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. Allowing for belief errors reverses the welfare comparison to favor the deferred acceptance algorithm.

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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 the DA algorithm offers welfare improvements over the baseline given the belief errors we observe in the data, but that an analyst

[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.

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 41 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, and respondents who describe the mechanism correctly are less likely to report large belief errors.

Errors in subjective beliefs matter because, together with preference intensity, they are inputs to strategic behavior. 34% of respondents are 'revealed strategic' in the sense that they list a

[2] In 2017, New Haven used a standard Boston mechanism. In 2015, it used a mechanism that coincides with the Boston mechanism when all students are in the same priority group, which is approximately the case for students choosing high schools. 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 38% 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 37 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 a deferred acceptance assignment mechanism would *increase* mean welfare by the equivalent of 0.382 fewer miles traveled per trip, or 35% of households' mean welfare gain relative to the outside option. This finding does not change when we allow for a wide variety of potential deviations from truthful play in the DA mechanism. Higher average welfare is driven by shifts upward across the welfare distribution.

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 0.168 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 0.55 miles traveled, or 51% 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.

Results from our best-case informational intervention suggest that the baseline mechanism could lead to higher welfare than DA if the district could help households learn to play optimally. We find that an intervention that allows all households to make choices using rational expectations beliefs would raise welfare by the equivalent of 0.568 miles (53%) traveled relative to the observed baseline, or by 0.214 miles (20%) relative to the DA counterfactual. Intuitively, this counterfactual shuts off application mistakes, so welfare differences are driven by participants' ability to express cardinal preferences through strategic play in the baseline mechanism. 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 is currently available to households.

The paper proceeds as follows. Section 2 describes the New Haven school district. Section 3 describes our survey instrument. Section 4 describes our model of student behavior, Section 5 describes estimation, and Section 6 describes results and counterfactuals. Section 7 concludes.

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, New Haven has assigned students to schools using a centralized mechanism since at least 1997.[5] The school choice system includes both district-run magnet schools and charter schools run by outside operators, such as 'no excuses' charter brand Achievement First.

School choice follows a similar schedule each year. The process begins in January, when students and families can learn more about schools and the choice process by visiting open houses at different schools or by attending one of several 'magnet fairs' where schools set up information booths. The school district provides students with a magnet school guide that includes a description of the rules of choice and data on available seats and applicant counts by priority group from the previous year. This guide is available in English and Spanish, both in print and on a website. Students typically submit their applications in February, and receive notice of their placements in late March or April.

Dur et al. (2018) make use of data on the frequency with which students access a school choice website to proxy 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. We have verified the use of the centralized mechanism as far back as 1997 by inspection of the code used to run the process.

The district administers the choice mechanism through a vendor, Smart Choice Technologies, which also administers school choice programs in Bridgeport CT, Hartford CT, Syracuse NY, New Orleans LA, and Tulsa OK, among others (Smart Choice, 2016). The institutions surrounding school choice in New Haven are similar to 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 1,545 (1,664) potential ninth graders. From this total, students who do not leave the city or enroll in private school may enter a lottery to enroll in one of 12 high schools. Ten of these schools are administered by the district 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. Another is a transitional school primarily for students who have been asked to leave other district schools. 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 and surrounding towns. New Haven students apply to primary schools through the same centralized system. We focus on grade nine students because, as we discuss in the next section, the assignment mechanism New Haven uses more closely resembles the mechanisms used in other districts for high school choice than for primary school choice.[7] Many schools at both the high school and primary levels reserve some seats for suburban applicants, with the remaining seats reserved for within-city applicants. Consistent with our sample frame, we 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

[6]New Haven adopted centralized school choice several years before New York, which introduced a centralized application in 2003, and other cities such as Denver, New Orleans, Newark, and Washington DC, which built on the New York example (Abdulkadiroğlu et al., 2017a). Other choice districts offer a similar mix of schooling options and choice calendars. See also Corcoran et al. (2018) (New York), and Agarwal and Somaini (2018) (Cambridge) for a description of choice institutions in other districts.

[7]Previous drafts of this paper report results for primary school choice. These findings are qualitatively and quantitatively similar to those reported here. See Kapor, Neilson, and Zimmerman (2017).

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. If they decline, they 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,[8] 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:

$$rsp_{ij} = R * priority_{ij} + rank_{ij},$$

where $R = 4$ is the maximum number of schools permitted on an application.[9]

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

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

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}. In New Haven, 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. In the student-optimal stable matching

[8] 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.

[9] The report-specific priority rsp_{ij} depends on the rank of school j on i's application list, and hence on the report a that i submitted to the mechanism. If j is listed in rth place on a list a, then $rsp_{ij}(a) = R * priority_{ij} + r$. We make the dependence explicit where needed.

mechanism, report-specific priorities depend on the exogenous priority group only.

$$rsp_{ij}^{SOSM} = priority_{ij}$$
$$rsp_{ij}^{\text{Boston}} = (rank_{ij}, priority_{ij})$$
$$rsp_{ij}^{\text{New Haven}} = (priority_{ij}, rank_{ij})$$

The New Haven Mechanism differs from the SOSM mechanism in that the tiebreaking rule within priority groups depends on submitted ranks. Sibling priority plays a relatively more important role and submitted rank lists a relatively less important role in determining report-specific priority in the New Haven mechanism than the Boston Mechanism. In particular, in the Boston mechanism in our setting, report-specific priority is given by

$$rsp_{ij} = priority_{ij} + T * rank_{ij},$$

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

In the Boston and New Haven mechanisms, the report-specific priority rsp_{ij} depends on i's submitted rank-order list a via the position of j on this list. We will make this dependence explicit where necessary, writing $rsp_{ij}(a)$.

When all students have the same priority, the Boston and New Haven mechanisms produce the same assignments. At the high school level, 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, 5% of applicants in 2015 and 7% 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.

The small share of students with priority means that the change in assignment mechanism has little affect on assignment outcomes for the large majority of students without priority. As shown in Panel A, 67% of applicants in 2015 and 71% 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 67% in both years. Conditional on participation, about half 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 all placed students placed in their first-listed school. The remainder were unplaced.

Table 1: Placement outcomes and priority groups by year

	All	2015	2017
A. Priorities			
Any sibling priority	0.06	0.05	0.07
None	0.94	0.95	0.93
B. Participation and placement			
Submits applications	0.69	0.67	0.71
Participates in choice	0.67	0.67	0.67
Places first	0.51	0.47	0.56
Places second	0.02	0.03	0.00
Places third	0.01	0.03	0.00
Places fourth	0.01	0.02	0.00
Unplaced	0.45	0.46	0.44
N	3,209	1,545	1,664

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).

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} 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

$$P_{ija} = Pr(z_{ij} \leq \pi_j - rsp_{ij}(a),\ z_{ij'} > \pi_j' - rsp_{ij'}(a) \text{ for all } j' \succ j)$$

As we describe in the next section, we use this formulation to simulate rational expectations 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 D and present question text in Online Appendix E. 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 a combination of 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, and c) measuring the effect of placement outcomes on survey reports, conditional on applications.

55% of respondents correctly report the student's first-choice application. Most respondents who answered this question incorrectly reported listing their stated most-preferred school first; in total 80% responded with either their most-preferred school or their actual first-listed school. 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, applying to a particular school, or using district-provided information sources is also weak (Section 3.7). Finally, we show that survey reports are strongly correlated with observed high-stakes behavior and do not appear to respond to placement outcomes (Section 3.6). The picture that emerges is one in which subjective beliefs inform high-stakes behavior but households have trouble learning about admissions chances even when the value of doing so is high.

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. Once 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.'

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 describe demographic characteristics for the population, the 1,053 target households, and the 417 respondents. The first column describes the population, the second column the sample of households we intended to survey, and the third the households who we successfully surveyed.

Table 2: Characteristics of population and survey respondents

Category	Population Mean	Surveys Mean	Pop v. Survey
SES quintile			
Bottom 20%	0.20	0.22	0.02
20-40%	0.22	0.27	0.05
40-60%	0.23	0.20	−0.04
60-80%	0.14	0.15	0.00
Top 20%	0.20	0.16	−0.04
Race/Ethnicity			
Black	0.46	0.38	−0.10
Hispanic	0.42	0.55	0.14
White Non-Hispanic/Other	0.14	0.10	−0.05
Educational program			
English language learner	0.12	0.20	0.09
Any special education	0.16	0.18	0.02
Number of applications			
Participates in choice	0.67	0.77	0.11
1	0.17	0.20	0.04
2	0.21	0.24	0.05
3	0.37	0.34	−0.02
4	0.20	0.14	0.03
N	3,209	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 application (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 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. (This 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 88% 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 for a map.

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 (77% of respondents vs. 67% in the population), but includes many observations from both groups. Conditional on submitting an application we observe applications of all possible lengths.

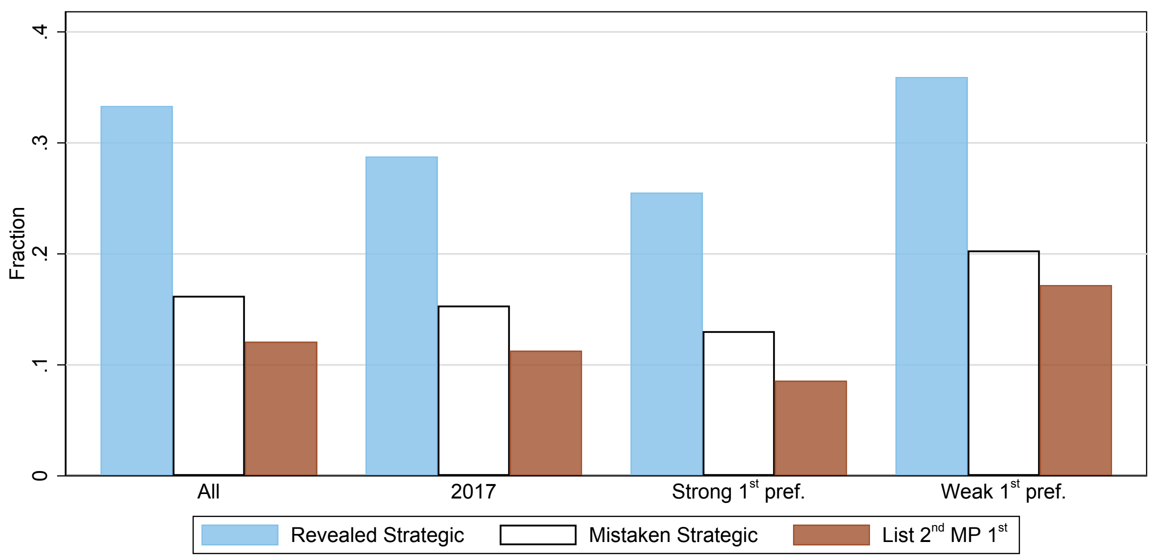
3.3 Rational expectations admissions chances

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. We calculate these probabilities by resampling $n = 200$ markets, drawing individuals, together with their applications and priority types, iid with replacement from the population. In each resampled market, we calculate the market-clearing cutoffs. Given a realization of the cutoff vector, we calculate admissions chances for each student. 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.

3.4 Preference data and strategic play

Reported preferences are closely related to application behavior, but also 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, 34% 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.'[10] Of these, more than a third list their stated second-most preferrred school first, so that overall 78% of respondents list one of their two most-preferred schools first on their application.

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. 'List 2^{nd} MP 1st gives the rate at which unconstrained second choice schools are listed first.

[10] 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.

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 students 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 second-most-preferred school, with X equal to 0.25, 0.5, and 0.75. We label the 67% of students who report a willingness to accept at least one of these lotteries as 'strong first preference' students.[11] 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.

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, 26% of students were revealed strategic, compared to 36% of students in the weak first preference group, for a gap of 10 percentage points, or 38%. The p-value from a test of of equality across the strong- and weak-first preference groups is 0.13.

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. 53% 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 also 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 19.1% of students but appears first on 11.5% of applications, while Engineering & Science is most preferred for 11.1% of students but appears first on 21.5% of applications.

[11] 91% of students report consistent preferences in the sense that if they decline a gamble for one value of X they also decline gambles for smaller X. 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, conditional on willingness to accept some some value of X.

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 subjective belief about his admissions chance at j under application a and our estimate of the rational-expectations chance p^{true}_{ija}:

$$optimism_{ija} = \hat{p}_{ija} - p^{\text{true}}_{ija}$$

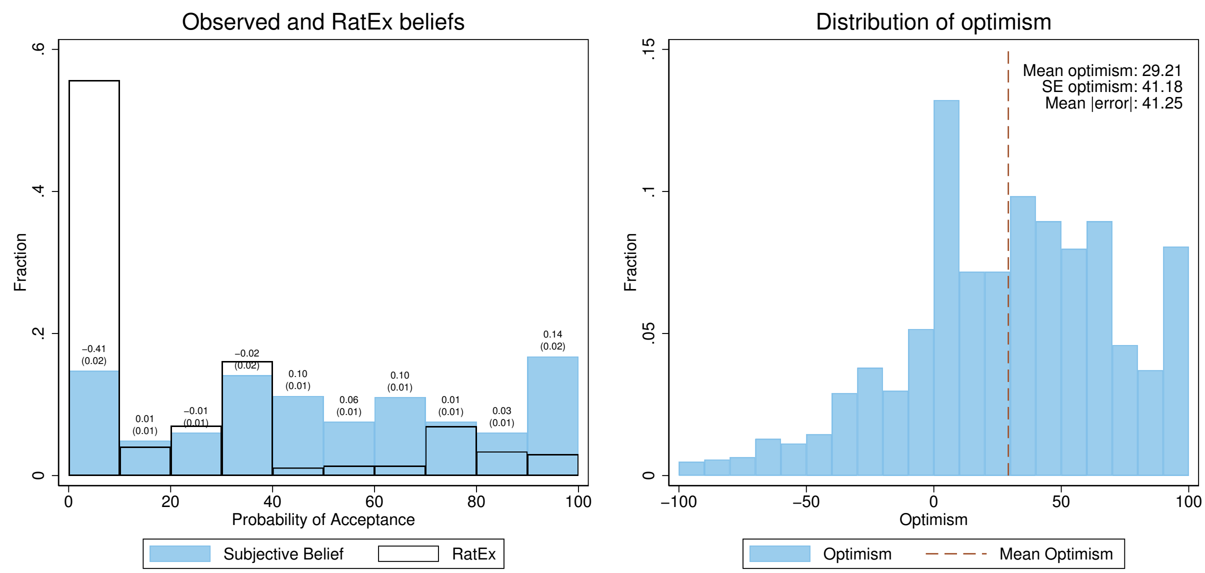
The survey asked respondents about their beliefs for schools ranked first and second on two hypothetical applications, for a total of four elicited beliefs per respondent. Since some respondents declined to answer some questions, we obtained a total of 1,249 elicited beliefs about admission to some school 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 at 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. 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.[12] 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.

The left panel of Figure 2 plots the distribution of rational expectations and subjective beliefs for the sample of hypothetical applications. Above each bar is printed the difference between the share of subjective beliefs and the share of rational expectations beliefs in the bin. Differences between subjective and rational expectations shares are large and statistically significant in many bins. Many fewer respondents believe they have very low chances of admission than actually do. 56% of all elicited probabilities had rational expectations values in the lowest range, but respondents reported beliefs in this range in only 15% of cases.

[12]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.

Figure 2: Distribution of beliefs and optimism

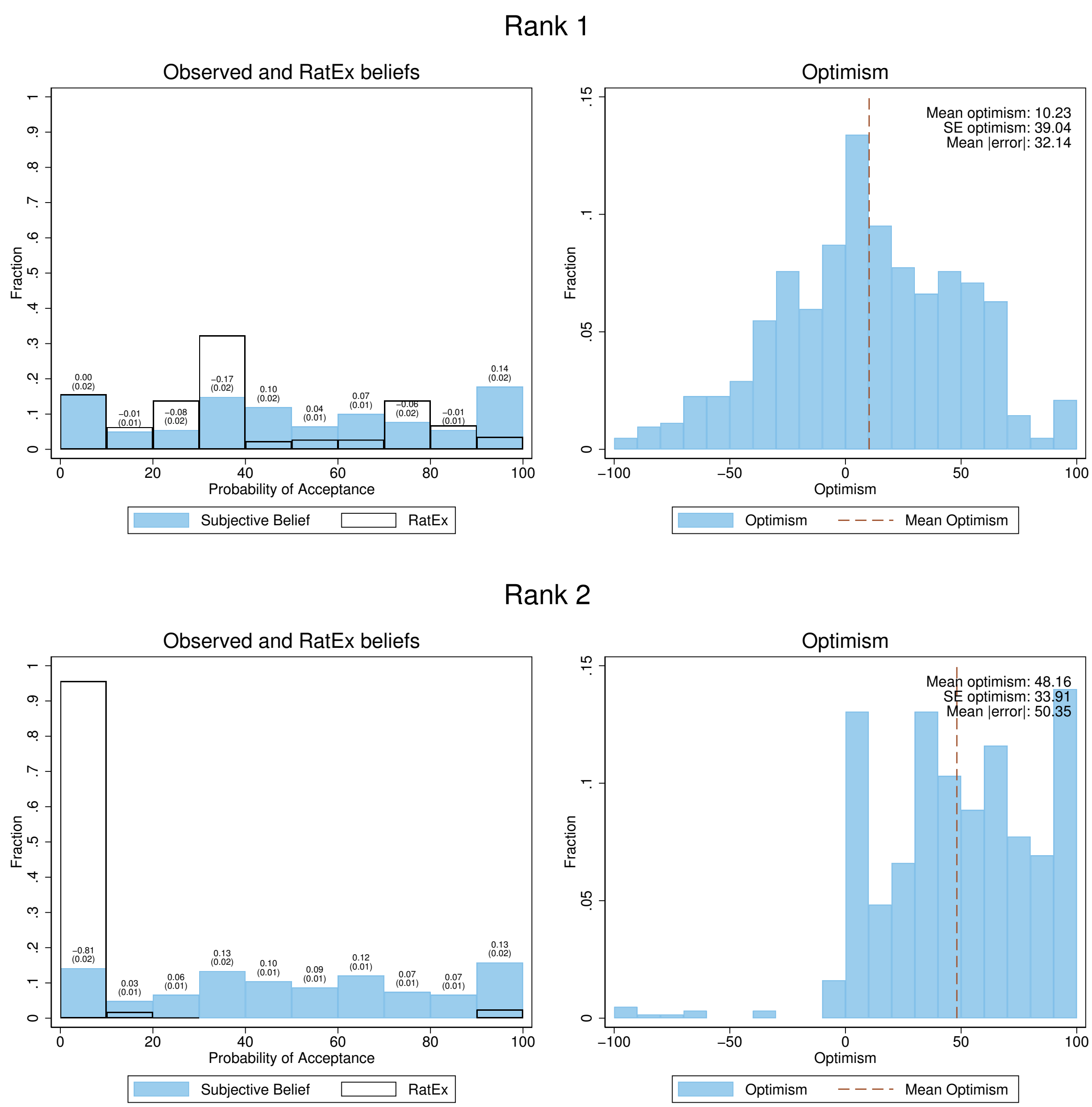


Notes: N=1,249. Left panel: distribution of subjective and rational expectations assignment probabilities. Right panel: distribution of optimism. Bars show shares of population within bins of width 10. Red line indicates mean of the distribution. In both panels, beliefs for second-ranked options are 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 right panel of Figure 2 plots the distribution of (conditional) optimism in the sample of hypothetical applications. Respondents overestimate their conditional admissions chances by 29 percentage points on average, and the spread around this value is wide. The mean absolute error in conditional beliefs is 41 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 38 percentage points more optimistic about second-ranked options than first ranked options, for which optimism values are centered near 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.

Figure 3: Distribution of beliefs and optimism by application rank



Notes: N=1,249. 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.

3.6 Validating belief and preference data

The goal of the survey is to recover information on inputs to the application process. There are several reasons this may not work. The first is measurement error. Respondents may know true admissions chances but report noisy values to surveyors, creating the appearance of deviations from rational expectations. Similarly, respondents may make choices using an underlying set of preferences but report different preferences to us. The second reason for 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. The third, which applies to our measurements of belief errors, 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 three types of evidence that our survey captures meaningful data on application inputs. First, we show that being placed in a school does not affect the rate at which respondents list that school as most preferred. This suggests a limited role for ex-post revision to reported preferences. Second, we show that rational expectations beliefs map one-to-one with placement outcomes, and that conditional on rational expectations beliefs, subjective beliefs do not predict placement. This suggests that our model of the assignment mechanism is accurate and that updates in subjective beliefs in response to placement outcomes are small. Third, we show that beliefs predict high-stakes choices on application forms, and that, conditional on beliefs, rational expectations chances do not. This finding suggests that our belief estimates do not simply reflect measurement error. It supplements results from Section 3.4 above showing that reported preferences correspond closely to application behavior.

Panel A of Table 3 describes the relationship between rational expectations beliefs, subjective beliefs, and application outcomes. The first two columns explore how placement outcomes affect 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 the null of zero placement effect (p=0.205). The second column adds controls for subjective beliefs. We fail to reject the null of zero effect here as well (p=0.299). There is no evidence that households update their preference reports in favor of schools where they receive a placement.

Columns three through five 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, $\hat{p}^{\text{true}}_{i1a^*}$ be our measure of the rational expectations admissions probability at that school for observed application portfolio a^*, and $\hat{p}_{i1a^*}$ be i's subjective belief. If our model of the assignment mechanism is accurate and students do not update beliefs in response to placement outcomes, $E\left[place_{i1}|\hat{p}^{\text{true}}_{i1a^*} = p, \hat{p}_{i1a^*} = s\right] = E\left[place_{i1}|\hat{p}^{\text{true}}_{i1a^*} = p\right] = p$. We test this restriction using linear probability specifications of the form $place_{i1} = \alpha_0 + \alpha_1\hat{p}^{\text{true}}_{i1a^*} + \alpha_2\hat{p}_{i1a^*} + e_i$. 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 linear regressions of an indicator for placement in a student's listed first choice degree program on our rational expectations belief measure and a constant. The sample is all rising ninth graders who participate in choice in 2015 or 2017. This regression tests that our rational expectations measures correspond to placement outcomes. We estimate a coefficient of 0.943 on rational expectations beliefs and an intercept of approximately zero. Despite the large observation count (N=2,161) we fail to reject the joint null of zero constant and slope of one in at the five-percent level (p=0.059). The second column repeats this test in the sample of surveyed school choice participants for whom beliefs about first choice schools are available (N=189). 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=0.683).

The third column tests the predictive power of subjective beliefs for placement outcomes conditional on rational expectations beliefs. We fail to reject the null hypothesis that our rational expectations model is correct and that conditional on rational expectations beliefs, subjective beliefs should have no effect on placement (p=0.374). An alternate test here is to focus on the subjective belief coefficient itself. This coefficient is marginally statistically significant (p=0.093). Given that we are running a number of statistical tests, that we would reject the null for some is unsurprising. A joint test of the null hypothesis that placement does not predict either subjective beliefs or reported preferences (specifically, that the 'subjective belief' coefficient in column 5 is zero and the 'placed' coefficient in column 1 is zero) returns a p-value of 0.12. Overall, our findings suggest that our calculated rational expectations values accurately reflect the placement process, and that the effect of placement outcomes on survey measurements is limited.

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		0.088			0.160
		(0.104)			(0.095)
RatEx	−0.310	−0.327	0.943	1.019	0.973
	(0.149)	(0.150)	(0.025)	(0.085)	(0.089)
Placed	0.104	0.096			
	(0.082)	(0.083)			
Constant	0.785	0.747	0.016	0.019	−0.051
	(0.055)	(0.072)	(0.013)	(0.040)	(0.055)
Dep. var. mean	0.709	0.709	0.417	0.407	0.407
Model test	0.205	0.299	0.059	0.683	0.374
N	189	189	2,161	189	189

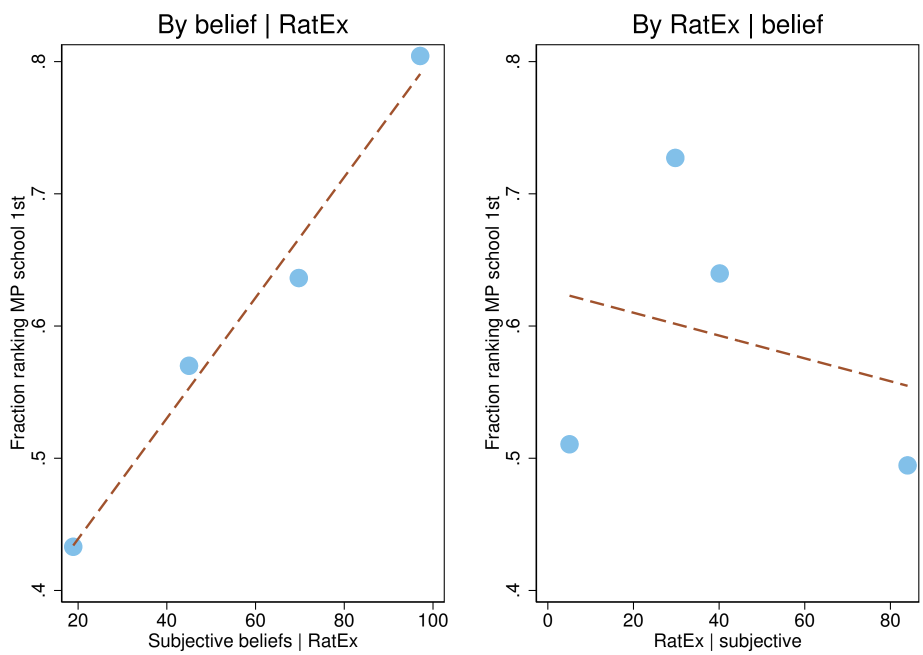
B. Beliefs and application choices	(1) Rank MP 1st	(2) Rank MP 1st	(3) Place MP	(4) Place MP	(5) Place MP | Rank MP 1st
Subjective belief	0.311	0.325	0.312	0.261	0.175
	(0.118)	(0.119)	(0.101)	(0.096)	(0.119)
RatEx		−0.140		0.521	1.043
		(0.150)		(0.128)	(0.109)
Constant	0.854	0.914	0.438	0.216	0.032
	(0.228)	(0.238)	(0.288)	(0.254)	(0.278)
Dep. var. mean	0.604	0.604	0.256	0.256	0.424
Model test		0.353			0.355
R^2	0.123	0.150	0.129	0.254	0.464
N	159	159	159	159	95

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 controls for SES, race, ethnicity, and year. 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 , constant= 0. The p-value for the simultaneous joint test of (A1) Placed = 0 and (A5) Subjective belief = 0 is 0.122.

The next question is 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 of 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 or her most-preferred school is more likely will be more likely to list that school first.

We find an economically large and highly statistically significant relationship between subjective beliefs and application behavior. An increase in subjective beliefs corresponding to one standard deviation of the first-ranked-school optimism distribution (39 percentage points) raises the probability a student ranks their most-preferred school first by 12 percentage points (compare to the sample mean of 60 percent). Effect size is unchanged when we add controls for rational expectations beliefs (column 2). The effect of rational expectations chances are negative and statistically indistinguishable from 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 37 percentage points from the bottom quartile of the subjective belief distribution to the top quartile.

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



Upper panel: N=120. 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 SES, race/ethnicity, year, and other belief type.

Because subjective beliefs influence application behavior, they also influence 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 12 percentage point increase in the rate students are placed in their most-preferred school. 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-preferrred 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. The effect of subjective beliefs on placement outcomes operates through application choices, not placement conditional on application. 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

We next use survey data to study the determinants of beliefs and belief errors. We first describe the informational environment facing potential school choice participants using the information source questions from the 2015 survey. Respondents use the informational resources the district provides. Panel A of Table 4 displays the fraction of students who reported using different resources to inform their school choice decision. 64% of respondents reported reading the choice catalog provided by the district, which contains descriptions of schools and information on demand from the previous year.

Other common sources of information are the school choice website, which includes information similar to the catalog, school visits, counselors, and teachers. 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. 24% of students reported looking up data on the seats available at schools of choice. Low- and high-SES students use similar information sources.

Though respondents consult a variety of information sources, they are unlikely to answer questions about how the assignment mechanism works correctly. Panel B of 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 13.5% of respondents correctly identified the neighborhood priority group as being preferred to the sibling priority group, and only 24.3% 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 hypothesis that respondents perform as well as random guessing at the 1% level in both cases.[13] 4.5 percent of students answer both questions about the choice mechanism correctly. Despite not understanding how the mechanism works, only 6% of students describe the choice process as difficult.

Table 4: Sources of information and understanding of choice rules

	All	High SES	Low SES	p-value
A. Sources of information				
Visit fair	0.429	0.474	0.418	0.689
Visit school	0.535	0.579	0.525	0.637
Visit website	0.566	0.632	0.550	0.753
Talk to teacher	0.556	0.632	0.537	0.748
Talk to counselor	0.495	0.588	0.475	0.695
Talk to friend	0.414	0.474	0.400	0.827
Read catalog	0.643	0.667	0.637	0.704
Read newspaper	0.245	0.222	0.250	0.829
Any admin. source	0.891	1.000	0.866	0.006
Looked up capacity	0.243	0.281	0.240	0.734
B. Choice process				
Process difficult	0.059	0.034	0.058	0.318
Understand priorities	0.135	0.098	0.144	0.648
Understand ranking penalty	0.243	0.203	0.256	0.515
Understand both	0.045	0.020	0.051	0.198

Notes: Columns are samples. Not all questions were asked in both years. All but 'Looked up capacity' in Panel A were asked only in 2015. 'High SES' ($N = 35 - 40$ for 2017 $16 - 19$ for 2015), corresponds to respondents in the bottom quintile of census tract poverty rate while 'low SES' ($N = 189 - 210$ for 2017 $68 - 82$ for 2015) corresponds to respondents living in the remaining census tracts. Panel A describes means of dummy variables equal to one if students used the listed information source. 'Any admin. source' is a dummy equal to one if a respondent reports visiting a fair or a school, reading the website or school choice catalog, or talking to a counselor. Panel B describes means of dummy variables equal to one if students responded correctly to questions about priority ordering and the importance of the submitted rank to admissions outcomes, respectively. 'p-value' is the p-value associated with the regression adjusted difference between high and low SES samples with controls for year, race, and gender.

[13] The question on priorities offered a fourth 'I prefer not to answer' option. We exclude the 9% of respondents who chose this option from our calculation. Even under a best-case scenario where we assign all of these households correct responses, we could still reject the null that households perform as well as random guessing at the 1% level.

3.8 Correlates of belief errors

We now consider the correlates of belief errors. Table 5 presents results from regressions of indicator variables equal to one for large (above-median) values of optimism and absolute errors on student characteristics and descriptors of household interactions with the choice process. The median level of optimism is 35 percentage points; this is also the median value of absolute error.[14] All specifications include controls for hypothetical application rank and and an indicator for whether the student had sibling priority at the hypothetical school.

Panel A reports results from regressions of belief error measures on measures of participation in the school choice process. Respondents have similar rates of large errors for their most preferred and first-listed schools as they do for other schools, and respondents whose children participate in choice have large error rates similar to non-participants. We cannot reject the null hypothesis that our measures of choice participation jointly have have no effect on either belief error measure (p=0.553 and p=0.727 for positive and absolute error, respectively). These findings are consistent with the observation that the distributions of subjective beliefs and belief errors are very similar for participants and non-participants (Online Appendix Figure A3) and, among participants, at listed vs. non-listed schools (Online Appendix Figure A4). They are also consistent with the observation that students who participate in choice resemble non-participants in terms of race and SES (Online Appendix Table A3).

Results from this regression also show that hypothetical rank and priority are strongly correlated with belief errors. Respondents are 39 percentage points more likely to report above-median levels of optimism for second-ranked program, and 22 percentage points more likely to report above-median absolute errors. In contrast, respondents with sibling priority are 23 percentage points less likely to report above-median optimism. These effects hold steady as we cycle through control sets.

Panel B of Table 5 shows that households who correctly understand the assignment mechanism are less likely to make large belief errors than other households. Respondents who answer both mechanism questions correctly are 22 percentage points less likely to have above-median optimism and 16 percentage points less likely to report above-median belief error.

Panel C of Table 5 provides evidence that belief errors are weakly related to strategic play. We cannot reject the null that respondents who we identify as revealed strategic based on preference reports and applications make large belief errors at rates similar to other students. Students we identify as mistaken strategic are 7.6 percentage points more likely to make large absolute errors, but this effect is noisily estimated (p=0.150).

[14] Online Appendix Table A4 shows the same specifications with continuous outcomes. Results are similar.

Table 5: Correlates of belief errors

	A. Preferences & participation		B. Qualitative responses		C. Strategies	
	Large pos. error	Large abs. error	Large pos. error	Large abs. error	Large pos. error	Large abs. error
Hypothetical rank 2	39.3	21.9	39.1	21.7	39.8	22.3
	(2.2)	(2.6)	(2.2)	(2.6)	(2.4)	(2.9)
Have priority	-22.9	11.7	-20.6	12.6	-20.6	9.1
	(8.7)	(10.4)	(7.9)	(9.4)	(9.5)	(12.7)
Most preferred	0.7	-2.5				
	(4.0)	(4.0)				
Filed app	-1.7	-4.7				
	(5.0)	(4.7)				
Ranked 1st	4.2	1.5				
	(4.2)	(4.1)				
Understand mechanism			-21.6	-16.0		
			(7.7)	(9.1)		
Revealed strategic					-3.1	-5.7
					(4.3)	(4.3)
Mistaken strategic					2.5	7.8
					(5.4)	(5.4)
N	1,209	1,209	1,241	1,241	1,017	1,017

	D. Participant characteristics		E. Recall		F. Demographics	
	Large pos. error	Large abs. error	Large pos. error	Large abs. error	Large pos. error	Large abs. error
Hypothetical rank 2	39.0	20.8	39.6	22.0	38.9	21.1
	(2.3)	(2.7)	(2.4)	(2.8)	(2.2)	(2.6)
Have priority	-21.5	12.4	-20.6	8.4	-21.6	14.1
	(8.6)	(10.8)	(9.4)	(12.1)	(8.9)	(10.4)
Mother	-7.1	-10.1				
	(4.6)	(4.4)				
Helped with application	1.7	1.5				
	(4.2)	(4.3)				
Correctly recall application			-4.5	-4.3		
			(3.8)	(3.9)		
Tract poverty rate					13.6	30.8
					(14.2)	(14.4)
Black					-2.8	-4.5
					(3.7)	(3.8)
Female					1.6	-1.5
					(3.6)	(3.6)
N	1,122	1,122	1,039	1,039	1,217	1,217

Standard errors in parentheses. Errors clustered at the student level. All regressions include year fixed effects and exclude neighborhood schools from the sample. For the estimates in Preferences & participation, the p-values for the joint tests of Most preferred = 0, Filed app = 0, and Ranked 1st = 0 are 0.553 and 0.727 respectively. N varies across columns due to sample restrictions. See text for details

Panels D and E of Table 5 ask whether the respondent's relationship to the student and reported role in the application process relate to belief errors. The 73% of respondents who are the mothers of the student are less likely to have large belief errors than other respondents. However, respondents who report assisting in filling out the application or who correctly recall the the schools on the application do not have lower error rates. Because we ask questions about hypothetical applications, not observed applications, knowing what a students' application was is not a direct input to belief formation. Consistent with these findings, Online Appendix Figures A5 and A6 show 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, Panel F examines the relationship between belief errors and student demographics. We measure socioeconomic status as Census tract poverty rate (from 2016 ACS, on a zero to one scale). The standard deviation of tract poverty rate in the analysis sample is 0.12. A 15 percentage point increase in tract poverty rate is associated with a 4.6% percentage point rise in the share of respondents reporting large absolute belief errors. Student race and gender are not strongly correlated with belief errors.

Additional analyses ask how households' use of information sources relates to belief errors. We run regressions of our belief error measures on each information source listed in Table 4, controlling for rank and priority. We report results in Online Appendix Table A5. Estimated effects are small and statistically insignificant in each case. This is consistent with a broader story of application behavior in which households know they should strategize on their schooling applications, but have trouble learning how the mechanism works from district sources. Online Appendix Table A5 also reports results showing that respondents who understand how the assignment mechanism works are no more likely to be revealed strategic but are less likely to be mistaken strategic.

3.9 How the descriptive analysis informs modeling choices

We use four stylized facts from our descriptive analysis to inform modeling decisions in the next section. 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 systematically with submitted rank and priority group, as well as with student background. Fourth, belief errors do not appear to vary with participation in choice, with preferences for particular schools, or with use of different information sources.

These stylized 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. They further suggest that a realistic model of belief errors should allow for heterogeneity by student background and position in the application portfolio. In contrast, there is less evidence that strategic information gathering on more-preferred schools or by students who participate in choice drives differences in belief errors, or that the information gathering strategies that are available to students lead to reductions 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 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 accept or 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 K-12 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.[15] 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

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

one's own zoned high school is part of the outside option.[16] 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.[17]

The covariance matrix Σ is unrestricted. It therefore subsumes random coefficients on school characteristics such as academic quality.

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

$$U_{ij} = u_{ij} + \epsilon^e_{ij}$$

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

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

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

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

We interpret b_i as the cost of the actions i must take to accept or decline a placement. These include finding and getting in touch with the school placement office or the assigned school.

Students make participation and application decisions to maximize their expected utility subject given their subjective beliefs about placement chances. Let p_{ija} denote i's subjective estimate of the probability that he will be placed in school j if he submits report a to the mechanism. Students for whom $a = \emptyset$ are those who do not participate in school choice. i's decision solves

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

[17] 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.

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

The use of subjective beliefs for expected utility maximization 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 choice would be to model only the application decision, treating the choice to accept a placement as exogenous. In such a model, we would have $b_i \equiv 0$, and the value of a placement at j would be given by $v_{ij} = u_{ij}$. We have estimated such a model. The results are very similar to our main specification. See Online Appendix C for details. We prefer our main model because descriptive evidence shows that applicants are more likely to accept placements at more-preferred schools. See Table A6 in the Online Appendix for details.

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 $\tilde{p}_{ija}$ denote household i's belief about the probability of admission to j given report a, and $r\tilde{s}p_{ij}(a)$ and $\tilde{\pi}_j = \pi_j + \Delta\pi_j$ be his beliefs about his report-specific priority and the cutoff score for admission, respectively, with $\Delta\pi_j \in \mathbb{R}$. Then

$$\tilde{p}_{ija} = Pr(z_{ij} \leq \pi_j - rsp_{ij}(a) - shift_{ij}(a),\ z_{ij'} > \pi_j' - rsp_{ij'}(a) - shift_{ij'}(a) \text{ for all } j' \prec_a j)$$

where $shift_{ij}(a) = \pi_j - \tilde{\pi}_j - (rsp_{ij}(a) - r\tilde{s}p_{ij}(a))$. 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 $shift_{ijr}$. 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. As suggested by our descriptive analysis, we allow for separate parameters for students from high- and low-SES backgrounds.

The tradeoff to our flexible approach is that, 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, our survey evidence suggests that the costs of information acquisition on the margin may be prohibitively large in our setting. Families do not have smaller belief errors when their incentives to acquire information are greater because they have decided to participate, to apply to a particular school, or to play strategically, and use of the information sources the district provides is not associated with smaller errors. We leave the challenge of modeling information acquisition to future research.

The $rsp_{ij}(a)$ are constructed differently in the New Haven and Boston mechanisms. In the New Haven mechanism, a one-unit change in $rsp_{ij}(a)$ corresponds to a student choosing to list a school one rank lower on the application, while in the Boston mechanism, a one-unit change corresponds to the difference between students with and without sibling priority who list a school with the same rank. We therefore estimate separate models of belief errors for 2015 and 2017.

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 194 in 2017. For these students we incorporate the option to stay in the current school into the outside option. We allow outside option value to vary with distance to continuing 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 three-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 together with the estimated rational-expectations chances to estimate the parameters governing the belief distribution. Third, we estimate the remaining parameters taking estimates from the second step as given. To do so, we use data augmentation to pick utility vectors and beliefs for each individual consistent with their choices, 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 $shift$ 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 admissions chances

Our approach is 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 therefore 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 distribution of cutoffs feeds into our results in two places. First, the cutoffs $\left\{\pi_j^{(k)}\right\}_{k=1,\ldots,N}$ allow us to calculate rational-expectations admissions chances, which serve as a benchmark in our descriptive analysis. Student i's chance of being placed in school j under report a is given by

$$\begin{aligned} p_{ija} &\equiv Pr(placement_i(a) = j) = Pr\left(score_{ij} < \pi_j, score_{ij'} > \pi_{j'}\ \forall j' \text{ s.t. } j' \succ_j\right) \\ &\approx \frac{1}{N}\sum_k \int 1\left(score_{ij} < \pi_j^{(k)}\right) 1\left(score_{ij'} > \pi_{j'}^{(k)}\ \forall j' \text{ s.t. } j' \succ j\right) dF(score_i|rsp_i, a). \end{aligned}$$

Second, the true cutoffs are inputs into our model of subjective beliefs about admissions chances.

5.2 Recovering belief parameters

Having obtained the distribution of cutoffs in each year, we next estimate the parameters $\sigma_{\eta_{school}}, \sigma_{\eta_{school\times round}}$, Σ^η, and $\overline{\eta}$. We estimate equation 1 via a Gibbs sampler, iteratively updating $\sigma_{\eta_{school}}, \sigma_{\eta_{school\times round}}$, Σ^η, and $\overline{\eta}$ each conditional on the other parameters. This procedure can be interpreted as approximating the maximum-likelihood estimates of these parameters.

5.3 Recovering preference parameters

Before we describe the MCMC 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.

5.3.1 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.

5.3.2 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:

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

5.3.3 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 $u_{ij} + \epsilon_{i0}^e < \epsilon_{ij}^e$.

5.3.4 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 a the scale of a coefficient of known sign, such as the coefficient on distance, β_{dist}. When we incorporate the decision to accept or decline a placement, it is without loss to fix $\beta_{dist} = -1$ as well as normalizing the variance of matriculation-time shocks, $\lambda = 1$. See Online Appendix B.1 for details.

5.3.5 Prior distributions

We begin with prior distributions over the preference parameters and belief parameters. We place priors directly on β, Σ, $\mu_b = \lambda\mu_b$, $\sigma_b = \lambda\sigma_b$, 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 the following diffuse (flat) priors:

$$
\begin{aligned}
\beta &\sim N(0, 100 * I) \\
\Sigma &\sim IW(100, I) \\
\sigma_{survey}, \sigma_b &\sim InverseGamma(1,1) \text{ iid} \\
\overline{\eta} &\sim N(0, 100 * I) \\
\Sigma^{\eta} &\sim IW(4, I) \\
\sigma^2_{\eta_{school}}, \sigma^2_{\eta_{school \times round}} &\sim InverseGamma(1,1) \text{ iid}
\end{aligned}
$$

We assume that the priors are independent.

5.3.6 MCMC iteration

We iterate through the following steps, which consist of sampling from the conditional posterior distributions of utilities, utility shocks, beliefs, application costs, and model parameters:

1. Draw mean-utility parameters $\beta^{(s+1)}$ and mean benefit $\mu_b^{(s+1)}$ from the distribution of $\beta|u^{(s)}, \Sigma^{(s)}$ and $\mu_b|b^{(s)}, \sigma_b^{(s)}$
2. Draw variance of benefit term $(\sigma_b^2)^{(s+1)}$ from the distribution of $\sigma_c^2|\mu_b^{(s+1)}, b^{(s)}$.
3. Draw variance of shocks to reported preferences σ^2_{survey} from the distribution of $\sigma^2_{survey}|\epsilon^{survey}$.
4. Draw covariance matrix $\Sigma^{(s+1)}$ from the distribution of $\Sigma|\beta^{(s+1)}, u^{(s)}$.
5. For each individual in the dataset:
   (a) Draw utility $u_i^{(s+1)}$ from the posterior distribution of u_i given β, Σ, i's decision to accept or decline his placement (if offered one), and constraints implied by the optimality of i's report.
   (b) Draw $b_i^{(s+1)}$ from the posterior distribution of b_i given $v_i(u_i^{(s+1)})$ and constraints implied by the optimality of i's report.
   (c) Draw shock realizations ϵ_i^{survey} and ϵ_i^e from their posterior distributions given u_i and the household's decisions.
   (d) Draw belief random effects η_i^0, $\eta_i^{priority}$, η_i^{round}, and $\{\eta_{ij}\}_{j \in J}$ from their posterior distribution given $shift_i$, $\overline{\eta}$, Σ_η, $\sigma^2_{\eta_{school \times round}}$, and $\sigma^2_{\eta_{school}}$.

(e) Draw $shift_i$ from its posterior distribution conditional on η_i^0, $\eta_i^{priority}$, η_i^{round}, $\{\eta_{ij}\}_{j \in J}$, v_i, b_i, and the constraints imposed by the survey.

Implementation: To obtain belief parameters we use a chain of 80000 draws, discarding the first 20000 to allow for burn-in. In estimating preferences, we use a chain of 60000 iterations. We estimate beliefs separately by year and SES category. We estimate preferences separately by year. We discard the first half of the draws in order to allow for burn-in.

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 Table 6 displays estimates of belief model parameters by household SES. Estimates from 2015 are in the left three columns, while estimates from 2017 are in the right three columns. 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 contrast, 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 0.65 and 1.1 depending on SES category and year, while the $\sigma_{\eta_{school \times round}}$ are equal to roughly 0.9 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 their priority at a school and the round in which they apply to a school. We estimate $\overline{\eta}_{priority}$ for low-SES households at -4 (2015) and -0.9 (2017). Households underestimate the effect of sibling priority on admissions score by perhaps the entirety of the true effect. The credible intervals are large, as sibling priority is relatively uncommon in the data. Similarly, a $\overline{\eta}_{round}$ estimate of roughly

-1 in 2015 indicates that students underestimate this effect by its entirety. An estimate of -1.75 in 2017, when the true effect of round is 2, indicates that students massively underestimate the impact of round on average in this year as well. This is consistent with the finding from Figure 3 that the distribution of beliefs is similar for first- and second-ranked schools. Errors of both types are similar across SES groups. Estimates of $\sigma_{\eta_{pri}}$ and $\sigma_{\eta_{round}}$ indicate that there is substantial heterogeneity across households in the effects of priority group on belief errors, but most students substantially underestimate the impact of round.

Panel B of Table 6 presents estimates of the parameters governing household preferences. 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. See Online Appendix Tables A7 and A8 for the utility shock covariance matrix Σ.

There is no clear preference ordering on default schools, as the coefficient on 1(default is Cross) has ambiguous sign. The option to continue at the Achievement First charter makes the inside option less attractive. The coefficient on 1(low SES) is of ambiguous sign, consistent with the observation that choice participation is weakly correlated with socioeconomic status (see Table A3). On average, receiving a placement is costly, with $\mu_b < 0$, and a standard deviation of $\sigma_b \in (0.6, 1.3)$ in 2015. Measurement error in reported preferences has a standard deviation of 5 to 9 miles traveled, depending on year, suggesting that elicited preference data is informative but imperfectly so.

We observe differences in preferences across schools relative to the outside option. Mean utilities for inside-option schools are generally of ambiguous sign, indicating that the average household is ambivalent between the school and their neighborhood school. Point estimates of mean utility are substantially negative in both years for Riverside, the school aimed at students with disciplinary issues. Point estimates are positive in both years for Hill Regional Career, the school listed as most preferred by the largest share of surveyed students, and listed first on the largest number of submitted applications (see Table A2).

Trace plots are reported in Online Appendix Figures A7 through A13. The trace plots show that belief parameters and most utility parameters are precisely estimated but that there are certain utility parameters, such as the mean utility for Achievement First and the mean and variance of utility for Metro Business which are imprecisely estimated, although they are formally identified.[18] Despite this, the baseline and counterfactual welfare estimates are stable across specifications and simulation runs.

[18]Because we estimate an unrestricted covariance matrix our model has many parameters. An alternative decision would have been to restrict this matrix in some way, such as by assuming a particular random-coefficient structure.

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)	0.431	0.835	1.554	0.404	0.636	0.919
$\sigma_{\eta_{\text{individual}}}$ (low SES)	0.320	0.504	0.763	0.318	0.478	0.673
$\sigma_{\eta_{\text{priority}}}$ (high SES)	0.408	0.813	2.084	0.329	0.549	0.954
$\sigma_{\eta_{\text{priority}}}$ (low SES)	0.470	0.919	1.803	0.275	0.423	0.645
$\sigma_{\eta_{\text{school}\times\text{round}}}$ (high SES)	0.582	0.874	1.264	0.778	0.900	1.067
$\sigma_{\eta_{\text{school}\times\text{round}}}$ (low SES)	0.721	0.862	1.016	0.824	0.906	1.003
$\sigma_{\eta_{\text{school}}}$ (high SES)	0.742	1.097	1.590	0.472	0.669	0.923
$\sigma_{\eta_{\text{school}}}$ (low SES)	0.496	0.681	0.896	0.943	1.070	1.209
$\overline{\eta}_0$ (high SES)	-0.933	-0.280	0.377	-0.949	-0.662	-0.370
$\overline{\eta}_0$ (low SES)	-0.571	-0.307	-0.033	-0.654	-0.472	-0.300
$\overline{\eta}_{priority}$ (high SES)	-5.808	-4.394	-3.075	-1.470	-0.968	-0.526
$\overline{\eta}_{priority}$ (low SES)	-4.871	-4.014	-3.157	-1.152	-0.894	-0.633
$\overline{\eta}_{round}$ (high SES)	-1.525	-0.910	-0.332	-2.074	-1.754	-1.453
$\overline{\eta}_{round}$ (low SES)	-1.258	-0.972	-0.697	-1.899	-1.745	-1.576
$\sigma_{\eta_{round}}$ (high SES)	0.322	0.532	0.941	0.291	0.430	0.651
$\sigma_{\eta_{round}}$ (low SES)	0.266	0.379	0.564	0.237	0.316	0.427
B. Preference parameters						
δ Achievement First Amistad HS (1)	-0.240	2.002	4.886	-3.647	1.480	3.716
δ Common Ground Charter (2)	-4.426	-1.231	4.102	-3.036	-0.473	2.516
δ Coop. Arts and Humanities (3)	-1.747	0.209	3.261	-1.740	2.293	4.478
δ Engineering & Science Univ. HS (4)	-7.163	-3.618	1.574	-2.937	1.810	4.049
δ High School in the Community (5)	-2.682	-0.592	3.613	-6.449	0.039	1.860
δ Hill Regional Career (6)	1.465	3.495	7.733	-2.643	1.958	4.382
δ Hillhouse (7)	-4.162	0.087	4.325	-2.016	1.822	4.167
δ Hyde School (8)	-7.576	1.840	7.131	-0.021	5.263	8.920
δ Metropolitan Business Academy (9)	-6.119	-1.321	0.848	-2.314	2.008	4.342
δ New Haven Academy (10)	-4.050	-1.084	2.243	-6.216	-1.301	2.331
δ Riverside Education Academy (11)	-9.126	-2.845	1.543	-16.947	-5.523	0.485
δ Wilbur L. Cross High School (12)	-6.433	-0.011	5.188	-2.049	1.305	4.347
μ_b	-3.810	-2.878	-2.056	-4.689	-3.413	-2.344
σ_b	0.584	0.834	1.382	0.542	0.809	2.052
σ_{survey}	5.586	8.885	14.983	3.504	4.759	6.292
1(default is Cross)	-0.216	2.780	6.760	-6.302	-2.515	6.603
1(low SES)	-1.595	-0.147	2.241	-0.557	1.518	3.539
Achievement First	-13.383	-5.926	-0.498	-8.940	-6.405	-0.190
Achievement First$\times$ Dist.	-2.321	-0.304	0.716	-1.693	-0.042	0.711
Distance to default	-1.065	-0.128	0.258	0.232	1.223	2.770
Engineering & Science	-21.798	-8.732	5.854	-10.484	0.331	14.303
Engineering & Science$\times$ Dist.	-3.134	0.701	4.078	-5.402	-1.506	0.653

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 A7. Panel B: preference parameter estimates by grade. Coefficient on miles traveled is normalized to -1. Appendix Tables A7, and A8 provide 90% 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, Achievement First $\times$ dist, Eng. & Sci., and Eng. & Sci. $\times$ dist. coefficients are for incumbent students at those K12 schools.

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 households who participate in choice 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 (though we note that given the relatively small number of schools in New Haven, it is feasible in our setting). 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 several types of departures from optimal play in which households drop schools that they perceive to be unlikely, or stop listing schools once they believe they will be unplaced with low probability. We also consider cases in which varying shares of 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 on those measured in 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 New Haven mechanism. A factor of one corresponds to a best-case informational intervention, with $shift_{ijr} = 0$ for all ijr. A factor of zero corresponds to baseline case. An alternate interpretation of the best-case intervention counterfactual 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 list (Budish and Cantillon, 2012). By comparing findings from the first and second sets of counterfactuals, we can 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. Rows are quantiles of the posterior distribution of mean welfare. We take the median of this distribution as a point estimate, and the 0.025 and 0.975 quantiles as a 95% credible interval for the mean value. In the first column, labeled 'Baseline', we display quantiles of this distribution under the mechanism that was used at baseline. The second column, 'RatEx,' shows quantiles of 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. We discuss column six below. Welfare effects are averages over the 2015 and 2017 universes of rising ninth graders. We discuss year-specific findings in Section 6.3.2.

Table 7: Distance-Metric Welfare: Benchmark and Counterfactuals

Quantile	Baseline	RatEx	DA	RatEx-Base	DA-Base	No Survey DA-Base
A1. Mean distance metric						
0.025	0.786	1.168	1.002	0.368	0.182	-0.329
0.5	1.078	1.639	1.425	0.568	0.382	-0.168
0.975	1.394	2.347	2.176	0.997	0.822	-0.024
A2. SES gap						
0.025	-0.296	-0.581	-0.692	-0.361	-0.616	-0.128
0.5	0.008	0.004	-0.131	-0.044	-0.104	-0.029
0.975	0.203	0.358	0.351	0.224	0.216	0.032

Quantile	Drops (a)	Drops (b)	Stop early (a)	Stop early (b)	DA 4 - Base
B. Mistakes under DA					
0.025	0.091	0.004	0.104	0.062	0.266
0.5	0.325	0.216	0.325	0.260	0.408
0.975	0.857	0.768	0.926	0.815	0.721

Quantile	0%	25%	50%	75%	100%
C. Share surprised					
0.025	0.250	0.107	0.028	-0.034	-0.103
0.5	0.423	0.282	0.174	0.062	-0.032
0.975	0.769	0.646	0.479	0.303	0.087

	Switch to DA		Keep current mechanism	
Quantile	School and priority	School	School and priority	School
D. Error components				
0.025	0.048	0.049	0.279	0.242
0.5	0.223	0.241	0.465	0.423
0.975	0.658	0.670	0.838	0.805

Notes: This table displays quantiles of the posterior distribution of mean welfare by grade in baseline case and under policy counterfactuals. Welfare is measured using miles traveled as the numeraire good. 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-base' 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: difference between DA welfare and baseline welfare under 'drop' and 'stop' DA play (columns 1-4) and sophisticated truncated DA-4. See text for details. Panel C: Welfare gain from switch from baseline to truncated DA-4 by share of households continuing to submit 'baseline' applications. See text for details. 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. See text for details.

Aggregate welfare improves in both counterfactuals. The average household would be made better off by the equivalent of 0.568 fewer miles traveled under rational expectations. This gain is equal to 53% 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 0.382 fewer miles traveled, or 35% of mean utility relative to the outside option. 95% posterior probability intervals for these differences do not cover zero.

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 using 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 0.168 miles traveled. The magnitude of this reversal is large. The welfare comparison we obtain without using survey data overstates mean welfare of the baseline mechanism by 0.550 fewer miles traveled relative to the comparison incorporating subjective expecations. This is 51% of mean utility relative to the outside option in the benchmark case. Our finding that the baseline mechanism outperforms DA in no-survey estimates is consistent with results from Agarwal and Somaini (2018) and Calsamiglia et al. (2018). 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 deferred acceptance.

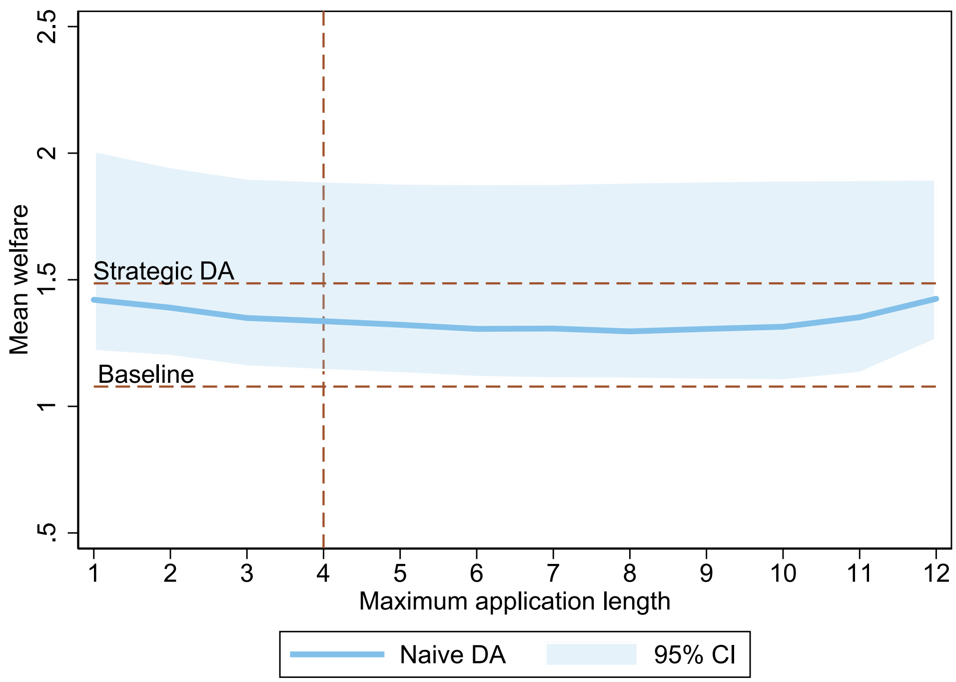
The welfare comparison between baseline and DA does not depend on list length. Figure 5 presents results from DA counterfactuals in which students truthfully report preferences on applications of varying length. The vertical axis is the median value of the posterior mean welfare distribution, and the horizontal axis is the number of schools households are allowed to rank on their application. Mean welfare estimates from the baseline mechanism case is marked by the lower horizontal line. Welfare under truthful DA is above benchmark welfare at all application lengths. This comparison could have gone either way: constraints on list length may be costly because they increase the chances that students will go unplaced, but they may increase welfare by reducing the externalities that households impose on others.[19]

Panel B of Table 7 shows gains or losses in mean welfare under different behavioral assumptions on counterfactual play in the DA mechanism relative to the baseline mechanism. The first two columns, labeled "drops", consider DA-4 outcomes in which households begin with their truthful applications, but drop schools if their perceived unconditional chances of placement are below 5%, according to baseline beliefs ("Drops (a)") or rational-expectations beliefs ("Drops (b)"). The

[19] In earlier versions of this paper, in the kindergarten market, we found that welfare under truthful DA was below baseline value for small numbers of schools. This was because many of the 34 schools in the kindergarten market have few spots, so students often go unplaced when their applications consist only of their most-preferred schools.

columns labeled "stop early" consider households that stop listing schools once their perceived chance of not receiving a placement falls below 20% according to baseline beliefs ("Stop early (a)") or rational-expectations beliefs ("Stop early (b)"). The point estimates of welfare gains are similar to those truthful DA, indicating that these forms of mistakes do not reverse our conclusions.

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. 'Strategic DA' is welfare under the sophisticated DA counterfactual at an application length of four.

The sixth column of panel B of Table 7 shows welfare changes under 'sophisticated truncated DA-4.' We plot this value as the upper horizontal line in Figure 5. Here, households use rational expectations beliefs to make choices about which schools to list on their application to maximize expected utility. It is likely unrealistic to assume that the switch to the DA mechanism would also coincide with a correction of belief errors. However, welfare in this case is very close to what we find under truthful reporting.

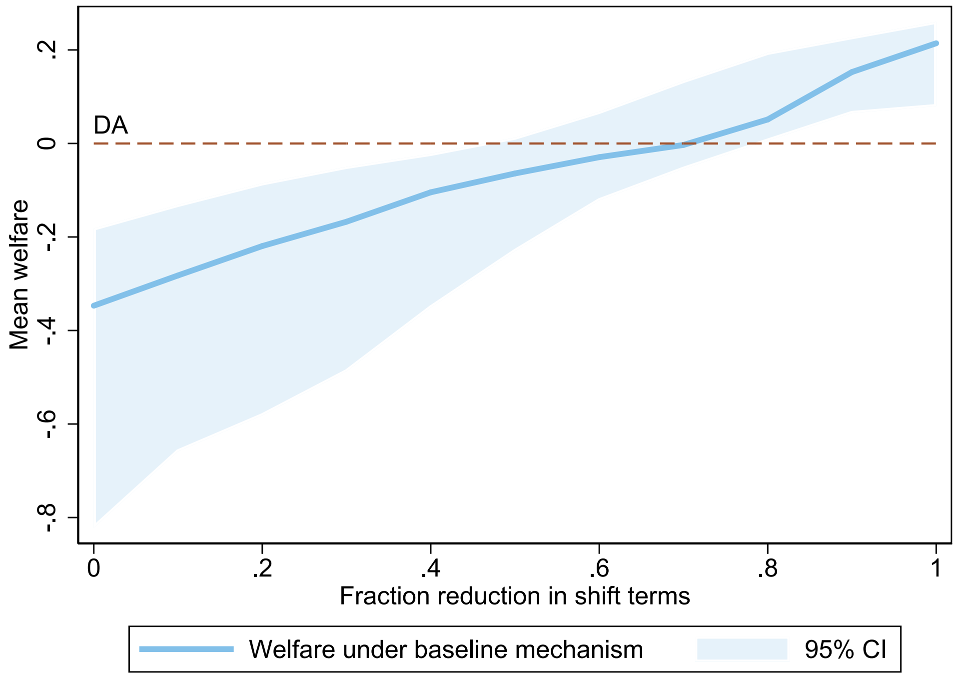
Panel C of Table 7 describes welfare changes under a 'surprise' implementation of deferred acceptance in which some households are not informed of the mechanism change and keep their baseline applications, while others report truthfully. An alternative interpretation is that "surprised" households maintain 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 shown in Figure 5. 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 50% 'surprise' rate, and point estimates of welfare effects remain positive until we the reach the "100% surprised' column, corresponding to a counterfactual where the mechanism changes but reports do not. When no households are informed of the change, 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. Even if households are not informed of the mechanism change, we cannot rule out the possibility of no welfare effect. Further, the zero effect for the baseline mechanism relative to DA is more favorable for DA than the negative effect we observe when we impose rational expectations beliefs on the analysis (Panel A.1, column 6).

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. For mean welfare under the baseline mechanism to break even with the deferred acceptance level requires roughly a 70% scale-down of shift terms.

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.

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



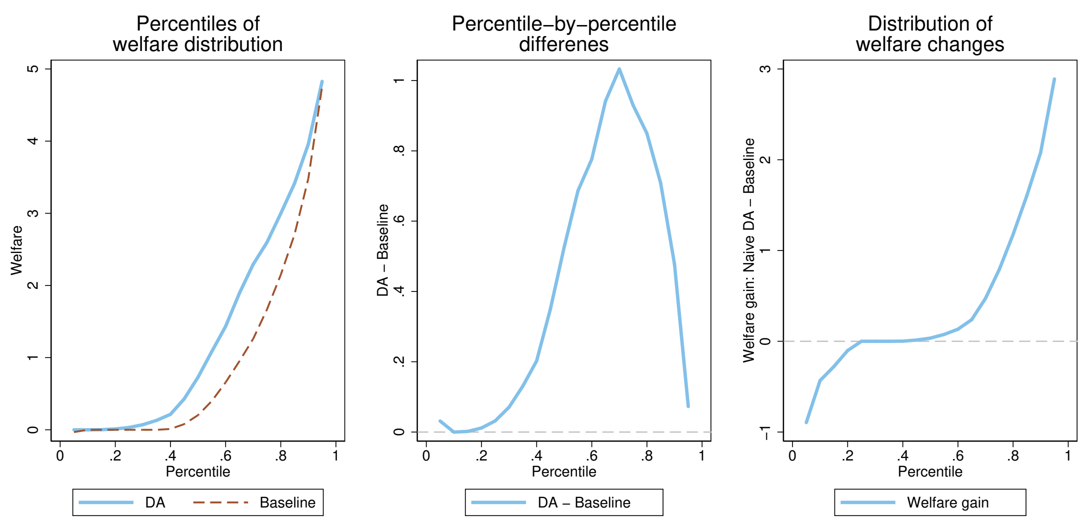
Notes: median of posterior distribution of differences in mean welfare between baseline and DA (vertical axis) by fraction reduction in $shift_{ijr}$ terms (horizontal axis).

The first column of this panel shows welfare gains relative to baseline when the mechanism is changed to deferred acceptance, 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 $\eta_i^{priority}$. Taking the posterior median as a point estimate, welfare would increase under this counterfactual by the equivalent of 0.223 miles traveled. The second 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 0.45 indicate that approximately 80% 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 of the arguments in favor of deferred acceptance mechanisms is that they may produce a more equitable distribution of welfare across participants. We explore this idea by examining the distribution of welfare across households under the baseline and deferred acceptance 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 weakly higher at each quantile. Gains in median welfare are larger than average gains. Welfare at the 50th percentile of the distribution rises by 0.53 under deferred acceptance. The right panel indicates that about 60%

of households would be made better off by a switch to DA. Intuitively, some households would be made worse off if they have accurate beliefs at baseline while others are misinformed.

A related point is that low-income households may be disadvantaged by mechanisms that reward strategic play. 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 counterfactual simulations. As shown in the first three columns, low-SES and high-SES households have similar mean utility under NH, RatEx, and truthful DA. Because utilities are normalized relative to each household's outside option, these level differences are uninformative. Columns four and five show that high-SES and low-SES households experience similar gains in mean welfare from switching to rational expectations play or to a deferred acceptance mechanism.

6.2.3 Distance-metric effects in dollar terms

We have shown that the welfare effects of changes in choice mechanism and informational environment represent large shares of mean utility relative to students' outside options. To place welfare effects in broader context, we conduct a back of the envelope calculation that maps distance-metric utility to travel time, and travel time to dollars. There were 21,712 students enrolled in NHPS in the 2014-2015 academic year. All were assigned to schools through the placement process or following a decision not to participate. There are 180 school days in the year, and each student must travel both to and from school, for an estimated 7.8 million trips per year. From Table 7, students receive per-trip welfare gains equivalent 0.382 fewer miles traveled per trip from a switch to the DA mechanism, for a total welfare gain of 3.0 million fewer miles per year. Using Google Maps walk- and drive-time measures and assuming that students who live within two miles of a school[20] choose to walk, we compute average hours per mile of travel time to the zoned school as 0.34, for a total time gain of 1.0 million hours. Valuing students' time at $10 per hour, the total dollar value of the welfare gain from the switch is roughly $10.0 million, or 12% of the $82 million NHPS spent on teachers in 2014-2015 (NHPS, 2014). A parallel calculation based on the rightmost column of Table 7 shows that a market designer who did not use survey data would have mis-estimated the welfare change from the switch to DA by $15 million per year, or 18% of the teaching budget.

These are large effects for a change that is close to costless. For a benchmark, the well-known Project STAR experiment reduced class size by about 30%, from 22 students per class to 15 (Chetty et al., 2011). The dollar value of utility mismeasurement relative to the no-survey case would be enough to implement a reduction of this size in roughly 60% of district schools. And because our

[20]This is corresponds to state guidelines for high school students as decribed in(Lohman, 2014).

assumptions here are conservative in several respects, we likely underestimate true utility gains.[21]

6.3 Robustness

6.3.1 Alternate modeling approach

Our model incorporates two features that previous research has generally abstracted from: participation costs, and enrollment choices. To explore how these model features affect our findings, we estimate an alternate model that excludes these features and compute counterfactuals paralleling those presented above. We describe this alternate model and present results in Online Appendix C. This alternate approach does not affect our conclusions. Given the errors we observe in the data, switching from the baseline mechanism to DA raises mean welfare by the equivalent of 0.323 miles traveled. When we exclude survey data and impose rational expectations beliefs, we find that the switch would *reduce* welfare by 0.314. In short, the estimated gains from the switch to DA given observed belief errors are larger under this model, as is the change in the conclusions about welfare effects that we draw from incorporating survey data.

6.3.2 Heterogeneity across years

The counterfactual results we present in Table 7 pool across the 2015 and 2017 application years. As we note in section 2, the assignment mechanism changed in those years, as did the details of our survey protocols and survey team. Online Appendix Tables A9 and A10 split Table 7 by year. Note that we already estimate separate belief and preference models in each year, so the two tables are generated using nonoverlapping datasets and estimation procedures. Our findings do not vary much across years. In both years we find that DA outperforms the baseline mechanism, and that we would have reached the opposite conclusion had we not used survey data and instead imposed rational expectations. Findings from the welfare analysis are consistent with descriptive findings that reported distributions of subjective beliefs and belief errors are very similar across the two years. See Figure A14. In a sense, results from 2017 replicate findings from 2015. Our findings do not appear to be driven by idiosyncrasies in survey implementation, or to depend on differences between the New Haven and Boston mechanisms in high school choice.

Neither do our findings depend on the choice of which central moment to use. Online Appendix

[21] Drive-times are based on car travel; buses are much slower. Students in cars and younger walking students are often accompanied by adults, whose welfare we do not include in our calculation. Our $10 per hour valuation of time is based on the minimum wage in Connecticut, which was $10.10 in January 2017. For the average student, the present value of an hour of school attendance is likely higher.

Table A11 reports year-specific means and standard deviations (rather than medians and quantiles) of the posterior mean welfare distribution.

6.3.3 Subsetting on correct recall

Our descriptive analysis in Section 3 found that a minority of survey participants did not correctly report their household's application portfolios. This may be because respondents forgot about aspects of the application process between the time of application and the time of survey, or because these respondents were not involved in the choice process. To test the effects of limited recall on our findings, we estimate models that condition on *correct* recall of the submitted application for choice participants, and correct recall of non-application for non-participants. We report results from welfare calculations in Online Appendix Table A12. Our findings are qualitatively unchanged and quantitatively very similar to those reported in Table 7.

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 actual and potential 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 analyze the effects of counterfactual policies without making strong assumptions on applicants' equilibrium play. The counterfactual policies 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 students 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 any particular setting depends on the experience of and resources available to the economic agent. In school choice settings, where a group of mostly lower-income households submit application portfolios at most a handful of times in their lives, belief errors are relatively large. 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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