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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* student outcomes, 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. These demand responses translate into real educational gains. Students exposed to the improved information enroll in more effective schools, achieve higher test scores, report improved socio-emotional well-being, and are more likely to enroll in college. The experimental evidence shows parents value school effectiveness even conditional on peer quality and that improving the informational environment can elevate numerous policy-relevant outcomes.

Keywords: school choice, school quality, preferences, information, value-added, social interactions

JEL Classification: I21, I24

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1 Introduction

Parents' valuation of effective schools govern the success of school choice policies, but many open questions remain as to what they prioritize and why. Some studies suggest that parents prioritize schools that improve student learning and other outcomes (Beuermann et al., 2023, 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). Four 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? And most importantly, does providing school quality–instead of peer quality—information elevate student outcomes? These four 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., 2023, 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. 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. Last, I follow the students for six years after the intervention and can assess the impact of information on student achievement, socio-emotional outcomes, and four-year college enrollment.

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.

[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.

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 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. 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-

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

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 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 eleventh-grade test scores, socio-emotional outcomes similar to Jackson et al. (2020), and college enrollment. The focus on the three provides a more holistic perspective regarding the various ways schools potentially influence student outcomes and how those influences are mediated by school quality. I find that achievement is elevated among students in highly exposed schools—schools where the effects on choices were also largest. I also find that 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. Last, I find that college enrollment also improves by approximately 10 percent of the baseline mean. Together, these results indicate that the intervention not only altered school assignments but also produced measurable gains across academic, socio-emotional, and longer-term outcomes. The findings highlight the power of transparent information policies and lend support to recent efforts to incorporate growth-based measures of school quality into accountability systems (Data Quality Campaign, 2019), suggesting that making such data salient to families can meaningfully enhance student trajectories.

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., 2023, 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, decomposing treatment effects to better understand information provision mechanisms, and assessing the overall impact of information on achievement and college enrollment outcomes. My results also shed 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, Neal and Root, 2024).

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. For historical background and the 2012 expansion, see Campos and Kearns (2024).

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.

[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.

Once applications are submitted, students are assigned using the immediate-acceptance (Boston) mechanism (Abdulkadiroğlu and Sönmez, 2003). The mechanism respects neighborhood and sibling priorities but does not include the additional priorities or screening features used in some other cities (Cohodes et al., 2022, Corcoran et al., 2018). Families are required to rank all options in their zone, circumventing issues associated with list-length caps (Calsamiglia et al., 2010, Haeringer and Klijn, 2009). However, the mechanism is not strategy-proof, so families could, in principle, benefit from misreporting preferences to avoid assignment to a lower-ranked option.

In practice, district-wide declining enrollment substantially weakens those strategic incentives.[4] Many programs are undersubscribed, and roughly three-quarters of applicants face no admission risk at their most-preferred option. When admission is effectively guaranteed at the top choice, there is little benefit to manipulating the rank order. Moreover, ZOC requires applicants to rank all options in their zone, yielding complete lists that are largely insulated from strategic distortions. Administrative data from 2019–2024 confirm that at least seven markets were consistently undersubscribed, with every applicant receiving their first-listed option. In this environment, reported rankings are closer to genuine preferences rather than strategic play.

During high school, students take standardized exams in eleventh-grade, report socio-emotional outcomes in the district's School Experience Survey, and are followed into college via linkages to the National Student Clearinghouse. Therefore, this setting is unusually well-suited to answer the questions posed in this paper. Because we intervene precisely before families submit applications, we can both shift choices and follow students into early adulthood to assess how information policies affect achievement, socio-emotional development, and college enrollment.

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 field 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.

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

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.

The first challenge lies in defining and accurately measuring school and peer quality. Researchers typically rely on value-added models (VAMs), 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 but I validate them using randomized admission lotteries available in ZOC and magnet programs across LAUSD.[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 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 use 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.

I now discuss some descriptive facts about school and peer quality in LAUSD. As shown in Appendix Figure B.2, school and peer quality are positively associated with a rank-rank correlation of 0.61, but the correlation drops to 0.24 among ZOC schools. From Appendix Figure B.2, we can also see that ZOC schools generally score above average on value-added measures

[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 ZOC schools between 2013 and 2019, 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).

but below average on peer quality, although there is considerable variation. Appendix Table B.2 indicates that few standard administrative characteristics predict school quality, while Appendix Table A.1 points to school-level bullying outcomes—drawn from LAUSD's School Experience Survey—as the strongest predictor of both school and peer quality. A school connectedness index is also a significant predictor of school quality. Although these non-cognitive measures from the School Experience Survey are not part of the information provision, they are central to understanding the broader impacts of the intervention on student outcomes and will be discussed in more detail below.

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 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. Impor-

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

tantly, 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 Field Survey

The field 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 instrument is reported in Appendix Section D.1.

The survey's distribution method evolved throughout 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 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, intending to collect 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

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

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 students in other schools who are not around anyone else that is exposed to the treatment, thereby isolating any effects arising from social interactions. In this context, spillovers refer to the 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 70% and low saturation to 40%. This creates a market-specific experiment within each zone, with two treatment levels, high (H) and low (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, combined with the school-level experiment, identifies intent-to-treat effects both for households

[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.

directly receiving information and for those indirectly exposed through their peers. These effects are estimated by comparing treated households—whether directly or indirectly treated—to households in pure control schools, 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 assignment probability for each treatment, and Ochoa MS is assigned to low saturation, where π^ℓ is the assignment probability for each treatment. Nimitz is the pure control school. 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.

Complementary Online Survey

The purpose of this complementary survey is twofold. First, it provides an external, corroborating perspective on parents' preferences, allowing me to assess whether the patterns observed in the field generalize beyond the experimental context. Second, it helps uncover the mechanisms behind the role of social interactions in school choice, offering insights into why and how information shared among parents affects their decisions. The survey's design was developed with these objectives in mind.

Like the field experiment, I present parents with educational videos explaining school and peer quality differences. Afterward, their beliefs are measured and compared to objective indicators—such as Great Schools Test Score and Progress ratings—which measure peer and school quality. The survey also features choice experiments designed to estimate how far parents would be willing to travel for better school or peer quality, after they have seen the pedagogical videos. By revealing parents' preferences once they are informed, these experiments provide complementary evidence related to the core questions surrounding parents' valuation of effective schools. 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 provided in Appendix E.

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

[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.

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.

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

[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.

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 I still control for the reported baseline covariates to increase precision in the estimates.

4 Reduced-Form Evidence

In this section, I report the experimental estimates. Appendix Table C.1 and Appendix Table C.2 report the estimates of the eight-parameter model discussed in Figure 3 for each cohort separately. The key finding across both cohorts is that treatment effects are remarkably similar within saturation groups, so in the rest of the analysis, I aggregate treatments for power purposes. In what follows, 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 distinguish between treatment types. The change between the two models emphasizes the importance of social interactions from different perspectives. Throughout, the evidence paints a remarkably consistent story of the intervention's impacts and the empirical relevance of social interactions. Additional experimental 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. First, there is a boost in statistical precision due to the absorption of time-invariant unobserved preference heterogeneity across treatment groups. Second, Appendix Table C.1 and Appendix Table C.2 reveal that there is limited heterogeneity in treatment effects within saturation groups. Third, there are convenient falsification tests that implicitly test for balance on pre-intervention trends in outcomes of interest, providing a stronger assessment of the intervention's randomization process. For a given outcome Y_i, I consider the following specification

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

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. Assignment to the spillover group in the pre-intervention years is determined identically that in the intervention years. 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 average 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 average demand for peer quality was unaffected by the intervention. Appendix Figure C.5 and Appendix Figure C.6 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 and effects that averaged to zero. This latter finding mirrors the importance of social engagement with information in generating meaningful changes in behavior (Banerjee et al., 2018).

What other school attributes are affected by changes in demand for school quality? To assess how changing demand for school quality affects other school attributes, I examine both standard measures—such as racial composition, socioeconomic status, special education shares, and suspensions—and socio-emotional learning outcomes in the school experience survey. The latter measures of socio-emotional learning have been shown to be predictive of long-term outcomes, even conditional on school quality (Jackson et al., 2020). Table 2 shows little evidence that shifting demand for higher-quality schools alters demand for these standard attributes.[14] However, Table 3 focuses on survey-based measures and indicates notable changes. Parents' top-listed programs not only tend to be more effective in terms of their academic effectiveness, but these schools enroll students who subsequently report less bullying, stronger feelings of

[14]Appendix Section C.1.1 provides further analysis of treatment effect heterogeneity.

connectedness, greater effort, higher interpersonal skills and grit, and higher overall happiness. By increasing the demand for more academically effective schools, demand for schools that perform well in terms of these alternative socio-emotional outcomes also increased. These results motivate the final analysis in the paper, which examines how the intervention affects both test scores and non-cognitive outcomes measured in the School Experience Survey.

4.2 What Can Explain Saturation-Level Heterogeneity?

A puzzling result so far is the absence of detectable treatment effects for students in low-saturation schools. One possible concern is that unobserved school-level shocks may explain these differences. However, Appendix Table C.4 shows that roughly 80 percent of the variation in school-level treatment effects occurs between zones rather than within zones. This pattern reinforces the idea that, within a given market, parental demand tends to move cohesively in a common direction. The next section presents distributional estimates that further support this interpretation. Evidence notwithstanding, there are two additional forces that are worth exploring: baseline differences in demand and neighborhood-level interactions as a complement to school-based interactions.

Although Figure 4 shows that trends in demand for effective schools were similar between schools assigned to different treatment groups, there may have been differences in demand for effective schools at baseline. There is less scope for changes in demand in settings where a large share of parents are already demanding relatively effective schools, so it is natural to see if differences in baseline demand are associated with differences in treatment effects. Panel A of Appendix Table C.5 shows that this is indeed the case. Both high and low saturation students experienced positive demand effects for higher AG schools if demand for AG at their school was relatively low at baseline. In schools where demand was larger at baseline, we see much more muted—and even negative—effects which dampens the overall averages we report in Figure 4. These patterns do not emerge for demand changes related to IA. This preference-based heterogeneity is intuitive: where there was scope to increase demand for AG, the intervention was effective in both high- and low-saturation schools, with spillovers also playing a role in each.

To further examine the role of social interactions—this time outside the school context—I exploit variation in parents' proximity to treated peers in their residential neighborhoods. Conditional on school treatment status, parents differ in the number of treated families living nearby, measured at the Census Block level. Two parents may have children in the same low-saturation school but experience very different levels of neighborhood exposure to treated peers. If social interactions operate beyond school boundaries, parents with more treated neighbors should be more likely to shift their preferences toward effective schools. Panel B of Appendix Table C.5 provides suggestive evidence consistent with this hypothesis. The effect of neighborhood exposure is particularly relevant for parents in low-saturation schools—those with fewer opportunities for within—-school spillovers. Although noisy, we cannot reject that the effects for parents in high saturation schools—who experienced sizable increases in demand for AG—differ from parents in low saturation schools who were exposed to many treated parents at the neighborhood level. Although suggestive, the evidence further underscores the importance of social interactions, this time emphasizing that they can happen at both the school and neighborhood level.

4.3 Distributional Estimates

The findings in Figure 4 report average treatment effects potentially masking heterogeneity across the quality distribution. Distributional estimates can help unpack some of this heterogeneity, offer a more nuanced view of how the intervention shaped demand, and may help explain the limited responsiveness of parents in low-saturation schools. Following the approach in the previous section, I first consider saturation-specific treatment groups that distinguish between treatment and spillover groups. I then present results that aggregate the treatment by information type, without differentiating by saturation level.

To study how the entire distribution of an outcome Y_i responds to the intervention, we use a distribution regression approach (Chernozhukov et al., 2013). We consider the empirical cumulative distribution function (CDF) as outcomes $\mathbf{1}\{Y_i \leq a\}$ for different points of support a. We estimate the following regression for 100 equally spaced values of $a \in [\underline{a}, \bar{a}]$:

$$\mathbf{1}\{Y_i \leq a\} = \alpha^a_{z(i)t(i)} + \alpha^a_{g(i)} + \gamma' X_i + \sum_x \beta^a_x T^x_{it(i)} + u_i, \quad a \in [\underline{a}, \bar{a}]. \tag{2}$$

In this specification, α_{zt} is a zone-by-year fixed-effect, α_g are treatment group indicators, $T^x_{it(i)}$ are individual-level treatment indicators for the treatment periods where, depending on the model, x can belong to $\{AG,\ IA,\ Both,\ Spillover\}$, or alternatively, x can belong to $\{High,\ SpilloverHigh,\ Low,\ SpilloverLow\}$ and X_i is a vector of baseline covariates. The support $[\underline{a}, \bar{a}]$ includes 100 equally spaced points. 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. The parameters β^a_x correspond to effects on the CDF of the outcome Y_i at the point a. Therefore, for a given attribute corresponding to a parents' top-listed program Y_i, if $\beta^a_x < 0$, the probability of choosing a school whose attribute is below a decreased, indicating that parents are choosing schools whose attributes are higher than a. Standard errors are robust and clustered at the school level and randomization-based inference is reported in Appendix C.

Figure 5 presents estimates from Equation 2, showing saturation-specific effects on the distributions of incoming achievement and achievement growth percentile ranks in Panels (a) and (b), respectively. At each percentile, the estimates indicate the direction and magnitude by which the cumulative distribution function shifts. For instance, at the 40th percentile, the probability that a family's top choice fell below that percentile in peer quality rose by about ten percentage points for high-saturation schools and by about five percentage points for low-saturation schools. Panel (a) illustrates that many families ended up selecting schools with lower peer quality rankings compared to the baseline, while Panel (b) demonstrates an increase in demand for school quality, albeit with weaker effects at the top of the distribution, possibly due to ceiling effects. Comparing Panels (a) and (b) with Figure 4 Panel (a) provides a different perspective on the zero mean effects in low-saturation schools. Although both low- and high-saturation groups show similarly signed distributional shifts, the magnitude is smaller for low-saturation schools. In other words, the most pronounced changes appear in the lower part of the distribution, where both groups exhibit comparable directional effects on families' choices, but these effects are more slightly muted in the low-saturation setting. This evidence is consistent with the lack of school-level shocks explaining differences in treatment effects across high and low saturation arms explored in Appendix Table C.4.

Panels (c) and (d) show aggregated treatment effects based on the information parents received. Panel (c) tracks effects on the distribution of top-listed peer quality at various percentile ranks. At the 40th percentile, for instance, the probability that a family's top choice fell below this rank in peer quality rose by roughly seven percentage points among those receiving AG information, suggesting that these parents chose schools with lower peer quality at the top of their ranking. Notably, the effects are similar across all treatment and spillover groups, highlighting strong social interactions. Although families generally shifted toward schools with lower peer quality, these changes are less pronounced in areas with higher peer quality schools. Panel (d) demonstrates that such shifts accompany an increase in demand for higher school quality. At the 40th percentile, for example, the probability that a family's top choice fell below this rank in school quality decreased by roughly eight percentage points among those exposed to any treatment. Crucially, the treatment effects observed among untreated parents in treated schools mirror those for treated parents. The striking visual evidence across Panels (a), (b), (c) and (d) indicate a broader community-level convergence in preferences that ultimately favors more effective schools. Appendix Figures C.7 and C.8 display analogous results using randomization-based inference.

4.4 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 comparable 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. In the field survey, nearly all parents rank school quality above peer quality in terms of importance. Similarly in the national sample, 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, field 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 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 and Appendix Table C.4 suggest that parents with fewer peer parents to discuss the information with were less likely to use it, emphasizing the importance of validation and interpretation through social interactions. 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, evidence suggesting a more compressed effect distribution in the low-saturation arms and consistent with Appendix Table C.4. 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 Figure C.4 shows a low rank concordance in parents' reported preferences, suggesting little coordination, with no significant effect introduced by the field experiment shown in Appendix Table C.6. Overall, both the online survey and field experiment indicate that social interactions are more about interpretation and credibility than coordination.[15]

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.[16] I first

[15]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.

[16]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

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 that are note in their zone.

Panel (a) of Figure 6 illustrates the average beliefs for each position on the rank-ordered list (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.[17]

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

D.

[17] 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.

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.[18]

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 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. I am also able to quantify the intervention's impacts in terms of distance or a willingness to travel measure. Last, combined with a few additional assumptions, I can provide suggestive evidence regarding the intervention's mechanisms, unpacking a belief updating and salience channel.

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

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

[18] 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.

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, let $\mathcal{I}_B$ correspond to the families receiving information about both, and let $\mathcal{I}_{Spill}$ be the set of families indirectly exposed to information. The effects of the information campaign can be summarized by changes in the weights families assign to peer and school quality. In particular,

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

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

$$\mathcal{L}(R_i|\delta_j, d_{ij}) = \prod_{k=1}^{Z(i)} \frac{e^{V_{iR_{ik}}}}{\sum_{\ell\in\mathcal{J}_{ik}} e^{V_{i\ell}}}, \tag{5}$$

where $\mathcal{J}_{ik} = \mathcal{J}_{Z(i)} \ \{R_{i1}, \cdots, R_{ik-1}\}$ is the remaining set of options once we are at position k of the rank-ordered list. 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 4 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 Spillover row of Table 4 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 4 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 4. 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).[19] 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 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 ef-

[19] 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.

fects 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.[20] 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).

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

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.[21] 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 three types of outcomes. The first focuses student-level 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 that students take in eleventh grade. Finally, college enrollment data are available only for the first experimental cohort—students who began high school in 2020 and graduated in 2024—so the college enrollment analysis is limited to this group.

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 5 presents results for additional outcomes of interest including test scores, college outcomes, and the SES. 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 skills 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

[21] 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.

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 observed in eleventh grade, the only year high school students in California take standardized exams. Because the second cohort has not graduated from high school yet, we only observe college enrollment outcomes for students part of the first cohort.

Panel A of Table 5 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 6.4 percent of a standard deviation. Other indices, including interpersonal skills, school connectedness, academic effort, and bullying, also improved, with gains ranging from 3 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. The novelty of Panel A is that it demonstrates 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, alluding to a complementarity between achievement and socio-emotional impacts.

Panel B of Table 5 examines test score outcomes. Both IA and AG were based on ELA exams, so it is not surprising to find stronger impacts on ELA scores. In contrast to the socio-emotional outcome evidence, I find improvements in students' ELA scores for both high and low saturation groups that are part of the 2021 cohort. The effects are sizable, ranging between 5-11 percent of a standard deviation. I do not find meaningful changes in test scores for students enrolled in low-saturation schools, students whose choices did not respond much to the information. The achievement improvements reveal that improving the achievement-based informational environment can elevate student outcomes by allowing them to more effectively sort into higher quality schools. This is the first piece of evidence demonstrating that the dissemination of school quality information—not peer quality information—can positively influence achievement.

We can follow the 2019 cohort into college but the 2021 cohort is still in high school. Consistent with the rest of the intervention's findings, for students enrolled in highly saturated schools, I find an increase in college enrollment amounting to four percentage points, approximately 10 percent of the baseline mean. I do not find any significant impacts for students in lowly saturated schools. Effects are spread out across two- and four-year college enrollment but are independently less precise. Therefore, in addition to improvements in student learning, the evidence reveals that improving the informational environment also positively affected the post-secondary trajectories of students. Overall, the constellation of findings related to student outcomes demonstrates that the intervention did more than alter educational pathways; it also played a critical role in shaping important developmental aspects of students' lives.

8 Discussion

The assorted results in this paper have three broad implications. First, they deepen our understanding of parents' preferences and how that relates to effective K-12 policy. Second, they shed light on how social interactions shape educational inequality and access to effective schools. Third, they demonstrate that improving the informational environment about school quality can meaningfully improve student outcomes. I discuss each 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 several implications for K–12 policy. First, given the weak correlation between racial composition and school effectiveness (Angrist et al., 2022), large-scale, effectiveness-oriented information campaigns could influence segregation patterns by shifting demand toward more effective schools. Second, such campaigns can reorient demand toward dimensions of quality that reflect true school productivity rather than student composition, potentially motivating supply-side investments in learning-enhancing inputs (Andrabi et al., 2017). Finally, this paper emphasizes information frictions regarding school attributes rather than just the rules of assignment mechanisms (Arteaga et al., 2022, Kapor et al., 2020). Both types of frictions contribute to welfare-relevant mistakes, but future research should disentangle their relative importance and interaction (Agte et al., 2024).

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 are therefore less dependent on assignments. If parents' information sets are shaped by their networks, then the lower responsiveness of disadvantaged families to school quality information (Cohodes et al., 2022, Corcoran et al., 2018, Finkelstein and Notowidigdo, 2019, Hastings et al., 2006) may reflect network-specific informational barriers rather than differences in preferences per se. For instance, Fong (2019) finds that low-income families are less embedded in advice networks about school choice, and Rangel et al. (2020) show network-formation is weaker in lower income communities. Interventions that both provide information and foster network interactions could therefore reduce gaps in access to effective schools (Banerjee et al., 2018). Future theoretical and empirical work could explicitly model these network-based preference externalities.

Finally, I find that improved information environments increase enrollment in more effective schools and, critically, enhance a range of student outcomes beyond test scores alone. This broader impact underscores the potential for long-run gains when families have better access to school quality—rather than peer quality—information. These findings have important policy implications. Since the passage of the Every Student Succeeds Act, nearly all states have incorporated growth-based metrics into their accountability systems (Data Quality Campaign, 2019). My results demonstrate that making growth metrics more accessible can translate into measurable improvements in student outcomes, supporting the value of including these metrics in accountability systems. However, states currently measure and report growth in disparate ways, with varying levels of rigor, and often present this information on difficult-to-navigate websites. This inconsistency likely limits the effectiveness of these policies. The Every Student Succeeds Act also required states to incorporate non-cognitive components targeting socio-emotional outcomes into their accountability systems. My findings show that reducing information search costs for achievement growth data can improve not only academic outcomes but also socio-emotional outcomes. Future research could explore the effects of more comprehensive information environments that integrate both achievement and socio-emotional metrics, potentially amplifying these benefits.

9 Conclusion

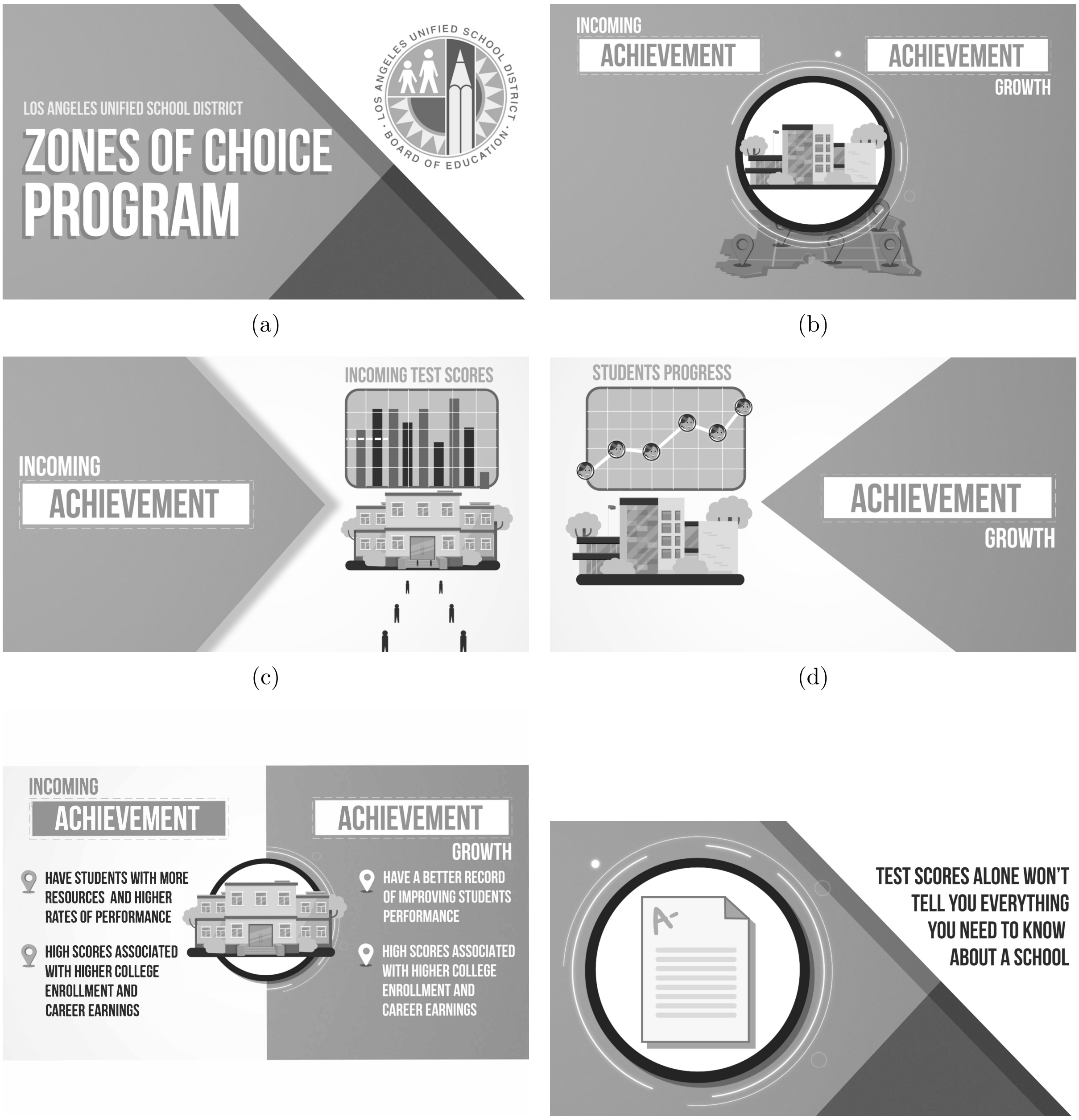
Parents' choices govern the success of school choice initiatives and it is paramount to understand both their preferences, factors that mediate their choices, and how varying information policies that affect demand shape student outcomes. This paper provides survey and experimental evidence about parents' beliefs and valuation of effective schools in a select set of high school markets in Los Angeles and the impact of providing such information on an array of student outcomes, while also studying the role of social interactions during the preference formation stage, and the eventual effect of information on achievement and college enrollment.

Survey evidence shows that when choosing among nearby schools, families tend to *underestimate* how effective schools are and *overestimate* the incoming achievement of their peers. When credible information about both school and peer quality is made widely available, families shift their preferences toward more effective schools, responding more strongly to measures of school quality than to student composition. Providing accurate information therefore encourages families to prioritize educational effectiveness in their decisions. This behavioral shift translates into meaningful improvements for students: those whose parents received quality information enroll in more effective schools and subsequently achieve higher test scores, stronger socio-emotional outcomes, and higher rates of college enrollment. These gains highlight the potential for information-based interventions to generate substantial, multi-dimensional benefits for students—even in the absence of changes to school supply.

Beyond individual behavior, this paper provides the first experimental evidence of a network-based externality in preference formation: parents' social interactions amplify how information shapes school demand, aligning with emerging theoretical work on information diffusion in choice environments (Harless and Manjunath, 2015, Immorlica et al., 2020, Maxey, 2021). These findings highlight the dual role of parent networks—as both conduits and validators of information—and underscore their importance in realizing the potential of school choice reforms.

While the results speak primarily to partial-equilibrium effects, future work should examine how these demand shifts affect equilibrium outcomes such as school supply responses and segregation patterns. Finally, my results focus on academic quality as measured by achievement-based effectiveness, but the education production function is inherently multi-dimensional (Beuermann et al., 2023). Many districts are now experimenting with publishing alternative indicators—such as measures of social-emotional growth, student well-being, or civic engagement—alongside test-based metrics (Angrist et al., 2025). Understanding how parents interpret and respond to these broader measures of quality, and how such information shapes student outcomes, is an important next step for research on school choice and education policy.

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.

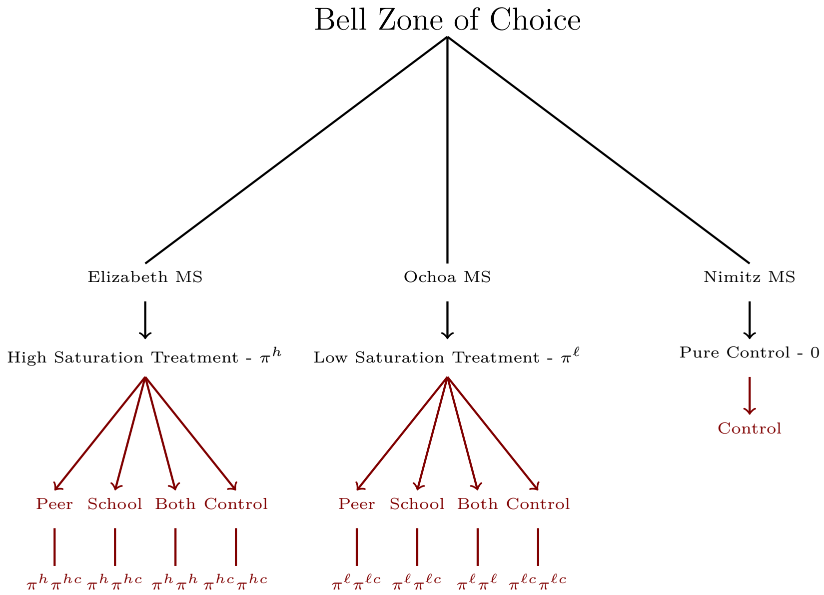
Esperamos que utilice esta información al elegir la escuela adecuada para su estudiante.

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.

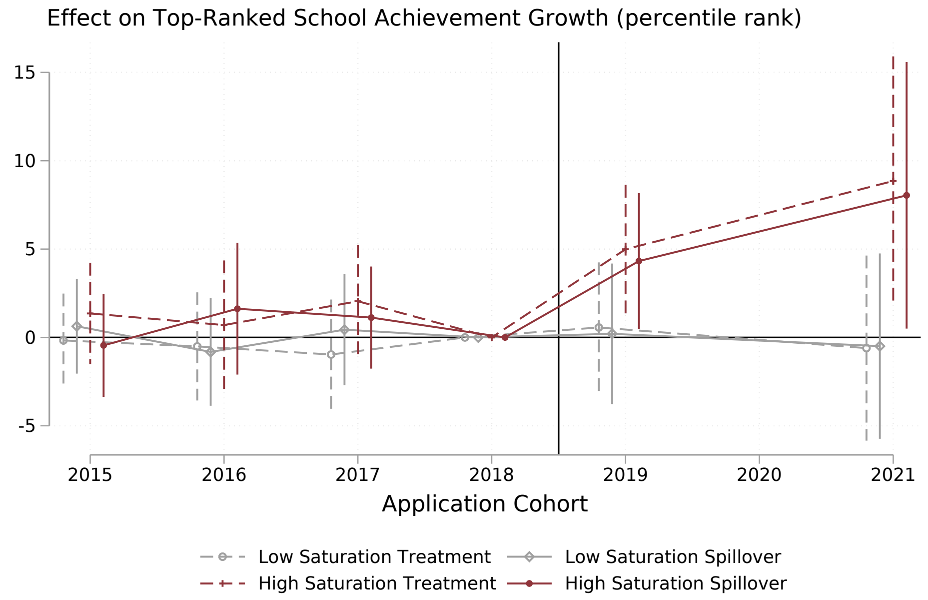
Notes: The front page of the letter is in English, while the second page is in Spanish. The letters list all the relevant schools for the Zone of Choice that the feeder middle school feeds into. The letters provide a simple and short explanation about incoming achievement and achievement growth, along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

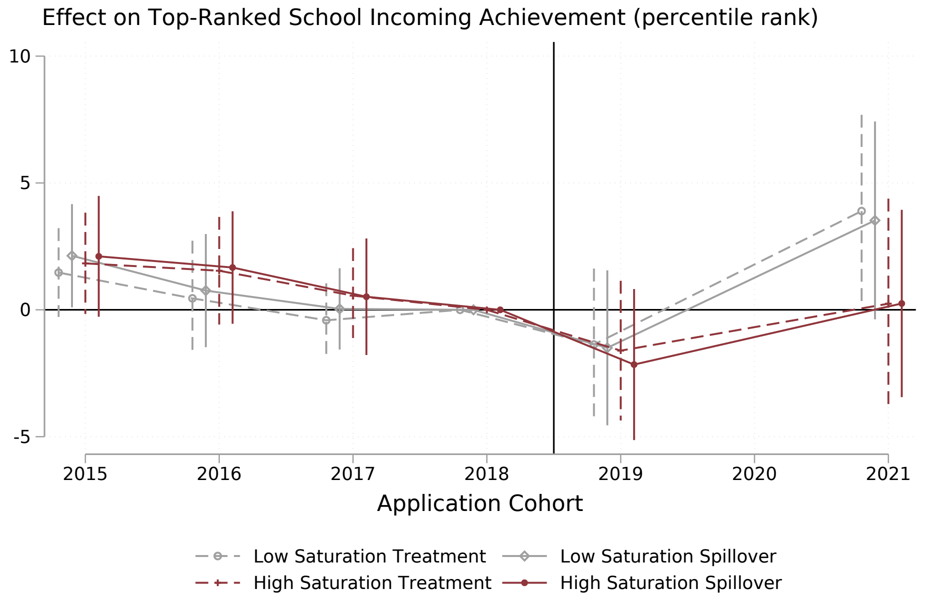


Notes: This figure describes the randomization for a candidate zone with three feeder middle schools. There are certain zones with more than three feeder schools but less than six, so the block sizes were either three or four schools. π_h is the saturation level for high-saturation schools, and π^ℓ is 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



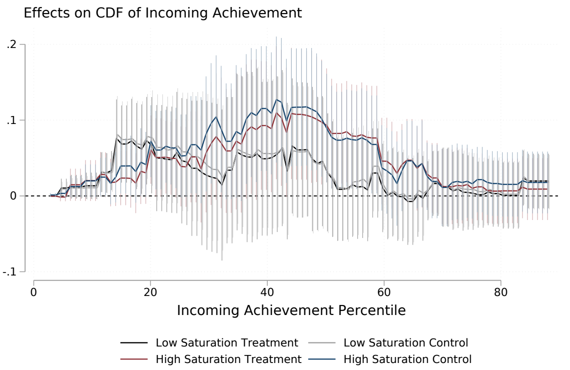
(a) Impacts on Most-Preferred Achievement Growth



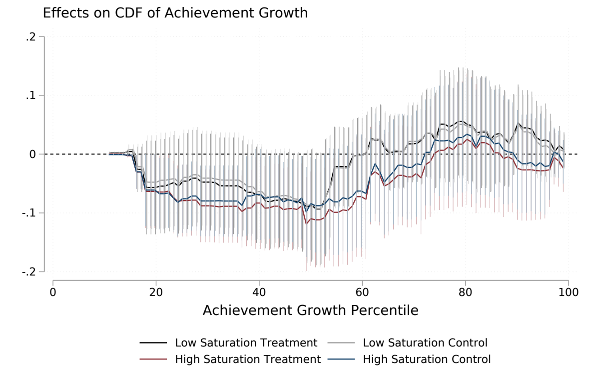
(b) Impacts on Most-Preferred Incoming Achievement

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.

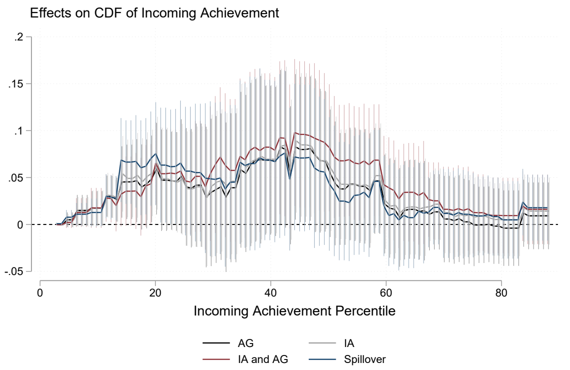
Figure 5: Distributional Estimates



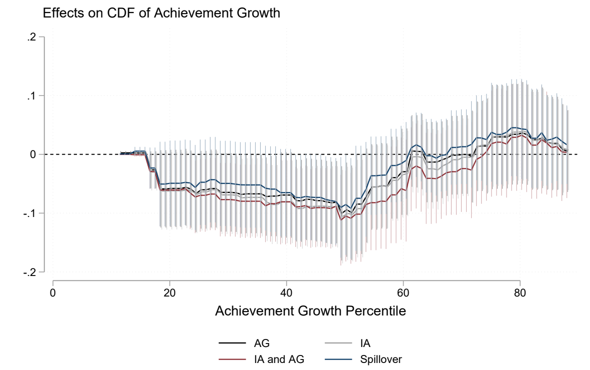
(a) Saturation-Specific Effects on Incoming Achievement



(b) Saturation-Specific Effects on Achievement Growth



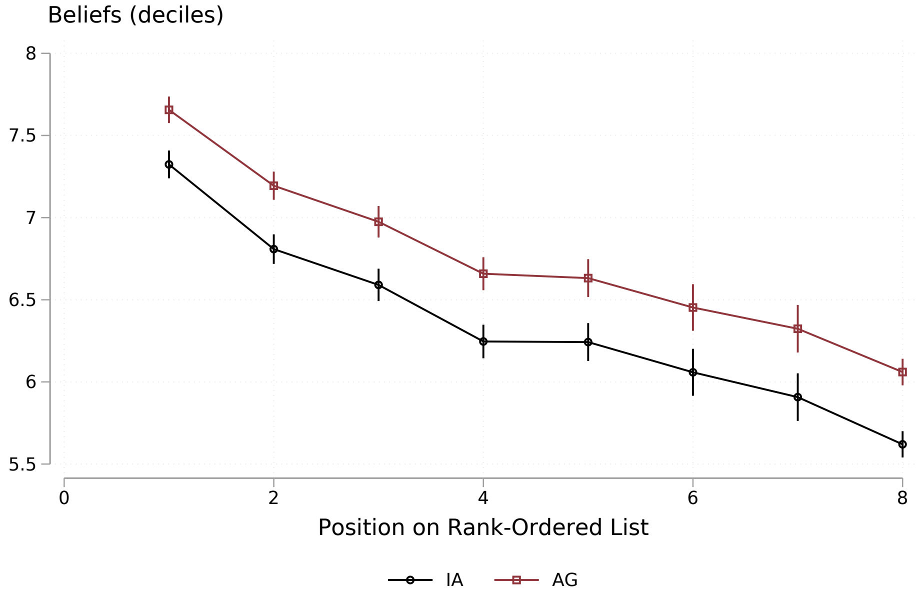
(c) Information-Specific Effects on Incoming Achievement



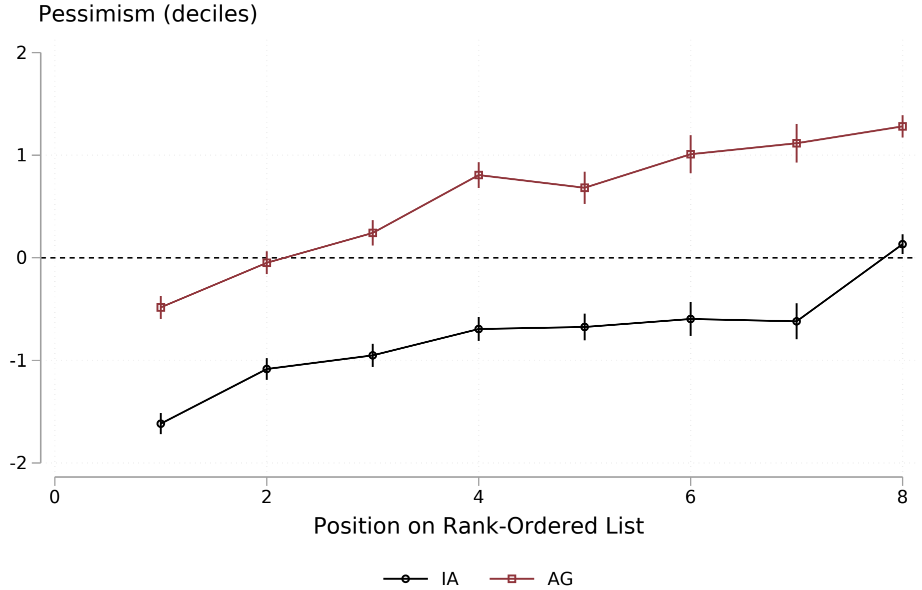
(d) Information-Specific Effects on Achievement Growth

Notes: This figure displays distributional difference-in-difference 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 saturation level and separate treated and spillover groups. Panels (c) and (d) report effects from models that aggregate treatment at the information type, with types corresponding to peer quality (IA), school quality (AG), both, or spillover. Each panel reports 100 estimates at different points of support. Standard errors are robust and clustered at the school level, and 95 percent confidence intervals are reported by vertical bars around each estimate.

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



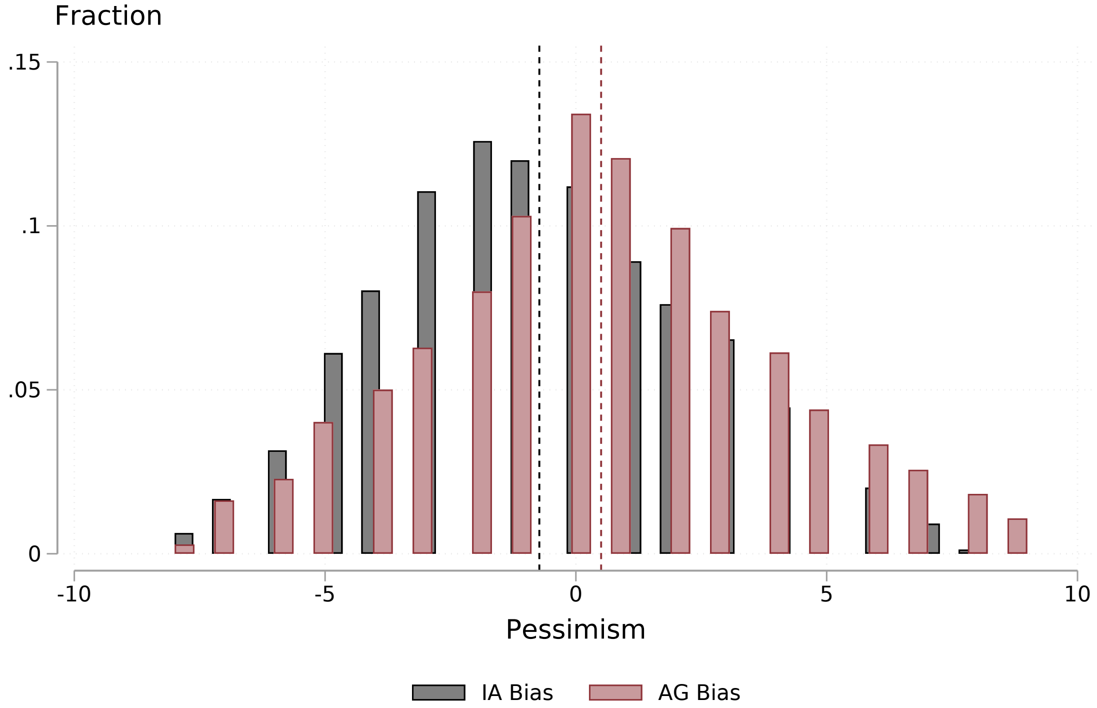
(a) Beliefs



(b) Bias

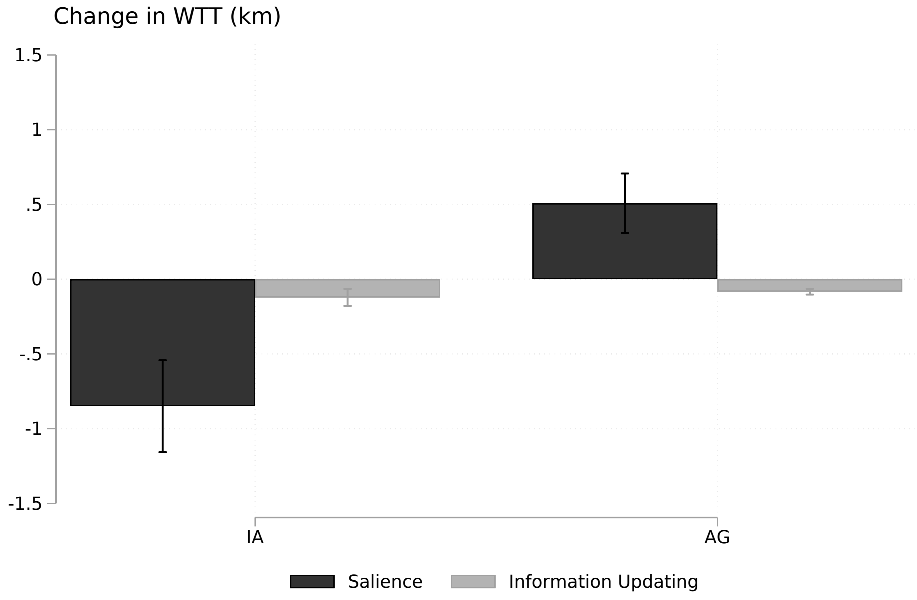
Notes: This figure reports mean beliefs and pessimism for incoming achievement (IA) and achievement growth (AG) at various points of parents' rank-ordered lists. Panel (a) reports mean beliefs and Panel (b) reports mean pessimism. In each subfigure, the black points and line correspond to Incoming Achievement 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

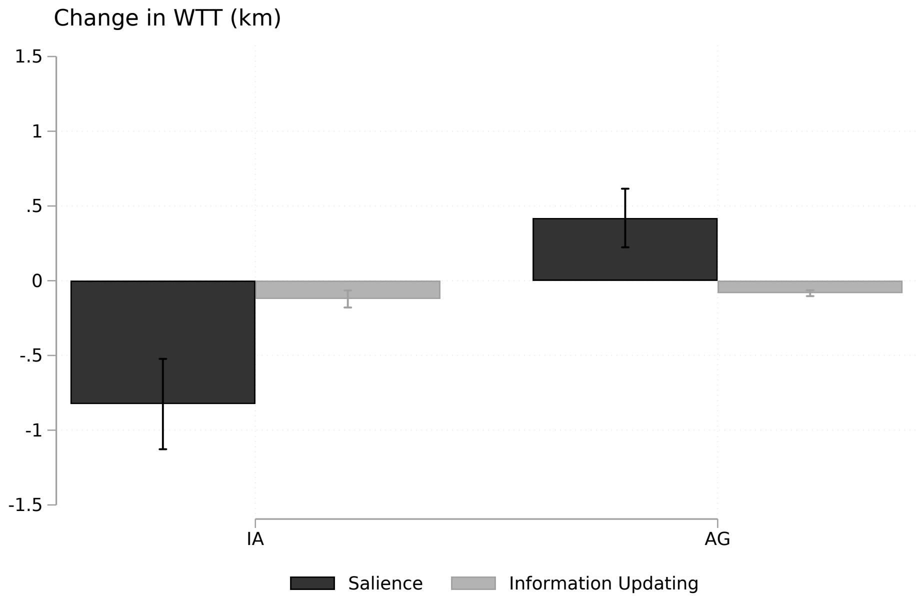


Notes: This figure reports the pessimism distribution for incoming achievement (IA) and achievement growth (AG). Beliefs are collected in terms of deciles, and pessimism is calculated by the difference in between the estimated objective value and the elicited belief. Dashed lines correspond to mean pessimism 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). Standard errors are robust, clustered at the individual application 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	0.003	-0.001	0.006	-0.001
		(0.002)	(0.002)	(0.005)	(0.003)
		[.248]	[.505]	[.183]	[.498]
Migrant	0.082	0.000	0.002*	-0.002	-0.001
		(0.001)	(0.001)	(0.003)	(0.002)
		[.248]	[.068]	[.2]	[.338]
Poverty	0.979	0.000	0.003*	0.005	0.002
		(0.002)	(0.002)	(0.006)	(0.004)
		[.248]	[.133]	[.178]	[.383]
Special Education	0.119	0.003**	0.001	0.004	0.000
		(0.001)	(0.001)	(0.004)	(0.002)
		[.033]	[.355]	[.22]	[.383]
English Learner	0.146	0.002	0.004**	-0.010	0.000
		(0.003)	(0.002)	(0.009)	(0.005)
		[.293]	[.172]	[.153]	[.383]
College	0.054	0.001	-0.002	0.002	0.000
		(0.002)	(0.002)	(0.006)	(0.003)
		[.35]	[.16]	[.418]	[.383]
Black	0.044	0.000	0.000	-0.014	-0.003
		(0.002)	(0.001)	(0.013)	(0.004)
		[.35]	[.16]	[.163]	[.335]
Hispanic	0.908	-0.002	0.002	0.008	0.002
		(0.003)	(0.003)	(0.014)	(0.007)
		[.347]	[.243]	[.248]	[.44]
White	0.019	0.002*	-0.002	0.005	0.001
		(0.001)	(0.001)	(0.004)	(0.002)
		[.188]	[.13]	[.213]	[.453]
Suspension Days	12.310	-0.572	0.162	-1.485	-0.582
		(0.605)	(0.545)	(3.517)	(2.832)
		[.218]	[.415]	[.445]	[.518]
Suspension Incidents	0.007	0.000	0.000	-0.001	0.000
		(0.000)	(0.000)	(0.001)	(0.001)
		[.218]	[.415]	[.195]	[.518]
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. Three stars correspond to $p < 0.01$, two stars corresponds to $p < 0.05$, and three stars correspond to $p < 0.10$, all for p-values associated with the asymptotic standard errors reported in parentheses.

Table 3: Difference-in-Difference Estimates on Top-Listed School Socio-Emotional School Attributes

	(1)	(2)	(3)	(4)	(5)
	Pure Control Mean	High Saturation 2019	Low Saturation 2019	High Saturation 2021	Low Saturation 2021
Bullying Index	0.009	0.006	0.005	0.018**	0.014*
		(0.013)	(0.013)	(0.008)	(0.009)
School Connectedness Index	0.021	0.034***	0.017	0.054**	0.009
		(0.011)	(0.012)	(0.023)	(0.020)
Effort Index	-0.017	0.031**	0.002	0.031*	0.000
		(0.013)	(0.011)	(0.018)	(0.016)
Interpersonal Skills Index	-0.036	0.023**	0.006	0.032**	0.004
		(0.009)	(0.009)	(0.015)	(0.011)
Happiness Index	0.027	0.036***	0.016	0.061**	0.010
		(0.012)	(0.011)	(0.025)	(0.021)
Grit Index	-0.033	0.025**	0.000	0.031**	-0.002
		(0.012)	(0.010)	(0.016)	(0.012)
N			37,060		

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. Three stars correspond to $p < 0.01$, two stars corresponds to $p < 0.05$, and three stars correspond to $p < 0.10$, all for p-values associated with the asymptotic standard errors reported in parentheses.

Table 4: 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. School and peer quality measures are in decile units. 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. Three stars correspond to $p < 0.01$, two stars corresponds to $p < 0.05$, and three stars correspond to $p < 0.10$, all for p-values associated with the asymptotic standard errors reported in parentheses.

Table 5: 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	-0.040	-0.007	0.024	0.065**
		(0.027)	(0.030)	(0.027)	(0.027)
		[0.108]	[0.438]	[0.265]	[0.035]
Interpersonal Skills Index	0.030	-0.059**	-0.003	-0.023	0.055**
		(0.023)	(0.019)	(0.026)	(0.026)
		[0.028]	[0.440]	[0.190]	[0.048]
School Connectedness Index	0.514	-0.014	0.001	0.001	0.035**
		(0.015)	(0.016)	(0.015)	(0.015)
		[0.213]	[0.513]	[0.500]	[0.035]
Academic Effort Index	0.053	-0.042	0.013	-0.005	0.052***
		(0.030)	(0.027)	(0.022)	(0.020)
		[0.078]	[0.390]	[0.418]	[0.058]
Grit Index	0.028	-0.059**	0.019	-0.063**	0.023
		(0.024)	(0.024)	(0.027)	(0.023)
		[0.060]	[0.315]	[0.035]	[0.237]
Bullying Index	0.175	0.048	0.032	0.098***	0.095***
		(0.033)	(0.026)	(0.035)	(0.028)
		[0.142]	[0.205]	[0.020]	[0.013]
Observations			23,280		
		Panel B: Eleventh Grade Test Scores			
Math Score	-0.020	-0.050*	0.019	-0.033	0.000
		(0.027)	(0.032)	(0.032)	(0.028)
		[0.083]	[0.350]	[0.223]	[0.485]
ELA Score	0.069	-0.011	0.110***	0.006	0.056*
		(0.040)	(0.032)	(0.038)	(0.031)
		[0.390]	[0.008]	[0.472]	[0.105]
Observations			23,306		
		Panel C: College Enrollment			
Any College Enrollment	0.580	0.000		0.037**	
		(0.014)		(0.016)	
		[0.493]		[0.018]	
Two Year College Enrollment	0.252	0.000		0.018	
		(0.012)		(0.013)	
		[0.510]		[0.130]	
Four Year College Enrollment	0.387	0.005		0.015	
		(0.015)		(0.014)	
		[0.375]		[0.150]	
Observations			24,939		

Notes: This table reports estimates from separate student-level regressions of the row variable on year indicators, treatment group indicators, a vector of baseline student covariates, and school-level treatment group indicators interacted with treatment year indicators. Panel A corresponds to outcomes measured in the School Experience Survey (SES). Panel B focuses on eleventh-grade test scores. Panel C focuses on college enrollment observed in the National Student Clearinghouse. 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. 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. Three stars correspond to $p < 0.01$, two stars corresponds to $p < 0.05$, and three stars correspond to $p < 0.10$, all for p-values associated with the asymptotic standard errors reported in parentheses.

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Online Appendix for:

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

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A Data Appendix

A.1 School Experience Survey

The School Experience Survey (SES) is an annual survey administered by the Los Angeles Unified School District (LAUSD) every academic year since 2010. The survey is administered to parents, students, and staff. Response rates for students and staff are high, while response rates for parents vary substantially. For example, in the most recent academic year with available survey data, 2022-23, students had a 95% response rate, teachers had a 98% response rate, and parents had a 69% response rate. The survey has evolved over time, with questions entering and leaving the survey in some years, the formatting of questions also changing, and new categories being introduced over time. The analysis I conduct focuses on a somewhat stable part of the student survey that is less prone to changes, the sections I refer to as the core survey elements.

The core survey is organized into three categories, Academics, School Climate, and Social and Emotional Learning. The survey elements mirror data collected by Chicago Public Schools (CPS) studied by Jackson et al. (2020) and many other large urban school districts. Within the Academics category, there are subcategories related to Academic Focus, Cognitive Engagement, Future Orientation, and Technology, with the Technology subcategory being the most recent addition post-pandemic. The School Climate category consists of questions related to Safety, Expectations for Behavior, School Connectedness, and Bullying. The Social and Emotional Learning section contains questions related to Growth Mindset, Responsible Decision-Making, Self Awareness, Self-Efficacy, Self-management, and Student Social Awareness. The categorizations I reference are created by LAUSD.

In recent years, there has been growing emphasis on the importance of socio-emotional development and the potential ways teachers and schools affect these outcomes (Fricke et al., 2019, Jackson et al., 2020, Loeb et al., 2018). Jackson et al. (2020) finds that school impacts on socio-emotional measures in CPS, closely related to socio-emotional measures in the LAUSD SES, are predictive of long-run outcomes and suggestive evidence they are causal. I follow Jackson et al. (2020) in categorizing survey elements as their categorizations have closer associations to a large body of work across economics and psychology (Alan et al., 2019, Duckworth et al., 2007, Heckman and Rubinstein, 2001, Lindqvist and Vestman, 2011).

Using the wealth of data in the survey, I construct five indices that serve as outcomes in my analysis. The first four closely mirror the indices created by Jackson et al. (2020), including an interpersonal skills index, school connectedness index, academic effort index, and bullying index. The fifth is a happiness index which includes elements from the other four but is constructed to more closely isolate school satisfaction. I now report the questions related to each index.

Interpersonal Skills Index : This index consists of six questions. They include the following: During the past 30 days,

1. How often did you compliment others' accomplishments?

2. How well did you get along with students who are different from you?

3. When others disagreed with you, how respectful were you of their views?
4. How clearly were you able to describe your feelings?
5. How carefully did you listen to other people's points of view?

Please answer how often you did the following during the past 30 days,

6. I stayed calm even when others bothered or criticized me.

School Connectedness Index: This index consists of thirteen questions. They include the following: Do you strongly agree, agree, neither agree or disagree, somewhat disagree, or strongly disagree with the following statements:

1. I am happy to be at this school.
2. I fell like I am part of this school.
3. I feel close to people at this school.
4. The teachers at this school treat students fairly.
5. Teachers care if I am absent from school.
6. I feel accepted for who I am at this school.
7. Adults at this school treat all students with respect.
8. I feel safe in this school.
9. I feel safe in the neighborhood around this school.
10. Lesbian, gay, bisexual, transgender, and/or queer students at this school are accepted.
11. Teachers encourage students to make decisions.
12. There are lots of chances for students at my school to get involved in sports, clubs, or other school activities outside of class.
13. I participate in extra-curricular activities offered through my school, such as school clubs or organizations, musical groups, sports teams, student government, or any other activities.

Academic Effort Index: This index consists of ten questions. They include the following: During the past 30 days,

1. I came to class prepared.
2. I remembered and followed directions.
3. I got my work done right away instead of waiting until the last minute.
4. I paid attention even when there were distractions.

Do you strongly agree, agree, neither agree or disagree, somewhat disagree, or strongly disagree with the following statements:

5. School is important for achieving my future goals.

6. When learning new information, I try to put the ideas into my own words.

7. In my classes, I use evidence or collect data to come to my own conclusions.

8. In my classes, I work on projects or assignments with other students.

9. For my assignments, I explain my thinking in writing.

10. In my classes, I think about how to solve problems in new ways.

Bullying Index: This index consists of eight questions. Questions are recoded so that positive means an improvement in bullying outcomes. They include the following: During the past 30 days,

1. How many times on school property have you had mean rumors or lies spread about you?

2. How many times on school property have you been teased about what your body looks like?

3. How many times on school property have you been made fun of because of your looks or the way you talk?

4. How many times on school property have you been pushed, shoved, slapped, hit, or kicked by someone who wasn't just kidding around?

5. How many times on school property have you had sexual jokes, comments, or gestures made at you?

6. How many times have other students from your school bullied you online?

Do you strongly agree, agree, neither agree or disagree, somewhat disagree, or strongly disagree with the following statements:

7. Kids at this school are kind to each other.

8. If I told a teacher or other adult at this school that another student was bullying me, he or she would try to help me.

A.2 School Experience Survey Descriptive Statistics

Table A.1: School Experience Survey AG-IA Correlates

	Univariate	Multivariate
	(1)	(2)
	Incoming Achievement (student σ)	
Bullying Index	1.50***	1.44***
	(0.26)	(0.35)
Connectedness Index	1.08***	0.62
	(0.34)	(0.64)
Effort Index	0.74***	0.07
	(0.24)	(0.57)
Interpersonal Index	0.46*	0.15
	(0.24)	(0.44)
	Achievement Growth (student σ)	
Bullying Index	1.09***	0.89***
	(0.11)	(0.15)
Connectedness Index	0.89***	1.12**
	(0.23)	(0.44)
Effort Index	0.56***	0.28
	(0.14)	(0.19)
Interpersonal Index	0.21	-0.57
	(0.18)	(0.35)
N	280	

Notes: This table reports school-level regression estimates of Incoming Achievement and Achievement Growth (in student standard deviation units) on standardized socio-emotional outcomes. Column 1 reports estimates from univariate regressions, while Column 2 reports estimates from multivariate regressions. All LAUSD high schools are included in the sample. Standard errors are robust and reported in parentheses.

A.3 Experimental Balance

Table A.2: Saturation School-Level Balance

	Control	Low - Control	High - Control
	(1)	(2)	(3)
ELA	-0.094	-0.051	-0.069
		(0.104)	(0.111)
Math	-0.108	-0.054	-0.076
		(0.096)	(0.103)
College	0.082	0.007	-0.012
		(0.024)	(0.028)
Migrants	0.086	-0.011	0.006
		(0.007)	(0.013)
Female	0.495	-0.016	-0.004
		(0.010)	(0.010)
Poverty	0.954	-0.024	0.026
		(0.035)	(0.029)
Special Education	0.115	0.015	0.021
		(0.008)	(0.010)
English Learner	0.158	0.014	0.032
		(0.016)	(0.019)
Black	0.051	-0.007	-0.012
		(0.013)	(0.015)
Hispanic	0.863	-0.011	0.013
		(0.043)	(0.033)
White	0.001	0.000	-0.001
		(0.001)	(0.000)
Number of Schools	41	32	31

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

Table A.3: Within-School Randomization Balance

	Control (1)	Peer - Control (2)	School - Control (3)	Both - Control (4)	P-value (5)
ELA Scores	-0.126	0.006	-0.015	-0.006	0.860
		(0.020)	(0.020)	(0.024)	
Math Scores	-0.124	0.013	-0.010	-0.018	0.607
		(0.017)	(0.016)	(0.019)	
Parents College	0.077	-0.001	-0.001	0.000	0.993
		(0.005)	(0.004)	(0.005)	
Migrant	0.034	0.006	-0.002	0.004	0.182
		(0.004)	(0.004)	(0.003)	
Female	0.485	-0.005	0.001	0.003	0.892
		(0.009)	(0.010)	(0.008)	
Poverty	0.938	0.001	0.000	-0.005	0.561
		(0.004)	(0.003)	(0.004)	
Special Education	0.138	-0.002	0.008	-0.002	0.597
		(0.006)	(0.007)	(0.006)	
English Learners	0.152	0.002	0.001	0.013	0.324
		(0.005)	(0.006)	(0.007)	
Black	0.031	0.002	-0.004	0.002	0.663
		(0.003)	(0.003)	(0.004)	
Hispanic	0.906	-0.004	0.003	-0.005	0.506
		(0.005)	(0.005)	(0.004)	
White	0.016	-0.002	0.000	0.001	0.802
		(0.002)	(0.002)	(0.002)	
Joint Test P-value		0.769	0.951	0.716	

Notes. Column 1 reports within-school control group means, and Columns 2–4 contain mean differences between treated and control group individuals. Column 5 contains p-values on a joint test of equality of means across groups for that given row. The p-values reported on the bottom of the table come from a column-wise test of no difference between the treated and control groups. Note that the population in this table is those assigned to non-pure control schools. Standard errors are clustered at the school level for all tests.

A.4 Treatment Letters

Figure A.1: Incoming Achievement Treatment Letter Example: Belmont 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.

Belmont Zone of Choice

We determine the quality of a school based on students' average scores on state exams

Some schools have high scores because they attract high-achieving students. We can measure a school's ability to attract high-achieving students by measuring the average test scores of their incoming students—**Incoming Achievement**. 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.

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

School	Incoming Achievement*	Campus Location	Type of School
School Of Social Justice	37	Miguel Contreras LC	Pilot School
Science, Arts & Green Engineering	27	Belmont HS	Small Learning Community
Computer Science Academy	66	Edward R. Roybal LC	Small Learning Community
Academy Of Educational Empowerment: School of Medicine and Law	58	Edward R. Roybal LC	Small Learning Community
Visual Arts Academy	72	Ramon C. Cortines School for the Visual & Performing Arts	Small Learning Community
School Of Business & Tourism	31	Miguel Contreras LC	Pilot School
Los Angeles School Of Global Studies	19	Miguel Contreras LC	New Technology HS
Dance Academy	60	Ramon C. Cortines School for the Visual & Performing Arts	Small Learning Community
Music Academy	54	Ramon C. Cortines School for the Visual & Performing Arts	Small Learning Community
Academy of Social Work and Child Development: Spanish Dual Language Program	75	Edward R. Roybal LC	Small Learning Community
Los Angeles Academy Of Medical & Public Services	47	Belmont HS	Small Learning Community
Theater Academy	44	Ramon C. Cortines School for the Visual & Performing Arts	Small Learning Community
Academic Leadership Community School	42	Miguel Contreras LC	Small Learning Community
Business & Finance Academy	59	Edward R. Roybal LC	Small Learning Community
Multimedia Academy Of Film And Photography	19	Belmont HS	Small Learning Community

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

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 Belmont

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

Algunas escuelas tienen puntuaciones altas porque atraen a estudiantes de alto rendimiento. Podemos medir la capacidad de una escuela para atraer estudiantes de alto rendimiento midiendo los puntajes promedio de las pruebas de sus estudiantes entrantes: **rendimiento entrante**. 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.

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

Escuela	Rendimiento Entrante*	Ubicación del campus	Tipo de escuela
Escuela de Justicia Social	37	Miguel Contreras LC	Escuela Piloto
Escuela de Ciencia, Arte e Ingeniería Ecológica	27	Belmont HS	Comunidad Educativa Pequeña (SLC)
Academia de Ciencias de Computación	66	Edward R. Roybal LC	Comunidad Educativa Pequeña (SLC)
Academia de Empoderamiento Educativo: Escuela de Medicina y Leyes	58	Edward R. Roybal LC	Comunidad Educativa Pequeña (SLC)
Academia de Artes Visuales	72	Ramon C. Cortines School for the Visual & Performing Arts	Comunidad Educativa Pequeña (SLC)
Escuela de Negocios y Turismo	31	Miguel Contreras LC	Escuela Piloto
Preparatoria de Estudios Globales de Los Ángeles	19	Miguel Contreras LC	Escuela Secundaria de Tecnología
Academia de Danza	60	Ramon C. Cortines School for the Visual & Performing Arts	Comunidad Educativa Pequeña (SLC)
Academia de Música	54	Ramon C. Cortines School for the Visual & Performing Arts	Comunidad Educativa Pequeña (SLC)
Academia del Trabajo Social y Desarrollo Infantil: Programa de Lenguaje Dual en Español	75	Edward R. Roybal LC	Comunidad Educativa Pequeña (SLC)
Academia de Servicios Médicos y Públicos de Los Ángeles	47	Belmont HS	Comunidad Educativa Pequeña (SLC)
Academia de Teatro	44	Ramon C. Cortines School for the Visual & Performing Arts	Comunidad Educativa Pequeña (SLC)
Escuela Académica, Liderazgo y Comunidad	42	Miguel Contreras LC	Comunidad Educativa Pequeña (SLC)
Academia de Negocios y Finanzas	59	Edward R. Roybal LC	Comunidad Educativa Pequeña (SLC)
Academia Multimedia de Cine y Fotografía	19	Belmont HS	Comunidad Educativa Pequeña (SLC)

*El rendimiento entrante 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.

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, 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 A.2: Achievement Growth Treatment Letter Example: South Gate 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.

South Gate Zone of Choice

We determine the quality of a school based on students' average scores on state exams

Some schools have high average scores because their students experience larger learning gains—**achievement growth**. We can measure a school's ability improve test scores by measuring the growth of their students' test scores between entry into the school and some later date. We provide each school's district-wide ranking below and hope you use this information when choosing the right school for your student.

Achievement Growth

We measure a school's ability to 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	Achievement Growth*	Campus Location	Type of School
Media & Communications	77	South Gate HS	Small Learning Community
Law, Government & Public Service	80	South Gate HS	Small Learning Community
School Of Health Science & Environment	90	South East HS	Small Learning Community
Science, Technology, Engineering, Arts & Math (STEAM) High School	94	Legacy HS	Small School
Visual & Performing Arts (VAPA) High School	67	Legacy HS	Small School
School Of Business, Innovation & Leadership	95	South East HS	Small Learning Community
International Studies Learning Center	97	Legacy HS	Small School
School Of Justice & Law	99	South East HS	Small Learning Community
Math, Science & Engineering	83	South Gate HS	Small Learning Community
School Of Visual & Performing Arts	98	South East HS	Small Learning Community
Business & Technology	75	South Gate HS	Small Learning Community
Health Science & Medicine	80	South Gate HS	Small Learning Community

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

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 South Gate

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

Algunas escuelas tienen puntajes altos porque sus estudiantes experimentan mayores ganancias de aprendizaje: **crecimiento de logros**. Podemos medir 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 una fecha posterior. A continuación, proporcionamos la clasificación de cada escuela en todo el distrito.

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	Crecimiento de logros*	Ubicación del campus	Tipo de escuela
Medios de Comunicación	77	South Gate HS	Comunidad Educativa Pequeña (SLC)
Ley, Gobierno y Servicio Público	80	South Gate HS	Comunidad Educativa Pequeña (SLC)
Escuela de Ciencia de Salud y del Medioambiente	90	South East HS	Comunidad Educativa Pequeña (SLC)
Preparatoria de Ciencia, Tecnología, Ingeniería, Artes y Matemáticas (STEAM)	94	Legacy HS	Escuela Pequeña
Preparatoria de Artes Visuales y Técnicas (VAPA)	67	Legacy HS	
Escuela de Negocio, Innovación y Liderazgo	95	South East HS	Comunidad Educativa Pequeña (SLC)
Centro de Aprendizaje de Estudios Internacionales	97	Legacy HS	Escuela Pequeña
Escuela de Justicia y Ley	99	South East HS	Comunidad Educativa Pequeña (SLC)
Matemáticas, Ciencia e Ingeniería	83	South Gate HS	Comunidad Educativa Pequeña (SLC)
Escuela de Artes Visuales y Escénicas	98	South East HS	Comunidad Educativa Pequeña (SLC)
Negocios y Tecnología	75	South Gate HS	Comunidad Educativa Pequeña (SLC)
Ciencia de Salud y Medicina	80	South Gate HS	Comunidad Educativa Pequeña (SLC)

*El crecimiento del logros de las escuelas se proporcionan en percentiles. Por ejemplo, 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 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.

B Peer and School Quality Estimation

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

$$Y_{ij} = \mu_j + a_i \tag{8}$$

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

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

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

$$Y_i = \mu_0 + \sum_j \alpha_j D_{ij} + \gamma' X_i + u_i \tag{9}$$

which is the canonical causal value-added model considered in the literature (Campos and Kearns, 2024). In estimation, however, u_i need not be uncorrelated with D_{ij}, and $\alpha_j \neq \mu_j - \mu_0$.

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

B.1 VAM Validation

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

$$Y_i = \xi + \phi \widehat{Y}_i + \sum_\ell \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i' \delta + \varepsilon_i \tag{10}$$

$$\widehat{Y}_i = \psi + \sum_\ell \pi_\ell Z_{i\ell} + \boldsymbol{X}_i' \xi + e_i. \tag{11}$$

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

Table B.1 reports results for two value-added models. Column 1 reports results for a model that considers test-score VA. Column 2 reports estimates from a model that considers socioemotional VA as defined in Campos et al. (2025). For both value-added measures, we cannot reject the estimates are forecast unbiased. While the results in Table B.1 do not entirely rule out bias in OLS value-added estimates, they are reassuring.

Table B.1: Forecast Bias and Overidentification Tests

	Test Score VA	Socioemotional VA
	(1)	(2)
Forecast Coefficient	0.907	0.989
	(0.067)	(0.147)
$H_0 : \phi = 1$	0.168	0.942
F	10.971	4.987
Overidentification p-val	0.127	0.055

Notes: This table reports the results of lottery-based tests for bias in estimates of school effectiveness. The sample is restricted to students in the baseline sample who applied to an oversubscribed LAUSD school subject to a lottery. Column (1) considers test score value-added and Column (2) considers value-added on an index of socio-emotional value-added. See Campos et al. (2025) for details. The forecast coefficients and overidentification tests reported in Columns (1)–(2) come from two-stage least squares regressions of test scores on OLS-fitted values estimated separately, instrumenting OLS-fitted values with school-cohort-specific lottery offer indicators, controlling for baseline characteristics (Angrist et al., 2017).

B.2 School and Peer Quality Measures

School average achievement follows from Equation 9

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

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

$$Q_j^S = \text{int}\left(\frac{\text{rank}(\hat{\alpha}_j)}{J} \times 100 \right) \tag{12}$$

$$Q_j^P = \text{int}\left(\frac{\text{rank}(\hat{\gamma}' \bar{X}_j)}{J} \times 100 \right) \tag{13}$$

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

B.3 Peer Effects

In this section, I briefly assess the potential influence of peer effects. The constant effects model does not explicitly model peer effects or the influence of the student body on school quality. An extreme case would have peer effects entirely mediate value-added estimates, so in this section, I explore that potential with observables.

A linear-in-means model would suggest school quality is

$$\hat{\alpha}_j = \alpha_j + \delta \bar{X}_j.$$

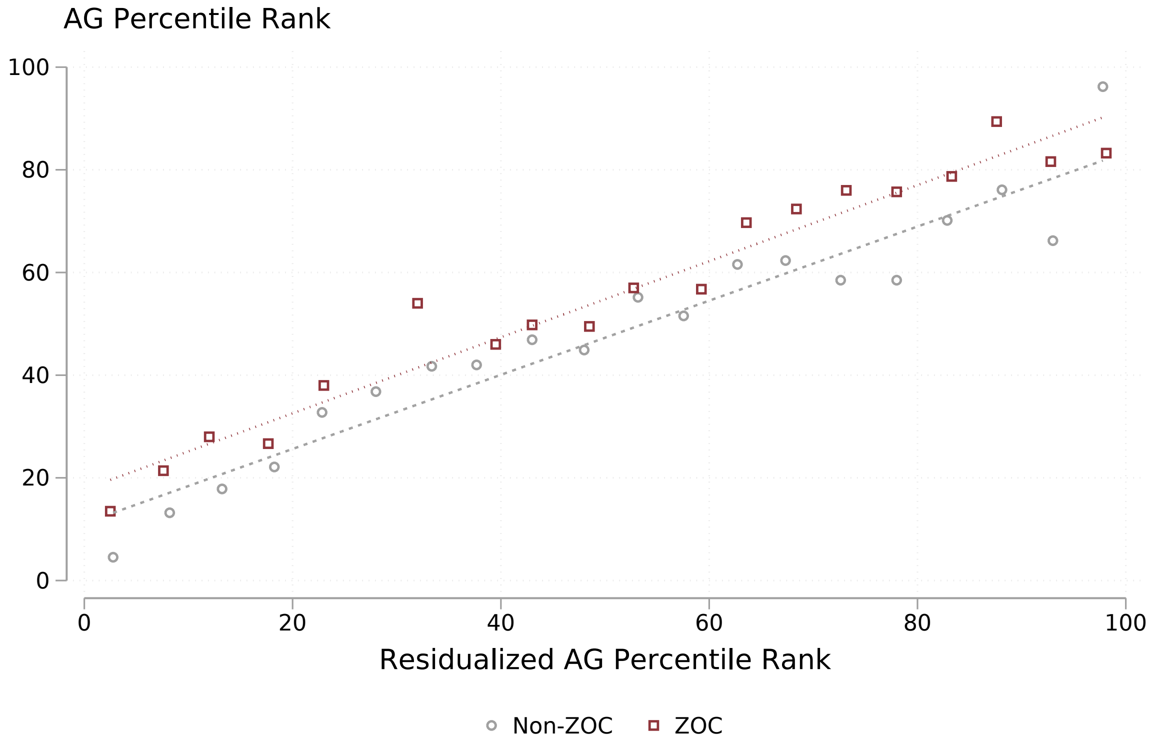
We can assess this possibility by relating estimated values of $\hat{\alpha}_j$ to $\bar{X}_j$. Appendix Table B.2 demonstrates that estimated school quality is unrelated to essentially all of the observables in the data. In particular, lagged achievement is not a strong predictor of school quality both unconditionally and conditional on other observables. Evidence notwithstanding, one may still have chosen to regression adjust school quality estimates to remove the influence of student attributes. Appendix Figure B.1 shows that doing so produces minimal changes in the ordinal ranking of schools and, as a consequence, would have minimally affected the information contained in treatment letters. The evidence in this section suggests peer effects do not play a significant role in mediating school quality estimates.

Table B.2: Relationship between α_j and student observables

	(1)	(2)	(3)	(4)
	α	α	α	α
Poverty Share			0.4573	0.5344
			(0.3258)	(0.3552)
Black Share			-0.6247	-0.6173
			(0.3647)	(0.3850)
White Share			-0.5110	-0.4251
			(0.5157)	(0.5625)
College Share			0.4637	0.3071
			(0.9182)	(0.9399)
English Learner Share			-0.4083	-0.3489
			(0.3652)	(0.4032)
English at Home Share			0.1554	-0.0106
			(0.3367)	(0.3765)
Spanish at Home Share			0.2423	0.0917
			(0.2490)	(0.2906)
Special Education Share			0.2443	0.3085
			(0.4116)	(0.3992)
Female Share			0.0375	0.0584
			(0.1394)	(0.1366)
Migrant Share			0.2889	0.2122
			(0.3358)	(0.3625)
Lagged ELA Achievement	0.0531			0.0231
	(0.0472)			(0.0841)
School Enrollment		0.0003		0.0004
		(0.0004)		(0.0003)
R-squared	0.011	0.010	0.156	0.176

Notes: This table reports bivariate and multivariate relationships between estimated school quality and school-level observables. Column (1) reports the bivariate relationship between estimated school quality and school average achievement levels. Column (2) reports the bivariate relationship between school quality and school size. The following two columns report multivariate relationships between school quality and an array of school attributes. Robust standard errors are reported in parentheses.

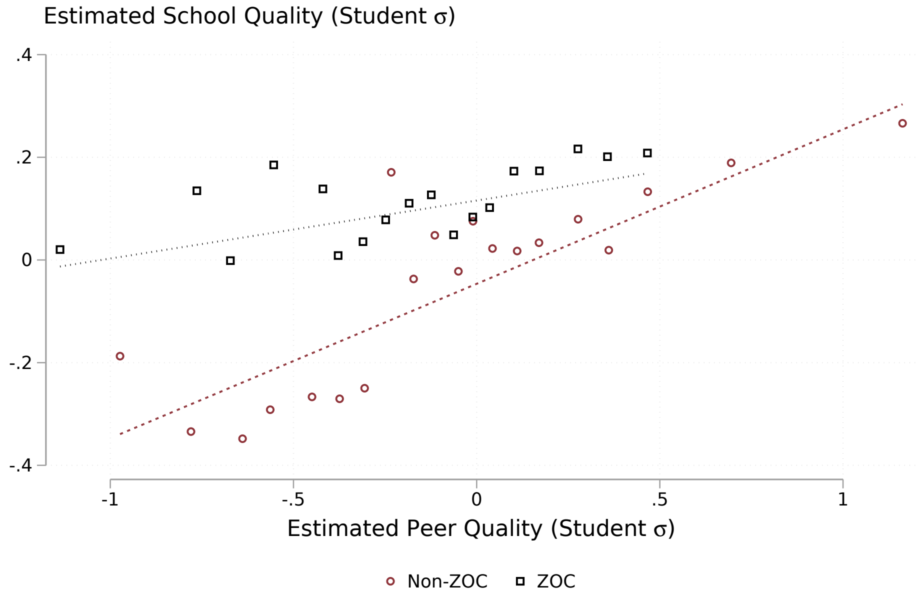
Figure B.1: Rank-rank Correlation Between Estimated School Quality and Regression-Adjusted School Quality



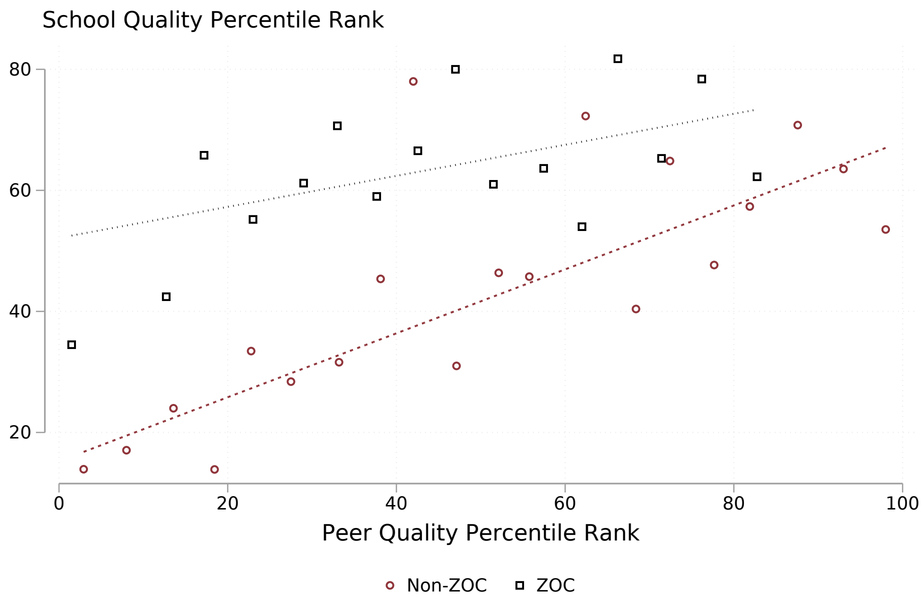
Notes: This figure reports the rank-rank relationship between estimated school quality used in the intervention and an alternative that regression adjusts for observable school-level attributes. The rank-rank relationship is reported separately for ZOC and non-ZOC schools; the differences are not statistically significant or meaningful.

B.4 Summary Statistics

Figure B.2: AG-IA Bivariate Relationship



(a) Student Standard Deviation Units



(b) Percentile Rank Units

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

C Additional Experiment Results

In this section, I report additional experimental evidence discussed in the main paper. To begin, I report disaggregated estimates for each experimental arm and evidence regarding other outcomes of interest. Heterogeneity results follow. I also report additional impacts on enrollment outcomes and the reduced form estimates implied by the structural model estimated in the paper. I also report evidence documenting the lack of parental coordination efforts by school-specific rank-concordance measures and assessing how the intervention affected them. I conclude with evidence discussed in the paper but with corresponding randomization-based inference.

C.1 Additional Evidence and Outcomes

The experiment's design contains eight treatment groups whose effects can be estimated using the following regression specification

$$\begin{aligned} Y_i = \alpha_z &+ \underbrace{\beta_{Ph} T_i^P \times D_{s(i)}^h + \beta_{Sh} T_i^S \times D_{s(i)}^h + \beta_{Bh} T_i^B \times D_{s(i)}^h}_{High\ Saturation\ Effects} \\ &+ \underbrace{\beta_{P\ell} T_i^P \times D_{s(i)}^\ell + \beta_{S\ell} T_i^S \times D_{s(i)}^\ell + \beta_{B\ell} T_i^B \times D_{s(i)}^\ell}_{Low\ Saturation\ Effects} \\ &+ \underbrace{\beta_h C_i \times D_{s(i)}^h + \beta_\ell C_i \times D_{s(i)}^\ell}_{Spillover\ Effects} + u_i, \end{aligned} \tag{14}$$

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

Appendix Table C.1 and Appendix Table C.2 report estimates for the 2019 and 2021 wave, respectively. Column 1 reports effects on most-preferred school AG, and Column 2 reports effects on most-preferred IA. Each column reports estimates for the eight parameters from the full specification. Effect sizes tend to be similar within saturation group. For example, I cannot reject that most preferred AG impacts are the same for those in the high-saturation treatment arm regardless of being directly treated or in the spillover group. The same is true for most-preferred IA. The evidence motivates the aggregation of the evidence reported throughout the paper.

C.1.1 Heterogeneity

Prior information interventions tend to find that relatively advantaged families and students are more responsive to information, exacerbating existing gaps that information interventions aim to address (Cohodes et al., 2022, Corcoran et al., 2018). In the ZOC setting, there is less variation in socioeconomic status but there is variation in student's baseline achievement, so I focus on that.

Appendix Table C.3 summarizes the evidence. Panel A reports treatment effects on the most preferred incoming achievement for various groups of students categorized based on their baseline achievement levels. Although most estimates are not distinguishable from each other statistically, there is suggestive evidence that higher-achieving families are most responsive to incoming achievement information. It is also worth noting that higher-achieving families tend to apply to schools with higher achievement levels. This finding mirrors evidence in Corcoran et al. (2018) in that relatively advantaged families are more responsive to information treatments.

Panel B reports similar evidence for most-preferred achievement growth. To begin, I find that higher-achieving families in the control group rank better schools at the top of their list in terms of their achievement growth. Mirroring the evidence displayed in Figure 4, most impacts are detected among parents in high-saturation schools. In the first experimental wave, I find the most pronounced effects among low-achieving and moderately-low-achieving families, that is, students performing below district averages on standardized exams at baseline. In the second experimental wave, I find mostly similar effects across the various achievement groups. Throughout, however, differences are noisy and indistinguishable from statistical noise so they are suggestive at best. The evidence does suggest that the intervention reduced achievement-based differences in accessing higher-quality schools in the first experimental wave and kept it constant in the second experimental wave.

Table C.1: Baseline Experimental Effects 2019 Wave

	(1) AG	(2) IA
High Saturation Treatment		
Peer Quality	3.966	-5.222**
	(3.259)	(2.462)
School Quality	3.117	-5.317**
	(3.164)	(2.373)
Both	3.123	-4.991**
	(3.217)	(2.396)
Low Saturation Treatment		
Peer Quality	1.885	-5.294*
	(2.803)	(2.821)
School Quality	0.495	-4.719*
	(2.997)	(2.806)
Both	3.376	-5.213*
	(2.805)	(2.807)
Spillover Treatment		
High Saturation	2.322	-5.867**
	(2.843)	(2.444)
Low Saturation	1.519	-5.267*
	(2.814)	(2.839)
Pure Control Mean	65.739	45.749
R2	0.240	0.400
N	11,541	11,541

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

Table C.2: Baseline Experimental Effects, 2021 Wave

	(1) AG	(2) IA
High Saturation Treatment		
Peer Quality	6.307	-3.007
	(4.156)	(2.160)
School Quality	7.816**	-2.659
	(3.717)	(2.370)
Both	7.241*	-3.852*
	(4.029)	(2.226)
Low Saturation Treatment		
Peer Quality	0.871	0.563
	(3.410)	(2.231)
School Quality	0.205	0.079
	(3.416)	(2.480)
Both	1.322	1.037
	(3.369)	(2.317)
Spillover Treatment		
High Saturation	5.910	-3.308*
	(4.090)	(1.949)
Low Saturation	0.787	0.171
	(3.313)	(2.274)
Pure Control Mean	66.914	51.647
R2	0.290	0.380
N	9,008	9,008

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

Table C.3: Heterogeneity Results

	(1) Pure Control Mean	(2) High Saturation 2019	(3) Low Saturation 2019	(4) High Saturation 2021	(5) Low Saturation 2021
		Panel A: Incoming Achievement Percentile			
Low Achievers	33.402	-1.785	-1.061	-2.673	1.099
		(1.421)	(1.527)	(2.261)	(1.729)
Moderate Low Achievers	36.428	-1.769	-0.071	-0.112	2.358
		(1.479)	(1.500)	(2.325)	(1.551)
Moderate High Achievers	37.352	-2.186	-1.704	0.060	4.787***
		(1.420)	(1.337)	(2.280)	(1.294)
High Achievers	40.900	-1.664	-1.996*	-1.635	3.616*
		(1.074)	(1.158)	(2.605)	(2.014)
		Panel B: Achievement Growth Percentile			
Low Achievers	63.966	5.293***	0.336	8.296*	-1.788
		(1.714)	(1.399)	(4.553)	(2.173)
Moderate Low Achievers	65.990	3.475**	1.906	7.587**	-1.068
		(1.707)	(1.487)	(3.559)	(2.953)
Moderate High Achievers	66.752	1.027	-0.730	5.615	-0.852
		(2.119)	(1.819)	(3.660)	(2.022)
High Achievers	67.700	2.755	-0.007	6.698**	1.886
		(1.737)	(1.446)	(3.219)	(2.371)

Notes: This table reports cluster-specific treatment and spillover effects on top-listed Incoming Achievement (in Panel A) and Achievement Growth (in Panel B) for various subgroups of students. The group Low Achievers corresponds to students whose baseline test scores are -0.5σ or below. Moderate Low Achievers are students whose baseline test scores are between -0.5σ and 0, while Moderate High Achievers are students whose baseline test scores are between 0 and 0.5σ. High Achievers are students whose baseline test scores are 0.5σ or greater. Each row corresponds to estimates from a separate regression conditioning on the group. As elsewhere in the paper, standard errors are robust and clustered at the school level and reported in parentheses.

C.2 Zone-Specific Heterogeneity

Table C.4: Correlation of Zone-Specific Treatment Effects Across Treatment Groups

	High	Low	Spillover High	Spillover Low
	(1)	(2)	(3)	(4)
			Panel A: School Quality	
High	1	0.79	0.99	0.77
Low	-	1	0.83	0.99
Spillover High	-	-	1	0.82
Spillover Low	-	-	-	1
Treatment Effect SD	15.08	9.07	14.43	9.32
Share of Variation Explained by Zone Effects			0.827	
			Panel B: Peer Quality	
High	1	0.73	0.97	0.73
Low	-	1	0.72	1.00
Spillover High	-	-	1	0.72
Spillover Low	-	-	-	1
Treatment Effect SD	9.84	10.51	9.47	10.32
Share of Variation Explained by Zone Effects			0.817	

Notes: This table reports correlations between school-and-treatment-specific effects. Each cell corresponds to a correlation coefficient. The groups High, Low, Spillover High, and Spillover Low correspond to saturation-specific groups that are further differentiated by treatment or spillover status. Panel A reports correlations between treatment effects for a model that considers school quality rankings as the outcome variable and the regression model is analogous to Equation 1 with zone-specific treatment effects for each of the four treatment groups. Panel B reports correlations between treatment effects for a model that considers peer quality rankings as the outcome variable. The final row in each panel reports the noise-adjusted standard deviation of market-specific effects on the outcome for the given panel and the treatment group defined by the column, where the noise-adjusted variance is calculated using the following formula:

$$\frac{1}{M}\sum_{m=1}^{M}(\hat{\beta}_m^k - \bar{\beta}^k)^2 - SE(\hat{\beta}_m^k)^2)$$

where m is an index for a market and k is an index for a treatment group, so that $k \in \{High, Low, Spillover\ High, Spillover\ Low\}$. Each panel reports an estimate of the share of variation explained by zone-specific effects.

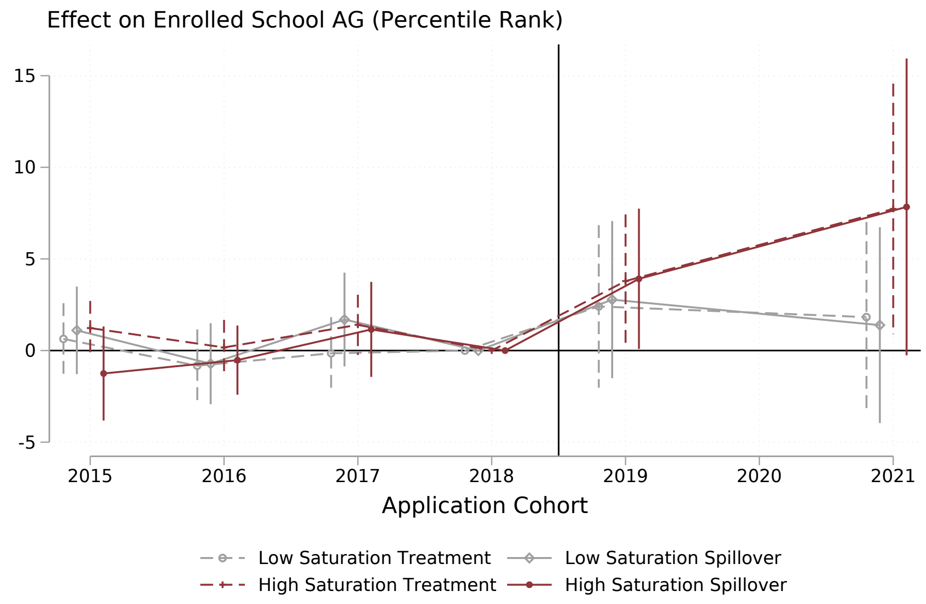
Table C.5: Heterogeneity by Baseline Demand and Exposure to Treatment

	(1)	(2)	(3)	(4)
	Achievement Growth		Incoming Achievement	
	Low	High-Low	Low	High-Low
	Panel A: Heterogeneity by Baseline Demand			
2019 Low Saturation Cohort	7.077	-14.331	-0.895	-2.595
	(3.740)	(4.498)	(3.258)	(4.387)
2021 Low Saturation Cohort	9.175	-20.845	4.803	-3.673
	(4.436)	(5.197)	(2.715)	(4.233)
2019 High Saturation Cohort	5.205	-0.014	-3.431	2.464
	(4.091)	(6.005)	(2.664)	(4.014)
2021 High Saturation Cohort	11.491	-6.821	-1.414	2.548
	(5.304)	(7.893)	(3.018)	(3.895)
	Panel B: Heterogeneity by Proximity to Treated Students			
2019 Low Saturation Cohort	0.505	2.079	-2.259	5.916
	(2.281)	(2.403)	(1.898)	(2.532)
2021 Low Saturation Cohort	-0.547	3.196	3.220	0.641
	(3.342)	(3.432)	(2.174)	(2.344)
2019 High Saturation Cohort	4.084	-1.851	-2.489	-2.260
	(1.937)	(1.896)	(1.424)	(1.884)
2021 High Saturation Cohort	7.806	-0.287	-0.543	-2.665
	(4.393)	(3.198)	(2.336)	(2.137)

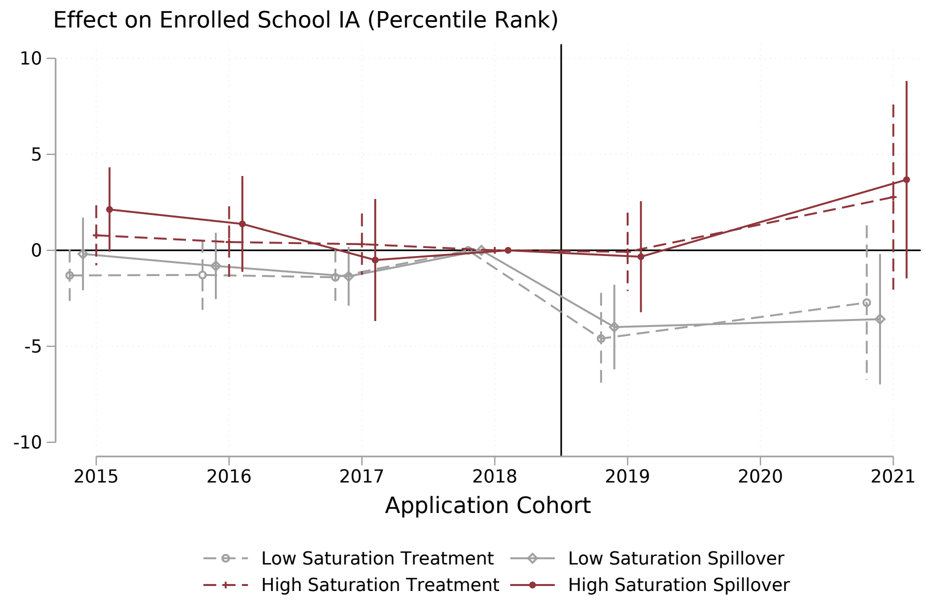
Notes: This table reports estimates from multiple regressions that assess heterogeneity in the baseline results. Panel A considers heterogeneity by feeder-school level baseline demand for effective schools, implicitly defined by the percent of families that rank the top-ranked AG school as their most-preferred in the pre-intervention year. Panel B considers heterogeneity by exposure to other treated parents, implicitly defined by the total number of treated parents residing within a student's same Census block. In all estimates, we consider models where the outcomes is a student's most-preferred AG (or IA) school ranking; Columns 1 and 2 correspond to models where AG is the outcome and Columns 3 and 4 correspond to models where IA is the outcome. Within a panel, estimates from Columns 1 and 2 come from the same model, while estimates from Columns 3 and 4 come from a separate regression model. The labels "Low" correspond to belonging to the Low group in terms of the heterogeneity; for example, for Panel A being in the low group corresponds to belonging to a school whose baseline demand for effective schools is in the bottom three quartiles of the feeder-school distribution, and for Panel B, Low corresponds to belonging to bottom three quartiles of the exposure distribution. The labels "High-Low" correspond to a column of estimate that respond differentials between the High and Low groups. For a given pair of columns within a panel—that correspond to the same regression model—we report cohort-and-saturation-specific effects. We report a standard error under each estimate in parentheses where standard errors are robust and clustered at the feeder school level.

C.2.1 Impacts on Enrollment

Figure C.1: Difference-in-Difference Estimates



(a) Impacts on Enrolled School Achievement Growth

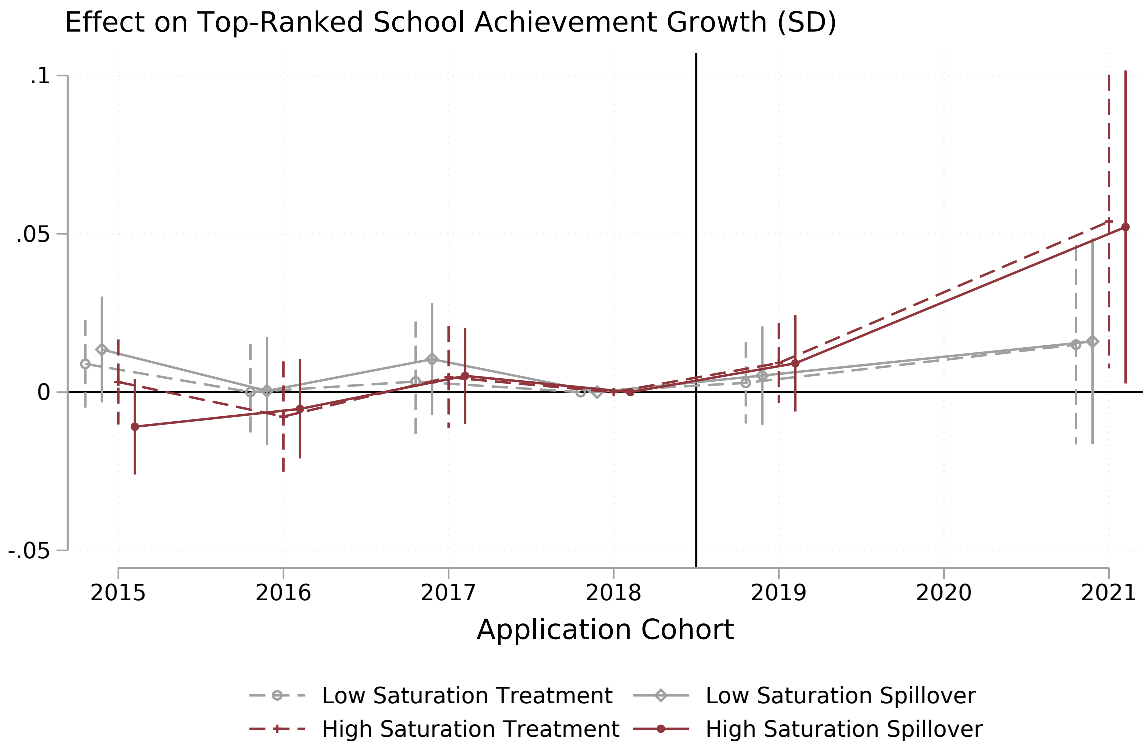


(b) Impacts on Enrolled School Incoming Achievement

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

C.2.2 Measuring AG and IA in Student SD Units

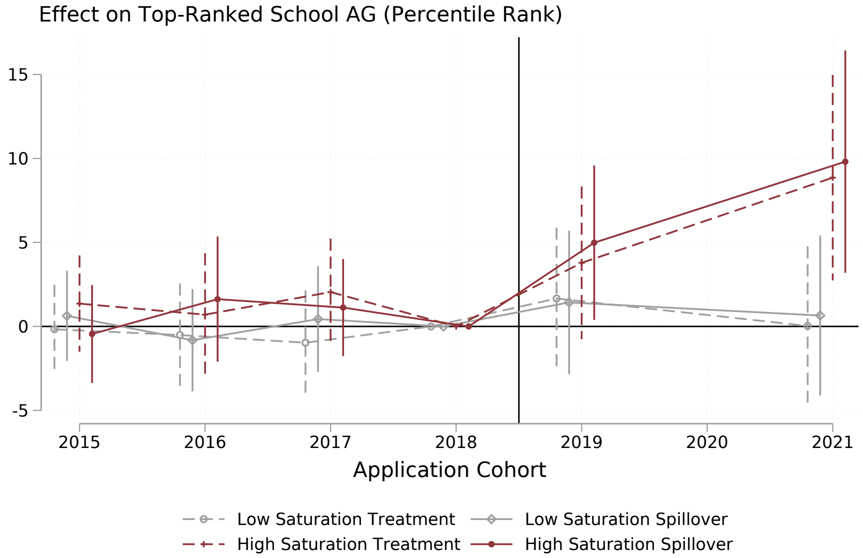
Figure C.2: Impacts on Top-Ranked School Achievement Growth in Student SD Units



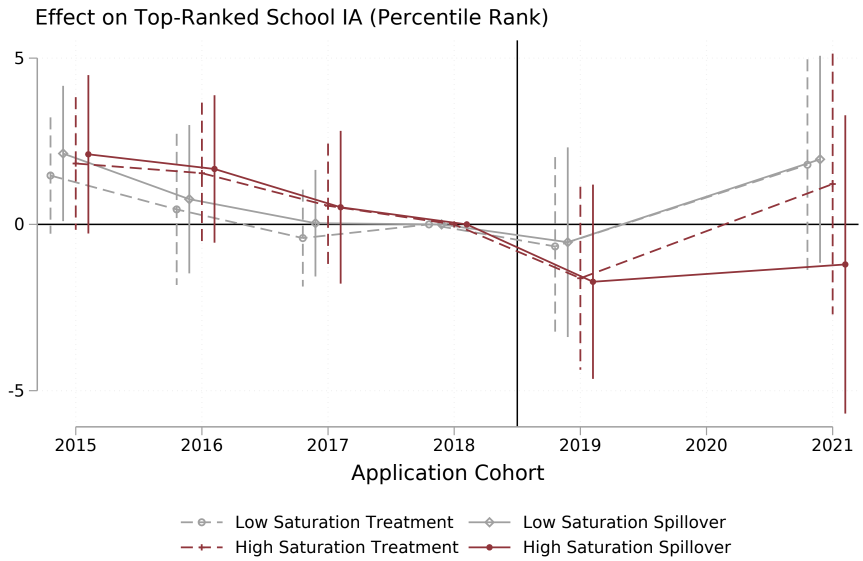
Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of most-preferred achievement growth in student standard deviation units on year and treatment group fixed effects along with treatment group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

C.3 Reduced Form Estimates Implied by Structural Model

Figure C.3: Implied Reduced Form 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 most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. Most-preferred schools are the implied most-preferred school using the structural estimates. In practice, we take random draws of the unobserved preference heterogeneity for each option and add that to the estimated systematic component of utility for each option. We use these indirect utility estimates to construct new rank-ordered lists. All estimates are identified with comparisons between the treatment groups and pure control schools. The omitted year is 2018, the year before the first wave of the intervention. Estimates are robust and clustered at the school level with 95 percent confidence bands reported by bars.

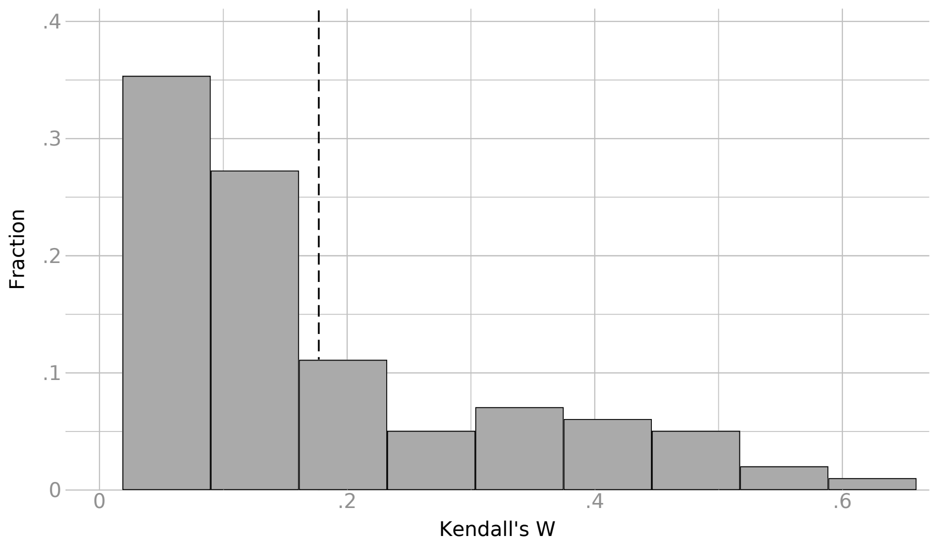
C.4 Evidence on the Lack of Parental Coordination Efforts

This section provides evidence suggesting that coordination among parents in ZOC markets is not widespread. To measure coordination or agreement in rank-ordered school lists, I estimate Kendall's W for each school (Kendall and Smith, 1939). A value of Kendall's W close to one indicates a high degree of similarity in parents' submitted rankings, while values closer to zero indicate little similarity.[22] This concordance measure allows me to assess the extent to which parents from each ZOC feeder school align their schooling decisions, with higher values indicating greater coordination or less variation in preferences.

Appendix Figure C.4 shows the distribution of concordance estimates across all feeder-year schools in the experiment. The average concordance level is low, at 0.18, and approximately 75% of schools have concordance values at or below 0.2. This indicates little coordination in the submitted rankings among parents across feeder schools.

Despite this, it is possible that the intervention increased coordination among parents. To explore this, Appendix Table **??** reports the treatment effects on rank-ordered list concordance. Across both treatments, I find no substantial evidence that the information interventions significantly altered concordance levels. Even when adjusting for school size, the results remain consistent. Overall, the findings suggest that parental coordination efforts played a limited role in the ranking process.

Figure C.4: Rank-ordered list concordance across schools



Notes: This figure reports the distribution of school-level measures of rank-ordered list concordance as measured by Kendall's W. A value of zero is associated with no concordance and a value of one is associated with high concordance.

[22] Kendall's W is similar to the average value of Spearman's rank coefficient across all applicants for a given school (Kendall and Gibbons, 1990).

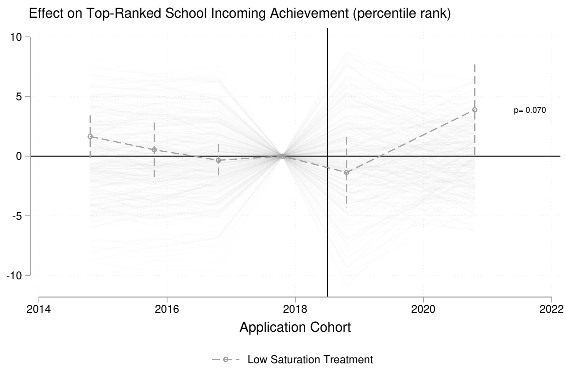
Table C.6: Changes in ranked-ordered list concordance

	(1) Kendall's W	(2) Kendall's W
Treatment High	.01	.01
	(.04)	(.04)
Treatment Low	-.01	.01
	(.04)	(.04)
Control Mean	.18	
Weighted by Size	No	Yes

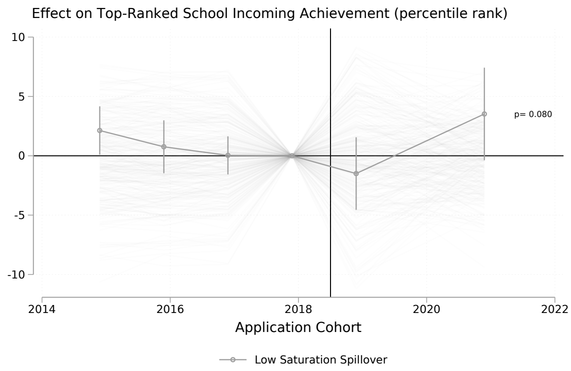
Notes: This table reports results from a regression of school-level estimates of Kendall's W measuring concordance of rank-ordered lists within each cluster (school) unit. A value of zero is associated with no concordance and a value of one is associated with high concordance. Column 1 reports differences between treated and untreated schools, and Column 2 reports similar differences but weighing each observation by the size of the unit. Standard errors are robust.

C.5 Randomization Inference

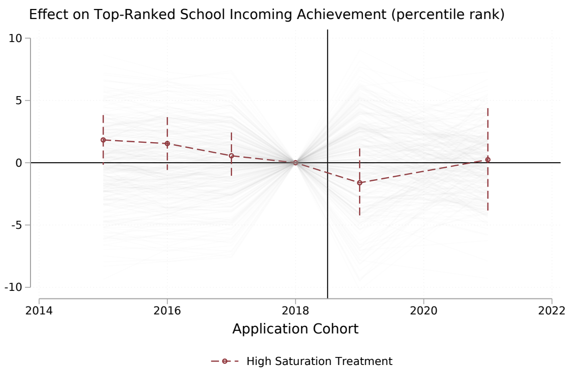
Figure C.5: Impacts on Most-Preferred IA (with Randomization Inference)



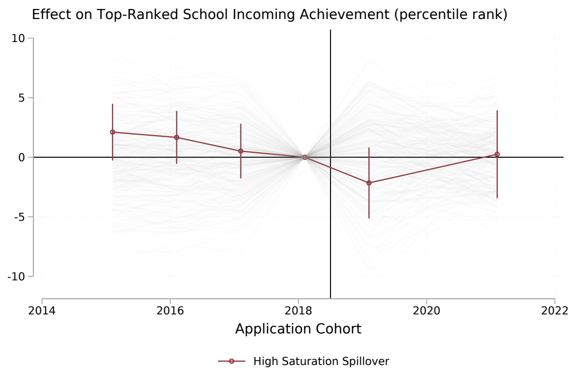
(a) Low Saturation



(b) Low Saturation Spillover



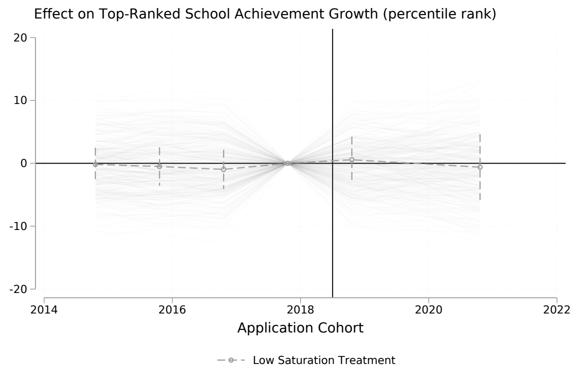
(c) High Saturation



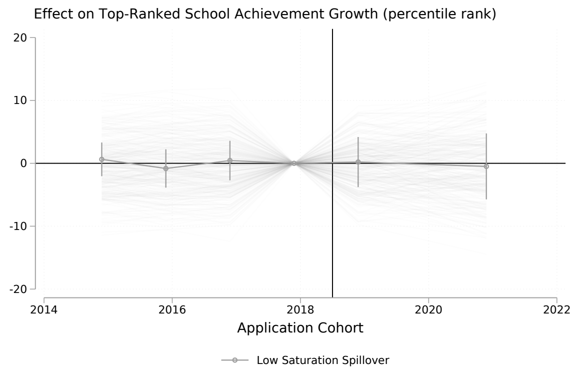
(d) High Saturation Spillover

Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. All estimates are identified by comparisons with pure control schools. The omitted year is 2018, the year before the first wave of the intervention. The shaded lines correspond to estimates under alternative treatment assignments and provide a visual perspective on the distribution of treatment effects under the sharp null of no treatment effect.

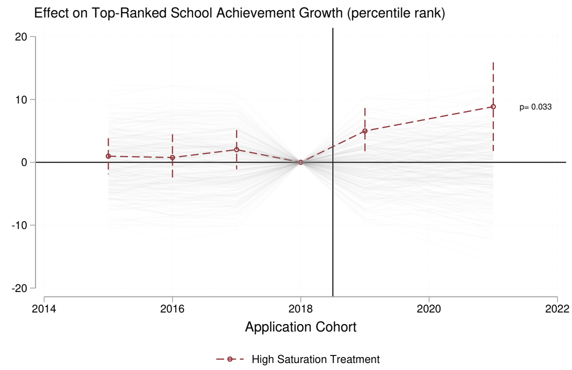
Figure C.6: Impacts on Most-Preferred AG (with Randomization Inference)



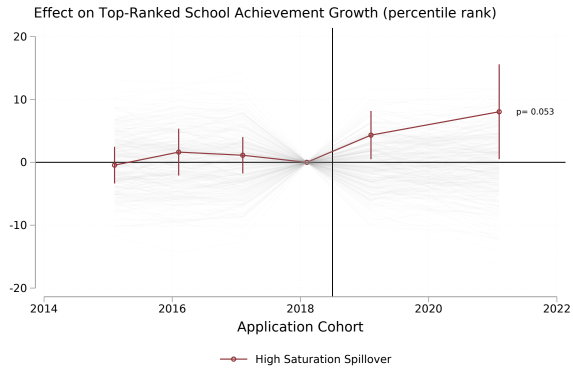
(a) Low Saturation



(b) Low Saturation Spillover



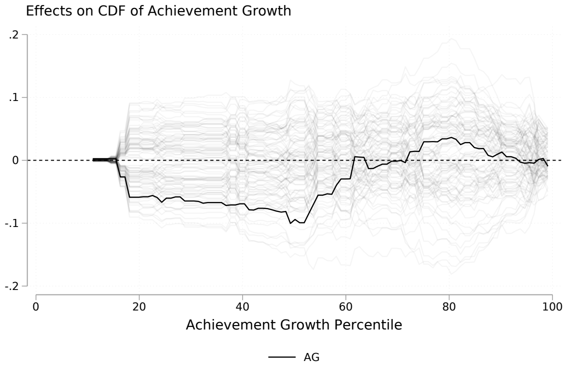
(c) High Saturation



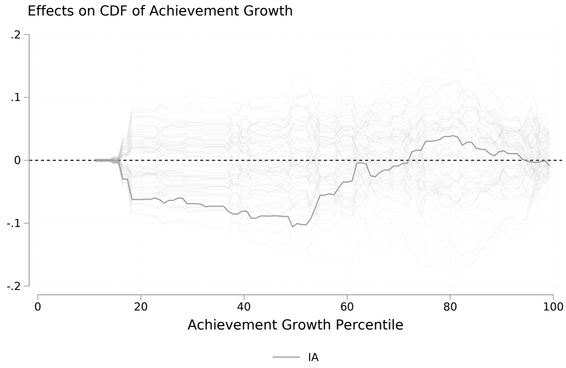
(d) High Saturation Spillover

Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. All estimates are identified by comparisons with pure control schools. The omitted year is 2018, the year before the first wave of the intervention. The shaded lines correspond to estimates under alternative treatment assignments and provide a visual perspective on the distribution of treatment effects under the sharp null of no treatment effect. Randomization inference-based p-values are reported for the 2021 cohort.

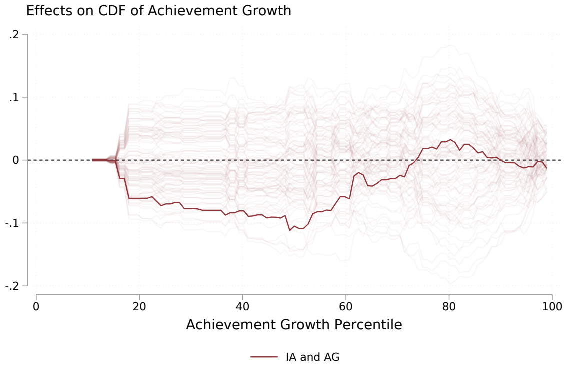
Figure C.7: AG Distributional Estimates (with Randomization Inference)



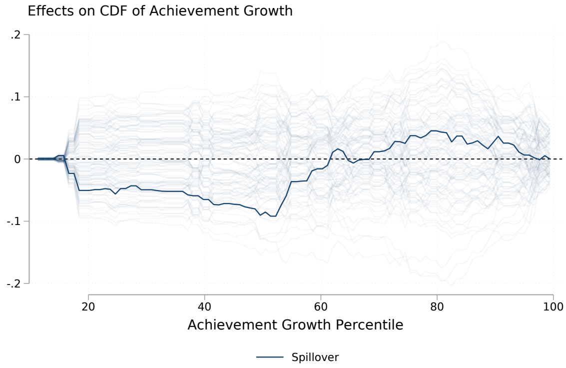
(a) AG Treatment



(b) IA Treatment



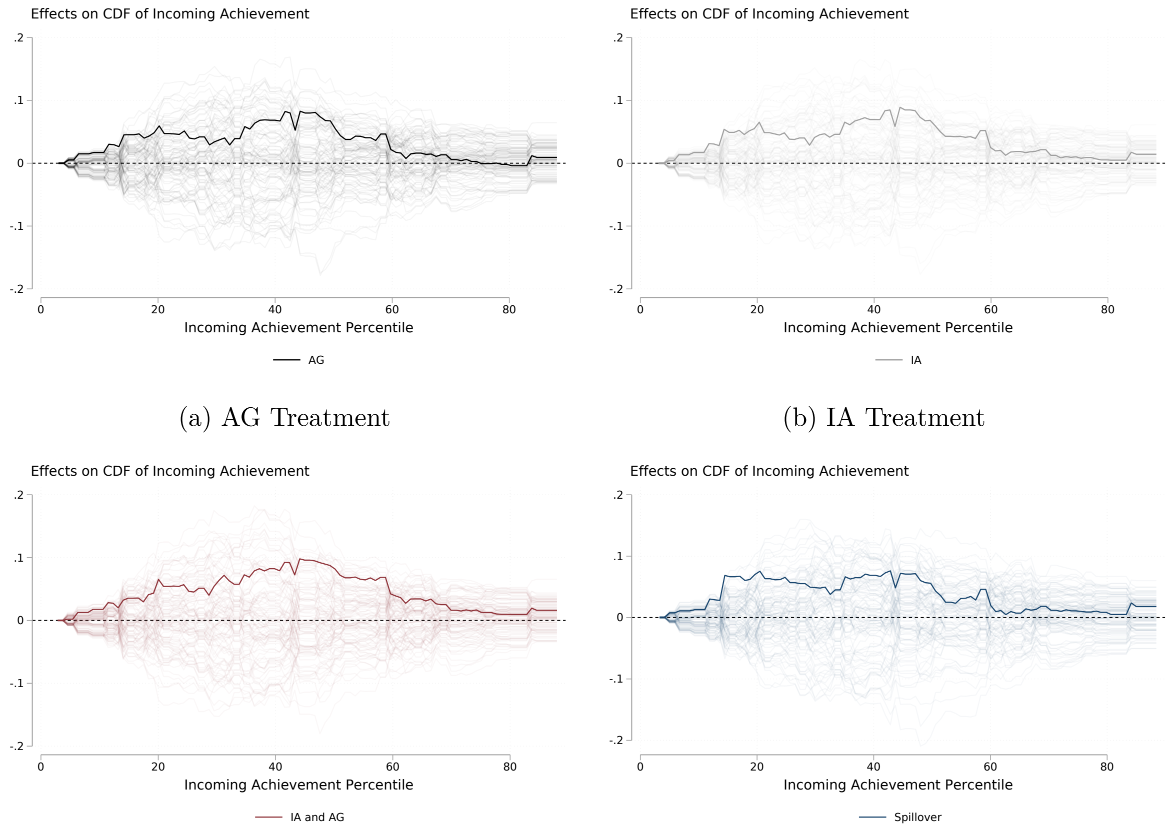
(c) AG and IA Treatment



(d) Spillover

Notes: This figure displays distribution regression estimates across the achievement growth distribution, mirroring estimates in the main body of the paper. The sample stacks experimental waves and includes experiment-year fixed effects along with student baseline controls included in other estimates throughout the paper. Panel (a) reports estimates among those in the AG-only treatment; Panel (b) reports estimates among those in the IA-only treatment; Panel (c) reports estimates among those in the IA and AG treatment; and Panel (d) reports estimates among those in the spillover group. The shaded lines correspond to estimates under alternative treatment assignments and provide visual perspective on the distribution of treatment effects under the sharp null of no treatment effect.

Figure C.8: IA Distributional Estimates (with Randomization Inference)



(a) AG Treatment

(b) IA Treatment

(c) AG and IA Treatment

(d) Spillover

Notes: This figure displays distribution regression estimates across the incoming achievement distribution, mirroring estimates in the main body of the paper. The sample stacks experimental waves and includes experiment-year fixed effects along with student baseline controls included in other estimates throughout the paper. Panel (a) reports estimates among those in the AG-only treatment; Panel (b) reports estimates among those in the IA-only treatment; Panel (c) reports estimates among those in the IA and AG treatment; and Panel (d) reports estimates among those in the spillover group. The shaded lines correspond to estimates under alternative treatment assignments and provide visual perspective on the distribution of treatment effects under the sharp null of no treatment effect.

D Field Survey Details and Evidence

In this section, I report the survey instrument used in the paper and details about a pilot regarding messaging strategies. In Section D.3, I report additional survey evidence alluded to in the main paper.

The additional survey evidence is categorized into four topics. The first corresponds to the attributes of survey respondents (see Table D.2). The second is additional survey evidence not reported in the main paper (see Table D.3 and Figure D.1). The third corresponds to descriptive evidence about belief correlates, including both student-level attributes and researcher-generated measures of quality.

D.1 Survey Questions

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

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

1. What is your relationship to the student?

   - Father
   - Mother
   - Grandparent
   - Guardian

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

   - Yes
   - No

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

3. How many hours do you anticipate spending researching schools?

   - Less than 2 hours
   - 2-5 hours
   - 6-10 hours
   - 11-15 hours
   - More than 15 hours

4. Do you anticipate doing any of the following? (check all that apply)

   - Visit school fair
   - Watch school promotional videos
   - Online research
   - Talk to teachers
   - Talk to other parents
   - Consider your student's input

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

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

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

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

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

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

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

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

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

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

D.2 Pilot Details

Several months before the intervention, I piloted different messaging strategies on Amazon Mechanical Turk (mTurk). Importantly, the pilot did not include the pedagogical videos so it aimed to assess what terms were most effective conveying the level versus growth difference in quality that is the focus of the intervention. I provided respondents with brief descriptions of each quality measure and asked questions to assess two things: (i) whether they were paying attention and (ii) their level of understanding. To gauge attention, I presented hypothetical scenarios where respondents had to infer peer and school quality based on the available information. In these scenarios, either incoming achievement (IA) or achievement growth (AG) was held constant, and respondents had to distinguish between schools based on the other measure. To assess their understanding, I asked them to describe the difference between the two measures. Independent researchers then subjectively evaluated the responses.

To better reflect the demographic characteristics of ZOC families, I imposed a few restrictions on who could participate in the mTurk survey. Respondents had to be parents, under the age of 60, and have at most a high school diploma. However, there were too few Hispanic participants at the time to hold that attribute constant across respondents.

Table D.1 presents the results. Approximately 90% of participants were able to correctly infer IA and AG. Hispanic respondents had a slightly lower correct response rate, but the difference was not statistically significant. For the written responses, about 70% of participants demonstrated an understanding of the difference between IA and AG. Interestingly, Hispanic respondents provided correct written explanations at a slightly higher rate, though this difference was also not statistically significant. Overall, the pilot results suggest that the chosen terms for school and peer quality effectively convey the differences to parents, and the pedagogical videos should further enhance their understanding.

Table D.1: MTurk Piloting Results

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

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

D.3 Additional Survey Evidence

Table D.2: Survey Respondent Characteristics

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

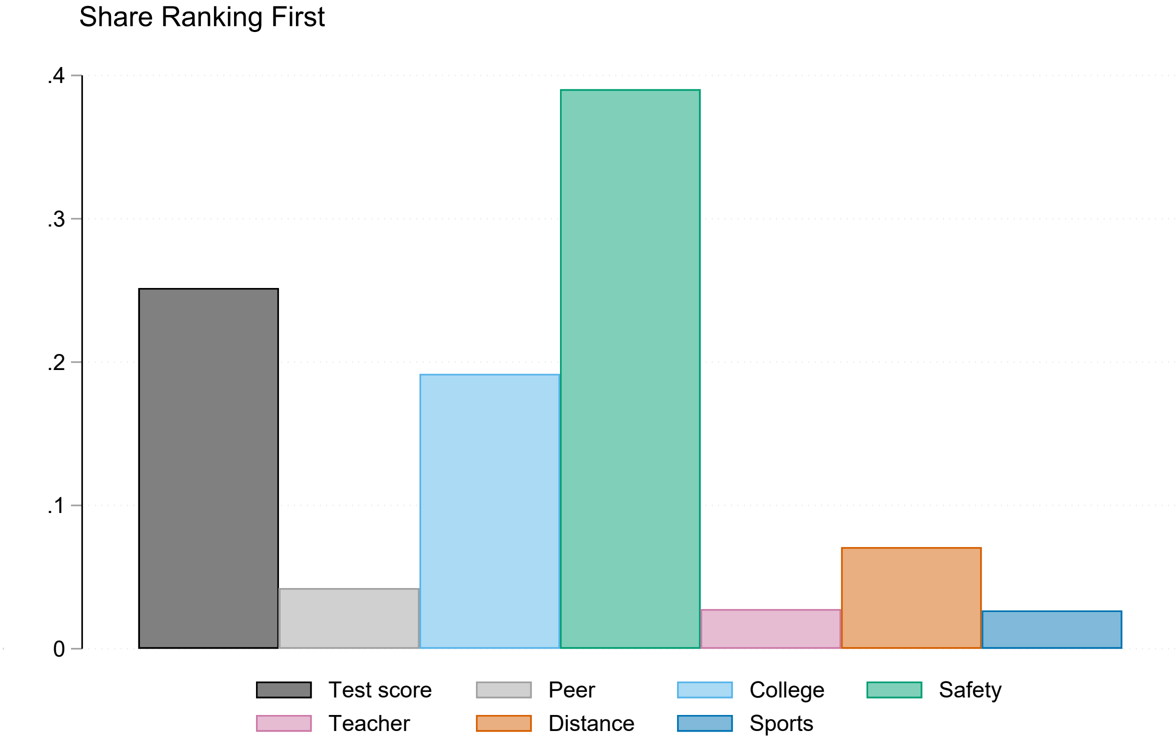
Notes: This table reports estimates from regressions of each row variable on indicators for survey completion status. Partial indicates that the respondent did not finish the survey, usually corresponding to missing beliefs information, and complete corresponds to respondents who completed the survey.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

Table D.3: Survey Responses

Panel A: Anticipated Participation in the School Choice Process				
Respondent Relationship	Father: 0.109	Mother: 0.866	Grandparent: 0.006	Legal Guardians: 0.019
Anticipated Research Hours	Less than 2 hours: 0.373	2-5 hours: 0.352	6-10 hours: 0.119	10+ hours: 0.156
	Yes		No	
Have you heard of ZOC	0.340		0.660	
Do you anticipate doing any of the following:				
Visit a school fair	0.470		0.530	
Watch promotional videos	0.430		0.570	
Talk to teachers	0.520		0.480	
Talk to parents	0.470		0.530	
Online research	0.640		0.360	
Panel B: Perception of school characteristics				
	Not Important	Somewhat Important	Important	Very Important
Peer importance	0.080	0.224	0.326	0.370
Test score importance	0.013	0.079	0.369	0.539
Do you think that...	Yes, definitely		Not necessarily	
Good Peers Imply High Growth?	0.320		0.680	

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

Figure D.1: Stated Preferences over School Attributes



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

Table D.4: IA and AG Pessimism Correlation with 2021 Application Cohort Student Characteristics for Top-Ranked School

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

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

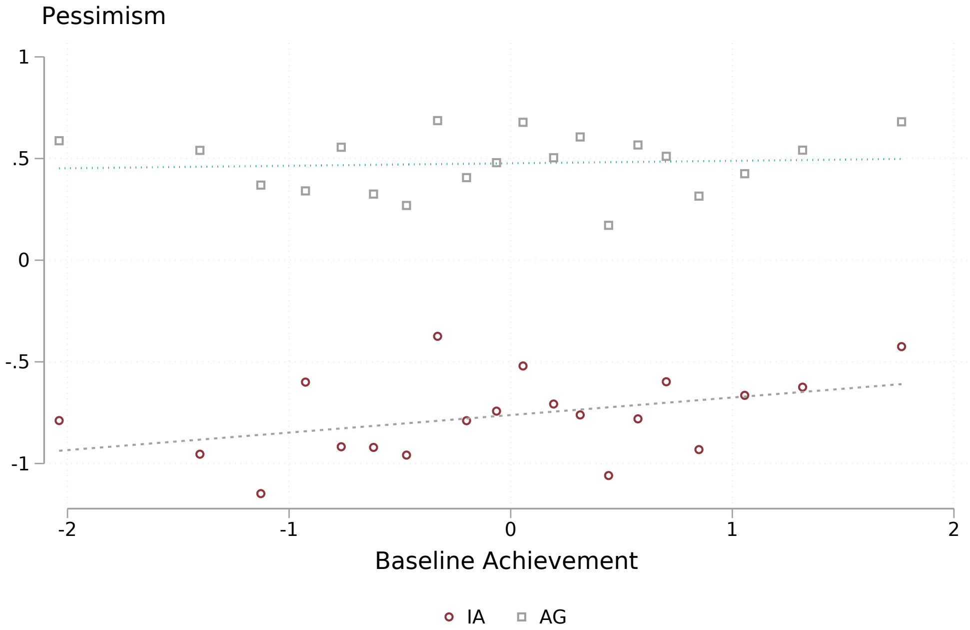
Table D.5: IA and AG Absolute Bias Correlation with 2021 Application Cohort Student Characteristics for Top-Ranked School

Bias Measure	IA		AG	
	Bivariate	Multivariate	Bivariate	Multivariate
Parent College	-0.604 ***	-0.615 ***	-0.165 ***	-0.096 *
	(0.042)	(0.049)	(0.046)	(0.055)
Hispanic	-0.051	-0.441 ***	-0.209 ***	-0.242 ***
	(0.051)	(0.051)	(0.076)	(0.067)
English Learner	0.460 ***	0.233 ***	0.296 ***	0.181 ***
	(0.038)	(0.041)	(0.048)	(0.052)
Special Education	0.054	-0.172 ***	0.173 ***	-0.028
	(0.039)	(0.043)	(0.045)	(0.050)
Black	-0.060	-0.724 ***	0.425 ***	0.197
	(0.092)	(0.099)	(0.127)	(0.131)
White	-0.114	-0.044	0.363 ***	0.144
	(0.107)	(0.128)	(0.152)	(0.181)
Female	-0.053 **	-0.020	0.039	0.026
	(0.026)	(0.029)	(0.028)	(0.031)
Poverty	0.371 ***	0.197 ***	-0.072	-0.190 ***
	(0.043)	(0.051)	(0.050)	(0.059)
Math Z-Score	-0.187 ***	-0.068 ***	-0.213 ***	-0.296 ***
	(0.015)	(0.016)	(0.024)	(0.027)
ELA Z-Score	-0.203 ***	-0.123 ***	-0.123 ***	0.127 ***
	(0.015)	(0.016)	(0.025)	(0.028)
Migrant	-0.096	-0.237	-0.036	-0.179
	(0.156)	(0.225)	(0.149)	(0.241)
Mean	2.88		2.62	
SD	1.94		2.17	

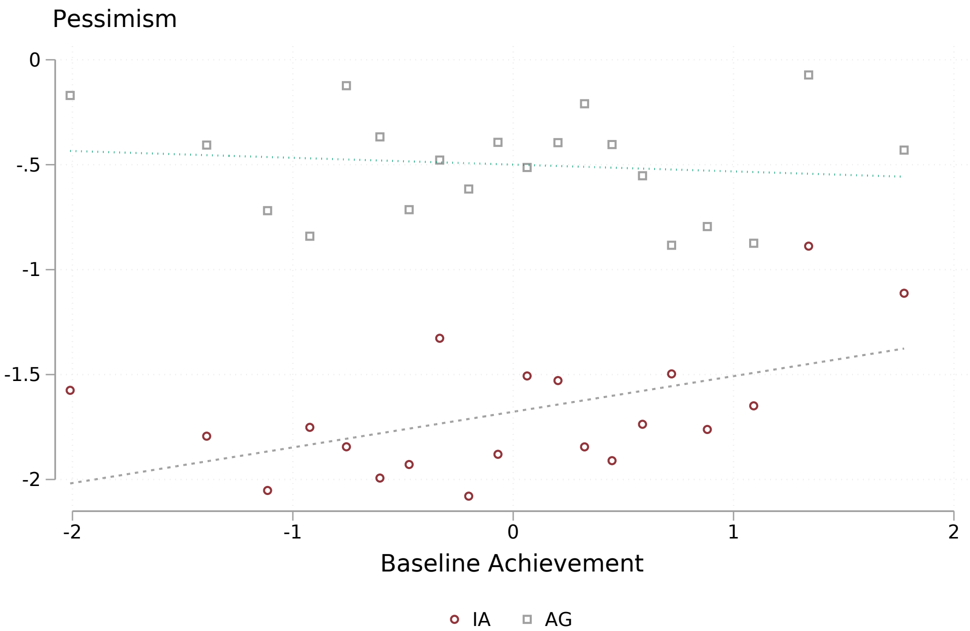
Notes: This table reports univariate and multivariate correlations between student-level IA and AG absolute bias measures (in deciles) and student-level covariates. Column 1 and Column 2 consider IA bias and Column 3 and Column 4 consider AG bias. Odd-numbered columns consider bivariate regressions of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

Figure D.2: Pessimism-Achievement Relationship

(a) All Options on Rank-Ordered List



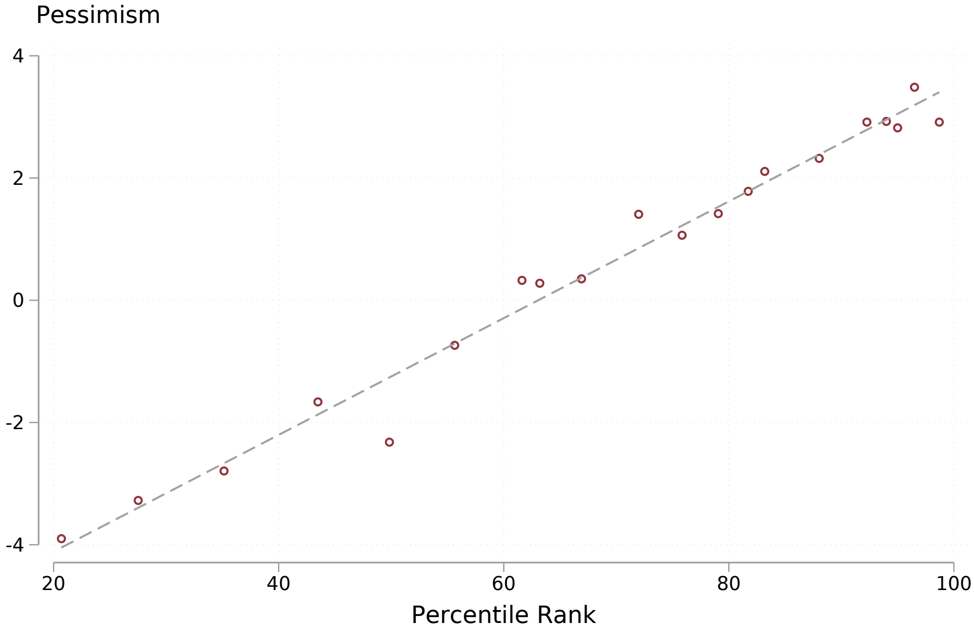
(b) Only Top-Ranked Option on Rank-Ordered List



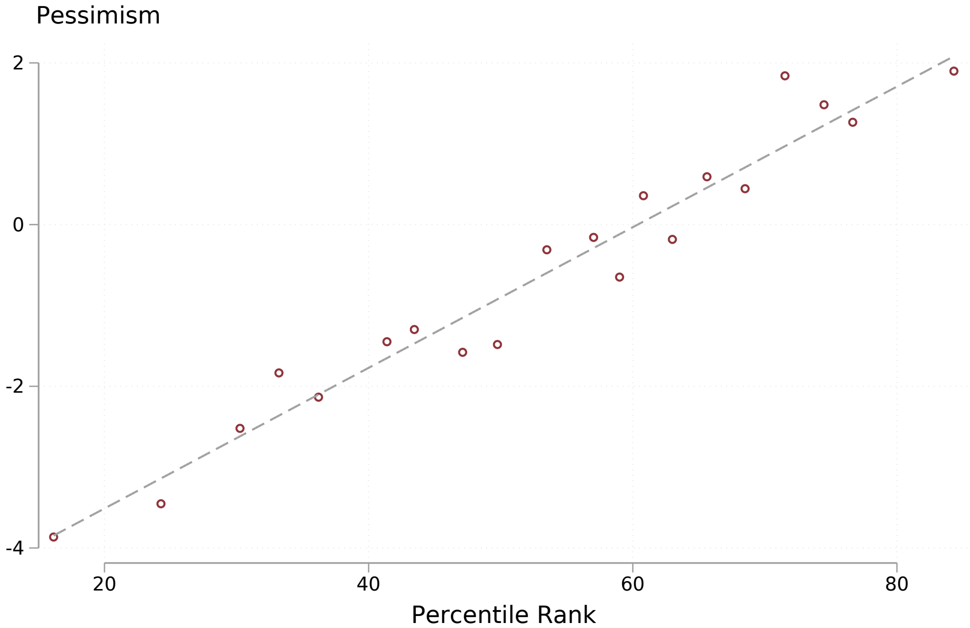
Notes: This figure reports bivariate-binned scatter plots of the pessimism-achievement relationship. Panel (a) reports the relationship across all options contained on the rank-ordered list, while Panel (b) reports the relationship only among the top-ranked option of applicants' rank-ordered lists.

Figure D.3: AG/IA Bias-Truth Relationship

(a) Achievement Growth



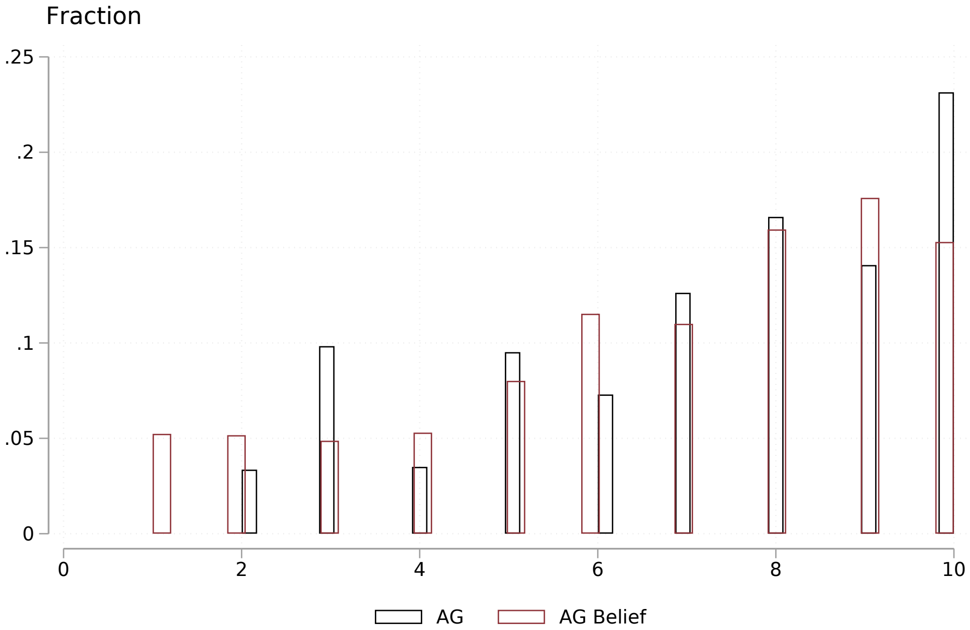
(b) Incoming Achievement



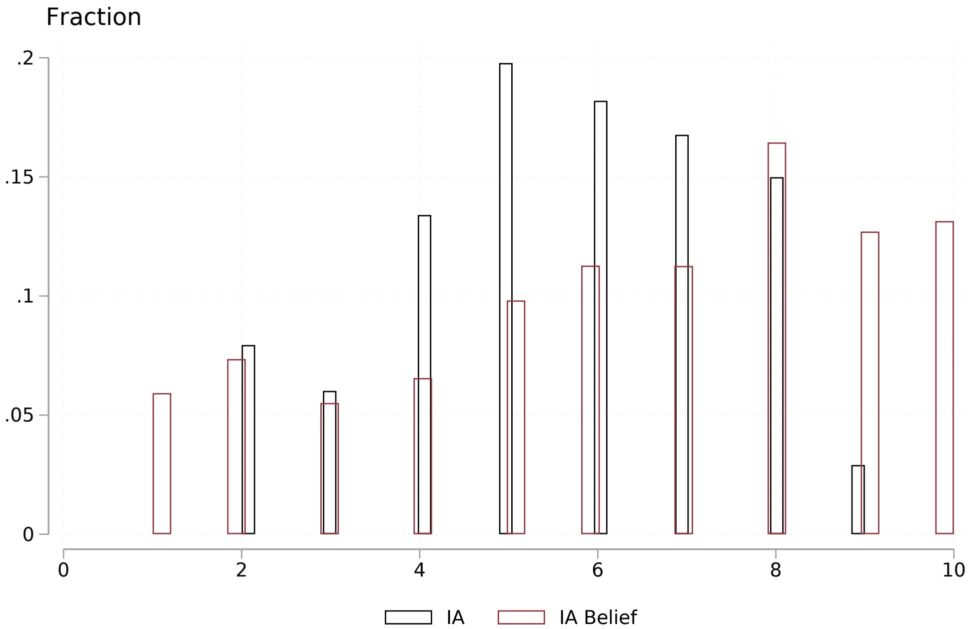
Notes: This figure reports bivariate-binned scatter plots summarizing relationship between AG and IA pessimism (measured in deciles) and the true AG and IA percentile rank. Panel (a) reports this relationship for academic growth, while Panel (b) reports the relationship for incoming achievement.

Figure D.4: AG/IA Decile and AG/IA Belief Distribution

(a) Achievement Growth



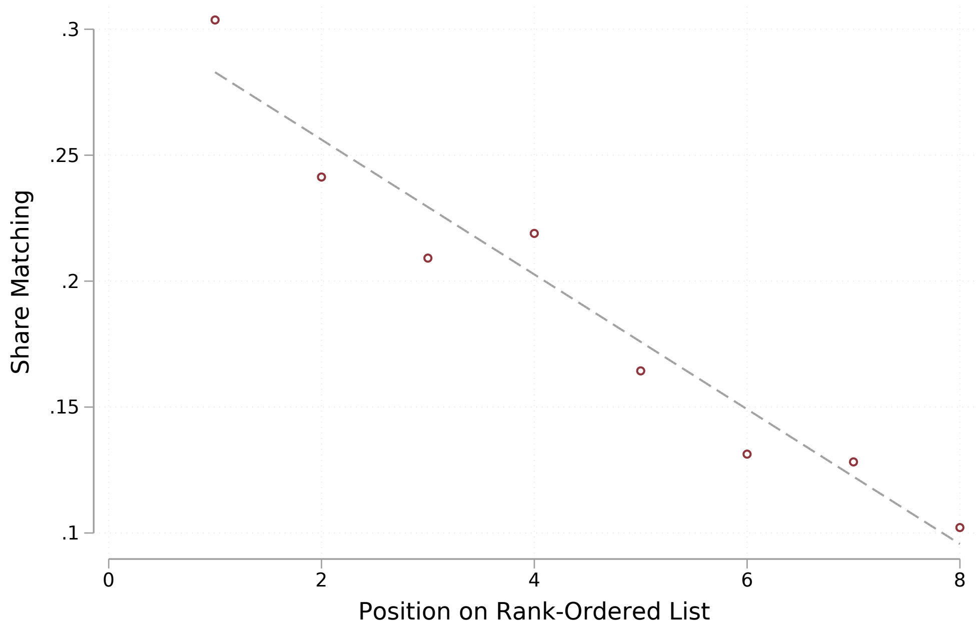
(b) Incoming Achievement



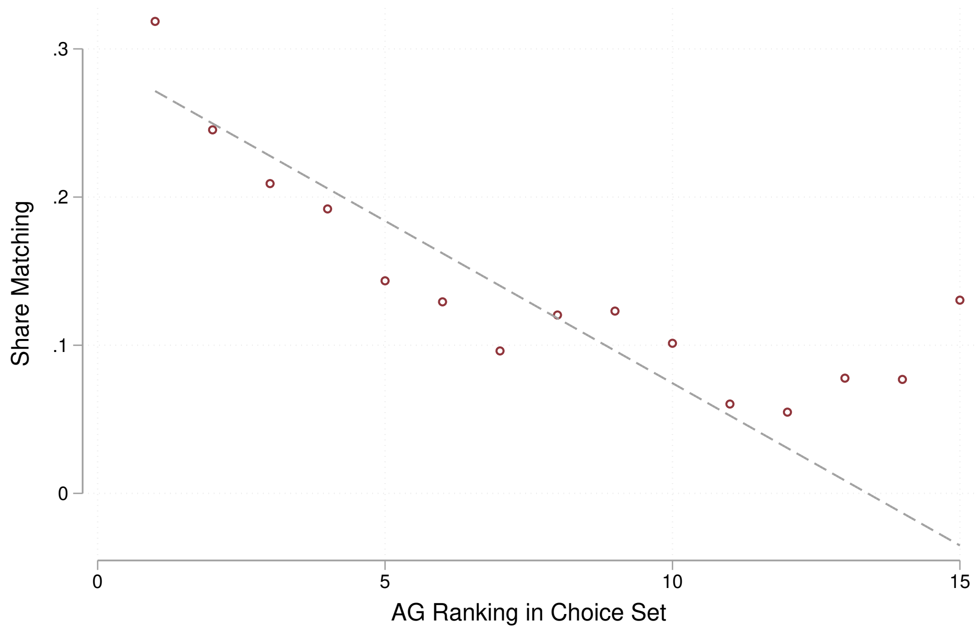
Notes: This figure reports option-specific distributions of AG (IA) deciles and AG (IA) beliefs. If applicants' decile beliefs were perfectly on target, then their belief distribution would perfectly overlap with the decile distribution.

Figure D.5: Choice Relevance of AG Biases

(a) By Position on the Rank-ordered List



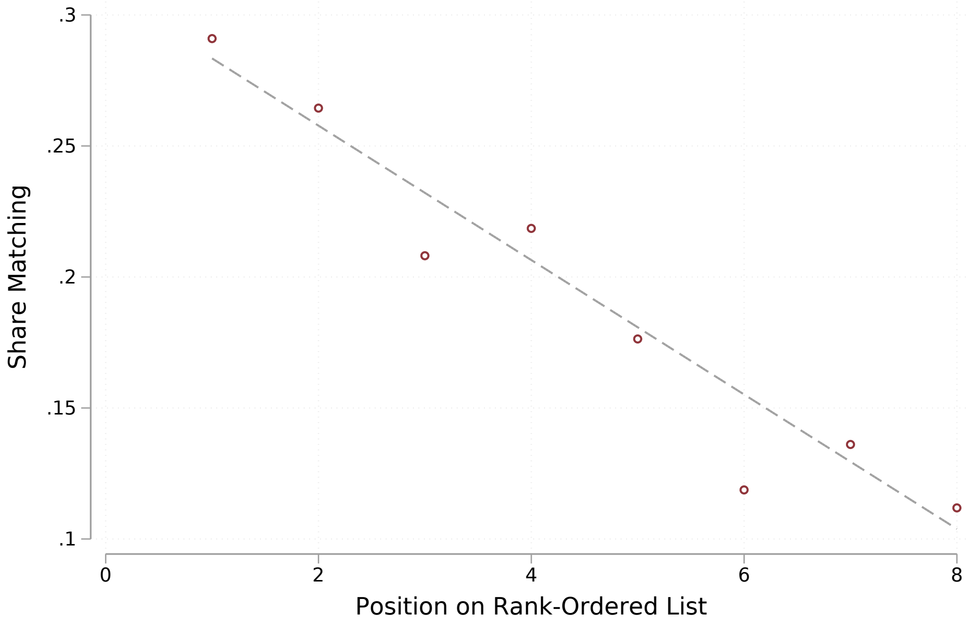
(b) By Option's Actual Ranking



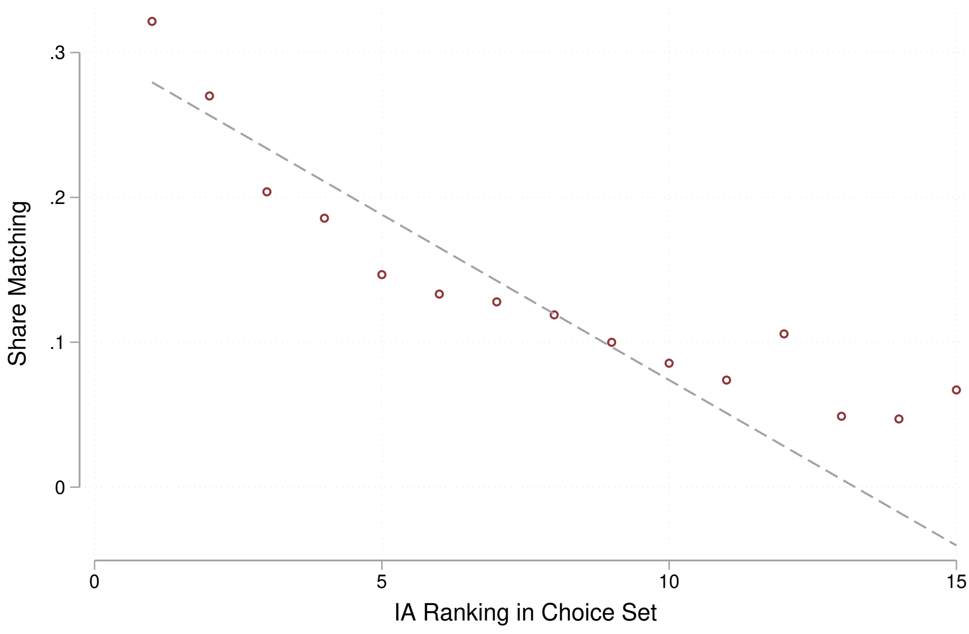
Notes: This figure reports the share of applicants whose ordinal ranking of schools using beliefs matches the ordinal ranking using objective AG. Panel (a) reports that by position on the applicant's rank-ordered list and Panel (b) reports that by the actual ranking for that option.

Figure D.6: Choice Relevance of IA Biases

(a) By Position on the Rank-ordered List



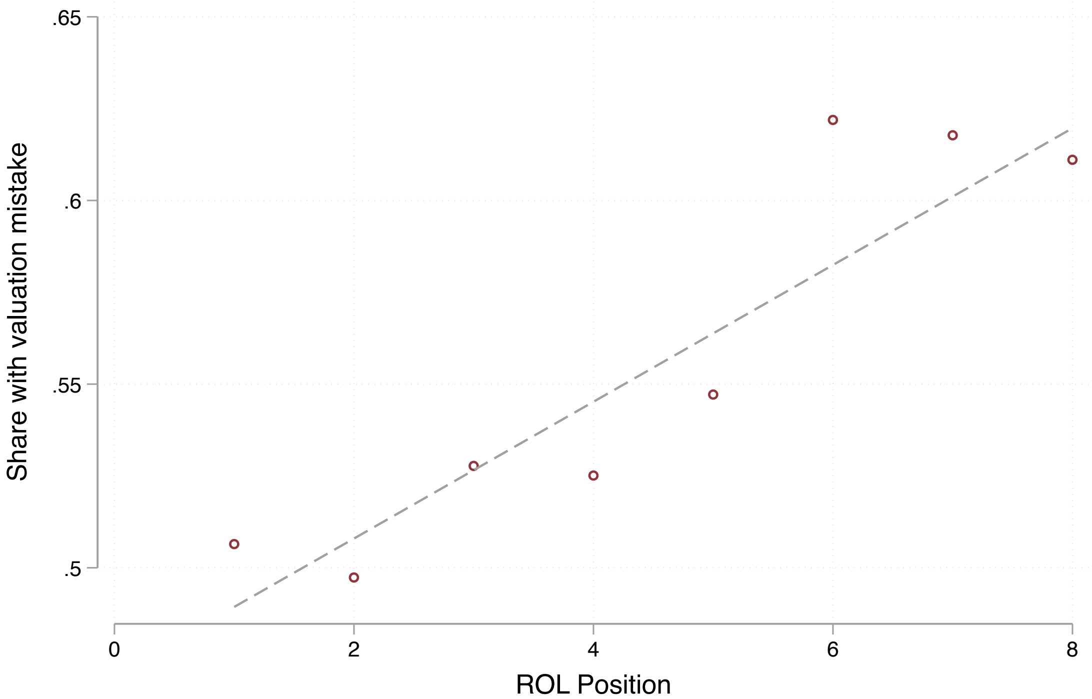
(b) By Option's Actual Ranking



Notes: This figure reports the share of applicants whose ordinal ranking of schools using beliefs matches the ordinal ranking using objective IA. Panel (a) reports that by position on the applicant's rank-ordered list and Panel (b) reports that by the actual ranking for that option.

D.4 Application Mistakes

Figure D.7: Valuation-Induced Application Mistakes



Notes: This figure reports the share of applicant-level valuation-induced application mistakes across the rank-ordered list. To define a valuation mistake, I first estimate preferences for schools using elicited beliefs about IA and AG and distance to schooling options. With those preference estimates, I then predict the systematic component of utility using beliefs and researcher-generated quality separately. I then take random EVT1 draws to capture unobserved preference heterogeneity, and combined with estimated systematic components of utility, I generate rank-ordered lists using beliefs and researcher-generated quality. If there is disagreement at a given position of the ROL, I define that as a valuation-induced application mistake. This figure reports the share of these across the rank-ordered list at baseline.

E Online Survey Details and Evidence

I complement the field experiment with an online survey administered on the Prolific platform. I survey parents with school-aged children to mirror the field experiment and provide additional details about social interactions in the school choice process, while also eliciting beliefs about peer and school quality after using pedagogical videos to teach parents about the concepts in a nationally representative sample. The survey is packed with information, but only a few are emphasized in the main body of the paper that I report in this section.

The goals of the online survey directly relate to the core questions of the field experiment. The survey, therefore, mirrors the field experiment in that respondents are provided with similar pedagogical videos to teach them about school and peer quality, then asked about their beliefs about each. Preferences are then experimentally identified, and a series of descriptive questions establish that social interactions are important to parents and then aim to understand why.

E.1 Measuring Beliefs and Biases

The Prolific sample contains parents from all parts of the United States and we do not have any information about the schools their children are enrolled in before they take the survey. To benchmark beliefs against an objective measure, we use information on GreatSchools.org. To measure beliefs, after showing parents pedagogical videos explaining peer and school quality, we ask them about their beliefs about their schools' decile rank across all other schools in their particular state; those are the measures of beliefs about school and peer quality. After that, we ask them to look up their school on GreatSchools.org and to enter the URL of the link, and then to report their school's Great Schools Summary, Test Score, Progress, and Equity rating. The Summary Rating is a weighted average of the subcomponents. Because we elicit their beliefs in terms of deciles and the Great Schools ratings are analogous to decile ranks, we use the Great Schools ratings as an objective benchmark. We also inspect responses to ensure the URL parents provide corresponds to actual schools in respondents' reported county and state.

E.2 Sample Summary Statistics and Beliefs

Appendix Table E.1 presents the demographic and regional characteristics of survey participants, along with information about their children's schools and their beliefs about school ratings. Compared to the most recent decennial census, the survey sample has a slightly lower proportion of Hispanic respondents and a higher proportion of Black respondents. Additionally, there is a slight underrepresentation of individuals with annual incomes below $100,000. In terms of regional representation, the sample closely mirrors Census statistics. Panel C shows the GreatSchools ratings of the schools attended by respondents' children. On average, respondents enroll their children in schools with a Summary rating in the sixth decile of the GreatSchools distribution. The GreatSchools Test Score rating, which corresponds to peer quality or incoming achievement, and the Progress rating, which reflects school quality or achievement growth, are also reported. Parents tend to be optimistic about both peer and school quality, though their optimism is more modest for school quality.

Mirroring the field survey evidence, Appendix Figure E.1 shows that respondents on Prolific tend to overestimate peer and school quality if their schools' are below the median and underestimate if their schools are over the median. This pull-to-the-center effect, also appearing in the field survey evidence (see Appendix Figure D.3), is common in many studies. Appendix Figure E.2 reports the mean pessimism measures across all the states represented in the sample, showing there is substantial spatial heterogeneity.

E.3 Preferences

The respondents watched videos similar to the ones in the field experiment. After the videos and questions that allow us to gauge respondents' overall understanding of the content, we asked them about their preferences for school and peer quality. Appendix Figure E.3 reports the share of parents who report preferring school quality over peer quality, demonstrating that roughly 80 percent of parents report having a stronger preference for school quality. We also experimentally elicited their preferences for peer and school quality using a sequence of hypothetical choice trials. Appendix Figure E.4 reports experimental preference estimates for various subgroups, quantifying preferences in willingness to travel units. The typical parent in the sample is willing to travel an additional 5.5 minutes to enroll their child in a school with a one-unit higher GS Progress rating, a measure analogous to school quality. In contrast, the willingness to travel for peer quality is 28 percent lower. The findings that preferences tend to exhibit a stronger preference for school quality over peer quality after being informed about each mirrors the key findings in the main paper. In terms of heterogeneity, there is some heterogeneity with the most pronounced corresponding to URM families exhibiting larger willingness to travel for both peer and school quality. Across all groups we find that families have a stronger taste for school quality that is statistically and economically significant.

E.4 Social Interactions

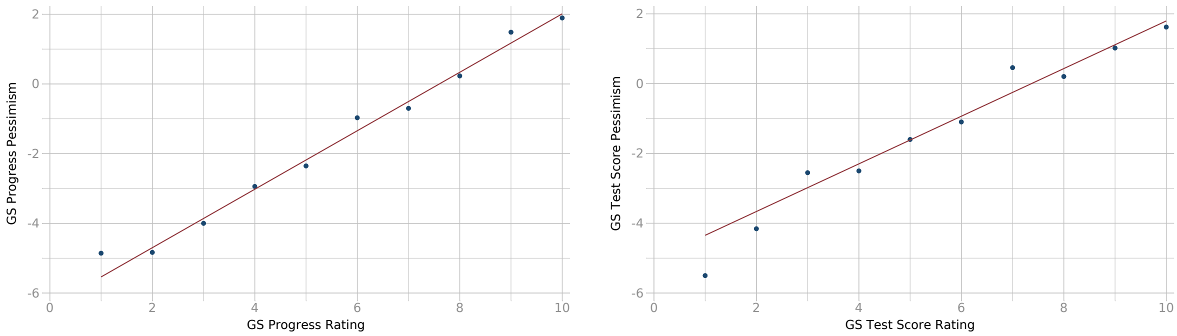
Parental interactions are common throughout the school choice process. To begin, Appendix Figure E.5 demonstrates that parents rely on other parents for information during the school choice process, with 73 percent of parents reporting that they talk to other parents for information about schools, coming in second to online research. Appendix Figure E.6 demonstrates that information shared by the district influences schooling decisions as much as information shared by other parents, and that most parents believe district-provided information is more likely to influence schooling decisions if discussed with other parents. To unpack why parents believe talking to other parents is important, Appendix Figure E.7 reports the reasons why parents rely on engagement with other parents. The overwhelming majority of parents rely on parental discussions because they think discussions with other parents make the information more credible and help them understand complex information. A minority of parents report talking to other parents to coordinate schooling decisions. In summary, the evidence reported in Appendix Figure E.5, Appendix Figure E.6, and Appendix Figure E.7 demonstrate that social interactions are important in the school choice process, and when it comes to how district-provided information affects choices, social interactions give information more credibility and help parents better understand and distill information.

Table E.1: Prolific Sample Descriptive Statistics

	Mean	Standard Deviation
	Panel A: Respondent Demographic Variables	
College Educated	0.58	0.49
White	0.66	0.47
Black	0.24	0.43
Hispanic	0.08	0.27
Asian	0.06	0.23
Lower Income	0.57	0.50
Higher Income	0.43	0.50
	Panel B: Respondent Census Regions	
Northeast Region	0.18	0.38
Midwest Region	0.20	0.40
South Region	0.42	0.49
West Region	0.19	0.39
	Panel C: Respondent Great School Ratings	
GS Summary Rating	5.99	2.23
GS Test Score Rating	6.42	2.50
GS Progress Rating	6.11	2.46
GS Equity Rating	5.27	2.50
	Panel D: Respondent Great School Rating Biases	
GS Progress Pessimism	-0.67	2.53
GS Test Score Pessimism	-1.26	2.69
N	1,000	

Notes: This table reports summary statistics for the sample of Prolific respondents. A sample of parents with school-aged children was surveyed on Prolific with an aim to mirror the typical parent in the United States. Panel A reports demographic characteristics. Lower income is defined as someone self-reporting annual earning of less than $100,000, and Higher Income is the complement. Racial/ethnic categories are not mutually exclusive. Panel B reports the representation of different Census regions using respondents' self-reported state and county information. Panel C reports self-reported Great Schools ratings of schools respondents' children attend. To elicit Great School ratings, we asked respondents to search for their school on GreatSchools.org and report the URL. After that, we asked them to report the GS Summary Rating, Test Score Rating, Progress Rating, and Equity Rating. Panel D reports Great School rating pessimism measures. Before asking respondents to search for their school on GreatSchools.org, we asked them to rank the decile they believed their school belonged to with respect to the distribution of schools in their state. We asked them this question for both peer and school quality. Beliefs were elicited after they viewed pedagogical videos explaining the differences between peer and school quality.

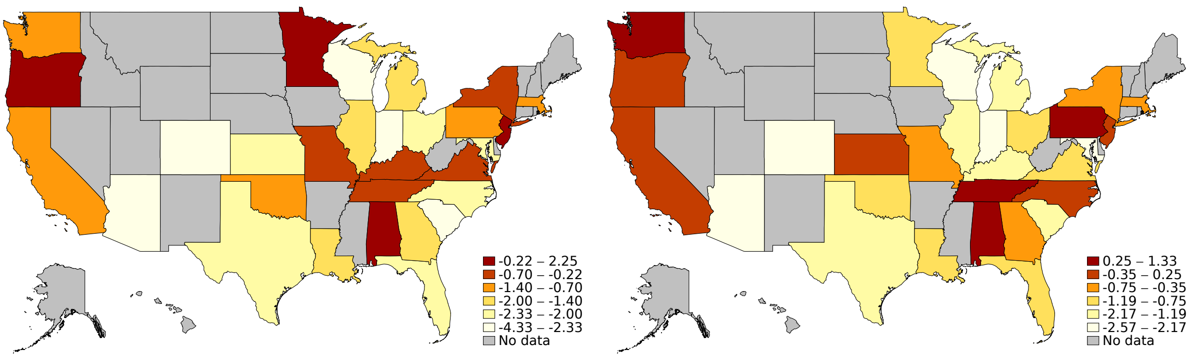
Figure E.1: GS Summary Ratings Biases



(a) School Quality (b) Peer Quality

Notes: This figure reports a binscatter relationship between respondents' elicited pessimism for both GS-based school and peer quality against objective GS-based school and peer quality.

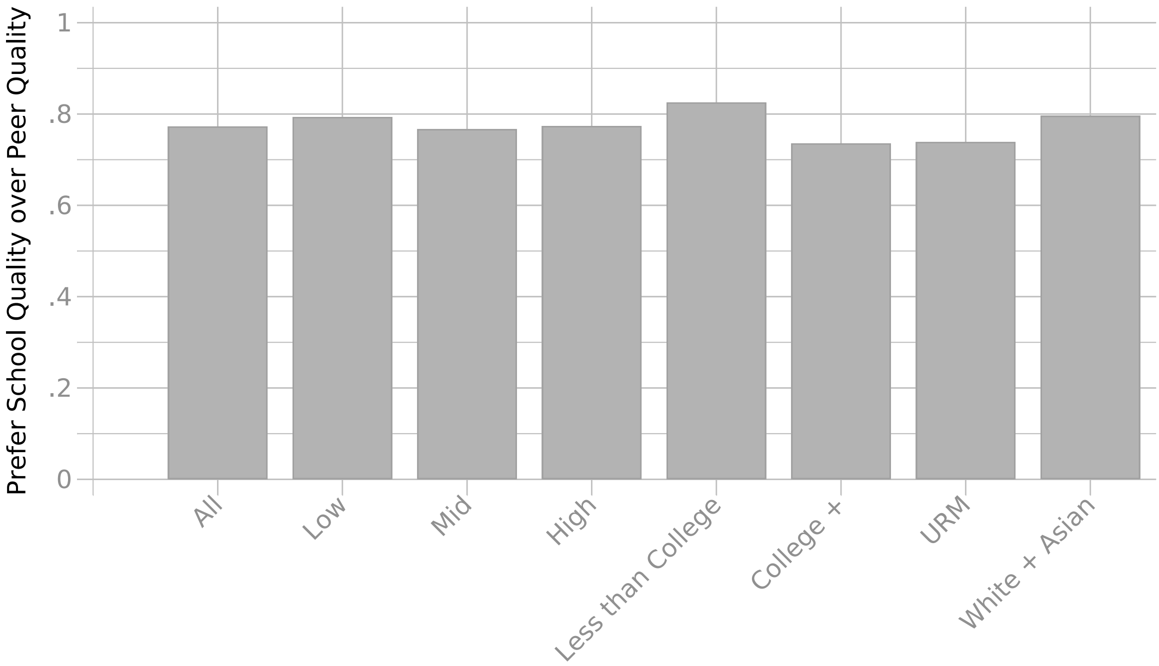
Figure E.2: Spatial Distribution of GS Summary Ratings Biases



(a) School Quality (b) Peer Quality

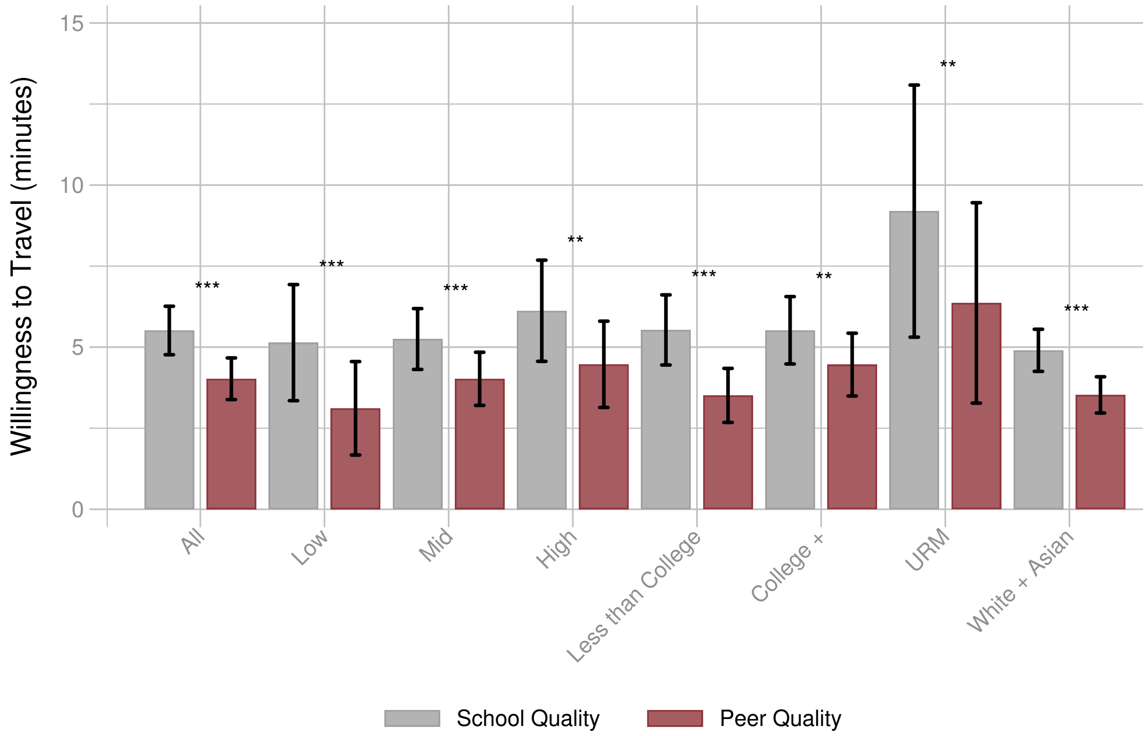
Notes: This figure reports a mean pessimism score of GS-based peer and school quality measures for each state represented in the sample. Statistics for states with fewer than ten respondents are not included in the figure.

Figure E.3: Share of Parents Preferring School Quality



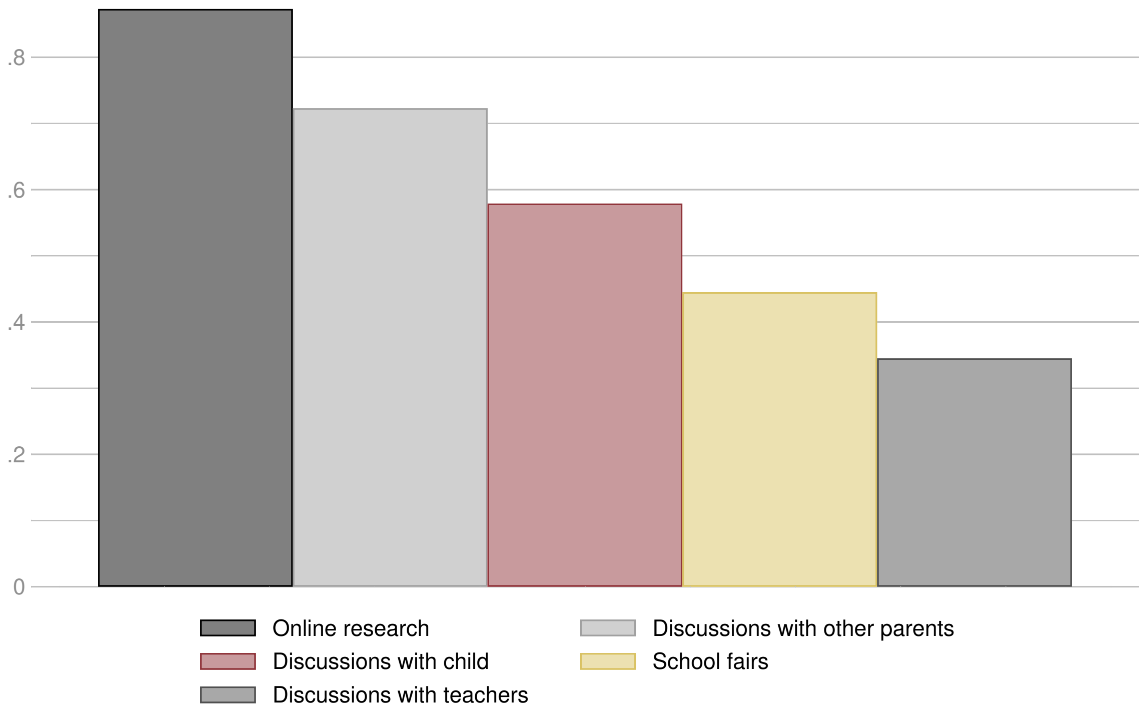
Notes: This figure reports the share of parents stating they prefer school quality over peer quality. The question is asked after the parents watch pedagogical videos explaining the difference between the two quality measures. Parents are asked to list an ordinal ranking over the two measures and the bars report the share of parents listing school quality as their most-preferred. The first bar reports the mean for the entire sample, the next three bars list the means for different groups with different GS Summary ratings. Less than College correspond to parents who report not having a four-year college degree, College + corresponds to parents stating they have at least a four-year college degree, URM corresponds to parents reporting they are Black or Hispanic, and the final bar corresponds to White and Asian parents.

Figure E.4: Experimental Preferences for School and Peer Quality



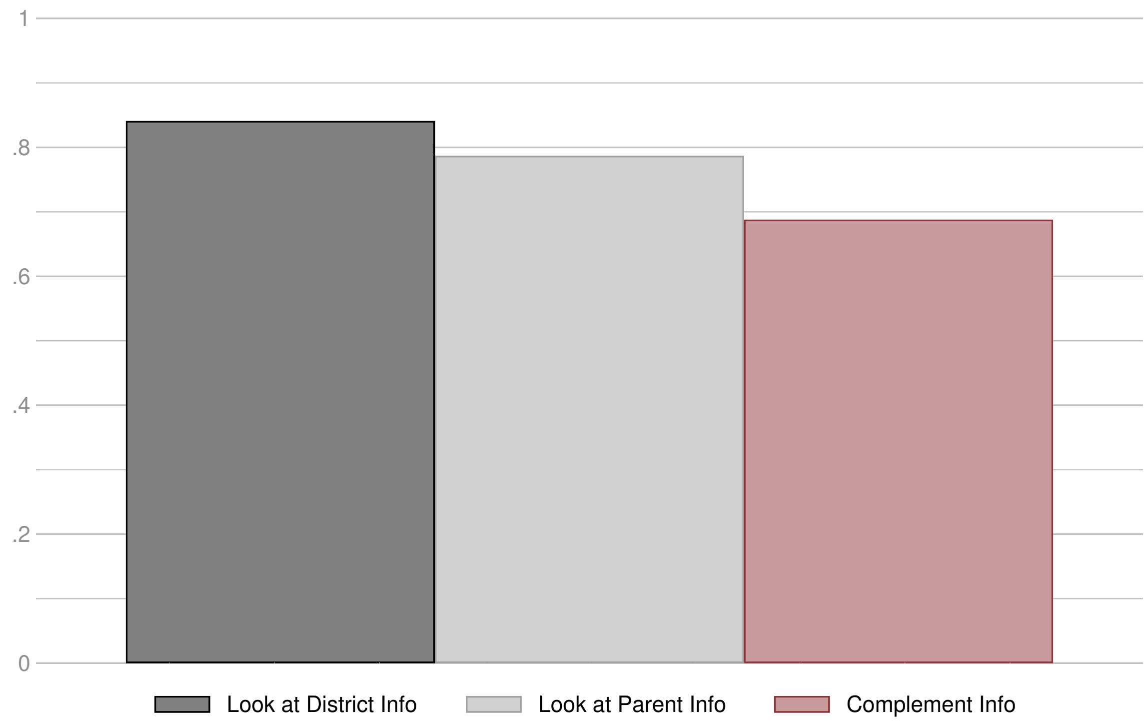
Notes: This figure reports experimental willingness to travel estimates for school and peer quality. Respondents are presented with hypothetical schools that vary in terms of travel time, school quality, and peer quality. Respondents report a ranking of the hypothetical schools. We assume logit preference shocks for each hypothetical scenario, and each respondent is presented with ten hypothetical scenarios. We aggregate across respondent-choice trials to estimate utility weights via maximum likelihood. Estimates reported in the figure correspond to the ratio of estimated utility weights on each attribute scaled by the estimate distance coefficient, so they correspond to marginal willingness to travel estimates. Standard errors are robust and clustered at the respondent level. Gray bars correspond to school quality willingness to travel estimates, while maroon bars correspond to peer quality willingness to travel estimates. The first pair of bars—labeled "All"—report the WTT for the entire sample, the next three pairs of bars list the WTT for different groups with different GS Summary ratings, the less than College pair corresponds to parents who report not having a four-year college degree, the College + pair corresponds to parents stating they have at least a four-year college degree, the URM pair corresponds to parents reporting they are Black or Hispanic, and the final pair corresponds to White and Asian parents. 95 percent confidence intervals are reported. The stars above each pair of bars indicate statistical significance corresponding to rejections of tests of the null hypothesis that willingness to travel for peer and school quality are equal. One star corresponds to significance at the 10 percent level, two stars correspond to significance at the 5 percent level, and three stars correspond to significance at the one percent level.

Figure E.5: Sources of Information



Notes: This figure reports information on the share of activities parents report doing when researching schools. Parents may report doing various activities so they are not mutually exclusive. Online Research corresponds to any kind of research online, discussion with parents corresponds to parents reporting talking to other parents as a source of information, Discussions with child corresponds to parents asking for the opinion of their child, School fairs corresponds to parents reporting attending school fairs, and Discussion with teachers corresponds to parents talking to teachers about schooling options.

Figure E.6: Information that influences school choices

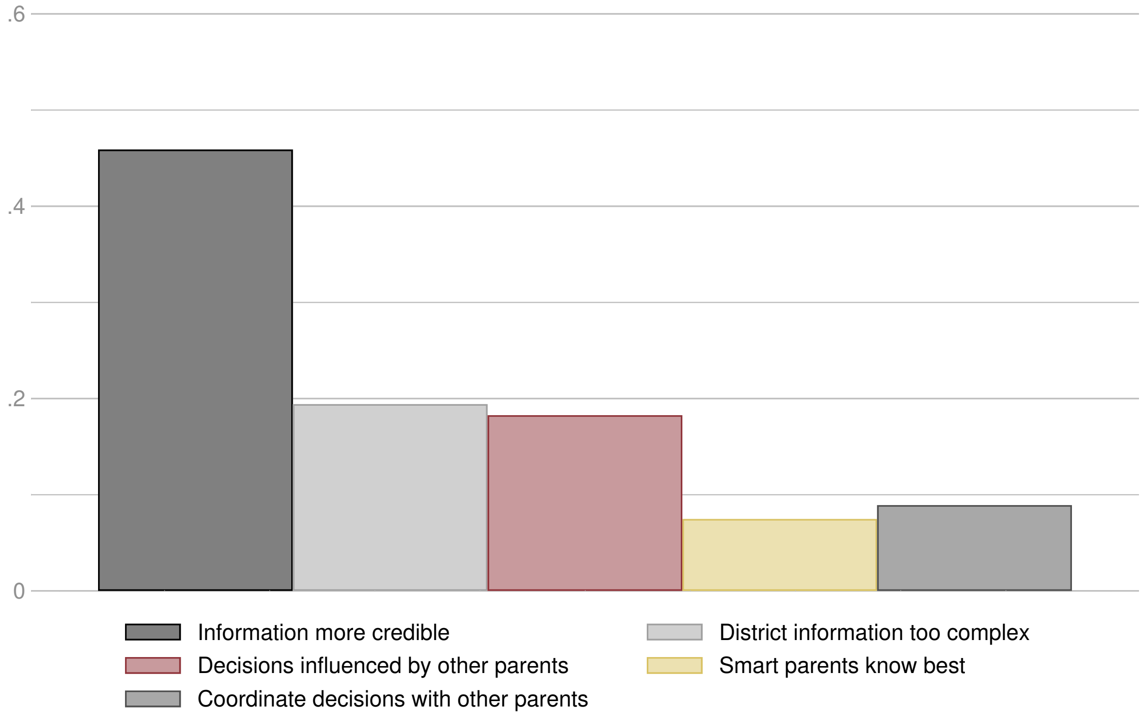


Notes: This figure reports the share of parents stating that they at least agree (or at least somewhat likely) with the following statements.

- Look at District Info: Suppose your school district sends you information about several schools' Incoming Achievement and Achievement Growth ratings. How likely is the information to influence your school choice?
- Look at Parent Info: Suppose a parent sends you information about several schools' Incoming Achievement and Achievement Growth ratings. How likely is the information to influence your school choice?
- Complement Info: It is more likely that district-provided Incoming Achievement and Achievement Growth information influences my school choices if other parents also engage with it and we discuss it together.

The questions were asked after parents watched pedagogical videos explaining the differences between Incoming Achievement and Achievement Growth.

Figure E.7: Reasons for social interactions



Notes: This figure reports the share of parents ranking the various categories as at least second most important. Parents were asked to rank the categories from most to least important. The categories are reasons for why other parents' discussions about district-provided information influence their school choices. The listed categories in the figure correspond to the following reasons:

- Information is more credible: The information is more credible after the discussion.
- District information too complex: The information is hard to understand.
- Decisions influenced by other parents: My decisions are influenced by the opinions of other parents.
- Smart parents know best: Knowledgable parents help me understand the information.
- Coordinate decisions with other parents: I coordinate with other parents about my schooling decisions.

F Decomposition Exercise Details

Canonical school choice models assume families have accurate information at the time they make decisions, yet a growing body of evidence suggests this assumption is far from true (Ainsworth et al., 2023, Andrabi et al., 2017, Arteaga et al., 2022, Hastings and Weinstein, 2008). Imperfect information will distort choices and introduce allocative inefficiencies and affect outcomes (Abaluck and Compiani, 2020, Ainsworth et al., 2023). In this section, I outline a school choice model that models the effects of information treatments in a setting with and without information frictions. The comparison of the settings allows for a natural decomposition of treatment effects that inform about the role of salience and information updating in contributing to the effects induced by information campaigns.

Families are indexed by $i \in \mathcal{I}$ and schooling options by $j \in \mathcal{J}$. 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.[23] The effects of the information campaign can be summarized by changes in the weights families assign to peer and school quality. In particular,

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

where β_{St}, β_{Pt}, and β_{Bt} summarize the average change in weights treated families assign to the various quality measures. In a model without information frictions, any changes in the weights families place are due to changes in preferences or salience. This is analogous to the salience impacts driven by bottom-up attention discussed by Bordalo et al. (2013) and Bordalo et al. (2022).[24] In this framework, any change in preferences must be due to families making it more prominent in their decision-making after being reminded of the information.

Turning to a model with imperfect information, families make decisions using their *beliefs* about Q_j^P and Q_j^S. One way to model beliefs is to allow families to have idiosyncratic quality-

[23]For expositional purposes, I ignore the spillover group but that is readily accommodated.

[24]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.

specific biases, b_{Pi} and b_{Si}, that produce proportional deviations from Q_j^P and Q_j^S: $\tilde{Q}_{ji}^P = (1 + b_{Pi})Q_j^p$ and $\tilde{Q}_{ji}^S = (1 + b_{Si})Q_j^S$. I assume b_{Pi} and b_{Si} are drawn from a distribution F_P and F_S with mean μ_P and μ_S, respectively.

In the absence of the information campaign, families' perceived indirect utility is

$$\tilde{U}_{ij} = \tilde{\gamma}_{Pi} Q_j^P + \tilde{\gamma}_{Si} Q_j^S - \lambda d_{ij} + \varepsilon_{ij} \tag{15}$$

where $\tilde{\gamma}_{Pi} = \gamma_P(1 + b_{Pi})$ and $\tilde{\gamma}_{Si} = \gamma_S(1 + b_{Si})$. Making decisions with beliefs distorts the effective weights families assign the various attributes. As in the case with perfect information, the information campaign induces salience effects but also affects belief biases, b_{Pi} and b_{Si}, and the combined effects are summarized by changes in the implicit weights families assigned to Q_j^P and Q_j^S,

$$\tilde{U}_{ij} = \tilde{\gamma}_{Pi} Q_j^P + \tilde{\gamma}_{Si} Q_j^S + \sum_{t \in \{P,S,B\}} (\beta_{Pti} Q_j^P + \beta_{Sti} Q_j^S) \times \mathbf{1}\{i \in \mathcal{I}_t\} - \lambda d_{ij} + \varepsilon_{ij}, \tag{16}$$

where $\beta_{Sti} = \beta_{St}(1 + b_{Si})$ and $\beta_{Pti} = \beta_{Pt}(1 + b_{Pi})$. Because the implied change in average marginal willingness to travel is identified by comparing the choices of applicants across treatment groups that are making choices with and without information, we can decompose the impact.[25]

To do so, we can define potential outcomes for the marginal willingness to travel for peer quality of individual i with treatment t, $MWTT_{iPt}$. In practice, only one outcome is observed for each individual, so the observed marginal willingness to travel for peer quality is

$$MWTT_{iP} = \sum_{t \in P,S,B,0} MWTT_{iPt} D_{it},$$

where $D_{it} = \mathbf{1}\{i \in \mathcal{I}_t\}$. The estimand of interest that summarizes the effects of receiving peer quality information is the observed average change in the marginal willingness to travel,

$$E[\Delta MWTT_{iP}] = E[MWTT_{iPP} - MWTT_{iP0}] \quad . \tag{17}$$

In a randomized intervention, this quantity is identified by comparing the implied $MWTT$ of treated and control applicants.[26] Through the lens of the model, the estimand is equal to

$$E[\Delta MWTT_{iPP}] = \frac{\beta_{PP} - \gamma_P \mu_P}{\lambda}. \tag{18}$$

[25]Implicit in this is a constant salience effect assumption, a perfect compliance assumption, and a similar variances of unobserved preference heterogeneity across treatment groups assumption. The compliance assumption assumes that treated individuals update perfectly, or in other words, their $b_{Pi} = 0$ or $b_{Si} = 0$. This would be implied by a model where families perceive zero noise in the signal of quality they receive. Even without this assumption, one can generate a range of estimates for a variety of compliance rates. Related to similar variances across treatment groups, the randomized assignment to groups makes this assumption plausible.

[26]There are a variety of estimation approaches that aid in identifying this change. Train (2009) argue that a simple logit can be used to approximate average tastes and average changes in tastes. Alternatively, one can estimate treatment group by school indirect mean utilities in willingness to travel units in a first step, and then estimate the relationship in a multivariate regression model in similar spirit to Abdulkadiroğlu et al. (2020), Bayer et al. (2007), Campos and Kearns (2024).

The intervention's impacts nest both a change in preferences governed by the salience term present in the frictionless model and a term governed by imperfect information. The latter term pins down the portion of the change attributable to the mean baseline bias in the population. In the perfect information setting, we have $\mu_P = 0$ and the changes in willingness to travel are only due to salience. As alluded to above, with a randomized intervention, $E[\Delta MWTT_{iPP}]$ is estimated by comparing treated parents to control group parents, γ_P is identified by choices made among control group parents, and auxiliary survey data pins down the moment μ_P. The salience impact is, therefore,

$$\frac{\beta_{PP}}{\lambda} = E[\Delta MWTT_{iPP}] + \frac{\gamma_P \mu_P}{\lambda}.$$

The salience impact, β_{PP}, is attenuated or amplified depending on the direction of the bias at baseline. For example, if $\gamma_P \mu_P > 0$, then the estimated salience impact will, in general, be biased downward. The opposite is true if $\gamma_P \mu_P < 0$. The intuition for this follows from the fact that an information intervention nests two somewhat sequential steps, a debiasing step and a salience step. Appendix Figure F.1 provides some intuition.

Similar expressions can be derived for those receiving only the school quality treatment and those receiving both. In addition, one may hypothesize that receiving treatment about only one attribute may have information and salience effects on other attributes through a correlated beliefs channel, but additional assumptions related to the second moments of the belief distribution are necessary.

One way to model beliefs is to allow families to have idiosyncratic quality-specific biases, $\tilde{Q}_{Pji} = (1 + b_{Pi})X_{Pj}$ and $\tilde{Q}_{Sji} = (1 + b_{Si})Q_{Sj}$. I assume that beliefs are bivariate normal,

$$\begin{pmatrix} b_{Pi} \\ b_{Si} \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} \mu_P \\ \mu_S \end{pmatrix}, \begin{pmatrix} \sigma_P^2 & \rho\sigma_P\sigma_S \\ \rho\sigma_P\sigma_S & \sigma_S^2 \end{pmatrix} \right),$$

with ρ governing the correlation of biases and σ_P and σ_S the respective standard deviations. As before, conditional on receiving a treatment (or signal), families update via an updating rule where the variance of the signal is zero, reflecting the perfect compliance assumption. Given the distributional assumptions on the biases, the posterior mean of peer quality bias, given the school quality treatment—and the fact that agents update their school quality bias to zero—is

$$E[b_{Pi} \mid \mathcal{I}_S = 1] = \mu_P - \rho\frac{\sigma_P}{\sigma_S}\mu_S.$$

With a defined updating rule for one bias given a signal for the other, the average willingness to travel estimands for the different groups are:

$$E[WTT_{iP0}] = \frac{\gamma_P(1 + \mu_P)}{\lambda} \tag{19}$$

$$E[WTT_{iPP}] \equiv E[WTT_{iPP}|\mathcal{I}_P = 1] = \frac{\gamma_P + \beta_{PP}}{\lambda} \tag{20}$$

$$E[WTT_{iPS}] \equiv E[WTT_{iPS}|\mathcal{I}_S = 1] = \frac{\gamma_P(1 + \mu_P - \rho\frac{\sigma_P}{\sigma_S}\mu_S)}{\lambda} + \frac{\beta_{PS}(1 + \mu_P - \rho\frac{\sigma_P}{\sigma_S}\mu_S)}{\lambda} \tag{21}$$

$$E[WTT_{iPB}] \equiv E[WTT_{iPB}|\mathcal{I}_B = 1] = \frac{\gamma_P + \beta_{PB}}{\lambda}. \tag{22}$$

As before, the experimental assignment helps identify changes in willingness to travel induced by the information intervention. The results from the single attribute model translate to the multiple attribute model, but it is worth discussing how correlated beliefs about quality influence the effects of information about one attribute on preferences for other attributes. Continuing from the leading example above, individuals assigned treatment 2 may exhibit a change in their willingness to travel for attribute 1. The change in willingness to travel will nest several factors governed by the degree of imperfect information in the population. The change in the average willingness to travel for this group is

$$E[\Delta MWTT_{i12}] = \frac{\beta_{12}(1+\mu_1)}{\lambda} - \frac{(\gamma + \beta_{12})\rho\frac{\sigma_1}{\sigma_2}\mu_2}{\lambda}. \tag{23}$$

The expression is intuitive and has two countervailing forces. If the information about attribute 2 induces a salience effect for attribute 1 due to a reprioritization of the importance of each, this is captured by β_{12} which is amplified by the degree of bias in the population at baseline, μ_1. This effect is potentially offset by the correlated nature of beliefs. In particular, if biases are positively correlated and families–at baseline—overestimate the quality of attribute 2, then learning about two will dampen the overall salience effect governed by the first term. Similarly, if biases are positively correlated but families underestimate the quality of attribute 2 at baseline, then learning about attribute 2 will amplify the observed change in marginal willingness to travel. Overall, the factors influencing the effects of one attribute on another depend on the presence of salience effects, the degree of imperfect information at baseline, and the correlation of biases. In the case with perfect information, the average change in willingness to travel is only due to salience. In the core of the paper, I only report decomposition estimates for $E[\Delta MWTT_{iP}]$ and $E[\Delta MWTT_{iS}]$ as those are the most policy relevant.

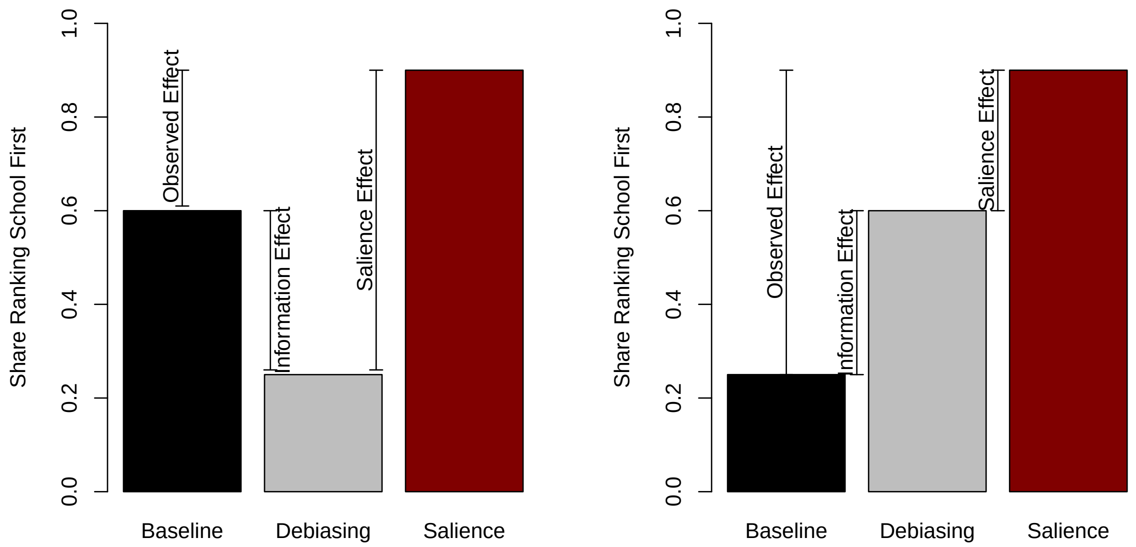
F.1 Intuition for Decomposition

I discuss a hypothesized scenario with one school, School A, and an outside option with families being informed about the relative quality of School A and families only care about one attribute. Appendix Figure F.1 provides intuition for the decomposition, considering cases where families overestimate or underestimate quality at baseline. In both cases, I assume families have a positive taste for the attribute.

In Panel (a), the case where $\gamma\mu > 0$, the debiasing step induces individuals to revise their beliefs downward, leading to a ceteris paribus decrease in their demand for X_j; this is the information effect. The act of providing the information makes families reprioritize the importance they assign X_j, what I refer to as salience, the effect from the second bar to the third bar. The estimand, however, recovers a quantity that subtracts the information effect from the salience effect, since we only observe the change from the first to the third bar.

Panel (b) provides a visual description of the case where families beliefs are biased downward (on average) at baseline. In this case, the information effect leads to a ceteris paribus increase in demand for School A as families revise their beliefs upward. The salience effect is also positive.

Figure F.1: Intuition for Decomposition



(a) With Positive Bias ($\gamma\mu > 0$)

(b) With Negative Bias ($\gamma\mu < 0$)

Notes: This figure reports two panels demonstrating factors contributing to treatment effects in information interventions. The figure relates to a hypothesized scenario with one school, School A, and an outside option with families being informed about the relative quality of School A. The black bars correspond to the share of families choosing school A before the intervention. The gray bar corresponds to the share of families choosing School A in a setting where they had perfect information. The maroon bar depicts the share of families choosing School A in a setting where an information intervention is used to debias their beliefs. Panel (a) reports a setting where families were initially biased upward in their beliefs about relative quality, and Panel (b) reports a setting where families are initially biased downward. In both cases there is a positive salience effect. Comparing the black to the gray bar pins down the information effect. The salience effect is identified by comparing the gray bar to the maroon bar. Empirical estimates identify the difference between the maroon and black bar, which nests both salience and information effects.

G Evidence on Strategic Behavior

The evidence documented throughout the paper demonstrates that the prevalence of information led to families placing substantially more weight on school effectiveness in their schooling decisions. However, both reduced-form and discrete choice perspectives are silent about the role of families' perceived changes in admissions chances at schools which is an additional channel contributing to changes in choices. The potential scope for strategic behavior introduces additional concerns. In this section, I provide distinct pieces of evidence to assuage these concerns and provide suggestive evidence that changes in admissions chances or strategic behavior play a minimal role in this setting.

I approach this in four ways. First, as discussed in the main body of the paper, I demonstrate that many families face no risk in applying as most admissions probabilities at their top-ranked program are degenerate. This is consistent with many discussions with ZOC administrators. In settings with degenerate risk, optimal portfolio models no longer apply and standard discrete choice models identify preferences. Second, I report static evidence regarding strategic behavior in the spirit of Abdulkadiroglu et al. (2006), demonstrating little evidence that families behave strategically as would be implied by simple descriptive tests. Third, I do not find evidence of changes in market-level strategic behavior that would be implied by changes in families' perceived admission chances. Last, I assess the robustness of my leading estimates to various assumptions that attenuate strategic considerations.

G.1 Admissions Probabilities

Appendix Table G.1 reports statistics on applicants' admission probabilities at their top-ranked program for each market. I simulate admissions probabilities by fixing the population of applicants and rerunning the match by redrawing lottery numbers. I do this 1000 times for each market and an applicant's admission probability is the mean across all iterations. I report the mean admission probability, the standard deviation, the share that are exactly equal to zero, and the share that are exactly equal to one.

Across all markets, the mean admission probability across applicants is 0.968 indicating most applicants in the experimental sample face no risk when applying. In fact, Column 4 shows that 73 percent of applicants face no risk, and four markets are entirely risk-free. This is partly a consequence of broader enrollment trends in urban school districts suffering from enrollment decline over the past two decades. LAUSD, in particular, has lost 46% of its enrollment from its peak in 2004.[27]

The prevalence of degenerate risk in ZOC markets opens the door for more straightforward discrete choice models to estimate preferences. Indeed, an applicant with rational expectations and no admission risk will treat the school choice problem as a typical discrete choice problem proposed in the paper. While the share of applicants without admission risk is high, some applicants do face risk. The large share of applicants without admission risk provides a sizable sample to assess the robustness of results to subsamples of applicants with and without

[27] In the 2003-2004 academic year, LAUSD had 746,000 Grade 1-12 students enrolled in the district. Enrollment is 406,000 in the 2022-2023 academic year.

admission risk. I return to this in a following subsection.

Table G.1: Admission Probability Statistics by Zone

	Mean	SD	Share Zero	Share One
Bell	0.885	0.318	0.000	0.713
Belmont	0.999	0.001	0.000	0.270
Boyle Heights	1.000	0.000	0.000	1.000
Carson	1.000	0.000	0.000	1.000
Eastside	0.876	0.330	0.124	0.876
Fremont	0.948	0.221	0.052	0.948
Hawkins	0.999	0.000	0.000	0.463
HuntingtonPark	0.999	0.000	0.000	0.394
Jefferson	1.000	0.000	0.000	1.000
Jordan	1.000	0.000	0.000	1.000
Narbonne	1.000	0.000	0.000	1.000
NorthEast	1.000	0.000	0.000	1.000
NorthValley	1.000	0.000	0.000	1.000
RFK	1.000	0.000	0.000	0.680
SouthGate	0.971	0.168	0.029	0.971
All Zones	0.968	0.176	0.019	0.734

Notes: This table reports summary statistics for simulated admissions probabilities of applicants' top-ranked option on their rank-ordered list. Each row corresponds to summary statistics of applicants in that market. For each market and iteration, I draw new lottery numbers for each applicant, assign them the same priority they had in the match, and reassign applicants to programs using the immediate acceptance mechanism. I do this 1000 times for each market. For each applicant, their simulated admission probability is their mean acceptance rate across all iterations. Each row reports summary statistics corresponding to applicants' simulated admission probabilities. Column (1) reports the mean across applicants, Column (2) reports the standard deviation, Column (3) reports the share of applicants with admission probability equal to zero, and Column (4) reports the share of applicants with admission probability equal to one.

G.2 Evidence on Strategic Behavior

The rules of the mechanism used for assignment are not salient to ZOC families. In fact, the mechanism is not a typical discussion point in the numerous information sessions ZOC administrators organize for parents. If anything, families are instructed to report truthfully and any mention of the benefits of strategic play is nonexistent. This is similar to school choice in Charlotte studied by Hastings et al. (2009) in that the rules of the mechanism are not salient to families.

A few additional facts make strategic play less of a concern in these markets. First, 66 percent of families have not heard of the program one month before applications are due (see Appendix Table D.3), suggesting strategic incentives are not a salient feature of the application process. Second, Campos and Kearns (2024) evaluates the ZOC policy and finds that demand estimation that accounts for strategic incentives yields estimates that are statistically similar to estimates that do not account for strategic incentives. Third, as documented in the preceding section, many families face no admission risk, attenuating the incentives to behave strategically. Evidence notwithstanding, I now provide additional empirical evidence suggesting strategic behavior is not an important feature of the choice process in ZOC markets.

An intuitive test for the presence of strategic behavior is to focus on the most demanded schools in each market and look for sharp drops in demand. As Abdulkadiroglu et al. (2006) point out, under an Immediate Acceptance mechanism it is a mistake to rank an overdemanded school second. Appendix Figure G.1 reports evidence for these intuitive tests. I restrict to the markets that contain evidence of potential strategic behavior.[28]. For zones that have schools that meet this requirement, I then report the share of families that rank the given school at the top of their list and the share of families who rank it second.

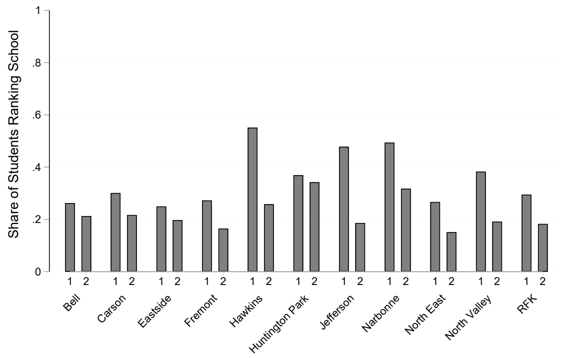
Panel (a), which focuses on the year before the intervention, does not reveal striking evidence of steep drops in demand. In fact, there is not a zone containing a school where most families rank it at the top of their ROL, an indication of substantial preference heterogeneity. Panel (b) reports the same for the 2019 cohort. The first difference between both panels is the increased representation of zones, a consequence of families changing their choices due to the prevalence of information. Except for the North Valley zone, where Humanitas Futures Academy experienced a sizable increase in demand from pre-intervention to post, all zones do not contain a school that most families rank at the top of their ROL.

Evidence of preference heterogeneity notwithstanding, three zones, Huntington Park (HP), Jefferson, and North Valley, stand out with relatively mild drops in demand. For example, in the case of Lyndon Elementary and Quincy Elementary in Abdulkadiroglu et al. (2006), the number of families ranking these schools at the top of their ROL was 5 to 6 times as many as the number of families ranking them second. The drops in demand in North Valley ZOC, for example, are nowhere near as high as the Quincy and Lyndon case. The patterns for Jefferson and North Valley also appear to be similar across all three years. That leaves Huntington Park as a candidate zone where the intervention may have induced mild strategic behavior.

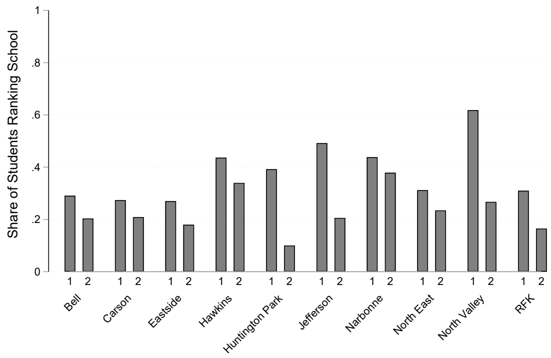
[28] A zone like Belmont is excluded as the number of families ranking the most popular school at the top of their ROL is roughly 10%, limiting the scope for a sharp drop in demand. In general, I focus on zones where the most-demanded school has at least 25 percent of families ranking it first

Overall, however, evidence of strategic behavior is not present in nearly all zones (or markets), corroborating the anecdotal evidence that the rules of the mechanism are not salient to most parents.

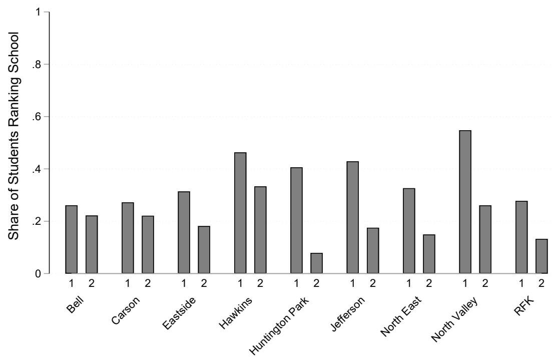
Figure G.1: Reporting Behavior Before and After the Intervention



(a) 2018



(b) 2019



(c) 2021

Notes: This figure reports evidence about reporting behavior in the year before the first experimental wave, 2018, and in the first experimental wave, 2019. In each panel, we report reporting behavior in zones where the most-demanded school had at least 25 percent of families ranking it first. The first bar corresponds to the share of families ranking the given school as their most preferred, and the second bar corresponds to the share of families ranking the school second.

G.3 Robustness Exercises

The evidence in Appendix Figure G.1 motivates additional robustness exercises to assess how the potential strategic incentives of a small subset of families affect the conclusions of the primary findings. Given that an immediate acceptance mechanism has the strongest bite at the top of the rank-ordered list, one reasonable assessment is to probe the robustness of the results when excluding the top-ranked school. Second, we can assess the robustness of the results when excluding the markets where we found some indirect evidence of strategic behavior in Appendix Figure G.1. Last, we can focus on the subset of applicants who face no admission risk, and thus no strategic incentives under a rational expectations framework, to assess if strategic incentives affect the conclusions in the paper.

Appendix Table G.2 and Appendix Table G.3 report evidence regarding the first two tests, with Appendix Table G.2 focusing on models that consider information treatments and Appendix Table G.3 focusing on saturation-level treatments. The first two columns report evidence documented in the paper coming from the preferred estimates. Column (3) and Column (4) report estimates from a sample that excludes the top-ranked option in the estimation procedure. Column (5) and Column (6) report estimates that exclude the potentially concerning zones in Appendix Figure G.1. Across all specifications, the results are qualitatively similar and statistically identical to the baseline specification. This assuages concerns about the potential influence of strategic behavior driven by particular zones or regions of the rank-ordered list most prone to strategic behavior.

Appendix Table G.4 and Appendix Table G.5 compare baseline estimates to estimates from samples of applicants who face no admission risk. These analyses are restricted to the 2019 cohort because we do not observe capacities for 2021 and are unable to replicate the match.[29] Like the other evidence in this section, the baseline estimates are statistically identical to the estimates from applicants without admission risk. This suggests that the behavior of applicants for whom strategic incentives are largest is highly similar to those who face no strategic incentives. The assorted set of results in this section strongly suggest that strategic incentives are weak in ZOC markets and, as a consequence, do not find evidence that strategic behavior influences the primary findings in the paper.

[29] This can be requested if necessary for a revision.

Table G.2: Rank-ordered logit estimates (Information-specific model)

	WTT Estimates					
	Baseline		Excluding Top-Ranked		Excluding Zones	
	IA	AG	IA	AG	IA	AG
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment						
Untreated	0.392***	0.658***	0.594***	0.755***	0.483***	0.734***
	(0.093)	(0.078)	(0.116)	(0.095)	(0.101)	(0.087)
Information: IA	-0.972***	0.474	-1.150***	0.459	-1.164***	0.425
	(0.174)	(0.104)	(0.206)	(0.117)	(0.192)	(0.107)
Information: AG	-0.865	0.424***	-1.010	0.431***	-1.040	0.413***
	(0.171)	(0.101)	(0.200)	(0.114)	(0.186)	(0.106)
Information: Both	-0.815***	0.565***	-0.892***	0.471***	-0.977***	0.534***
	(0.154)	(0.100)	(0.176)	(0.108)	(0.168)	(0.103)
Spillover	-0.947***	0.336***	-1.139***	0.417***	-1.153***	0.320***
	(0.172)	(0.100)	(0.204)	(0.115)	(0.191)	(0.104)
Distance	-0.068***		-0.065***		-0.070***	
	(0.006)		(0.007)		(0.007)	

Notes: This table reports estimates from three separate random utility models. Each considers treatment effects on utility weights for IA and AG (measured in deciles) that vary by the information treatment that is either IA, AG, Both, or Spillover. Spillover refers to parents in treated schools who did not receive information. The first two columns report estimates from the baseline model including all applicants and choices. The third and fourth columns consider all applicants but exclude their top-ranked choice. The fifth and sixth columns consider applicants not belonging to Huntington Park, Jefferson, and North Valley, zones flagged with weak evidence of strategic behavior. Estimates correspond to the average marginal willingness to travel except for the reported distance coefficient. Standard errors are robust and clustered at the school level and estimated via the delta method.

Table G.3: Rank-ordered logit estimates (Saturation-specific model)

	WTT Estimates					
	Baseline		Excluding Top-Ranked		Excluding Zones	
	IA	AG	IA	AG	IA	AG
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment						
Untreated	0.391***	0.656***	0.612***	0.757***	0.483***	0.733***
	(0.093)	(0.077)	(0.120)	(0.097)	(0.101)	(0.087)
Information: High	-0.977***	0.616***	-1.090***	0.424***	-1.103***	0.561***
	(0.154)	(0.095)	(0.185)	(0.098)	(0.168)	(0.097)
Information: Low	-0.743***	0.312***	-0.960***	0.467***	-0.981***	0.323***
	(0.147)	(0.088)	(0.182)	(0.109)	(0.166)	(0.093)
Spillover: High	-1.358***	0.642***	-1.544***	0.528**	-1.471***	0.598***
	(0.322)	(0.196)	(0.367)	(0.223)	(0.332)	(0.206)
Spillover: Low	-0.852***	0.255**	-1.083***	0.405***	-1.078***	0.248**
	(0.175)	(0.105)	(0.214)	(0.125)	(0.194)	(0.109)
Distance	-0.068***		-0.063		-0.070	
	(0.006)		(0.007)		(0.007)	

Notes: This table reports estimates from three separate random utility models. Each considers treatment effects on utility weights for IA and AG that vary by the saturation status of an applicant's middle school treatment and whether they directly received treatment or were part of the spillover group. Spillover refers to parents in treated schools who did not receive information. The first two columns report estimates from the baseline model including all applicants and choices. The third and fourth columns consider all applicants but exclude their top-ranked choice. The fifth and sixth columns consider applicants not belonging to Huntington Park, Jefferson, and North Valley, zones flagged with weak evidence of strategic behavior. Estimates correspond to the average marginal willingness to travel except for the reported distance coefficient. Standard errors are robust and clustered at the school level and estimated via the delta method.

Table G.4: Rank-ordered logit estimates (Saturation-specific model): Baseline versus Sample Without Risk

	WTT Estimates			
	Baseline		No Risk	
	IA	AG	IA	AG
	(1)	(2)	(3)	(4)
Treatment				
Untreated	0.209	0.777***	-0.091	0.834***
	(0.157)	(0.142)	(0.173)	(0.164)
Information: High	-0.364	0.450***	-0.499*	0.476***
	(0.234)	(0.134)	(0.264)	(0.150)
Information: Low	-1.774***	0.429***	-1.616***	0.372**
	(0.354)	(0.142)	(0.373)	(0.151)
Spillover: High	-1.504**	0.479	-1.689**	0.490
	(0.630)	(0.291)	(0.700)	(0.322)
Spillover: Low	-2.246***	0.388**	-2.257***	0.355**
	(0.443)	(0.167)	(0.492)	(0.181)
Distance	-0.056***		-0.054	
	(0.009)		(0.009)	

Notes: This table reports estimates from two separate random utility models. The sample of applicants corresponds to the 2019 cohort of applicants, the cohort for which we can simulate admission risk. The first two columns estimated IA and AG willingness to travel in the baseline model. Treatment is allowed to vary by saturation status and whether an applicant is directly or indirectly treated. The third and fourth columns restrict to the sample of applicants without admission risk, meaning their admissions chances are equal to one at their top-ranked program. The problem reduces to a standard discrete choice program in this case. All estimates are average marginal willingness to travel estimates except for the distance coefficient. Standard errors are robust and clustered at the school level and estimated via the delta method.

Table G.5: Rank-ordered logit estimates (Information-specific model): Baseline versus Sample Without Risk

	WTT Estimates			
	Baseline		No Risk	
	IA	AG	IA	AG
	(1)	(2)	(3)	(4)
Treatment				
Untreated	0.209	0.776***	-0.092	0.838***
	(0.156)	(0.141)	(0.174)	(0.165)
Information: IA	-1.371***	0.539	-1.453***	0.594
	(0.341)	(0.162)	(0.389)	(0.185)
Information: AG	-1.141	0.371**	-1.047	0.336**
	(0.316)	(0.152)	(0.346)	(0.167)
Information: Both	-0.560**	0.415***	-0.606**	0.404***
	(0.259)	(0.142)	(0.289)	(0.156)
Spillover	-2.111***	0.404**	-2.161***	0.384**
	(0.418)	(0.157)	(0.473)	(0.172)
Distance	-0.056***		-0.054***	
	(0.009)		(0.009)	

Notes: This table reports estimates from two separate random utility models. The sample of applicants corresponds to the 2019 cohort of applicants, the cohort for which we can simulate admission risk. The first two columns report utility weight impacts on IA and AG in the baseline model. Treatment is allowed to vary by information treatment and whether or not individuals are indirectly or directly treated. The third and fourth column restrict to the sample of applicants without admission risk, meaning their admissions chances are equal to one at their top-ranked program. The problem reduces to a standard discrete choice program in this case. All estimates are average marginal willingness to travel estimates except for the distance coefficient. Standard errors are robust and clustered at the school level and estimated via the delta method.