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

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Abstract

This paper studies how parents' school choices are affected by information about school and peer quality and how social interactions mediate changes in demand. I design an information intervention that cross-randomizes whether parents receive information about school quality (school value-added) and peer quality. Using a spillover design that varies the saturation of information across schools, I also randomize parents' proximity to other parents with similar information. I find that the information leads to changes in parental preferences toward higher value-added schools, and this occurs when both parents and their neighbors receive information. These results imply substantial information spillovers. I complement this evidence with survey data on the distribution of beliefs over school and peer quality and conclude that the direct and spillover effects of my experiment come primarily from changes in parental preferences rather than an updating of parental beliefs in response to information. These findings show that when parents are informed about school and peer quality, their social interactions lead to changes in preferences in a way that rewards more effective schools. Enrollment in more effective schools led to improved socio-emotional outcomes not captured by standardized exams. This evidence suggests that the intervention did more than alter educational pathways; it also played a critical role in shaping important developmental aspects of students' lives.

Keywords: school choice, school quality, preferences, information
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1 Introduction

Parents' valuation of effective schools govern the success of school choice policies, but many open questions remain as to what they prioritize. Some studies suggest that parents prioritize schools that improve student learning and other outcomes (Beuermann et al., 2022, Campos and Kearns, 2023), 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). It also is not obvious that parents should prioritize school quality if there are other incentives governing school choices (MacLeod and Urquiola, 2019), adding importance to empirically understanding their choices. Much of the existing evidence, however, tends to rely on revealed preference arguments that are complicated by the presence of imperfect information. As a consequence, the existing evidence encounters challenges isolating preferences in settings where choices are made with imperfect information. In addition to uncertainty about parents' valuations, open questions remain about what parents know when making decisions and what factors mediate their choices.

This paper reports evidence from an information provision experiment that sheds light on these open questions. I cross-randomize information about school quality and peer quality to better understand what quality variation parents are most responsive to while simultaneously addressing information gaps. I elicit parents' beliefs about school and peer quality in a baseline survey to better understand the severity of imperfect information before the intervention. Both measures have been extensively studied in prior work (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023, Beuermann et al., 2022, Corradini, 2024, Hastings and Weinstein, 2008, Mizala and Urquiola, 2013, Rothstein, 2006), but to date, we have a limited understanding of what parents actually know about them. For the purposes of the intervention and belief elicitation, school quality is referred to as achievement growth (AG), and peer quality is referred to as incoming achievement (IA). Combining information about AG and IA beliefs with the information provision allows for a decomposition of treatment effects into factors driven by salience and information updating, allowing me to provide additional perspective regarding the intervention's impacts. Last, to gain insight into factors that mediate parents' choices, I introduce a component into the design that allows me to measure the importance of social interactions as captured by spillover effects of information provision (Crépon et al., 2013).

The setting is a market of high schools in Los Angeles neighborhoods referred to as Zones of Choice (ZOC) neighborhoods (Campos and Kearns, 2023).[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 middle schools across two experimental waves.

The experiment's design explicitly considers two primary objectives. The first and most

[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.

important is to assess parents' relative responsiveness to peer and school quality variation, and I accomplish this by cross-randomizing information about each. A second objective is to quantify the importance of social interactions in the school choice process, which I measure using a two-stage randomization procedure (Crépon et al., 2013). Therefore, I first randomize schools to different saturation levels, high, low, or pure control. Conditional on a school's saturation level, I then cross-randomize information about school and peer quality. This design allows me to assess parents' 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 analysis focusing on difference-in-difference estimates of the intervention's effects. This provides several advantages, including a boost in precision and natural falsification tests of effects in pre-intervention periods. I find increases in demand for schools with higher AG among those receiving any treatment. I find sizable spillover effects, statistically and nominally equivalent to treatment effects, the first piece of evidence that social interactions matter. The treatment effects are nuanced, however, in that any effects, direct or spillover, are only detected in high saturation schools. These findings suggest that social interactions are key to generating any meaningful changes in demand, so important that if not enough parents are nearby to discuss the information, then treated parents also do not act on the information. Nonetheless, the impacts on most-preferred school AG suggest there is scope to increase the competitive pressure schools face in public education markets and where competition may be lacking due to apparent preferences for peer quality (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023, MacLeod and Urquiola, 2019).

The reduced form evidence is an informative summary of the intervention's impacts on choices and the importance of social interactions but does not provide insights into the underlying mechanisms producing changes in choices. Changes in choices may occur because families updated their information sets due to information provision, a channel I refer to as information updating. Alternatively, by actively distributing information about particular school features, the intervention may cause parents to change the importance they assign to the quality measures contained in the information treatments, a channel I refer to as salience (Bordalo et al., 2013, 2022). Both channels are likely relevant for multiple information interventions in the literature, but without additional data, it is hard to distinguish between them.

To further explore the potential channels, I complement the information intervention with survey data I collect about parents' beliefs about both measures of quality.[2] The survey data sheds light on what parents know about school and peer quality (AG and IA, respectively) when making decisions in centralized assignment systems. Combining the survey data with the information provision experiment viewed through a discrete choice lens allows for a decomposition of treatment effects that nest the combination of salience and information updating effects. The decomposition relies on the first and second moments of the beliefs distribution, which I collect

[2]Eliciting beliefs about two different measures of quality presents some challenges in conveying messaging to parents. To address this, I deploy videos to teach and aid families' understanding of the differences between IA and AG. The videos serve an instrumental role in improving families' understanding of the content, working in tandem with the social interactions the experiment is designed to measure. Section 4 provides additional details about these factors.

in the survey. Therefore, the baseline survey is intrinsically linked to the moderately structural experimental analysis that complements the reduced form evidence.

Three facts arise from the survey data. First, families tend to underestimate their school's AG and overestimate IA; I refer to overestimation as optimism and underestimation as pessimism.[3] 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 that they generate different rank-ordered lists than in a setting without the biases. Third, I do not find salient student-level attributes that correlate with either IA or AG biases. This finding mirrors evidence that value-added measures tend to weakly correlate with observables, with a key distinction being that I focus on beliefs about value-added.

With the survey data, I return to analyzing the intervention viewed through a discrete choice lens. This analysis features a few key advantages. First, it uses information from the entire rank-ordered list (ROL), providing a comprehensive summary of how families trade off school and peer quality. Second, the reduced-form analysis studies effects on demand for IA and AG 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 AG; similarly, I find that their willingness to travel for IA decreases. The increases in willingness to travel for AG range between 0 and 0.7 kilometers for a school that has AG scores that are 10 percentile points higher. The decreases in willingness to travel for IA range between 0.4 and 1.4 kilometers. The findings are mostly consistent with the reduced-form results, with magnitudes that are quantifiable in terms of willingness to travel. Spillover effects remain mostly identical to the treatment effects within saturation clusters, a third and final piece of evidence highlighting the importance of social interactions. Last, decompositions demonstrate that most of the changes are due to changes in preferences, also interpreted as salience effects. Importantly, equipped with simulated rank-ordered lists implied by the model estimates, I replicate the main reduced form results, suggesting the estimated model accurately captures the intervention effects. Taken at face value, the structural estimates suggest that although families do update their information in response to the intervention, the observed changes in choices mostly reflect a reorientation of their preferences toward higher value-added schools, in part a consequence of bottom-up attention discussed by Bordalo et al. (2013) and Bordalo et al. (2022). Overall, the experiment provides robust evidence that when properly informed, families make choices in a way that is consistent with rewarding effective schools and that social interactions are important mediators governing changes in demand.

The final piece of analysis focuses on how information provision affected student outcomes. I consider both eleventh-grade test scores and socio-emotional outcomes similar to Jackson et

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

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

The findings in this paper contribute to three strands of literature, with the most immediate related to parents' valuation of effective schools. Early findings focus on implications from school choice experiments where some students are lotteried into their most-preferred schools, while others fail to receive offers (Abdulkadiroğlu et al., 2014, Cullen et al., 2006, Deming et al., 2014, Lucas and Mbiti, 2014). The findings in these papers more or less conclude that there are minimal impacts from enrolling in a most-preferred school, indicating that parents do not systematically sort into schools with higher value-added or school quality differences within local markets are minimal. More recently, a growing body of evidence has turned to estimating preferences leveraging the full suite of information contained in rank-ordered lists submitted to centralized assignment systems. Some find that parents place substantial weight on effective schools (Beuermann et al., 2022, Campos and Kearns, 2023), and others find that families are unresponsive to quality variation conditional on other school attributes such as peer composition (Abdulkadiroğlu et al., 2020, Ainsworth et al., 2023).

While these recent papers are a step forward in understanding parents' preferences, they all invariably rely on revealed preference arguments in settings where it is plausible that imperfect information looms large. The presence of imperfect information muddles the interpretation as families unresponsiveness to quality variation may not be due to a lack of valuation but a lack of awareness (Abaluck and Compiani, 2020). This paper contributes to the literature in two ways. It is the first to show evidence of the joint distribution of families' beliefs on peer and school quality. Existing papers have alluded to the potential presence and importance of biases, while this paper measures them. The paper also provides experimental evidence of how families' choices systematically change under various information scenarios, alleviating concerns about interpreting estimates in the presence of information frictions.

A large body of work has deployed information interventions to answer and address a variety of policy-relevant questions (Haaland et al., 2020). In education, the seminal work of Hastings and Weinstein (2008) highlights the importance of information frictions in school choice settings and the potential for information to change both choices and outcomes. Follow-up work has emphasized the importance of easily accessible information and potential inequities in who takes up the information (Cohodes et al., 2022, Corcoran et al., 2018, Corradini, 2024), while also emphasizing the importance of participants lack of awareness with underlying mechanism rules (Arteaga et al., 2022). More recently, a turn to the potential equilibrium effects of large-scale policies has further motivated the usefulness of these interventions in affecting outcomes (Allende et al., 2019, Andrabi et al., 2017).

The existing papers, however, tend to focus on measures that are similar to what I refer to as peer quality and do not distinguish between preferences for peer or school quality. Ainsworth et al. (2023) is the only paper to consider a school-quality-based intervention but focuses on quantifying how much value-added families leave on the table after the intervention, with less emphasis on the potential frictions regarding both peer and school quality. This paper builds on this existing work by further distinguishing between responsiveness to both peer and school quality information, shedding light on families' preferences over both, and decomposing treatment effects to gain further insight into information provision mechanisms. By further providing empirical evidence regarding families' responsiveness to information about both school and peer quality variation, this paper speaks to the broader implications of large-scale school-quality-based campaigns and the impacts they may have on school enrollment segregation (Corradini, 2024, Hasan and Kumar, 2019, Houston and Henig, 2021, 2023)

A third and nascent literature has focused on the implications of peer effects in the school choice process. Existing papers have primarily focused on how externalities permeate through demand systems, with Allende et al. (2019) studying how preferences for peers distort school incentives in a structural model based on insights from Rothstein (2006). Another strand of papers in the market design literature has highlighted that stable matchings may not exist if preferences are interdependent (Sasaki and Toda, 1996). A recent strand of papers has tackled studying the existence of stable matchings, allowing market participants to express preferences for peer attributes (Cox et al., 2021, Leshno, 2021). This paper provides empirical evidence that such peer preferences may not matter in some markets and is consistent with findings for prior ZOC cohorts (Campos and Kearns, 2023). My findings also pivot the peer effect discussion from externalities that do not generate interdependent preferences as captured by preferences for peer composition to externalities operating through information and social networks (Golub and Sadler, 2017). The evidence of social interactions in the school choice process gives rise to potential network-based inequalities that have received less empirical attention in the school choice literature, opening up an avenue for future work.

The rest of the paper is organized as follows. Section 2 provides a description of the setting in which the intervention takes place. Section 3 presents a simple school choice framework that aids the interpretation of effects and motivates a decomposition. Section 4 discusses the experiment's design in detail as well as the data and standard checks in the randomized control trials. Section 5 reports results from a reduced-form analysis of the intervention's impacts. Section 6 reports descriptive evidence arising from the survey and studies the intervention's effects from a discrete choice perspective providing insights into the various channels contributing to the intervention's impact. Section 9 discusses the implications of the findings for future research, and Section 10 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.[4] The markets vary in size and programs' spatial differentiation. Some markets contain as few as two schools (2 programs) to as many as five schools (15 programs), and families apply to programs in their market the year before enrollment. Campos and Kearns (2023) provide a more detailed description of the program's history and expansion in 2012.

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

Families residing within ZOC boundaries apply to high schools during the fall semester of their students' eighth-grade year. During this time, ZOC administrators and guidance counselors make the application a salient aspect of this semester. It is during this time period where most families learn about the program's existence and start researching their options.[5] Failure to submit an application may result in being assigned to an undesirable school that is not a students' neighborhood school. In addition to application submission incentives, district and high school administrators devote a considerable amount of time and resources to inform parents about the program and their options. District administrators meet with middle schools to help facilitate application submissions, and they also hold information sessions to inform parents about the program, their options, and how to submit applications. Open houses are hosted by high schools to help recruit students. In past years, the district experimented with sending mailers to families informing them about the program and their options.

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

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

[5]The survey results discussed in Section 6 show that roughly 70% of families in the 2021 application cohort had not heard of the program at the start of the application cycle.

2009), the mechanism is not strategy proof as it incentivizes families to misreport their ordinal preferences to avoid being assigned to a school far down their preference list (Abdulkadiroğlu and Sönmez, 2003).

In general, there is mixed evidence about the degree of sophistication and incentives to misreport preferences under immediate acceptance mechanisms. One body of evidence from various cities shows that low socioeconomic status families are more prone to misunderstand the rules and are less likely to strategize (Abdulkadiroglu et al., 2006, Agarwal and Somaini, 2018, Terrier et al., 2021), while other research finds weaker socioeconomic status gradients with respect to strategizing (Calsamiglia et al., 2020). ZOC anecdotes suggest that the mechanism's rules are not too salient during information sessions. Therefore, it is likely that strategizing is not a first-order concern given the disproportionate share of low socioeconomic status families and the low importance assigned to the mechanisms' technical rules beyond priorities. Evidence notwithstanding, I provide extensive evidence that strategic behavior is not a first-order concern in ZOC markets in Appendix E.

Information gaps are likely prevalent in ZOC markets. To begin, many families are unaware of their eligibility and the necessity to participate in the program at the start of the application cycle (see Appendix Table C.3). In addition, many "low-touch" information interventions have been shown to influence K-12 choices across the United States (Cohodes et al., 2022, Corcoran et al., 2018, Hastings and Weinstein, 2008, Valant, 2014, Weixler et al., 2020) and around the world (Ajayi and Sidibe, 2020, Ajayi et al., 2020, Allende et al., 2019, Andrabi et al., 2017, Arteaga et al., 2022). The findings from low-touch interventions argue that treatment effects imply the presence of imperfect information, as perfectly informed families would not change their choices in response to researcher-provided information.

These implications are limited as a combination of factors influence changes in K-12 choices in response to information interventions. For example, simply showing families information about any attribute will make them rethink the importance of that attribute, effectively "changing" their preferences, referred to as bottom-up attention by Bordalo et al. (2022). Without additional data on families' beliefs, however, it is impossible to distinguish between information-updating and salience (or preference) effects. Perhaps surprisingly, the existing literature is thin in terms of collecting families' beliefs (Ainsworth et al. (2023) and Corradini (2024) are notable exceptions) and thus cannot distinguish between the confluence of factors contributing to changes in K-12 choices. The following section bridges this gap with a simple model that motivates the survey collection and intervention.

3 Conceptual Framework

Canonical school choice models assume families have accurate information at the time they make decisions, yet a growing 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 4 for intervention details). Let $\mathcal{I}_P$ and $\mathcal{I}_S$ be the set of families receiving peer quality and school quality information, respectively, and let $\mathcal{I}_B$ correspond to the families receiving information about both. The effects of the information campaign can be summarized by changes in the weights families assign to peer and school quality. In particular,

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

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).[6] 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.

In a model with information frictions, 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-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 random with mean μ_P and μ_S, respectively.[7]

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

$$\tilde{U}_{ij} = \tilde{\gamma}_{Pi} Q_j^P + \tilde{\gamma}_{Si} Q_j^S - \lambda d_{ij} + \varepsilon_{ij} \tag{1}$$

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

[6] 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.

[7] Appendix B discusses alternative distributional assumptions in greater detail.

the combined effects are summarized by changes in the implicit weights families assigned to Q_j^P and Q_j^S:

$$\tilde{U}_{ij} = \tilde{\gamma}_{Pi} Q_j^P + \tilde{\gamma}_{Si} Q_j^S + \sum_{t \in \{P,S,B\}} (\beta_{Pt} Q_j^P + \beta_{St} Q_j^S) \times \mathbf{1}\{i \in \mathcal{I}_t\} - \lambda d_{ij} + \varepsilon_{ij}. \tag{2}$$

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.[8]

Conceptually, we can define potential outcomes with respect to 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,

$$E[\Delta MWTT_{iP}] = E[MWTT_{iPP} - MWTT_{iP0}] \quad . \tag{3}$$

In a randomized intervention, this quantity is identified by comparing the implied $MWTT$ of treated and control applicants.[9] Through the lens of the model, the estimand is equal to

$$E[\Delta MWTT_{iPP}] = \frac{\beta_{PP} - \gamma_P \mu_P}{\lambda}. \tag{4}$$

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,

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

The salience impact, β_{PP}, is attenuated or amplified depending on the direction of the bias

[8] 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.

[9] 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 (2023).

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 B.1 provides some intuition.

Similar expressions can be derived for those receiving only the school quality treatment and those receiving both; see Appendix B for the details. The model allows for the utility weight of one attribute to be affected by information about another, and this is also discussed in further detail in Appendix B.

Most importantly, Equation 4 and its analogs allow for a decomposition of treatment effects into salience and information updating effects. This provides perspective into the underlying mechanisms at play that produce average changes in behavior from information campaigns. I design a survey that measures the relevant moments for the decomposition of both direct and indirect effects. And more generally, I design an experiment oriented around this conceptual framework to identify preference impacts and report the implied decomposition in addition to reduced-form effects.

4 Experimental Design

Timeline

I incorporate a survey and information provision into a typical application cycle discussed in Section 2. The four phases that summarize the experiment are (i) the baseline survey, (ii) the information intervention, (iii) deliberation, and (iv) application submission. The survey distribution happens before the application cycle begins so that it can document parents' beliefs and preferences before the intervention. Information is distributed before applications are collected and well before the deadline. The wide interval of time between information and submission allows parents to internalize the information and deliberate among themselves. After the deliberation process, parents submit applications and the intervention is complete.

Baseline Survey

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

The survey distribution changed in each wave. In the first year, only a paper survey was issued to students in their eighth-grade homeroom classrooms; in the second wave, both the paper and digital surveys were distributed.[10] The digital survey was messaged to families via internal district messaging services. While the survey distribution methods changed across

[10] Every year, LAUSD administers the School Experience Survey to every student and parent in the district. The school district believed a paper version would yield the highest response rate but that was incorrect.

waves, the questions remained constant. Efforts to digitize the paper surveys produced few surveys with enough signal to use in the paper, so the survey results in this paper consist of survey evidence from the second wave in digital format.

While there is precedent eliciting beliefs about peer or school quality in isolation (Ainsworth et al., 2023), there are substantial hurdles in eliciting beliefs about each jointly. Effective messaging that succinctly explains the differences between peer and school quality is challenging to produce. I addressed these belief elicitation challenges in two ways. First, focus groups with LAUSD parents were conducted along with piloting different messages on Amazon MTurk (see Appendix Section C.2 for summary statistics from the piloting). The results from the pilot were mimicked during focus group discussions. Extensive piloting suggested IA as the most effective term for peer quality and AG as the most effective term for school quality. The term IA aims to signal that it is a measure of peer quality that is less associated with school inputs as it is captured as students enter the school. In contrast, AG clearly signals that it is a measure of students' academic progress occurring during their tenure at the school. This choice of messaging avoids having to use terminology such as value-added, which is arguably more challenging to describe, but still conveys the message that one measure is about growth and another is about an achievement level.

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

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

Defining School and Peer Quality

The measures of school and peer quality are conceptually tied to a constant effects potential outcome model of achievement.[11] IA is calculated as the implied peer quality estimates derived from a model described in Appendix D, and AG is the estimated school value-added from the

[11] 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, 2023), with more recent evidence of its importance for the choice between remote and in-person instruction (Bruhn et al., 2023).

same model. Given the lack of quasi-experimental variation in school assignment, the model is estimated via ordinary least squares.[12] 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.[13]

Randomization

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

The randomization process occurs within separate ZOC markets or zones, with the first randomization layer occurring at the school level and the second at the individual level. Each zone is considered a separate market and has different middle schools that feed into the zone. Students from a set of schools that uniquely feed into a zone have the same effective market of schools to choose from, so each block of schools is a different experiment.[14]

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

Within each treated school and conditional on their assigned saturation level, the second randomization layer cross-randomizes the different information treatments. The individual-level randomization coupled with the school-level experiment helps to identify intent-to-treat effects for households directly receiving information and for households indirectly receiving information (a spillover effect) by comparing treated households (direct and indirect) to households in the pure control school, where no one received any information.[15]

[12]Campos and Kearns (2023) find that school quality is forecast unbiased in Los Angeles, and I report similar findings in Appendix D.

[13]Peer effects potentially influence AG estimates. In Appendix D, 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).

[14]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.

[15]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

Figure 2 provides a visual representation for the experiment in the Bell Zone of Choice. Elizabeth Middle School (MS) is randomly assigned to high saturation (treatment H), where π^h share of households receive each treatment, and Ochoa MS is assigned to low saturation. Nimitz is the pure control school, highlighted by the red arrows. Among treated schools, the two information treatments are cross-randomized with the share receiving each determined by the school-level saturation levels. This design has a total of eight treatment statuses, one for each information- and saturation-specific treatment, and each treatment status is identified relative to households in the pure control school.

Treatment Letters

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

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

Data and Experimental Sample

The data for this paper come from the LAUSD and the ZOC office. There are two types of outcomes I consider that require me to pull from various sources of data. The most relevant to the questions posed in this paper relate to choices submitted to the centralized assignment system, captured in rank-ordered lists. These data come from the ZOC office. I also focus on enrollment, cognitive, and non-cognitive outcomes later in my analysis, and for this, I use administrative data provided by LAUSD. The enrollment and cognitive (test score) outcomes are standard in most administrative data for school districts. The non-cognitive outcomes are collected in an annual School Experience Survey (SES) that the school district administers since 2010 and similar to data collected in Chicago and studied by Jackson et al. (2020).

The experimental sample contains students attending a feeder middle school during the eighth grade. In 2019, there are 13,015 students meeting this requirement and slightly fewer in 2021.[16] These students are not a random sample from the LAUSD population.

Table 1 reports descriptive statistics of eighth-grade students enrolled in LAUSD schools in fall 2019. The typical ZOC student is noticeably different from the typical eighth-grade student elsewhere in the district. This student is entering high school performing roughly 22% of a

interactions occur between middle school parents with children enrolled in the same school.

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

standard deviation more poorly on math and reading scores than the typical non-ZOC student. Roughly 12% of ZOC parents have earned a four-year degree, and 94% of ZOC students are classified as poor. They are also more likely to be classified as English learners. In addition to these socioeconomic differences, there are vast racial and ethnic differences. Ninety percent of rising ZOC students are classified as Hispanic compared to 64% elsewhere in the district. The approximate racial and socioeconomic homogeneity of ZOC students was similar for past cohorts studied in Campos and Kearns (2023). While these students are notably different from the LAUSD population, treatment assignment occurs within the experimental sample.

Balance

Table A.2 reports balance for the school-level randomization. Across 52 feeder-year middle schools, 32 get randomly assigned to the low-saturation treatment, 31 get randomly assigned to the high-saturation treatment, and 41 remain as pure control schools. There are minimal differences between treated and pure control schools across an array of school attributes, including achievement and various demographic characteristics. Special education status is a notable omission that is not balanced, but joint tests fail to reject the null hypothesis pointing to an imbalance by chance.

Table A.3 reports balance for the student-level randomization conditional on saturation status. These balance checks are limited to the sample of low- and high-saturation status schools as pure control schools do not contain any treated families. Mirroring the school-level balance checks, the randomization procedure produces a balanced sample across an array of student baseline outcomes and characteristics, including achievement and demographic characteristics. Both tables point to the success of the randomization process. Throughout the analysis, however, I still control for the reported baseline covariates to increase precision in the estimates.

5 Reduced-Form Evidence

In this section, I begin by reporting experimental difference-in-difference estimates, where I initially do not distinguish between different treatment types and emphasize cluster-specific effects and corresponding spillover effects. I then focus on models that ignore saturation clusters but do distinguish between treatment types. The combination of reduced-form results emphasizes the importance of social interactions from different perspectives. Additional evidence is reported in Appendix F.1.

5.1 Difference-in-Differences

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

$$Y_i = \alpha_{z(i)t(i)} + \alpha_{g(i)} + \gamma' X_i + \sum_{k \neq -1} \Bigg(\underbrace{\beta_{Lk} D_{L(i)} \times Post_{k(i)} + \beta_{Hk} D_{H(i)} \times Post_{k(i)}}_{\textit{High and Low Treatment Groups}} + \underbrace{\psi_{Lk} C_{L(i)} \times Post_{k(i)} + \psi_{Lk} C_{H(i)} \times Post_{k(i)}}_{\textit{High and Low Spillover Groups}} \Bigg) + u_i \tag{5}$$

where α_{zt} are zone-by-year effects, α_g are treatment group fixed effects, $D_{L(i)}$ and $D_{H(i)}$ are low- and high-saturation treatment indicators, $C_{L(i)}$ and $C_{H(i)}$ are low- and high-saturation spillover group indicators, and $Post_{k(i)} = \mathbf{1}\{t(i) - 2019 = k\}$. The β_{Lk} and β_{Hk} terms capture difference-in-difference estimates relative to the year before the first experimental wave in 2019 for low- and high-saturation groups, respectively, and ψ_{Lk} and ψ_{Hk} are defined similarly for parents in the spillover group. All parameters are identified by comparing changes in application behavior between applicants in the respective groups and applicants in pure control schools. As previously mentioned, this approach provides improvements in precision relative to the static experimental specification.[17] 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 F.1 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 5, considering top-ranked school incoming achievement and achievement growth as outcomes. In both panels, gray lines correspond to estimates of effects for those in low-saturation schools, and maroon lines correspond to effects for those in high-saturation schools. Dashed lines correspond to treated applicants and solid lines correspond to spillover applicants.

Panel (a) reports effects on most-preferred achievement growth. The maroon lines demonstrate that applicants in high saturation schools increased their demand for schools with higher AG in both experimental waves. Both direct and indirect treatment effects are similar, with larger effects in the second experimental wave. In contrast, the gray lines demonstrate no effects among applicants in low-saturation schools. Across all groups, there is no evidence that treated groups' application behavior trended differently leading into the intervention. Turning to Panel (b), the evidence shows that demand for IA was unaffected by the intervention. Appendix Figure F.3 and Appendix Figure F.4 report analogous findings with randomization-based inference.

The results in Figure 4 emphasize two findings. First, any meaningful changes in demand are reflected 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 C.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

[17]The static results are reported in Appendix F.1 and are similar but less precise.

to both treated and untreated parents' lower take-up of the information. This latter finding mirrors the importance of social engagement with information in generating meaningful changes in behavior (Banerjee et al., 2018).

Table 2 reports treatment effects on other school attributes potentially correlated with school incoming achievement and achievement growth. The table finds minimal evidence that changes in demand for AG substantially alter other attributes of most-preferred schools, suggesting that the information did not alter families' perceptions about other school attributes in a way that generated changes in demand for those attributes, a finding that is corroborated in the structural analysis. Appendix Section F.1.1 further assesses treatment effect heterogeneity.

5.2 Distributional Estimates

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

$$\mathbf{1}\{Y_i \leq a\} = \alpha_{z(i)t(i)} + \alpha_{g(i)} + \gamma' X_i + \beta_P T^P_{it(i)} + \beta_S T^S_{it(i)} + \beta_B T^B_{it(i)} + \beta_C C_{it(i)} + u_i, \quad a \in [\underline{a}, \bar{a}] \tag{6}$$

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

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

The assemblage of reduced-form evidence is summarized with a few key points. First, imperfect information about school effectiveness is potentially empirically relevant as families changed their choices in response to the intervention. This has been highlighted in Ainsworth et al. (2023) and alluded to in prior research (Abdulkadiroğlu et al., 2020, Beuermann et al., 2022, Rothstein, 2006). Second, and in contrast to previous research, I show that when information about both peer and school quality are prevalent, families systematically choose more effective schools without meaningful average changes in their most preferred school peer quality. This evidence suggests that effectiveness-oriented campaigns can orient demand in a way that parents reward effective schools, with implications for school competition and student outcomes (Abdulkadiroğlu et al., 2020, Campos and Kearns, 2023). Third and last, the reduced-form results reveal that social interactions, corresponding to parents discussing the information among themselves, are important determinants for own-school choices. The existing literature thus far has provided anecdotes and qualitative evidence about the importance of networks (Fong, 2019, Kosunen and Rivière, 2018); this is the first evidence documenting social interactions matter for individual choices in school choice settings.[18]

The reduced-form results thus far show how choices changed on average and cannot speak to the factors influencing the changes in choices. As discussed in the conceptual framework, some of the changes may be due to information updating, while others are due to changing preferences or salience impacts. In the next section, I begin exploring these possibilities by analyzing the survey data I collected, yielding insights about what parents know about schools before information provision.

6 Survey Evidence

The baseline survey elicited baseline preferences and beliefs about school and peer quality.[19]In addition, there were other questions that revealed information about parents' intentions during the school choice process, which are discussed in detail in Appendix C. In this section, I first focus on descriptive evidence on elicited preferences and beliefs. I then return to the experiment, combining the survey results with a slightly more structural approach to shed light on the various factors contributing to the treatment effects.

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

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

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

[19]See Appendix Table C.2 for a characterization of survey respondents.

6.1 Descriptive Evidence

Figure 6 reports a histogram of elicited pessimism for both IA and AG. On average, parents are pessimistic about school AG but are slightly optimistic about school IA. While roughly 50% of parents are pessimistic about AG, only 34% are pessimistic about IA. These patterns are not a consequence of center tendency bias; Appendix Figure C.4 reports the overlap in estimated deciles and elicited belief deciles. The figure shows substantial overlap between AG beliefs and measured AG, and to a lesser extent, the same is true for IA, with both findings indicating that elicited beliefs carry some signal.

It is worth noting that these biases are choice-relevant. Appendix Figure C.5 and Appendix Figure C.6 demonstrate that biases affect choice set-specific ordinal rankings of IA and AG. 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 C.7 demonstrates that biases generate substantial shares of application mistakes across the rank-ordered list, implying that these biases are choice-relevant.[20] As this is the first finding in the literature regarding beliefs about both of these measures, it is worth reporting some additional patterns about beliefs.

The pessimism patterns documented in Figure 6 hold across most of the entire rank-ordered list. Figure 7 reports average pessimism across each position of the rank-ordered list. There are four findings that immediately stand out. Throughout the list, parents are more pessimistic about AG than they are about IA. They also get progressively more pessimistic about schools they rank farther down their list, and the patterns is slightly more pronounced for AG. For top-ranked options, parents tend to be optimistic about both IA and AG. They are optimistic about IA across the entire list, while AG optimism shifts toward pessimism at the third-ranked option.

To explore potential differences in beliefs by student ability, I use baseline achievement as a summary measure. Figure C.2 reports the relationship between pessimism and students' baseline achievement. Panel (a) reports the relationship for all options, and Panel (b) focuses on the top-ranked option. Perhaps surprisingly, both panels indicate a lack of a relationship between AG pessimism and students' baseline achievement. In contrast, there is a modest achievement gradient for IA, indicating that higher-achieving families have beliefs that are closer to the truth. The latter finding may not be surprising as there are numerous publicly available sources reporting measures similar to IA, and more-resourced families likely access this information at a higher prevalence.

Appendix Table C.4 reports additional correlations between most-preferred school biases and student baseline covariates. I find that college-educated parents tend to underestimate IA both unconditionally and conditional on other covariates, mirroring the correlation between pessimism and baseline achievement. There are some racial and ethnic differences in IA pessimism, but they are not large. Low socioeconomic status parents tend to be overestimate IA.

[20] 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.

Turning to AG pessimism, few student characteristics correlate with it. Hispanic families tend to underestimate AG the most, and few other covariates stand out with meaningful differences.

In summary, there is substantial heterogeneity in beliefs about schools in families' choice sets as displayed in Figure 6. 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 5, 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.

7 Discrete Choice Evidence

The starting point of this analysis is an indirect utility model discussed in the conceptual framework in Section 3. The model assumes families take into account both school and peer quality, have a distaste for distance, and have unobserved preference heterogeneity that is known to the family at the time they state their preferences.[21] The information intervention causes families to change the utility weights they assign to the various quality measures, allowing us to estimate the intervention's impacts in terms of changes in families' willingness to travel for each measure of quality. An important feature of this approach is that this analysis allows me to hold constant the treatment effect on one quality measure while studying the other, a feature that is subtle yet important given the correlation structure of the quality measures.

I summarize the intervention's impacts with a model analogous to Equation 2, which focuses on changes in parents' willingness to travel. The distributional assumptions on beliefs and perfect compliance imply that for families receiving only the peer quality treatment, the observed change in the average marginal willingness to travel is

$$E[\Delta MWTT_{iPP}] = \frac{\beta_{PP} - \gamma_P \mu_P}{\lambda}. \tag{7}$$

Families receiving school-quality-only treatment have an analogous decomposition:

$$E[\Delta MWTT_{iSS}] = \frac{\beta_{SS} - \gamma_S \mu_S}{\lambda}. \tag{8}$$

Last, for families receiving both treatments,

$$E[\Delta MWTT_{iXB}] = \frac{\beta_{XB} - \gamma_X \mu_X}{\lambda}., \tag{9}$$

where X corresponds to either P or S. As displayed in Appendix Figure B.1, the observed

[21]Note that this model abstracts away from families' beliefs about admission chances, implicitly assuming the intervention does not affect such beliefs. As discussed further in Section 2, the mechanism choice is not a salient feature of the ZOC setting; families do not know the rules, and the rules are not discussed during information sessions. Therefore, it is unlikely that the intervention caused families to revise their beliefs about admissions chances. It is also worth noting that due to declining enrollment in the district, most schools are undersubscribed and applicants get into their most-preferred school with certainty. Appendix E discusses this in further detail.

change in willingness to travel nests an information effect and a salience effect. The sign of the former depends on population mean bias before the intervention and the latter is ambiguous. Standard errors are clustered at the school level and estimated via the delta method where appropriate.

Table 3 begins by reporting the intervention's impacts, not discerning between the two channels. The first two columns report utility weight estimates for IA and AG, reported in willingness to travel units (in kilometers). 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 Column (1) and (2) show that untreated families tend to place a positive weight on IA and AG, with a higher weight on AG that is statically different from the weight on IA (p-value = 0.017). This finding mirrors previous findings documented for earlier ZOC cohorts in Campos and Kearns (2023) 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 IA and increase their willingness to travel for AG, regardless of the information treatment they receive. Mirroring the reduced-form evidence, the ninth and tenth rows of Table 3 show robust evidence of spillovers with effects statistically equal to information effects.[22] The evidence also reveals that willingness to travel impacts on IA are statistically similar, regardless of the information treatment (p-value=0.73); the same is true for willingness to travel impacts on AG (p-value=0.19). Overall, the evidence in Table 3 demonstrates that families responded to information about AG and IA 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. For example, 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 F.2, I report the reduced form effects implied by the corresponding model in Table 3. I first construct new rank-ordered lists using the indirect utility estimates obtained by summing the estimated systematic component of utility and random draws of the unobserved preference heterogeneity, and then I estimate reduced form effects as in Figure 4. The treatment effects are identical, providing suggestive evidence that the intervention mostly influenced the relative weights of the family assigned to IA and AG. 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. 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

[22] Tests of equality between each treatment arm and spillover arm fail to reject equality.

figure correspond to IA MWTT treatment effects, while the subsequent two bars correspond to AG MWTT treatment effects. The estimated information updating component is represented by the gray bars and the salience component is represented by the black bars. The takeaway from Figure 8 is that salience effects explain most of the changes in choices, a consequence of bottom-up attention discussed in Bordalo et al. (2013) and Bordalo et al. (2022). The evidence suggests that the information campaign reoriented families' relative prioritization of school and peer quality, leading to a relative increase in the demand for AG above and beyond what can be explained by baseline mean IA and AG 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 is a consequence of 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.

7.1 The Role of Strategic Incentives and Perceived Admissions Chances

The evidence in the previous section shows that families average MWTT for AG increased and their average MWTT for IA 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 E, I show that a majority of applicants (roughly three-quarters) face no admission risk. In fact, four 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. This feature of the setting 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. As a consequence, 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 E provides extensive robustness checks assuaging concerns about the role of strategic behavior affecting the interpretation of the findings. I provide evidence from four exercises. First, I descriptively show that behavior implying strategic behavior is not too prevalent in the

ZOC setting, following intuitive descriptive checks suggested by Abdulkadiroglu et al. (2006). Second, I show that the evidence implying strategic behavior did not substantially change with the intervention, an indication beliefs about admissions chances were not severely affected by the intervention.[23] 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

8 Impacts on Outcomes

In this section, I focus on how the intervention affected outcomes. I start by assessing whether capacity constraints led to smaller enrollment impacts than implied by application behavior. I then focus on two sets of outcomes. The first corresponds to student-level responses to the district's annual School Experience Survey (SES), capturing measures of socio-emotional development as in Jackson et al. (2020) and other measures of overall satisfaction. I denote these as non-cognitive outcomes. The second focuses on standardized test scores, but due to the nature of testing in California is limited to only include the first experimental wave.[24]

Appendix Figure F.1 demonstrates effects on *enrolled* school attributes. Mirroring the most preferred impacts displayed in Figure 4, we find increases in the AG of enrolled schools among those in high saturation schools. Treatment effects on enrolled school IA are mostly indistinguishable from statistical noise and small in magnitude. The evidence shows that the intervention was successful in increasing demand for effective schools, which also led to enrollment in more effective schools. The similarity between effects on most-preferred ranking and enrollment is partly due to declining enrollment in LAUSD, making most ZOC programs in the experimental years undersubscribed.

Table 4 focuses on other outcomes of interest drawn from the SES and test score data. The SES is administered to students every year to students in most grades and all students in high school. I categorize the wealth of questions into five indices mostly following Jackson et al. (2020). The first is a happiness index measuring students' levels of satisfaction at the school where they enroll in ninth grade. The second is an interpersonal index including questions about students' proclivity to get along with others and those whose points of view differ. The school

[23]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.

[24]LAUSD high school students only take standardized exams in eleventh grade, so that is the only year for which there is available test score data.

connectedness index includes questions like “I feel like I am part of this school.” The Academic Effort index includes questions such as “When learning new information, I try to put the ideas into my own words” and “I come to class prepared.” The Bullying index includes a host of questions covering teasing, physical bullying, and cyberbullying. Each index is standardized to be mean zero with a standard deviation equal to one. Appendix A.1 discusses the indices with greater detail. Test score outcomes are measured in eleventh grade, the only year high school students are tested in California. The focus on eleventh grade limits the test score coverage to students part of the first experimental wave that I observe test scores for.

Panel A of Table 4 focuses on survey-based non-cognitive outcome measures. Across all survey measures, treatment effects for students in low-saturation schools tend to be indistinguishable from statistical noise. Treatment effects are most pronounced among students in highly saturated schools for the 2021 cohort. The happiness index reveals that students in high saturation schools in the most recent experimental wave, experience an increase in their school satisfaction index of roughly 7 percent of a standard deviation. The interpersonal skills index also improved, as did the school connectedness, academic effort, and bullying indices, with index improvements ranging between 4 percent to 9 percent of a standard deviation. Students in highly saturated schools in the 2019 cohort also experienced improvements in bullying-related outcomes. Appendix Table A.1 suggests that the consistent improvements in bullying-related outcomes for both cohorts in the high saturation group are due to the fact that bullying is most predictive of higher AG rankings.

These findings contribute to the mounting evidence that schools and teachers impact an array of outcomes, not strictly limited to cognitive scores (Beuermann et al., 2023, Jackson, 2018, Jackson et al., 2020, Petek and Pope, 2023, Rose et al., 2022). The evidence in Panel A suggests that by changing parents’ choices, treated students were more likely to enroll in more effective schools which also affected their non-cognitive and socio-emotional outcomes. Further support for the significance of school quality on these broader outcomes is found in the appendix, where Appendix Table A.1 shows a strong correlation between school quality and four key socio-emotional defined similarly as in Jackson et al. (2020). This evidence suggests that the intervention did more than alter educational pathways; it also played a critical role in shaping important developmental aspects of students’ lives.

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

9 Discussion

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

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

A second key finding is that social interactions facilitate measurable changes in demand. The spillover results provide evidence of an externality in school choice that is distinct from a preference for peers that has received much attention in the empirical (Allende, 2019, Mizala and Urquiola, 2013, Rothstein, 2006) and theoretical literature (Cox et al., 2021, Leshno, 2021). Demand externalities seem to operate through information acquisition *before* centralized matches occur and become less dependent on assignments. This pivots the discussion to the endogenous information acquisition stage (Chen and He, 2021, Harless and Manjunath, 2015, Immorlica et al., 2020, Maxey, 2021) and emphasizes network-based externalities. For example, if parents' information sets are shaped by their networks, then common findings that disadvantaged families have a lower taste for academic quality (Hastings et al., 2006) or less take-up of information (Cohodes et al., 2022, Corcoran et al., 2018, Finkelstein and Notowidigdo, 2019) can be potentially explained by biased or lack of information that flows in their networks. Information campaigns that further motivate interactions can potentially reduce existing school quality gaps, similar to other information campaigns in other settings (Banerjee et al., 2018).[25] Incorporating network-based preference externalities is an important avenue for future theoretical and empirical research.

The third key finding relates to the role of salience present in many information interventions. The beliefs data I collected allowed me to shed light on factors influencing treatment effects, something information interventions are typically silent about (Haaland et al., 2020). The decomposition I provide demonstrates that information campaign average effects potentially operate by changing preferences and, perhaps to a lesser degree, information updating. Information interventions, however, are commonly motivated to allow consumers to make more informed decisions and reduce information gaps. The findings suggest that information interventions play a powerful role in shaping families' preferences and choices, above and beyond addressing information gaps that were present in ZOC markets. At one extremity, this suggests that information interventions can be used as tools to reorient demand in a way consistent with policymaker goals. For example, policymakers interested in successful school choice policies can make school-quality information widely available, serving a dual purpose of eliminating information gaps and reorienting demand to potentially improve student outcomes. A better understanding of the mechanisms through which information campaigns operate continues to be an important topic for future work.

10 Conclusion

Parents' choices govern the success of school choice initiatives and it is paramount to understand both their preferences and factors that mediate their choices. This paper provides survey and experimental evidence about parents' beliefs and valuation of effective schools in a select set of high school markets in Los Angeles, while also studying the role of social interactions during the preference formation stage.

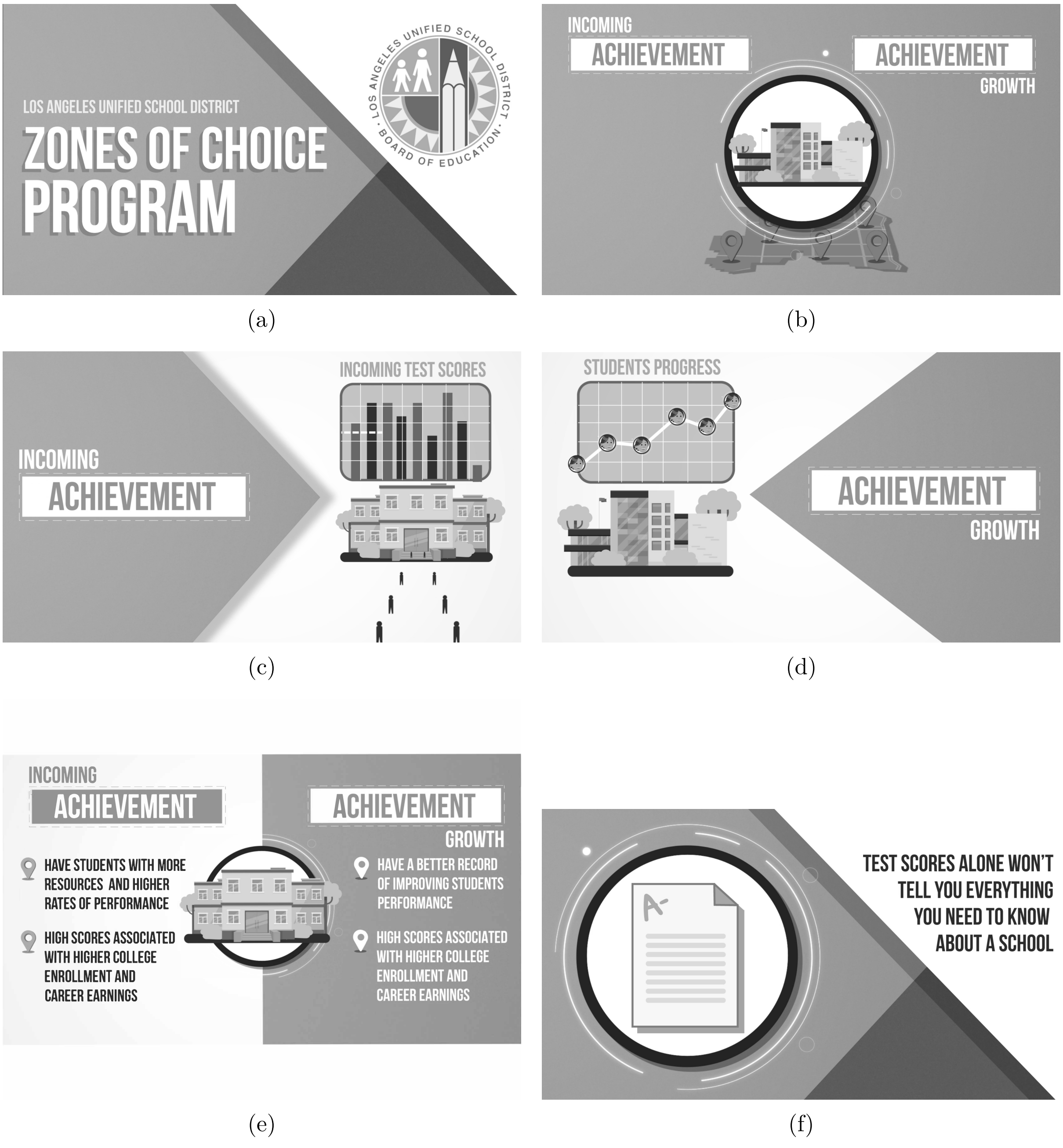
The survey findings suggest that when selecting schools within their local areas, families often underestimate the schools' actual quality and overestimate the student body's perceived quality. When information about both peer and school quality is made widely available, families tend to prefer higher-quality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

Social interactions and spillovers are important mediators governing new market-level consensus of desirable schools. This is the first paper to show the relevance of social interactions for preference formation discussed in nascent theoretical literature (Harless and Manjunath, 2015, Immorlica et al., 2020, Maxey, 2021), providing experimental evidence about a network-based externality in preference formation, which is distinct from the commonly studied preference for peers (Abdulkadiroğlu et al., 2020, Allende et al., 2019, Rothstein, 2006).

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

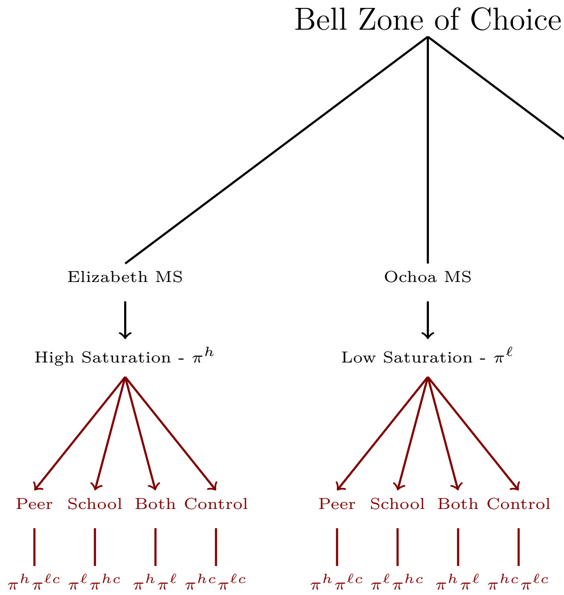
This paper advances what we know about parents' beliefs and preferences but is limited along certain dimensions. The results speak to short-run partial equilibrium effects, providing, at best, suggestive evidence for potential supply-side responses. Moreover, the findings are silent about how changes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

Figure 1: Video Frames



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

Figure 2: Assignment to Treatment



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

Figure 3: Treatment Letter Example: Bell Zone of Choice

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

Bell Zone of Choice

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

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

Therefore, a school's observed quality is a combination of both their students' **incoming achievement** and the **achievement growth** they obtain while at the school. Some parents may prefer schools with high incoming achievement, and others may prefer schools with high achievement growth. The table below provides each school's district-wide ranking.

Incoming Achievement

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

Achievement Growth

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

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

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

*Schools' Incoming Achievement and Achievement Growth are provided in percentiles. For example, if a school has a incoming achievement of 55, this means that the average test scores of its incoming students are better than 55 percent of other high schools in LAUSD. Similarly, if achievement growth is 75, then the school's ability to improve test scores is better than 75 percent of high schools in LAUSD.

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

Zona de Opción Bell

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

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

Por lo tanto, la calidad observada de una escuela es una combinación tanto del **rendimiento entrante** de sus estudiantes como del **crecimiento de logros** o **crecimiento del rendimiento** que obtienen mientras están en la escuela. Algunos padres pueden preferir escuelas con alto rendimiento entrante, y otros pueden preferir escuelas con alto crecimiento de logros. A continuación, proporcionamos la clasificación de cada escuela comparado a todas escuelas en el distrito.

Rendimiento Entrante

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

Crecimiento de logros

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

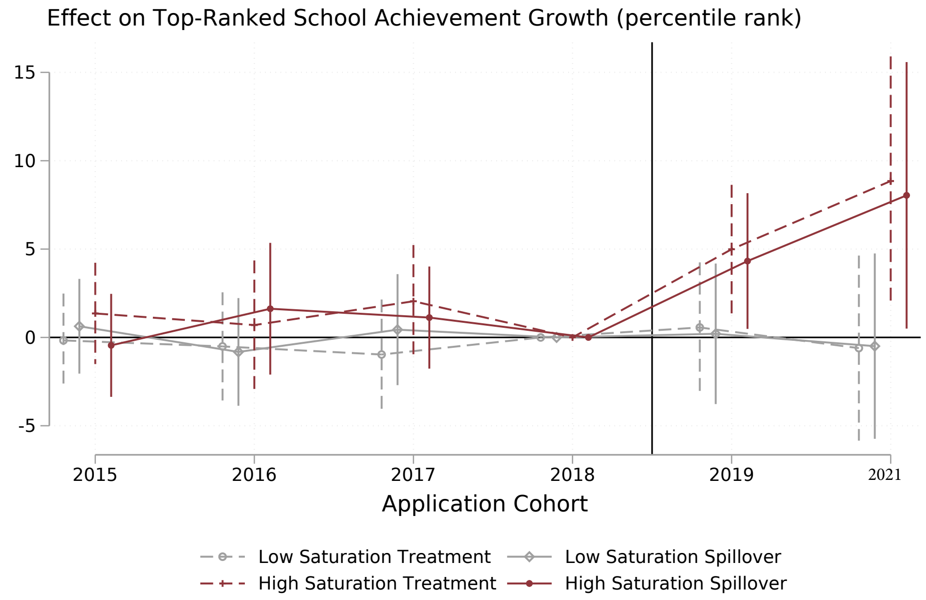
Esperamos que utilice esta información al elegir la escuela adecuada para su estudiante.

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

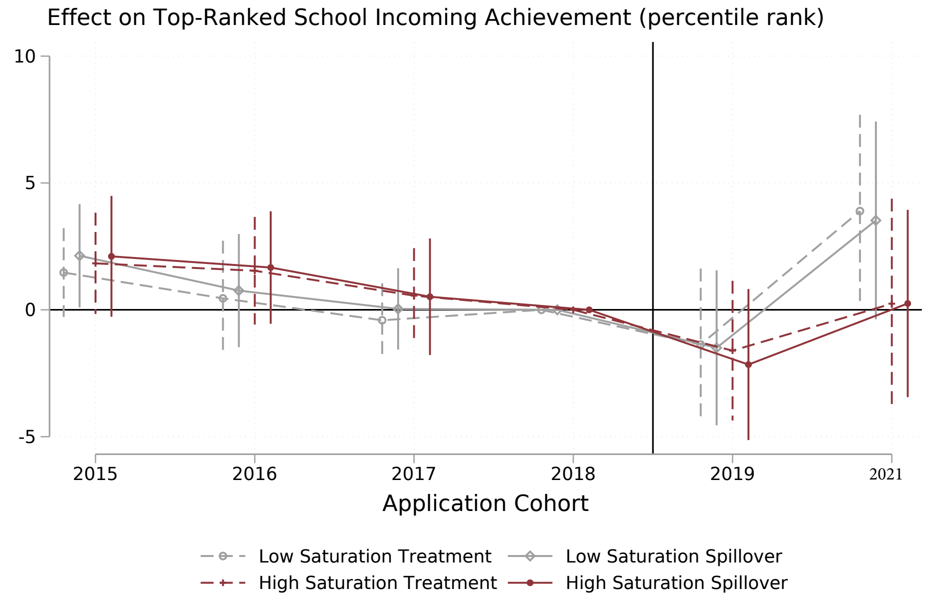
*El rendimiento entrante y el crecimiento del logros de las escuelas se proporcionan en percentiles. Por ejemplo, si una escuela tiene un rendimiento entrante de 55, esto significa que los puntajes promedio de las pruebas de sus estudiantes entrantes son mejores que el 55 porciento de otras escuelas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

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

Figure 4: Difference-in-Difference Estimates



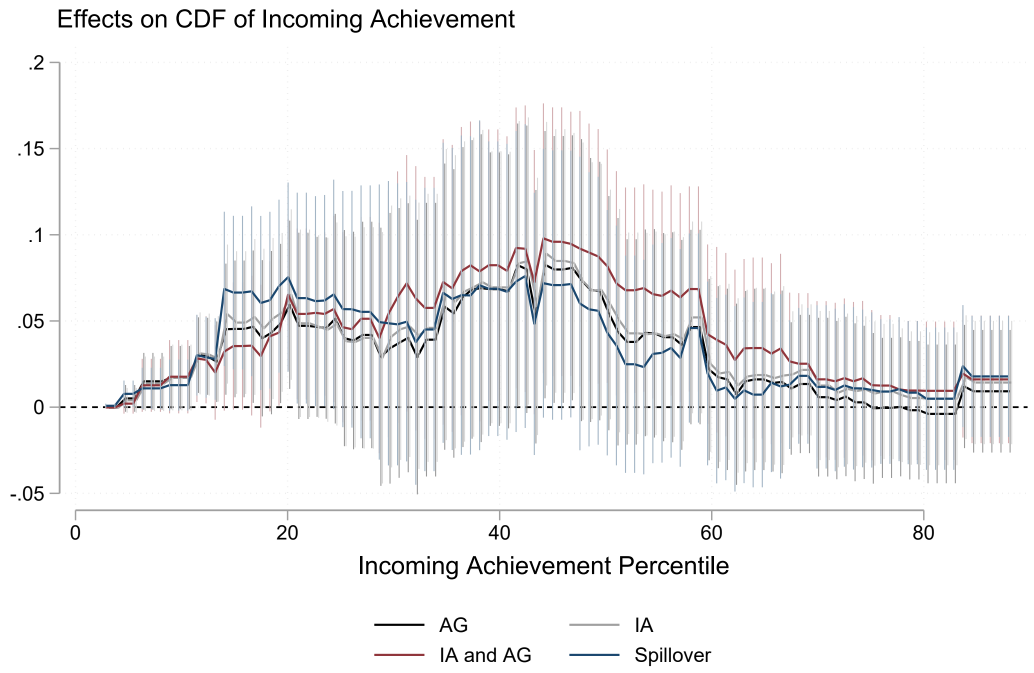
(a) Impacts on Most-Preferred Achievement Growth



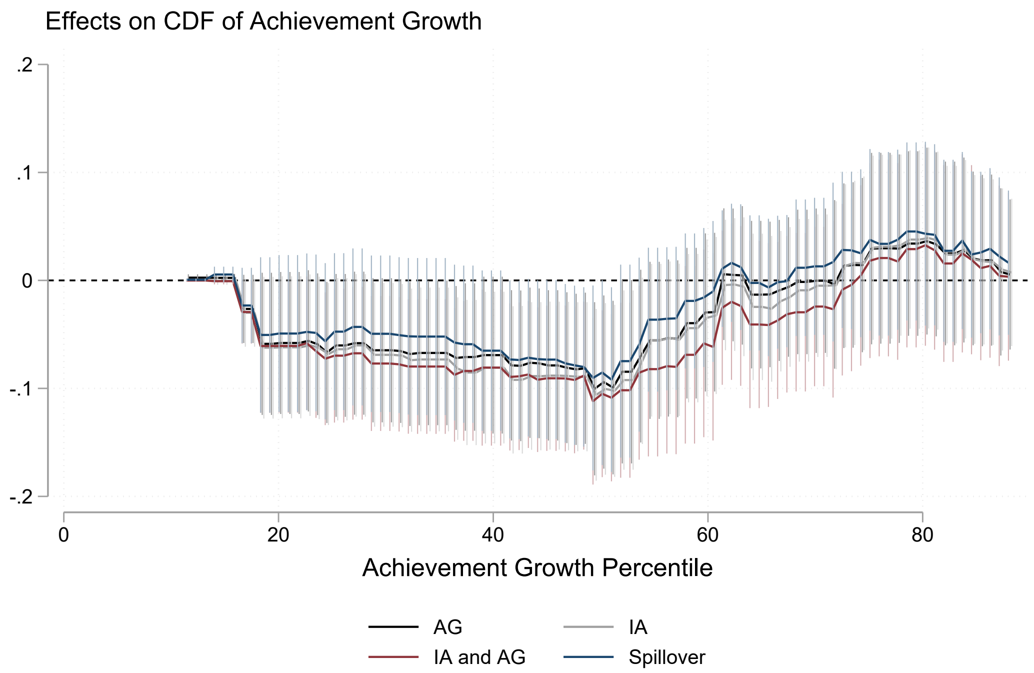
(b) Impacts on Most-Preferred Incoming Achievement

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

Figure 5: Distributional Estimates



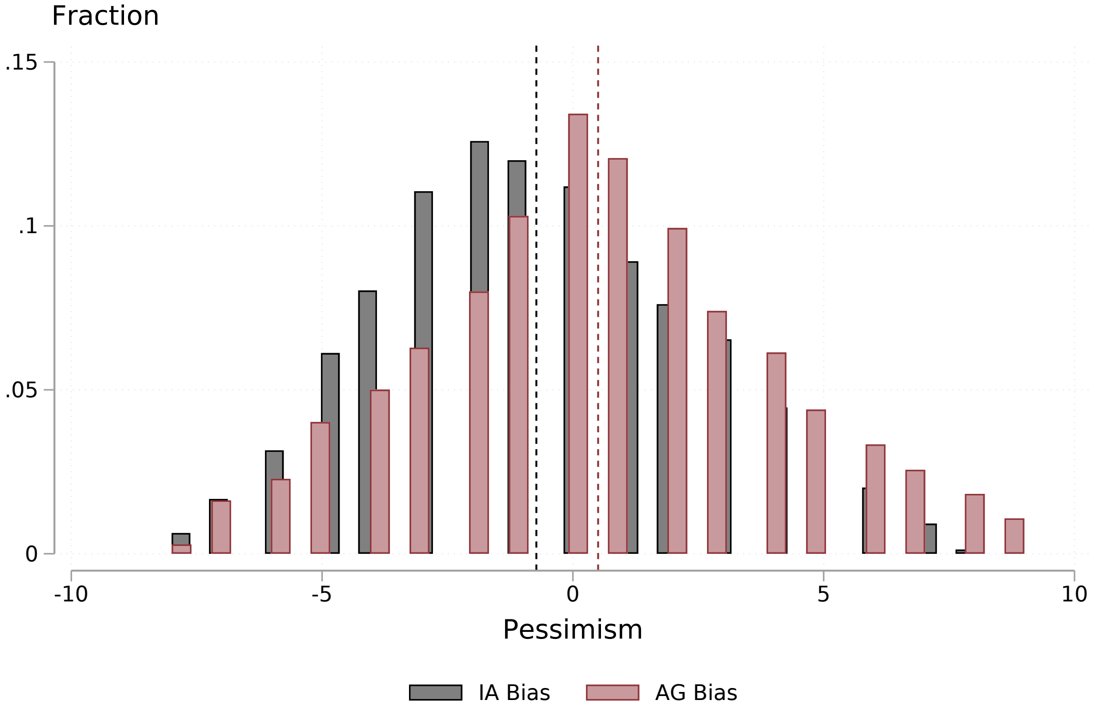
(a) Incoming Achievement



(b) Achievement Growth

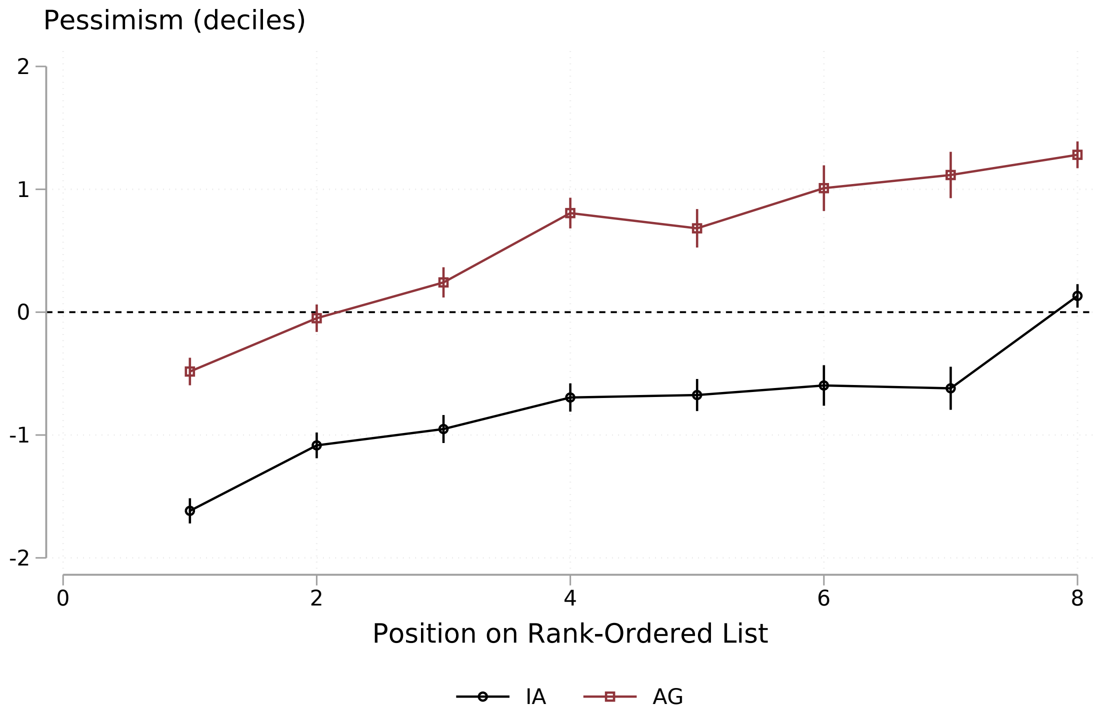
Notes: This figure displays distribution regression estimates across the incoming achievement or achievement growth distribution. The sample stacks both experimental waves and includes experiment-year fixed effects, treatment group fixed effects, student baseline controls, and treatment group indicators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

Figure 6: IA and AG Pessimism Distribution



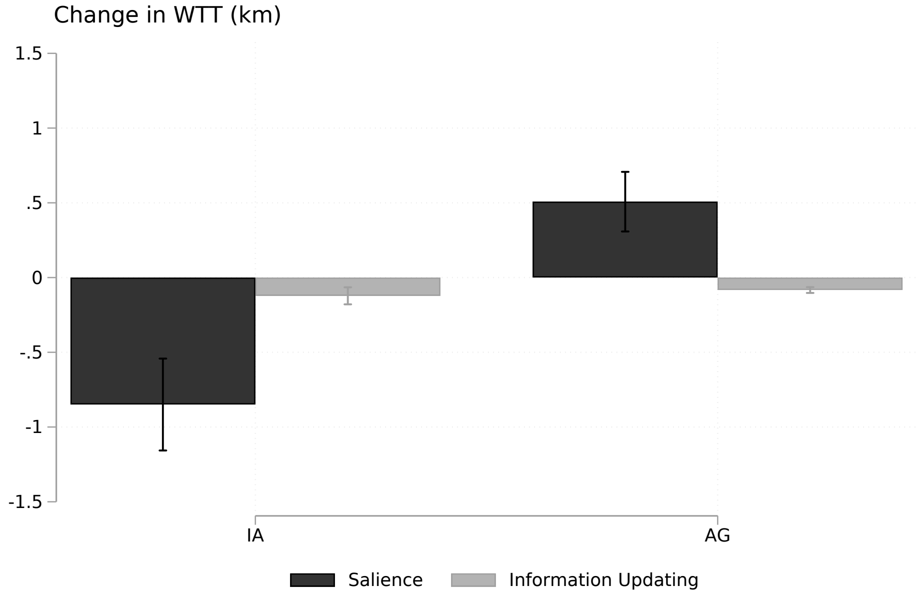
Notes: This figure reports the pessimism distribution for incoming achievement (IA) and achievement growth (AG). Beliefs are collected in terms of deciles, and pessimism is calculated by the difference in between the elicited belief and the estimated belief. Dashed lines correspond to mean pessimism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

Figure 7: Pessimism across the Rank-Ordered List

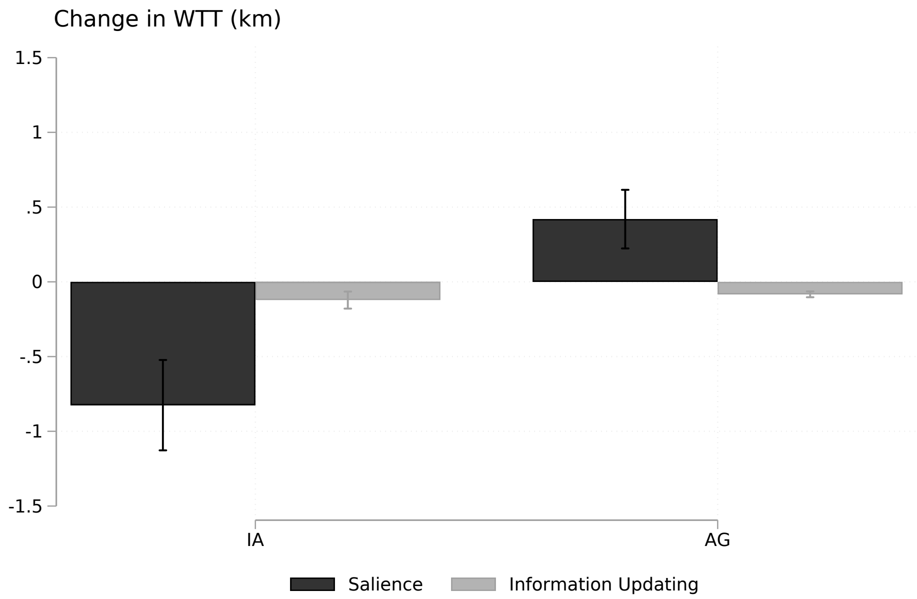


Notes: This figure reports mean pessimism for incoming achievement (IA) and achievement growth (AG) at various points of parents' rank-ordered lists. Points corresponds to means, and 95% confidence intervals are represented by the bars.

Figure 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 from an information-specific model, 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 IA weights among those receiving IA only. Similarly, the next two bars are decomposition estimates of AG weight impacts among those receiving AG only. Black bars correspond to the salience component and grey bars correspond to the information updating component. In Panel (b), the treatment status for each set of bars corresponds to the spillover group. The underlying parameters used for the decomposition, bias variances, and correlations are jointly estimated via maximum likelihood. These estimates are used in combination with control group utility weight estimates to calculate decomposition factors. Standard errors are robust, clustered at the school level, and estimated via the delta method.

Table 1: ZOC and Non-ZOC Differences

	Non-ZOC (1)	ZOC (2)	Difference (3)
Reading Scores	0.102	-0.116	-0.218
			(0.011)
Math Scores	0.106	-0.113	-0.220
			(0.011)
College	0.182	0.064	-0.118
			(0.003)
Migrant	0.095	0.065	-0.029
			(0.003)
Female	0.490	0.483	-0.006
			(0.005)
Poverty	0.710	0.940	0.229
			(0.004)
Special Education	0.095	0.120	0.025
			(0.003)
English Learners	0.103	0.118	0.015
			(0.003)
Black	0.104	0.033	-0.071
			(0.003)
Hispanic	0.635	0.904	0.270
			(0.004)
White	0.155	0.016	-0.139
			(0.003)
N	23,723	13,015	

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

Table 2: Difference-in-Difference Estimates on School Attributes

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

Notes: This table reports difference-in-difference estimates of the effect of different treatments on row variables. These estimates come from regressions of most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with treatment group indicators interacted with event-time indicators. All estimates are identified by comparisons of changes between students at treated schools with pure control schools. Standard errors are robust, clustered at the school level, and reported in parentheses. Randomization inference p-values are reported in brackets underneath each standard error based on 400 placebo treatment statuses for both school and individual-level treatments.

Table 3: Willingness to travel estimates

	WTT Estimates		P-value
	IA	AG	
Treatment			
Untreated	0.392***	0.658***	0.017
	(0.093)	(0.078)	
Information: IA	-0.972***	0.474***	0.000
	(0.174)	(0.104)	
Information: AG	-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 6. Column (1) corresponds to estimates of IA utility weights and Column (2) corresponds to estimates of AG utility weights. Rows labeled as Untreated correspond to utility weight estimates for families in the pure control group. Information:IA, Information:AG, and Information:Both correspond to directly receiving IA, AG, or Both types of information, respectively, and represent changes in estimated willingness to travel for the column attribute. Each cell, except for distance estimates, report estimates in willingness to travel units. These are calculated by dividing the unreported utility weight estimate (or impact) by the corresponding distance disutility estimate. Column (3) reports the p-value of a test of equality of estimates in Column (1) and (2) within a row. The p-value reported in the bottom rows corresponds to a test with the null hypothesis that all utility weight impacts within a given column are equal. Standard errors are reported in parentheses and estimated via the delta method.

Table 4: Effects on Cognitive and Non-Cognitive Outcomes

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

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

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Online Appendix for:

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

Christopher Campos

March 2024

Table of Contents

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. 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	

A.3 Experimental Balance

Table A.2: Saturation School-Level Balance

	Control (1)	Low - Control (2)	High - Control (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.

B Model Details

B.1 Single Attribute Model

The starting point for the decomposition discussed in Section 3 is a discrete choice model with perfect information. To simplify exposition, let's assume we are interested in the impacts of information on a single attribute. In a model with perfect information, we can summarize the intervention's effects with a change in families' utility weights. The indirect utility of student i enrolling in school j is

$$U_{ij} = \gamma X_j + \beta X_j \times T_i - \lambda d_{ij} + \varepsilon_{ij}$$

where T_i corresponds to a treatment indicator, X_j is the attribute of school j, d_{ij} is distance to school j, and ε_{ij} is any remaining unobserved preference heterogeneity. The willingness to travel for the control group is

$$WTT_0 = \frac{\gamma}{\lambda},$$

and the willingness to travel for the treatment group is

$$WTT_1 = \frac{\gamma + \beta}{\lambda},$$

so that the change in the willingness to travel induced by the information is

$$\Delta WTT = \frac{\beta}{\lambda}.$$

In a model with perfect information, the change in the willingness to travel only comes from families re-prioritizing the importance of X_j after receiving information. One can interpret this as a change in their preferences or what I refer to as a salience effect.

In a model with imperfect information, families that receive information make decisions using the information they received and families without information make decisions using their beliefs. Beliefs are modeled as proportional shifts away from their true value, with shifts varying at the individual and school level:

$$\tilde{X}_{ji} = (1 + b_i) X_j$$

In this model, families in the control group effectively assign weight $\tilde{\gamma}_i = \gamma(1 + b_i)$ to each X_j, so that the underlying indirect utility model is

$$U_{ij} = \tilde{\gamma}_i X_j + \tilde{\beta}_i X_j \times T_i - \lambda d_{ij} + \varepsilon_{ij}.$$

The parameterization above allows for different groups to have different willingness to travel for attribute X. In particular, the average willingness to travel for the two groups are

$$E[WTT_{i0}] = \frac{\gamma(1 + \mu_X)}{\lambda} \tag{10}$$

$$E[WTT_{i1}] = \frac{\gamma + \beta}{\lambda}, \tag{11}$$

so that the average change in the willingness to travel induced by the information is

$$\Delta \tilde{WTT} = \frac{\beta - \gamma \mu_X}{\lambda}.$$

Because researchers estimate models using X_j, this means that the average change in willingness to travel estimates nest both the salience effect and the information effect. With survey data, one can pin down the portion of the mean change due to information and the residual is allocated to salience or corresponds to the actual change in willingness to travel.

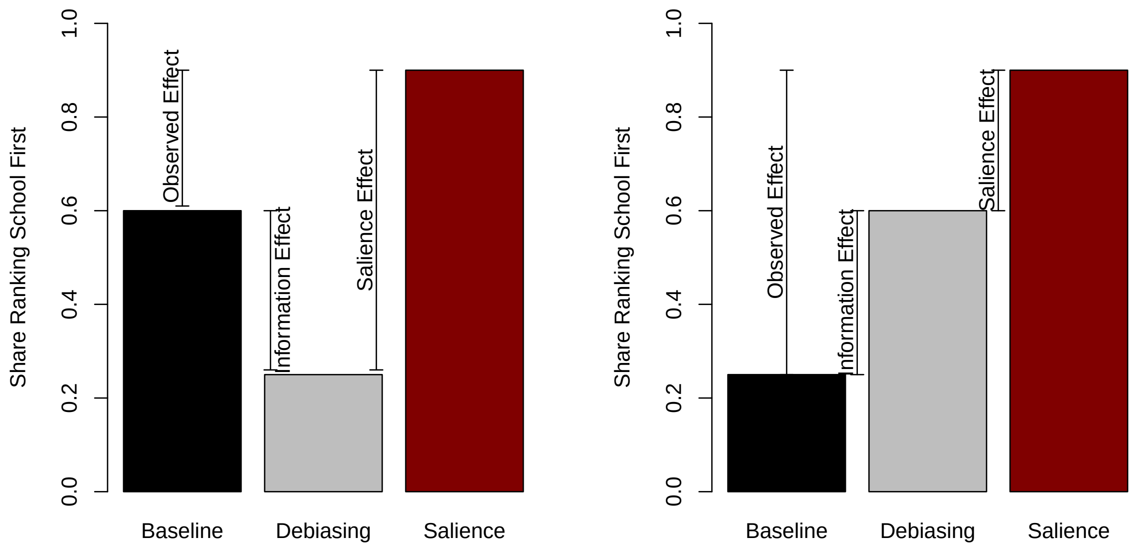
B.2 Intuition for Decomposition

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 B.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 B.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.

B.3 Multiple Attribute Model

The multiple attribute extension is similar to the single attribute model, but also considers how information about only one attribute can potentially influence beliefs and salience about another attribute. Families make decisions using their beliefs about X_{1j} and X_{2j}. One way to model beliefs is to allow families to have idiosyncratic quality-specific biases, $\tilde{X}_{1ji} = (1+b_{1i})X_{1j}$ and $\tilde{X}_{2ji} = (1+b_{2i})Q_{2j}$. I assume that beliefs are jointly normal,

$$\begin{pmatrix} b_{1i} \\ b_{2i} \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} \mu_1 \\ \mu_2 \end{pmatrix}, \begin{pmatrix} \sigma_1^2 & \rho\sigma_1\sigma_2 \\ \rho\sigma_1\sigma_2 & \sigma_2^2 \end{pmatrix} \right),$$

with ρ governing the correlation of biases and σ_1 and σ_2 the respective standard deviations.

In a setting with this structure, indirect utility of student i enrolling in school j is

$$U_{ij} = \gamma_{i1}X_{1j} + \gamma_{i2}X_{2j} + \sum_{t\in\{1,2,B\}}\sum_{k\in\{1,2\}}\beta_{ikt}X_{kj} \times T_{it} - \lambda d_{ij} + \varepsilon_{ij}$$

where T_{it} is an indicator equal to one if individual i is in treatment group t and the β_{kt} correspond to changes in utility weights for attribute k among those in treatment group t.

The willingness to travel for the attributes now depends on the different treatment statuses. A key difference from the single attribute model is that information about one attribute affects beliefs about another through a correlated beliefs channel. An important assumption made in this decomposition is a perfect compliance assumption, meaning that individuals receiving information about attribute X update so that their $b_{iX} = 0$. The willingness to travel estimands are the following:

$$E[WTT_{i10}] = \frac{\gamma(1+\mu_1)}{\lambda} \tag{12}$$

$$E[WTT_{i11}] \equiv E[WTT_{i11}|b_{i1} = 0] = \frac{\gamma + \beta_{11}}{\lambda} \tag{13}$$

$$E[WTT_{i12}] \equiv E[WTT_{i12}|b_{i2} = 0] = \frac{\gamma(1 + \mu_1 - \rho\frac{\sigma_1}{\sigma_2}\mu_2)}{\lambda} + \frac{\beta_{12}(1 + \mu_1 - \rho\frac{\sigma_1}{\sigma_2)}\mu_2}{\lambda} \tag{14}$$

$$E[WTT_{i1B}] \equiv E[WTT_{i1B}|b_{i1} = 0, b_{i2} = 0] = \frac{\gamma + \beta_{1B}}{\lambda}. \tag{15}$$

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

$$E[\Delta WTT_{i12}] = \frac{\beta_{12}(1+\mu_1)}{\lambda} - \frac{(\gamma + \beta_{12})\rho\frac{\sigma_1}{\sigma_2}\mu_2}{\lambda}. \tag{16}$$

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 beliefs are positively correlated and families overestimate school quality, then the second term offsets the amplification in the first term. Overall, the factors influencing the effects of one attribute on another depend on the presence of salience effects and the degree of imperfect information at baseline. 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 the primary effects of interest.

C 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 C.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 C.2). The second is additional survey evidence not reported in the main paper (see Table C.3 and Figure C.1). The third corresponds to descriptive evidence about belief correlates, including both student-level attributes and researcher-generated measures of quality (see Table C.4-**??** and Figure C.2).

C.1 Survey Questions

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

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

1. What is your relationship to the student?
    - Father
    - Mother
    - Grandparent
    - Guardian

2. Has anyone mentioned the Zones of choice to you before?
    - Yes
    - No

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

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

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

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

5. Rank the following school characteristics in terms of importance (1-7), where 1 is the most important
   - Test score improvement
   - Performance of other students
   - Safety
   - Reputation of teachers
   - Distance from home
   - Available sport offerings

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

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

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

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

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

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

9. For the next table, please give each school a rating between 0-10, 10-20, $\cdots$, 90-100 according to your beliefs about their ability in terms of (1) Incoming Achievement and (2) Achievement Growth.
10. Please rank the schools as if you were submitting the application today. Note there are K schools you can choose from, so rank your most preferred as 1 and the least preferred as K.

C.2 Pilot Details

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

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

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

Table C.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)

C.3 Additional Survey Evidence

Table C.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

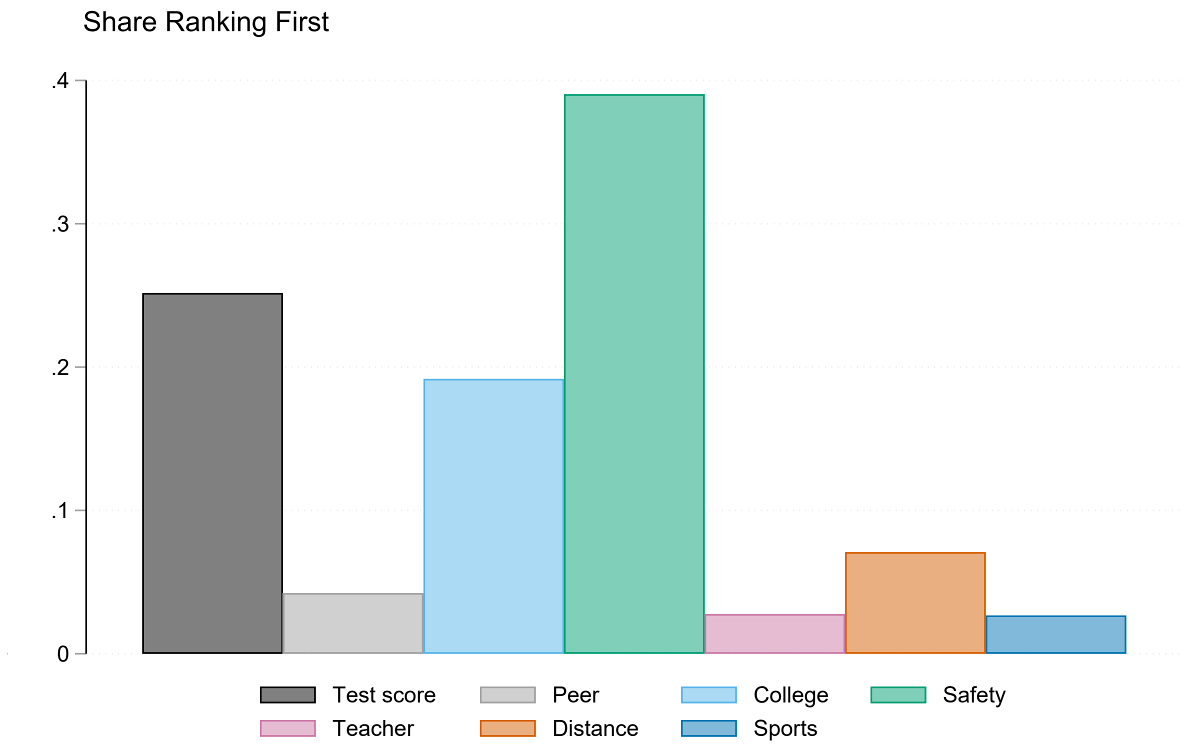
Notes: This table reports estimates from regressions of each row variable on indicators for survey completion status. Partial indicates that the respondent did not finish the survey, usually corresponding to missing beliefs information, and complete corresponds to respondents who completed the survey.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

Table C.3: Survey Responses

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

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

Figure C.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 C.

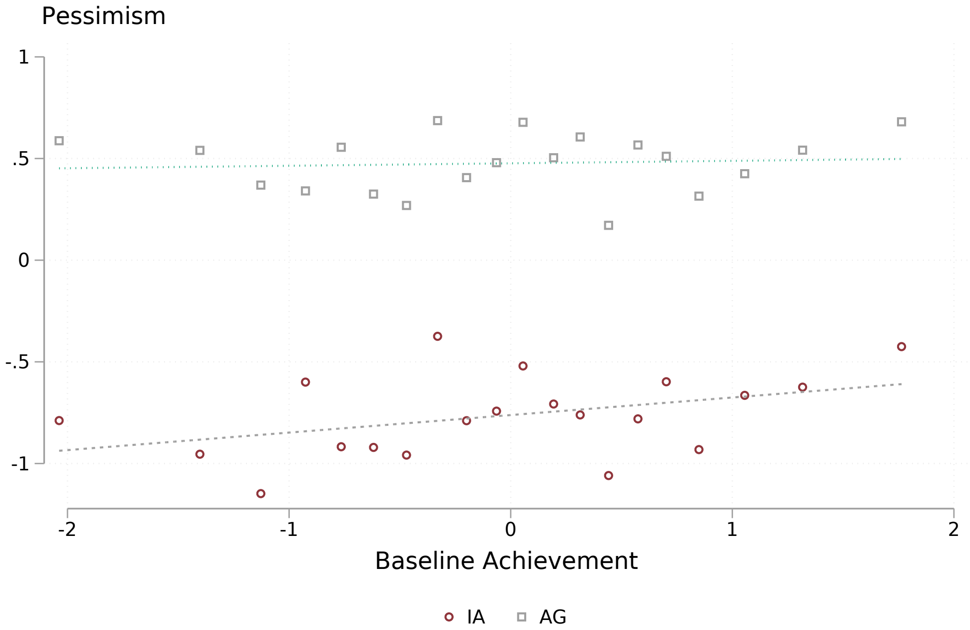
Table C.4: IA and AG Pessimism Correlation with Student Characteristics for Top-Ranked School

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	

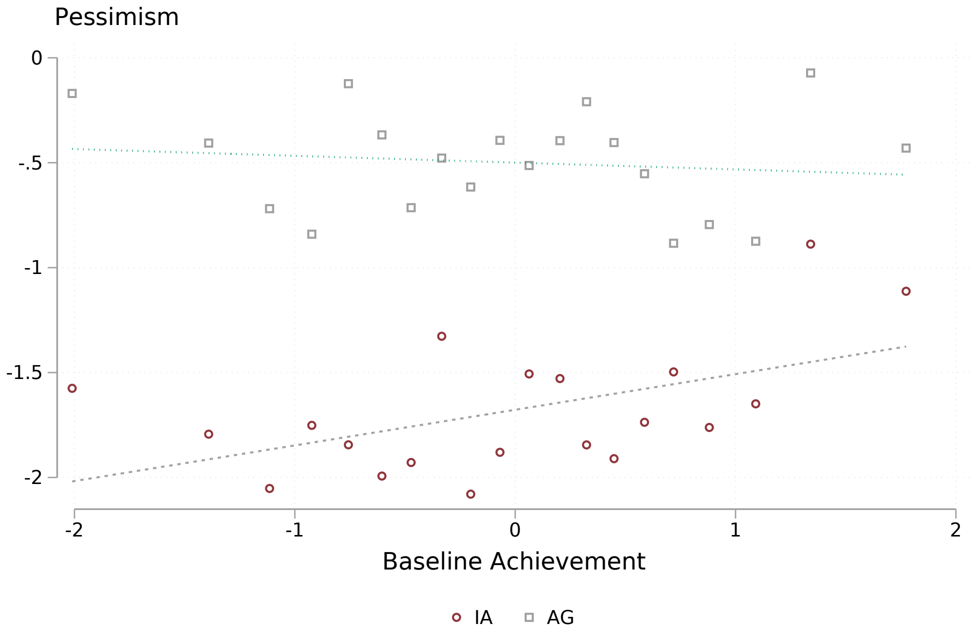
Notes: This table reports univariate and multivariate correlations between student-level IA and AG pessimism measures and student-level covariates. Column 1 and Column 2 consider IA pessimism and Column 3 and Column 4 consider AG pessimism. Odd-numbered columns consider bivariate regressions of the pessimism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

Figure C.2: Pessimism-Achievement Relationship

(a) All Options on Rank-Ordered List



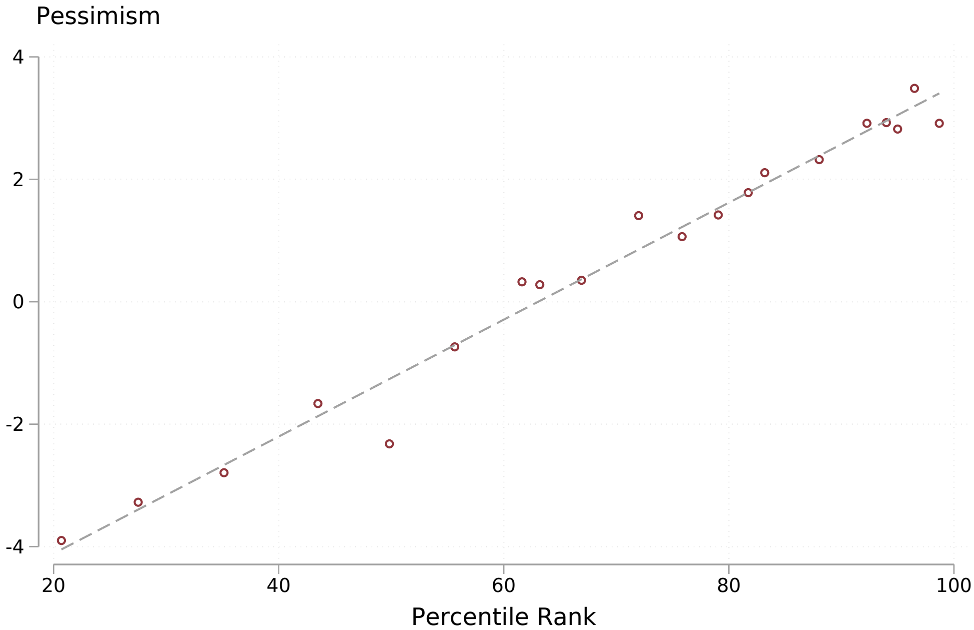
(b) Only Top-Ranked Option on Rank-Ordered List



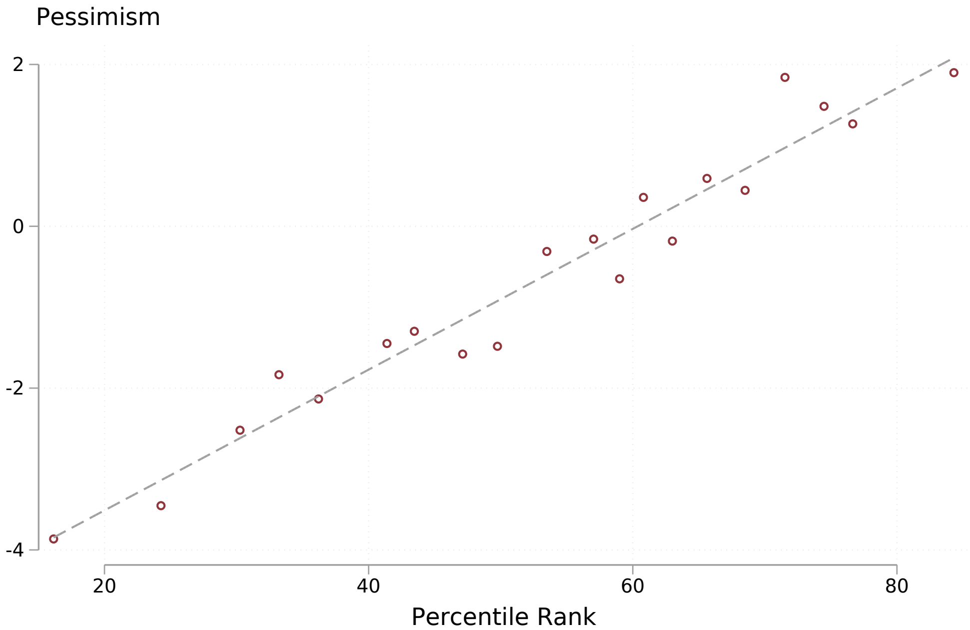
Notes: This figure reports bivariate-binned scatter plots of the pessimism-achievement relationship. Panel (a) reports the relationship across all options contained on the rank-ordered list, while Panel (b) reports the relationship only among the top-ranked option of applicants' rank-ordered lists.

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

(a) Achievement Growth



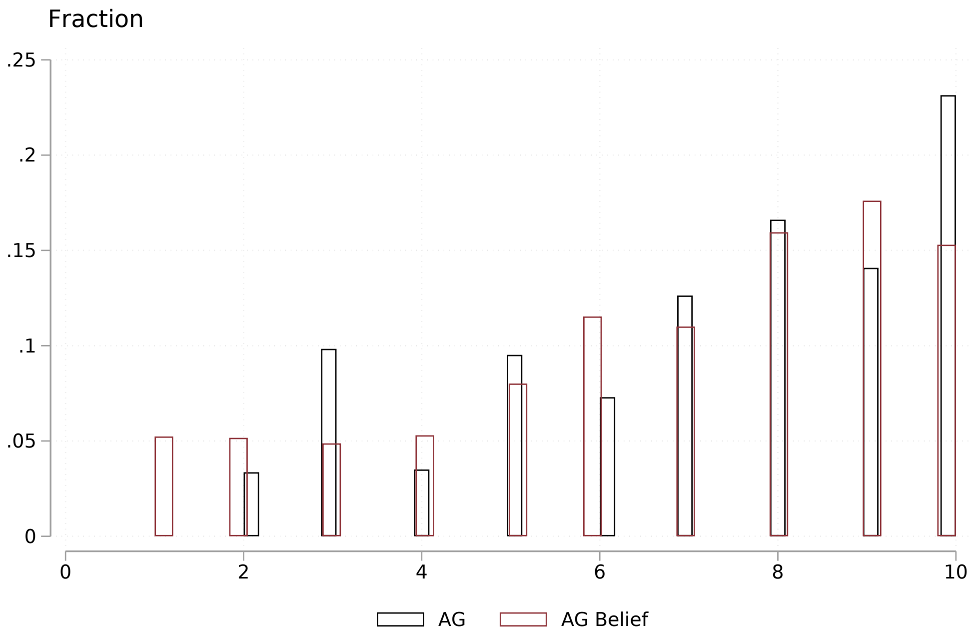
(b) Incoming Achievement



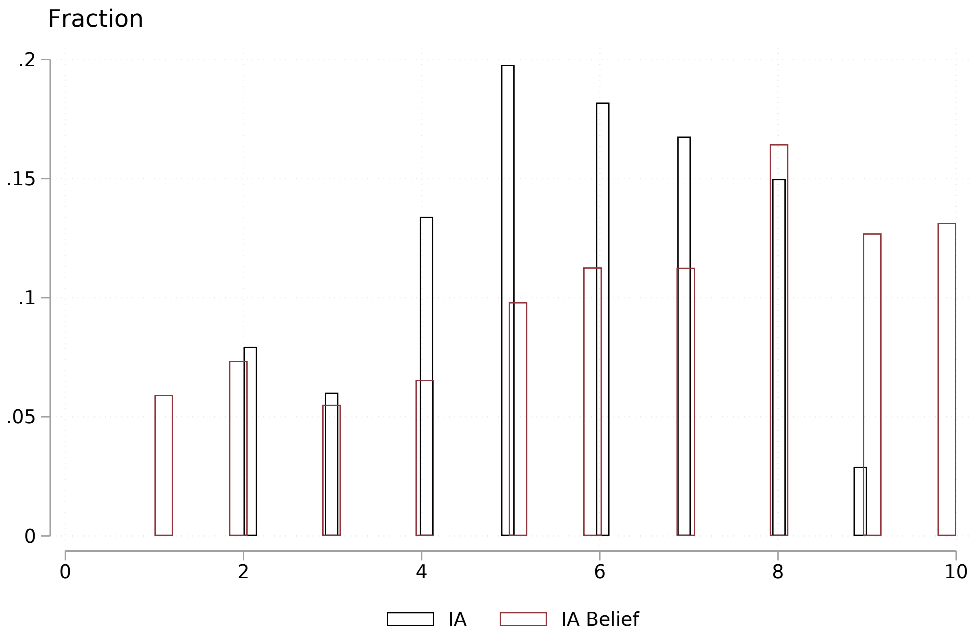
Notes: This figure reports bivariate-binned scatter plots of the pessimism-achievement relationship. Panel (a) reports the relationship across all options contained on the rank-ordered list, while Panel (b) reports the relationship only among the top-ranked option of applicants' rank-ordered lists.

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

(a) Achievement Growth



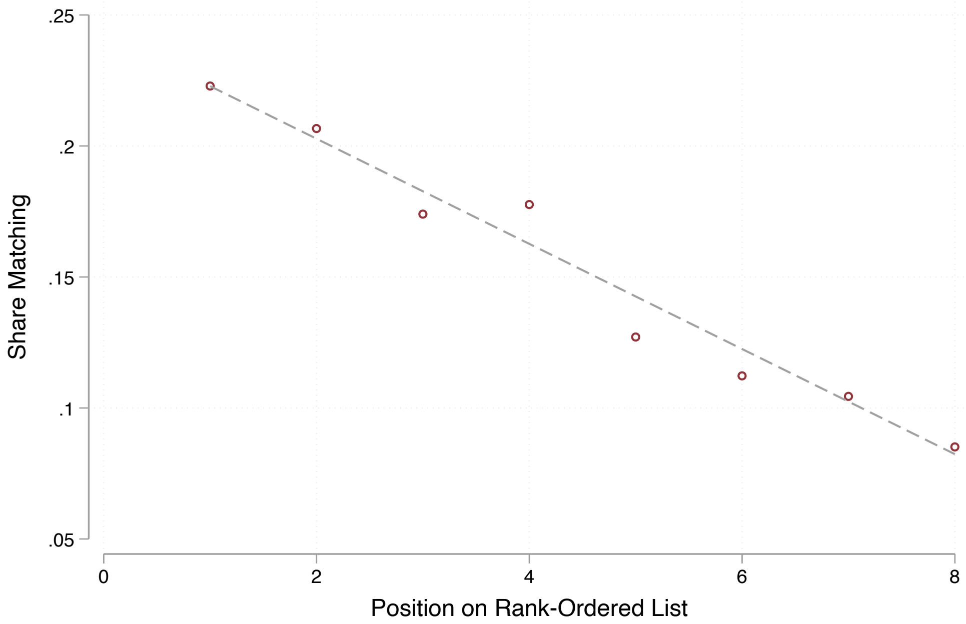
(b) Incoming Achievement



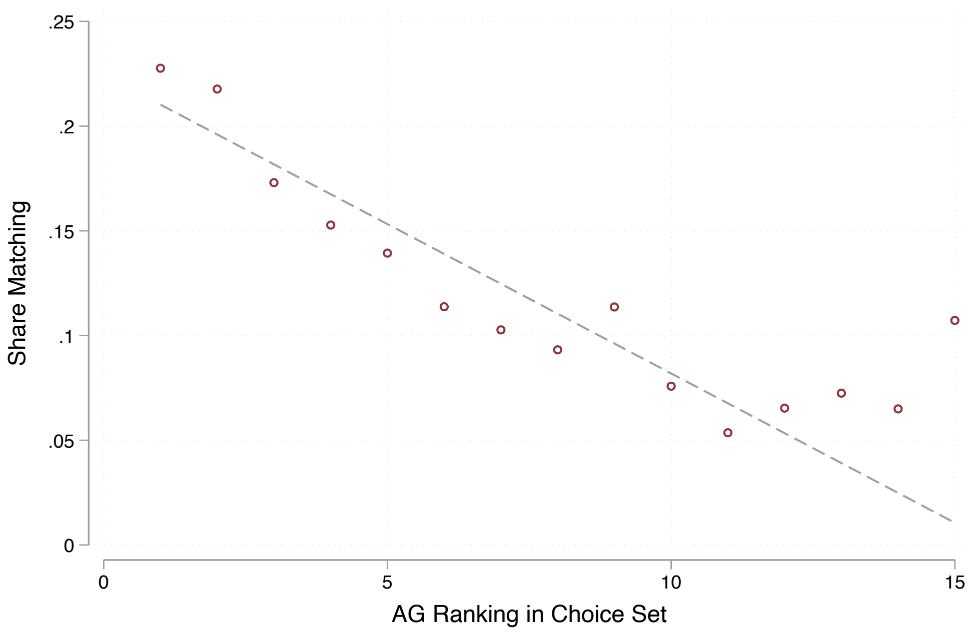
Notes: This figure reports option-specific distributions of AG (IA) deciles and AG (IA) beliefs. If applicants' decile beliefs were perfectly on target, then their belief distribution would perfectly overlap with the decile distribution.

Figure C.5: Choice Relevance of AG Biases

(a) By Position on the Rank-ordered List



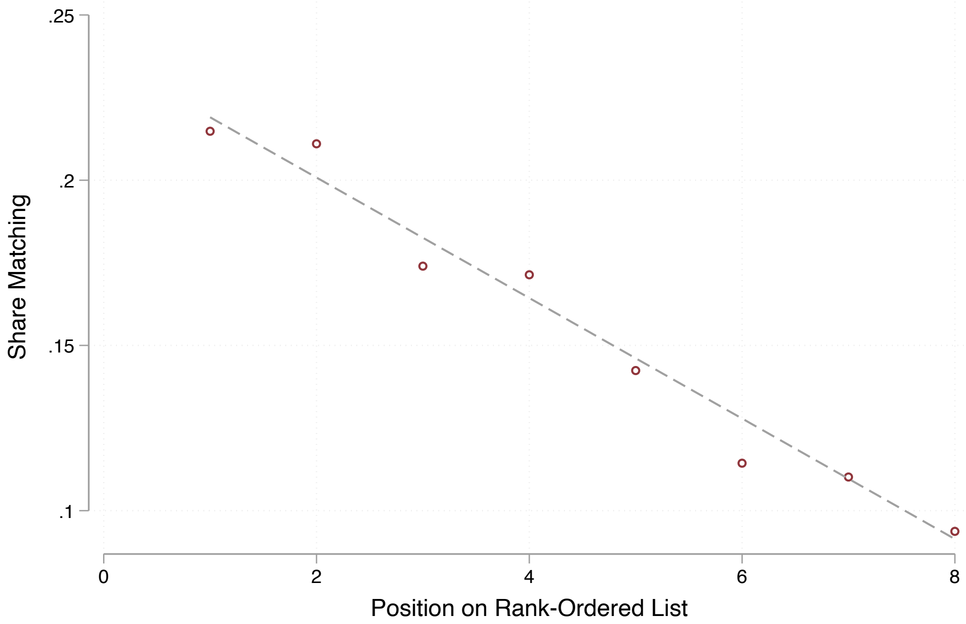
(b) By Option's Actual Ranking



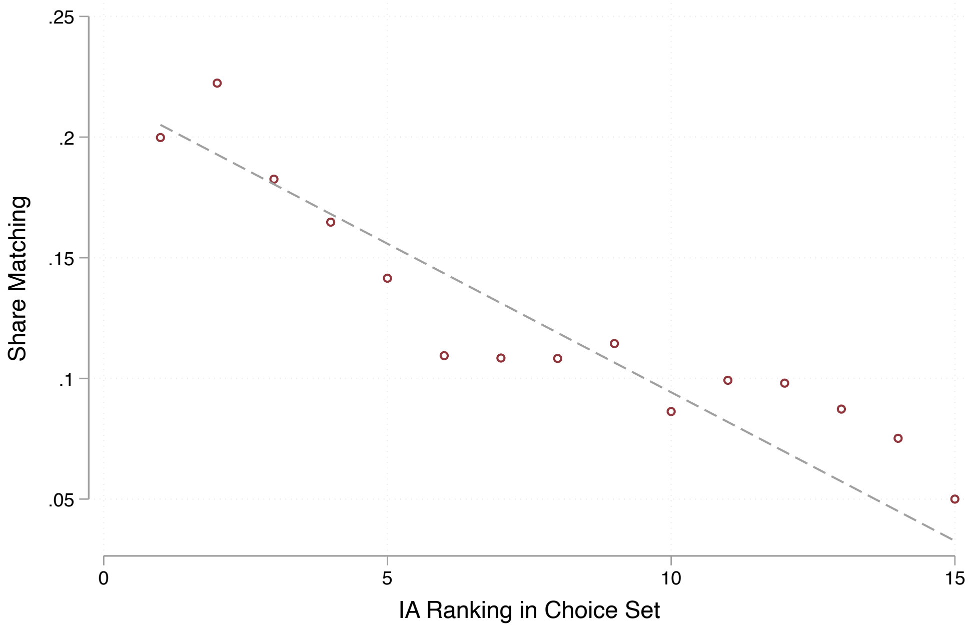
Notes: This figure reports the share of applicants whose AG relative belief ranking for their kth ranked option matches the actual belief ranking for that option. Panel (a) reports that by position on the applicant's rank-ordered list and Panel (b) reports that by the actual ranking for that option.

Figure C.6: Choice Relevance of IA Biases

(a) By Position on the Rank-ordered List



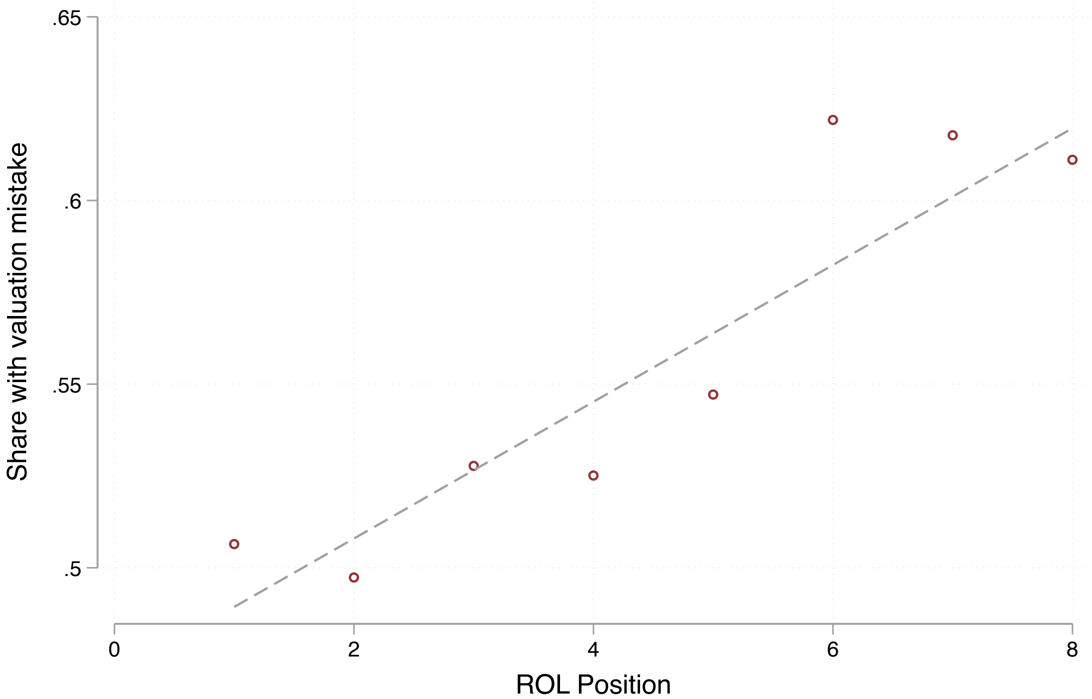
(b) By Option's Actual Ranking



Notes: This figure reports the share of applicants whose IA relative belief ranking for their kth ranked option matches the actual belief ranking for that option. 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.

C.4 Application Mistakes

Figure C.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-generate quality separately. I then take random EVT1 draws to capture unobserved preference heterogeneity, and combined with estimated systematic components of utility, I generate new rank-ordered lists. 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.

D 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

$$Y_{ij} = \mu_j + a_i \tag{17}$$

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

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

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

$$Y_i = \mu_0 + \sum_j \beta_j D_{ij} + \gamma' X_i + u_i \tag{18}$$

which is the canonical causal value-added model considered in the literature (Campos and Kearns, 2023).

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

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

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

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

D.1 VAM Validation

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

$$A_i = \xi + \phi \widehat{A}_i + \sum_\ell \kappa_\ell Z_{i\ell} + \boldsymbol{X}_i' \delta + \varepsilon_i \tag{19}$$

$$\widehat{A}_i = \psi + \sum_\ell \pi_\ell Z_{i\ell} + \boldsymbol{X}_i' \xi + e_i. \tag{20}$$

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

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

Table D.1: Forecast Bias and Overidentification Tests

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

Notes: This table reports the results of lottery-based tests for bias in estimates of school effectiveness. The sample is restricted to students in the baseline sample who applied to an oversubscribed school within a school choice zone. Column (1) measures school effectiveness as the school mean outcome, Column (2) uses time-invariant value-added estimates. The forecast coefficients and overidentification tests reported in Columns (1)–(2) come from two-stage least squares regressions of test scores on OLS-fitted values estimated separately, instrumenting OLS-fitted values with school-cohort-specific lottery offer indicators, controlling for baseline characteristics.

D.2 School and Peer Quality Measures

School average achievement follows from Equation 18

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

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

$$Q_j^S = \text{int}\left(\frac{\text{rank}(\hat{\alpha}_j)}{J} \times 100\right) \tag{21}$$

$$Q_j^P = \text{int}\left(\frac{\text{rank}(\hat{\beta}'\bar{X}_j)}{J} \times 100\right) \tag{22}$$

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

D.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

$$\alpha_j^* = \alpha_j + \delta\bar{X}_j.$$

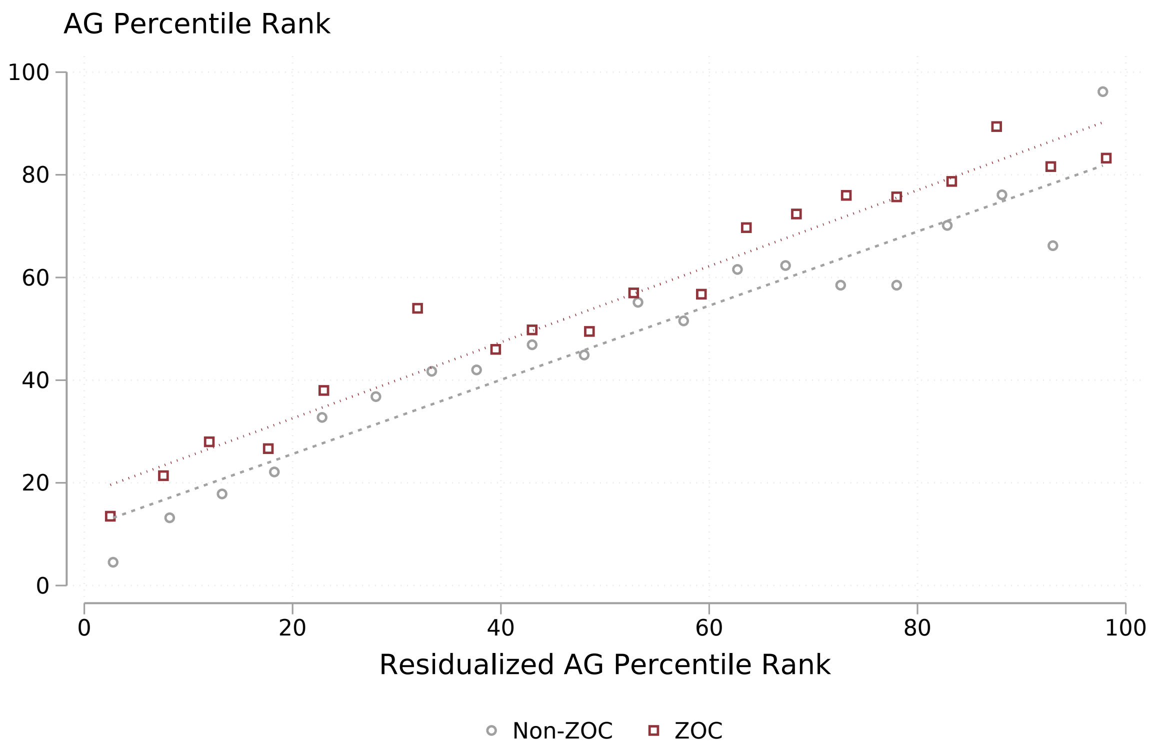
We can assess this possibility by relating estimated values of α_j^* to $\bar{X}_j$. Appendix Table D.2 demonstrates that estimated school quality is unrelated to essentially all of the observables in the data. In particular, lagged achievement is not a strong predictor of school quality both unconditionally 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 D.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 D.2: Relationship between α_j and student observables

	(1)	(2)	(3)	(4)
	α_j	α_j	α_j	α_j
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.

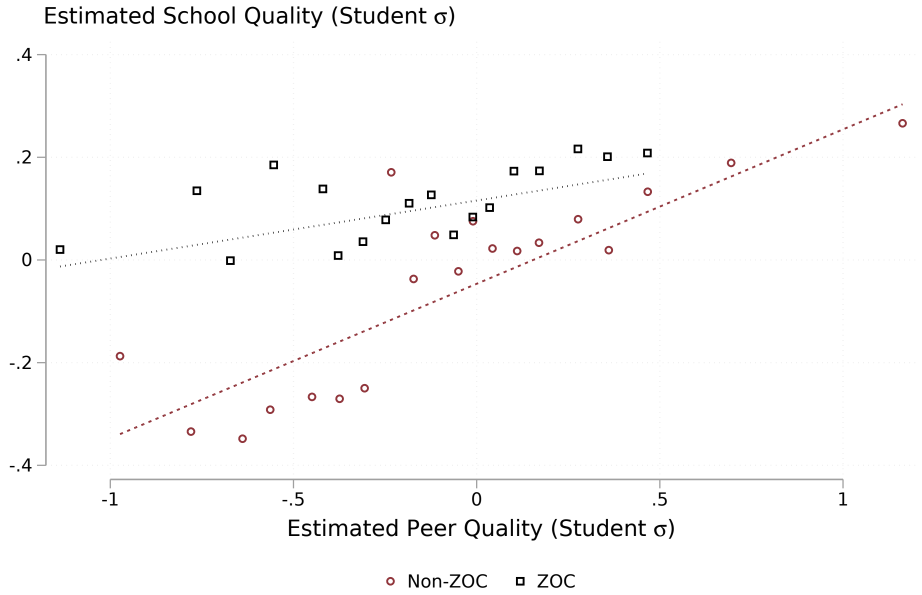
Figure D.1: Rank-rank Correlation Between Estimated School Quality and Regression-Adjusted School Quality



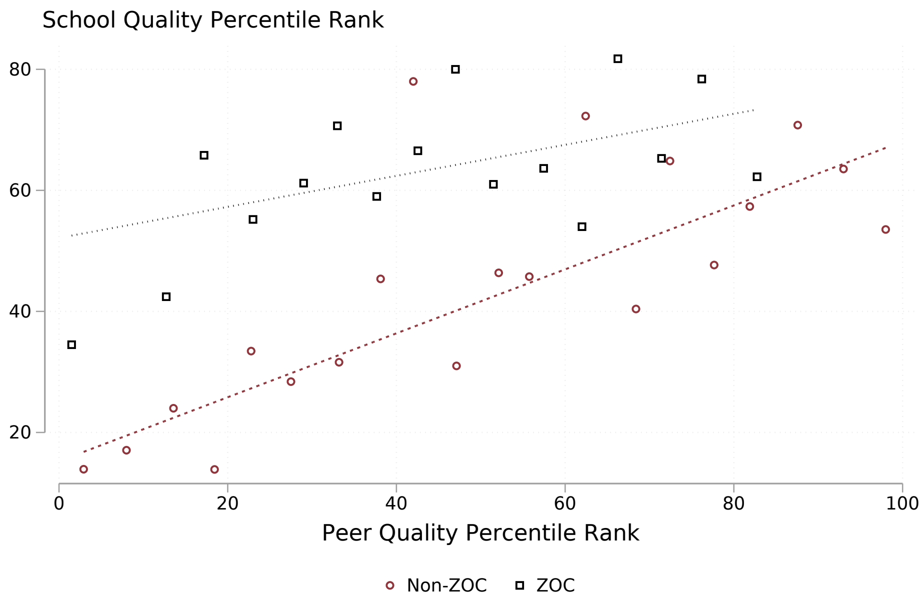
Notes: This figure reports the rank-rank relationship between estimated school quality used in the intervention and an alternative that regression adjusts for observable school-level attributes. The rank-rank relationship is reported separately for ZOC and non-ZOC schools; the differences are not statistically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

D.4 Summary Statistics

Figure D.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.

E 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, I begin by demonstrating that many families face no risk in applying as most admissions probabilities at their top-ranked program are degenerate. 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 which would be implied by changes in families' perceived admissions chances. Last, I assess the robustness of my leading estimates to a variety of assumptions that attenuate strategic considerations.

E.1 Admissions Probabilities

Appendix Table E.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.[26]

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

[26] 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.

Table E.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
BoyleHeights	1.000	0.000	0.000	0.673
Carson	0.999	0.000	0.000	0.260
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	0.854
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.

E.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 C.3), suggesting strategic incentives are not a salient feature of the application process. Second, Campos and Kearns (2023) 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.

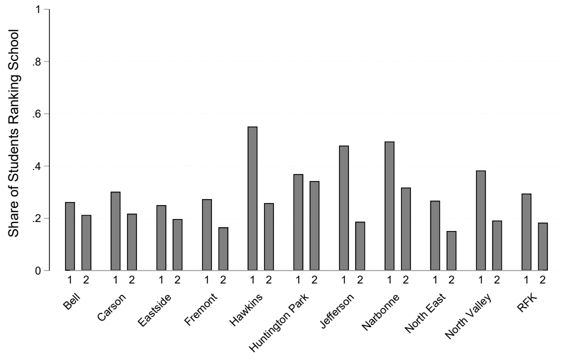
An intuitive test for the presence of strategic behavior is to focus on the most demanded schools in each market and look for sharp drops in demand. As Abdulkadiroglu et al. (2006) point out, under an Immediate Acceptance mechanism it is a mistake to rank an overdemanded school second. Appendix Figure E.1 reports evidence for these intuitive tests. I restrict to the markets that contain evidence of potential strategic behavior.[27] 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.

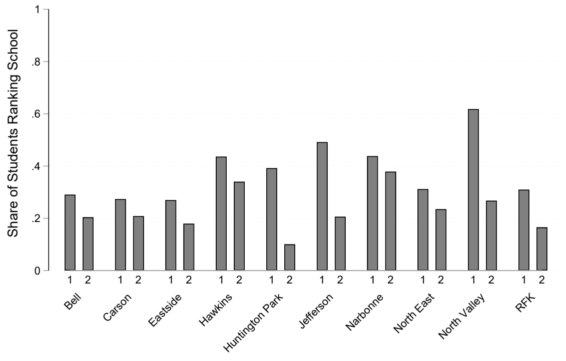
Evidence of preference heterogeneity notwithstanding, three zones, Huntington Park (HP), Jefferson, and North Valley, stand out with relatively mild drops in demand. For example, in the case of Lyndon Elementary and Quincy Elementary in Abdulkadiroglu et al. (2006), the number of families ranking these 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. 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.

[27] 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.

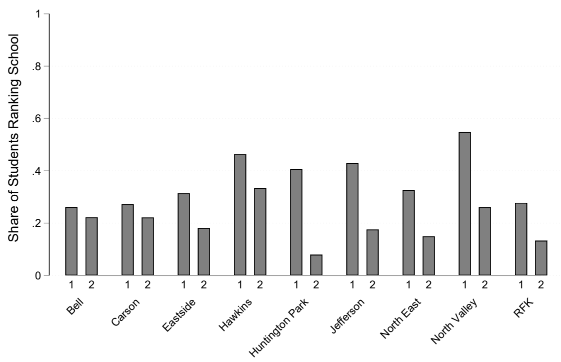
Figure E.1: Reporting Behavior Before and After the Intervention



(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.

E.3 Robustness Exercises

The evidence in Appendix Figure E.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 E.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 E.2 and Appendix Table E.3 report evidence regarding the first two tests, with Appendix Table E.2 focusing on models that consider information treatments and Appendix Table E.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 E.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 E.4 and Appendix Table E.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.[28] 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.

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

Table E.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 that vary by the information treatment that is either IA, AG, Both, or Spillover. The latter corresponds to indirectly treated parents in treated schools. 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 E.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. The latter corresponds to indirectly treated parents in treated schools. 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 E.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 report utility weight impacts on IA and AG 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 E.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.

F 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 conlcude with evidence discussed in the paper but with corresponding randomization-based inference.

F.1 Additional Evidence and Outcomes

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

$$\begin{aligned} Y_i = \alpha_z + & \underbrace{\beta_{Ph} T_i^P \times D_{s(i)}^h + \beta_{Sh} T_i^S \times D_{s(i)}^h + \beta_{Bh} T_i^B \times D_{s(i)}^h}_{High\ Saturation\ Effects} \\ & + \underbrace{\beta_{P\ell} T_i^P \times D_{s(i)}^\ell + \beta_{S\ell} T_i^S \times D_{s(i)}^\ell + \beta_{B\ell} T_i^B \times D_{s(i)}^\ell}_{Low\ Saturation\ Effects} \\ & + \underbrace{\beta_h C_i \times D_{s(i)}^h + \beta_\ell C_i \times D_{s(i)}^\ell}_{Spillover\ Effects} + u_i, \end{aligned} \tag{23}$$

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

Appendix Table F.1 and Appendix Table F.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.

F.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 F.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 F.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 AG (IA) on eight separate treatment indicators, including two saturation-specific spillover indicators, and three saturation-specific information-specific indicators. Column 1 reports estimates for a model with most-preferred AG as the outcome, and Column 2 reports estimates from a model with most-preferred IA as the outcome. Standard errors are robust and clustered at the school level.

Table F.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

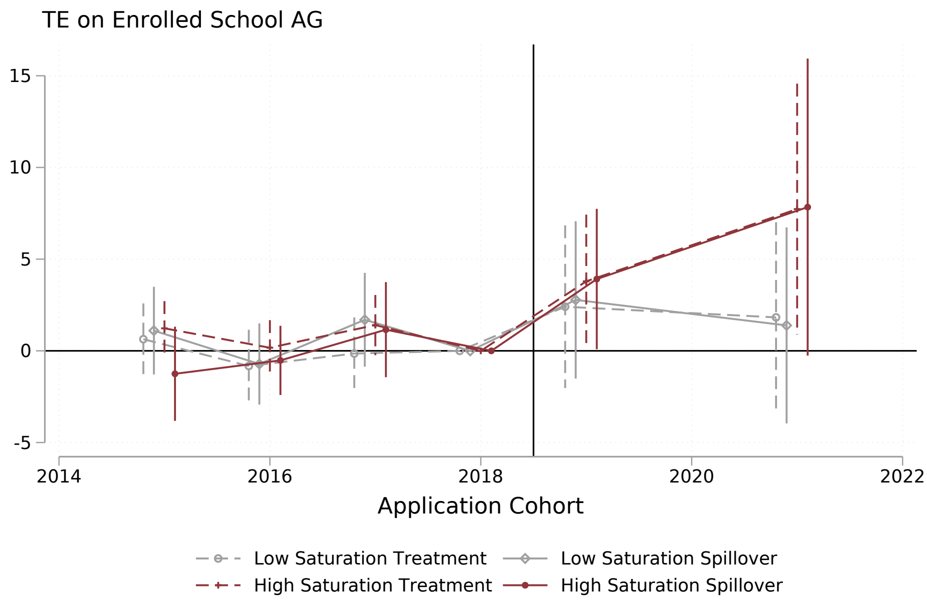
Notes: This table reports baseline experimental effects from the 2021 wave of the experiment. Estimates come from regressions of most-preferred AG (IA) on eight separate treatment indicators, including two saturation-specific spillover indicators, and three saturation-specific information-specific indicators.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

Table F.3: Heterogeneity Results

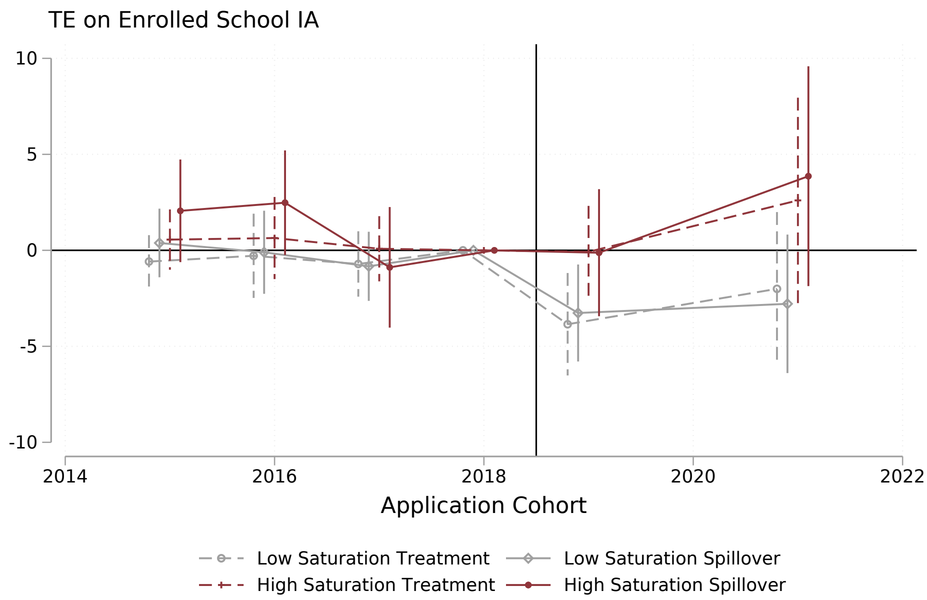
	(1) Pure Control Mean	(2) High Saturation 2019	(3) Low Saturation 2019	(4) High Saturation 2021	(5) Low Saturation 2021
Panel A: Incoming Achievement Percentile					
Low Achievers	33.402	-1.785	-1.061	-2.673	1.099
		(1.421)	(1.527)	(2.261)	(1.729)
Moderate Low Achievers	36.428	-1.769	-0.071	-0.112	2.358
		(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)

F.1.2 Impacts on Enrollment

Figure F.1: Difference-in-Difference Estimates



(a) Impacts on Enrolled School Achievement Growth

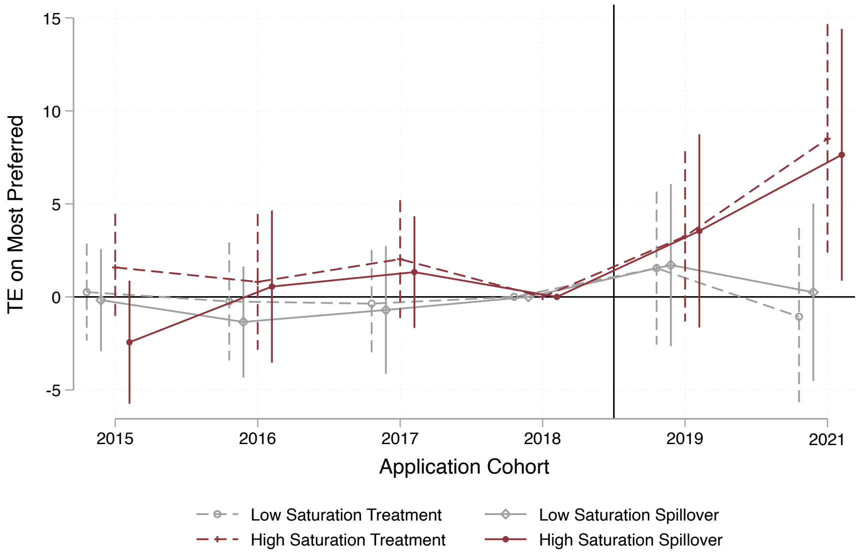


(b) Impacts on Enrolled School Incoming Achievement

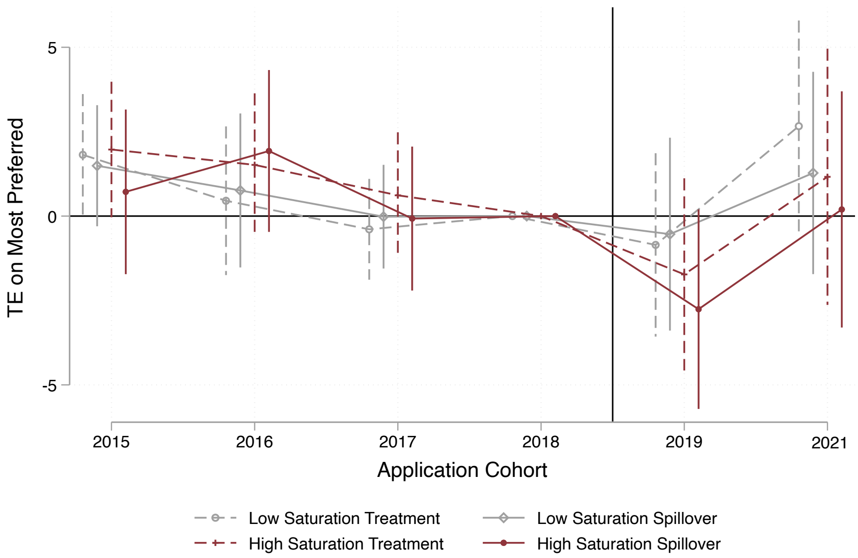
Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of ninth-grade enrolled school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along 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.

F.2 Reduced Form Estimates Implied by Structural Model

Figure F.2: Implied Reduced Form Estimates



(a) Impacts on Most-Preferred Achievement Growth

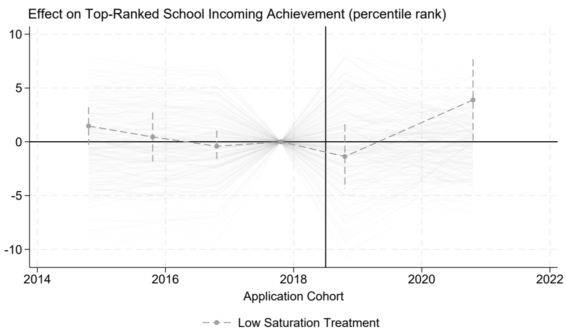


(b) Impacts on Most-Preferred Incoming Achievement

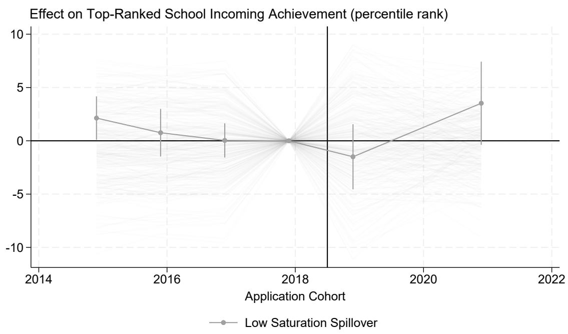
Notes: This figure reports difference-in-difference estimates from models considering four different school-level treatments. These estimates come from regressions of most-preferred school attributes—either incoming achievement or achievement growth—on year and treatment group fixed effects along with 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.

F.3 Randomization Inference

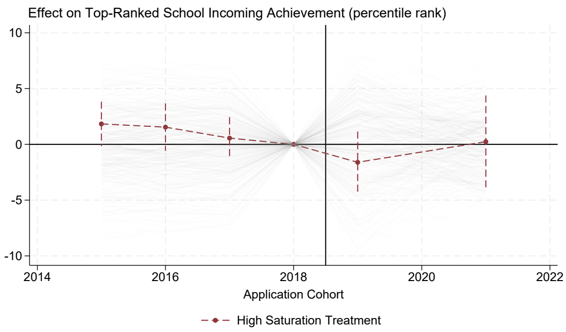
Figure F.3: Impacts on Most-Preferred IA (with Randomization Inference)



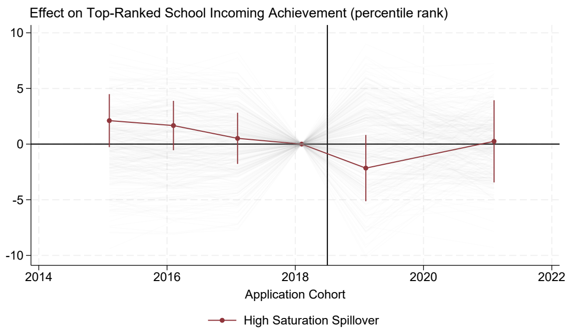
(a) Low Saturation



(b) Low Saturation Spillover



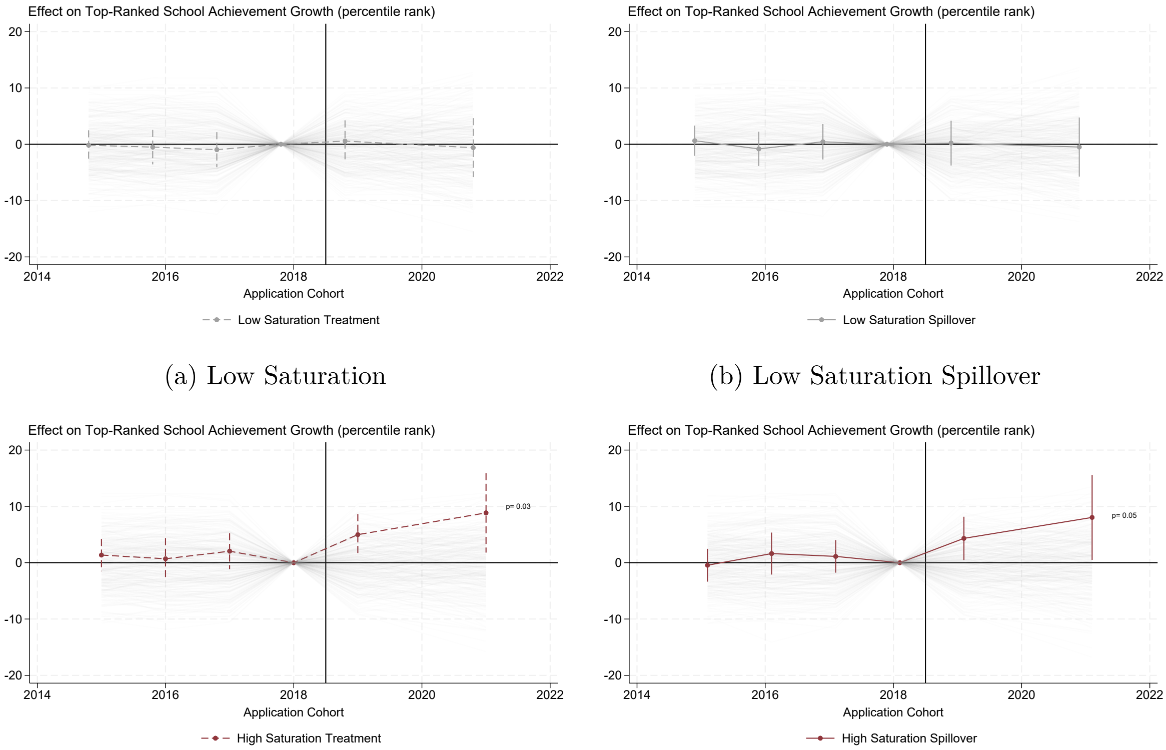
(c) High Saturation



(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 F.4: Impacts on Most-Preferred AG (with Randomization Inference)



(a) Low Saturation

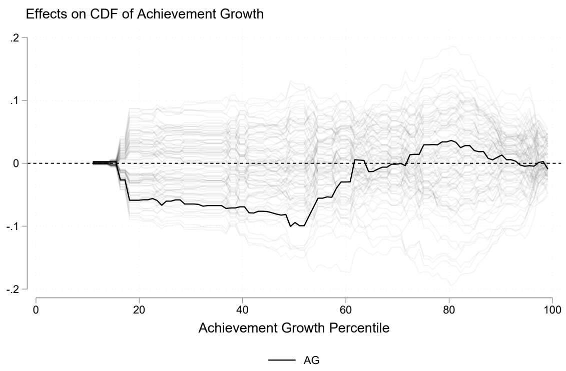
(b) Low Saturation Spillover

(c) High Saturation

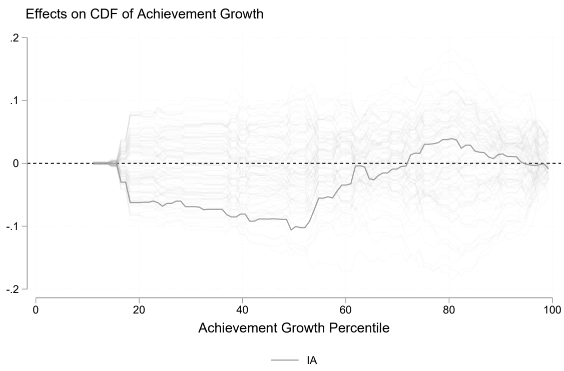
(d) High Saturation Spillover

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 (labeled 2022 because of academic year 2021-2022).

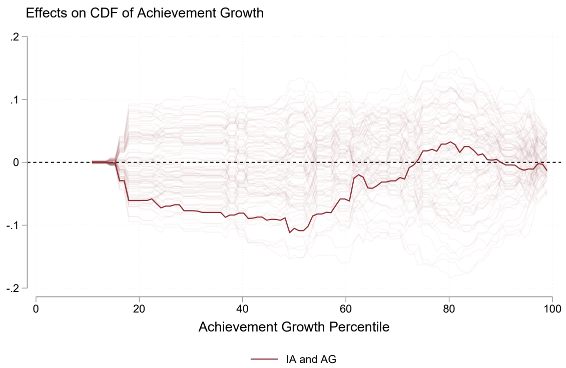
Figure F.5: AG Distributional Estimates (with Randomization Inference)



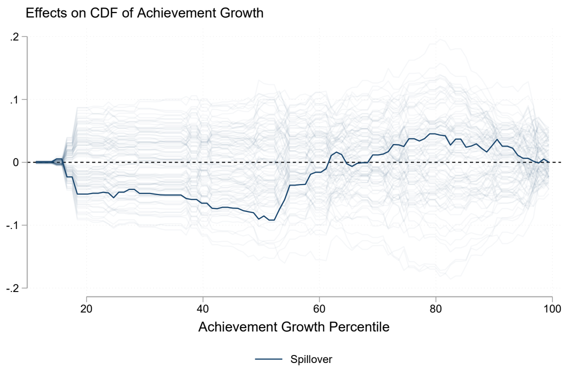
(a) AG Treatment



(b) IA Treatment



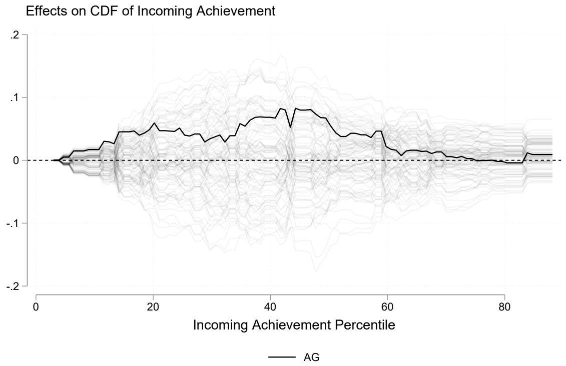
(c) AG and IA Treatment



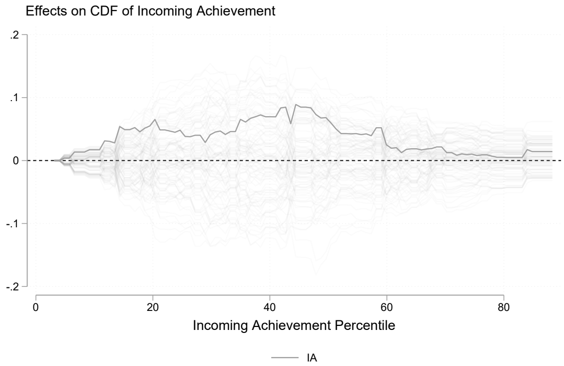
(d) Spillover

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.

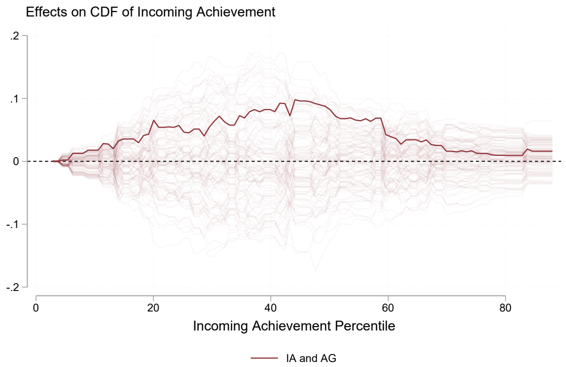
Figure F.6: IA Distributional Estimates (with Randomization Inference)



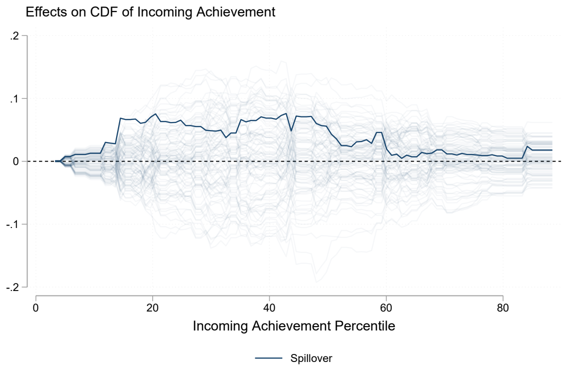
(a) AG Treatment



(b) IA Treatment



(c) AG and IA Treatment



(d) Spillover

Notes: This figure displays distribution regression estimates across the 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.