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SOCIAL INTERACTIONS, INFORMATION, AND PREFERENCES FOR SCHOOLS: EXPERIMENTAL EVIDENCE FROM LOS ANGELES

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ABSTRACT

This paper measures parents' beliefs about school and peer quality, how information about each affects school choices, and how social interactions mediate these effects. Parents underestimate school quality and overestimate peer quality. Cross-randomized school and peer quality information combined with a spillover design shows that when parents received information, they and their neighbors' preferences shifted toward higher value-added schools, underscoring stronger tastes for school quality and the role of social interactions. Increased enrollment in effective schools improved socio-emotional outcomes. The experimental evidence shows parents value school effectiveness even conditional on peer quality and that social interactions strongly influence school choice.

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A data appendix is available at http://www.nber.org/data-appendix/w33010
A randomized controlled trials registry entry is available at AEARCTR-0004844

1 Introduction

Parents' valuation of effective schools govern the success of school choice policies, but many open questions remain as to what they prioritize and why. Some studies suggest that parents prioritize schools that improve student learning and other outcomes (Beuermann et al., 2022, Campos and Kearns, 2024), 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., 2023, Rothstein, 2006). Substantial attention has been placed on this question because it is not obvious that parents should prioritize school quality if there are other incentives governing school choices (MacLeod and Urquiola, 2019). However, much of the existing evidence tends to rely on revealed preference arguments whose inferences are complicated by imperfect information (Abaluck and Compiani, 2020). Three open questions remain in light of these facts. Do parents value effective schools? What do parents know about school and peer quality? What factors mediate parents' choices? These three questions are central to better understanding the effectiveness of school choice policies.

This paper reports evidence from an information provision experiment that sheds light on these open questions. I cross-randomize information about school and peer quality to better understand what quality variation parents are most responsive to while simultaneously addressing information gaps. I elicit parents' beliefs about school and peer quality in a baseline survey to better understand the severity of imperfect information before the intervention. Both measures have been extensively studied in prior work (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023, Beuermann et al., 2022, Corradini, 2024, Hastings and Weinstein, 2008, Mizala and Urquiola, 2013, Rothstein, 2006), but to date, we have a limited understanding of what parents actually know about them when they make decisions. Last, to gain insight into factors that mediate parents' choices, I introduce a component into the design that allows me to measure the importance of social interactions as captured by spillover effects of information provision (Crépon et al., 2013). An abundance of anecdotal and descriptive evidence alludes to the importance of social interactions (Schneider et al., 2000), but no causal evidence exists demonstrating its importance for engaging and interpreting information in the context of school choice.

The setting is a market of high schools in Los Angeles neighborhoods referred to as Zones of Choice (ZOC) neighborhoods (Campos and Kearns, 2024).[1] In eighth grade, students living in ZOC neighborhoods apply to their neighborhood-based market with several nearby schools. 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 setting provides roughly 20,000 eighth-grade students enrolled at 104 school-year cohorts across two experimental waves.

The experiment's design considers three primary objectives. The first is effectively learning about parents' beliefs about school and peer quality. To accomplish this goal, I first teach families about school quality, peer quality, and their differences using pedagogical videos to

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

explain the concepts intuitively. Once parents have a better understanding of these key quality measures, I can convincingly elicit their beliefs about school and peer quality through a field survey. The second objective is to gauge how parents respond to changes in school and peer quality, which I do by randomly providing information about each. Finally, the third objective is to measure the role of social interactions in the school choice process. This is done through a two-stage randomization process (Crépon et al., 2013). First, schools are randomized to different levels of treatment saturation: high, low, or pure control. Then, within each school's saturation level, I randomly assign information about school quality, peer quality, or both. This design allows me to learn about parents' beliefs, assess their responsiveness to different sources of quality variation, and simultaneously assess the empirical relevance of social interactions by comparing untreated parents in treated schools to parents in pure control schools.

I begin with a reduced-form difference-in-differences analysis of the intervention's effects. I find an increased demand for school quality in all treatment groups. I also find sizable spillover effects, statistically and nominally equivalent to treatment effects, the first evidence that social interactions matter for engaging with information in school choice environments. The treatment effects are nuanced in that any effects, direct or spillover, are only detected in high-saturation schools. These findings suggest that social interactions are so crucial to driving meaningful changes in demand that if there aren't enough parents nearby to discuss the information, even those who receive it are unlikely to act on it. Complementary online survey evidence corroborates this interpretation, finding that parents do indeed report other parents as valuable sources of information and indicate that their reliance on other parents is to reinforce their understanding of the information. Overall, the reduced form findings suggest that most of the existing evidence documenting a stronger preference for peer quality may have been a product of imperfect information, as families seem to exhibit a stronger taste for school quality, and social interactions help nurture a better understanding of the information landscape in school choice environments.

To further explore the potential channels, I turn to the field survey containing parents' beliefs about both quality measures. Three facts arise from the survey data. First, families tend to underestimate their school quality and overestimate peer quality; I refer to overestimation as optimism and underestimation as pessimism.[2] These differences hold across the rank-ordered list (ROL), with modest gradients indicating that families are more pessimistic about the schooling options that they prefer less. Second, the biases are choice-relevant in the sense that they induce application mistakes (Larroucau et al., 2024). In other words, the biases are sufficiently large for many applicants to generate different rank-ordered lists than in a setting without the biases. Third, I do not find student-level attributes that correlate with either peer or school quality 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.

With the survey data, I return to analyzing the intervention viewed through a discrete choice lens. This analysis features a few key advantages. First, it uses information from the entire rank-ordered list (ROL), providing a comprehensive summary of how families trade off school and peer quality. Second, the reduced-form analysis studies effects on demand for peer and school

[2]Only beliefs about schools in families' choice set were elicited.

quality in isolation, while this analysis can hold constant preference impacts for one quality measure while studying preference impacts for the other. Third, with information about mean biases in the population, 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 school quality; conversely, their willingness to travel for peer quality decreases. Specifically, their willingness to travel for a school with ten percentile points higher school quality increases by 0 to 0.7 kilometers, while their willingness to travel for better peer quality decreases by 0.4 to 1.4 kilometers. These findings align with the reduced-form results, with the impact measured in terms of the distance families are willing to travel. Spillover effects are mostly identical to treatment effects, a third and final piece of evidence highlighting the importance of social interactions. A decomposition of the results shows that most of the changes are driven by shifts in preferences, likely due to increased salience. This reflects the idea of bottom-up attention, as discussed by Bordalo et al. (2013) and Bordalo et al. (2022). Overall, both reduced-form and structural experimental results provide strong evidence that parents do indeed value effective schools and that social interactions play a strong role in influencing choices.

The final piece of analysis focuses on how information provision affected student outcomes. I consider both eleventh-grade test scores and socio-emotional outcomes similar to Jackson et al. (2020). The emphasis on both provides a more holistic perspective regarding the various ways schools potentially influence student outcomes. For test score outcomes, I am limited to one cohort due to the fact that students only take exams in eleventh grade, three years after the experiment. Because the pandemic severely interfered with the 2019 cohort's educational experience in high school, it is not surprising I do not find any test score impacts. Related to socio-emotional outcomes, I find student happiness improves, along with improvements in interpersonal skills, school connectedness, academic effort, and bullying. The effects are most pronounced for the second experimental cohort, the cohort with more pronounced effects on choices. Although I do not detect test score impacts in the first cohort, I do find sizable improvements in students' stated academic effort in the second cohort, potentially alluding to post-pandemic positive test score impacts in 2025. Overall, the intervention altered the schools some students attended, and this translated to better socio-emotional outcomes and may translate into positive test score impacts in the future.

Related Literature

The findings in this paper contribute to three strands of literature, with the most immediate related to parents' valuation of effective schools. Early studies from school choice lottery experiments show minimal impacts from attending most-preferred schools, suggesting that parents do not systematically select schools with higher value-added, or that school quality differences are negligible within local markets (Abdulkadiroğlu et al., 2014, Cullen et al., 2006, Deming et al., 2014, Lucas and Mbiti, 2014). More recent research has examined preferences using rank-ordered lists from centralized assignment systems, with mixed findings: some suggest parents highly value effective schools (Beuermann et al., 2022, Campos and Kearns, 2024), while others find little responsiveness to quality variation, with peer composition playing a larger role (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023). Despite advances in understanding preferences, most studies rely on revealed preference arguments, leaving room for misinterpretation due to imperfect information. This paper addresses this gap by providing the first evidence on the joint distribution of families' beliefs about peer and school quality in the United States, and offers experimental evidence on how families' choices shift under different information scenarios, mitigating concerns about information frictions.

A large body of research has used information interventions to address policy-relevant questions, particularly in education. Seminal work by Hastings and Weinstein (2008) highlights how information frictions affect school choice and outcomes, with subsequent studies emphasizing the importance of accessible information and addressing inequities in its uptake (Cohodes et al., 2022, Corcoran et al., 2018, Corradini, 2024). Additionally, participants' lack of awareness of mechanism rules is crucial (Arteaga et al., 2022), and recent work has explored the equilibrium effects of large-scale policies, underscoring the effectiveness of information interventions (Allende et al., 2019, Andrabi et al., 2017). However, most existing research, with the exception of Ainsworth et al. (2023), focuses on peer quality and does not differentiate between preferences for peer and school quality. This paper advances the literature by distinguishing between families' responsiveness to peer and school quality information, providing insights into their preferences, and decomposing treatment effects to better understand information provision mechanisms. It also sheds light on the broader implications of large-scale school-quality campaigns, including their potential impacts on school enrollment segregation (Corradini, 2024, Hasan and Kumar, 2019, Houston and Henig, 2021, 2023).

A third and emerging body of literature examines the role of peer preferences in the school choice process. Existing research has largely focused on how peer externalities shape demand systems, such as in Allende (2019), who uses a structural model to show how preferences for peers distort school incentives, building on insights from Rothstein (2006). Hahm and Park (2022) shows that students' school environment affects future preferences, alluding to a potential role of social interactions in preference formation. In market design, another strand of work has demonstrated that stable matchings may not exist when preferences are interdependent (Sasaki and Toda, 1996), while recent studies have explored the conditions for stable matchings when participants can express preferences for peer attributes (Cox et al., 2021, Leshno, 2021). This paper provides empirical evidence suggesting that peer preferences may not be significant in certain markets, aligning with findings from previous ZOC cohorts (Campos and Kearns, 2024). My results shift the focus of peer effects from externalities tied to peer composition toward those driven by information and social networks. The presence of social interactions in the school choice process, studied descriptively by Schneider et al. (2000), raises the possibility of network-based inequalities, a topic that has received limited empirical attention in the school choice literature and presents an opportunity for future research.

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 discusses the experiment's design in detail as well as the data and standard checks in the randomized control trials. Section 4 reports results from a reduced-form analysis of the intervention's impacts. Section 5 reports field survey evidence, while Section 6 returns to the experiment viewed through a discrete choice lens and incorporates the survey data. Section 7 analyzes the intervention's impact on student outcomes. Section 8 discusses the implications of the findings for future research, and Section 9 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.[3] 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 (2024) provide a more detailed description of the program's history and expansion in 2012.

The ZOC program does not cover the entire LAUSD district, with most zones concentrated in Central, South, and East Los Angeles, extending as far south as Narbonne and as far north as Sylmar in the San Fernando Valley. While LAUSD is predominantly Hispanic (68%), ZOC neighborhoods have an even higher concentration, with 86% of students identifying as Hispanic. Additionally, 90% of ZOC students are classified as poor, and their parents are less likely to have college degrees. This relative homogeneity of students in ZOC markets distinguishes the program from other controlled choice initiatives (Orfield and Frankenberg, 2013).

Families residing within ZOC boundaries apply to high schools during the fall semester of their students' eighth-grade year, a period when ZOC administrators and guidance counselors make the application process highly salient. Failure to apply can result in an assignment to an undesirable school outside the neighborhood, incentivizing families to participate. To support this, district administrators and high schools dedicate significant time and resources to inform parents about the program. Administrators visit middle schools to facilitate applications and hold information sessions to explain the process and available options, while high schools host open houses to recruit students. In previous years, the district also experimented with sending mailers to raise awareness among families. Despite these efforts, the informational landscape for ZOC families remains fragmented. Schools produce promotional videos, but their dissemination is unclear, and school performance data, such as achievement levels and growth metrics, are buried on a district webpage. The ZOC office does not actively promote these quality measures, leaving families with limited access to important information.

The ZOC office assigns students to schools using the immediate acceptance mechanism, also known as the Boston mechanism (Abdulkadiroğlu and Sönmez, 2003), which takes neighborhood and sibling priorities into account but lacks the additional priorities or screening strategies

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

seen in cities like New York (Cohodes et al., 2022, Corcoran et al., 2018). Although families can list as many schools as they want, avoiding some common constraints in other systems (Calsamiglia et al., 2010, Haeringer and Klijn, 2009), the mechanism is not strategy-proof. Families are incentivized to misreport their preferences to avoid being placed in a lower-ranked school (Abdulkadiroğlu and Sönmez, 2003).

However, strategic behavior is limited in ZOC markets, as many programs are undersubscribed because of district-wide declining enrollment.[4] In fact, roughly three-quarters of applicants face no admission risk at their most-preferred programs, effectively removing the need for strategic misreporting. When families are guaranteed admission to their top choice, the incentives to manipulate rankings disappear. Moreover, ZOC requires families to rank all available options in their zone, providing a complete and mostly strategy-immune ranking. Data from the ZOC office confirms that between 2019 and 2024, at least seven markets were consistently undersubscribed, ensuring that every applicant was assigned to their top-listed option. This widespread undersubscription significantly alters the strategic incentives typically associated with school choice mechanisms, as declining enrollment has created an environment where families' rankings reflect genuine preferences rather than strategic manipulation.

3 Experimental Design

All families with eighth-grade students enrolled at ZOC feeder middle schools are part of the experimental sample. These families participate in the application cycle, which includes information sessions and interactions with ZOC field administrators. The field experiment is augmented to the application cycle in 2019 and 2021.

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 the information intervention and application submission allows parents to internalize the information and deliberate among themselves. After the deliberation process, parents submit applications, and the intervention is completed.

School and Peer Quality Definition

Notions about school and peer quality are central to the intervention's goals. School quality corresponds to a school's effectiveness in improving student achievement, while peer quality pertains to the average ability or characteristics of the school's student body. However, measuring and conveying these qualities in a field experiment presents two significant challenges.

[4] From the peak in 2004, enrollment in LAUSD has fallen by nearly 50 percent.

The first challenge lies in defining and accurately measuring school and peer quality. Researchers typically rely on value-added models (VAMs) to estimate these qualities, where school quality is captured by the school's contribution to student achievement, controlling for prior performance, and peer quality is assessed through the average ability of students attending the school. For this paper, the measures of school and peer quality are conceptually tied to a constant effects potential outcome model of achievement.[5] Peer quality is calculated as the implied average ability of students enrolling in schools with estimates derived from a model described in Appendix B, and school quality is the estimated school value-added from the same model. Given the lack of quasi-experimental variation in school assignments, the model is estimated via ordinary least squares.[6] Equipped with validated school and peer quality estimates, 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 for the beliefs elicited in the baseline survey.[7]

The second, and perhaps more consequential, challenge is effectively conveying the distinction between school and peer quality to parents. While researchers might have clear definitions rooted in statistical models, parents may interpret these terms differently, often conflating peer quality with overall school quality. To address this, I avoid using terms such as value-added, peer quality, and school quality. Instead, the terms *Achievement Growth* and *Incoming Achievement* are used to represent school and peer quality, respectively. The choice of terms is based on the piloting of different phrases with parents at an earlier stage. However, the labeling of peer and school quality alone does not suffice to surmount the messaging challenge. To further address this, I employ pedagogical videos that can clarify these concepts by presenting school and peer quality in terms parents can easily grasp. I discuss these in the following section.

Pedagogical Videos

Ensuring that parents comprehend the distinction between school and peer quality is crucial at multiple stages of the study. During the baseline survey, it's essential for parents to grasp these differences so that their expressed beliefs reflect a meaningful understanding. Similarly, for the treatment phase, clear comprehension is necessary to ensure that the information provided influences decision-making effectively.

To address these challenges, I use pedagogical videos in the baseline survey and the treatment letters. These videos were designed to visually communicate the differences between the two quality measures–Incoming Achievement (IA) and Achievement Growth (AG)–to ensure

[5]This paper omits potential match quality. In general, there is mixed evidence about the empirical relevance of match quality, with Bau (2022) finding important equilibrium implications. Other evidence in the United States tends to find it explains a relatively small share of the variation in outcomes (Abdulkadiroğlu et al., 2020, Campos and Kearns, 2024), with more recent evidence of its importance for the choice between remote and in-person instruction (Bruhn et al., 2023).

[6]Campos and Kearns (2024) find that school quality is forecast unbiased in Los Angeles, and I report similar findings in Appendix B.

[7]Peer effects potentially influence school quality estimates. In Appendix B, I show that a variety of student covariates are unrelated to value-added estimates. In addition, I report the rank-rank correlations between the estimates I use and estimates that regression-adjust, showing both measures produce qualitatively similar results. The two pieces of evidence demonstrate that peer effects are not a first-order concern in this setting, contributing to the mounting mixed evidence regarding peer effects on academic achievement (Sacerdote, 2014).

parents could accurately interpret the information presented. This approach mirrors recent work by Stantcheva (2022) using pedagogical videos before eliciting respondents' perceptions and opinions. In the field experiment, the pedagogical videos play an instrumental role in improving the quality of the elicited beliefs by being displayed before elicitation and in helping parents understand the information contained in their treatment letters.

The videos, lasting approximately two minutes, were crafted to reinforce the distinctions between IA and AG through clear visual aids and straightforward explanations. The survey provided a QR code for accessing the video, while the digital version embedded it directly before the section where respondents were asked about their beliefs. The treatment letters contained QR codes that mapped to treatment-specific videos. Figure 1 showcases relevant frames from the video all participants viewed when completing the survey, each designed to emphasize key points.[8]

Frame (a) begins by establishing the video's credibility, showing that it was produced in collaboration with the Zone of Choice (ZOC) and the Los Angeles Unified School District (LAUSD). Frame (b) introduces the terms Incoming Achievement and Achievement Growth, setting the stage for the explanation of each concept. Frame (c) explains that peer quality is associated with the achievement levels of students as they enter the school, illustrated with a graphic depicting students entering a school building. This visual reinforces the idea that peer quality is a measure of the student body's starting academic level. Frame (d) introduces school quality as a measure of academic progress that occurs during a student's time at the school. A dynamic graphic showing student progress visually supports this concept, emphasizing the ongoing nature of achievement growth. Frame (e) highlights the distinctions between peer and school quality, ensuring viewers understand they are separate and distinct measures. Importantly, the video remains neutral, avoiding suggesting that one measure is more important than the other. Finally, Frame (f) broadens the perspective by reminding families to consider other non-test-score-based attributes of schools, suggesting that while peer and school quality are important, they are not the only factors to weigh when choosing schools.

Baseline Survey

The survey was designed with two primary objectives. First, it aimed to gather insights into parents' awareness of the Zone of Choice (ZOC) program, their available school options, and the factors that influence their school choice decisions. Despite the program's decade-long existence and its neighborhood-based structure, some parents may still be unaware of the full range of options it provides. Second, the survey serves as a crucial tool for the empirical analysis, providing baseline data on parents' beliefs and preferences. This data is not only descriptive, highlighting the prevalence of information gaps regarding school attributes, but also instrumental in decomposing the factors that drive changes in school choice behaviors.

The survey's distribution method evolved over the course of the study. In the first wave, the survey was distributed solely in paper to students in their eighth-grade homeroom classrooms. In the second wave, both paper and digital versions were offered.[9] The digital version was delivered

[8] To see the video in English, go here, and to see the video in Spanish, go here.

[9] Each year, LAUSD administers the School Experience Survey to all students and parents. Based on that

to families through internal district messaging services. While the mode of distribution changed between waves, the survey questions remained consistent. Unfortunately, efforts to digitize the paper surveys in the first wave resulted in insufficient data quality, leading to a focus on the second wave's digital survey responses in this analysis.

The baseline survey targeted all eighth-grade students enrolled in ZOC feeder middle schools, specifically those whose parents had a cell phone number on record with the district. In the second experimental wave, this amounted to approximately 10,600 students, of whom around 5,400 responded to the digital survey. Notably, 77% of these respondents completed the entire survey, including the sections measuring beliefs. The survey, available in both Spanish and English, was conducted in collaboration with LAUSD, the ZOC office, and researchers, with the intent of collecting data that would inform future district practices. Descriptive statistics comparing respondents and non-respondents can be found in Appendix Table D.2.

Treatment Letters

Families with children enrolled in treated feeder schools may receive treatment letters designed to convey crucial information about school and peer quality, referred to in the letters as Achievement Growth and Incoming Achievement, respectively–terms consistent with those used in the survey. The content of these letters varies: some families receive information about Incoming Achievement, others about Achievement Growth, and a subset receives details on both measures.

Figure 2 illustrates sample treatment letters for the Bell Zone of Choice, available in both English and Spanish. The design of these letters follows a format similar to those used in prior studies (Corcoran et al., 2018, Hastings and Weinstein, 2008). Each letter begins with a brief description of its content, followed by a list of schools specific to the recipient's zone. A notable innovation in these treatment letters is the randomized order of schools within the list. This randomization is intended to detect and control for potential order biases, a factor that may have influenced treatment effect estimates in previous research.

In addition to the examples shown in Figure 2, there are two other versions of the letters that focus on a single measure of quality, either Incoming Achievement or Achievement Growth; these are shown in Appendix Figure A.1 and Appendix Figure A.2. The next section discusses the randomization process and details how different families are assigned to receive these various versions of the treatment letters.

Randomization

The randomization strategy is designed to answer two questions: First, how responsive are parents' school choices to different measures of school quality? Second, how significant are social interactions in the school choice process? To explore the role of social interactions, I utilize a two-stage randomization procedure commonly employed in spillover studies (Andrabi et al., 2020, Crépon et al., 2013). The core idea behind spillover designs is to compare control group participants who are in close proximity to treated participants with those who are not, thereby isolating any effects arising from social interactions. In this context, spillovers refer to the

experience, the district believed a paper survey would yield the highest response rate. However, this assumption proved incorrect, and the paper surveys posed significant challenges in digitization.

diffusion of information from treated to untreated parents, potentially influencing their school choices. To examine parents' responsiveness to school quality information, I cross-randomize the information provided about peer and school quality, enabling an assessment of which aspects of quality most influence parental decisions.

The randomization process unfolds within distinct Zone of Choice (ZOC) markets or zones, each considered a separate experiment. These zones comprise different middle schools that feed into the same set of high schools, creating a shared market of school options for students. The randomization is executed in two stages: first at the school level and then at the individual level. Within each zone, feeder middle schools are grouped and randomly assigned to one of three categories: high-saturation, low-saturation, or pure control.[10]

In the first stage, feeder middle schools are assigned to either high-saturation, low-saturation, or pure control groups. Saturation levels indicate the proportion of parents within a school who receive information about a specific quality measure, with high saturation corresponding to 50% and low saturation to 30%. This creates a market-specific experiment within each zone, with two treatment levels, high (H) and low (L).

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, the second stage of randomization is conducted at the individual level. Here, the specific information treatments (school and peer quality) are cross-randomized based on the assigned saturation level of the school. 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 3 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.

[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.

Data and Experimental Sample

In addition to the survey data I collect, the data used in this paper is drawn from a combination of administrative records provided by the Los Angeles Unified School District (LAUSD), survey data collected by LAUSD, and application data provided by the Zones of Choice (ZOC) office. These comprehensive data allow for a detailed examination of both application behaviors and educational outcomes.

The administrative data from LAUSD includes standard variables typically found in school district records, such as demographic variables and cognitive outcomes, particularly test scores. These variables are crucial for analyzing students' academic performance and progression through the school system. In addition to the administrative data, the analysis incorporates non-cognitive outcomes derived from the School Experience Survey (SES), which has been administered annually by LAUSD since 2010. These survey data capture important aspects of students' non-cognitive skills and experiences, similar to the data utilized in studies of other large urban districts like Chicago (Jackson et al., 2020) and Los Angeles (Bruhn et al., 2023).

The ZOC office provides critical data on applications to the program, specifically the rank-ordered lists submitted by families to the centralized assignment system. These application data serve as key outcomes when examining how information influences school choice behavior. Additional information contained in these data allows for a replication of the assignment of students to schools, which allows us to simulate admissions probabilities to programs, demonstrating most programs are undersubscribed.[12]

The experimental sample includes students attending a feeder middle school during their eighth-grade year. In 2019, this sample consisted of 13,015 students, with slightly fewer in 2021.[13] It is important to note that these students are not a random sample of the broader LAUSD population.

Table 1 presents descriptive statistics for eighth-grade students enrolled in LAUSD schools in the fall of 2019. The typical ZOC student differs notably from other eighth-grade students in the district. For example, ZOC students enter high school performing approximately 22% of a standard deviation lower on math and reading assessments compared to their non-ZOC peers. Socioeconomically, only about 12% of ZOC parents hold a four-year degree, and 94% of ZOC students are classified as economically disadvantaged. Additionally, ZOC students are more likely to be English learners. Racial and ethnic differences are also pronounced: 90% of ZOC students are Hispanic, compared to 64% in the rest of the district. These demographic and socioeconomic characteristics have been consistent across past cohorts studied, as noted in Campos and Kearns (2024). While ZOC students differ substantially from the broader LAUSD population, the treatment assignment for this study is conducted within the experimental sample.

[12] In fact, declining enrollment has affected Zones of Choice schools so much that in many zones, everyone gets assigned their top-listed program.

[13] These counts reflect assignments made just before the start of the semester. While some students may transfer afterward, attrition is minimal.

Balance

Table A.2 reports balance for the school-level randomization. Across 104 feeder-year middle schools, 32 get randomly assigned to the low-saturation treatment, 31 get randomly assigned to the high-saturation treatment, and 41 remain as pure control schools. There are minimal differences between treated and pure control schools across an array of school attributes, including 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 A.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.

Complementary Online Survey

I complement the field experiment with an online survey of parents across a broader national sample. The survey aims to build on the field experiment by providing more detailed insights related to the key questions this paper poses. It closely follows the field experiment in that parents watch similar educational videos that explain school and peer quality differences. Afterward, their beliefs are assessed and compared to objective measures like Great Schools Test Score and Progress ratings, which reflect peer and school quality. The survey also includes choice experiments to experimentally estimate how far parents are willing to travel for better school or peer quality, which is assessed after watching the pedagogical videos. Finally, a set of descriptive questions explores why social interactions might affect the school choice process, providing richer insights into why social interactions may matter empirically. More details on the survey are available in Appendix E.

4 Reduced-Form Evidence

In this section, I begin by reporting experimental difference-in-difference estimates, where I initially do not distinguish between different treatment types and emphasize cluster-specific effects and corresponding spillover effects. I then focus on models that ignore saturation clusters but do distinguish between treatment types. The combination of reduced-form results emphasizes the importance of social interactions from different perspectives. Additional evidence is reported in Appendix C.

4.1 Difference-in-Differences

I organize the empirical analysis in a difference-in-differences model that compares changes in outcomes between treated—both direct and indirect—parents and parents in pure control schools. There are a few advantages to the difference-in-differences approach. To begin, there is a boost in statistical precision due to the absorption of time-invariant unobserved preference heterogeneity across treatment groups. Second, there are convenient falsification tests that implicitly test for balance on pre-intervention trends in outcomes of interest. For a given outcome Y_i, I consider the following specification

$$\begin{aligned} Y_i = \alpha_{z(i)t(i)} + \alpha_{g(i)} + \gamma' X_i + \sum_{k \neq -1} \Bigg(& \underbrace{\beta_{Lk} D_{L(i)} \times Post_{k(i)} + \beta_{Hk} D_{H(i)} \times Post_{k(i)}}_{\textit{High and Low Treatment Groups}} \\ & + \underbrace{\psi_{Lk} C_{L(i)} \times Post_{k(i)} + \psi_{Lk} C_{H(i)} \times Post_{k(i)}}_{\textit{High and Low Spillover Groups}} \Bigg) + u_i \end{aligned} \tag{1}$$

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, $C_{L(i)}$ and $C_{H(i)}$ are low- and high-saturation spillover group 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 experimental wave in 2019 for low- and high-saturation groups, respectively, and ψ_{Lk} and ψ_{Hk} are defined similarly for parents in the spillover group. All parameters are identified by comparing changes in application behavior between applicants in the respective groups and applicants in pure control schools. Standard errors are robust and clustered at the school level, allowing for correlation of preferences within schools and following inference suggestions in Breza (2016) and precedent (Andrabi et al., 2020, Crépon et al., 2013). Appendix C reports randomization inference-based p-values based on sharp null hypotheses of no treatment effects and inference conclusions are similar.

Figure 4 reports estimates of Equation 1, considering top-ranked school incoming achievement and achievement growth as outcomes. In both panels, gray lines correspond to estimates of effects for those in low-saturation schools, and maroon lines correspond to effects for those in high-saturation schools. Dashed lines correspond to treated applicants and solid lines correspond to spillover applicants.

Panel (a) reports effects on most-preferred achievement growth. The maroon lines demonstrate that applicants in high saturation schools increased their demand for schools with higher AG in both experimental waves. Both direct and indirect treatment effects are similar, with larger effects in the second experimental wave. In contrast, the gray lines demonstrate no effects among applicants in low-saturation schools. Across all groups, there is no evidence that treated groups' application behavior trended differently leading into the intervention. Turning to Panel (b), the evidence shows that demand for peer quality was unaffected by the intervention. Appendix Figure C.4 and Appendix Figure C.5 report analogous findings with randomization-based inference.

The results in Figure 4 emphasize two findings. First, any meaningful changes in demand are driven by an increase in demand for more effective schools, as captured by achievement growth rankings. This finding is corroborated by descriptive evidence shown in Appendix Figure D.1 showing that parents report caring more about test score growth than the academic achievement of peer students. Second, social interactions are an important factor contributing to meaningful changes in demand. The importance of social interactions operates through two channels. In the high saturation schools, social interactions facilitated changes in choices among control group parents. In low-saturation schools, the lower prevalence of social interactions led to both treated and untreated parents' lower take-up of the information. This latter finding mirrors the importance of social engagement with information in generating meaningful changes in behavior (Banerjee et al., 2018).

Table 2 reports treatment effects on other school attributes potentially correlated with school incoming achievement and achievement growth. I do not find evidence that changes in demand for school quality substantially alter other demand for other top-listed school attributes, suggesting that the information did not alter families' perceptions about other school attributes in a way that generated changes in demand for those attributes. Appendix Section C.1.1 further assesses treatment effect heterogeneity, finding little evidence of meaningful treatment effect heterogeneity.

4.2 Distributional Estimates

The findings reported in Figure 4 and Table 2 do not distinguish between information arms, masking the fact that treated families received different information. In this section, I consider a specification that distinguishes between treatment types and assesses how demand for achievement growth and incoming achievement changed across the distribution. I consider distributional regressions of the following form

$$\mathbf{1}\{Y_i \leq a\} = \alpha_{z(i)t(i)} + \alpha_{g(i)} + \gamma' X_i + \beta_P T^P_{it(i)} + \beta_S T^S_{it(i)} + \beta_B T^B_{it(i)} + \beta_C C_{it(i)} + u_i, \quad a \in [\underline{a}, \bar{a}] \tag{2}$$

where $\mathbf{1}\{Y_i \leq a\}$ is the cumulative distributive function of an outcome Y_i at point a point a, α_z is a zone fixed-effect, $T^x_{it(i)}$ are individual-level treatment x indicators for $x \in \{P, S, B\}$, C_{it} are individual-level indicators for untreated parents in treated schools in cohort t, and X_i is a vector of baseline covariates. As in the differences-in-differences model from the previous section, all parameters are identified by comparing changes between treated families and families in pure control schools. Standard errors are robust and clustered at the school level and randomization-based inference is reported in Appendix C.

Figure 5 reports estimates of Equation 2. Panel (a) begins by demonstrating impacts across the most preferred school peer quality across different percentile rank points. At a given point, the estimate reveals the direction and magnitude the cumulative distribution function shifted. For example, at 40, the probability that a most-preferred school peer quality ranking was below the 40th percentile increased by approximately seven percentage points for the families receiving AG, an indication that families were ranking lower-ranked schools in terms of peer quality at the top of their ROL. Treatment effects are remarkably similar across the various treatment groups, including the spillover group, underscoring the strength of social interactions. Overall, families tended to shift their most preferred school choices to schools with lower peer quality, with much less pronounced changes in markets with higher peer quality schools. While Panel (a) detects that families shifted their choices toward schools with lower peer quality, these changes are coupled with increased demand for higher school quality schools as Panel (b) demonstrates. Similar to impacts on most preferred peer quality, the treatment effects of untreated parents in treated schools mirror the effects of treated parents. The striking visual evidence in Panels (a) and (b) suggests a community-level convergence in preferences moving average demand in a way that rewards effective schools. Appendix Figure C.6 and Appendix Figure C.7 report analogous figures with randomization-based inference.

4.3 Interpreting Changes in Schooling Decisions and Social Interactions

The preceding evidence suggests that imperfect information about school effectiveness is empirically significant as families adjust their choices following information provision. This has been underscored in Ainsworth et al. (2023) and suggested in earlier work by Rothstein (2006), Abdulkadiroğlu et al. (2020), and Beuermann et al. (2023). The two new findings relative to the existing literature correspond to relative changes in demand following information provision and the empirical relevance of social interactions. To further corroborate and interpret the field experiment findings, I use the complementary online survey to provide additional insights. See Appendix E for additional details related to the sample and findings.

I interpret the evidence in Figure 4 and Figure 5 as showing that when information about both peer and school quality is available, families systematically choose more effective schools without significant changes in their demand for peer quality. This indicates that effectiveness-oriented campaigns can steer demand so parents reward effective schools, potentially influencing school competition and student outcomes. Appendix Figure E.3 shows that roughly 80 percent of parents indicate a stronger preference for school quality than peer quality after watching similar pedagogical videos as in the field experiment. Experimental estimates of marginal willingness to travel for peer and school quality reported in Appendix Figure E.4 show that willingness to travel for school quality is 28 percent larger than willingness to travel for peer quality, showing that, as in the field experiment, parents tend to exhibit stronger demand for higher value-added schools after learning about peer and school quality. Overall, the online survey and field experiment demonstrate that once parents are informed about the differences between school and peer quality, they show a stronger preference for school quality. The field experiment and only survey findings suggest that most of the existing evidence documenting a stronger preference for peer quality may have been a product of imperfect information. It is evident that both in the field and laboratory settings, parents clearly tilt their demand toward more effective schools.

Social interactions play a critical role in shaping school choice decisions. While previous research has provided anecdotal and qualitative evidence on the influence of social networks in this process (Fong, 2019, Kosunen and Rivière, 2018, Schneider et al., 2000), the reduced-form evidence in the previous section offers the first causal insights into how these interactions affect parental decision-making. The field experiment demonstrates the significance of social interactions in actual school choices, while complementary survey evidence sheds light on the underlying mechanisms.

The field experiment suggests that parents with fewer peers to discuss the provided information were less likely to use it, highlighting the importance of validation and interpretation through social interactions. In other words, other parents play a key role in reinforcing and making sense of school-related information. To explore this further, the national survey asked parents about their use of district-provided information after watching similar videos to those in the ZOC experiment. They were also asked about their reliance on social networks during their school search process. Appendix Figure E.5 shows that 72 percent of parents talked to other parents as part of their research. When it came to district-provided information, Appendix Figure E.6 shows that 70 percent were more likely to trust or be influenced by the information after discussing it with other parents. Notably, Appendix Figure E.7 reveals that 83 percent relied on social interactions to help distill and interpret the information, emphasizing credibility. In contrast, explanations related to the coordination of preferences or direct influence from others—often linked to herding behavior—were much less common. The field experiment supports this conclusion, as Appendix C.2 shows a low rank concordance in parents' reported preferences, suggesting little coordination, with no significant effect from the experiment. Overall, both the online survey and field experiment indicate that social interactions are more about interpretation and credibility than coordination.[14]

5 Field Survey Evidence

How prevalent are information frictions about school and peer quality in ZOC markets? The baseline field survey elicited preferences and beliefs about school and peer quality.[15] I first focus on descriptive evidence of elicited preferences and beliefs in this section. To underscore the empirical importance of biases, I show suggestive evidence that biases lead to choice-relevant mistakes. I then return to the experiment, combining the survey results with a slightly more structural approach to corroborate the reduced-form evidence and 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 $\tilde{Q}_{ji}^x$. Both researcher-generated measures and beliefs are measured in decile units. The biases are

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

5.1 Descriptive Evidence

Figure 6 reports evidence related to parents' mean school and peer quality beliefs and bias. Beliefs about schools in each parent's zone-specific choice set were elicited. For example, parents with a child in a school that feeds into the Bell Zone of Choice were only asked about high schools in the Bell Zone of Choice, as displayed in the example treatment letter shown in Figure 2. This ensures that parents are surveyed about schools they are more likely to be aware of and avoids asking them about schools they would not consider enrolling in.

Panel (a) of Figure 6 illustrates the average beliefs for each position on the rank-ordered list

[14]Another piece of evidence from the field experiment consistent with the social interaction mechanisms associated with credibility and learning is found in Appendix C.1.1. Parents with lower-achieving students had larger treatment effects than parents with higher-achieving students, and this differential is most pronounced in high-saturation schools. This suggests that the parents who likely needed the most reinforcement interpreting and engaging with the information did so the most when there were enough parents nearby to engage with them.

[15]See Appendix Table D.2 for a characterization of survey respondents. Additional questions revealed information about parents' intentions during the school choice process, which are discussed in detail in Appendix D.

(ROL). It shows that parents have higher opinions of the schools they rank at the top of their list and lower opinions of those ranked further down. On average, parents rate their schools higher in terms of Achievement Growth, and these perceptions are generally accurate. For both school and peer quality, parents typically rank their schools above the district median. While this perception is often correct for school quality, it is usually incorrect for peer quality.

Panel (b) of Figure 6 depicts the average level of pessimism for each position on the ROL. Throughout the list, parents tend to be more pessimistic about school quality than peer quality. Their pessimism increases for schools ranked lower on their list, with a slightly stronger pattern for Achievement Growth. Parents are optimistic about both school and peer quality for their top-ranked choices. However, while they remain optimistic about peer quality throughout the list, their optimism about school quality shifts to pessimism starting at the third-ranked option.

To summarize the variation in pessimism among parents, Figure 7 presents a histogram of elicited pessimism for both peer and school quality. On average, parents tend to underestimate school quality and slightly overestimate peer quality. Approximately 50 percent of parents underestimate school quality, while only 34 percent underestimate peer quality. These trends are not due to central tendency bias; Appendix Figure D.4 demonstrates the overlap between estimated deciles and elicited belief deciles.[16]

Appendix Table D.4 and Appendix Table D.5 report additional correlations between top-listed school belief biases and student baseline covariates. Appendix Table D.5 focuses on absolute bias. College-educated and parents with higher-achieving students tend to have lower absolute peer quality bias, while low-income and Hispanic parents tend to have higher absolute peer quality bias. Parental education, low-income status, and student achievement are most predictive of peer quality bias.

5.2 Choice-Relevant Biases

Are the reported biases choice-relevant? Appendix Figure D.5 and Appendix Figure D.6 demonstrate that biases affect choice set-specific ordinal rankings of peer and school quality. Extending Larroucau et al. (2024), I define a valuation mistake with respect to a vector of attributes (Q_j^P, Q_j^S) as a mistake induced by biases with respect to the vector (Q_j^P, Q_j^S). If a rank-ordered list submitted using beliefs $\tilde{Q}_{ji}^P$ and $\tilde{Q}_{ji}^S$ differs from a rank-ordered list an applicant would submit using Q_j^P and Q_j^S, then that is an application mistake. Appendix Figure D.7 demonstrates that biases generate substantial shares of application mistakes across the rank-ordered list, implying that these biases are choice-relevant.[17]

In summary, there is substantial heterogeneity in beliefs about schools in families' choice sets as displayed in Figure 7. There is additional heterogeneity across the positions of the rank-ordered list. Mean bias, however, is not drastically large, indicating families do a decent job of predicting the quality of their schools along both dimensions, on average. Documenting the

[16] The figure shows a substantial overlap between beliefs about school quality and measured school quality, and to a lesser extent, this is also true for peer quality.

[17] This exercise takes a stand on the source of valuation mistakes, so it is suggestive. Ainsworth et al. (2023) conduct analyses in a similar spirit to show that belief biases are choice and welfare-relevant. A more recent paper by Agte et al. (2024) further quantifies how misperceptions about school attributes affect search behavior and the welfare implications of such misperceptions.

presence of imperfect information points to one channel explaining the reduced-form effects in Section 4, but the survey evidence does not speak to the role of salience or the phenomenon where families reprioritize the importance of attributes due to the information intervention. In the next section, I transition to a standard discrete choice setting that allows me to discern between the two likely channels, salience and information.

6 Discrete Choice Evidence

In this section, I return to the intervention and analyze it's impacts through a discrete choice lens. This allows me to provide a corroborating perspective to the reduced-form evidence with a few advantages. To begin, this analysis uses information contained in the entire rank-ordered list as opposed to just the most preferred options. Discrete choice models also allow me to hold constant changes in willingness to travel for one quality measure while studying changes in willingness to travel for another. Last, combined with a few additional assumptions, I can provide suggestive evidence regarding the intervention's mechanisms.

6.1 A Simple Model with Information Provision

Families are indexed by $i \in \mathcal{I}$ and schooling options by $j \in \mathcal{J}_{z(i)}$ where $z(i)$ corresponds to family i's zone-specific choice set. The indirect utility of family i being assigned school j is

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

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:

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

The school district distributes information to a subset of families, randomizing the families who receive information and the information they receive (see Section 3 for intervention details). Let $\mathcal{I}_P$ and $\mathcal{I}_S$ be the set of families receiving peer quality and school quality information, respectively, and let $\mathcal{I}_B$ correspond to the families receiving information about both. The effects of the information campaign can be summarized by changes in the weights families assign to peer and school quality. In particular,

$$U_{ij} = \underbrace{\gamma_P Q_j^P + \gamma_S Q_j^S + \sum_{t \in \{P,S,B\}} (\beta_{Pt} Q_j^P + \beta_{St} Q_j^S) \times \mathbf{1}\{i \in \mathcal{I}_t\} - \lambda d_{ij}}_{V_{ij}} + \varepsilon_{ij} \tag{3}$$

where β_{St}, β_{Pt}, and β_{Bt} summarize the average change in weights treated families assigned to the various quality measures. The utility weight impacts can be translated into a marginal willingness to travel changes by scaling by the distance distaste coefficient.

The quantities of interest are the average marginal willingness to travel for control and treatment parents. Take, for example, the average marginal willingness to travel for peer quality.

Through the lens of the model, parents in the control group have average marginal willingness to travel,

$$MWTT = \frac{\gamma_P}{\lambda},$$

and parents that receive peer quality information have average marginal willingness to travel,

$$MWTT = \frac{\gamma_P + \beta_{PP}}{\lambda}.$$

I assume applicants reveal their preferences truthfully and $\varepsilon_{ij} \sim EVT1 \mid (Q_j^P, Q_j^S, \mathbf{1}\{i \in \mathcal{I}_P\}, \mathbf{1}\{i \in \mathcal{I}_S\}, \mathbf{1}\{i \in \mathcal{I}_B\}, d_{ij})$, a common assumption in the discrete-choice literature and reasonable in a setting where applicants face little admissions uncertainty. The preference profile for each applicant is as follows:

$$R_{ik} = \begin{cases} \arg\max_{j \in \mathcal{J}_{z(i)}} U_{ij} & \text{if } k = 1 \\ \arg\max_{j: U_{ij} < U_{iR_{ik-1}}} U_{ij} & \text{if } k > 1 \end{cases}, \tag{4}$$

where $R_i = (R_{1i}, \cdots, R_{iZ(i)})$ is the rank-ordered list (ROL) that applicant i submits. The conditional likelihood of observing list R_i is

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{ij}}}{\sum_{\ell \in \{r | U_{ir} < U_{iR_{ik-1}}\}} e^{V_{i\ell}}}. \tag{5}$$

Equation 5 is aggregated across individuals to construct the complete likelihood and we estimate the utility specification's parameters via maximum likelihood. While truth-telling may seem like too strong of an assumption, evidence discussed in Section 6.4 reveals that strategic considerations are less of a concern in ZOC markets.

6.2 Results

Table 3 summarizes the intervention's impacts. The first two columns report willingness to travel estimates (in kilometers) for the control group and changes in willingness to travel for the various treatment groups. The third column reports a p-value from a test where the null hypothesis is that the estimates in Columns (1) and (2) are equal in a given row.

The first two rows of Columns (1) and (2) show that untreated families tend to place a positive weight on peer and school quality, with a higher weight on school quality that is statically different from the weight on peer quality (p-value = 0.017). This finding mirrors previous findings documented for earlier ZOC cohorts in Campos and Kearns (2024) but is distinct from findings in New York from Abdulkadiroğlu et al. (2020) and in Romania from Ainsworth et al. (2023). The conditions affecting the school choice process likely vary across settings and help explain the diverse findings. For example, in ZOC markets, there is much less pronounced variation in race and socioeconomic status, a common proxy for peer quality, potentially reducing the effective weight families place on peer quality.

The subsequent rows show that families receiving information reduce their willingness to travel for peer quality and increase their willingness to travel for school quality, regardless of the information treatment they receive. Mirroring the reduced-form evidence, the ninth and tenth rows of Table 3 show robust evidence of spillovers with effects statistically equal to information effects. The evidence also reveals that willingness to travel impacts on peer quality are statistically similar, regardless of the information treatment (p-value=0.73); the same is true for willingness to travel impacts on school quality (p-value=0.19). Overall, the evidence in Table 3 demonstrates that families responded to information about school quality and peer quality by changing their choices in a way that increases schools' incentives to invest in factors that contribute to student learning.

It is worth noting that the parsimonious model used to estimate impacts on utility weights potentially fails to account for changes along other dimensions. Although the evidence in Table 2 suggests otherwise, the intervention may have changed beliefs about other school attributes, and the parsimonious model does not account for this directly. To explore this possibility, in Appendix Figure C.3, I report the reduced form effects implied by the corresponding model in Table 3. I first construct new rank-ordered lists using the indirect utility estimates obtained by summing the estimated systematic component of utility and random draws of the unobserved preference heterogeneity, and then I estimate reduced form effects as in Figure 4. The treatment effects are identical, providing suggestive evidence that the intervention mostly influenced the relative weights of the family assigned to peer quality or school quality. If other important omitted factors featured prominently in parents' decisions, the model would do a poor job replicating the reduced-form results. Given the model's good predictive validity of reduced form effects, I now turn to decomposing the various potential forces governing changes in choices.

6.3 Information and Salience Decomposition

In a setting where families are perfectly informed about school and peer quality, the marginal willingness to travel changes are due to families re-prioritizing the importance of each, which I refer to as salience (Bordalo et al., 2013, 2022).[18] In a setting with imperfect information, marginal willingness to travel changes reflect both information and salience effects. Distinguishing between the two channels is challenging without additional data, so additional assumptions are necessary.

The simplifying assumptions are more thoroughly outlined in Appendix F and summarized intuitively here. The key assumption is that treated families perfectly update their beliefs. That is analogous to them receiving a signal without noise or a perfect compliance assumption, an assumption that likely overstates the information effect. Equipped with that assumption, we can decompose experimentally identified treatment versus control comparisons into an information and a salience channel.

Let μ_P and μ_S correspond to the mean peer and school quality bias measured in the field survey. Appendix F shows that the estimated change in the average marginal willingness to

[18]Three salience mechanisms are discussed in Bordalo et al. (2022). The framework discussed above is most closely related to the prominence channel. The prominence channel indicates that an information intervention will make attributes related to the intervention more prominent in the decision maker's choice, causing a reorientation of their relative importance.

travel for peer quality among families that receive the peer quality treatment is

$$\Delta MWTT_P = \frac{\beta_{PP} - \gamma_P \mu_P}{\lambda}, \tag{6}$$

and the average change in the marginal willingness to travel for school quality among families receiving the school quality treatment is

$$\Delta MWTT_S = \frac{\beta_{SS} - \gamma_S \mu_S}{\lambda}. \tag{7}$$

The compliance assumption allows us to pin down the portion of the change governed by the baseline bias in the population, which is identified in the survey. That then allows us to distinguish between the information and salience channel. It is important to emphasize that this decomposition is suggestive as it relies on a strong information updating assumption, likely overstating the degree of information updating and affecting the estimated salience channel. It is nonetheless important to distinguish between the two channels as they have differing policy implications for information interventions more generally.

Figure 8 reports estimates of the decomposition. Panel (a) reports estimates of the decomposition among parents receiving treatments and Panel (b) corresponds to parents in the spillover group. The first two bars in each figure correspond to peer-quality MWTT treatment effects, while the subsequent two bars correspond to school-quality MWTT treatment effects. The estimated information updating component is represented by the gray bars and the salience component is represented by the black bars. The takeaway from Figure 8 is that salience effects explain most of the changes in choices, a consequence of bottom-up attention discussed in Bordalo et al. (2013) and Bordalo et al. (2022). The evidence suggests that the information campaign reoriented families' relative prioritization of school and peer quality, leading to a relative increase in the demand for school quality above and beyond what can be explained by baseline mean peer and school quality biases. Viewed through the model lens, information updating proves to correspond to a small share of the overall *average* changes in MWTT. This latter finding results from families' beliefs not being too far off from the truth on average. Overall, the evidence demonstrates shows that the intervention's effects operated by re-orienting demand in a way that families increase their valuation of effective schools and decrease their valuation of peer quality.

6.4 The Role of Strategic Incentives and Perceived Admissions Chances

The evidence in the previous sections show that families average MWTT for school quality increased and their average MWTT for peer quality decreased. The underlying model used to arrive at these conclusions abstracts away from families' perceived admissions chances and any changes in those perceptions induced by the intervention. Optimal portfolio models widely used in the school choice literature (Agarwal and Somaini, 2018, Chade and Smith, 2006, Kapor et al., 2020, Walters, 2018) combined with a rational expectations assumption imply that families would perfectly forecast demand so that their submitted ROLs reflect changes in admissions chances, information, and preferences. The presence of strategic behavior introduces additional concerns in interpreting observed demand as reflective of true preferences (Agarwal and Somaini, 2018).

In Appendix G, I show that a majority of applicants (roughly three-quarters) face no admission risk. In fact, seven markets consist solely of applicants without admission risk at their top-ranked programs, meaning that the probability they are accepted to their top-ranked program is equal to one.[19] This reality is a product of district-wide declining enrollment, with LAUSD enrollment decreasing by approximately 40 percent between its peak in 2004 and 2023. The wide prevalence of degenerate risk reduces the reliance on portfolio models of school choice that allow applicants to weigh their admissions chances when applying, reducing the decision to a standard discrete choice problem. Consequently, between the 2016 and 2021 cohorts, the share of families enlisting in their most preferred program ranged between 89 to 92 percent. Evidence notwithstanding, Kapor et al. (2020) emphasize that families' beliefs about admissions chances are highly heterogeneous and biased. While that may also be true in our setting, as long as biases and heterogeneity are unaffected by the intervention, then choices will also mostly reflect changes in preferences and information. I conduct exercises that probe the potential presence of strategic behavior and the role of changing beliefs.

Appendix G provides extensive robustness checks assuaging concerns about the role of strategic behavior affecting the interpretation of the findings. I provide evidence from four exercises. First, I descriptively show that behavior implying strategic behavior is not too prevalent in the ZOC setting, following intuitive descriptive checks suggested by Abdulkadiroglu et al. (2006). Second, I show that the evidence implying strategic behavior did not substantially change with the intervention, an indication beliefs about admissions chances were not severely affected by the intervention.[20] Third, I demonstrate that demand estimates are robust to restricting to portions of the ROL that are less prone to misreporting due to strategic incentives. Among these I consider models excluding the top-ranked option and excluding zones with potentially larger strategic incentives. Fourth, given the wide prevalence of degenerate risk, I assess the robustness of the main findings by comparing estimates from the main sample to estimates from a sample that faces no admission risk. My results are qualitatively and quantitatively similar in all of these exercises. The evidence suggests that strategic behavior and perceived changes in admissions chances are unlikely culprits distorting the interpretation of the primary findings

7 Impacts on Outcomes

In this section, I examine how the intervention influenced outcomes, beginning with an analysis of whether capacity constraints reduced the enrollment impacts that might be expected based on application behavior. I then focus on two types of outcomes. The first involves student-level

[19]This is corroborated by discussions with ZOC administrators revealing that in several markets all applicants are assigned their top-listed program.

[20]Existing literature has studied how information interventions shape beliefs about admissions chances (Arteaga et al., 2022, Larroucau et al., 2024). Even in interventions where admission risk is the sole feature of information provision, beliefs move relatively little in response to these interventions. For example, in Arteaga et al. (2022), applicants who faced admission risk at the margin of 0.3 that received a warning through WhatsApp message updated their admission risk (probability of no assignment) belief from .165 to .201. This is after being told that their admission risk far exceeded their beliefs. It is natural to expect beliefs to move less in response to interventions that do not target them. This is even more so in settings where applicants face no risk at all given the wide prevalence of degenerate probabilities in the ZOC setting.

responses to the district's annual School Experience Survey (SES), which includes measures of socio-emotional development, following the framework of Jackson et al. (2020), as well as overall satisfaction. I refer to these as non-cognitive outcomes. The second set of outcomes involves standardized test scores, though this analysis is limited to the first experimental wave due to California's testing schedule.[21]

Appendix Figure C.1 demonstrates effects on *enrolled* school attributes. Similar to the impacts on most-preferred schools shown in Figure 4, we find increases in school quality of enrolled schools among those in high saturation schools. Treatment effects on enrolled school peer quality are mostly indistinguishable from statistical noise and small in magnitude. The evidence shows that the intervention successfully increased demand for effective schools, which also led to enrollment in more effective schools. The close alignment between effects on most-preferred rankings and actual enrollment is partly driven by declining enrollment in LAUSD, which left most ZOC programs undersubscribed during the experimental years.

Table 4 presents results for additional outcomes of interest derived from the SES and test score data. The SES is administered annually to most students across grades, including all high school students. Following Jackson et al. (2020), I categorize the numerous survey questions into five indices. The first is a happiness index, which captures students' satisfaction at the school they enroll in during ninth grade. The second is an interpersonal index, which measures how well students get along with others, including those with differing viewpoints. The school connectedness index includes questions like, "I feel like I am part of this school." The academic effort index includes items such as, "When learning new information, I try to put the ideas into my own words," and "I come to class prepared." Lastly, the bullying index covers various forms of bullying, including teasing, physical bullying, and cyberbullying. Each index is standardized with a mean of zero and a standard deviation of one, with further details provided in Appendix A.1. Test score outcomes are measured in eleventh grade, the only year high school students in California take standardized exams.

Panel A of Table 4 focuses on survey-based non-cognitive outcomes. Across all measures, treatment effects for students in low-saturation schools are generally indistinguishable from statistical noise. However, treatment effects are more pronounced for students in highly saturated schools, particularly in the 2021 cohort. Results for the happiness index show that students in high-saturation schools during the most recent experimental wave experienced an increase in school satisfaction of about 7 percent of a standard deviation. Other indices, including interpersonal skills, school connectedness, academic effort, and bullying, also improved, with gains ranging from 4 to 9 percent of a standard deviation. Additionally, students in high-saturation schools from the 2019 cohort saw improvements in bullying outcomes. Appendix Table A.1 suggests that these consistent improvements in bullying outcomes across both cohorts may be due to bullying being most predictive of higher school quality (AG) rankings.

These findings contribute to the mounting evidence that schools and teachers impact an array of outcomes, not strictly limited to cognitive scores (Beuermann et al., 2023, Jackson, 2018, Jackson et al., 2020, Petek and Pope, 2023, Rose et al., 2022). The evidence in Panel

[21] LAUSD high school students take standardized exams only in eleventh grade, so data is available only for that year. The 2021 cohort is scheduled for testing in Spring 2025, with data available in Fall 2025.

A suggests that by changing parents' choices, treated students were more likely to enroll in more effective schools which also affected their non-cognitive and socio-emotional outcomes. Further support for the significance of school quality on these broader outcomes is found in the appendix, where Appendix Table A.1 shows a strong correlation between school quality and four key socio-emotional defined similarly as in Jackson et al. (2020). This evidence suggests that the intervention did more than alter educational pathways; it also played a critical role in shaping important developmental aspects of students' lives.

Panel B of Table 4 focuses on test scores. Test score impacts are more nuanced in this setting for two reasons. First, test score outcomes for the 2021 cohort are available in 2025, so I am restricted to focusing on the 2019 cohort. Second, and most importantly, the COVID-19 pandemic interfered with the 2019 cohorts educational experience. The 2019 cohort's first high school year was almost entirely remote, which has been shown to have varying but mostly negative consequences (Bruhn et al., 2023, Goldhaber et al., 2023, Jack et al., 2023). For these reasons, it is not surprising to not find much of an impact on test score outcomes given the multitude of factors affecting student learning in nuanced ways during the initial cohort's high school years. The non-cognitive impacts for the 2021 cohort, however, suggest that changes in effort and motivation may materialize into increases in test scores once they are observed in 2025. Overall, the evidence does reveal that more informed parental decisions led to students' enrollment in more effective schools, which led to richer experiences in high school for many students.

8 Discussion

The assorted set of results in this paper have two broad implications. The first relates to our understanding of parents' preferences and the policy implications of their preferences. The second relates to the implications of social interactions for educational inequality and access to effective schools. I discuss each now in turn.

The evidence in this paper shows that when both peer and school quality were made widely available in Los Angeles, measurable changes in demand were oriented toward higher value-added schools. Similar behavior was observed among parents exposed to similar information in a more nationally representative sample. These findings have particular implications for K-12 policy more generally. First, given the relatively weak correlation between racial composition and school effectiveness (Angrist et al., 2022), large-scale effectiveness-oriented information campaigns have the potential to affect school enrollment segregation patterns. Second, the findings suggest that effectiveness-oriented information campaigns can reorient demand in a way that can compel schools to invest more in inputs that contribute to student learning *and* that parents are more responsive to this kind of quality variation instead of quality 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 through supply-side responses (Andrabi et al., 2017). Third, my findings do not speak to whether or not families "max" out on school effectiveness (Ainsworth et al., 2023). The multidimensional nature of a school's production function makes it plausible that families need not maximize only school effectiveness (Beuermann et al., 2022). Fourth, a growing body of research has demonstrated the importance of information frictions with respect to the rules of the mechanisms (Arteaga et al., 2022, Kapor et al., 2020), and this paper emphasizes frictions in terms of attributes that lead to choice-relevant mistakes. It is clear both contribute to welfare-relevant mistakes in behavior, but more research is necessary to understand the interactions of each and their relative importance.

A second key finding is that social interactions facilitate measurable changes in demand. 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, Mizala and Urquiola, 2013, Rothstein, 2006) and theoretical literature (Cox et al., 2021, Leshno, 2021). Demand externalities seem to 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 or lack of information that flows in their networks. Information campaigns that further motivate interactions can potentially reduce existing school quality gaps, similar to other information campaigns in other settings (Banerjee et al., 2018).[22] Incorporating network-based preference externalities is an important avenue for future theoretical and empirical research.

9 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 in a select set of high school markets in Los Angeles, while also studying the role of social interactions during the preference formation stage.

The survey findings suggest that when selecting schools within their local areas, families often underestimate the schools' actual quality and overestimate the student body's perceived quality. When information about both peer and school quality is made widely available, families tend to prefer higher-quality schools, indicating greater responsiveness to information about the schools' effectiveness rather than the student composition. This demonstrates that providing families with accurate information can lead them to prioritize educational quality in their school selection process. Such shifts not only benefit students by improving educational outcomes but also encourage schools to focus on quality improvements.

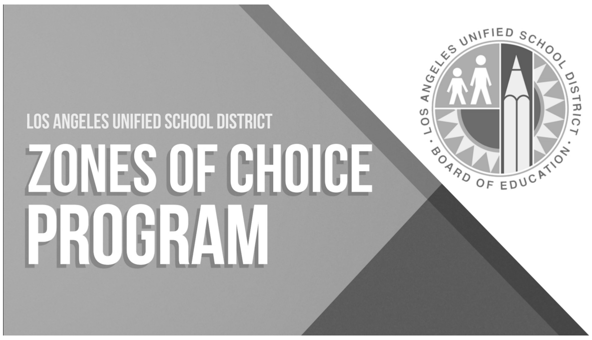
Social interactions and spillovers are important mediators governing new market-level consensus of desirable schools. This is the first paper to show the relevance of social interactions for

[22]Widespread effectiveness information campaigns potentially introduce some additional issues or benefits. For example, they can realign enrollment and have consequential effects on school segregation, as recent laboratory experiments have shown (Houston and Henig, 2021).

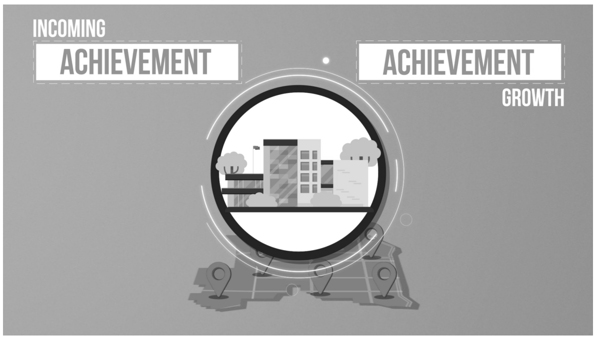
preference formation discussed in nascent theoretical literature (Harless and Manjunath, 2015, Immorlica et al., 2020, Maxey, 2021), providing experimental evidence about a network-based externality in preference formation, which is distinct from the commonly studied preference for peers (Abdulkadiroğlu et al., 2020, Allende et al., 2019, Rothstein, 2006). In general, families rely on other parents as sources of information but also to boost the credibility and accessibility of district-provided information, underscoring the importance of networks in the school choice process.

This paper advances what we know about parents' beliefs and preferences about school and peer quality 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 and the importance of social networks in general equilibrium. These are all important avenues for future research.

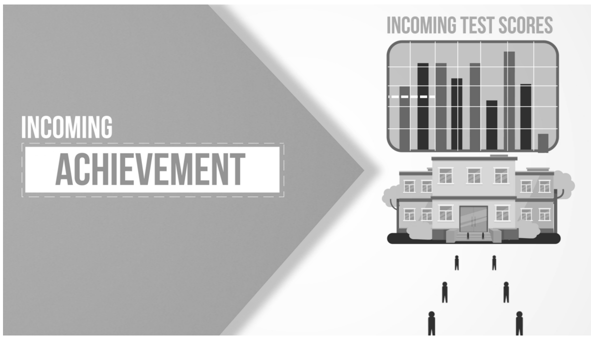
Figure 1: Video Frames



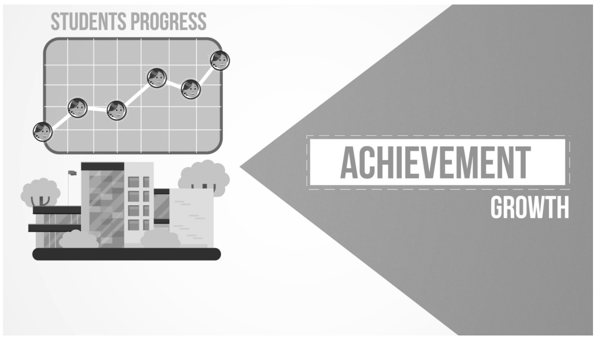
(a)



(b)

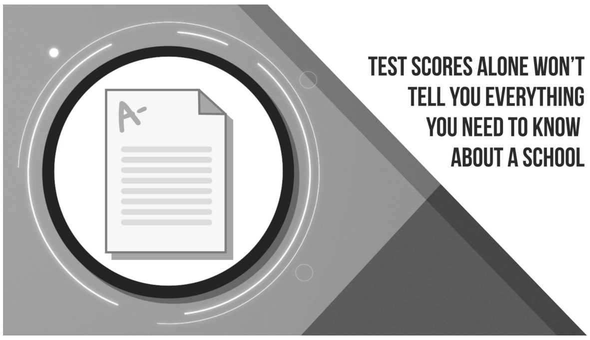


(c)



(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: 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.

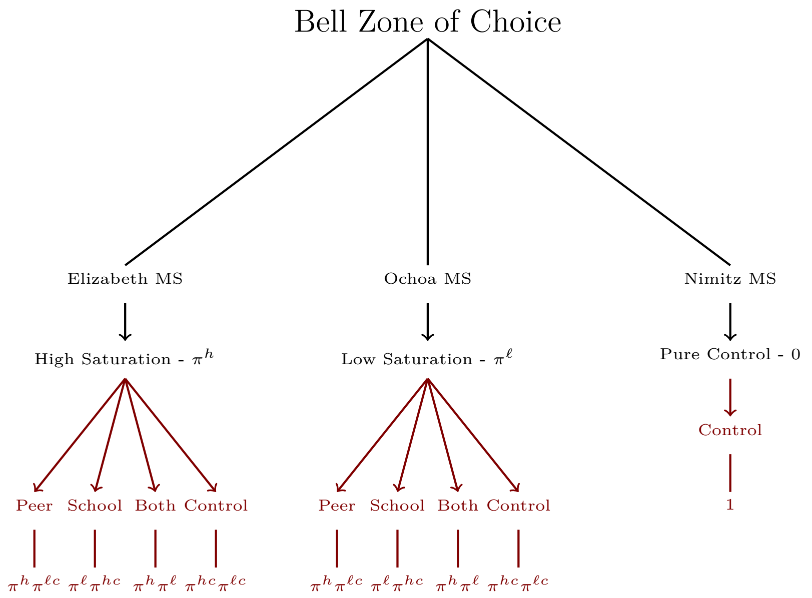
Esperamos que utilice esta información al elegir la escuela adecuada para su estudiante.

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.

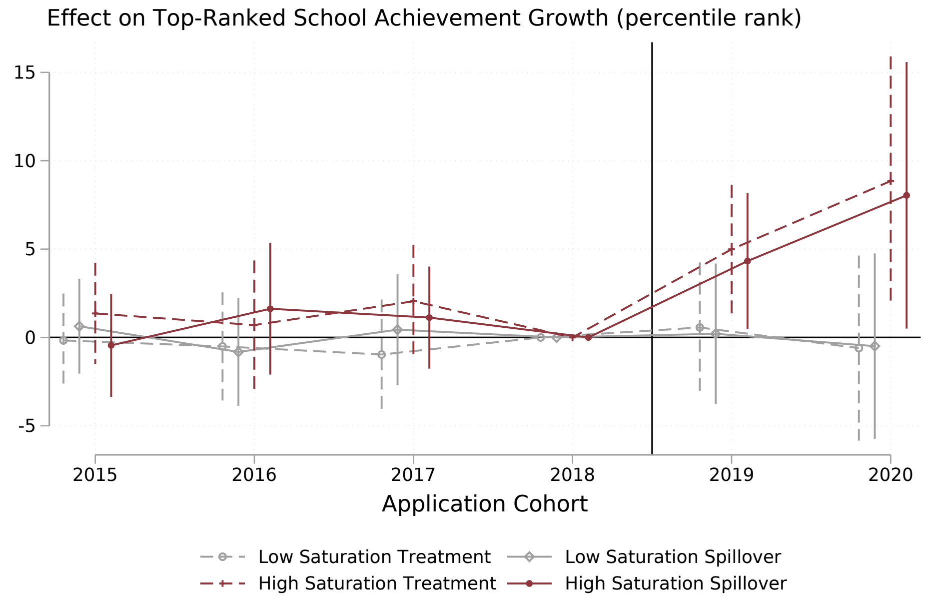
Notes: The front page of the letter is in English, while the second page is in Spanish. The letters list all the relevant schools for the Zone of Choice that the feeder middle school feeds into. The letters provide a simple and short explanation about incoming achievement and achievement growth, along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

Figure 3: Assignment to Treatment

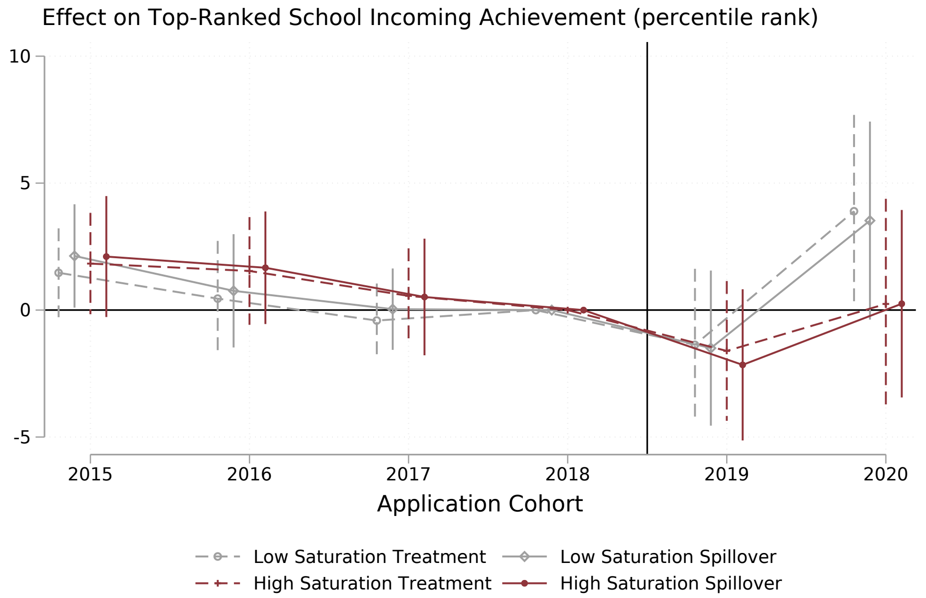


Notes: This figure describes the randomization for a candidate zone with three feeder middle schools. There are certain zones with more than three feeder schools but less than six, so the block sizes were either three or four schools. π_h is the saturation level of high-saturation schools, and π^ℓ is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

Figure 4: Difference-in-Difference Estimates



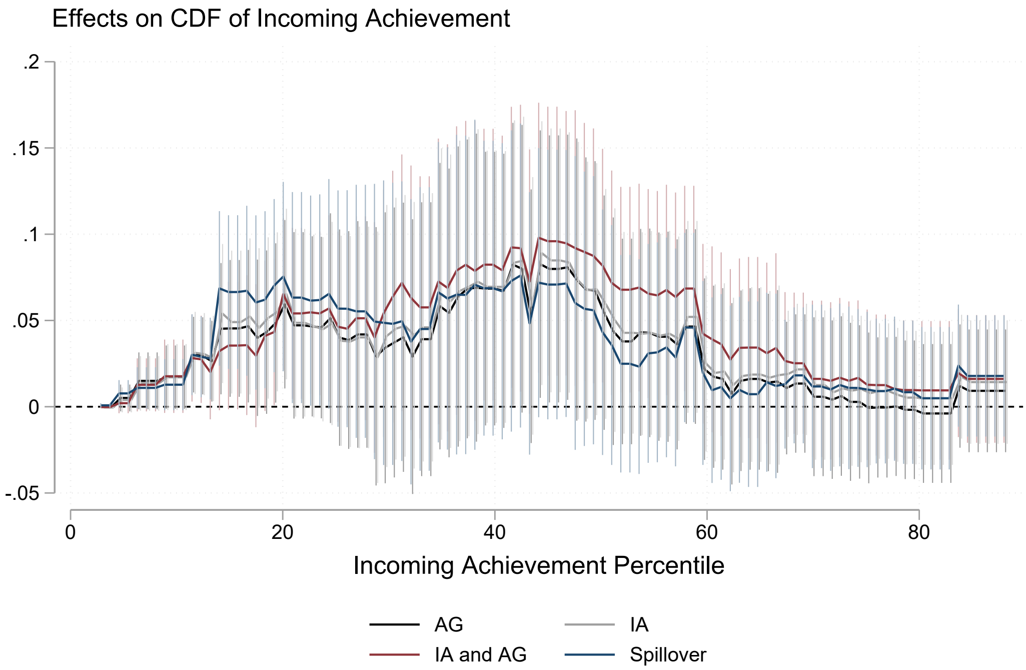
(a) Impacts on Most-Preferred Achievement Growth



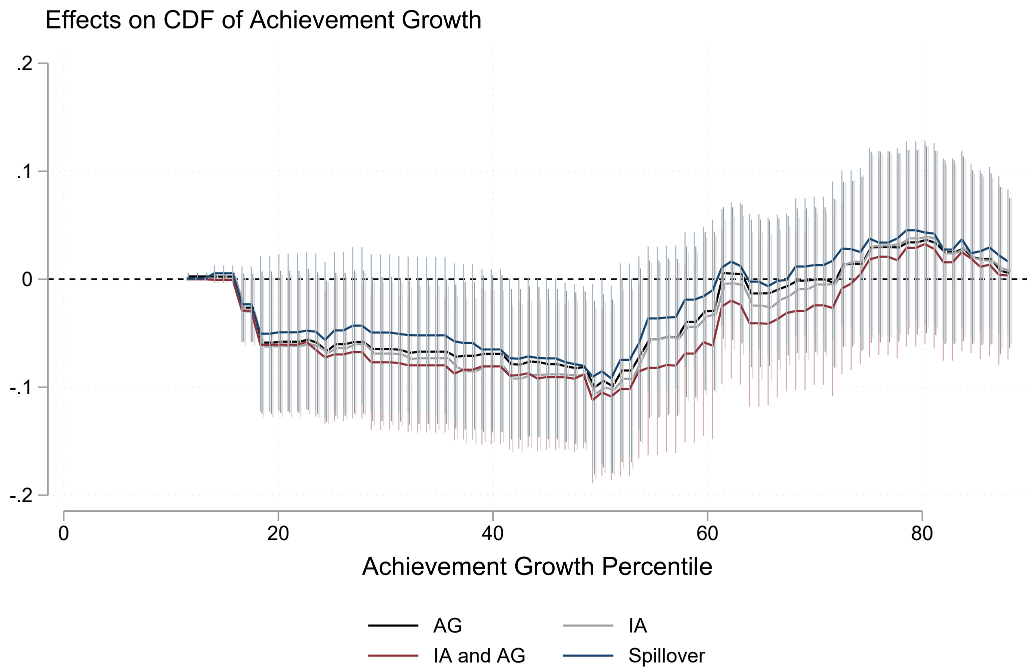
(b) Impacts on Most-Preferred Incoming Achievement

Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of top-listed school attributes—either incoming achievement or achievement growth—on year, treatment group fixed effects, student baseline controls, and treatment group indicators interacted with event-time indicators. All estimates are identified by comparisons of changes between treated groups and 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: Distributional Estimates



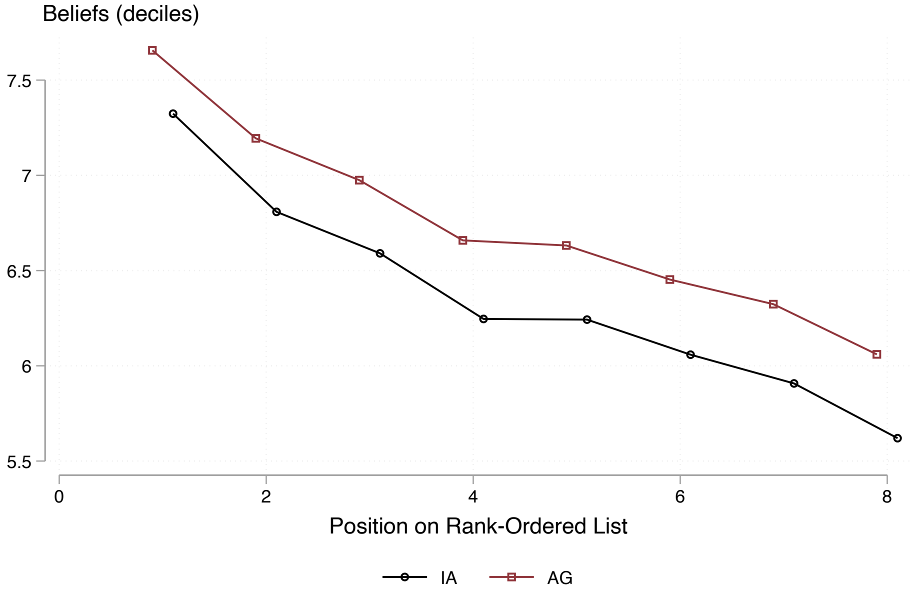
(a) Incoming Achievement



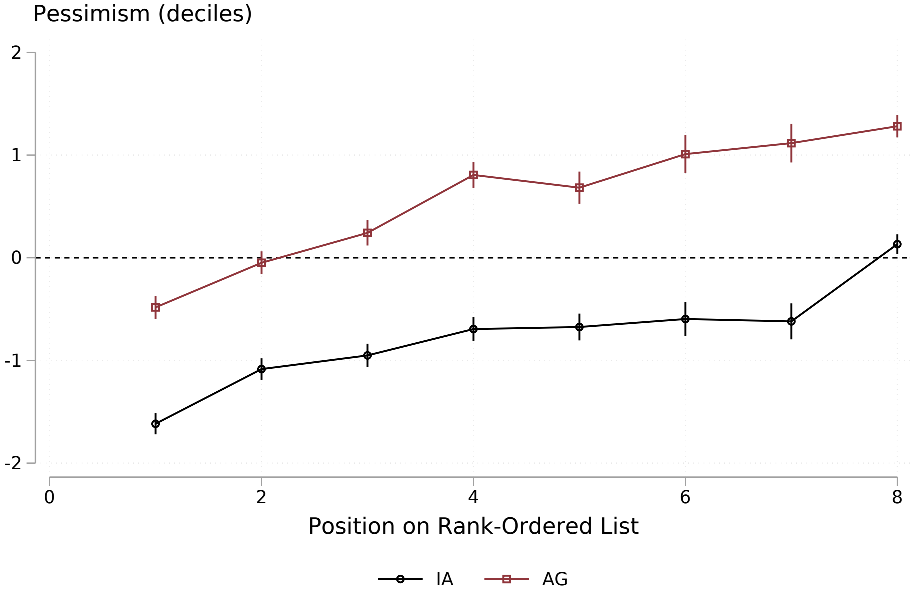
(b) Achievement Growth

Notes: This figure displays distribution regression estimates across the incoming achievement or achievement growth distribution. The sample stacks both experimental waves and includes experiment-year fixed effects, treatment group fixed effects, student baseline controls, and treatment group indicators interacted with event-time indicators. Panels (a) and (b) report treatment effects from models that aggregate treatment at the treatment type level, with types corresponding to peer quality (IA), school quality (AG), both, or spillover. Throughout, standard errors are robust, clustered at the school level, and reported by vertical bars around each estimate.

Figure 6: Beliefs and Bias Across the Rank-Ordered List



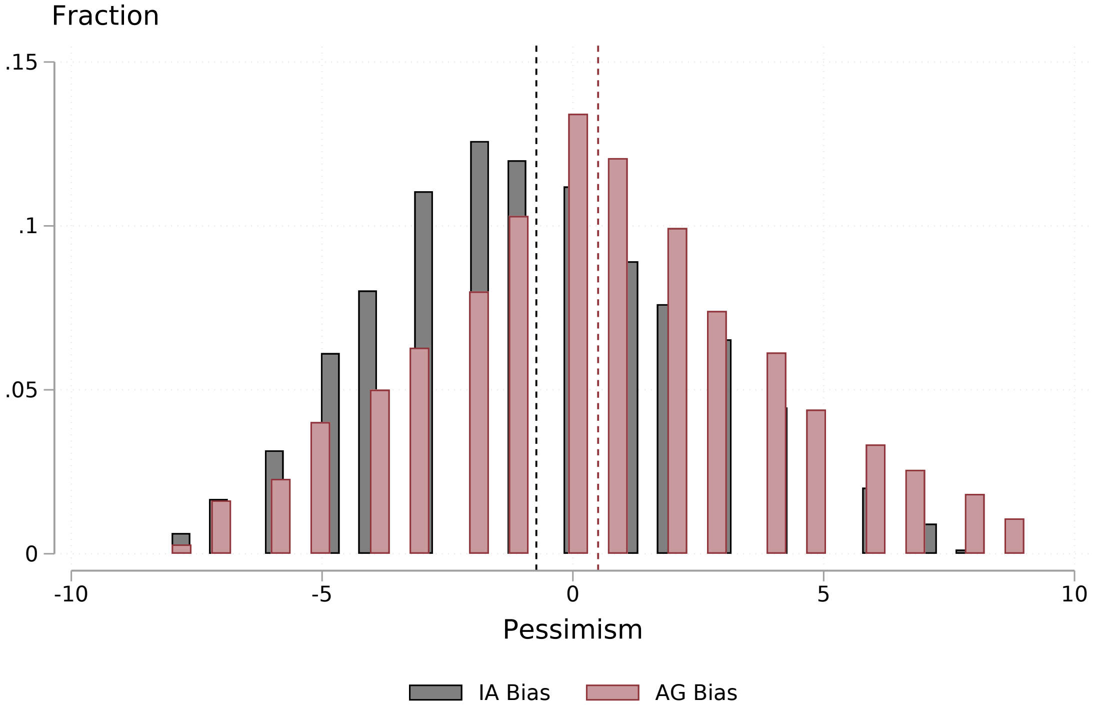
(a) Beliefs



(b) Bias

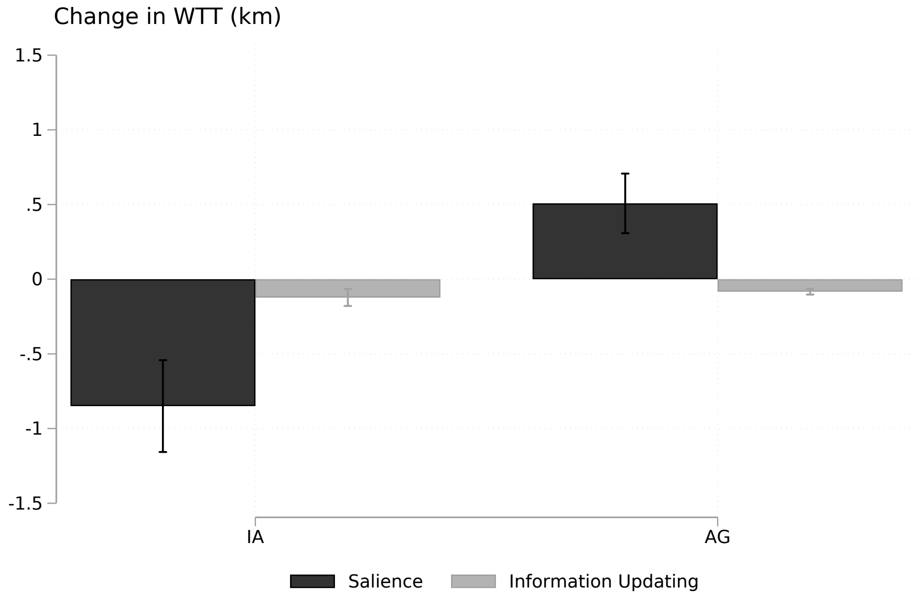
Notes: This figure reports mean beliefs and pessimism for incoming achievement (IA) and achievement growth (AG) at various points of parents' rank-ordered lists. Panel (a) reports mean beliefs and Panel (b) reports mean pessimism. In each subfigure, the black points and line correspond to Incoming Achievement and the red points and line correspond to Achievement Growth. Points corresponds to means, and 95% confidence intervals are represented by the bars.

Figure 7: IA and AG Pessimism Distribution

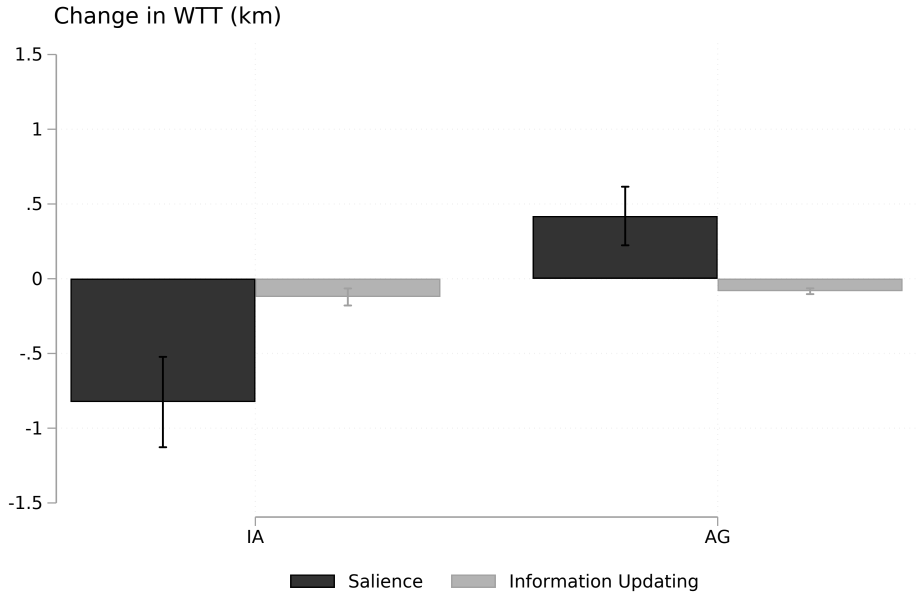


Notes: This figure reports the pessimism distribution for incoming achievement (IA) and achievement growth (AG). Beliefs are collected in terms of deciles, and pessimism is calculated by the difference in between the elicited belief and the estimated belief. Dashed lines correspond to mean pessimism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

Figure 8: Decomposition of Utility Weight Impacts



(a) Treatment Effects



(b) Spillover Effects

Notes: This figure reports decomposition estimates for two separate models. Panel (a) and Panel (b) report decomposition estimates for a model that considers information-specific treatments, where Panel (a) reports treatment effects for directly treated parents and Panel (b) reports estimates for the spillover group. For example, in Panel A the first two bars correspond to decomposition estimates of peer quality weights among those receiving only peer quality information. Similarly, the next two bars are decomposition estimates of school quality weight impacts among those receiving only school quality information. Black bars correspond to the salience component and grey bars correspond to the information updating component. Specifically, the black bar corresponds to an estimate of $\frac{\beta_{PP}}{\lambda}$ ($\frac{\beta_{SS}}{\lambda}$) and the gray bar corresponds to estimates of $\frac{\gamma_P \mu_P}{\lambda}$ ($\frac{\gamma_S \mu_S}{\lambda}$) outlined in Equation 6 (7). In Panel (b), the treatment status for each set of bars corresponds to the spillover group. Standard errors are robust, clustered at the school level, and estimated via the delta method.

Table 1: ZOC and Non-ZOC Differences

	Non-ZOC (1)	ZOC (2)	Difference (3)
Reading Scores	0.102	-0.116	-0.218
			(0.011)
Math Scores	0.106	-0.113	-0.220
			(0.011)
College	0.182	0.064	-0.118
			(0.003)
Migrant	0.095	0.065	-0.029
			(0.003)
Female	0.490	0.483	-0.006
			(0.005)
Poverty	0.710	0.940	0.229
			(0.004)
Special Education	0.095	0.120	0.025
			(0.003)
English Learners	0.103	0.118	0.015
			(0.003)
Black	0.104	0.033	-0.071
			(0.003)
Hispanic	0.635	0.904	0.270
			(0.004)
White	0.155	0.016	-0.139
			(0.003)
N	23,723	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: Difference-in-Difference Estimates on Top-Listed School Attributes

	(1) Pure Control Mean	(2) High Saturation 2019	(3) Low Saturation 2019	(4) High Saturation 2021	(5) Low Saturation 2021
Female	0.487	0.002	-0.002*	0.005	-0.002
		(0.001)	(0.001)	(0.004)	(0.002)
		[.368]	[.443]	[.288]	[.428]
Migrant	0.082	0.000	0.002**	-0.001	0.000
		(0.001)	(0.001)	(0.003)	(0.001)
		[.368]	[.368]	[.418]	[.428]
Poverty	0.979	0.001	0.005**	0.005	0.002
		(0.002)	(0.003)	(0.005)	(0.003)
		[.493]	[.338]	[.445]	[.455]
Special Education	0.119	0.003***	0.001	0.003	-0.001
		(0.001)	(0.001)	(0.003)	(0.002)
		[.3]	[.388]	[.308]	[.443]
English Learner	0.146	0.002	0.002	-0.008	-0.001
		(0.003)	(0.001)	(0.007)	(0.003)
		[.448]	[.375]	[.265]	[.477]
College	0.054	-0.001	-0.003*	0.001	0.000
		(0.001)	(0.002)	(0.005)	(0.002)
		[.502]	[.368]	[.42]	[.477]
Black	0.044	0.000	-0.001	-0.011	-0.002
		(0.002)	(0.001)	(0.011)	(0.003)
		[.502]	[.48]	[.165]	[.42]
Hispanic	0.908	-0.001	0.004	0.008	0.001
		(0.002)	(0.003)	(0.012)	(0.005)
		[.52]	[.415]	[.328]	[.47]
White	0.019	0.001	-0.002*	0.004	0.000
		(0.001)	(0.001)	(0.003)	(0.002)
		[.43]	[.405]	[.33]	[.47]
Suspension Days	12.310	-0.537	-0.310	-1.026	-0.404
		(0.395)	(0.465)	(2.758)	(1.838)
		[.45]	[.458]	[.435]	[.472]
Suspension Incidents	0.007	0.000	0.000	-0.001	0.000
		(0.000)	(0.000)	(0.001)	(0.000)
		[.45]	[.458]	[.34]	[.472]
N			69,054		

Notes: This table reports difference-in-difference estimates of the effect of different treatments on row variables. These estimates come from regressions of most-preferred school attributes 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, clustered at the school level, and reported in parentheses. Randomization inference p-values are reported in brackets underneath each standard error based on 400 placebo treatment statuses for both school and individual-level treatments.

Table 3: Information Effects on MWTT for School and Peer Quality

	MWTT Estimates		p-value
	Peer Quality	School Quality	
Treatment			
Untreated	0.392***	0.658***	0.017
	(0.093)	(0.078)	
Information: Peer Quality	-0.972***	0.474***	0.000
	(0.174)	(0.104)	
Information: School Quality	-0.865***	0.424***	0.000
	(0.171)	(0.101)	
Information: Both	-0.815***	0.565***	0.000
	(0.154)	(0.100)	
Spillover	-0.947***	0.336***	0.000
	(0.172)	(0.100)	
Distance	-0.068***		
	(0.006)		
p-Value	0.733	0.189	
Number of Choices	142,589		
Number of Students	21,774		

Notes: This table reports estimates from the model outlined in Equation 3. Column (1) corresponds to estimates associated with peer quality MWTT and changes in MWTT, and Column (2) corresponds to estimates associated with school quality MWTT and changes in MWTT. Rows labeled as Untreated correspond to utility weight estimates for families in the pure control group. Information: School Quality, Information: Peer Quality, and Information: Both correspond to directly receiving peer quality, school quality, or both types of information, respectively, and estimates associated with these rows correspond to changes in MWTT. Each cell, except for distance estimates, report estimates in kilometers. These are calculated by dividing the unreported utility weight estimate (or change) by the corresponding distance disutility estimate. Column (3) reports the p-value of a test of equality of estimates in Column (1) and (2) within a row. The p-value reported in the bottom rows corresponds to a test with the null hypothesis that all utility weight impacts within a given column are equal. Standard errors are reported in parentheses and estimated via the delta method.

Table 4: Effects on Cognitive and Non-Cognitive Outcomes

	(1)	(2)	(3)	(4)	(5)
		Low Saturation		High Saturation	
	Control Mean	2019	2021	2019	2021
		Panel A: School Experience Survey			
Happiness Index	0.048	-0.038	-0.006	0.028	0.072**
		(0.027)	(0.030)	(0.027)	(0.028)
		[0.117]	[0.445]	[0.223]	[0.028]
Interpersonal Skills Index	0.030	-0.060**	-0.004	-0.019	0.056*
		(0.024)	(0.021)	(0.026)	(0.028)
		[0.035]	[0.412]	[0.248]	[0.055]
School Connectedness Index	0.514	-0.014	0.000	0.004	0.039**
		(0.015)	(0.017)	(0.015)	(0.016)
		[0.213]	[0.477]	[0.423]	[0.025]
Academic Effort Index	0.053	-0.048*	-0.006	-0.002	0.046*
		(0.031)	(0.029)	(0.022)	(0.022)
		[0.068]	[0.393]	[0.453]	[0.085]
Bullying Index	0.175	0.048	0.029	0.099**	0.094**
		(0.033)	(0.026)	(0.036)	(0.028)
		[0.148]	[0.228]	[0.020]	[0.010]
Observations			23,792		
		Panel B: Eleventh Grade Test Scores			
Math Score	-0.020	-0.039	-	-0.031	-
		(0.037)	-	(0.040)	-
		[0.180]	-	[0.233]	-
ELA Score	0.069	-0.007	-	-0.001	-
		(0.036)	-	(0.036)	-
		[0.393]	-	[0.445]	-
Observations			16,145		

Notes: This table reports estimates from several regressions. Each row corresponds to a separate student-level regression of the row variable on year indicators, treatment group indicators, a vector of baseline student covariates, and treatment group indicators interacted with treatment year indicators. Panel A corresponds to outcomes measured in the School Experience Survey (SES) for the 2018 cohort, 2019 cohort, and 2021 cohort. Appendix A.1 discusses the construction of the indices in Panel A. Panel B focuses on eleventh-grade test scores and is limited to estimates related to the 2019 experimental cohort as test scores are not available for the 2021 cohort. Column (1) reports control group means for the 2018 cohort. The next four columns report treatment- and year-specific treatment effects. Columns (2) and (3) focus on treatment effects for students enrolled in low saturation schools and Columns (4) and (5) focus on effects for students enrolled in high-saturation schools. Throughout, standard errors are robust, clustered at the school level, and reported in parentheses. Randomization inference-based p-values are reported in brackets underneath each standard error.

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