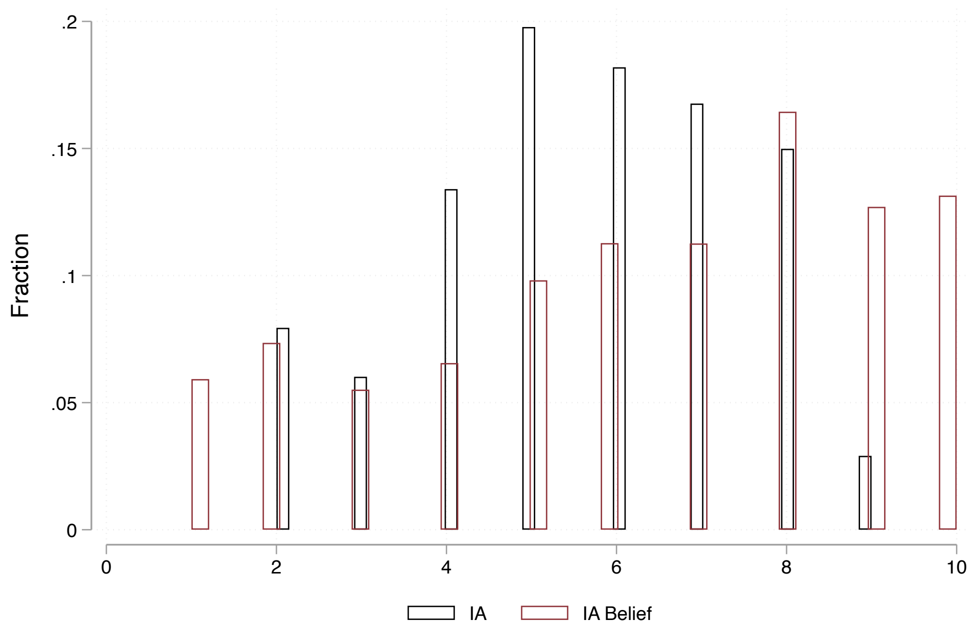
Notes: This figure reports bivariate-binned scatter plots of the pessimism-achievement relationship. Panel (a) reports the relationship across all options contained on the rank-ordered list, while Panel (b) reports the relationship only among the top-ranked option of applicants' rank-ordered lists.

Figure B.2: AG Decile and AG Belief Distribution

(a) Achievement Growth



(b) Incoming Achievement



Notes: This figure reports option-specific distributions of AG (IA) deciles and AG (IA) beliefs. If applicants' decile beliefs were perfectly on target, then their belief distribution would perfectly overlap with the decile distribution.

Table B.1: Survey Respondent Characteristics

	(1) No Survey	(2) Partial	(3) Complete
Reading Z-Score	-0.199	0.011	0.151***
		(0.032)	(0.025)
Math Z-Score	-0.187	0.010	0.162***
		(0.044)	(0.022)
Female	0.495	-0.011	-0.018**
		(0.013)	(0.009)
Migrant	0.002	0.002	0.000
		(0.002)	(0.001)
Poverty	0.901	0.004	-0.012
		(0.009)	(0.008)
Special Education	0.144	0.012	-0.008
		(0.010)	(0.008)
English Learner	0.179	0.009	-0.028***
		(0.009)	(0.008)
College	0.081	-0.010	0.023**
		(0.010)	(0.010)
Black	0.032	-0.010***	0.000
		(0.003)	(0.002)
Hispanic	0.911	-0.001	-0.017*
		(0.009)	(0.010)
White	0.016	0.001	0.001
		(0.003)	(0.002)
N	5,154	1,355	4,132

Notes: This table reports estimates from regressions of each row variable on indicators for survey completion status. Partial indicates that the respondent did not finish the survey, usually corresponding to missing beliefs information, and complete corresponds to respondents who completed the survey. The response rate is 51.5%, and the completion rate is 38%. Robust standard errors are reported in parentheses.

Table B.2: Pessimism Summary Statistics by Zone

	(1)	(2)	(3)
	IA	AG	N
Boyle Heights	-2.18	-0.09	697
	[3.42]	[2.51]	
Bell	-0.26	-0.26	5,181
	[2.86]	[3.25]	
Belmont	-0.88	0.27	10,283
	[3.22]	[3.64]	
Bernstein	-1.65	1.31	417
	[3.46]	[3.14]	
Carson	1.02	-1.47	1,963
	[2.69]	[3.16]	
Eastside	-2.09	1.91	5,439
	[2.85]	[3.3]	
Fremont	-1.99	-0.17	4,348
	[3.02]	[3.71]	
Huntington Park	-0.35	1.75	1,995
	[3.09]	[2.65]	
Jefferson	-1.45	-0.03	2,979
	[3.1]	[3.86]	
North Valley	-0.56	0.15	4,055
	[2.73]	[3.4]	
Narbonne	0.12	-3.32	773
	[2.78]	[3.06]	
Northeast	-1.1	-0.75	1,677
	[3.56]	[2.72]	
RFK	-3	0.07	987
	[2.78]	[2.7]	
South Gate	0.85	2.06	6,843
	[2.76]	[2.72]	

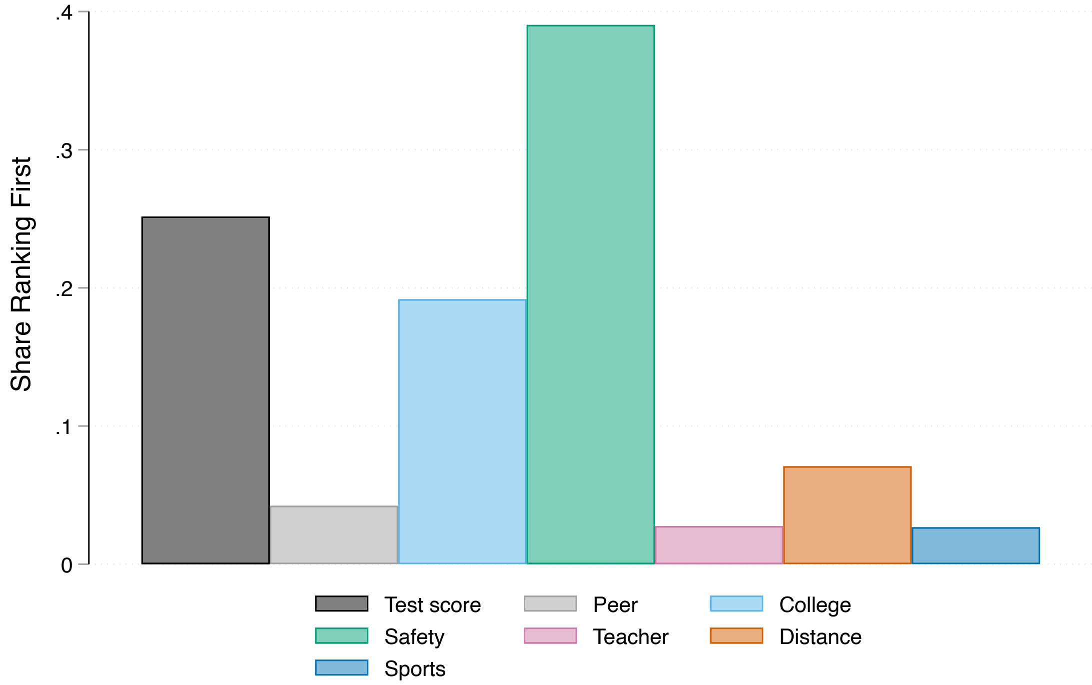
Notes: This table reports IA and AG mean and standard deviations for each Zone of Choice in our sample. IA and AG are in terms of pessimism as defined in the main text. Standard deivations are reported in brackets underneath their corresponding mean. Column 3 reports the number of observations with elicited beliefs in each market.

Table B.3: Survey Responses

Panel A: Anticipated Participation in the School Choice Process				
Respondent Relationship	Father: 0.109	Mother: 0.866	Grandparent: 0.006	Legal Guardians: 0.019
Anticipated Research Hours	Less Than 2 Hours: 0.373	2-5 Hours: 0.352	6-10 Hours: 0.352	10+ Hours: 0.156
	Yes		No	
Have you heard of ZOC?	0.340		0.660	
Do you anticipate doing any of the following:				
Visit a school fair	0.470		0.530	
Watch promotional videos	0.430		0.570	
Talk to teachers	0.520		0.480	
Talk to parents	0.470		0.530	
Online research	0.640		0.360	
Panel B: Perception of school characteristics				
	Not Important	Somewhat Important	Important	Very Important
Peer importance	0.080	0.224	0.326	0.370
Test score importance	0.013	0.079	0.369	0.539
Do you think that...	Yes, definitely		Not necessarily	
Good peers imply high growth?	0.320		0.680	

Notes: This table reports a series of descriptive statistics from the baseline survey. The questions correspond to Section A and Section B of the baseline survey discussed in Appendix B.

Figure B.3: Stated Preferences over School Attributes



Notes: This figure reports survey item results from a question asking parents to rank various school attributes from most important (1) to least important (7). Each bar corresponds to the share of parents ranking the attribute first. The precise question is listed in Appendix Section B.

Table B.4: IA and AG Correlation with Student Characteristics - All Options

Bias Measure	IA		AG	
	Bivariate	Multivariate	Bivariate	Multivariate
Parents College	1.085 ***	0.620 ***	-0.009	0.177 **
	(0.179)	(0.197)	(0.079)	(0.085)
Hispanic	-0.883 ***	0.268 **	0.844 ***	1.112 ***
	(0.178)	(0.196)	(0.107)	(0.116)
English Learner	-0.365 **	-0.226 ***	-0.064	-0.426 ***
	(0.152)	(0.167)	(0.072)	(0.078)
Special Education	0.202	0.469 ***	0.202	0.426 ***
	(0.157)	(0.171)	(0.070)	(0.075)
Black	0.723 **	1.015 ***	-0.882 **	0.908 ***
	(0.323)	(0.359)	(0.190)	(0.207)
White	0.924 **	0.820 ***	-0.024	0.636 ***
	(0.410)	(0.449)	(0.227)	(0.245)
Female	-0.091	-0.121 ***	-0.094	-0.165 ***
	(0.107)	(0.118)	(0.044)	(0.047)
Poverty	-1.708 ***	-1.365 ***	0.086	-0.321 ***
	(0.171)	(0.190)	(0.081)	(0.088)
Math Z-Score	0.161 ***	-0.009	-0.040	0.054
	(0.060)	(0.066)	(0.038)	(0.041)
Reading Z-Score	0.194 ***	0.150 ***	-0.026	0.011
	(0.061)	(0.067)	(0.040)	(0.043)
Migrant	-1.265	-0.905 **	-1.484	-1.581 ***
	(1.026)	(1.123)	(0.376)	(0.405)

Notes: This table reports univariate and multivariate correlations between student-level IA and AG pessimism measures and student-level covariates across all options on the rank-ordered list. Column 1 and Column 2 consider IA pessimism and Column 3 and Column 4 consider AG pessimism. Odd-numbered columns consider bivariate regressions of the pessimism measure on the row variable, and even-numbered columns report estimates from the multivariate analog. Robust standard errors are reported in parentheses.

Table B.5: Preference Estimates: Beliefs and Estimated Quality

	Estimated Quality		Elicited Beliefs	
	(1) AG	(2) IA	(3) AG	(4) IA
Decile 2			0.344	0.056
			(0.104)	(0.093)
Decile 3	0.215	0.574	0.343	0.073
	(0.052)	(0.052)	(0.111)	(0.102)
Decile 4	0.204	0.454	0.280	0.170
	(0.064)	(0.039)	(0.111)	(0.102)
Decile 5	0.608	0.467	0.327	0.211
	(0.056)	(0.039)	(0.110)	(0.101)
Decile 6	0.229	0.573	0.424	0.405
	(0.065)	(0.041)	(0.110)	(0.101)
Decile 7	0.390	0.539	0.527	0.498
	(0.050)	(0.045)	(0.110)	(0.102)
Decile 8	0.183	0.491	0.692	0.776
	(0.048)	(0.046)	(0.109)	(0.101)
Decile 9	0.268	0.714	0.906	0.915
	(0.051)	(0.068)	(0.110)	(0.104)
Decile 10	0.470		1.221	1.049
	(0.050)		0.115)	(0.109)
Unknown School			0.333	0.116
			(0.140)	(0.134)
Distance	-0.028		-0.014	
	(0.009)		(0.009)	

Notes: This table reports willingness to travel estimates for enrolling in different ranked school deciles relative to the lowest quality school in the model. These estimates are derived from rank-ordered logit models with distance and quality decile indicators for both IA and AG. Column 1 and Column 2 report willingness to travel estimates for a model considering researcher-estimated quality, and Column 3 and Column 4 report willingness to travel estimates using elicited beliefs. The first model reports willingness to travel estimates relative to schools in the bottom two deciles, while the belief model estimates willingness to travel estimates relative to bottom decile schools in terms of beliefs. The belief model also reports willingness to travel estimates for unknown schools; that is, schools parents admitted they had not heard about during the baseline survey. Robust standard errors are reported in parentheses.

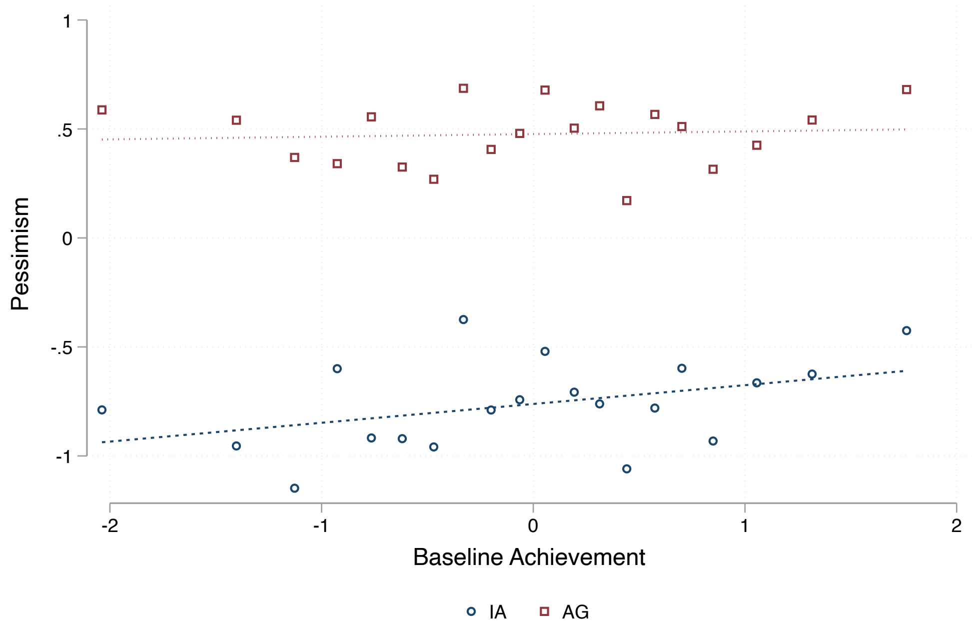
Table B.6: IA and AG Absolute Bias Correlation with Student Characteristics

Bias Measure	IA		AG	
	Bivariate	Multivariate	Bivariate	Multivariate
Parent College	-0.678 ***	-0.633 ***	-0.035	-0.095 *
	(0.113)	(0.127)	(0.051)	(0.056)
Hispanic	0.221 *	-0.443 ***	-0.300 **	-0.262 ***
	(0.113)	(0.127)	(0.069)	(0.076)
English Learner	0.319 ***	0.227 ***	0.231 **	0.174 ***
	(0.096)	(0.108)	(0.047)	(0.051)
Special Education	0.061	-0.177 ***	0.236 **	-0.023
	(0.099)	(0.111)	(0.045)	(0.049)
Black	-0.202	-0.777 ***	0.549 **	0.155
	(0.204)	(0.232)	(0.122)	(0.135)
White	-0.061	-0.041	0.385	0.141
	(0.260)	(0.290)	(0.146)	(0.160)
Female	-0.044	-0.017	0.044	0.022
	(0.068)	(0.076)	(0.028)	(0.031)
Poverty	0.501 ***	0.193 ***	-0.094	-0.190 ***
	(0.109)	(0.123)	(0.052)	(0.057)
Math Z-Score	-0.151 ***	-0.066 ***	-0.182 ***	-0.296 ***
	(0.038)	(0.043)	(0.024)	(0.027)
ELA Z-Score	-0.168 ***	-0.129 ***	-0.119 ***	0.125 ***
	(0.039)	(0.043)	(0.025)	(0.028)
Migrant	0.004	-0.233	0.045	-0.177
	(0.649)	(0.724)	(0.242)	(0.265)

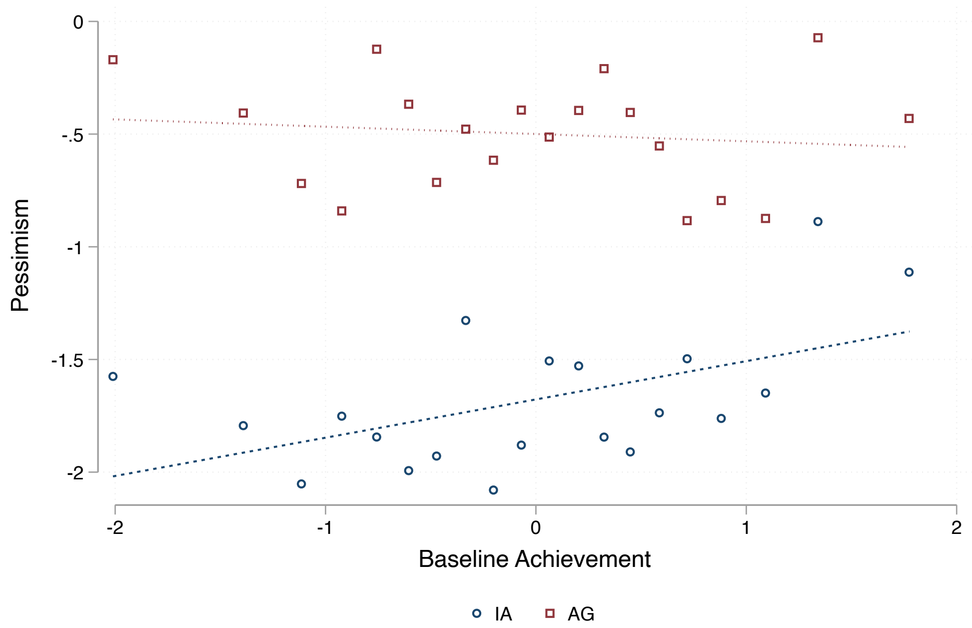
Notes: This table reports univariate and multivariate correlations between student-level IA and AG absolute bias measures and student-level covariates across all options on the rank-ordered list. In contrast to pessimism, we consider absolute bias for these correlations. Column 1 and Column 2 consider IA bias and Column 3 and Column 4 consider AG bias. Odd-numbered columns consider bivariate regressions of the absolute bias measure on the row variable, and even-numbered columns report estimates from the multivariate analog. Robust standard errors are reported in parentheses.

Figure B.4: Pessimism-Achievement Relationship

(a) All Options on Rank-Ordered List



(b) Only Top-Ranked Option on Rank-Ordered List



Notes: This figure reports bivariate-binned scatter plots of the pessimism-achievement relationship. Panel (a) reports the relationship across all options contained on the rank-ordered list, while Panel (b) reports the relationship only among the top-ranked option of applicants' rank-ordered lists.

C Peer and School Quality Estimation

In this section, we discuss the peer and school quality estimation. We consider a constant-effects value-added model (Angrist et al., 2017). In particular, potential outcomes are denoted as

$$Y_{ij} = \mu_j + a_i \tag{18}$$

where α_j is the mean potential outcome at school j and a_i is student ability. We denote school j enrollment indicators as D_{ij}, so that we can write the observed outcome Y_i as

$$Y_i = \mu_0 + \sum_j \beta_j D_{ij} + a_i.$$

We further assume that $a_i = \gamma' X_i + u_i$, where X_i is a vector of student baseline covariates including lagged test scores. With this assumption, the observed outcome is

$$Y_i = \mu_0 + \sum_j \beta_j D_{ij} + \gamma' X_i + u_i \tag{19}$$

which is the canonical causal value-added model considered in the literature (Campos and Kearns, 2022).

In estimation, however, a regression of observed outcomes on school indicators and the vector of student covariates is

$$Y_i = \alpha_0 + \sum_j \alpha_j D_{ij} + \theta' X_i + e_i$$

and e_i need not be uncorrelated with D_{ij}, and $\alpha_j \neq \beta_j$.

Although we estimate school quality using the standard selection on observables assumption, we leverage the lottery variation embedded in the Zones of Choice markets to assess for bias in the school quality estimates (Angrist et al., 2017). With forecast unbiased estimates, we then proceed to construct our measures of school and peer quality.

C.1 VAM Validation

We use the procedure outlined by Angrist et al. (2017) to test for bias in the VAM estimates. We can construct predictions using the value-added model we estimate, which we denote as $\widehat{A}_i$. To test for bias, we treat $\widehat{A}_i$ as an endogenous variable in a two-stage least squares framework using L lottery offer dummies $Z_{i\ell}$ that we collect across zones and cohorts:

$$A_i = \xi + \phi \widehat{A}_i + \sum_\ell \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i' \delta + \varepsilon_i \tag{20}$$

$$\widehat{A}_i = \psi + \sum_\ell \pi_\ell Z_{i\ell} + \boldsymbol{X}_i' \xi + e_i. \tag{21}$$

If lotteries shift VAM predictions in proportion to the shift of realized test scores A_i, on average, then $\phi = 1$, which is a test of forecast bias (Chetty et al., 2014, Deming, 2014). The overidentifying restrictions further allow us to test whether this applies to each lottery and thus to test the predictive validity of each lottery.

Table C.1 reports results for two value-added models. Column 1 reports results for a model that omits any additional covariates beyond school-by-year dummies; this is the uncontrolled model. As discussed in Deming et al. (2014), Chetty et al. (2014), and Angrist et al. (2017), models that do not adjust for lagged achievement tend to perform poorly in their average predictive validity. Indeed, we find the forecast coefficient to be 0.63, indicating that the uncontrolled model does not pass the first test. Column 2 reports estimates from a constant effects VAM specification and demonstrates that our VAM estimates are forecast unbiased and the overidentification tests provide reassuring evidence regarding the predictive validity of each VAM estimate. While the results in Table C.1 do not entirely rule out bias in OLS value-added estimates, they are reassuring.

Table C.1: Forecast Bias and Overidentification Tests

	(1) Uncontrolled	(2) Constant Effect
Forecast Coefficient	.63	1.111
	(.105)	(.134)
	[0]	[.41]
First-Stage F	277.507	37.016
Bias Tests:		
Forecast Bias (1 d.f.)	12.528	.683
	[0]	[.409]
Overidentification (180 d.f)	172.281	187.744
	[.647]	[.331]

Notes: This table reports the results of lottery-based tests for bias in estimates of school effectiveness. The sample is restricted to students in the baseline sample who applied to an oversubscribed school within a school choice zone. Column (1) measures school effectiveness as the school mean outcome, Column (2) uses time-invariant value-added estimates. The forecast coefficients and overidentification tests reported in Columns (1)–(2) 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.

C.2 School and Peer Quality Measures

School average achievement follows from Equation 19

$$\bar{Y}_j = \alpha_j + \theta' \bar{X}_j$$

School quality is therefore defined as $\hat{\alpha}_j$ and peer quality is defined as $\hat{\theta}'\bar{X}_j$. We convert these measures to percentile ranks in terms of the LAUSD high school distribution. In particular,

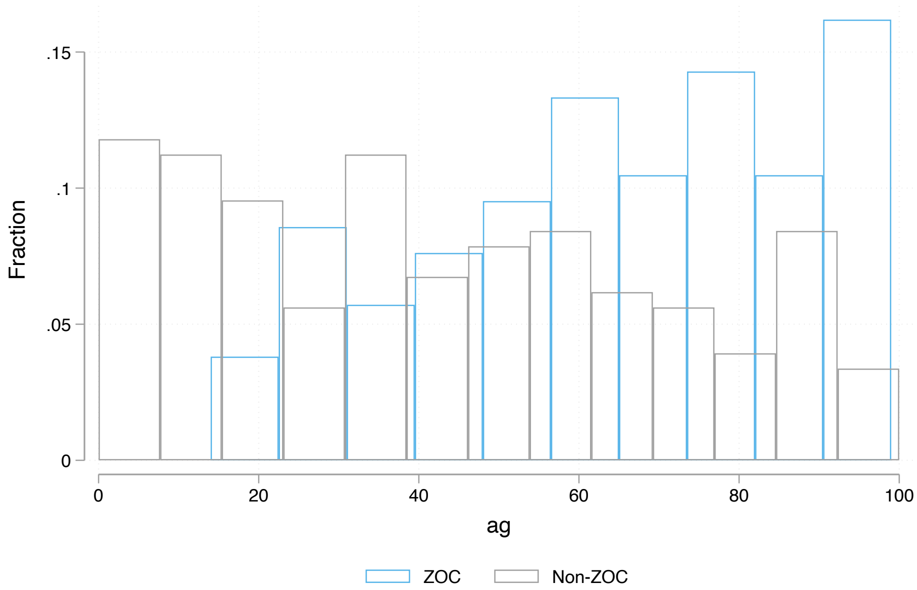
$$Q_j^S = \text{int}\left(\frac{\text{rank}(\hat{\alpha}_j)}{J} \times 100\right) \tag{22}$$

$$Q_j^P = \text{int}\left(\frac{\text{rank}(\hat{\beta}'\bar{X}_j)}{J} \times 100\right) \tag{23}$$

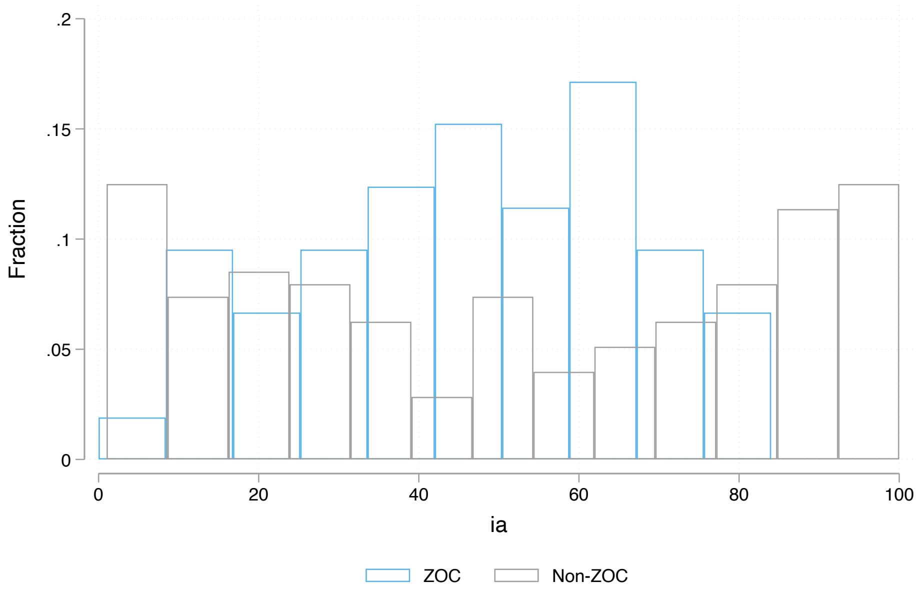
where Q_j^S and Q_j^P are school and peer quality, respectively, measured in percentile ranks, rounded to the nearest integer.

C.3 Summary Statistics

Figure C.1: AG-IA Relationship (Percentile Rank)



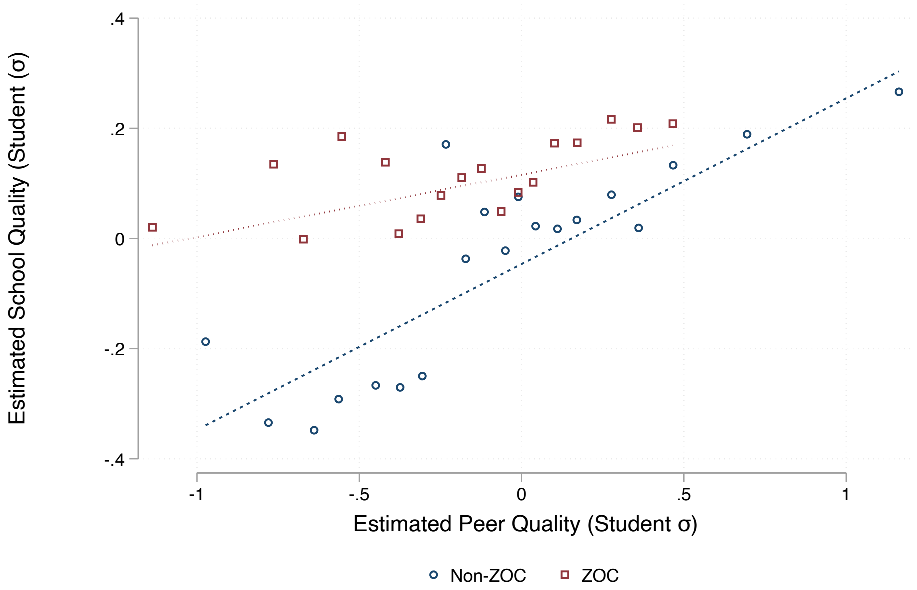
(a) Achievement Growth



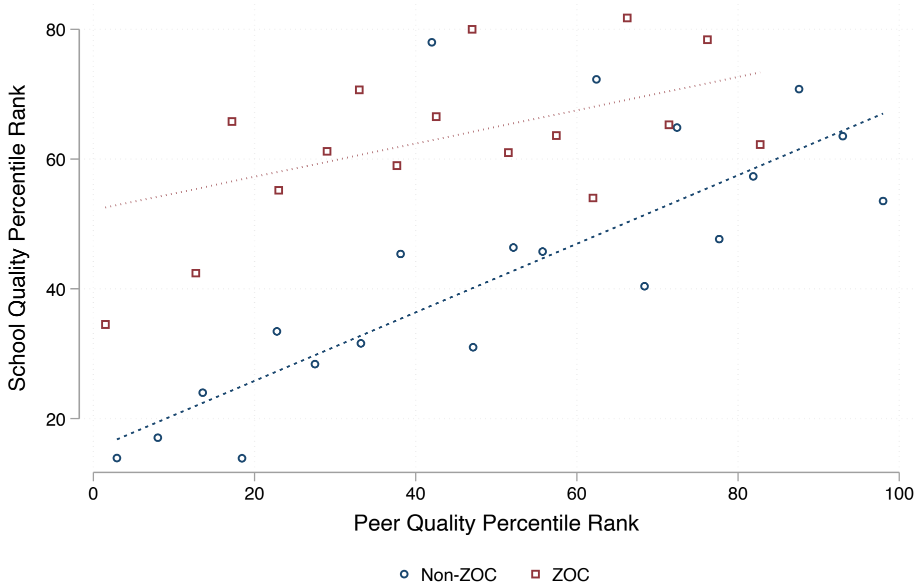
(b) Incoming Achievement

Notes: This figure reports histograms for the Incoming Achievement (IA) and Achievement Growth (AG) percentile rank distributions for ZOC and non-ZOC schools separately.

Figure C.2: AG-IA Bivariate Relationship



(a) Student Standard Deviation Units



(b) Percentile Rank Units

Notes: This figure reports bivariate-binned scatter plots of the AG-IA relationship. Panel (a) reports the relationship of AG and IA in student standard deviation units. AG, also referred to as value-added, is demeaned with respect to the mean in the district, so it reflects the average treatment effect of enrolling in a given school. IA, also referred to as incoming achievement, is fraction of test scores predicted by baseline covariates. Panel (b) reports the IA-AG relationship in terms of percentile ranks defined above.

D Additional Experiment Results

Table D.1: Baseline Experimental Effects 2019 Wave

	(1) Higher VA	(2) Higher 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	0.495	-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	11,541	11,541

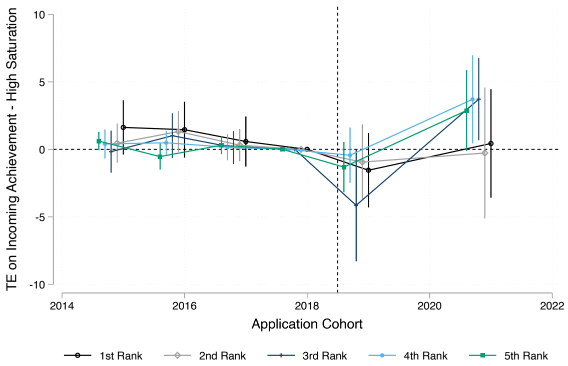
Notes: This table reports baseline experimental effects from the 2019 wave of the experiment. Estimates come from regressions of most-preferred AG (IA) on eight separate treatment indicators, including two saturation-specific spillover indicators, and three saturation-specific information-specific indicators. Column 1 reports estimates for a model with most-preferred AG as the outcome, and Column 2 reports estimates from a model with most-preferred IA as the outcome. Standard errors are robust and clustered at the school level.

Table D.2: Baseline Experimental Effects, 2021 Wave

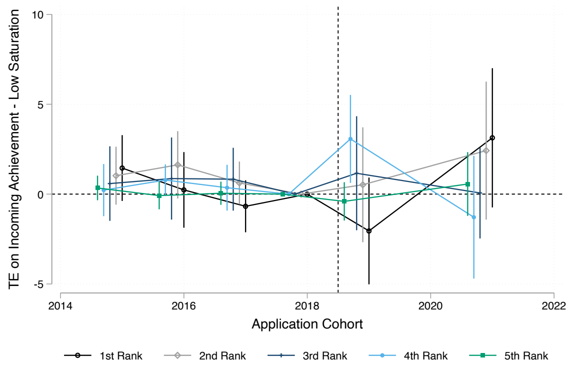
	(1) AG	(2) IA
High-Saturation Treatment		
Peer Quality	6.307	-3.007
	(4.156)	(2.16)
School Quality	7.816**	-2.659
	(3.717)	(2.37)
Both	7.241*	-3.852*
	(4.029)	(2.226)
Low-Saturation Treatment		
Peer Quality	0.871	0.563
	(3.41)	(2.231)
School Quality	0.205	0.079
	(3.416)	(2.48)
Both	1.322	1.037
	(3.369)	(2.317)
Spillover Treatment		
High Saturation	5.91	-3.308*
	(4.09)	(1.949)
Low Saturation	0.787	0.171
	(3.313)	(2.274)
Pure Control Mean	66.914	51.647
R2	0.290	0.380
N	9,008	9,008

Notes: This table reports baseline experimental effects from the 2021 wave of the experiment. Estimates come from regressions of most-preferred AG (IA) on eight separate treatment indicators, including two saturation-specific spillover indicators, and three saturation-specific information-specific indicators. Column 1 reports estimates for a model with the most-preferred AG as the outcome, and Column 2 reports estimates from a model with most-preferred IA as the outcome. Standard errors are robust and clustered at the school level.

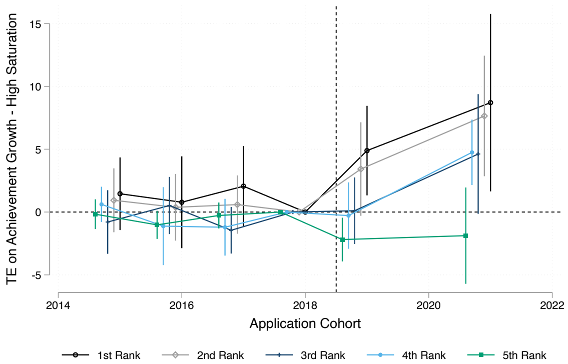
Figure D.1: Treatment Effects across the Rank-Ordered List



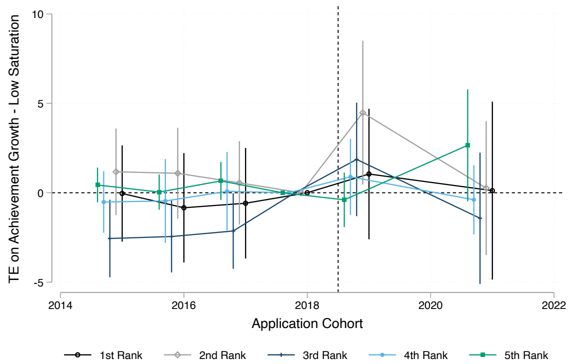
(a) Effects on IA: High Saturation



(b) Effects on IA: Low Saturation



(c) Effects on AG: High Saturation



(d) Effects on AG: Low Saturation

Notes: This figure reports difference-in-difference estimates on most-preferred IA and AG across the rank-ordered list. Each subfigure reports treatment effects that are saturation-specific for different most-preferred attributes. Subfigure (a) and Subfigure (b) correspond to estimates from the same model, and Subfigure (c) and Subfigure (d) correspond to estimates from the same model. Within each subfigure, each line within each figure corresponds to treatment effects on schools ranked at different portions of the rank-ordered list estimated separately. Estimates come from difference-in-differences regressions of most-preferred attributes on saturation-specific indicators, treatment group indicators, year indicators, and baseline controls. 95 percent confidence intervals are reported by bars and standard errors are robust and clustered at the school level.

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