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HETEROGENEOUS BELIEFS AND SCHOOL CHOICE MECHANISMS

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ABSTRACT

This paper studies how welfare outcomes in centralized school choice depend on the assignment mechanism when participants are not fully informed. Using a survey of school choice participants in a strategic setting, we show that beliefs about admissions chances differ from rational expectations values and predict choice behavior. To quantify the welfare costs of belief errors, we estimate a model of school choice that incorporates subjective beliefs. We evaluate the equilibrium effects of switching to a strategy-proof deferred acceptance algorithm, and of improving households' belief accuracy. We find that a switch to truthful reporting in the DA mechanism offers welfare improvements over the baseline given the belief errors we observe in the data, but that an analyst who assumed families had accurate beliefs would have reached the opposite conclusion.

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1 Introduction

Many cities in the US and abroad use centralized school choice mechanisms to assign students to schools. Most centralized assignment mechanisms work by eliciting rank-order lists of schools from applicants and then making school assignments based on a combination of coarse priorities and random lotteries. However, districts differ in the extent to which their chosen assignment algorithms reward informed strategic play by choice participants. Charlotte, Barcelona, and Beijing use mechanisms that reward strategic play, while Boston, New York, and Denver use mechanisms which aim to make truthfully reporting one's preferences a dominant strategy.[1] Which type of mechanism is preferable is a central debate in the literature on school choice mechanism design. Mechanisms that reward informed strategic play can raise welfare by allowing participants to express the intensity of their preferences as opposed to just the ordering (Abdulkadiroğlu et al., 2011), but they can also lead to costly application mistakes and inequitable outcomes if some participants lack the information or sophistication to strategize effectively (Pathak and Sönmez, 2008).

Despite the critical role of beliefs and strategic play in the welfare comparison between the two mechanism types, there is little empirical evidence on what families know about school choice and how this affects the allocation of students to schools. This paper studies how welfare outcomes depend on the assignment mechanism when school choice participants are not fully informed. We combine a new household survey measuring the preferences, sophistication, and beliefs of potential school choice participants with administrative records of choice and academic outcomes to conduct two types of analysis.

First, we present a descriptive analysis of families' subjective beliefs and strategic behavior, and how these translate to school placement outcomes. We find that many families engage in strategic play, but do so on the basis of subjective beliefs that are often wrong. Second, we estimate a model of school choice in which families make decisions on the basis of subjective beliefs about admissions chances. The model allows us to quantify the tradeoff between welfare-reducing mistakes and families' ability to express cardinal preferences in terms of both aggregate welfare and equity. We use our model estimates to evaluate the equilibrium effects of improving the information available to households in a mechanism that rewards strategic play, and of switching from such a mechanism to a strategy-proof deferred acceptance (DA) algorithm. We find that a switch to truthful reporting in the DA mechanism offers welfare improvements over the baseline given the belief errors we observe

[1] Boston, New York, Denver: Abdulkadiroğlu et al. (2005a,b, 2017b). Barcelona: Calsamiglia and Güell (2018); Charlotte: Hastings et al. (2009); Beijing: He (2012). See Pathak and Sönmez (2013) for a discussion of incentives to report truthfully in these mechanisms.

in the data, but that an analyst who assumed families had accurate beliefs would have reached the opposite conclusion.

We conduct our study in the context of high school choice in the New Haven, Connecticut school district (henceforth NHPS). NHPS is a low-income, majority-minority school district that has used a centralized mechanism to assign students to schools since at least 1997. We conducted home surveys of the families of rising ninth graders in 2015 and 2017. In total, we surveyed 417 households. We link our survey data to administrative records of the school placement process.

The assignment mechanism NHPS uses (henceforth, the 'baseline' mechanism) closely resembles the 'Boston' or immediate acceptance mechanism, which rewards strategic play by giving applicants higher admissions priority at schools they rank higher on their application forms.[2] A theoretical literature on school choice mechanism design provides conditions under which all students prefer the Boston mechanism to the student-optimal stable matching mechanism, and others under which it is (weakly) worse for all students (Ergin and Sonmez, 2006; Abdulkadiroğlu et al., 2011).[3] Which mechanism will perform best in a particular district is therefore an empirical question. The answer depends on whether applicants' ability to express cardinal preferences through strategic play in the Boston mechanism outweighs the welfare costs of strategic mistakes due to misunderstandings about the mechanism or lack of information about demand conditions. Observations of beliefs and preferences help us quantify this tradeoff.

We begin our analysis by using our survey to describe participants' preferences, subjective beliefs, and strategic sophistication, as well as the relationship between beliefs and choice behavior. We show that many families misunderstand the assignment mechanism and make errors in their estimates of the admissions probabilities associated with different application portfolios. Fewer families can correctly describe key features of the assignment mechanism than would be expected from random guessing. When asked about admissions chances for hypothetical application portfolios, respondents report subjective beliefs that differ from rational expectations admission probabilities by a mean (absolute) value of 37 percentage points. Consistent with the hypothesis that families do not understand the assignment mechanism, respondents underestimate how much ranking a school lower on their application reduces admissions chances.

Errors in subjective beliefs matter because, together with preference intensity, they are inputs to strategic behavior. 32% of respondents are 'revealed strategic' in the sense that they list a

[2] In 2017, New Haven used the Boston mechanism. In 2015, it used a mechanism that coincides with the Boston mechanism when all students are in the same priority group. We discuss the mechanism in detail in Section 2.

[3] See also Pathak and Sönmez (2008), who provide a model in which sophisticated students benefit, and naive students suffer, from the Boston mechanism, and Pathak (2011) for a review.

school other than their most-preferred school first on their application. Households reporting weak relative preferences for their most-preferred school are 58% more likely to be revealed as strategic. Conditional on rational expectations admissions chances, students with subjective beliefs in the upper quartile of the belief distribution are 17 percentage points more likely to rank their most-preferred school first on their application than students with subjective beliefs in the bottom quartile. In contrast, conditional on subjective beliefs, rational expectations admissions chances do not predict the rates at which applicants list their most-preferred school first.

Motivated by these descriptive findings, we use an empirical model of school choice to study the equilibrium effects of alternative school choice policies. Our approach combines survey evidence with a revealed preference analysis of students' application and enrollment choices. Households in our model maximize expected utility given their subjective beliefs about admissions probabilities, not rational expectations beliefs. The survey data help us overcome the challenges associated with separately identifying beliefs and preferences described by Manski (2004) and Agarwal and Somaini (2018) without imposing strong assumptions on applicants' equilibrium play.

Because we cannot ask families about the admissions probabilities associated with each possible application portfolio, we develop a parsimonious model of belief formation that captures key features of our survey results. In the model, students' beliefs about their own admissions rankings relative to cutoff rankings for admission to each school are equal to the true values plus a shift term. The shift term depends on a) the student's priority at a target school, b) the school's rank on a student's submitted application, c) a student level shock that is common across all schools, and d) person-school components. The first two terms allow us to capture systematic misunderstanding of the assignment mechanism, while the latter two allow, respectively, for levels of optimism to vary across students and for errors in belief about school-specific demand.

We incorporate subjective beliefs into a model of choice in which households choose whether to participate in choice and, if they participate, what application to submit. The model allows for correlated heterogeneous preferences across schools. We estimate the model using an MCMC procedure (McCulloch and Rossi, 1994; Agarwal and Somaini, 2018) that incorporates both survey and administrative data. For surveyed students, the model fits both administrative records of submitted applications and survey reports of beliefs and preferences. The model also uses belief errors to rationalize choices for unsurveyed households.

With parameter estimates in hand, we study two sets of counterfactual simulations. The first counterfactual exercise simulates a switch to a DA mechanism. In the DA mechanism, students do not need to understand assignment probabilities to play an optimal strategy. The second considers a best-case informational intervention allowing households to play the Bayes Nash equilibrium in the game induced by the baseline mechanism. To evaluate welfare in these counterfactuals, we consider each student's expected utility, according to the utility he or she gets from placement at each school and the rational expectations chances associated with their lottery application. We measure utility relative to the outside option of attending a neighborhood school.

Results from these exercises show that errors in subjective beliefs reverse the welfare comparison between the baseline and deferred acceptance mechanisms, and that this reversal is economically large. Given the beliefs we observe in the data, switching from the baseline mechanism to truthful reporting under a deferred acceptance assignment mechanism would *increase* mean welfare by the equivalent of 3.9 fewer miles traveled per trip, or 27% of households' mean welfare gain relative to the outside option. This finding does not change across a wide variety of potential deviations from truthful play in the DA mechanism. Welfare gains are larger for low-SES households.

To highlight the importance of subjective beliefs data for this welfare comparison, we estimate an alternate version of the model that does not use information on subjective beliefs. We assume that observed application portfolios reflect the Bayes Nash equilibrium in the game induced by the baseline mechanism. Results from this exercise suggest that switching from baseline to DA would *reduce* mean welfare by 1.8 miles traveled. The effect of incorporating data on subjective beliefs is thus to raise our estimate of the benefit of the switch to DA by 5.7 miles traveled, or 39% of households' baseline mean welfare. In sum, when the analysis allows for application mistakes, the costs of mistakes in the baseline mechanism outweigh the benefits of expressiveness.

The finding that mechanisms rewarding strategic play outperform DA under the assumption that households have rational expectations beliefs is consistent with a number of previous papers in the empirical school choice literature. In the absence of data on beliefs, this research assumes that participants are informed and sophisticated, or deviate from optimal behavior in specific ways. For example, Agarwal and Somaini (2018) assume, as a baseline specification, that participants are fully rational and correctly anticipate their chances in the lottery when choosing applications. Alternatively, Calsamiglia and Güell (2018) consider school choice under a Boston mechanism in Barcelona. They allow two types of participants: one type is sophisticated and informed while the other type uses a rule of thumb to determine choices. Calsamiglia et al. (2018), He (2012), and Abdulkadiroğlu et al. (2017b) take similar approaches. Our findings show that accounting for application mistakes in an empirically guided way reverses the welfare comparison between deferred acceptance and a mechanism that rewards strategic play. To the best of our knowledge this is the first paper to collect belief and preference data from actual and potential school choice participants.[4]

[4]Two recent papers incorporate some survey elements to unpack school choice participation decisions and reports. Dur et al. (2018) make use of data on the frequency with which students access a school choice website to proxy

Results from our best-case informational intervention suggest that the baseline mechanism could yield aggregate welfare outcomes similar to DA if the district could help households learn to play optimally. An intervention that allows all households to make choices using rational expectations beliefs would raise welfare by the equivalent of 4.5 fewer miles traveled (31%) relative to the observed baseline, or by 0.55 miles (3%) relative to the DA counterfactual. Descriptive evidence that using district-provided informational resources does not reduce belief errors suggests that the form of the best-case intervention may differ from what was available to households during the study period.

2 Empirical Setting

2.1 The school choice process in New Haven

We study the school choice process in New Haven, Connecticut, an urban district composed mostly of lower-income minority students.[5] The school choice system includes district-run schools and charter schools run by outside operators, such as 'no excuses' charter Achievement First.

The school choice process begins in January, when students and families can learn about schools and the choice process by visiting schools or attending 'magnet fairs' where schools set up information booths. The district provides students with descriptions of the rules of choice, data on available seats, and applicant counts by priority group from the previous year. Students typically submit their applications in February, and receive notice of their placements in late March or April. School choice institutions in New Haven resemble those in other districts that offer centralized choice, and have been around for long enough that they are familiar to students and parents.[6]

We focus our analysis on eighth grade students living in New Haven who are making choices about where to attend high school. We conducted two surveys, one in the school year ending in 2015 and the other in the school year ending in 2017. In the 2015 (2017) school year, there were 1,544 (1,645) potential ninth graders. Of this group, students who do not leave the city or enroll in

for strategic and sincere participants in a school choice mechanism. Students who visit the site multiple times are assumed to be sophisticated, while those visiting only once are assumed sincere. de Haan et al. (2015) measure cardinal utility in Amsterdam using a survey that asks students to assign points to each school, with the top choice receiving 100 points, but do not ask about beliefs. Neither paper incorporates survey data on beliefs into a model of household behavior or considers counterfactuals that vary the information available to households.

[5] Over 80% of New Haven students are black or Hispanic, and the majority are eligible for free or reduced price lunch. See Online Appendix Table A1 for district-level descriptive statistics.

[6] New Haven has used centralized choice since at least 1997. New York introduced a centralized application in 2003, followed by Denver, New Orleans, Newark, and Washington DC (Abdulkadiroğlu et al., 2017a). Other districts offer a similar mix of schooling options and choice calendars. See Corcoran et al. (2018) (New York), and Agarwal and Somaini (2018) (Cambridge).

private school may enter a lottery to enroll in one of 12 high schools. Ten of these are public schools and two are charter schools. Two schools are K12 institutions that offer spots to already-enrolled eighth graders outside of the choice process and use the centralized process to fill remaining seats. Students who do not apply or who are not placed and who are not already enrolled in a K12 school are assigned to one of two neighborhood schools according to geographic zone boundaries.

High school choice for rising ninth graders is part of a larger choice system in New Haven. We focus on grade nine students because the assignment mechanism New Haven used in 2015 more closely resembles the mechanisms used in other districts for high school choice than for primary school choice. Some high schools reserve seats for suburban applicants. We exclude these seats from our sample and focus on the seats reserved for within-city applicants.

2.2 School choice mechanisms in New Haven

The district used different mechanisms to assign students to schools in our two survey years. Beginning in 2016, the district used the Boston mechanism to assign students to schools. This was the mechanism in place during our 2017 survey. Prior to 2016, the district used an alternative mechanism that we label the 'New Haven' mechanism. The difference between the two mechanisms is that in the Boston mechanism, the rank in which a school is listed on the application takes precedence over a student's priority group when determining placement outcomes, while in the New Haven mechanism the reverse is true. When all students have the same priority, the Boston and New Haven mechanisms coincide. This is approximately the case for high school choice in New Haven. In this section we describe how the two mechanisms work, and show that the New Haven mechanism closely resembles the Boston mechanism for ninth grade applicants.

Most school choice mechanisms use some form of coarse priorities to favor certain applicants. In New Haven, each student is assigned a priority at each school $j \in J$, which is a number between one and two:

$$priority_{ij} = \begin{cases} 1 & \text{if } i \text{ has a sibling at } j \\ 2 & \text{otherwise} \end{cases}$$

Similar priority structures are in place in Boston, Cambridge, New York, Barcelona, Beijing, and other cities. The priority groups in New Haven do not change over the years we study.

The New Haven mechanism assigns students to schools using the following algorithm:

1. Consider each student's first choice submission. Each school ranks applicants up to its capacity, in order of priority group, using random lottery numbers as a tiebreaker. Each school provisionally accepts students up to its capacity and rejects the rest of its applicants.

2. Consider the next listed choice of students who were rejected in the previous step, together with the applications provisionally assigned in the previous step. Make provisional assignments at each school in order of a) priority group and b) submitted rank, again using lottery numbers as a tiebreaker.

3. Repeat Step 2 until all students are provisionally assigned to schools or have been considered and rejected at each listed school.

4. Following the conclusion of Step 3, permanently assign students to the schools where they are provisionally assigned.

The mechanism assigns each student to at most one school. Students may choose to accept or decline this placement. Students who are unplaced or decline their placement have the option to enroll in their neighborhood school or leave NHPS.

Like the familiar student-proposing deferred acceptance algorithm, the New Haven mechanism employs provisional assignment. It differs from the standard deferred acceptance approach (Roth, 2002) in the use of submitted ranks to break ties within priority groups. The centralized mechanism in New York also combines provisional assignments with the use of submitted ranks as tiebreakers (Abdulkadiroğlu et al., 2005b). However, while in the New York mechanism the set of student-school-rank combinations for which such tiebreakers play a role is relatively small,[7] New Haven uses rank-based tiebreaks for all applications.

To compare the New Haven mechanism to Boston and deferred acceptance mechanisms, we employ a cutoff representation of matching algorithms introduced by Azevedo and Leshno (2016) for stable matchings and extended to a class of 'report-specific priority plus cutoff' mechanisms by Agarwal and Somaini (2018). The cutoff representation of the New Haven mechanism is as follows. The mechanism assigns student i a 'report-specific priority' at school j when i submits rank-order list a, given by:

$$rsp_{ij}(a) = R \times priority_{ij} + rank_{ija},$$

where $R = 4$ is the maximum number of schools permitted on an application, and $rank_{ija}$ is j's rank on application a.[8]

Ties are broken with uniform random draws that assign each student a score at each school:

[7] A subset of New York schools offered automatic admission to students scoring in the top 2% on a standardized exam who rank a school first on their application list.

[8] That is, if j is ranked rth on a, then $rank_{ija} = r$. Report-specific priority $rsp_{ij}(a)$ is undefined when j is not listed in a, but this does not matter because i cannot be placed in a school he did not apply to.

$$score_{ij}(a) = rsp_{ij}(a) + z_{ij},\ z_{ij} \sim U[0,1].$$

The resulting assignment is characterized by cutoffs π_j that fill schools' capacities when each student is matched to his earliest-listed school at which $score_{ij} < \pi_j$. If a school is undersubscribed, its cutoff is set above all applicants' scores. The New Haven mechanism is a mapping from profiles of applications to distributions over cutoffs $\pi \in \mathbb{R}^J$.

The New Haven mechanism differs from Boston and student-optimal stable matching ("SOSM") mechanisms in the construction of $rsp_{ij}(a)$. In the New Haven mechanism, report-specific priority depends lexicographically on the exogenous priority $priority_{ij}$ and the rank that the student assigns to the school. In the Boston mechanism, this lexicographic order is reversed. Sibling priority plays a relatively less important role and submitted rank lists a relatively more important role in determining report-specific priority. In the Boston mechanism in our setting, report-specific priority is given by

$$rsp_{ij}^{Boston}(a) = priority_{ij} + T \times rank_{ija},$$

where $T = 2$ is the number of distinct priority groups.

In the SOSM mechanism, report-specific priorities depend on the exogenous priority group only, and not on school j's position on i's submitted rank-order list a. That is, for all a,

$$rsp_{ij}^{SOSM}(a) = priority_{ij}.$$

The New Haven Mechanism differs from the SOSM mechanism in that the tiebreaking rule within priority groups depends on submitted ranks.

When all students have the same priority, the Boston and New Haven mechanisms produce the same assignments. In our setting, students are assigned to unconstrained neighborhood schools outside of the choice process, and few students have sibling preference. The New Haven mechanism and the Boston mechanism are therefore quite similar. Table 1 describes placement outcomes and priority groups for ninth grade applicants in 2015 and 2017. As shown in Panel A, 7% of applicants in 2015 and 8% of applicants in 2017 applied to at least one school where they had sibling priority, with the remaining students having no priority at any listed school. Simulation indicates that, because few students have sibling priority, the change in assignment mechanism has little effect on assignment outcomes. Using the 2015 application data, we simulate random lottery draws 500 times, running both the Boston mechanism and the New Haven mechanism each time. The mean share of placements that differ across mechanisms is 1.18%.

Table 1: Placement outcomes and priority groups by year

	All	2015	2017
A. Priorities			
Any sibling priority	0.08	0.07	0.08
None	0.92	0.93	0.92
B. Participation and placement			
Submits applications	0.68	0.66	0.70
Participates in choice	0.66	0.66	0.66
Places first	0.62	0.64	0.59
Places second	0.10	0.13	0.07
Places third	0.03	0.06	0.00
Places fourth	0.02	0.06	0.00
Unplaced	0.26	0.18	0.34
N	3,189	1,544	1,645

Placement outcomes and priority group for in-district eighth graders by year. Students participate in choice when they submit a lottery application containing at least one non-neighborhood school. Placement outcomes and priorities are conditional on participation. 'Unplaced' tabulates students who do not receive a placement during the main lottery or who are placed into their neighborhood schools (2017 only).

As shown in Panel B, 66% of applicants in 2015 and 70% of applicants in 2017 submitted applications to the centralized system. In 2015, the electronic application did not allow students to list their neighborhood school, while in 2017 students were permitted to list the school, and 4% of students listed it first. The share of students participating in choice– defined as submitting an application with a non-neighborhood school listed first– was thus 66% in both years. Conditional on participation, about 60% of students placed first in each year. A small number of students in 2015 placed in their second- through fourth-listed choices, while in 2017 no student placed lower than second. The remainder were unplaced.

2.3 Placement chances and cutoff representations

An appealing feature of the cutoff representations of the New Haven and Boston mechanisms is that placement probabilities for student-school pairs are determined by the cutoff vector π and the students' $rsp_{ij}(a_i)$ under the applications that they submitted. Consider a rank-order list $a : j_1 \succ \ldots \succ j_k$. We say that $j \succ j'$ if school j is listed ahead of school j' on application a. The probability that applicant i will be assigned to school j given that he submits report a to the mechanism is

$$p_{ija} = Pr\left(z_{ij} \leq \pi_j - rsp_{ij}(a),\ z_{ij'} > \pi_j' - rsp_{ij'}(a) \text{ for all } j' \text{ such that } \left(\exists r' < rank_{ija} : rank_{ij'a} = r'\right)\right).$$

In the next section, we use this formulation to simulate rational expectations (or 'RatEx') admissions chances for observed and hypothetical application portfolios.

3 Household Survey

3.1 Survey overview

We conducted in-person interviews with the parents or guardians of 417 rising ninth graders beginning in the summers following the 2014-2015 (henceforth '2015') and 2016-2017 (henceforth '2017') school years. We drew our sample from the universe of New Haven residents enrolled in New Haven public schools. We interviewed 120 households in 2015 and 297 households in 2017. Our survey team conducted interviews at parents'/guardians' residences using a tablet application that generated questions tailored to each household and recorded respondents' answers. Both the 2015 and 2017 surveys included questions on preferences and beliefs about admissions probabilities. The 2015 survey included questions on sources of information and consideration sets, while the 2017 survey included measures of preference intensity.

We describe survey procedures in Online Appendix E and present question text in Online Appendix F. Several survey design elements are important to highlight. The first is timing. We surveyed households in the summers following the student's eighth grade year. Our interviews thus took place after households learned of choice placements. An alternative approach is to conduct surveys prior to the choice process. The post-application approach has two advantages. The first is that it cannot alter choice behavior. An ex ante survey would likely affect behavior by pushing respondents to think through the outcomes resulting from different application portfolios. The second is that the process of information gathering is complete. A survey conducted in advance of the submission deadline will not capture 'finalized' beliefs and preferences for households that wait until the deadline to think through the process.

There are also disadvantages. Respondents may forget the preferences and beliefs they took as inputs to choice, or may update preferences and beliefs in response to placement outcomes due to learning or ex post rationalization. We mitigate these disadvantages through survey design choices, direct measurement, and robustness tests. On the design side, we formulate questions as hypotheticals set in the past ('think back to the time you were filling out your own application, or deciding whether to fill one out,' and 'say that you had submitted the following application') so as not to highlight respondents' placement outcomes. We address concerns about forgetfulness or ex post updating of belief and preference reports by a) testing recall of submitted applications, b) examining correlations between survey reports and high-stakes application behavior, c) measuring the effect of placement outcomes on survey reports conditional on applications. Findings from these exercises suggest that our survey succeeded in capturing inputs to high stakes behavior with limited ex post updating. In addition, the model-based analysis presented in Sections 4 and 5 explicitly incorporates measurement error in belief and preference reports.

72% of respondents who submit an application correctly report the first choice listed on that application. To assess the sensitivity of our findings to forgetfulness, we examine how belief errors vary with correct recall (Section 3.7), and how the exclusion of respondents with incorrect recall from the analysis affects welfare findings (Section 6). The restriction to correct-recall respondents does not affect our findings in either case. This is consistent with the observations that a) the survey asks about hypothetical applications, so correct recall of one's own application is not a direct input into survey reports, and b) the relationship between belief errors and other measures of engagement with choice such as submitting an application, stating a preference for a particular school, or using district-provided information sources is also weak (Section 3.7).

The second survey design element is the choice of who to talk to. At the high school level, both parents and students likely have input into the choice process. One concern about surveying parents/guardians is that the child may have made choice decisions without their knowledge. However, 74% of parents reported participating in filling out the school choice application, and 92% report that either they or their child was the 'most important [person] in deciding which schools to list.' Section 3.7 shows that there is little variation in the distribution of belief errors along this dimension.

The third survey design element is the survey medium. We conduct surveys in person at students' homes. We also considered phone surveys and online surveys nested in the choice process. We ruled out phone surveys due to concerns about takeup, while implementing surveys as part of the choice process rules out surveying non-participants. The fourth is incentives. We do not incentivize 'correct' beliefs, e.g. by paying people to state beliefs that are close to rational expectations chances.

3.2 Coverage

Our survey covers individuals from across the distribution of demographics and participation choices. Panels A, B, and C of Table 2 compare respondents to the sample universe in terms of student socioeconomic status, race/ethnicity, and English language learner status. We measure socioeconomic status using poverty rate in the student's census tract of residence, divided into quintiles. (The count of students across quintiles is not equal because some tracts are relatively large.) Our survey population covers each quintile, with some oversampling of lower-income families. In what follows we define the group of 'high-SES students' to be those from the top SES quintile. Black and Latino students make up 86% of the student population. We undersample black students and oversample Latinos, but have many students in both groups. Similarly, our sample includes both English language learners and special education students. The distribution of surveyed students across neighborhoods closely matches the distribution in the population. See Online Appendix Figure A1.

Panel D of Table 2 describes school choice participation. Households who participate may list up to four schools on their application. Our surveyed population somewhat oversamples school choice participants (76% of respondents vs. 66% in the population), but includes many observations from both groups. We observe applications of all possible lengths.

3.3 Rational expectations admissions chances

Analyses of effective strategic play and belief errors require estimates of rational expectations beliefs about admissions chances. We construct a measure that represents the beliefs about admissions chances that an agent would have if he knew his own report-specific priority, the rules of the mechanism, schools' capacities, the number of other applicants, and the underlying distribution of preference lists and report-specific priorities for other applicants, but did not know which preference lists and priorities had been drawn from this distribution.

Table 2: Characteristics of population and survey respondents

Category	Population Mean	Surveys Mean	Pop v. Survey
SES quintile			
Bottom 20%	0.24	0.27	0.03
20-40%	0.18	0.22	0.05
40-60%	0.24	0.23	−0.02
60-80%	0.15	0.14	−0.01
Top 20%	0.20	0.15	−0.05
Race/Ethnicity			
Black	0.46	0.36	−0.11
Hispanic	0.40	0.53	0.14
White Non-Hispanic/Other	0.13	0.10	−0.02
Educational program			
English language learner	0.13	0.20	0.07
Any special education	0.20	0.20	0.01
Number of applications			
Participates in choice	0.66	0.76	0.12
1	0.17	0.22	0.05
2	0.20	0.22	0.04
3	0.37	0.36	−0.01
4	0.19	0.12	0.02
N	3,189	417	

Means of indicator variables for demographic and socioeconomic characteristics for sample universe and surveyed population. 'Population' is universe of NHPS students in 8th grade at time surveyed. 'Surveys' describes surveyed households. 'SES' represents quintiles of the distribution of the poverty rate in households' census tract, using data from the 2016 American Community Survey. The count of students across quintiles is not equal because some tracts are relatively large. 'Race/Ethnicity' are observed in administrative data. 'Number of applications' presents counts of schools listed on choice applications (in 2017, non-neighborhood schools only), conditional on participation. 'Pop v. Survey' column displays differences between population and survey means, regression adjusted by adding year fixed effects.

We calculate these probabilities using an approach similar to Agarwal and Somaini (2018). Within each market (defined here by years) we draw a large number ($N = 200$) of resampled markets by sampling from the population iid with replacement. Each resampled market is a list of individuals with a participation decision, a report if they participated in the lottery, and a priority at each school. In each resampled market, we solve for market-clearing cutoffs by running the assignment mechanism.

The cutoffs $\left\{\pi_j^{(k)}\right\}_{k=1,\dots,N}$ allow us to calculate rational-expectations admissions chances. For example, if an individual has $rsp_{ij} = 9$ in the New Haven mechanism (no sibling priority, first-ranked school) and lists j first, if the cutoff is $\underline{\pi}_j = 9.4$ then the individual has a 0.4 chance of placing in j. For each individual i, we compute the propensity to place in each school j under the individual's observed application and the given cutoff vector, and then average these chances over the resampled market-clearing cutoffs. Student i's chance of being placed in school j under report a is given by

$$\begin{aligned} p_{ija} &= Pr\left(z_{ij} \le \pi_j - rsp_{ij}(a),\ z_{ij'} > \pi_j' - rsp_{ij'}(a) \text{ for all } j' \text{ such that } \left(\exists r' < rank_{ija} : rank_{ij'a} = r'\right)\right) \\ &\approx \frac{1}{N}\sum_{k=1}^{N} Pr\left(z_{ij} \le \pi_j^{(k)} - rsp_{ij}(a),\ z_{ij'} > \pi_{j'}^{(k)} - rsp_{ij'}(a) \text{ for all } j' \text{ such that } \left(\exists r' < rank_{ija} : rank_{ij'a} = r'\right)\right) \end{aligned}$$

3.4 Preference data and strategic play

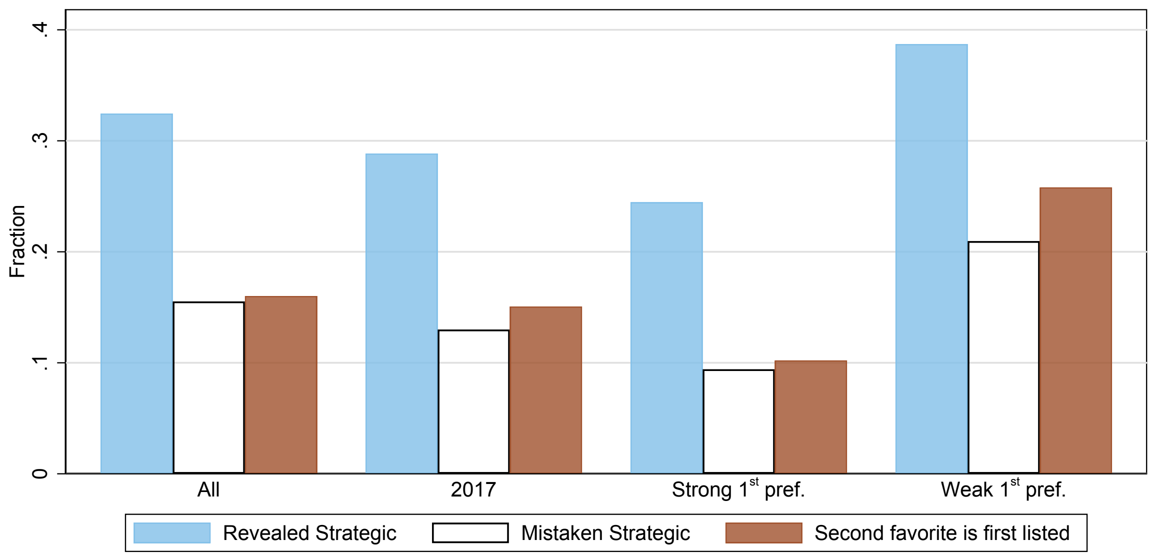
Together with application data, preference reports suggest that many households play strategically. Our survey asked respondents to list their first- and second-most preferred schools if they could choose to attend any school with certainty. As shown in the left two bars of Figure 1, 32% of respondents who submit an application list a school other than their stated most-preferred school first. We label this set of respondents 'revealed strategic.'[9] Of these, roughly half list their stated second-most preferred school first, so that overall 81% of respondents list one of their two most-preferred schools first on their application.

Rates of revealed strategic play vary with reports of preference intensity. In our 2017 survey, we measured cardinal preferences in addition to ordinal preferences. We asked respondents whether they would prefer a lottery that assigned them to their most-preferred school with probability X and to their neighborhood school (no placement) with probability $1 - X$ to a sure assignment to their

[9] It is possible there is measurement error in preference data such that not all of these households to which we apply this designation are in fact strategic. Our analysis in Section 4 incorporates survey measurement error.

second-most-preferred school, with X equal to 0.25, 0.5, and 0.75. We label the 68% of students who report a willingness to accept at least one of these lotteries as 'strong first preference' students.[10] The right three groups of bars in Figure 1 describe application behavior for the 2017 sample overall, the strong first preference sample, and the weak first preference sample, respectively.

Figure 1: Revealed strategic play overall and by preference intensity



Share of revealed strategic and mistaken strategic households overall, in 2017 only, and by intensity of preference for listed first choice. 'Revealed strategic' households are those who list a program other than their stated most-preferred school first. 'Mistaken strategic' are the subset of revealed strategic households whose rational expectations admissions chances are higher (if listed first) at their most-preferred school than at their first-listed school. 'Second favorite is first listed' gives the rate at which unconstrained second choice schools are listed first.

Households that report preferring their most-preferred school more strongly relative to their second-most preferred school are more likely to list it first on their application. In the full 2017 sample, 29% of students who submitted an application were revealed strategic. In the strong first preference group, 24% of students were revealed strategic, compared to 39% of students in the weak first preference group, for a gap of 14 percentage points, or 58%. The p-value from a test of equality across the strong- and weak-first preference groups is 0.050.

[10] We cannot reject the null hypothesis that the likelihood of listing the most-preferred program first is equal for different minimum acceptable values of X at conventional levels.

A large share of strategic households appear to be making mistakes. We define 'mistaken strategic' as a household that is revealed strategic but for which the first-listed school on a submitted application offers lower odds of admission than the household's most-preferred school. This is a mistake because the student could have obtained a greater chance at attending a more-preferred school by substituting his or her most-preferred school for the first-listed school on the application. The unfilled bars in Figure 1 show the share of mistaken strategic individuals. 48% of revealed strategic applications (16% of applications in the sample) are mistaken strategic. That students attempt to play strategically but appear to make errors while doing so is consistent with evidence from beliefs data we discuss in the next section.

Households form preferences after considering many schools. 20% of surveyed students in 2015 considered each school in the district and two-thirds considered at least half of schools. Online Appendix Table A2 presents statistics for each school. The school-by-school statistics illustrate how the use of application data to infer preferences can be misleading in a strategic setting. For example, Co-op Arts is the most preferred school for 19.2% of students but appears first on 10.9% of applications, while Engineering & Science is most preferred for 10.5% of students but appears first on 21.6% of applications.

3.5 Beliefs about admissions chances

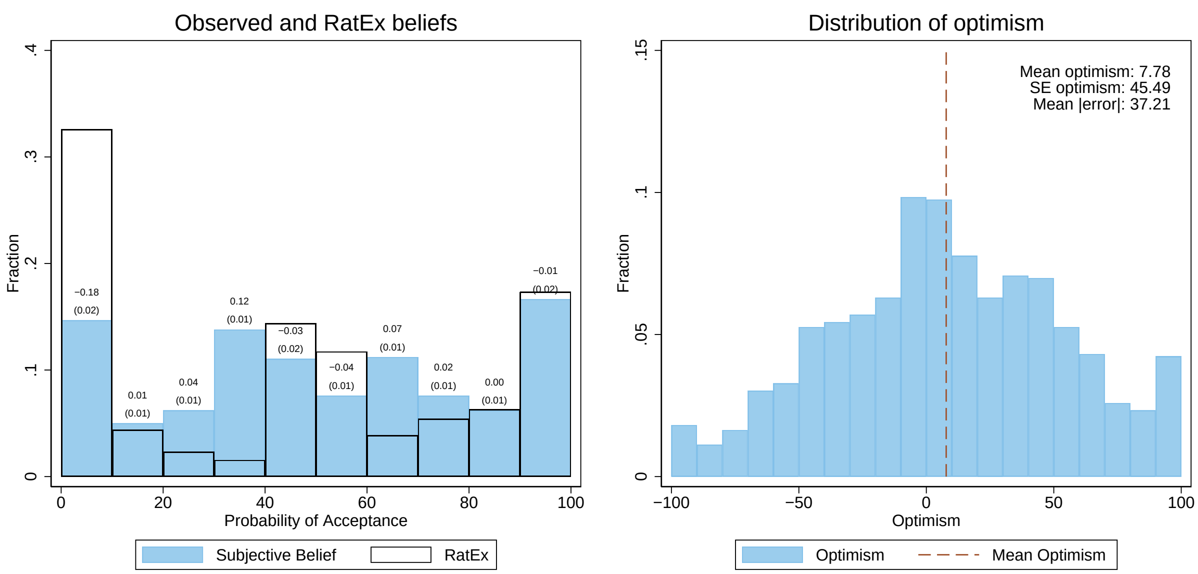
We next document respondents' beliefs about admissions chances and compare them to objective measures of admissions probabilities. We define $optimism_{ija}$ as the difference between i's reported subjective belief about his admissions chance at j under application a, $\hat{p}_{ija}$, and the rational-expectations chance p_{ija}:

$$optimism_{ija} = \hat{p}_{ija} - p_{ija}$$

The survey asked respondents about their beliefs for schools ranked first and second on two hypothetical applications, for a total of four elicited beliefs per respondent. Since some respondents declined to answer some questions or were asked about schools to which they could have been admitted outside the centralized process, we obtained a total of 1,159 elicited beliefs about admission to some school j under an application that listed j. We chose hypothetical applications that contained a mix of nearby schools, high-performing schools, and popular schools at the district level. The distribution of rational expectations admissions probabilities for the hypothetical applications is similar to the distribution of rational expectations probabilities for the actual applications that households in our sample submitted. See Online Appendix Figure A2 for the distribution of rational expectations probabilities in hypothetical and submitted applications.

The survey elicited subjective probabilities in bins with widths of 10 percentage points (1 to 10%, 11 to 20%,..., 91-100%). For second-ranked options, the survey elicited beliefs conditional on non-admission to the first ranked option.[11] To facilitate graphical comparison between rational expecations and subjective probabilities, we place the (conditional) rational expectations probabilities in the same set of bins as the subjective probabilities. When computing averages of subjective expectations and differences between rational expectations and subjective expectations, we set subjective expectations to the midpoint of the reported bin.

Figure 2: Distribution of beliefs and optimism



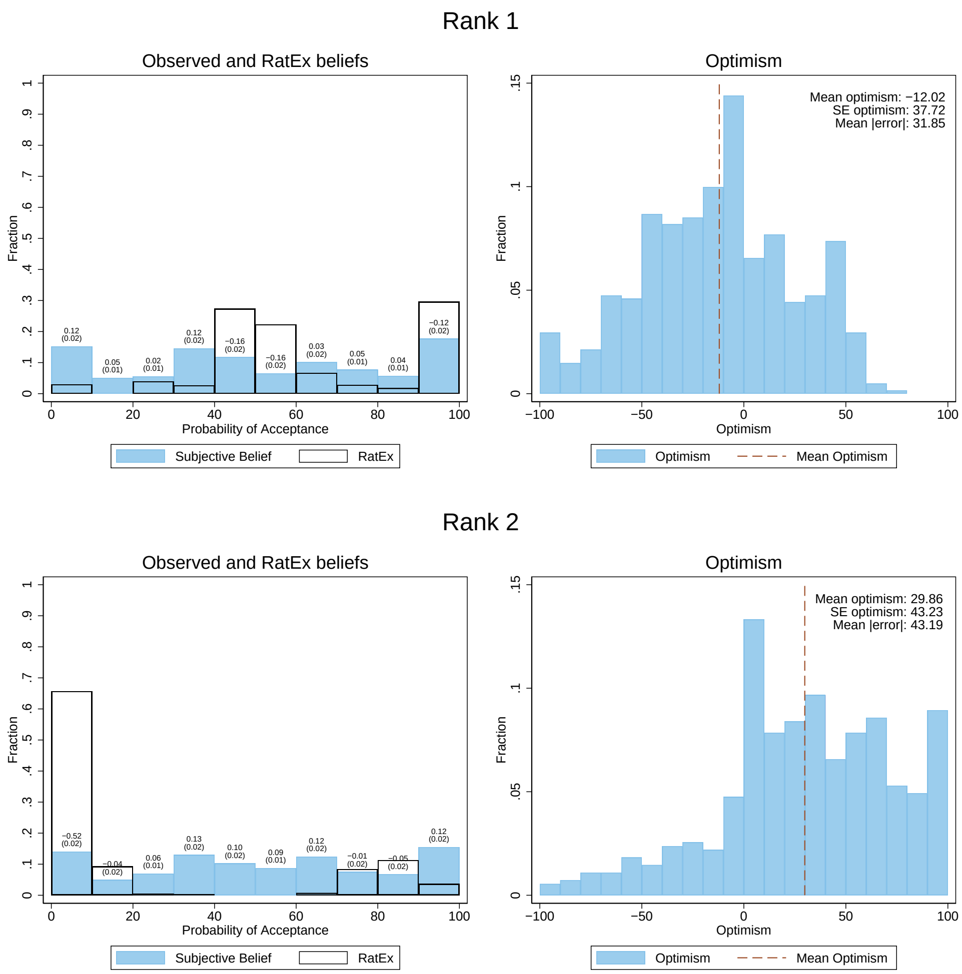
Notes: N=1,159. Left panel: distribution of subjective and rational expectations assignment probabilities. Right panel: distribution of optimism. Bars show shares of population within bins of width 10. Red line indicates mean of the distribution. In both panels, beliefs for second-ranked options are conditional on non-admission to the first-ranked choice. Text in the left panel indicates the gap between rational expectations and observed beliefs with standard errors clustered at the respondent level in parentheses below. Red line in the right panel shows the distribution mean.

The left panel of Figure 2 plots the distribution of rational expectations and subjective beliefs for the sample of hypothetical applications. The text above each bar displays the difference between the

[11]The 2017 survey also included separate categories for 'at most 1%' and 'at least 99%.' For cross-year consistency we aggregate the 2017 survey to 10 point bins as in the 2015 survey when conducting descriptive analysis.

share of subjective beliefs observations and the share of rational expectations beliefs observations in the bin. Many fewer respondents believe they have very low chances of admission than actually do. 33% of all elicited probabilities had rational expectations values in the lowest range, but respondents reported beliefs in this range in only 15% of cases.

Figure 3: Distribution of beliefs and optimism by application rank



Notes: N=1,159. Upper panel: beliefs for first-ranked applications. Lower panel: beliefs for second-ranked applications. Left panel: distribution of observed and rational expectation chances. Right panel: distribution of optimism. Bars show shares of population within bins of width 10. Beliefs for second-ranked options are conditional on non-admission to the first-ranked choice. Text in the left panel indicates the gap between rational expectations and observed beliefs with standard errors clustered at the respondent level in parentheses below. Red line in the right panel shows the distribution mean.

The right panel of Figure 2 plots the distribution of (conditional) optimism in the sample of hypothetical applications. Respondents overestimate their conditional admissions chances by 8 percentage points on average, and the spread around this value is wide. The mean absolute error in conditional beliefs is 37 percentage points.

Optimism is systematically related to rank. Figure 3 shows the distribution of beliefs and optimism by submitted rank. Households are an average of 42 percentage points more optimistic about second-ranked options than first ranked options, for which optimism values are centered just below zero. This reflects a large decline in rational expectations probabilities between the first and second ranked choices coupled with almost no change in subjective beliefs. The observed distribution of optimism suggests beliefs to not correspond to rational expectations, and that a realistic model of belief errors should allow for systematic variation by rank. We return to this point in section 4.

3.6 Validating belief and preference data

Survey results may provide flawed measures of inputs to the application process. One concern is measurement error. Respondents may report noisy or systematically biased measures of their true beliefs and preferences. A second concern is ex post changes in beliefs or preferences. Our survey took place after the realization of lottery outcomes. Students may adjust reported beliefs to reflect what they have learned from lottery outcomes, or may revise their preferences ex post in response to placement outcomes. A third concern is private information. If our model of the assignment process is incomplete and students have information about their application portfolio or the assignment mechanism that we do not, we may record accurate subjective beliefs as errors because our rational expectations benchmark is wrong.

This section presents descriptive evidence on each of these issues. We first consider ex post updating in response to placement outcomes. Columns 1 and 2 in Panel A of Table 3 show how placement outcomes relate to reported preferences. The outcome in both columns is an indicator variable for reporting the first-listed school on the application as the most-preferred school in our survey. The independent variables in column one are the rational expectations admissions chances and an indicator for placement in the first choice school. We fail to reject a null of zero placement effect (p=0.560). The second column adds controls for subjective beliefs. We fail to reject the null that coefficients on placement and subjective beliefs are jointly zero (p=0.658). This suggests a limited role for ex-post revision to reported preferences in response to placement.

We next consider private information and belief updating in response to placement. Columns three through five in Panel A of Table 3 describe the relationship between rational expectations beliefs, subjective beliefs, and application outcomes. Let $place_{i1}$ be an indicator variable equal to one if a student is placed in his or her first-listed school on the choice application, p_{i1a^*} be the measured rational expectations admissions probability at that school for observed application portfolio a^*, and $\hat{p}_{i1a^*}$ be i's reported subjective belief. If our model of the assignment mechanism is accurate and students do not update beliefs in response to placement outcomes,

$$E\left[place_{i1}|p_{i1a^*}=p,\hat{p}_{i1a^*}=s\right]=E\left[place_{i1}|p_{i1a^*}=p\right]=p.$$

We test this restriction using linear probability specifications of the form

$$place_{i1}=\alpha_0+\alpha_1 p_{i1a^*}+\alpha_2\hat{p}_{i1a^*}+e_i.$$

Under the null hypothesis of an accurate assignment model and no updating, we expect $\alpha_0=0$, $\alpha_1=1$, and $\alpha_2=0$. We would expect to reject the null if respondents had private information about placement probabilities, if respondents updated their beliefs in response to placement, or if we mis-specified our model of rational expectations chances.

Column three shows results from a linear probability specification in the full sample of ninth grade choice participants in which the outcome is first-choice placement and the only covariates are our rational expectations belief measure and a constant. We estimate a coefficient of 0.990 on rational expectations beliefs and an intercept of approximately zero. We fail to reject the joint null of zero constant and slope of one (p=0.183). Column 4 repeats this test in the sample of surveyed school choice participants for whom beliefs about first choice schools are available (N=186). The slope is again close to one and the intercept close to zero, and we cannot reject the joint hypothesis that our rational expectations estimates model is correct (p=0.594). Column 5 adds subjective beliefs to the regression. We fail to reject the null hypothesis that our rational expectations model is correct and that conditional on rational expectations beliefs, subjective beliefs have no effect on placement (p=0.460). We also fail to reject the alternate null that the subjective beliefs coefficient itself is zero (p=0.203). Our findings suggest that our rational expectations values accurately reflect the placement process, and that the effect of placement outcomes on reported beliefs is limited.

Finally, we consider whether subjective beliefs predict high-stakes choices. Figure 4 and Panel B of Table 3 report evidence indicating that subjective beliefs predict choice but that, conditional on subjective beliefs, rational expectations beliefs do not. Column one in Panel B reports results from the linear regression of an indicator for listing the most-preferred school first on the school choice application on subjective admissions beliefs. The sample is the group of students who submit a school choice application and for whom we have an elicited belief about admissions probabilities at the most preferred school when it is ranked first. The intuition is that a student who believes placement at his most-preferred school is more likely will be more likely to list that school first.

Table 3: Subjective vs. RatEx beliefs and application behavior

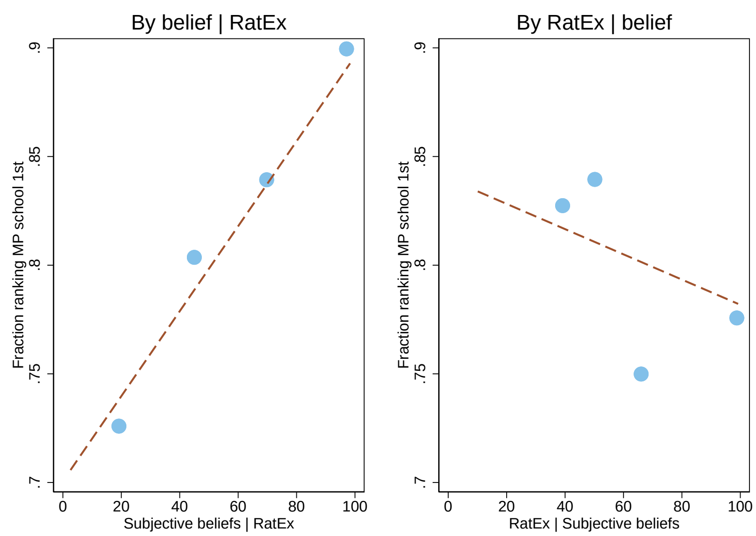
A. Testing survey quality					
	(1) State 1st listed as MP	(2) State 1st listed as MP	(3) Placed	(4) Placed	(5) Placed
Subjective belief		0.073			0.127
		(0.104)			(0.099)
RatEx	−0.072	−0.084	0.990	0.956	0.927
	(0.158)	(0.160)	(0.027)	(0.094)	(0.095)
Placed	0.047	0.041			
	(0.080)	(0.081)			
Constant	0.723	0.694	−0.005	0.057	0.007
	(0.090)	(0.098)	(0.023)	(0.080)	(0.091)
Dep. var. mean	0.706	0.706	0.616	0.634	0.634
Model test	0.560	0.658	0.183	0.594	0.460
N	186	186	2,101	186	186
B. Beliefs and application choices					
	(1) Rank MP 1st	(2) Rank MP 1st	(3) Place MP	(4) Place MP	(5) Place MP | Rank MP 1st
Subjective belief	0.189	0.196	0.304	0.206	0.151
	(0.095)	(0.097)	(0.120)	(0.120)	(0.123)
RatEx		−0.056		0.797	0.920
		(0.138)		(0.135)	(0.129)
Constant	0.704	0.734	0.368	−0.060	0.009
	(0.063)	(0.094)	(0.077)	(0.096)	(0.109)
Dep. var. mean	0.805	0.805	0.530	0.530	0.644
Model test		0.687			0.453
R^2	0.087	0.079	0.088	0.220	0.317
N	164	164	164	164	132

Robust standard errors in parentheses. Panel A sample is students for whom we observe beliefs about first listed schools, except (A3), which is the entire universe of first-listed schools for students not applying to their neighborhood school. Panel B sample is students for whom we observe beliefs about first listed schools and covariates. Regressions in Panel B contain de-meaned controls for year, SES quintile, race, gender, and whether a student has a continuation option at either Achievement First or Engineering and Science University Magnet. Subjective belief are observed subjective belief probabilities (on 0-1) while RatEx reflect rational expectations chances of admission. Placed is an indicator for placement during the initial lottery. Model test displays p-values for a variety of statisitcal tests: (A1) Placed = 0 (A2) Subjective belief = 0, Placed = 0 (A3-A4) RatEx=1, constant = 0 (A5) Subjective belief = 0, RatEx=1, constant = 0 (B2) RatEx=0 (B5) Subjective belief = 0, RatEx = 1. Appendix Table A4 reports alternate versions of the model tests in Panel A that condition on students' most-preferred and first-listed schools for columns 1 and 2 and colums 3 through 5, respectively.

We find an economically large and statistically significant relationship between subjective beliefs and application behavior. A decrease in subjective beliefs corresponding to one standard deviation of the first-ranked-school optimism distribution (38 percentage points) raises the probability a respondent lists a non-most-preferred school first by 7 percentage points, or 37% of the sample mean rate. Effect size is unchanged when we add controls for rational expectations beliefs (column 2). The effect of rational expectations chances are close to zero. Figure 4 shows binscatter plots for each bivariate relationship (conditional on the other). Rates at which students rank their most preferred school first rise by 17 percentage points from the bottom quartile of the subjective belief distribution to the top quartile.

We note that this analysis is non-experimental. These patterns could arise through channels other than a causal effect of beliefs on reports to the mechanism. One possibility is that preference intensity might be positively correlated with subjective beliefs, but not rational expectations beliefs. Our findings are strongly suggestive but not definitive evidence (of the type that might come from a randomized informational intervention) that subjective beliefs affect reports to the mechanism.

Figure 4: Fraction listing most-preferred first by subjective and RatEx beliefs



Notes: N=164. Points are binned means within quartiles of belief type listed in title. Means and fitted lines are obtained using regressions of the dummy for listing the most-preferred school first on the listed type, controlling for year, other belief type, SES quintile, race, gender, and whether a student has a continuation option at either Achievement First or Engineering and Science University Magnet. Covariates are set to mean values.

Because subjective beliefs influence application behavior, they affect placement. Columns three and four of Panel B report specifications that parallel those in columns one and two but with placement in the most-preferred school as the outcome. A one-standard deviation increase in subjective beliefs corresponds to an 8 percentage point increase in the rate students are placed in their most-preferred school (column 4). The final column of Panel B repeats the model test from Column 3 of Panel A for the set of individuals who rank their most-preferred degree first. We again fail to reject the joint null that the coefficient on rational expectations is one, the constant is zero, and the coefficient on subjective beliefs is zero at conventional levels (p=0.453). We interpret findings from Panel B of Table 3 as evidence both that subjective beliefs are important in choice and that our survey recovers credible measures of these beliefs.

3.7 Information acquisition and the correlates of belief errors

Though students use the information the district provides, they do not provide accurate reports about how the mechanism works. 89% of households report using some administrative information source, defined here to include a visit to a school or choice fair, reading the choice catalog or choice website, or talking to a counselor.[12] Table 4 presents the fraction of students who correctly answer questions about the ordering of priority groups and the role of rank in the choice mechanism. Only 10.8% of respondents correctly identified the neighborhood priority group as being preferred to the sibling priority group, and only 20.6% correctly stated that a student rejected from her first choice school has a (weakly) lower chance of admission at her second choice school than if she had ranked the second choice school first. There were three possible responses to each question, so correct answer rates are worse than under random guessing, and we can reject the null that respondents perform as well as random guessing at the 1% level in both cases. 3.4 percent of respondents answer both questions about the choice mechanism correctly. Despite not understanding how the mechanism works, only 5% of respondents describe the choice process as difficult.

[12] Online Appendix Table A6 displays the fraction of students who reported using different resources to inform their school choice decision.

Table 4: Difficulty of process and understanding of choice rules

	All	High SES	Low SES	p-value
Process difficult	0.053	0.031	0.057	0.347
Understand priorities	0.108	0.094	0.110	0.825
Understand ranking penalty	0.206	0.203	0.207	0.868
Understand both	0.034	0.031	0.034	0.719

Notes: Columns are samples. 'High SES' ($N = 55$), corresponds to respondents in the bottom quintile of census tract poverty rate while 'low SES' ($N = 362$) corresponds to respondents living in the remaining census tracts. Table reports shares of students who responded correctly to questions about priority ordering and the importance of the submitted rank to admissions outcomes, respectively. Respondents who answered 'I prefer not to answer' are coded as not understanding the mechanism; recoding these values as missing or correct does not affect conclusions that results are worse than random guessing. 'p-value' tests the regression-adjusted difference between high and low SES samples, controlling for year, race, and gender.

Compared to the relationship between optimism and application rank, other correlates of belief errors are relatively weak. Table 5 presents results from regressions optimism and absolute errors on student characteristics and descriptors of household interactions with the choice process. All specifications include controls for hypothetical application rank and an indicator for whether the student had sibling priority at the hypothetical school, and year fixed effects. Panel A shows that respondents who answer both mechanism questions correctly are 18 percentage points less optimistic on average. As in Figure 3, optimism is much greater at second-ranked schools. It is lower for the small share of respondents with sibling priority.

Panel B reports results from regressions with controls for preferences and participation in the school choice process. Participation in choice has a small and statistically insignificant relationship with both optimism and absolute error. Optimism is higher at most-preferred schools (first column), but this relationship is due to the negative correlation between preferences and RatEx admissions chances, not to any correlation between preferences and subjective beliefs. Controlling for RatEx chances, the relationship between preferences and optimism disappears (second column). Panel C shows that demographic variables are weakly correlated with belief errors. Poorer students may be have somewhat higher rates of large absolute error and lower optimism, but we cannot reject the null of no effect at conventional levels.

Additional analyses ask how strategic play, the respondent's relationship to the student, and the respondent's use of information sources relate to belief errors. Online Appendix Table A5 shows that belief errors are weakly related to strategic play, to whether the respondent helped with or correctly recalled the application, and to whether the respondent is the student's mother (the most common relationship) or not. Online Appendix Figures A5 and A6 provide further evidence that the distributions of subjective beliefs and optimism are similar across splits by respondent involvement in choice and correct recall of the submitted application. Finally, Online Appendix Table A6 shows that relationships between belief errors and the use of specific information sources are also weak. Our findings are consistent with a story of application behavior in which households know they should strategize on their schooling applications, but have trouble learning how the mechanism works, even when they are involved in the application process. Applicants may seek out information about admissions chances on the basis of participation or preferences, but the effects of this search appear to be second order relative to their misunderstanding of the mechanism.

Table 5: Correlates of belief errors

	A. Qualitative responses		B. Preference & participation				C. Demographics	
	Optimism	Abs. Error	Optimism	Optimism	Abs. Error	Abs. Error	Optimism	Abs. Error
Hypothetical rank 2	41.7	11.4	41.5	6.4	11.5	10.7	42.0	11.4
	(1.3)	(1.7)	(1.3)	(1.9)	(1.7)	(1.8)	(1.3)	(1.7)
Have priority	-24.4	5.6	-26.6	-4.5	5.3	5.7	-23.1	6.1
	(7.3)	(4.8)	(8.1)	(7.6)	(5.2)	(5.2)	(7.8)	(5.1)
Understand mechanism	-18.2	0.5						
	(6.8)	(2.7)						
Most preferred			9.9	2.7	-0.3	-0.4		
			(3.1)	(2.3)	(1.8)	(1.7)		
Filed app			-3.0	-2.9	-3.4	-3.4		
			(5.2)	(3.9)	(2.5)	(2.4)		
RatEx				-0.9		-0.0		
				(0.0)		(0.0)		
Tract poverty rate							-12.3	8.5
							(16.1)	(8.6)
Black							-3.7	0.8
							(4.0)	(2.0)
White							-0.5	-4.9
							(5.9)	(3.3)
Female							0.1	-0.5
							(3.6)	(1.8)
N	1,159	1,159	1,126	1,126	1,126	1,126	1,149	1,149

Standard errors in parentheses. Errors clustered at the student level. Sample sizes change across panels due to covariate availability. All regressions include year fixed effects and exclude neighborhood schools from the sample. See section 3.7 for additional description.

3.8 How the descriptive analysis informs modeling choices

We use three stylized facts from our descriptive analysis to inform modeling decisions. First, households behave strategically, trading off preference intensity against admissions chances. Second, the admissions probability beliefs that students use to inform these tradeoffs are often in error. Third, belief errors vary with submitted rank and priority group, but have a weaker relationship with participation in choice and preferences over schools. These facts suggest a model of optimizing behavior in which students are misinformed about admissions chances. This contrasts with 'naive' behavior in which students simply list preferences in order and suggests that a realistic model of beliefs should allow for heterogeneity by position in the application portfolio. There is less evidence that strategic information gathering on more-preferred schools or by students who participate in choice drives differences in belief errors. This motivates a choice to abstract from a model of information acquisition.

4 Model

4.1 Student preferences

Our model consists of four stages. First, applicants learn their preferences over schools and the costs of applying to schools. Second, they choose whether to participate in the school choice process and, if they participate, what report to submit. Third, the lottery runs and participants receive placements. Fourth, students who receive placements choose whether to enroll in the placed school or to decline their placement. Students who decline a placement or do not receive a placement have the option to either enroll in their zoned neighborhood school or leave the district. Students who are enrolled in a K12 school may also choose to remain in that school.

Students $i \in I$ have underlying preferences over schools $j \in J$ according to:

$$u_{ij} = \delta_j + X_{ij}\beta + \epsilon_{ij},$$

where δ_j are a full vector of school dummies and X_{ij} are observed school and student characteristics. The X_{ij} include distance to the school from home $distance_{ij}$, and a household-level indicator for low SES. The errors ϵ_i are distributed according to

$$\epsilon_i \sim MVN(0, \Sigma),$$

iid across households.

In practice, each student has exactly one zoned school at which he is guaranteed a position.[13] Each student therefore has an outside option u_{i0} which consists of the choice between attending this school and leaving the district. We normalize the value of this outside option: $u_{i0} = 0$. Students who wish to attend their zoned school are encouraged not to submit a lottery application, and it is not possible to select one's own zoned school in the online version of the application. Therefore one's own zoned high school is part of the outside option.[14] Because the relative value of placing in an inside school depends on the identity of the zoned school and the distance to it, we control for these characteristics.[15] The covariance matrix Σ is unrestricted.

Once a student is placed in school j, he has the option to decline his placement. At the time of this decision, students receive a shock to preferences for j and for the outside option, giving a utility

$$U_{ij} = u_{ij} + \epsilon_{ij}^e$$

where the enrollment-time shock ϵ_{ij}^e has an extreme value distribution with scale parameter $\frac{1}{\lambda}$. The probability of accepting an offer is therefore

$$P(u_{ij} + \epsilon_{ij}^e > \epsilon_{i0}^e) = \frac{\exp(\lambda u_{ij})}{1 + \exp(\lambda u_{ij})}.$$

The expected value of school j at the time of matriculation is given by

$$v_{ij} = E(\max\{U_{ij}, U_{i0} | u_{ij}\}) = \frac{1}{\lambda} \log(1 + \exp(\lambda u_{ij})).$$

To permit nonparticipation and short application lists, we allow for a cost of receiving a placement. If i receives a placement in any inside school j, he receives a (possibly negative) payment

$$b_i \sim N(\mu_b, \sigma_b^2).$$

We interpret b_i as the cost of the actions i must take to accept or decline a placement. It reflects the real and psychological costs of finding and getting in touch with the school placement office or assigned school.

Students make participation and application decisions to maximize their expected utility given their subjective beliefs about placement chances. Let $\tilde{p}_{ija}$ denote i's subjective estimate of the

[13] There are two such schools: Wilbur Cross High School, and James Hillhouse High School.

[14] One may apply to the "opposite" zoned school via the lottery.

[15] That is, we include in X_{ij} an indicator for i's zoned school and the distance to the zoned school. Including zoned-school dummies and distance-to-zoned-school in each inside option is equivalent to parameterizing the outside option with those terms.

probability that he will be placed in school j if he submits report a to the mechanism.[16] Students for whom $a = \emptyset$ are those who do not participate in school choice. i's decision solves

$$\max_a \left(\sum_{j=1}^{J} \tilde{p}_{ija}(v_{ij} + b_i) \right).$$

The use of subjective beliefs for expected utility maximization is our main innovation relative to Agarwal and Somaini (2018) and Calsamiglia and Güell (2018), or Abdulkadiroğlu et al. (2017b). These papers impose rational expectations beliefs and/or stipulate that agents follow 'rule-of-thumb' approaches to portfolio choice. Our approach is consistent with findings from survey data that strategic behavior is common but that beliefs are often wrong. To explore the importance of the analysis of subjective beliefs for policy conclusions, we estimate additional specifications that impose rational expectations.

An alternative modeling approach is to consider only the application decision, treating the choice to accept a placement as exogenous. In this model, $b_i \equiv 0$, and the value of a placement at j is given by $v_{ij} = u_{ij}$. We estimate this alternate model and report details in Online Appendix C. We prefer our main model because the choice to accept a placement contains information on preferences that we would like our estimates to incorporate. Descriptive evidence shows that applicants are more likely to accept placements at more-preferred schools. See Table A7 in the Online Appendix.

4.2 Beliefs

Inaccurate beliefs about p_{ija} may arise because students mis-estimate $rsp_{ij}(a)$ or the distribution of cutoff values π_j. Mistaken beliefs about these two quantities can arise from similar thought processes. For example, households who do not understand how priority groups and submitted rankings jointly determine rsp_{ij} will have inaccurate beliefs about their own values of $rsp_{ij}(a)$ and also about π_j even given full knowledge of other households' submitted applications.

Errors in beliefs about π_j and rsp_{ij} sum to alter beliefs about admissions probabilities. Let $r\tilde{s}p_{ij}(a)$ and $\tilde{\pi}_{ij} = \pi_j + \Delta\pi_{ij}$ be household i's beliefs about report-specific priority and the cutoff score for admission, respectively, with $\Delta\pi_{ij} \in \mathbb{R}$. Then

$$\tilde{p}_{ija} = Pr(z_{ij} \leq \pi_j - rsp_{ij}(a) - \mathit{shift}^*{}_{ij}(a),\ z_{ij'} > \pi_j' - rsp_{ij'}(a) - \mathit{shift}^*{}_{ij'}(a) \text{ for all } j' \text{ahead of } j \text{ under } a)$$

[16] Subjective belief $\tilde{p}_{ija}$ may differ from reported subjective belief $\hat{p}_{ija}$ due to measurement error in $\hat{p}_{ija}$. We return to this point below.

where

$$shift^*_{ij}(a) = \pi_j - \tilde{\pi}_{ij} - (rsp_{ij}(a) - r\tilde{s}p_{ij}(a)).$$

The $shift^*_{ij}(a)$ term incorporates errors in beliefs about both rsp_{ij} and π_j. Rather than trying to distinguish between these two closely related sources of error, our empirical model takes a parsimonious approach and focuses on the $shift^*_{ij}$ term itself. This choice does not restrict the distribution of deviations of subjective beliefs from rational expectations values.

Our survey contains observations of beliefs for some application portfolios. Because the number of possible portfolios is very large, it is not feasible to survey families about each possible submission. We therefore use our survey data to estimate a flexible model of belief errors. We allow people to have mistaken beliefs about their priority or, equivalently, about schools' cutoffs, and about the role of priority and the rank of applications. For any application a that ranks school j in the rth place, we let i's error be given by

$$shift^*_{ij}(a) = shift_{ijr}$$

for some $shift_{ijr} \in \mathbb{R}$. Taking the individual-rank-school triple as given, belief errors do not depend on other features of the application. This assumption reduces the dimensionality of unknown beliefs while allowing for relevant misperceptions and mistakes. We let

$$shift_{ijr} = \eta_i^0 + \eta_i^r(r - \overline{r}_j) + \eta_i^{priority}\left(priority_{ij} - \overline{priority}_j\right) + \eta_{ij} + \eta_{ijr} \tag{1}$$

denote i's error about his own admissions ranking. Here, r is the rank of j on application a for student i, and $\overline{r}_j$ is the average rank of applications. Similarly, $priority_{ij}$ is $i's$ priority at j and $\overline{priority}_j$ is the average in the data. This functional form nests several relevant cases. For example, under the New Haven mechanism, $\eta_i^r = 0$ means students understand how priority groups affect choices, while $\eta_i^r = -1$ if students do not believe score depends on rank, as if a DA mechanism were used. $\eta_i^{priority} = -2$ corresponds to the case where students' beliefs about admissions probabilities do not change with changes in their priority group, while η_i^0 captures individual-specific optimism or pessimism and η_{ij}^0 captures idiosyncratic person-school error.

We assume $\eta_{ij} \sim N(0, \sigma^2_{\eta_{school}})$ iid across j, and $\eta_{ijr} \sim N(0, \sigma^2_{\eta_{school\times round}})$ iid. The remaining terms are distributed according to

$$(\eta_i^0, \eta_i^r, \eta_i^{\text{priority}}) \sim N(\overline{\eta}, \Sigma^\eta).$$

We let $\sigma_{\eta_0}, \sigma_{\eta_{pri}}$, and $\sigma_{\eta_{round}}$ denote the diagonal components of Σ^η. This specification allows us to capture many types of errors. For example, people who misunderstand priorities may also misunderstand the importance of rank. We allow for separate parameters for students from high- and low-SES backgrounds to facilitate flexible cross-group comparisons. In addition, we estimate separate models for 2015 and 2017 because the $rsp_{ij}(a)$ are constructed differently in the New Haven and Boston mechanisms and units have different interpretations. See section 2.2 for details.

One limitation of our approach is that it maintains the assumption that beliefs are independent of preferences. In particular, because we do not model households' search for information, we cannot address counterfactuals in which information acquisition behavior may differ endogenously. Though endogenous information acquisition is surely a first-order issue in many settings, there are several reasons to think its importance may be more limited here. First, our main counterfactuals focus on the DA mechanism, in which optimal play does not require knowledge of admissions chances. Second, survey evidence suggests that the costs of information acquisition on the margin may be prohibitively large in our setting. See sections 3.7 and 3.8 for a discussion. We leave the challenge of modeling information acquisition to future research.

4.3 Modeling institutional details

We adapt our model to incorporate several idiosyncratic features of the New Haven setting. These affect small numbers of students. First, at the two K12 schools (Achievement First and Engineering and Science), current eighth graders have the option to continue their enrollment without participating in the choice process. There are 179 such students in 2015 and 189 in 2017. We incorporate the option to stay in the current school into the outside option, and allow outside option value to vary with the identity of the current school for these individuals. Second, the school aimed at students expelled from other schools (Riverside) accepts applications through the centralized system but makes offers on a different day than other schools and never rejects applicants. We model applicants to this school as having the option to enroll if they want to, so that they are choosing between their zoned school, their placed school (if they have one and it is not Riverside), and Riverside at the enrollment stage. We observe 22 students placed at this school in total over both years. Third, households may apply to specific programs within an arts-themed school (Co-Op Arts). We treat Co-Op as one school in our analysis.

5 Estimation

We use a Bayesian Markov-Chain Monte Carlo (MCMC) procedure to estimate the model and sample from the posterior distribution of counterfactual outcomes. Similar methods have been used successfully in the marketing and industrial organization literatures to model consumers' demand for goods (McCulloch and Rossi, 1994) and have been applied successfully to centralized school choice (Agarwal and Somaini, 2018). Our strategy extends these methods to make use of surveyed beliefs and preferences as well as data on the decision to accept or decline a placement. We provide a sketch of our approach here with details in Online Appendix B.

We use a two-step procedure. In the first step, we estimate the distribution of market-clearing cutoffs at each school, which determine the rational-expectations chances of admission at each school conditional on a priority vector and a report. Second, we use the survey and administrative data together with the distribution of market-clearing cutoffs to estimate the parameters of the model. To do so, we use data augmentation to pick utility vectors, beliefs, cost terms b, and measurement error terms for each individual consistent with their choices. If individuals are surveyed, these terms must be consistent with their survey responses as well. We introduce prior distributions for the model parameters, and use MCMC in order to sample from the posterior distribution of parameters conditional on the data. In order to obtain distributions of outcomes under counterfactuals, we simulate alternative policies at many points drawn from this posterior distribution. This approach allows us to model belief errors even for non-surveyed individuals.

In summary, the survey is used in three ways. First, it is used to estimate the parameters of the belief model. Intuitively, the survey plays the critical role in pinning down the distribution of belief errors, but belief errors help rationalize observed choices for both surveyed and non-surveyed students. Second, the survey imposes restrictions directly on beliefs of surveyed households. Surveyed households' values of $shift_{ijr}$, together with their belief measurement error terms, must be such that their reported subjective beliefs lie in the intervals that they declared. Third, the survey constrains the preferences of surveyed households. The two reported most-preferred schools must give the highest utility up to measurement error.

5.1 Recovering preference and belief parameters

Before we describe the estimation procedure in detail, we discuss the restrictions implied by households' optimal application decisions, accept/decline decisions, and reported first and second choices, as well as the normalizations we make.

5.1.1 Optimality of applications

Let $v_i = (v_{i1}, \ldots, v_{iJ})$ denote the vector of inclusive values of admission to each of the J schools, and let $p_i(a)$ denote the vector of i's subjective beliefs about admissions chances under report a. Agarwal and Somaini (2018) observe that a report a is optimal for agent i if and only if $v_i \cdot p_i(a) \geq v_i \cdot p_i(a')$ for all reports a'. Hence, given the matrix $\Gamma_i = (p_i(a) - p_i(a_1), \ldots, p_i(a) - p_i(a_N))$, a report is optimal if and only if $\Gamma_i'(v_i + b_i) \geq 0$.

Optimal applications depend on beliefs, which depend on the distribution of cutoffs. The model may therefore exhibit multiple equlibria. Conditional on a distribution over cutoff vectors, however, each household faces a single-agent decision problem. Because we estimate and condition on the cutoff distribution that occurred in the data, potential multiplicity is not a problem for estimation of beliefs or preferences.

5.1.2 Reported preferences

In the survey we elicit households' first and second choices if parents could choose any school, unconstrained by admissions chances. We allow for measurement error in elicited preferences: If i says that j_1 is the household's first choice, then

$$u_{ij_1} + \epsilon_{ij_1}^{survey} > u_{ij} + \epsilon_{ij}^{survey} \ \forall j.$$

Similarly, if j_2 is the household's second choice, then

$$u_{ij_2} + \epsilon_{ij_2}^{survey} > u_{ij} + \epsilon_{ij}^{survey} \ \forall j \neq j_1.$$

We assume the measurement error is drawn iid from a normal distribution:

$$\epsilon_{ij}^{survey} \sim N(0, \sigma_{survey}^2), \text{ iid.}$$

5.1.3 Reported beliefs

In addition, we allow for measurement error in reported beliefs. That is, if the household optimizes according to beliefs $shift_i$ and utility vector u_i, the elicited belief about admissions chances at school j is generated according to:

$$\tilde{p}_{ija}^{obs} = Pr(z_{ij} \leq \pi_j - rsp_{ij}(a) - \widetilde{shift}_{ijr_a(j)}, \ z_{ij'} > \pi_j' - rsp_{ij'}(a) - \widetilde{shift}_{ij'r_a(j')} \text{ for all } j' \succ_a j),$$

where $r_a(j)$ is the rank of j on application list a, and

$$\widetilde{shift}_{ijr} = shift_{ijr} + \tilde{\eta}_{ijr}.$$

We observe the interval I_{ija} in which $\tilde{p}_{ija}^{obs}$ lies. For example, if $0.1 \leq \tilde{p}_{ija}^{obs} < 0.2$ then the household would report $10 - 20\%$. In contrast to our descriptive analysis, we do not restrict the $\tilde{p}_{ija}^{obs}$ to take values equal to the midpoint of the reported interval.

We assume the measurement error is drawn iid from a normal distribution:

$$\tilde{\eta}_{ijr} \sim N(0, \sigma_{\tilde{\eta}}^2), \text{ iid.}$$

Importantly, $\tilde{\eta}_{ijr}$ has the same distributional form as the "true" error η_{ijr}, so functional form is not being used to distinguish the two.

We note that we model measurement error in reported beliefs as being independent of measurement error in reported preferences. This is consistent with our maintained assumption that true beliefs are independent of true preferences, and with evidence from section 3.7 on the weak relationship between elicited beliefs and preferences. However, it would fail if, for example, survey respondents who erroneously report particular schools as most-preferred are also more likely to erroneously report higher or lower beliefs. A feature of our data that facilitates estimation of measurement error is presence of multiple measures of beliefs and preferences. Survey reports contain noisy measures of beliefs and preferences, while the enrollment decision is noisy measure of preferences. Reports to the mechanism measure true preferences and beliefs.

5.1.4 Enrollment decision

If i accepts a placement in j, then we require $u_{ij} + \epsilon_{ij}^e > \epsilon_{i0}^e$. If i receives and declines a placement in j, we require $u_{ij} + \epsilon_{ij}^e < \epsilon_{i0}^e$.

5.1.5 Normalization

We have already imposed the location normalization $u_{i0} = 0$, but have not imposed a scale normalization. In the multinomial probit model and its extensions to school choice settings, it is conventional to normalize the scale of a coefficient of known sign, such as the coefficient on distance, β_{dist}. Without loss, we fix $\beta_{dist} = -1$.

5.1.6 Abstract likelihood

Although we do not directly evaluate the likelihood, it is instructive to consider the likelihood of an individual observation, conditional on the distribution of market-clearing cutoffs that was previously estimated. This likelihood is given by

$$\int_{\{u_i, b_i, \mathit{shift}_i \,:\, a_i \text{ is optimal}\}} Pr(\text{enroll}_i \mid \text{placement}_i, u_i, \theta)\, Pr(\text{survey}_i \mid u_i, b_i, \mathit{shift}_i)\, \mathrm{d}F(u_i, b_i, \mathit{shift}_i \mid \{X_{ij}\}_{j=1,\ldots,J}, dist_i, \theta),$$

where $Pr(\text{survey}_i \mid u_i, b_i, \mathit{shift}_i) = 1$ if household i was not surveyed, and surveyed households have

$$\begin{aligned} Pr(\text{survey}_i \mid u_i, b_i, \mathit{shift}_i, \theta) = & \prod_{j,r \,:\, \text{belief}_{ijr} \text{ elicited}} Pr(\mathit{shift}_{ijr} + \tilde{\eta}_{ijr} \in I_{i,j,r} \mid \mathit{shift}_{ijr}, \sigma_{\tilde{\eta}}) \\ & \times Pr\left(u_{ij_1} + \epsilon^{\text{survey}}_{ij_1} > u_{ij_2} + \epsilon^{\text{survey}}_{ij_2} > u_{ij_k} + \epsilon^{\text{survey}}_{ij_k}\ \forall k \notin \{j_1, j_2\} \mid u_i, \sigma^2_{\text{survey}}\right), \end{aligned}$$

with $I_{i,j,r}$ the reported interval, and j_1 and j_2 the reported first- and second-choice schools.

The application decision enters the likelihood via the region of integration. MCMC methods are convenient when it is difficult to directly compute this integral, as in our setting, but relatively easy to sample from conditional distributions of parameters, utilities, beliefs, and measurement error terms conditional on optimality and survey reports.

5.1.7 Prior distributions

We begin with prior distributions over the preference parameters and belief parameters. We place priors directly on β, Σ, μ_b, σ_b, and σ_{survey} as well as on the belief parameters separately by SES category. In order to minimize the priors' influence on our estimates, we choose diffuse priors, which we describe in Online Appendix B.

5.1.8 MCMC iteration

We iterate through a sequence of steps which consist of sampling from the conditional posterior distributions of utilities, utility shocks, beliefs, belief measurement error, application costs, and model parameters. We describe these steps in detail in Online Appendix B. The steps are standard Gibbs-sampler steps, with the exception of the updates to belief shift terms $shift_{ijr}$ and belief measurement error $\tilde{\eta}_i$. To update these parameters in turn we take a sequence of Metropolis-Hastings steps. Hence our procedure is an example of a "Metropolis-within-Gibbs" procedure.

To obtain our estimates we use a chain of 300,000 iterations. We estimate separate models by year. We discard the first half of the draws in order to allow for burn-in. Trace plots and PSRFs for parameter estimates are reported in Online Appendix Figures A7 through A19. Online Appendix Figures A20 through A25 report trace plots and PSRFs for welfare levels and differences. See Online Appendix B.9 for a discussion.

6 Results

6.1 Estimation results

Table 6 reports estimates and credible intervals for model parameters. For each parameter we show .025, .5, and .975 quantiles of the posterior distribution. The median may be taken as a point estimate. Panel A of Table 6 displays estimates of belief model parameters by household SES. Estimates from 2015 are in the left panel and estimates from 2017 are in the right panel. To interpret the magnitudes, note that that there is an interval of length 1 for each report-specific priority type such that if the cutoff lies in this interval, the type is rationed. Further interpretation depends on the mechanism that was used. In 2015, students were allocated via the New Haven Mechanism. Under this mechanism, placing a school one rank lower would increase report-specific priority by 1. Therefore, a value of $\overline{\eta}_{round}$ of -1 would mean that, on average, students believe that the impact of rank on report-specific priority is zero, as if the mechanism were deferred acceptance. In 2017 when the Boston mechanism was used, placing a school one rank lower would have increased report-specific priority by 2, so that $\overline{\eta}_{round} = -2$ would indicate that students believe the impact of rank on report-specific priority is zero.

Focusing first on idiosyncratic school and school-rank specific errors, we find that $\sigma_{\eta_{school}}$ and $\sigma_{\eta_{school \times round}}$ converge to values far from zero. The $\sigma_{\eta_{school}}$ are between 0.6 and 1.7 depending on SES category and year, while the $\sigma_{\eta_{school \times round}}$ are between 0.25 and 0.4 across each year-SES combination. These values are sufficiently large to lead to mistaken beliefs about the round in which the capacity constraint binds. Households also make errors that are systematically correlated with the round in which they apply to a school. Estimates of $\overline{\eta}_{round}$ near -2.0 in 2017 and -3.5 in 2015 indicate that, on average, households underestimate the impact of round by the full value of the round penalty (2017) or more (2015). Estimates of $\sigma_{\eta_{round}}$ indicate that there is substantial heterogeneity across households in the effects of round, particularly in 2015, but that most students substantially underestimate its impacts on placement chances. Round error parameters are similar across SES groups. We estimate the variance of belief measurement error $\sigma_{\tilde{\eta}}$ at 0.24 in 2015 and 0.22 in 2017. See Online Appendix Table A8 for estimates of the Σ^{η}.

Table 6: Parameter Estimates

	2015			2017		
	Quantile			Quantile		
Variable	0.025	0.5	0.975	0.025	0.5	0.975
A. Belief parameters						
$\sigma_{\eta_{\text{individual}}}$ (high SES)	8.207	9.420	11.246	5.952	6.499	7.092
$\sigma_{\eta_{\text{individual}}}$ (low SES)	7.960	8.437	9.036	5.725	6.083	6.540
$\sigma_{\eta_{\text{priority}}}$ (high SES)	0.897	2.065	3.665	3.506	4.226	5.105
$\sigma_{\eta_{\text{priority}}}$ (low SES)	2.429	2.592	2.941	1.417	1.964	2.676
$\sigma_{\eta_{\text{round}}}$ (high SES)	2.754	3.207	3.733	0.242	0.346	0.445
$\sigma_{\eta_{\text{round}}}$ (low SES)	2.848	3.033	3.246	0.239	0.297	0.373
$\sigma_{\eta_{\text{school}\times\text{round}}}$ (high SES)	0.368	0.404	0.449	0.287	0.315	0.352
$\sigma_{\eta_{\text{school}\times\text{round}}}$ (low SES)	0.236	0.252	0.267	0.307	0.324	0.341
$\sigma_{\eta_{\text{school}}}$ (high SES)	0.734	0.861	1.022	1.024	1.177	1.393
$\sigma_{\eta_{\text{school}}}$ (low SES)	0.554	0.612	0.682	1.553	1.646	1.715
$\sigma_{\tilde{\eta}}$	0.196	0.240	0.292	0.191	0.223	0.260
$\overline{\eta}_{\text{individual}}$ (high SES)	5.649	6.949	8.356	5.348	6.412	7.030
$\overline{\eta}_{\text{individual}}$ (low SES)	7.738	8.081	8.427	6.723	7.346	8.081
$\overline{\eta}_{\text{priority}}$ (high SES)	-2.729	-1.749	-0.618	3.132	3.714	4.426
$\overline{\eta}_{\text{priority}}$ (low SES)	-2.624	-2.090	-1.686	1.823	2.032	2.268
$\overline{\eta}_{\text{round}}$ (high SES)	-3.828	-3.309	-2.861	-2.178	-2.066	-1.983
$\overline{\eta}_{\text{round}}$ (low SES)	-3.773	-3.655	-3.526	-2.113	-2.027	-1.944
B. Preference parameters						
δ Achievement First Amistad HS (1)	-57.495	-22.027	-9.121	-40.442	-17.809	-8.542
δ Common Ground Charter (2)	-83.025	-31.758	-15.635	-42.231	-17.899	-8.382
δ Coop. Arts and Humanities (3)	-10.454	1.345	15.193	-5.711	1.904	11.046
δ Engineering & Science Univ. HS (4)	-48.314	-17.024	-5.106	-21.036	-6.850	0.822
δ High School in the Community (5)	-54.204	-20.490	-8.133	-35.630	-15.267	-7.061
δ Hill Regional Career (6)	-7.554	3.457	18.544	-2.167	4.705	15.133
δ Hillhouse (7)	-103.372	-41.025	-21.758	-39.212	-16.923	-7.657
δ Hyde School (8)	-46.031	-11.964	0.423	-22.614	-4.330	3.794
δ Metropolitan Business Academy (9)	-28.682	-8.737	2.449	-7.152	0.810	9.326
δ New Haven Academy (10)	-42.527	-15.330	-3.921	-22.289	-8.227	-0.400
δ Riverside Education Academy (11)	-141.956	-57.463	-32.398	-236.918	-110.249	-64.335
δ Wilbur L. Cross High School (12)	-44.486	-15.629	-3.896	-7.431	1.326	10.395
λ	0.001	0.003	0.005	0.002	0.004	0.006
μ_b	-563.357	-234.140	-143.674	-316.581	-155.808	-106.071
σ_b	14.107	23.199	55.295	32.485	48.429	98.637
σ_{survey}	9.994	18.079	45.349	6.279	10.157	21.715
1(default is Cross)	-25.817	-9.668	-3.727	-24.873	-11.077	-4.655
Distance to default	1.705	3.308	7.636	0.700	2.384	5.384
1(low SES)	5.403	12.822	33.038	-0.955	5.013	15.020
Achievement First	-112.925	-44.590	-25.937	-46.813	-21.153	-9.607
Engineering & Science	-135.556	-52.069	-26.177	-51.152	-19.083	-0.475

Notes: Quantiles of distribution of posterior mean for parameters listed in the rows. Panel A: belief model by student SES. 'High SES' is top quintile of SES distribution. Off-diagonal elements of covariance matrices reported in Appendix Table A8. Panel B: preference parameter estimates by grade. Coefficient on miles traveled is normalized to -1. Appendix Table A9 provides credible intervals for elements of the utility shock covariance matrices Σ. The coefficients on Wilbur Cross and Hillhouse apply only to students who are not zoned into these schools. The coefficient on the own zoned school is set equal to zero. Achievement First and Eng. & Sci. coefficients are for incumbent students at those K12 schools.

The main cross-year difference we observe in belief model parameter estimates is for the $\overline{\eta}_{priority}$, which are negative in 2015 and positive in 2017. Households underestimate the benefits of sibling priority in 2015, and overestimate the benefits in 2017. This may reflect the lesser role of sibling priority in determining report-specific priorities under the Boston mechanism relative to the New Haven mechanism.

Panel B of Table 6 presents estimates of preference parameters. To interpret the coefficients, recall that the coefficient on miles traveled is equal to -1 and that the mean utility of the 'no placement' outcome, which includes the choice to leave the district, is normalized to zero. First consider preferences for outside relative to inside options. The coefficient on 1(default is Cross) has a negative sign in both years, meaning that students zoned to Cross find schools of choice relatively less appealing. Of the two high schools, Cross draws from the higher-SES catchment zone and scores higher on accountability metrics. Students with the option to continue at Achievement First or Engineering & Science also find inside options less attractive on average. Low-SES students find inside options more attractive. Students farther from their default school find the inside option more attractive. We also observe differences in preferences across schools relative to the outside option. Mean utility is negative in both years for several schools, including Hillhouse (for out-of-zone students), Riverside (a school aimed at students with disciplinary issues), and Achievement First (the no-excuses charter).

We also find evidence of horizontal differentiation across schools. Credible intervals for six schools span zero in at least one year. Strong preferences for schools specializing in arts, science, or business come in large part from high values of the school-student match terms ϵ_{ij}. For example, the 90% credible interval for the standard deviation of the Co-op Arts preference shock is $(16, 50)$ in 2015 and $(13, 31)$ in 2017. The 90% credible interval for the standard deviation of the Metropolitan Business preference shock is $(15, 46)$ in 2015 and $(12, 30)$ in 2017. See Online Appendix Table A9.

On average, receiving a placement is costly, with μ_b equal to -234 in 2015 and -156 in 2017. Dispersion around this central value is limited, with σ_b equal to 23 in 2015 and 48 in 2017. Measurement error in reported preferences has a standard deviation of 10 to 18 miles traveled, depending on year. Finally, scale parameter λ takes values between 0.003 and 0.004 depending on year. Our scale parameter estimates indicate that distance plays a relatively small role in decisions to accept or decline placements. As a result, our estimates of costs and mean utilities are large in distance terms.

6.2 Welfare analysis and counterfactual simulations

We now turn to an analysis of household welfare and test scores under observed and counterfactual policies. Our procedure estimates the joint distribution of parameters and utilities. Using this distribution, we are able to compute each household's expected welfare according to its utility and the true rational-expectations admissions chances under the application it submitted. We compute average utility at every 10th iteration along the Markov chain after the burn-in period. Because the coefficient on distance is normalized to -1, welfare is measured in units of (fewer) miles traveled.

We consider two sets of policy counterfactuals. The first set considers changing the assignment mechanism to DA. As a benchmark, we consider the truthful DA mechanism (henceforth 'DA'), in which applicants can list each school. The optimal strategy for participating households is to truthfully report their preferences. Households need not form beliefs about placement chances to make optimal reports under this policy, provided they trust the recommendation to play truthfully.

Districts may prefer to keep lists short if they think, e.g., that longer lists make the application process too challenging for students.[17] Truthful reporting need not be optimal under the resulting 'truncated deferred acceptance' procedure (Abdulkadiroğlu et al., 2009; Haeringer and Klijn, 2009; Calsamiglia et al., 2010; Fack et al., 2015). We consider welfare outcomes for 'naive' truthful reporting for lists of lengths one to twelve (DA-N), as well as for equilibrium 'sophisticated' play at the baseline list length under the assumption that households form rational expectations beliefs.

It is possible that households will not trust or not receive a recommendation to play truthfully. We augment our baseline analysis with departures from optimal play in which households drop schools where they think they are unlikely to be admitted, or stop listing schools once they believe they will be unplaced with low probability. We also consider cases in which some households do not receive the recommendation to play truthfully and continue to file the same applications as under the baseline mechanism, and cases in which households play strategically under the DA mechanism but with belief errors based our beliefs model.

Our second set of counterfactuals considers the effects of informational interventions by shrinking the $shift_{ijr}$ error terms by factors ranging from zero to one and then solving for the equilibrium of the baseline mechanism. A factor of zero corresponds to baseline case. A factor of one corresponds to a best-case informational intervention, with $shift_{ijr} = 0$ for all ijr. An alternate interpretation of the best-case intervention is as the result of providing a strategic and informed 'proxy' player with each applicant's cardinal utilities and allowing the proxy player to submit the application (Budish and Cantillon, 2012). By comparing findings from the first and second sets of counterfactuals, we

[17] Given the relatively small number of schools in New Haven, full lists are feasible in our setting.

assess whether the switch to deferred acceptance offers welfare benefits relative to the observed mechanisms given the observed distribution of belief errors, and whether this finding would change if students had access to more accurate information on admissions chances.

There may be multiple equilibria under rational expectations, under 'sophisticated' truncated deferred acceptance, and under strategic play in either mechanism when households maintain components of belief errors. We select an equilibrium as follows. We start with the distribution of cutoffs π^0 that we recovered from the data in step 1. We then compute optimal applications for each household. Given the new applications and our resampled draws, we compute a new distribution of cutoffs π'. We obtain new cutoffs $\pi^1 = (1-\alpha)\pi^0 + \alpha\pi'$ for $\alpha \in (0,1)$ pointwise in each resampled market, and compute optimal applications given π^1. We iterate this procedure until convergence. We take $\alpha = 0.9$ as a starting value and decrease this value as we iterate.

6.2.1 Aggregate welfare in policy counterfactuals

Panel A.1 of Table 7 describes the posterior distribution of mean welfare in the market for the benchmark case, the rational expectations counterfactual and the truthful DA counterfactual, as measured in miles traveled. For each welfare estimate we report the mean, median, and 95% credible interval for the posterior distribution of mean welfare. In the first column, labeled 'Baseline', describe the distribution under the mechanism that was used at baseline. The second column, 'RatEx,' describes the posterior distribution under optimal reports with rational-expectations beliefs in the baseline mechanism. The third column, 'DA,' describes the posterior distribution under the truthful DA, while columns four and five present the differences between the RatEx and DA mechanisms and baseline mechanism. All statistics are averages over the 2015 and 2017 universes of rising ninth graders.

Aggregate welfare improves in both counterfactuals. The average household would be made better off by the equivalent of 4.5 fewer miles traveled under rational expectations. This gain is equal to 31% of mean utility relative to the outside option of attending a neighborhood school or leaving the district under the baseline mechanism. Under DA, the average household is better off by the equivalent of 3.9 fewer miles traveled, or 27% of mean utility relative to the outside option. 95% posterior probability intervals for these differences do not cover zero.

Table 7: Distance-Metric Welfare: Benchmark and Counterfactuals

	Mean welfare			Welfare differences		
	Baseline	RatEx	DA	RatEx − Baseline	DA − Baseline	No Survey DA − Baseline
A1. Posterior distribution of mean distance-metric welfare						
Mean	14.420	18.898	18.346	4.478	3.926	−1.801
Median	13.652	17.454	17.215	4.056	3.557	−1.211
95% CI	[5.877, 30.995]	[8.863, 38.241]	[8.166, 37.635]	[2.395, 9.046]	[2.283, 7.607]	[−6.165, −0.542]
A2. High-SES mean minus low-SES mean						
Mean	−2.887	−3.767	−3.816	−0.881	−0.929	0.644
Median	−2.686	−3.459	−3.493	−0.818	−0.864	0.410
95% CI	[−5.842, −1.080]	[−7.716, −1.532]	[−7.542, −1.582]	[−2.333, 0.163]	[−2.224, 0.027]	[−0.366, 2.228]

	Truthful	Strategic	Drops	Stops
B. DA-4 - baseline under different strategy types				
Mean	3.455	3.645	3.443	3.452
Median	3.117	3.283	3.090	3.114
95% CI	[1.907, 7.005]	[1.906, 7.397]	[1.905, 7.004]	[1.906, 7.039]

	0%	25%	50%	75%	100%
C. Share submitting baseline application under DA-4					
Mean	3.455	2.702	1.946	1.181	0.398
Median	3.117	2.425	1.768	1.085	0.265
95% CI	[1.907, 7.005]	[1.532, 5.389]	[1.065, 3.688]	[0.502, 2.469]	[−0.363, 1.506]

	Switch to DA		Keep baseline mechanism	
	School and priority	School	School and priority	School
D. Eliminate specific error components under DA-4 and baseline				
Mean	2.308	2.310	1.737	1.745
Median	2.059	2.068	1.115	1.110
95% CI	[0.913, 5.500]	[0.905, 5.498]	[0.020, 5.733]	[0.038, 5.716]

Notes: This table describes the posterior distribution of mean welfare in the baseline case and under policy counterfactuals. Welfare is measured using miles traveled as the numeraire good. See text for details. Panels A1 and A2: 'Baseline' is baseline (New Haven or Boston) mechanism given observed beliefs. 'RatEx' is the baseline mechanism under rational expectations beliefs. 'DA' is the strategy-proof deferred acceptance mechanism. 'RatEx-baseline' and 'DA-baseline' columns compare welfare differences under the listed mechanisms. 'No survey DA-baseline' column compares welfare under the DA and baseline mechanisms using model estimates based on rational expectations beliefs. Panel A2 displays differences in each of these objects between high-SES and low-SES households. Panel B: Comparison between truncated DA-4 and baseline under truthful play, strategic play, and truthful play with 'drop' and 'stop' rules for listing schools. Panel C: Welfare gain from switch from baseline to truthful DA-4 by share of households continuing to submit 'baseline' applications. Panel D: Welfare change from switch from baseline to strategic truncated DA with school- and school by priority-specific errors (columns 1+2), and welfare change from switching to only school- and school by priority-specific errors while keeping the baseline mechanism (columns 3+4).

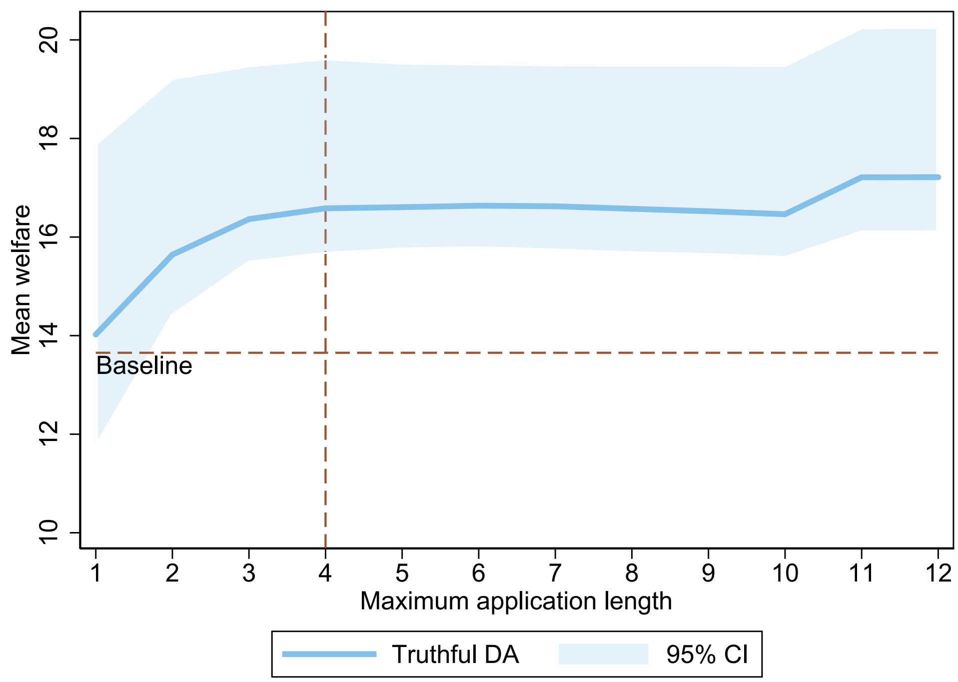
Data on subjective beliefs are important for market designers trying to choose the welfare-maximizing assignment mechanism. The sixth column of panel A.1 of Table 7 compares average welfare under the DA and baseline mechanisms using model estimates obtained without survey data. We impose rational expectations beliefs in estimation and in counterfactual simulations. These estimates reverse the welfare comparison between the DA and baseline mechanisms, with the baseline mechanism outperforming DA by 1.8 miles traveled. The welfare comparison we obtain without using survey data overstates mean welfare of the baseline mechanism by 5.7 fewer miles traveled relative to the comparison incorporating subjective expecations. This is 40% of mean utility relative to the outside option in the benchmark case.

Our finding that the baseline mechanism outperforms DA in no-survey estimates has the same sign as results from Agarwal and Somaini (2018) and Calsamiglia et al. (2018) but is larger in magnitude. For example, Agarwal and Somaini (2018) estimate a welfare loss of 0.08 additional miles traveled when switching from the Cambridge mechanism under rational expectations to DA. Our findings may reflect stronger preferences across schools, lower travel costs in New Haven relative to Cambridge, or lower travel costs for high school students than for the early-grade students studied in previous research. They may also reflect our addition of enrollment choice data to preference estimation; previous papers have not used enrollment in estimation. When we exclude the enrollment choice stage of the model, we find a welfare loss of 0.2 miles from the switch to DA in the specification that excludes survey data, much closer to findings from Agarwal and Somaini. Using the alternative model does not change the qualitative conclusions we draw about the welfare comparison between DA and baseline or the importance of using survey data. See section 6.3.

The welfare comparison between baseline and DA does not depend on list length. Figure 5 presents results from DA counterfactuals in which students truthfully report preferences on applications of varying length. The vertical axis is the mean of the posterior mean welfare distribution, and the horizontal axis is the number of schools households are allowed to rank on their application. Mean welfare from the baseline mechanism case holding list length fixed at four is marked by the lower horizontal line. Welfare under truthful DA is above benchmark welfare at all counterfactual application lengths greater than one.

Panel B of Table 7 compares the DA mechanism at an application length of four (DA-4) to the baseline mechanism under several assumptions on counterfactual play. The first column assumes that households would report preferences truthfully under DA-4. The second column assumes that households play strategically under DA-4 based on rational expectations beliefs. The "Drops" column considers DA-4 outcomes in which households begin with their truthful applications, but drop schools if their unconditional chances of placement are below 5%. The "Stops" column assumes that households stop listing schools once their chance of not receiving a placement falls below 20%. The point estimates of welfare gains are similar to those for strategy-proof DA in each column.

Figure 5: Welfare under truthful DA by list length

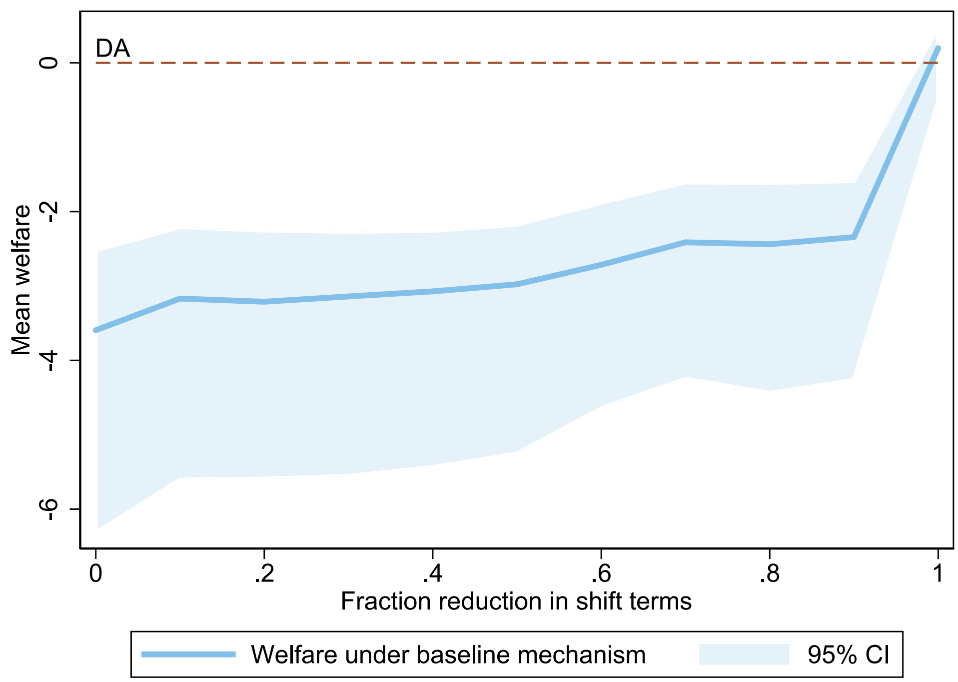


Notes: median of posterior mean welfare distribution (vertical axis) under truthful DA policy counterfactual by application length (horizontal axis). 'Baseline' line is median of posterior mean welfare under the baseline mechanism and observed beliefs with an application length of four.

Panel C of Table 7 describes welfare changes under a 'surprise' implementation of deferred acceptance in which some households are not informed of the mechanism change and keep their baseline applications, while others report truthfully. An alternative interpretation is that "surprised" households maintain the same beliefs as under the baseline mechanism. We fix the application length at four in this exercise. The '0% surprised' column corresponds to the truthful DA-4 counterfactual. As the share of households who do not change their play rises, welfare falls. A gain of zero falls outside the 95% credible interval through a 75% 'surprise' rate. When no households are informed of the change ('100% surprised'), welfare effects are close to zero, with a posterior probability interval that covers zero. The switch to DA seems likely to be welfare improving at realistic rates of truthful reporting. The empirical literature studying rates of truthful reporting in the DA context finds that large majorities of participants play truthfully. For example, Rees-Jones (2018) studies the medical residency match and reports that between 5% and 17% of participants do not report true preferences, while Chen and Sönmez (2006) report evidence from a lab setting that between 28% and 44% of participants misrepresent preferences.

We next ask how effective an informational intervention would have to be to cause the baseline mechanism to raise aggregate welfare relative to deferred acceptance. We scale all shift terms by values ranging from zero to one and simulate counterfactual welfare distribution in each case. Figure 6 presents results from this exercise. The horizontal axis is the fraction reduction in the shift term, and the vertical axis is the difference in mean welfare between baseline and DA. The gains from informational interventions of this type are limited until belief errors are completely eliminated, at which point welfare under the baseline mechanism is similar to welfare under DA.

Figure 6: Mean welfare under baseline mechanism by reduction in scale of shift term



Notes: median of posterior distribution of differences in mean welfare between baseline and DA (vertical axis) by fraction reduction in $shift_{ijr}$ terms (horizontal axis).

Another way to think about informational interventions is as eliminating certain types of errors. Information interventions that clarify how the assignment mechanisms work may eliminate belief errors with respect to the effect of rank on application score, while uncertainty about school-specific demand, priority groups, and person-specific optimism persist. We consider how eliminating this type of error affects welfare relative to the baseline in Panel D of Table 7, which shows welfare changes under sophisticated play for an alternative partial information intervention in which belief errors about rank are shut down.

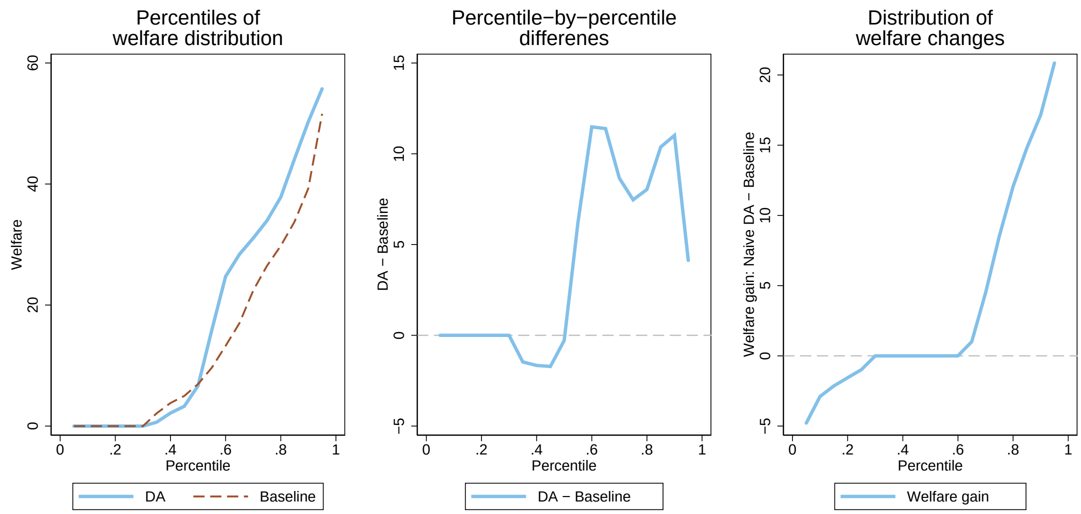
The first column of this panel ("Switch to DA– school and priority") shows welfare gains relative to baseline when the mechanism is changed to DA and η_{ijr}, η_i^r are set to zero for all households, but the other components of $shift_{ijr}$ are held fixed, including the errors about schools' cutoffs η_{ij} and errors about priority $\eta_i^{priority}$. We find that welfare would increase under this counterfactual by the equivalent of 2.3 fewer miles traveled. The "school" column considers welfare changes when, in addition, errors about priority $\eta_i^{priority}$ are set to zero for all households, with nearly identical results. These results suggest that welfare would increase under a switch to deferred acceptance, even if households attempt to play strategically but misforecast cutoffs, provided that errors about rank are corrected. The final two columns consider the same changes to $shift_{ijr}$ under the baseline mechanism. Welfare gains of roughly 1.7 indicate that approximately 40% of the gains from the perfect informational intervention could be realized by correcting errors about the impact of rank.

6.2.2 Distributional impacts of policy counterfactuals

One argument in favor of deferred acceptance mechanisms is that they may produce a more equitable distribution of welfare across participants. We consider this point in panel 1.B of Table 7. This table shows the difference between mean utility for high-SES and low-SES households under different counterfactuals. Negative numbers correspond to higher welfare for low-SES households than high-SES households, relative to the outside option for each. As shown in the first three columns, low-SES households have higher utility from choice under Baseline, RatEx, and DA. Point estimates in columns four and five show that low-SES students experience larger gains from the switches to RatEx or DA. 95% credible intervals extend just past zero in both cases.

We further explore this idea by examining the distribution of welfare across households under the baseline and DA mechanisms. For each household, we compute mean welfare by averaging the household's welfare across MCMC iterations. Figure 7 reports the welfare distribution. The left panel reports mean welfare for households in each centile of the welfare distribution under the baseline and deferred acceptance mechanisms. Recall that welfare is normalized to zero for unplaced households. The middle panel reports the centile-by-centile difference in the welfare distributions shown on the left panel. The right panel reports centiles of welfare gains or losses under DA relative to baseline.

Figure 7: Distribution of welfare and welfare changes



Notes: Left panel: posterior mean welfare by centile of welfare distribution under baseline and strategy-proof DA. Middle panel: centile-by-centile differences in welfare between DA and baseline policies. Right panel: percentiles of welfare gain distribution from switch to strategy-proof DA from baseline.

The middle panel indicates that the welfare distribution under DA is higher at all percentiles above the 50th. Quantiles just below the median are somewhat lower under DA than at baseline. The right panel indicates that about 40% of households would be made better off by a switch to DA while 30% of households would be unaffected. Intuitively, some households may be made worse off if they have accurate beliefs at baseline while others are misinformed.

6.3 Robustness and additional analyses

Our model incorporates two features that previous research has generally abstracted from: participation costs, and enrollment choices. To explore how these features affect our findings, we estimate an alternate model that excludes them and compute counterfactuals paralleling our main analysis. We describe this exercise in Online Appendix C. Under the alternate model, mean welfare relative to the outside option is lower for each of the Baseline, RatEx, and DA mechanisms. Switching to DA from the baseline mechanism raises welfare by the equivalent of 0.9 fewer miles traveled, a 32% gain on a base of 2.8 (compare to a 27% gain in our main analysis). When we exclude survey data and impose rational expectations beliefs, we find that the switch would *reduce* welfare by 0.2. The

1.1 mile-equivalent difference between the evaluations of the switch with and without the survey is equal to 39% of mean utility at baseline (compare to 40% in our main analysis). While our findings on the welfare gains from choice in general relative to the outside option as measured in miles traveled are sensitive to including the enrollment/participation decision, findings on differences across mechanisms and the gains from choice as a share of mean welfare at baseline are not.

Additional analyses show results a) separately by survey year and b) restricting the survey data used in estimation to respondents who correctly recall their submitted applications. Online Appendix Figure A26 and Tables A10 and A11 show that our findings on the distribution of belief errors and the welfare implications of counterfactual policies and estimation strategies have the same signs and are similar in percentage terms across years. Welfare levels across all mechanisms relative to the outside option are higher in 2015 than 2017. Online Appendix Table A12 shows that our findings are qualitatively unchanged and quantitatively very similar when we limit the survey data used for model estimation to respondents with *correct* recall of the submitted application.

Thus far we have quantified utility changes in terms of fewer miles traveled, and shown that the welfare gains from a mechanism change are equal to large shares of mean welfare at baseline, relative to the outside option. A final exercise uses estimated wage rates and summed travel times at the district level to convert our distance-metric utility into dollars by way of a simple back-of-the-envelope calculation. We find that the implied dollar values of mechanism changes are large relative to the costs of other educational interventions, such as reductions in class size (Krueger, 1999; Chetty et al., 2011). See Online Appendix D for details.

7 Conclusions

This paper studies the performance of a centralized school choice mechanism that rewards strategic behavior when households have heterogeneous beliefs about placement probabilities. We conduct a household survey asking choice participants about their preferences and beliefs, and link our survey data to administrative records of the school choice process. We use our linked data to describe heterogeneity in beliefs and to estimate a model of school choice that allows for belief and preference heterogeneity. Our survey data allow us to study the effects of counterfactual policies without making strong assumptions on participants' equilibrium play. The counterfactuals we consider highlight the tradeoff between applicants' ability to express preference intensity in mechanisms that reward strategic play and the increased likelihood of welfare-reducing application mistakes.

Our descriptive findings show that while households play strategically and attempt to trade off preference intensity against admissions chances, they do so using mistaken beliefs about admissions chances. Counterfactual policy simulations based on model estimates that incorporate survey data indicate that the ordering of deferred and strategic mechanisms by welfare outcomes depends on the accuracy of students' beliefs about admissions chances. Though the strategic mechanism is preferable when students have rational expectations about choice probabilities, the deferred acceptance mechanism raises aggregate welfare given the distribution of belief errors we observe in our data. The costs of application mistakes in the strategic mechanism outweigh the benefits of increased expressiveness. We abstract from other advantages of deferred acceptance, including the reduced chance of ex-post regret about the submitted application relative to strategic mechanisms.

Our findings suggest that if market designers choose to use school choice mechanisms that reward strategic play, offering students some means to learn about admissions probabilities for different portfolios is likely to be welfare-improving. We leave the discussion of what such an information intervention might look like for future work.

More generally, our findings suggest an important role for data on subjective beliefs in preference estimation and the evaluation of policy counterfactuals. We show that in our setting a market designer who did not account for application mistakes would reverse the welfare comparison between the baseline and deferred acceptance mechanisms. The magnitude of belief errors in any particular setting depends on the experience of and resources available to the economic agent, as well as on the efficacy with which the market designer or other interested parties impart the information necessary for informed strategic play. In school choice settings, households submit application portfolios at most a handful of times in their lives, and districts may vary in their ability to communicate effectively. We might expect similar challenges in, for example, matching markets for public housing (Thakral, 2016; Waldinger, 2018). In contrast, we might expect belief errors to be less important in market settings where sophisticated agents face decision problems repeatedly, such as the matching markets that dictate kidney exchange across hospitals (Roth et al., 2005; Agarwal et al., 2018) or food allocation across food banks (Prendergast, 2017). Extensions of subjective beliefs data and analysis to other matching markets is a topic for future research.

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