HOUSING SEARCH FRICTIONS:
EVIDENCE FROM DETAILED SEARCH DATA AND A FIELD EXPERIMENT

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ABSTRACT

We randomized school quality information onto the listings of a nationwide housing website for low-income families. We use this variation and data on families' search and location choices to estimate a model of housing search and neighborhood choice that incorporates imperfect information and potentially biased beliefs. We find that imperfect information and biased beliefs cause families to live in neighborhoods with lower-performing, more segregated schools. Families underestimate school quality conditional on neighborhood characteristics. If we had ignored this information problem, we would have estimated that families value school quality relative to their commute downtown by half that of the truth.

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1 Introduction

School quality is important for children's long run outcomes (Deming et al., 2014; Angrist et al., 2013; Chetty et al., 2011, 2014; Dobbie and Fryer, 2015), but access to high quality schools varies substantially within and across neighborhoods (Reardon, 2018). This geographic variation matters because, for most of the United States, residential choice is school choice: where you live determines where your child goes to school. Low-income families are disproportionately segregated into neighborhoods zoned to low-performing schools. This pattern of segregation is particularly true for the recipients of Housing Choice Vouchers (Section 8), a 20 billion dollar federal program that helps low-income families pay for their rent (Ellen et al., 2016; HUD, 2011).

To understand why these families do not move to areas with high-performing schools, it is important to understand how their housing choices relate to their underlying preferences for schools (Bayer et al., 2007; Galiani et al., 2015; Davis et al., 2019). Low-income families may prefer neighborhoods in areas with low-quality schools for a number of reasons, such as proximity to their family or employment. However, there may be frictions that impede their choices as well (Ferryman et al., 2008; Rhodes and DeLuca, 2014; Bergman et al., 2019).

This paper contributes to our understanding of low-income households' demand for schools and neighborhoods by quantifying the importance of one such friction: whether families lack information about school quality at the time of their housing search. To do so, we create random variation in the quality of households' information about school quality at a key point in their housing search process. We use this variation, together with a unique dataset on low-income households' housing search activities and residential locations, to estimate a model of demand for housing and neighborhood amenities in which households may have have imperfect information about school quality, as well as potentially systematically inaccurate beliefs about where high-quality schools can be found. We use the model to estimate how much families value school quality relative to other amenities, how much they would appear to value it if we had ignored imperfect information, and what prior beliefs about the distribution of school quality, conditional on other

neighborhood characteristics, can rationalize households' choices.

Our variation in information comes from a randomized controlled trial in which we added school quality information onto housing listings on the largest online provider of such listings for families with housing vouchers. We partnered with GoSection8, which provides online housing listings for housing voucher recipients across the country, to add school-quality information onto their listings for a random sample of users. We tracked detailed individual-level information on low-income households' search behavior on this platform, which we linked to the universe of data on voucher recipients' demographics and residential locations from the Department of Housing and Urban Development (HUD). These data allow us to assess the impacts of information on search behavior and location choices.

We find that imperfect information about school quality causes low-income families to live in neighborhoods with lower-performing, more segregated schools. We first show that providing information about zoned schools and their performance causes households to search for and move to locations with higher-performing zoned schools. The additional information received by "treated" households increases the number of inquiries families made to landlords of units assigned to areas with higher-performing schools. Using the post-intervention location data from HUD, we show that families in the treatment group live in areas assigned to schools that have 0.10 standard deviation higher ratings—equivalent to a 1.5 percentage point higher share proficient on the state exams—than schools assigned to the locations of control group families.[1]

These effects depend on households' informed preferences, on the amount of information that our treatment provides, and on the supply of desirable apartment units. Models that abstract from imperfect information may ascribe families' choices to weak preferences for school quality instead. Our model allows us to measure preferences and information separately while holding supply fixed, and provides context for the magnitude of the information effects. We estimate that families in the treatment group would trade an additional 54 minutes of commute to downtown for a 10 percentile point increase in school quality, holding other neighborhood characteristics constant. If we naively ignored these information frictions, we would

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[1]Note that spillover effects are less of a concern in our setting since in any single market we sample, on average, 3 study participants, which is roughly 1 participant per month.

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understate this valuation by more than 50%; families would trade 24 minutes of commute time for a 10 percentile point increase in school quality.

Moreover, we find that families do not use observable neighborhood characteristics well to predict school quality. We test the hypothesis that households' beliefs coincide with "rational expectations" beliefs, which we estimate using the housing data and OLS regression. We conduct a counterfactual in which households who are not treated with information continue to observe school quality with noise, but form posterior beliefs according to Bayes' rule and the true prior distribution, rather than according to the misspecified prior that we estimate. We find that, if households were misinformed only in the sense of having unbiased noise around the true measure of quality, our information intervention would have caused fewer moves to locations with high-quality schools. This result suggests that allowing for departures from rational expectations based on neighborhood characteristics may be an important in feature to incorporate into models of neighborhood choice for low-income families.

To understand our design, observe that a control-group household may appear to value an amenity such as (low) poverty rates both because of a direct preference for this amenity and because they believe that it predicts a high level of school quality. If the household's belief is correct, then our treatment will inform them only about the residual component of school quality that is orthogonal to the observables used to form the prediction. In contrast, if a household overestimates the extent to which low poverty predicts good schools, then our treatment will cause them to lower their expectation about the quality of schools in low-poverty areas; that is, our treatment will weaken their apparent preference for low-poverty neighborhoods.

Previous research has shown that housing vouchers can induce moves to lower-poverty neighborhoods, which improves the mental and physical health of adults (Katz et al., 2001; Kling et al., 2007; Clampet-Lundquist and Massey, 2008; Ludwig et al., 2013). These moves to low-poverty neighborhoods also improve the long-run outcomes of children who move at young ages (Chetty et al., 2016; Chyn, 2018). However, voucher-induced moves result in substantially smaller changes in school characteristics than in neighbor-

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hood characteristics (Jacob, 2004; Sanbonmatsu et al., 2011; Gennetian et al., 2012; Jacob et al., 2014). More recently, Bergman et al. (2019) show how barriers impede moves to neighborhoods that promote upward mobility for voucher recipients. Our research explicitly incorporates imperfect information into a model of residential choice to examine the extent to which this friction can explain why families move to neighborhoods with low-performing schools.

Several studies use data from centralized allocation mechanisms to estimate families' demand for schools. These studies find that distance to the school and its racial composition are strong determinants of families' choices (Hastings et al., 2009; Glazerman and Dotter, 2017). Families also adjust their listed preferences for schools when information is shown to them about test scores (Weinstein and Hastings, 2008; Corcoran et al., 2018; Allende et al., 2018), though families either cannot infer or do not respond to school value added (Abdulkadiroglu et al., 2017). In centralized open-enrollment settings, families can modify their school applications in response to information at little cost. In our setting, in contrast, moves to higher-quality schools typically require costly residential moves.

Other studies use data on residential location choices to estimate demand for houses and neighborhoods. Galiani et al. (2015) use the Moving to Opportunity experiment to estimate a model of neighborhood choice. Waldinger (2018) and van Dijk (2019) use observational data from centralized assignment mechanisms to estimate the demand for housing characteristics in low-income populations. Bayer et al. (2016) estimate a dynamic model of demand for housing. Davis et al. (2019) estimate a dynamic model of location choices of low-income households, which they use to evaluate the impacts of hypothetical restrictions on the Housing Choice Voucher. These models abstract from imperfect information. We contribute to this research by exploiting a unique dataset that links search behavior to residential locations,[2] by testing how low-income families respond to information in settings where schooling decisions and residential decisions are simultaneously determined, and by incorporating imperfect information into a model of residential choice.

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[2]Waldinger (2018) and van Dijk (2019) observe strategies—rank-order lists and lottery participation decisions—together with outcomes in centralized processes. Other studies of decentralized housing markets, in contrast, typically make use of data on location choices only.

Finally, several papers show that test scores are capitalized into housing prices (Black, 1999; Figlio and Lucas, 2004; Bayer et al., 2007). While in recent years the test scores of schools are shown prominently next to housing listings on all of the major real-estate websites, this information is generally *not* provided to low-income families and voucher holders focused on the rental markets. Prior to our study, the two largest providers of housing listings did not provide any information on school quality.[3] As a result of our work, school-quality information is now provided to low-income families nationwide across the GoSection8 platform.

The rest of the paper is organized as follows. Section 2 describes the setting and relevant background on housing choice vouchers, study partners, the intervention, and subject recruitment. Section 3 reviews the data, and Section 4 presents the results of the experiment. Section 5 and Section 6 describes the model and estimation, respectively. The model results are in Section 7. We conclude in Section 8.

2 Background and Experimental Design

2.1 Housing Choice Voucher Program

The Housing Choice Voucher (HCV) program, managed by the Department of Housing and Urban Development (HUD) program, supports 2.2 million families each year by providing a voucher to help families pay for their rent in the private market. Vouchers are administered locally by Public Housing Authorities (PHAs).

Eligibility for the program is determined by total annual gross income, family size, and citizenship.[4] PHAs are required to provide 75 percent of vouchers to very low income families, defined as families whose income does not exceed 30 percent of the area's median income. In practice, almost all families fall below this cutoff. The program is not an entitlement, so even eligible applicants are often placed on a waiting list. The length of time spent on the wait lists ranges widely between PHAs.

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[3]These two providers are GoSection8 and Socialserve.

[4]Some categories of non-citizens with legal immigration status may also be eligible.

Voucher holders are generally required to pay 30% of their adjusted gross income each month for rent and utilities.[5] The housing subsidy then covers the remainder of the rent up to a maximum amount which is a function of the Voucher Payment Standard.[6] An important implication of this rule is that the typical voucher holder faces little price variation within the set of units that will accept a voucher.

Qualified families who receive a voucher are then free to search for and choose any housing that meets the requirements of the program as long as the voucher is accepted by the property owner as rent. Each month, the housing subsidy portion is paid to the landlord by the PHA on behalf of the family. The family is responsible for the difference between the rent charged by the landlord and the amount subsidized by the program.

2.2   Study Partners

The implementation of the intervention relied on two study partners: GreatSchools and GoSection8. GreatSchools is a non-profit organization that provides free and accessible school quality ratings to families via a web-based platform on www.greatschools.org. The site provides school information, test score information, and school-level ratings for over 200,000 public, charter, and private PK-12 schools nationwide. The school-level rating, which we use as our measure of "school quality" for the treatment, aims to help families compare schools within states. Ratings are whole numbers from one to ten and, at the time of implementation, were constructed by converting proficiency rates within each school, subject, and grade level to a percentile rank within the state, and then averaging these percentiles within each school. GreatSchools labeled these ratings as below average [1-3], average [4-7], and above average [8-10] and color-coded these labels as green for above average scores, orange-yellow for average scores, and red for below average scores. This measure is used by most large-market share real-estate websites, such as Zillow, Trulia, and Redfin.

GoSection8.com (GS8) is the largest online platform for rental listings in the Housing Choice Voucher

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[5]It is possible to lease a unit over the payment standard, but the family is required to pay more than 30%. Legally, the maximum a family can pay is up to 40% of their adjusted gross income, though this can occassionally break down in practice when families lose income (McClure, 2005).

[6]This payment standard is a function of the Fair Market Rent, which is measure of area rental prices constructed by HUD.

housing market in the United States. Their website provides a database for low-income families, both with and without vouchers, to locate and compare rental listings. There is no charge to families for this web-based service. GS8 partners with housing authorities directly to host listings for their areas, and both landlords and housing authorities can post listings. In this manner, the GS8 database has become a primary source of rental listings for many local housing authorities. At the time of the study in 2015, the site received approximately 400,000 unique visitors per month and registered 11,000-13,000 prospective tenants per month.

GS8's interface is similar to that of Zillow or other housing search platforms. Upon arriving to the site, a potential tenant logs in and types in a search query, which can be a county, city, or zip code. This query results in a set of listing titles shown to the user. The listing results page displays the address, rent and the number of bedrooms and baths for each of a group of listings. Both treatment and control group users would observe the same information at this point in their search. Clicking on a listing brings up a detailed listing page, which displays a photo (if available) and characteristics of the unit and neighborhood. These characteristics include location, security deposit, and whether a voucher is required. In addition, the detailed listing page contains contact information of the landlord, as well as a text box in which to submit a "direct inquiry" to the landlord via the website. This implies that users can contact landlords by calling them directly or by using the GS8 direct inquiry button on the unit's listing page. As we describe below, this detailed listings page is what differs between the treatment and control group. We present screen shots of the website through each stage of the subject recruitment process as well.

2.3   Intervention

A registered user on GS8 is randomized into either the treatment group or the control group. When a user enters a search query into the searchbox on the landing page (see Figure 1), GoSection8 returns a list of search results. Both treatment and control group users see the resulting listings, as shown in Figure 2. After clicking on a particular listing, both groups see the property profile (Figures 3), but only those

randomized into the treatment group see the GreatSchools Rating module, shown in Figure 4, which is placed immediately below this profile. The module consists of a school quality ratings legend, a map showing the ratings of the assigned schools to the property being viewed, and a listing of the schools shown on the map.[7] The school listing below the map presents the rating, the school name, school type (all are traditional public), grades served, distance from the property, and a check mark if the school is the assigned school for that particular property.[8] Ratings are displayed within a colored circle, as described above.[9]

Both treatment and control group users can view property information other than the GreatSchools Ratings module (which is the same as what a user who refuses participation in the study views). This information consists of a property photograph, property details, a narrative property description, a Walk-Score section, utilities payment information, additional amenities and appliance details, and handicap accessibility features of the property. The property details also include rental price, number of beds and baths, and an occasional short description written by a landlord.

2.4   Subject Recruitment

Implementation of the intervention began in late May 2015 and ran through February 2017. After visiting the GS8 landing page (Figure 1) and entering a location into the search box, users saw a pop-up asking whether they would like to sign up for the study (Figure A.1). If a user selected "no," they were directed to the website as a non-participant of the study and would not see school-quality information. If a user instead selected "yes," they were taken to a page with the consent agreement and were asked to fill out an initial survey (Figure A.2). Upon completion of the survey, the user was registered both as a tenant registrant on the website and as a participant of our study. Randomization into treatment and control

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[7]Clicking on a school "details" link takes the user to a web page on GreatSchools.org, where they can view additional information for the specific school.

[8]If an assigned school is relevant to a unit's location because it is in an area with centralized school choice, users are shown the closest elementary, middle, or high school.

[9]Since our study, GoSection8 has updated the intervention to make it easier to find listings in areas with better schools, including a text message alert system, a link to find more listings in a given school zone, and the ability to sort listings by school ratings.

was then automated by the website at the moment of survey completion, and any subsequent browsing, searching, and page-viewing were tracked via IP addresses and cookies. Treatment assignment was stratified by voucher status.

We recruited 5,743 participants. Of these, 2,968 reported that they hold a voucher. Of the voucher holding participants, we were able to match 1,969 (66%) heads of households with stated vouchers to HUD data. There is no significant difference in the treatment status between matched and unmatched users.[10] There are various reasons why not all families who reported having a voucher were matched to HUD. First, HUD only records data on households that lease up. National lease-up rates conditional on voucher issuance are approximately 69%, comparable to our match rate (Finkel and Buron, 2001). Second, there may also be self-reporting error regarding first and last names or date of birth. We cannot, unfortunately, discern our consent rate because GS8 did not track a denominator to compute this number. We can, however, compare our study sample to a representative sample from the universe of voucher holders, and, conditional on consent, our HUD-matched sample to the HUD-unmatched sample.

Table A.1 shows this first comparison. Study participants are much less likely to be classified as disabled and have more total household members than the average voucher holder, but total income differences are small and insignificant. Demographic differences, while statistically significant given the large sample from HUD, are all less than 5 percentage points. Table A.2 shows the second comparison of recruited users not matched to the HUD data to those who were. We were much less likely to match those with very young children and those intending to move quickly—within three months.

Lastly, Table A.3 shows how the school ratings associated with the study participants' neighborhoods compares to that of the overall HUD population, GS8 website listings, and US elementary schools. Study participants' school ratings are low, and minority and free-reduced-priced lunch shares are high, compared to the overall HUD population. Ratings for GS8 listings are comparable to overall elementary schools, though these listings tend to be much higher minority share and higher share free-reduced-priced lunch

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[10]See Table 1, which we discuss further below. The last variable shows that there are no significant differences in match rates between treatment and control. We were able to match about two-thirds of GS8 users to HUD databases.

than the average elementary school.

3 Data

In this section, we describe the baseline and outcome data, which are primarily from two sources: GoSection8 and HUD. A third data source, GreatSchools, provided some of the baseline and school characteristics and ratings used for the intervention.

3.1 GoSection8 Data

From GoSection8, we obtained data on users who consented to be in the study and property information. Tenant registration collected information on treatment status, initial sign up date, and results from the baseline survey. The baseline survey (shown in Figure A.2) collected the registrant's name, contact information, head of household's name and date of birth, whether the family has children and their age ranges, intended move date, primary reason for moving, and whether they have a voucher and the associated housing agency. We used name, date of birth, and public housing authority (PHA) in the baseline survey to link to HUD administrative data, which is described further below.

In addition to tenant registration and survey data, we also received a list of properties viewed and direct inquiries (i.e. contacts made via the website to a landlord) by each study participant. With these data, we observe the set of properties a participant viewed, along with property descriptions, rental prices, location, and other characteristics. Direct inquiries are online inquiries sent from prospective tenants to a landlord via a "contact the landlord" link on shown on the right hand side of every GS8 rental listing. It is one method for tenants to show interest in a property, however, it is not the only method. Prospective tenants can also directly call or email the landlord, provided that either or both is available. Inquiry data is useful because tenants cannot view school quality data prior to viewing the detailed property listing.

3.2 GreatSchools Data

GreatSchools provided school-quality ratings, geocoded school locations, and school attendance zones.[11] As of writing, ratings are based on multiple measures of academic quality (student achievement as measured by performance on standardized tests, student progress as measured by year-over-year test score gains, college readiness, etc.), however, at the time of the intervention, ratings were based on each state's test-score proficiency rates, as described above.

The distribution of the universe of elementary school ratings is shown in Figure 5. Ratings tend to peak at six or seven and are approximately normally distributed around a mean of 5.5 and standard deviation of 2.5. Middle and high schools have similar distributions (not shown), though the highest ratings on average are for elementary schools and the lowest are from high schools.

Using clear bars with red outlines, Figure 5 also show the distribution of ratings for properties listed on GoSection8. These properties, which reflect rental supply available to voucher holders, have significantly lower ratings, on average, compared to the general population of schools. Further, there is relatively less variation in ratings within metropolitan areas, although in most counties and cities there is nontrivial variation within the area. 88% of the top 100 cities by number of GS8 rental property listings have a standard deviation in elementary-school ratings of less than 2.5, which is the standard deviation of the universe of schools. The standard deviation of GS8 listings within each city is, on average, approximately 1.8.[12]

3.3 HUD Data

HUD provided quarterly data on the universe of voucher recipients, which includes household member's demographics and income as well as information about where they live: the bedrooms, rent, and geocoded locations.[13] We linked these data to GoSection8 tenant participant data based on first name, last name,

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[11]School attendance zones were purchased from Maponics.

[12]We show elementary school rating standard deviations by county, city, district, and zip code in figure A.3.

[13]The data provided to us are from users who formally filed a Form 50058 with their PHA, which is required annually and when any transaction, such as a new move, occurs.

and the date of birth of the head of household.[14]

As part of the study on GoSection8, we recruited 2,968 eligible participants who reported that their household had a voucher. Of these participants, we were able to match 1,965 heads of households (66%) to HUD data based on name and date of birth using data from 2013 through 2018. Our HUD data span roughly seven quarters after the end of the experiment.

We show baseline sample characteristics and treatment-control group balance in Table 1 by regressing an indicator for treatment assignment on baseline covariates. The regression to check for balance is as follows:

$$X_i = \gamma_0 + \gamma_1 \text{Treatment}_i + \eta_i,$$

where X is the baseline variable of interest for family i and γ is the coefficient of interest for the treatment-control differences. Here, we find that treatment and control groups are generally similar in terms of baseline characteristics. There are some significant differences for the head of household being Hispanic and the probability of being issued a voucher from a housing authority with Moving to Work (MTW) designation (this designation means that housing authority has greater autonomy over how it allocates its resources). The last row of the table shows that match rates to the HUD data are balanced across the treatment and control groups. An omnibus test for treatment-control differences is not significant (p-value=0.19).

As part of our baseline survey, we also asked participants the primary reason for their move. Thirty-two percent of registrants noted that the primary reason for moving is to "get better schools for my children." This share is high relative to prior results from the Moving to Opportunity study (Kling et al., 2007), but this result is in line with more recent evidence from Bergman et al. (2019), which also finds that one third of families' primary reason for moving is for better schools. In addition, more than 75% of the sample intends to move within three months. This intention is in line with the time families have to use a voucher

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[14]We merged data from GoSection8 to HUD using the Stata fuzzy-merging package *dtalink*.

before it expires.

Empirical Strategy

To measure the effect of the treatment on various outcomes, we estimate the following:

$$Y_i = \beta_0 + \beta_1 Treatment_i + \varepsilon_i$$

where Y_i is the outcome of interest for family i, and β is the coefficient of interest on the treatment variable. Our primary outcome is the average rating of the elementary, middle and high school of the assigned schools to a given property. We also look at the associated school demographic characteristics, and the neighborhood and property characteristics as well, such as tract poverty rates, demographics, commute to downtown, and walk score. The walk score is a proprietary measure from Walkscore.com that assesses the "walkability" of an area based on a neighborhood's access to public transportation, friendliness to biking, and local amenities.[15] In the following section, we provide the estimated impacts of the intervention.

4 Results

4.1 Impacts on Search Behaviors

The upper panel of Figure 6 shows the average number of properties families view each week relative to their intended move date, which is normalized to 0 on the X-axis. Weeks prior to the intended move date are therefore shown as negative and weeks after are positive on the X-axis. For both the treatment and control group, there is a rise in the number of properties viewed as the intended move date gets closer, which peaks around an average of two views in one week. These averages include users who did not view any properties in a given week. After the intended move date, the rate of additional properties viewed rapidly declines. The figure also shows there is no significant difference in the patterns across groups, including in

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[15]All users, treatment and control, were shown a module on each listings page which displayed the property's walk score.

the overall number of views. These results provide evidence that the intervention did not change how often families used the website, either in terms of whether a family made any views or how many views.

At the outset, it is less likely we should observe differences in the quality of views because families do not observe school quality before clicking to view a property. However, it is possible they learn and infer quality from neighborhood characteristics. Nonetheless, we do not observe significant differences in the school quality of listings viewed by families. Panel (a) of Figure 7 shows the average number of views a treatment group user makes broken out by the average school quality (averaged across elementary, middle, and high school). Most views have average school quality in the 1 to 4 range, and there are very few views made to listings associated with schools rated 6 and above, which is likely because the supply of such listings is low.[16]

After a family views a unit they may choose to make an inquiry to the landlord about that unit either by phone or via a link on the website. GoSection8 recorded whether a family sends an inquiry to the landlord through their platform, though we cannot observe whether families called landlords. The lower panel of Figure 6 plots the average number of inquiries made each week relative to families' intended move date. Again, these averages include families who made no inquiries in a given week. The pattern is similar to the pattern of views over time: inquiries increase over time with respect to the move date. In contrast to the views, however, the treatment group has a larger increase in inquiries in the week prior to when they would like to move, and slightly higher inquiries before then. The figure also shows that treatment group families make 0.80 inquiries more than the control group families (significant at the 10% level), who average 2.3 inquiries.

Once a treated family views a unit, they also observe the quality of schools assigned to that unit. Therefore, we might see a change in the school quality of the units that families inquire about. Figure 7,

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[16]Table A.4 further summarizes these and other search-related regression results. Families make, on average, 34 views. Roughly 80% of families make at least one view and view properties in 14 different census tracts. There are no significant effects on these outcomes. We also assess the effects on the number of views for schools rated at some school quality rating or above, and find no effects. The exception is the number of views rated 10. Given the small number of schools rated 10—the average control user only saw 0.07 listings with a 10-rated school—this finding may just be due to chance.

panel (b), shows that there is an increase in the school quality associated with the units families inquire about. Most of this increase is concentrated among units with average school quality ratings between 3 and 5.[17]

4.2 Endline School Quality

The additional information causes families to move to higher-rated schools. Using address information from HUD, we assigned the zoned elementary, middle and high school, and their respective Greatschools ratings, to each of our study participants' endline location. Table 2 shows the results across several measures of quality. School quality as measured by the average elementary, middle and high school rating improved by 0.26 points. We also show the maximum or minimum school quality across the schools assigned to an address; the impact for the maximum is 0.32 and the minimum is 0.17, and both remain statistically significant. Figure 8 graphs the density of school quality associated with where families live for the treatment and control groups. There is a shift away from ratings in the 1 to 3 range and toward ratings in 5 and 6 range, similar to where the impacts fall on the quality of inquiries. The effects are slightly larger for the middle school rating than for the elementary or high school rating, though this difference is not significant. To add context to these effects, the average treatment effect is approximately 0.10 standard deviations relative to the control group. Measured in terms of the share of students marked "proficient" or above across a schools' state exams, the treatment effect is 1.5 percentage points.

The intervention also causes families to move less segregated schools. Treatment group families live in neighborhoods assigned to schools with 3 percentage points fewer black or Hispanic students and 2 percentage points fewer students receiving free or reduced-priced lunch, which is an indicator of family income. These effects likely arise because the school ratings are based on test score levels, which strongly correlate with the demographics of the students attending the school.

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[17]Table A.5 summarizes the inquiry-related regression results. The intervention increased the number of inquiries for properties with schools rated 2 or above through 4 or above. We also find that the treatment increased total inquiries by 0.80 over the control mean of 2.3, which is significant at the 10% level. The intervention did not, however, significantly increase the probability that a user inquired about any property nor the number of tracts inquired about.

We also explore whether the effects are concentrated at the school level their parents may send their children to in the future. We define a "next-school" measure based on the age of a family's child(ren) and endline residential location. If the child is 0-4, the "next school" is the zoned elementary school. If the child is 5-10, the next school is the middle school. If the child is 11-13, the next school is high school. Table A.6, which is conditional on families whose child(ren) are in a single age range, shows evidence that families with similarly-aged children tend to target the next school level at the primary, middle, and high school levels. Table A.7 shows the estimates in aggregate for both families with a child or children in a single age range, and for families with children in multiple age ranges. The aggregate treatment effect for families with children in a single age range is larger than the overall treatment effect on school quality seen in table 2. The evidence suggests that families with children target school quality in residential decisions. This result is exploratory.

4.3 Endline Neighborhood Quality

We tested whether the additional school quality information causes families to move to neighborhoods with different characteristics. Table 3 shows these results. In terms of neighborhood demographics, the impacts are small and not significant. There is a 1 percentage point decrease in the neighborhood poverty rate, which may be due to the correlation between school ratings and family income. Figure 9 shows that there is a slight but broad-based shift leftward in the distribution of neighborhood poverty rates for treatment families. For comparison, the MTO experiment induced moves to neighborhoods with 15 percentage point lower poverty rates for families with young children (Kling et al., 2007; Chetty et al., 2016).[18] We also find no impact on the median rent in families' neighborhoods.

There are larger, statistically significant effects on the commute to downtown and the walk score of the neighborhood. The latter reflects what local amenities and public transportation are nearby. Commute to downtown increases by 2 minutes (the control-group mean is 15 minutes), which implies families are moving

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[18]15 percentage points is the Intent-to-Treat Effect.

slightly farther out from the city center to live in areas with better schools. The walk score decreases by 3.9 points (out of 100) compared to a control-group mean of 50. Families appear to move slightly out of downtown in favor of better schools, though we cannot discern from the reduced-form results to what extent families value commute to downtown *vis-à-vis* school quality.

HUD records limited data on the characteristics of the units families live in. Table A.5 shows the treatment effects on unit rent and the number of bedrooms. However, the design of the voucher program implies that there is unlikely to be an effect on these variables. Most families maximize the amount of voucher support they receive, and housing authorities place a maximum on the percent of income families can pay toward rent. Nearly the entirety of our sample pays 30% of their income toward rent. Moreover, families are eligible for a number of bedrooms and subsidy amount based on their household composition, and they cannot choose fewer bedrooms and put the additional funds toward a nicer unit. Thus, it is not surprising there is no impact on the rent paid and the number of bedrooms. Instead, as discussed above, families appear to trade off unit location—distance from downtown—for additional school quality.

The last neighborhood characteristic we study is a measure of upward mobility: Predicted household income rank. Predicted household income rank comes from the Opportunity Atlas, and measures, for a given census tract, the average household income percentile rank at age 35 for children who grew up in low-income (25th percentile) families (see Chetty et al. (2018)). Table A.5 shows that treatment group families live in tracts with, on average, a rank that is 0.7 percentile points higher than control group families. To provide context for this result, we follow the approach of Bergman et al. (2019), who test a much more intensive effort—called Creating Moves to Opportunity (CMTO)—to remove barriers that impede neighborhood choice for low-income families. Chetty et al. (2018) find that 62% of the variation in upward mobility is due to the causal effect of the place, so Bergman et al. (2019) multiply the treatment effect of CMTO on predicted household income rank by 0.62 to find the resulting causal effect for a child impacted by the intervention at birth. We find the additional information has a predicted causal effect on household income (when children reach their mid 30s) of 0.44 percentiles (0.71*0.62). This is 13% of the

impact of CMTO, which has a predicted increase in income of 3.3 percentiles.

Lastly, we explore whether families appear to respond to the portion of school quality that is not correlated with the neighborhood's other characteristics. We regress the school-quality measure on neighborhood racial composition, poverty rate, median gross rent, walk score, and distance to downtown, and then regress this residualized measure on the treatment indicator.

The effect on residual school quality, shown in the bottom row of table 3, is smaller but still large and statistically significant, suggesting that households are not just using school quality as a signal of the other amenities that we observe. We also show the other component of this regression of school quality on neighborhood characteristics: predicted school quality. If households had formed prior beliefs whose means coincided with OLS estimates of predicted quality, then our treatment would have informed them only about the residual component, which is mean-independent of the predicted value. Hence, if households were using these variables to formulate a "correct" prior, we should see a zero treatment effect on predicted quality. Our findings indicate that households may not be using this information to predict quality well. In what follows, we formalize this logic in a model.

5 Model

We now specify and estimate a model of households' search behavior and moving decisions, recovering households' preferences, their costs of using the platform and of inquiring about and visiting housing units, and their prior beliefs about the joint distribution of school quality and other characteristics. The experiment allows us to assess the importance of imperfect information for a model of neighborhood choice, and, in particular, how imperfect information affects estimated valuations of school quality *vis-à-vis* other neighborhood characteristics.

We do this as follows. First, we obtain estimates of search costs, treated households' preferences for neighborhood characteristics and school quality, and the "as-if-full-information" preferences of control households that an econometrician would estimate when not incorporating imperfect information. A direct

comparison of the treated households' preference parameters to the "as-if" preferences of the control group reveals the bias in estimated preferences for neighborhood characteristics and school quality when imperfect information is ignored. Second, we use the estimated parameters to solve for families' prior beliefs about school quality under the hypothesis that they are Bayesian updaters under our treatment, and then we test whether symmetric measurement error around rational expectations of school quality based on observable neighborhood characteristics could be a viable approach to modeling imperfect information in this setting.

The model is designed to capture the following features of the market. First, housing search involves a finite deadline, for households who are currently using a voucher as well as for new voucher recipients who have not yet leased up using the voucher. New voucher recipients lose their voucher if they fail to use it within a given period. Incumbent households' leases expire at dates which are known to them. These households must make a decision to renew their lease or move elsewhere before this deadline. A typical lease commits a household to pay rent at that location for a year. Therefore, all types of households in our model face a stopping problem with a finite deadline.

Second, because demand exceeds supply, vacancies are short-lived. At the same time, it may take landlords a few days to respond to inquiries. Our model therefore treats search as simultaneous within a period (in our empirical specification: one week), but we assume no recall across periods.[19]

Third, not all search is conducted online. Households may discover other apartments through social networks or other means. Our model therefore allows for off-platform search as well as on-platform search.

5.1 A model of search

Time is discrete: $t = 1, \ldots, T < \infty$. Household $i \in I$ in market $m \in M$ becomes active in period 1 (i.e. the head of household receives a new voucher, or decides to move), and has until time T to find an apartment. The set of available apartments in market m is denoted $\mathcal{J}_m$. If i moves to an apartment $j \in \mathcal{J}_m$ before

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[19]A potential tenant may submit multiple inquiries in one session, then wait a few days for a response, but must then decide quickly. We conducted an audit study of landlords' responses to inquiries; we found that 41% of landlords responded to inquiries, typically with a lag of a day or two.

time T, it receives a payoff of

$$u_{ij} = x_j \beta^x + q_j \beta^q + \epsilon^0_{ij} + \epsilon^1_{ij}$$

where x_j is a vector of neighborhood characteristics of unit j, q_j is school quality, and ϵ^0_{ij} and ϵ^1_{ij} are idiosyncratic preference shocks. There is no discounting.[20] However, if the household fails to accept an apartment by time T, it receives an outside-option payoff which is normalized to zero.

Within each period, each active household makes three choices: whether to use the online platform; which subset of units to inquire about and/or visit; and whether to accept a unit. We now discuss these in detail.

At the beginning of period t, household i observes a cost draw

$$c^{search}_{it} \sim F_{c^{search}}(\cdot),$$

i.i.d. across periods. i then chooses whether to pay this cost in order to use the search platform. If so, it discovers a set of on- and off-platform units. If not, it discovers only off-platform units. On-platform units $J^{on}_{it} \subset \mathcal{J}_m$ are drawn at random from a distribution over sets of apartments,

$$F_{go8}(\{x_{ij}, q_{ij}, \epsilon^0_{ij}, \epsilon^1_{ij}\}_{j \in J^{on}_{it}}; m),$$

conditional on searching. If i does not use the platform in period t, then $J^{on}_{it} = \emptyset$. Off-platform units J^{off}_{it} are drawn from an analogous distribution,

$$F_{off}(\{x_{ij}, q_{ij}, \epsilon^0_{ij}, \epsilon^1_{ij}\}_{j \in J^{off}_{it}}).$$

Let $J_{it} = J^{on}_{it} \cup J^{off}_{it}$ denote the set of units that i discovers in period t.

Upon discovering a unit, a household observes characteristics x_j, a preference shock component ϵ^0_{ij}, and

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[20]An interpretation is that the household is searching for a lease which begins at time T.

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a signal of school quality, $\hat{q}_{ij}$. In order to observe the second preference shock component ϵ_{ij}^1, the household must pay an additional cost $c_{it}^{inquire}$ *per unit* that it wishes to learn about. We model this second decision as a simultaneous search problem. After observing the set of units, the household learns its inquiry cost draw,

$$c_{it}^{inquire} \sim F_{c^{inquire}}(\cdot),$$

drawn i.i.d. across periods, and chooses a set of units to submit inquiries to,

$$J_{it}^{inquiry} \subseteq J_{it}$$

at cost $|J_{it}^{inquiry}|c_{it}^{inquire}$.

Finally, the household observes ϵ_{ij}^1 for each unit that it inquired about, then chooses whether to accept a unit. If the household accepts unit j it receives u_{ij}. Otherwise, it continues to the next period. Households may not accept units that they have not inquired about.

If household i is not part of the treatment group, it observes a noisy signal of quality, $\hat{q}_{ij} \sim G(\hat{q}_{ij}|q_{ij})$. If i is treated with information, then $\hat{q}_{ij} = q_{ij}$ almost surely.[21] Households are risk-neutral, and form expectations $E(u_{ij}|\hat{q}_{ij}; x_{ij}, G_{q|x}(\cdot))$ where $G_{q|x}(q|x)$ is a belief over the conditional distribution of q_{ij} given characteristics x_{ij}.

$G_{q|x}(q|x)$ arises from an information structure in which households observe the sum of the true quality q and additive measurement error η. Households assume that true quality and measurement error are jointly normal conditional on x. In particular, for prior-mean parameters γ and variance parameters σ_η^2 and σ_q^2, for untreated households, the signal distribution is given by

$$\begin{pmatrix} q \\ q + \eta \end{pmatrix} | x \sim N \left(\begin{pmatrix} x'\gamma \\ x'\gamma \end{pmatrix}, \begin{pmatrix} \sigma_q^2 & \\ \sigma_q^2 & \sigma_q^2 + \sigma_\eta^2 \end{pmatrix} \right).$$

---

[21] It is possible to estimate specifications in which households may have imperfect signals of quality for off-platform listings, $\hat{q}_{ij} \sim G_{\text{off}}(\hat{q}_{ij}|q_{ij})$. Our specification assumes that households learn about off-platform units as well when treated.

Importantly, the prior mean belief $x'\gamma$ need not coincide with the "rational expectations" prior, which we define as that which the econometrician would estimate from data on all listed units. Households who are Bayesian updaters will shrink the signal that they observe toward this (potentially misspecified) subjective mean.

In our empirical specification, preference terms x_j include a constant, ACS demographics (median gross rent, poverty rate, percent Black households), walk score, distance to downtown, and school quality. Households may also use non-school characteristics to form beliefs about school quality. Here, median rent proxies for the cost and amenities of the neighborhood.[22] Controlling for median rent absorbs the variation in school quality that is capitalized into the price of a typical housing unit in the neighborhood. The term ϵ_{ij}^0 is intended to capture match-specific characteristics, such as the proximity of the unit to family or childcare providers, which households can infer from the listing, while ϵ_{ij}^1 represents characteristics such as the quality and condition of the housing unit, which cannot be learned without contacting the landlord or visiting the unit.

We complete the model with the following distributional assumptions on preferences, costs and listings:

$$c_{it}^{\text{search}} \sim LogNormal(\mu_{\text{search}}, \sigma_{\text{search}}^2)$$
$$c_{it}^{\text{move}} \sim LogNormal(\mu_{\text{inquire}}, \sigma_{\text{inquire}}^2)$$
$$\epsilon_{ij}^0 \sim N(0, \sigma_{\epsilon}^2) \text{ i.i.d.}$$
$$\epsilon_{ij}^1 \sim Gumbel() \text{ i.i.d.}$$

These assumptions guarantee that costs are positive. Independence of costs across periods is not essential, and can, in principle, be relaxed.

On-platform listings are sampled from the empirical distribution of $\{J_{it}\}_{i \in I_m, \ t=1,...,T}$ by market. We assume off-platform units are drawn from the same distribution as on-platform units, and that the number

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[22]ACS median gross rent is not the rent that the tenant would pay. Recall that tenants typically pay 30% of their income, regardless of the unit chosen.

of off-platform units drawn in a period is distributed $Poisson(\lambda)$.

5.2   Optimal play and qualitative predictions

Let V_{it} denote the expected payoff of household i if it is active at the beginning of period t. Fix $V_{i,T+1} = 0$. Let

$$\hat{u}_{ij} \equiv E(u_{ij}|\hat{q}_{ij}, x_{ij}, \epsilon^0_{ij}, \epsilon^1_{ij}; G(\cdot))$$

denote i's expected utility from property j when i observes signal $\hat{q}_{ij}$ and has prior belief $G(\cdot)$. i accepts a unit with a successful inquiry if and only if $\hat{u}_{ij} > V_{i,t+1}$.

Let

$$\hat{v}_{ij} = E(x_j \beta^x + q_j \beta^q + \epsilon^0_{ij}|\hat{q}_j)$$

denote the expected value of unit j, not including the final shock ϵ^1_{ij}, when the household observes signal $\hat{q}_{ij}$. If the household is going to inquire about k units in period t, the optimal units are those with the k highest values of $\hat{v}_{ij}$. The value of this set of inquiries is given by

$$U^k_{m,t}(\theta) = V_{t+1}(\theta) + \log\left(1 + \sum_{j=1,\ldots,k} \exp(v_{ij}(\theta) - V_{t+1}(\theta))\right).$$

Because the cost is linear in k and the marginal return to an additional inquiry is decreasing, the optimal strategy is to choose the smallest k such that the marginal gain from the $k+1$th unit is smaller than $c^{inquire}_{it}$.

The payoff from on-platform search is given by

$$V^1_{it} = E\left(\max\left\{V_{i,t+1}, \max\{\hat{u}_{ij}|j \in J^{inquiry}_{it}\}\right\} - c^{inquire}_{it}|J^{inquiry}_{it}|\right),$$

where the expectation is over the sets $J_{it} = J^{off}_{it} \cup J^{on}_{it}$ and the inquiry cost $c^{inquiry}_{it}$.

Analogously, let V^0_{it} denote the expected payoff from no on-platform search in period t, i.e. when $J^{on}_{it} = \emptyset$ with probability 1.

i chooses to search in period t if

$$V_{it}^1 - V_{it}^0 > c_{it}^{search},$$

The value V_{it} is then given by the value of the option to search, i.e.

$$V_{it} = V_{it}^0 + \left(V_{it}^1 - V_{it}^0 - E(c_{it}^{search}|c_{it}^{search} < V_{it}^1 - V_{it}^0) \right) F_{c^{search}}(V_{it}^1 - V_{it}^0).$$

Intuitively, the search cost threshold is falling in time. With a long horizon, there is a good chance of low cost draws in future periods, and moreover it is more likely that a good unit will arrive via off-platform search in the future. With a short horizon, search is more valuable. Thus, conditional on being active, the probability of search and number of inquiries is higher as the deadline draws closer. Close to the deadline, however, many households may no longer be active, so that peak period of search activity may be interior.

6   Identification and Estimation

We first provide an intuitive discussion of identification and sources of variation. We then discuss parametric identification of our model's belief parameters and present our estimation procedure. Our parametric assumptions are convenient, but we do not believe that normality of signals or linearity of the utility index is essential.

6.1   Identification and testable restrictions

Observe that one may estimate a full-information model, obtaining estimates of the distribution of preference and cost parameters, using only treated households. Identification of treated households' demand is standard. In particular one may restrict attention to the final period, reducing the problem to a static problem.[23] The key novelty here is the estimation of subjective prior beliefs and signal parameters, which

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[23]Conditional on inquiry decisions, the problem is standard. There is selection into the decision to inquire about a property, but the characteristics of other units j' discovered in period t shift the threshold cost at which a household inquires about unit j.

relies on the variation induced by the randomized experiment.

Under Bayesian updating with a correctly specified prior, for a general utility function $v(x_j, q_j, \epsilon^0_{ij})$, the expected utility of a unit j is a Martingale with respect to the information provided by our treatment.[24] Because the average of the agent's posterior mean utility conditional on our treatment is identical to the average of the posterior mean utility conditional on a control-group agent's information for all x, treatment effects on mean utility of units cannot systematically vary with non-school characteristics x. That is, while the provision of information about q may increase demand for units with higher q conditional on x, it cannot systematically raise or lower the agent's expected utility of a unit given its information *as a function of x*.

In contrast, a nonzero treatment effect on the expected utility of units with characteristics (x', q, ϵ_0) relative to those with (x, q, ϵ_0) indicates a violation of the hypothesis of Bayesian learning with a correctly specified prior.[25] For instance, households may believe that high poverty rates in a neighborhood generally predict low school quality to a greater extent than warranted by the data. Our treatment will then increase the expected utility that households believe they receive from high-poverty neighborhoods, and therefore the probability of choosing such neighborhoods.[26] Intuitively, estimating prior beliefs about the quality of units with characteristics x relative to those with characteristics x' amounts to estimating treatment effects on the demand for units with characteristics x relative to those with x'.

We may also learn about the informativeness of control households' signals from their demand for units as a function of q. If demand is completely unresponsive to quality q conditional on other characteristics x, we may conclude that control-group households observe no additional information about q. A higher probability of choosing high-q units would indicate that control households observe a signal that conveys some information.

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[24]Let $v_{ij} = v(x_j, q_j, \epsilon^0_{ij})$ be any random variable with finite first moment, and suppose utility is given by $u_{ij} = v_{ij} + \epsilon^1_{ij}$ with $\epsilon^1_{ij} \perp v_{ij}$. Let $\hat{q}_{ij}$ be a signal of q_j. When households have a correctly specified prior belief $H(v|x, \epsilon^0)$, we have

$$E(E(v|x, \epsilon^0, q)|x, \epsilon^0) = E(E(v|x, \epsilon^0, \hat{q})|x, \epsilon^0) = E(v|x, \epsilon^0).$$

[25]We may integrate out ϵ_0.

[26]Mean utilities $E(v|x, q)$ are an invertible function of choice probabilities under fairly general conditions (Berry et al., 2013).

6.2 Beliefs and expected utilities

We now make this argument precise in the context of our model. We begin by deriving the distribution of indirect utilities according to the econometrician. The agent's expectation is given by

$$E(q|q + \eta) = s \cdot (q + \eta) + (1 - s) \cdot \mu_q,$$

where $s \equiv \frac{\sigma_q^2}{\sigma_q^2 + \sigma_\eta^2}$. Dropping subscripts, expected utility given i's information is

$$
\begin{aligned}
\hat{u} &= x'\beta + \left(s\hat{q} + (1 - s)x'\gamma\right)\beta_q + \epsilon \\
&= x'\left((1 - s)\beta_q\gamma + \beta_x\right) + (q + \eta)s\beta_q + \epsilon \\
&= x'\left((1 - s)\beta_q\gamma + \beta_x\right) + qs\beta_q + (s\beta_q\eta + \epsilon) \\
&= x'\tilde{\beta}_x + q\tilde{\beta}_q + \tilde{\epsilon},
\end{aligned}
$$

where

$$
\begin{aligned}
\tilde{\beta}_x &= (1 - s)\beta_q\gamma + \beta_x \\
\tilde{\beta}_q &= s\beta_q \\
\tilde{\epsilon} &\sim N(0, s^2\beta_q^2\sigma_\eta^2 + \sigma_\epsilon^2).
\end{aligned}
$$

It follows that, for each $(\beta_x, \beta_q, \gamma, \sigma_\eta^2, s, \sigma_\epsilon^2)$, there is an observationally equivalent "full information" model, in which q is observed without error. In this model, the household acts as if it has preference parameters $\tilde{\beta}_x, \tilde{\beta}_q$, and $\tilde{\sigma}_\epsilon^2$. The cost parameters are unchanged.

$\tilde{\beta}_x$ and $\tilde{\beta}_q$ are the parameters would we would estimate if we ignored imperfect information. The model specifies the resulting bias: β_q will be shrunk by the signal to noise ratio, s, so it will appear families value school quality less than they actually do. The bias in $\tilde{\beta}_x$ arises as families use other neighborhood

characteristics to predict school quality via γ. For example, say families dislike high neighborhood poverty rates, which implies that $\beta_{poverty}$ is negative. If families further believe that the poverty rate negatively correlates with school quality, the γ associated with this characteristic is negative as well. Then, ignoring imperfect information, the estimated distaste for the neighborhood poverty rate would be larger than that estimated under perfect information.

Our approach is to recover "full-information" preference parameters separately by treatment status. These parameters are sufficient for obtaining "as if" willingness-to-travel measures $\frac{\tilde{\beta}_q}{\tilde{\beta}_{commute}}$ relating taste for quality to distaste for distance from city center for treatment and control households.

We then solve for the values of γ, s, σ_η and subjective prior variance σ_q that uniquely rationalize the estimated parameters. In particular, given estimates $\hat{\beta}_x, \hat{\tilde{\beta}}_x, \hat{\beta}_q, \hat{\tilde{\beta}}_q, \hat{\sigma}_\epsilon^2, \hat{\tilde{\sigma}}_\epsilon^2$, we compute

$$\hat{s} = \frac{\hat{\tilde{\beta}}_q}{\hat{\beta}_q}$$

$$\hat{\gamma} = \frac{\hat{\tilde{\beta}}_x - \hat{\beta}_x}{\hat{\beta}_q - \hat{\tilde{\beta}}_q}$$

$$\hat{\sigma}_\eta^2 = \frac{\hat{\tilde{\sigma}}\epsilon^2 - \hat{\sigma}_\epsilon^2}{\hat{\tilde{\beta}}_q^2}$$

$$\hat{\sigma}_q^2 = \frac{\hat{\sigma}_\eta^2 \hat{s}^2}{1 - \hat{s}^2}.$$

Our model has a unique γ that exactly rationalizes the estimates. When the "objective" γ can be consistently estimated, we can test the hypothesis that households' subjective priors are equal to it. In particular, we test whether $\gamma = \gamma_{OLS}$, where γ_{OLS} are the coefficients that the econometrician would obtain from a regression of q on neighborhood characteristics x in the sample of listings.

6.3 Estimation procedure

To better fit the data, we extend the model in two ways. First, we allow for "inquiries" to take place via channels which we do not observe, such as phone calls. We estimate a parameter, $Pr(\text{observe inq.})$, which denotes the probability that an inquiry occurs via the "direct inquiry" text box. We assume that this probability is invariant to our treatment, and that inquiries by text box and by phone or other means are otherwise identical. Second, we allow for the presence of a persistently "passive" type, which completes our survey but never views a listing or moves. We estimate the probability, $Pr(\text{passive})$, that households are of this type.

We estimate the following parameters via the method of simulated moments: $\beta^x(\text{treat})$, $\beta^q(\text{treat})$, $\beta^x(\text{control})$, $\beta^q(\text{control})$, μ_c^{search}, σ_c^{search}, μ_c^{inquire}, $\sigma_c^{\text{inquire}}$, $\sigma_\varepsilon(\text{treat})$, $\sigma_\varepsilon(\text{control})$, $Pr(\text{observe inq.})$, λ, $Pr(\text{passive})$.

In order to restrict λ and cost parameters to be identical across treatment status, we estimate the model jointly on treatment and control groups. Doing so is not essential; an alternative procedure would involve estimating the model separately by group.

Our moments are of the form

$$g = \frac{1}{n} \sum_{i=1}^{n} \left(E(y_i^{model}(\theta)) - y_i^{data} \right) w_i$$

where $w_i \in \{0, 1\}$ are indicators for treatment groups, the sum is over households i, and y are the following objects:

1. $\sum_{t=1}^{T} Pr(\text{search in period } t) * b_k(t)$, for basis vectors $b_k(\cdot)$;

2. $\sum_{t=1}^{T} E(\text{number of inquiries in period } t) * b_k(t)$;

3. $\sum_{t=1}^{T} E\left((\text{number of inquiries in period } t)^2\right)$;

4. inquiry characteristics: $\sum_{\{j:i \text{ inquires at } j\}} x_j$, $\sum_{\{j:i \text{ inquires at } j\}} q_j$;

5. Lease-up and leased unit characteristics: $1(i \text{ leases up at some } j*)x_{j*}$; $1(i \text{ leases up at some } j*)q_{j*}$.

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We choose the following basis vectors in order to reduce the total number of moments needed:

$$b_k(t) = \cos(\pi kt/(T-1)) \text{ for } k \in 0, 1$$

. Thus, our moments relating to the timing of views and inquiries consist of the expected total number of views and inquiries in the $k = 0$ case, respectively, and decreasing functions that capture time trends for $k = 1$. Moments (3), which match the second moment of weekly views, are crucial for estimating the distribution of inquiry costs. Intuitively, when the variance of costs is high, so is the variance of the number of inquiries, as households will either inquire at no units or all the units in a week with high probability. The other moments match averages of household-level variables that pin down preferences. Here, x_j is a vector of all characteristics entering the model, including a constant term.

To compute the moments, we solve the model separately for each treatment group at each trial parameter vector. We use the standard two-step approximation to the optimal GMM weighting matrix. We obtain standard errors via the bootstrap, using 100 bootstrap draws in which we resample the sets of households and viewed properties. In addition, we use these draws to compute standard errors of estimates of derived parameters such as γ.

To obtain "rational expectations" estimates $\hat{\gamma}_{OLS}$ and $\sigma^2_{q,OLS}$, we estimate an OLS regression of q on x in the sample of housing units ever shown to a user on GoSection8. We then conduct a Wald test of the hypothesis $H_0 : \gamma_{OLS} - \gamma = 0$. To estimate the covariance matrix of $\gamma_{OLS} - \gamma$, we compute this quantity for each bootstrap draw.

We provide computational details in the appendix.

7   Results

In Figure A.5 we show the fit of targeted moments. In general our model fits the data well, especially for key moments such as school quality at endline and mean quality of units at which households inquire.

Figures A.6 and A.7 show model-predicted and observed patterns of views and inquiries by period. The model-predicted series are too variable, but tend to match the level and slope of the observed series.

We present parameter estimates in Table 4. Control households appear to dislike high-walkscore neighborhoods, perhaps because they view them as signals of poor schools. Households observe roughly one off-platform unit per week. We find that both treatment and control-group households have apparent tastes for higher-rated schools and lower commute time to city center. A treated household is willing to trade off commute distance and mean school quality at a rate $\beta_q/\beta_{\text{commute}} = -0.904$, or 54 minutes per GreatSchools rating point.[27] In contrast, the apparent preferences of control-group households imply a ratio of $\tilde{\beta}_q/\tilde{\beta}_{\text{commute}} = -0.403$, or 24 minutes per point, implying that, had we estimated the model ignoring information frictions, we would have mistakenly found roughly half the true willingness to travel for school quality.[28]

The implicit counterfactual underlying this willingness-to-travel measure involves moving people from neighborhoods with characteristics (x,q) to those with characteristics (x',q'). This is a standard counterfactual in the housing literature, e.g. see Black (1999), Bayer et al. (2007), and can be interpreted as measuring a willingness to travel to live in a neighborhood that has better schools but is similar in other neighborhood-level observables. Because our counterfactual does not change the joint distribution of (x, q, ϵ), we do not need to distinguish between a taste for q directly and a belief that high test scores predict other unobserved measures of school or neighborhood quality. A different willingness-to-pay measure, which we do not consider in this paper, would involve a willingness to trade off commute time for an improvement in the technology of the local school which raises proficiency rates, holding unobserved housing-unit characteristics fixed.

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[27]Not all households work in the city center, so distaste for commute to work location would likely be larger than β_{commute}, resulting in a ratio which would be smaller in magnitude.

[28]In interpreting our willingness to pay measure, one may ask whether we are simply making school quality more salient. If we are only reminding households of the existence or importance of school quality, then households who are already motivated to search for high-quality schools should be unaffected. In our intake survey, before we assigned households to treatment or control, we elicited a reason for moving. In table A.8 we examine heterogeneity in treatment effects on the basis of this reason. Households who report that they are moving in order to attend better schools do not differ in impacts on school quality, school % FRPL, school % Black or Hispanic, or neighborhood poverty.

In Table 5 we compare households' belief parameters (γ, σ_η) to those from an OLS regression of mean quality on characteristics in our sample of housing units. In addition, we report confidence intervals on the prior mean parameters, γ. Estimates of individual elements of γ are noisy, but point estimates indicate that households are pessimistic about school quality. A Wald test of equality of $\gamma = \gamma^{\text{OLS}}$ overwhelmingly rejects this hypothesis ($p < 0.0001$). Families appear to slightly underestimate the variance of q conditional on x, as we show further below. Moreover, control-group families have a signal with $(1-s) \approx 0.5$, implying that the signals are informative about school quality, but not perfectly so.

We plot the implied distributions of residual quality $q - E(q|x)$ in Figure 10. This residual is the object that our information treatment provides to households when their signal η is completely uninformative. The OLS residuals are centered around zero by construction while the subjective beliefs are shifted substantially to the right. This implies that households generally believe school quality is lower than would be predicted by the econometrician; households are pessimistic about locations at which they do not currently live. Our treatment causes households to update positively about the presence of higher-quality schools in their choice set. Overall, our findings imply that we cannot model households' beliefs as measurement error around rational expectations.

Finally, we quantify the importance of the subjective prior by comparing observed and model-predicted outcomes to counterfactual outcomes that would occur if control-group households had the same treated households' preferences, costs, and measurement-error distributions as in our estimated model, but were endowed with the OLS prior rather than the subjective prior. The first two columns of table 6 show model fit. The first column, labeled "data," reports treatment effect estimates. We report coefficients on treatment status from regressions of outcomes y on treatment status. These outcomes consist of total inquiries (1st row), 1(lease up in a location at which the household has not previously used a voucher) (2nd row), and endline "next-school" quality conditional on leasing up in a new location (3nd row). We find a noisy but positive point estimate of impact on total inquiries, and a significant positive impact on endline quality, as in our main results. The second column, labeled "model," reports treatment effects on predicted inquiries

and predicted endline quality according to our estimated model. We match the point estimates for inquiries, moves, and quality closely. Our treatment leads to about a tenth of an additional observed direct inquiry per household, an increase of about .3 GreatSchools points at endline conditional on leasing up at a new location, and zero impact on the rate of leasing up at a new location.

The final column of table 6 shows the results of a counterfactual in which households are endowed with the rational-expectations prior shown in table 5, but control-group households and treated households viewing off-platform units, continue to observe q with normal measurement error with the same variance σ_η^2 as in the estimated model.[29] In this counterfactual, we find that treatment effects conditional on leasing up at a new unit would be similar or slightly higher, but inquiries would decrease, and the probability of leasing up at a new unit would decrease by about two percentage points. In summary, households are pessimistic under the beliefs we estimate. Our treatment encourages leasing up at a new location by causing households to update positively about school quality. When this channel is absent, households may be more likely to remain in place (if currently using a voucher) and/or fail to use the voucher.

It is valuable to discuss two forms of landlord behavior that may affect interpretation of our results: the decision to make a unit available for renters, and the decision to discriminate against voucher holders. First, one may worry that units in areas with high rent or good schools are negatively selected in terms of unit quality, conditional on being listed on goSection8. In neighborhoods in which the median rent is low, the owner of the median unit has stronger incentives to advertise to voucher holders. In expensive areas, only lower-quality units may be listed. To the extent that this is the case, our measure, which includes any changes in unit quality associated with a move to a higher-school-quality neighborhood, may underestimate households' willingness to pay for a technological improvement in school quality.

Second, one may worry that landlords discriminate against voucher holders, for instance by falsely claiming that a vacancy no longer exists. Our error term ϵ^1 implicitly includes landlord behavior such as hostility or delays in responding to inquiries. If such behavior varies systematically with observables,

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[29]Our estimates of σ_ϵ and β_q imply $s = 0.49$ and $\sigma_\eta = 2.03$, so the signal remains nontrivially informative.

however, then our estimates of β will not be interpretable as household preference parameters. For example, if landlords in neighborhoods with better schools discriminate more, and families search in these areas, we may underestimate households' valuation of school quality. To test for this possibility, we conducted an audit study in which we sent 1,450 messages to landlords on GoSection8.com with units chosen at random from 9 states, and evaluate whether the landlord replied.[30] In roughly half of these messages, the potential tenant expressed intent to use a voucher.[31]

We summarize the results in table A.9. First, we find no discrimination on the basis of intent to use a voucher among these landlords. Column (1) regresses an indicator for landlord replies on voucher status and controls. All specifications include county fixed effects. We find a small *positive* effect on responses, and cannot reject a zero effect. This result may be unsurprising given that the landlords have chosen to advertise their properties on GoSection8.com, a site which, by name, caters to voucher holders. Second, we find no relationship between response rates and school quality. A 1-point increase in the GreatSchools rating is associated with a statistically insignificant 0.7% decrease in response rates. We can reject effects larger than roughly 2 percentage points. Third, of all characteristics we observe, we might expect market rents to be the most strongly correlated with discrimination. Nonetheless, columns (3) and (4) indicate no relationship between neighborhood median gross rent and response rates. We show these results hold if we restrict the sample to messages explicitly indicating the household intends to use a voucher. Taken together, these results suggest that, in our sample, differential discrimination against voucher holders as a function of neighborhood characteristics is less present than in the broader market (i.e., outside of GoSection8).

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[30]These states are Massachusetts, Connecticut, Rhode Island, New York, New Jersey, Pennsylvania, New Hampshire, Vermont and Maine.

[31]An example of one such message read, "Hello, I am interested in this unit. Can I take a look at it? Also, I will be using a Section 8 voucher. Thank you, [name]." The corresponding no-voucher-intent message read, "Hello, I am interested in this unit. Can I take a look at it? Thank you, [name]." Names were randomly assigned.

8   Conclusions

In this paper, we ask whether low-income families lack information at the time of their search for a home, and whether providing this information affects how they search and where they live. We show that providing this information causes families to live in neighborhoods with higher-performing, less segregated schools. We estimate a model of residential choice that incorporates imperfect information about school quality. Our estimates show that untreated households act as if they are mistaken about the distribution of school quality and other housing characteristics. As a result, there may be returns to providing information that can help households make informed choices. In this paper, however, we do not consider the general equilibrium impacts of information provision that might arise if scaled.[32]

Low-income households encounter many barriers to moving. Households can find it difficult to pay security deposits and application fees, and landlords can discriminate against their race or source of income.[33] Moreover, a lack of supply may incentivize households to accept units even where school quality is low. A number of interventions and policies attempt to ameliorate these frictions. Schwartz et al. (2017) found that short-run financial incentives and housing counseling had almost no impact on voucher holders' neighborhood choices, though Galiani et al. (2015) find that the counseling part of the Moving to Opportunity intervention had meaningful impacts. One policy promulgated by HUD is Small Area Fair Market Rents, which allows housing authorities to offer higher subsidies in higher-rent neighborhoods; this policy induces moves to more expensive areas, but its potential impact on economic mobility is much more muted (Collinson and Ganong, 2018; Bergman et al., 2019). Other policies attempt to alter the supply of available housing, such Low-Income Housing Tax credits and housing redevelopment (cf. Ellen et al. 2016). The Creating Moves to Opportunity intervention attempts to address multiple barriers simultaneously by providing short-run financial assistance, information and search assistance and landlord outreach, which

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[32] For recent evidence on the general equilibrium effects of low-income housing policies as well as information, see Allende et al. (2018); Davis et al. (2018); Diamond and McQuade (2019); Davis et al. (2019).

[33] A number of recent papers have conducted audit studies of online housing listings (outside of GoSection8), including Phillips (2017); Moore (2017); Faber and Mercier (2018); Aliprantis et al. (2019). See Kain and Quigley (1975) for a model of racial discrimination and housing choices.

generated large effects on households' moves to neighborhoods that promote economic mobility for young children.

Our results suggest that, if the goal is to help households move to neighborhoods with better schools or other amenities, information provision may be a valuable complement to these more-expensive policies. While not generating impacts on neighborhood choice as large as MTO or CMTO, it may be a valuable complementary policy and is considerably more cost effective. Our intervention has already been scaled across the GoSection8 platform, which serves hundreds of thousands of users each month.

Lastly, we demonstrated that families respond to one particular type of school-quality information. However, future work could study whether families respond differently to information about other school and neighborhood characteristics.

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Figures

Figure 1: Landing page
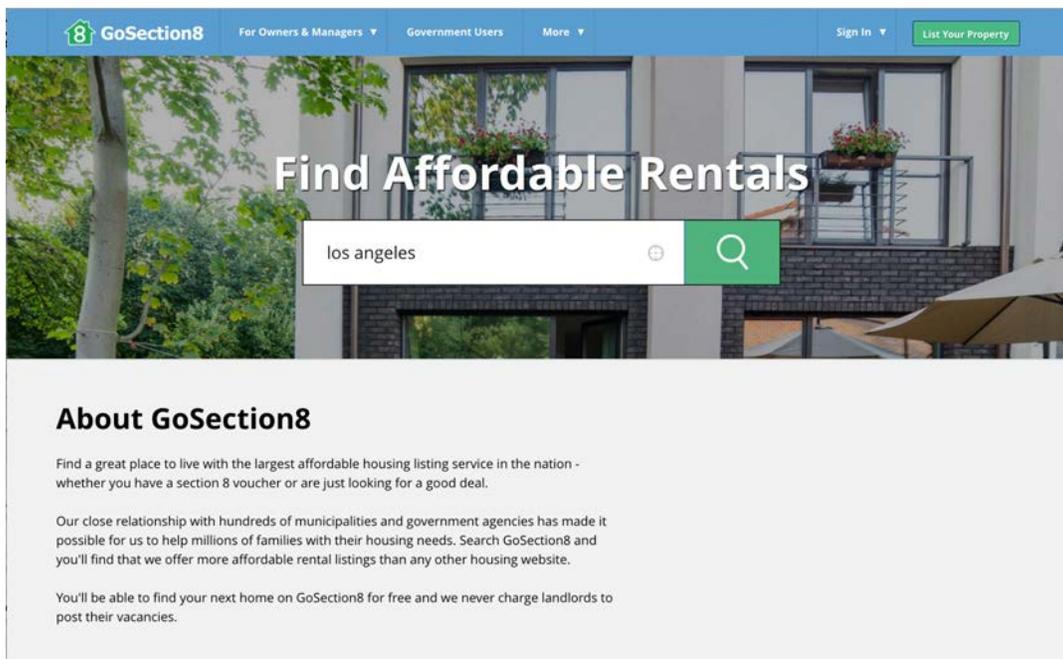
This figure shows the landing page on GoSection8.com, where users can search for rental properties by zip code, city, or county. Only geographic search terms are accepted. This is the first content a user sees when they visit GoSection8.com. Both treatment and control group users see this page.

Figure 2: Listings Results

This figure shows an example of the listings returned from a search query entered on the GoSection landing page.

Figure 3: Detailed Property Listing Page
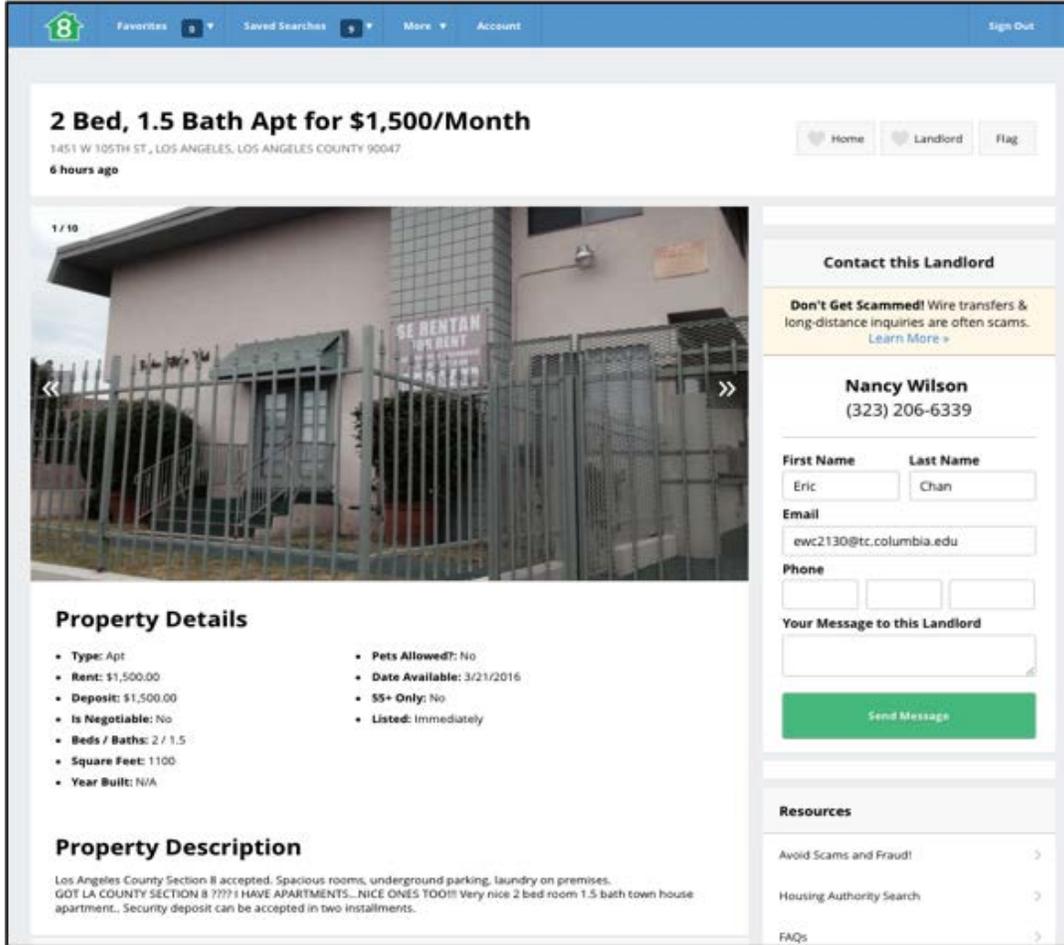
This figure shows the detailed property listing page on GoSection8.com, which is seen after a user clicks on a specific listing from a list of their search results. Because of space constraints, this figure shows only the top part of the property-listing web page. Both treatment and control group users see the information shown in this figure. The intervention module, which only treatment group users see, is on the same web page shown here, directly below the content shown in this figure (see the figure showing the GreatSchools Module.)

Figure 4: GreatSchools module
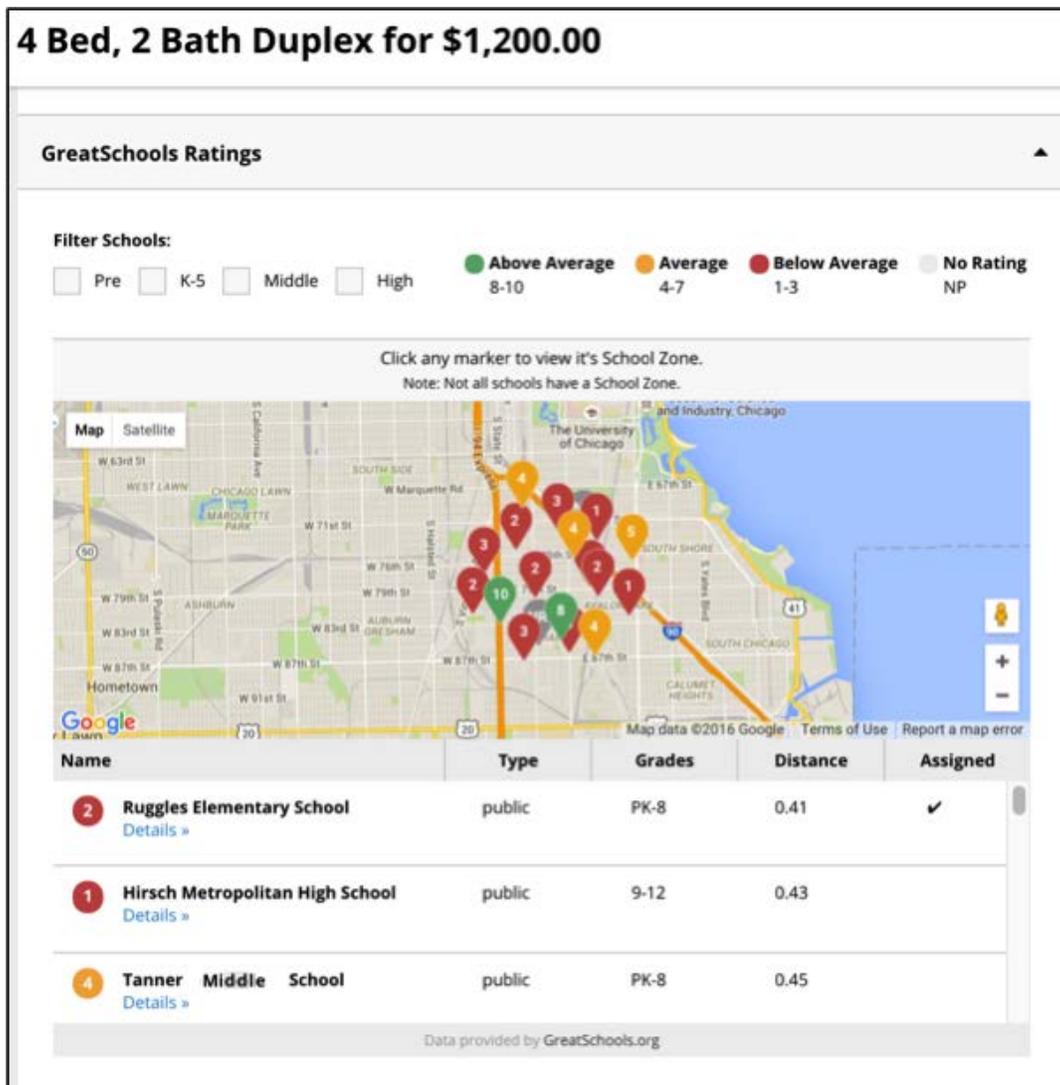
This figure shows the intervention module with hypothetical school information associated that is shown on a treated user's detailed listing page. It includes a map, the assigned schools (or closest school, if there is school choice) at each school level, and a school rating for each of these schools. The ratings range from 1 to 10, with 1-3 coded as red, 4-7 as orange, and 8-10 as green; these ratings and color codings come directly from GreatSchools. Only treatment group users see the content in this figure.

Figure 5: Distribution of GreatSchools Public School Ratings Nationally vs. GoSection8 Properties
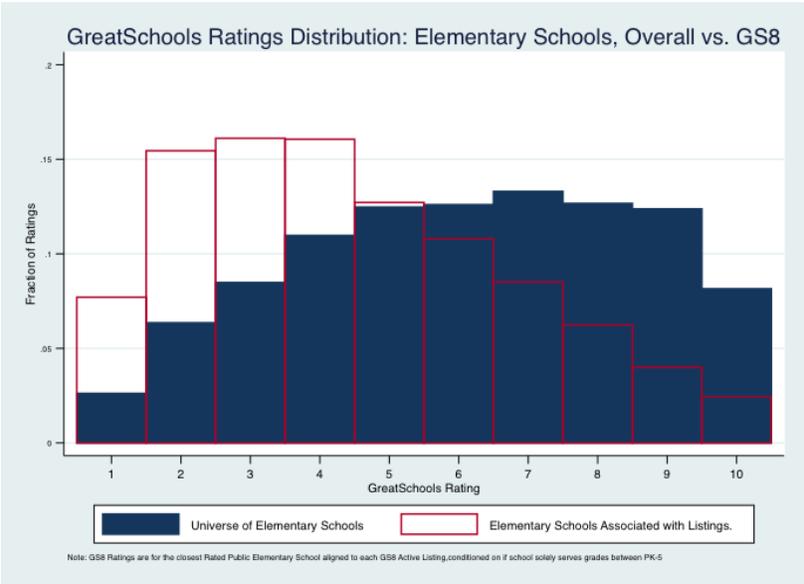
This figure shows the GreatSchools Public School Ratings for the universe of elementary schools compared to the elementary school ratings associated with every GoSection8 properties listed on its website during the study. The red, unfilled bars show the distribution of ratings for GoSection8 listings, and the blue-filled bars show the distribution of ratings for the universe of elementary schools. Data from GreatSchools and GoSection8. Results are similar for middle and high schools.

Figure 6: Number of Views and Inquiries Made Relative to Move Date

(a) Views by week
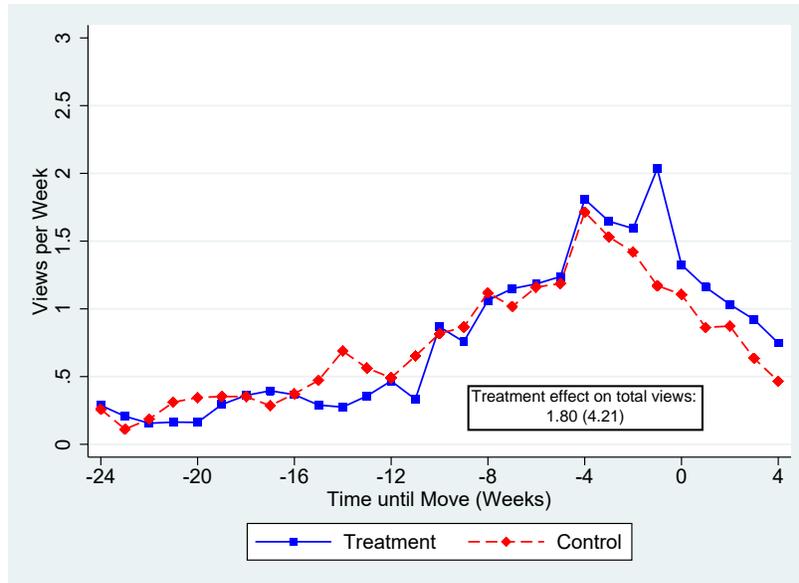
(b) Inquiries by week
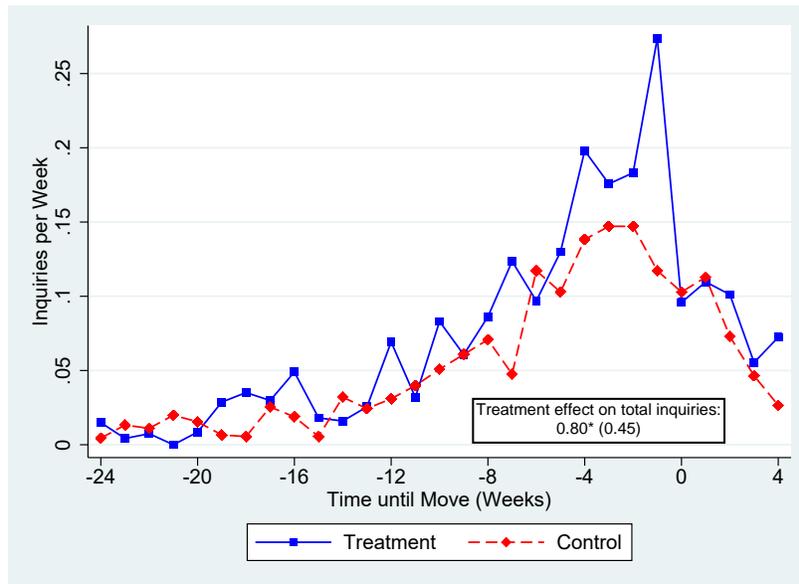
These figures compare the treatment and control group views and inquiries over time. The X-axis shows time until move, where intended move date has been re-centered to 9 for each user, so that -4 indicates four weeks prior to their intended move date, -8 indicates 8 weeks, etc. The Y-axis shows average views per week in panel (a) and inquiries per week in panel (b). The treatment group is shown in red and the control group is shown in blue. Intended move date is self-reported from the intake survey on GoSection8. Views per week come from GoSection8 as well. The "treatment effect on total views" shows the result of a regression of total views made by a user regressed on a treatment indicator. The "treatment effect on total inquiries" shows the result of a regression of total inquiries made by a user regressed on a treatment indicator. Heteroskedasticity-robust standard errors are shown in parentheses next to each point estimate.

Figure 7: Searches and Inquiries by School Quality

(a) Number of Views by Mean School Quality
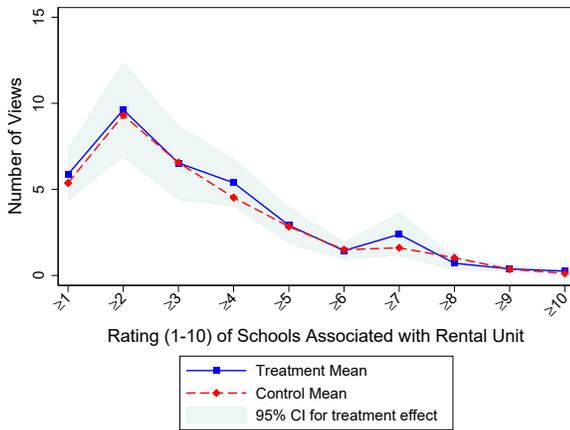
(b) Number of Inquiries by Mean School Quality
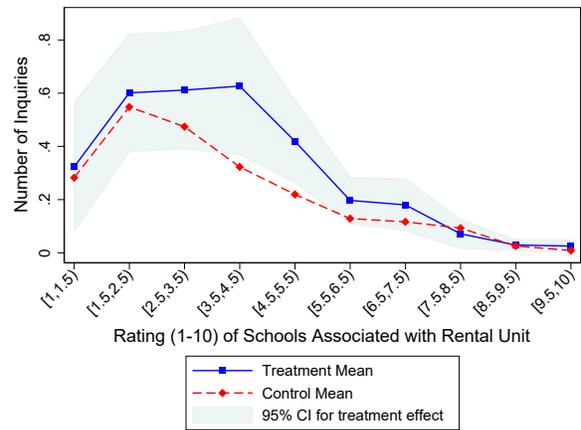
These figures show the number of views and inquiries by mean school quality. The X-axis shows the average school quality associated with a listing (averaged across the elementary, middle and high school rating associated with a particular listing) binned into the intervals shown in each panel. The Y-axis in panel (a) shows the average number of views within each bin, and panel (b) shows the average number of inquiries within each bin. The treatment group is shown in blue and the control group is shown in red. Confidence intervals for the treatment effect are shown in shaded light blue (see main text for the specification). Data on inquiries and views are from GoSection8, and the average school quality is from GreatSchools ratings linked to each listing.

Figure 8: Density of Average Ratings
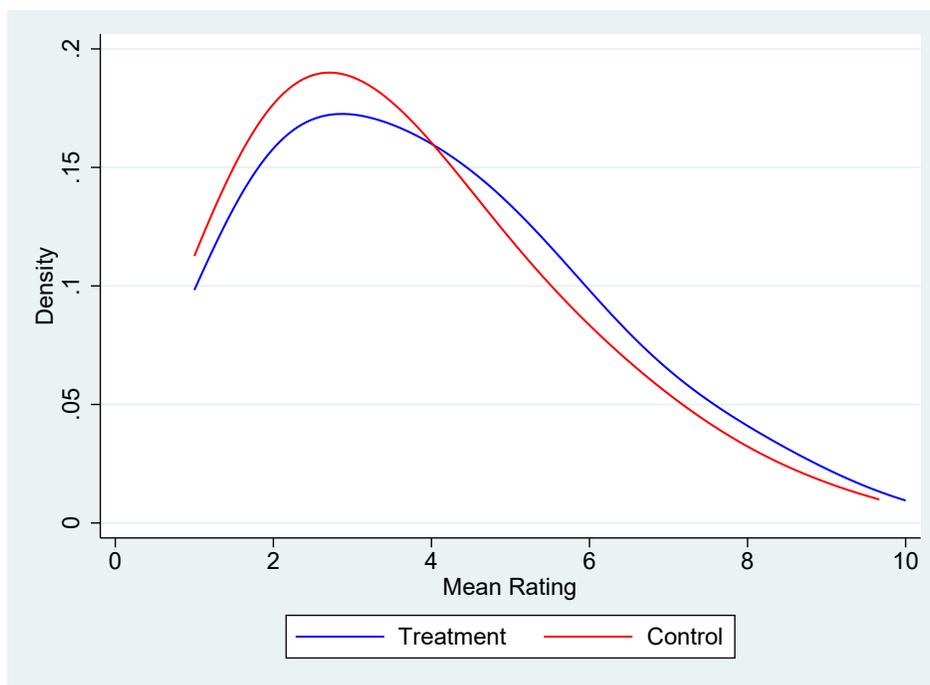
This figure shows the density of average school ratings associated with the endline-residential location of each study participant. The treatment group is shown in blue and the control group is shown in red. Average ratings are constructed from GreatSchools data on elementary, middle and high school quality ratings associated with each user's residential location. Data on locations come from the Department of Housing and Urban Development.

Figure 9: Poverty Density
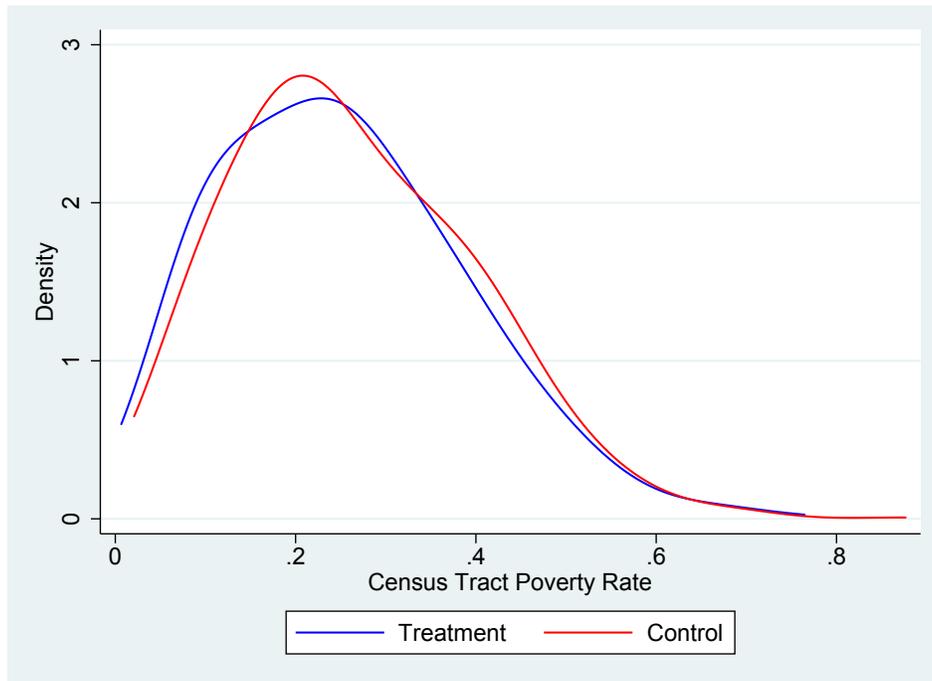
This figure shows the density of the census-tract poverty rate associated with the endline-residential location of each study participant. The treatment group is shown in blue and the control group is shown in red. Poverty rate data is from ACS data. Data on locations come from the Department of Housing and Urban Development.

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Figure 10: Kernel density of residuals $q - E(q|x)$
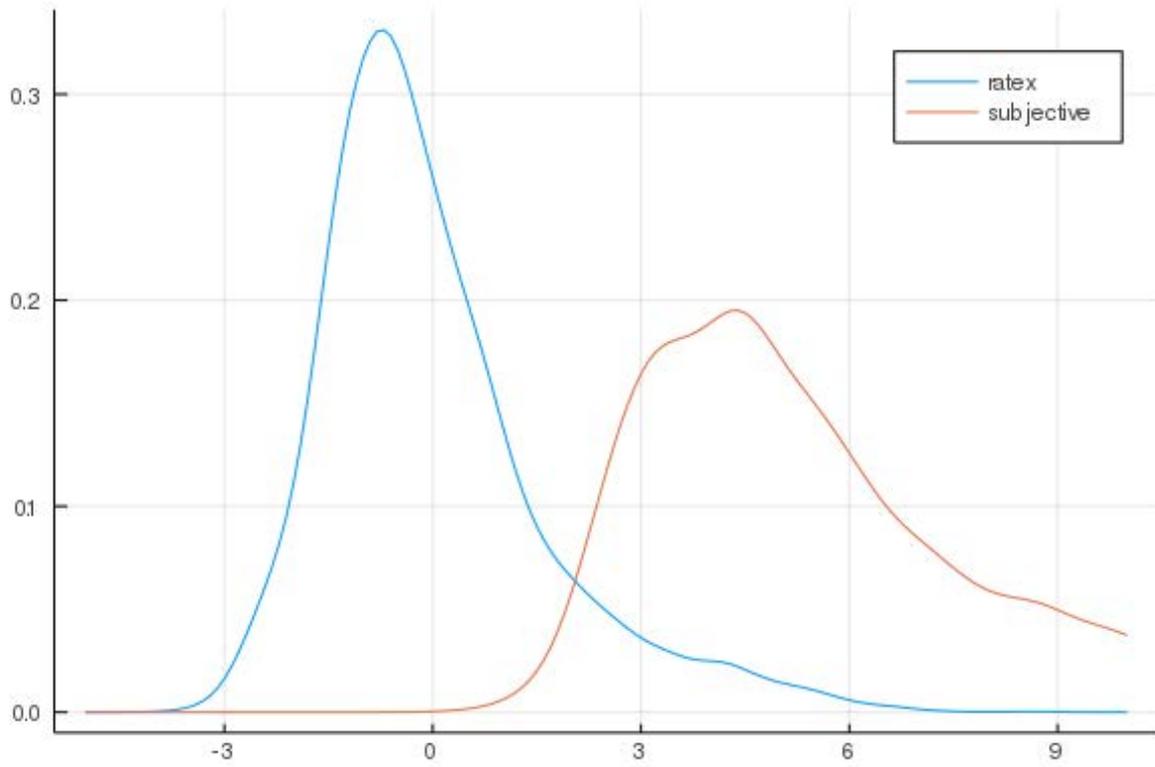
This figure displays the density of "residual quality" $q - E(q|x)$ according to OLS and subjective estimates of $E(q|x)$. This residual is the object that our information treatment provides to households when their signal η is completely uninformative.

Tables

Table 1: Experimental Sample Balance Table

Variable	Control Mean	Treatment-Control Difference	P-value	N
HH Female	0.88	−0.02	0.31	1,921
HH Hispanic	0.15	−0.03**	0.03	1,921
HH Black	0.59	0.01	0.55	1,932
HH White	0.24	0.01	0.63	1,916
Total annual income	14,512.75	103.54	0.84	1,921
Total household members	3.05	−0.07	0.39	1,921
Moving to Work (MTW)	0.18	−0.04**	0.02	1,932
HUD count of children <18 years old	1.84	−0.06	0.43	1,932
Intend to move within 3 months	0.76	0.01	0.63	1,932
Moving for schools	0.31	0.02	0.29	1,932
New voucher recipient	0.18	−0.01	0.45	1,933
HUD user matched to go8 user	0.66	−0.01	0.49	2,968

Omnibus Test P-value: 0.15

Notes: All data from HUD, with the exception of intention to move within three months and moving for schools, which are from the baseline survey on GoSection8. "HH" stands for Head of Household. Treatment and control differences are estimated from a regression of the baseline variable on a treatment indicator and P-values are calculated from heteroskedasticity-robust standard errors. The majority of the regressions here are estimated using only those who were matched from GoSection8 to the HUD system using head of household's full name, date of birth, and self-reported PHA. The exception is that "HUD user matched to go8 user" variable, which has more observations because it includes all users recruited to the study including those who were not subsequently matched to data from HUD.

Table 2: School Characteristics

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Average School Quality	3.65	0.26***	0.09	0.00	1,922
Maximum School Quality	4.82	0.31***	0.11	0.00	1,922
Minimum School Quality	2.64	0.18**	0.08	0.03	1,922
Next School Level Quality	3.65	0.31***	0.12	0.01	1,574
High School Rating	4.01	0.20*	0.11	0.06	1,821
Middle School Rating	3.52	0.36***	0.11	0.00	1,888
Primary School Rating	3.47	0.23**	0.11	0.03	1,864
Mean share FRPL	0.72	−0.02**	0.01	0.04	1,866
Fraction Black or Hispanic	0.66	−0.03**	0.01	0.02	1,866

Notes: This table shows the results of a regression of school characteristics on an indicator for treatment (see text for exact specification). The sample is conditional on being matched to HUD's database. Each family's residence is assigned to the neighborhood-assigned elementary, middle, and high school. Average school rating is the mean of the elementary, middle, and high school rating from Greatschools. Maximum rating is the maximum of these three ratings, and minimum is the minimum of these three ratings. Mean share FRPL is the share of students receiving free or reduced-priced lunch (an indicator of low-income students at the school) at these three schools, and Fraction Black or Hispanic is the average share of Black and Hispanic students at the assigned elementary, middle and high schools. The latter two variables come from the National Center for Education Statistics (NCES) Common Core of Data. Smaller N's indicate missing data; schools may not always be rated due to incomplete achievement or testing data. Heteroskedasticity-robust standard errors shown. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 3: Endline Neighborhood Characteristics

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Percent Hispanic (Census Tract)	0.23	−0.01	0.01	0.14	1,907
Percent White (Census Tract)	0.50	0.01	0.01	0.51	1,907
Percent Black (Census Tract)	0.34	−0.01	0.01	0.44	1,907
Percent B.A. Graduates (Census Tract)	0.19	0.00	0.01	0.45	1,907
Percent in Poverty (Census Tract)	0.25	−0.01**	0.01	0.03	1,919
Walkscore	50.02	−3.87***	1.03	0.00	1,929
Commute time to Downtown	15.36	1.89***	0.67	0.00	1,913
Median Gross Rent (Census Tract)	980.06	13.15	12.31	0.29	1,917
Rental Unit Gross Rent	1215.26	−18.08	21.24	0.39	1,921
Rental Unit Number of Bedrooms	2.45	−0.03	0.05	0.51	1,921
Predicted Household Income Rank	38.38	0.71***	0.24	0.00	1,917
Residual School Qlty	-0.09	0.18**	0.08	0.02	1,880
Predicted School Qlty	3.73	0.09**	0.04	0.04	1,887

Notes: This table shows the results of a regression of neighborhood characteristics on an indicator for treatment (see text for exact specification). Sample is conditional on being matched to HUD database at endline. Heteroskedasticity-robust standard errors shown in parentheses. Location data, unit rent, and bedrooms data come from HUD. Census Tract data from American Community Survey 5-year estimates (2012-2016) data. School quality data are from GreatSchools. Walkscore and commute time data is from Walkscore.com. Residual school quality is the residual from a regression of the average school quality associated with a residence regressed on the neighborhood characteristics used in the structural model: a constant, poverty, neighborhood share black, median gross rent, walk score, and commute to downtown. Predicted school quality is the complement of this result: the predicted school quality based on the latter regression. Predicted Household Income Rank comes from the Opportunity Atlas, which shows the average household income percentile rank at age 35 for children who grew up in low-income (25th percentile) families in the 1978-1983 birth cohorts (see Chetty, Friedman, Hendren, Jones, and Porter, 2018). Heteroskedasticity-robust standard errors shown.

*** p<0.01, ** p<0.05, * p<0.10

Table 4: Parameter Estimates

Variable	Estimate	SE	95% CI
$\beta(\text{constant})$	-3.1177	(0.0412)	$(-3.1826, -3.0424)$
$\beta(\text{medrent})$	-0.5317	(0.0425)	$(-0.6041, -0.4445)$
$\beta(\text{poverty})$	-0.1667	(0.0324)	$(-0.2616, -0.1121)$
$\beta(\text{black})$	0.0633	(0.0237)	$(0.0358, 0.1351)$
$\beta(\text{walkscore})$	-0.3321	(0.0396)	$(-0.468, -0.2948)$
$\beta(\text{commute})$	-0.3006	(0.022)	$(-0.3376, -0.2515)$
$\beta(\text{quality})$	0.2718	(0.0209)	$(0.2208, 0.3066)$
$\beta^{\text{control}}(\text{constant})$	-3.1819	(0.0387)	$(-3.2469, -3.1093)$
$\beta^{\text{control}}(\text{medrent})$	-0.0868	(0.0393)	$(-0.2024, -0.0515)$
$\beta^{\text{control}}(\text{poverty})$	-0.4082	(0.0297)	$(-0.467, -0.3742)$
$\beta^{\text{control}}(\text{black})$	0.1184	(0.0407)	$(0.0659, 0.2114)$
$\beta^{\text{control}}(\text{walkscore})$	-0.3568	(0.0265)	$(-0.4201, -0.3049)$
$\beta^{\text{control}}(\text{commute})$	-0.332	(0.0401)	$(-0.4016, -0.2809)$
$\beta^{\text{control}}(\text{quality})$	0.1338	(0.0228)	$(0.0923, 0.1807)$
μ_{search}	-2.1665	(0.0411)	$(-2.2583, -2.0859)$
$\sigma_c(\text{search})$	0.0217	(0.0005)	$(0.0206, 0.0225)$
μ_{inquire}	-4.3721	(0.0767)	$(-4.4107, -4.1079)$
$\sigma_c(\text{inquire})$	1.0119	(0.0155)	$(0.9642, 1.0307)$
$\sigma_\epsilon(\text{treat})$	0.4689	(0.0122)	$(0.4531, 0.4974)$
$\sigma_\epsilon(\text{control})$	0.7254	(0.0223)	$(0.7031, 0.8089)$
$Pr(\text{inquiry observed})$	0.6452	(0.0059)	$(0.6223, 0.6506)$
λ	1.0337	(0.0397)	$(1.011, 1.1635)$
pr(passive)	0.5954	(0.0173)	$(0.5549, 0.6324)$

Notes: This table shows the estimated parameters from our model. β (no superscript) denotes preference parameters of treated households. β^{control} denotes preferences parameters of control households. Standard errors and confidence intervals via bootstrap.

Table 5: Subjective Beliefs vs Rational Expectations

	Next School Quality	
	(RatEx)	(Subjective)
γ(constant)	3.5381	0.4655
	(0.0657)	(0.4319)
γ(medrent)	1.5222	-3.224
	(0.0416)	(0.499)
γ(poverty)	-2.1992	1.7499
	(0.0943)	(0.4568)
γ(black)	-1.8509	-0.399
	(0.0322)	(0.2731)
γ(walkscore)	-0.6166	0.1791
	(0.0436)	(0.3511)
γ(commute)	-0.3428	0.2277
	(0.0501)	(0.3052)
resid. variance	2.6605	1.3262
$\sigma_\eta^2/(\sigma^2\eta + \sigma_q^2)$		0.2424

ratex: N=15355 obs. Standard errors in parentheses.

$*p < 0.05$, $**p < 0.01$, $***p < 0.001$

Wald test $\gamma_{ols} == \gamma$: $\chi^2 = 472.0638$, $p = 0.0$

Notes: This table shows estimates of information parameters γ, σ_q^2, and s. First column ("RatEx"): coefficients from an OLS regression of quality on characteristics in the sample of all properties viewed at least once by some household. Second column ("subjective"): model estimates. These parameters str derived from the parameters shown in table 4. Resid. variance: residual variance in OLS regression, and σ_q^2 in model. Bootstrap standard errors shown in parentheses.

Table 6: Counterfactual: fix $\gamma = \gamma_{OLS}$

	(Data)	(Model)	(Counterfactual)
inquiries	0.1019	0.0663	−0.11
	(0.1111)	(0.0392)	(0.0695)
lease up at new location	−0.002	0.0076	−0.0247
	(0.0179)	(0.0089)	(0.0096)
endline quality | lease up at new loc.	0.2984	0.295	0.3285
	(0.0912)	(0.058)	(0.0488)

Notes: This table shows the impacts of a counterfactual in which control-group households are endowed with prior-mean parameters γ_{OLS} rather than the estimated subjective values γ. First column: difference in means between treatment and control groups in our dataset. Second column: model-estimated difference between treatment and control groups. Third column: difference between treatment and control groups under counterfactual informational environment. "Lease up at new location" is equal to one if the household is using a voucher at endline and either was not using a voucher at baseline, or was using a voucher at baseline in a different location than at endline. Bootstrap standard errors shown in parentheses.

Appendices

9   Additional Figures

Figure A.1: Consent pop up
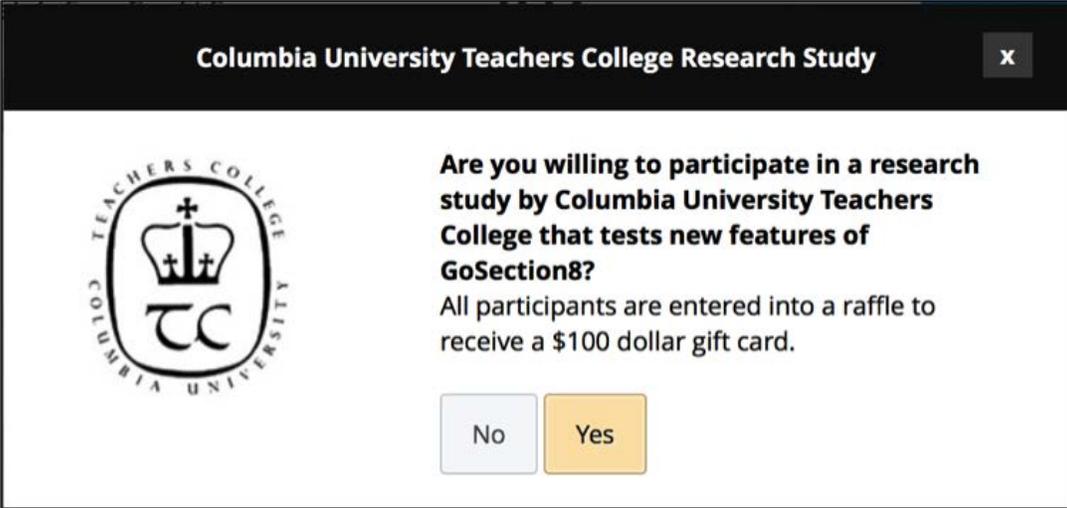
This figure shows the initial recruitment opt-in box, which pops up immediately after users enter their first search query into the search box shown on the landing page. Agreeing to this pop up leads to intake survey plus site registration and consent form. Those who opted out were directed to a site registration, but experienced the site without the intervention module.

Figure A.2: Intake survey

This figure shows the intake survey and consent box, which appears after users click "yes" to the consent pop up on GoSection8.com. We ask for the name of head of household and of the user to help merge to data from the Department of Housing and Urban Development. Voucher status asks the user whether they currently have a voucher or not. The name of the housing agency is a drop down menu to reduce typographical errors. Reason for moving is a drop down menu as well (see text for the possible answers). Intended move date opens up a small calendar from which families can pick a date.

Figure A.3: Elementary School Quality Variation associated with GoSection8 Listings.
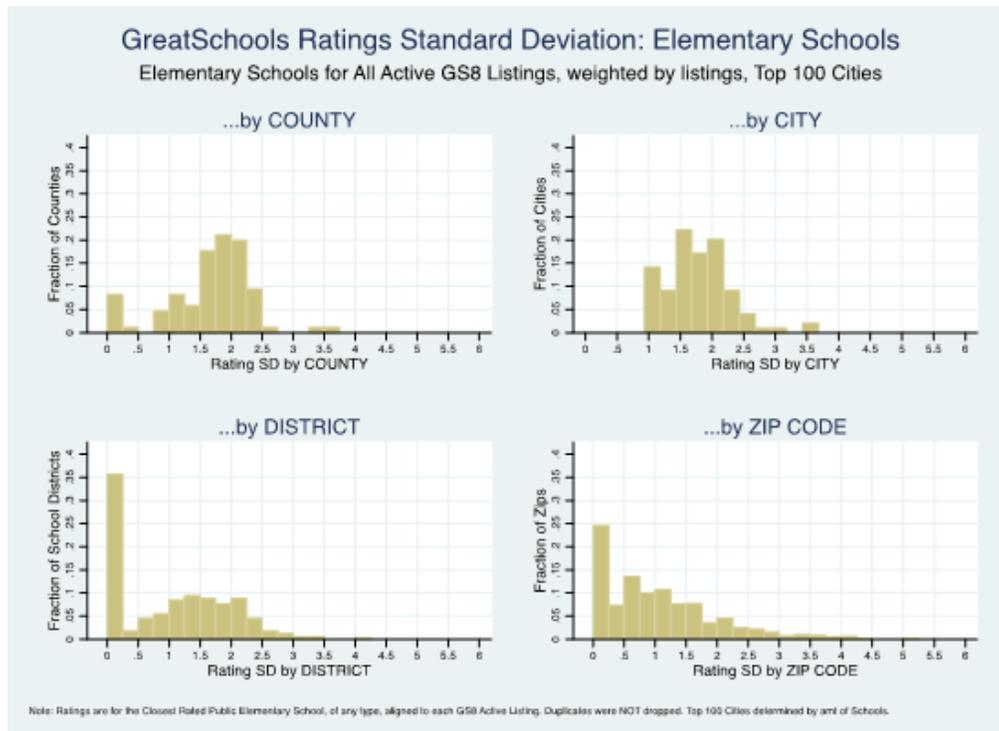

This figure shows the variation in school quality for GoSection8 listings, within the county, city, district, and zip code levels. In general, users can search listings at the county, city, and zip code levels on the web site. This figure includes duplicated schools; for example, if a school is associated with multiple listings, it is included multiple times. Data on school ratings come from GreatSchools. Property listings and locations come from GoSection8.

Figure A.4: Basis functions for time-varying moments
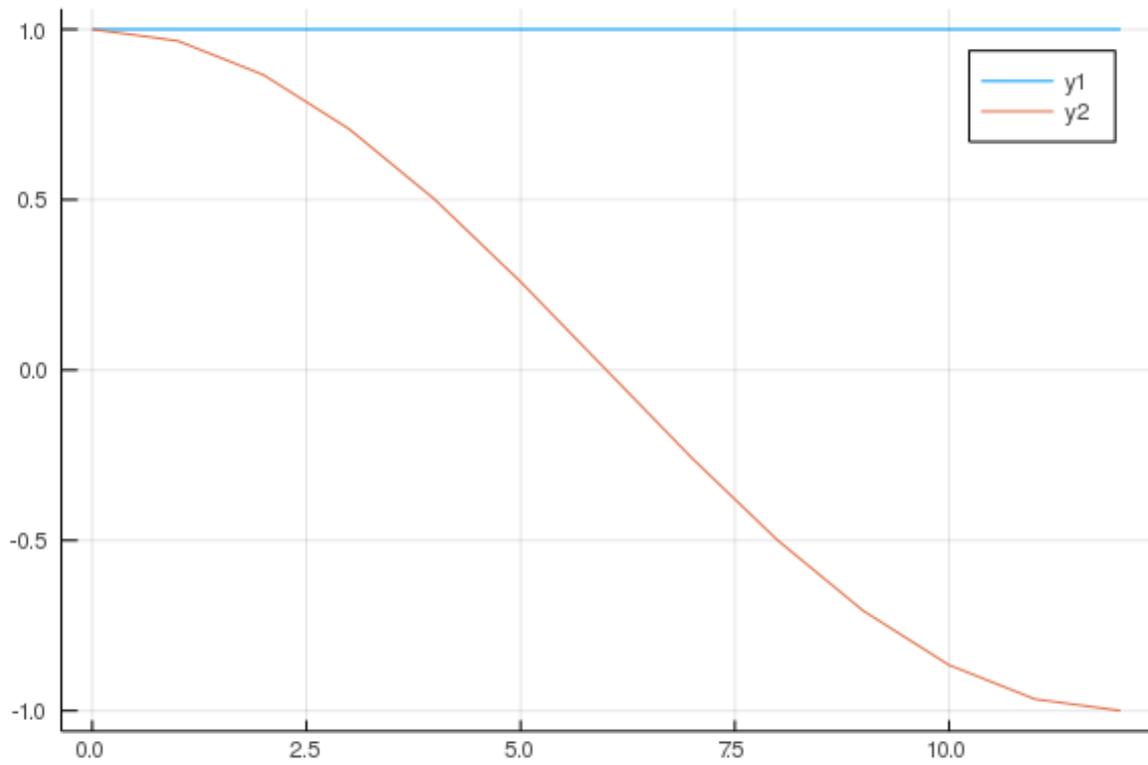
This figure shows the basis functions used to aggregate the time-varying moments, 1(used the platform in week t) and number of inquiries in week t.

Figure A.5: Model fit: all moments
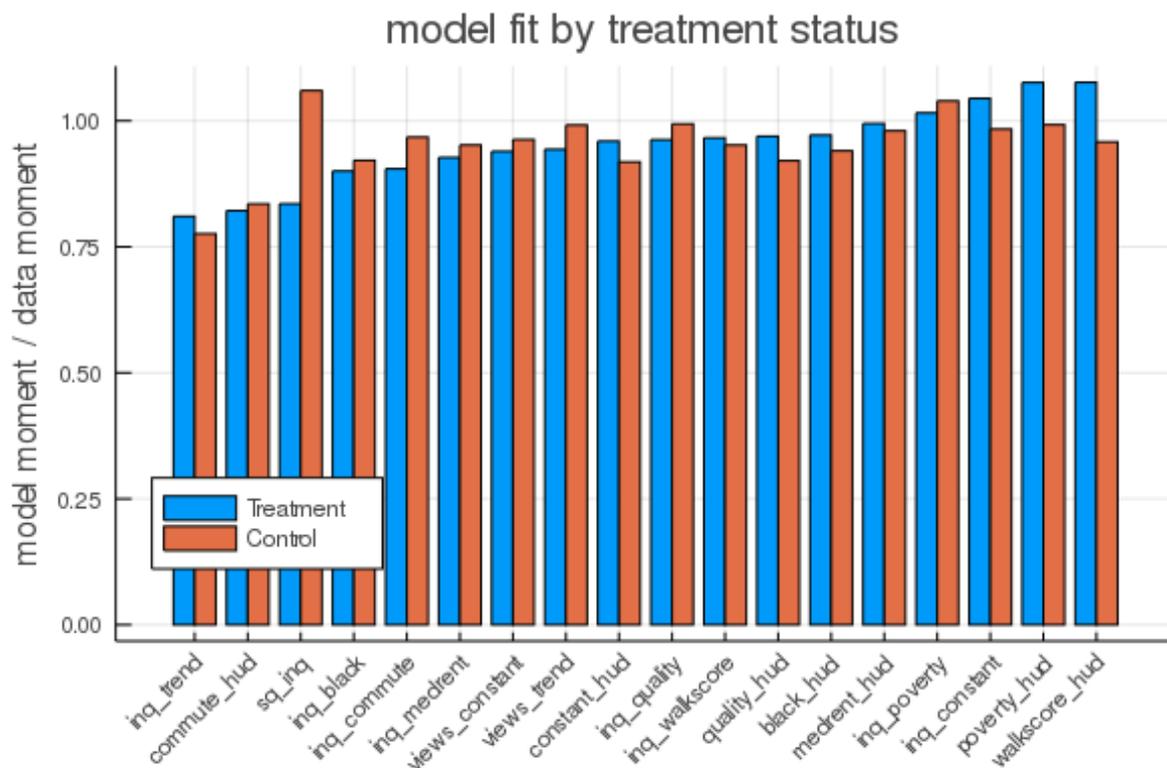
This figure compares the average over households in our dataset of each of the targeted moments to the value in our estimated model. Moments are sorted by ratio of model-predicted value to mean value over households in the data. Key: inq_x denotes the sum of $x \in \{$constant, median rent, poverty, share black, commute time, walkscore$\}$ over all inquiries made by a household. x_hud denotes the value of $x_j * 1(moved)$ where j is the unit that the household lives in at endline. sq_inq denotes the second moment of share of viewed units which receive direct inquiries within each household-week.

Figure A.6: Model fit: 1(use platform in week t)
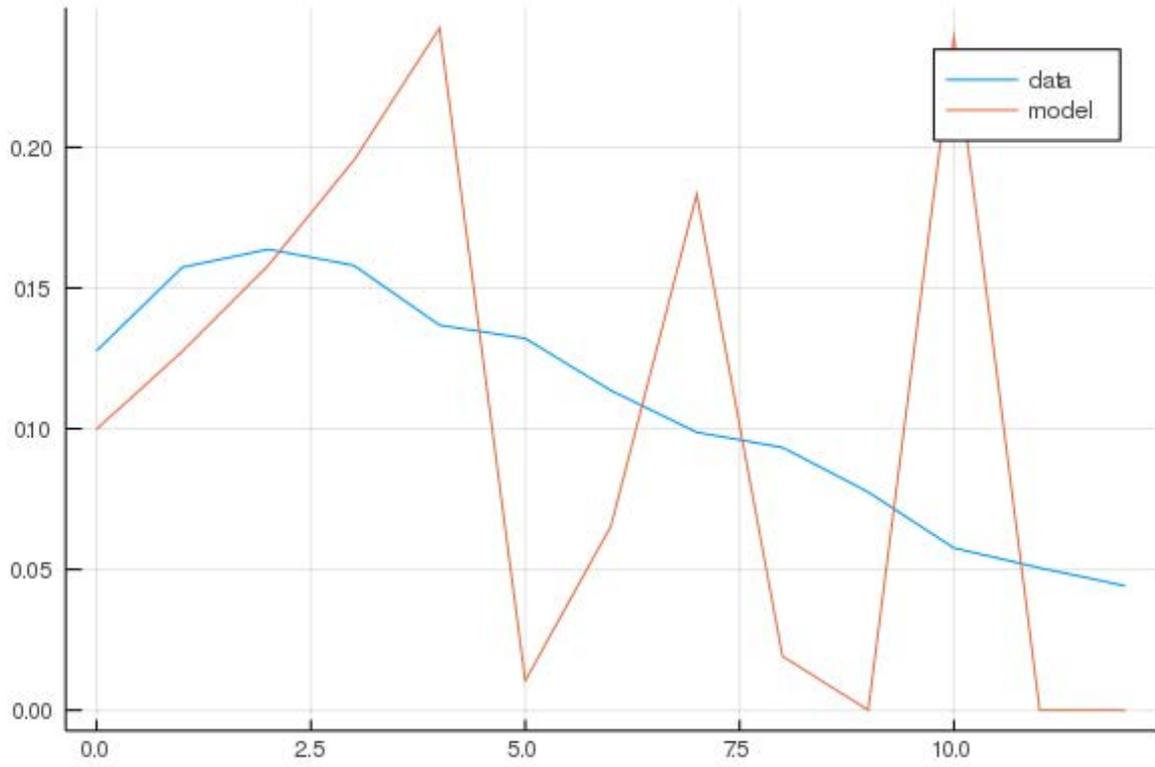
This figure compares data means and model estimates of the probability of using the platform in each week as a function of time until intended move date. The X-axis denotes weeks remaining until the household's reported intended move date.

Figure A.7: Model fit: number of inquiries in week t
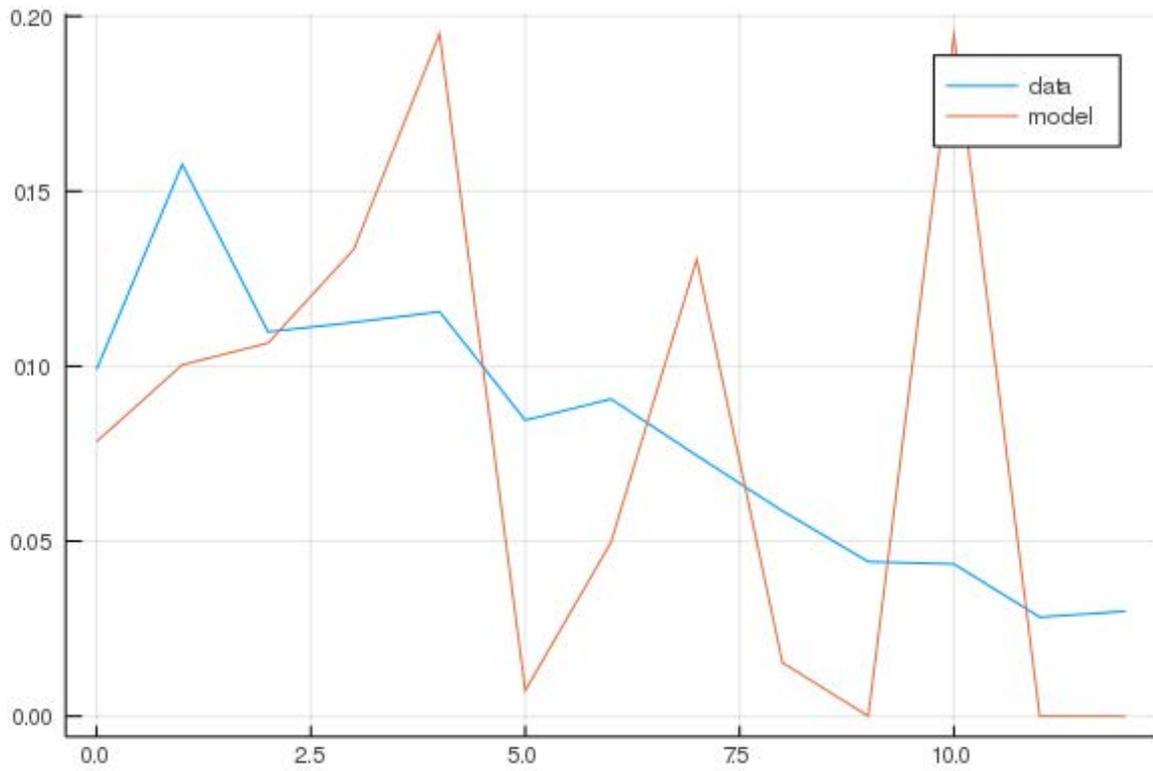
This figure compares data means and model estimates of the mean number of direct inquiries made by each household in each week as a function of time until intended move date. The X-axis denotes weeks remaining until the household's reported intended move date.

10 Additional Tables

Table A.1: HUD (5% Random Sample) and Study Sample Comparison at Baseline

Variable	GS8 (matched) Mean	Matched-HUD Difference	P-value	N (HUD)	N (matched)
HH female	0.87	0.03***	0.00	347,548	1,954
HH disabled	0.31	−0.16***	0.00	347,548	1,954
HH white	0.25	0.02**	0.02	347,543	1,949
HH Black	0.60	0.02**	0.05	347,545	1,951
HH Hispanic	0.14	−0.04***	0.00	347,548	1,954
Total household members	3.01	0.59***	0.00	347,548	1,954
Number of bedrooms	2.43	0.38***	0.00	347,544	1,954
Total annual income	14,634.54	23.20	0.93	347,548	1,954
Rent to owner	1071.22	45.46***	0.00	347,548	1,954
Gross rent	1213.23	76.52***	0.00	347,548	1,954
Utility allowance	138.63	28.52***	0.00	347,548	1,954

Notes: All household characteristic information are from the Departmment of Housing and Urban Development (HUD), at baseline. The abbreviation "HH" stands for "head of household," and GS* stands for GoSection8. Data is from a 5% random sample of heads of households, restricted to those who ever completed a HUD Form 50058 with HUD between 2013-2017. For GoSection8 observations, it is restricted to those who matched with the HUD database of voucher holders. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table A.2: Study Sample Matched to HUD Data v. Overall Study Sample

Variable	Unmatched Sample Mean	Difference from Matched Sample	P-value	N
Child age 0-4	0.53	−0.11***	0.00	2,968
Child age 5-10	0.32	0.03	0.13	2,968
Child age 11-13	0.20	0.04***	0.01	2,968
Child age 14-18	0.24	0.08***	0.00	2,968
Has Child	0.95	−0.02*	0.07	2,968
Intend to move within 3 months	0.85	−0.09***	0.00	2,968
Reason: Better transportation	0.17	0.01	0.45	2,968
Reason: Better schools	0.35	−0.02	0.25	2,968
Reason: Evicted	0.16	−0.03**	0.05	2,968

This table shows characteristics of the sample recruited on GoSection8 but not matched to data from the Department of Housing and Urban Development (HUD) and the difference in these variables from those were matched. All data are from the intake survey on the GoSection8 website. Differences are estimated from a regression of the characteristic on an indicator for being matched to the data from HUD. Unmatched sample mean is the mean for those recruited on the website but not matched to HUD data. The "Reason" variables are users' stated reasons for moving.

Table A.3: Descriptive Comparison of Baseline School Characteristics

	US Elem Schools	GS8 Listings	HUD (5%)	Matched
GreatSchools Rating	5.78	5.28	4.71	3.27
Share Black	0.15	0.25	0.20	0.44
Share Hispanic	0.26	0.33	0.48	0.38
Share White	0.49	0.33	0.22	0.12
Share Asian	0.05	0.04	0.06	0.03
Share FRPL	0.52	0.63	0.71	0.84
Pupil-FTE Ratio	17.67	18.22	18.58	18.35
Observations (schools)	125,346	17,583	328,301	1,853

This table shows the means of the GreatSchools school rating and demographic data for the universe of US elementary schools, elementary schools associated with GoSection8 property listings, elementary schools associated with a 5% random sample of HUD properties, and elementary schools associated with participants' addresses that we were able to match to HUD data at the time of baseline, respectively. Note that for the latter three columns, schools may be counted multiple times as they are matched to specific addresses. These columns can be interpreted as weighted means of ratings and characteristics. Also, a small share of schools do not have sufficient data to be rated, and thus are treated as missing. *** p<0.01, ** p<0.05, * p<0.10

Table A.4: Property Views

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Total vews made post treatment	33.57	1.80	4.21	0.67	1,932
Any views	0.77	−0.00	0.02	0.82	1,932
Number of Different Tracts Made Views In	14.36	0.11	1.26	0.93	1,932
Number of Views Rated 1 or Above	33.22	1.55	4.09	0.70	1,932
Number of Views Rated 2 or Above	24.73	2.72	3.29	0.41	1,932
Number of Views Rated 3 or Above	15.94	2.69	2.24	0.23	1,932
Number of Views Rated 4 or Above	10.27	1.88	1.56	0.23	1,932
Number of Views Rated 5 or Above	6.17	1.25	1.10	0.25	1,932
Number of Views Rated 6 or Above	3.90	0.97	0.82	0.24	1,932
Number of Views Rated 7 or Above	2.54	0.70	0.69	0.31	1,932
Number of Views Rated 8 or Above	1.03	0.03	0.21	0.88	1,932
Number of Views Rated 9 or Above	0.31	0.12	0.09	0.19	1,932
Number of Views Rated 10	0.07	0.12**	0.05	0.03	1,932

Notes: This table shows the results of a regression of the number of GoSection8 property views on an indicator for treatment. The sample is restricted to those who were matched to a database of HUD voucher holders. Heteroskedasticity-robust standard errors shown. *** p<0.01, ** p<0.05, * p<0.10

Table A.5: Property Inquiries

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Total inquiries made post treatment	2.32	0.80*	0.45	0.07	1,932
Any Inquiries	0.34	0.03	0.02	0.20	1,932
Number of Different Tracts Made Inquiries In	1.58	0.28	0.22	0.21	1,932
Number of Inquiries Rated 1 or Above	2.30	0.81*	0.45	0.07	1,932
Number of Inquiries Rated 2 or Above	1.84	0.76**	0.36	0.04	1,932
Number of Inquiries Rated 3 or Above	1.27	0.67**	0.30	0.02	1,932
Number of Inquiries Rated 4 or Above	0.82	0.46**	0.20	0.03	1,932
Number of Inquiries Rated 5 or Above	0.52	0.19	0.14	0.18	1,932
Number of Inquiries Rated 6 or Above	0.32	0.09	0.10	0.34	1,932
Number of Inquiries Rated 7 or Above	0.20	0.05	0.07	0.53	1,932
Number of Inquiries Rated 8 or Above	0.10	−0.01	0.03	0.66	1,932
Number of Inquiries Rated 9 or Above	0.03	0.01	0.02	0.65	1,932
Number of Inquiries Rated 10	0.00	0.02*	0.01	0.10	1,932

Notes: This table shows the results of a regression of the inquiries of GoSection8 property views on an indicator for treatment. The sample is restricted to those who were matched to a database of HUD voucher holders. Heteroskedasticity-robust standard errors shown. *** p<0.01, ** p<0.05, * p<0.10

Table A.6: School Characteristics by Subgroup: Families with child(ren) in single age range

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Child(ren) Ages 0-4 Only					
Primary School Rating	3.30	0.39*	0.22	0.07	449
Middle School Rating	3.49	0.24	0.22	0.29	455
High School Rating	4.02	0.20	0.24	0.41	441
Child(ren) Ages 5-10 Only					
Primary School Rating	3.62	0.25	0.39	0.52	169
Middle School Rating	3.24	0.89**	0.37	0.02	168
High School Rating	3.99	0.25	0.39	0.51	168
Child(ren) Ages 11-13 Only					
Primary School Rating	3.42	0.27	0.55	0.63	80
Middle School Rating	3.58	0.12	0.47	0.80	86
High School Rating	3.33	1.20**	0.50	0.02	84
Child(ren) Ages 14-18 Only					
Primary School Rating	3.87	−0.16	0.32	0.62	230
Middle School Rating	3.77	0.04	0.31	0.90	240
High School Rating	4.55	−0.01	0.32	0.97	230

Notes: This table shows the results of a regression of GreatSchools ratings on an indicator for treatment (see text for exact specification). Each family's endline residence is assigned to the neighborhood-assigned elementary, middle and high school. The results here are broken out for families who have a child or children at the age ranges shown and show effects on the rating for each primary, middle and high school. The age ranges are self-reported on the baseline survey on GoSection8. The age ranges were selected since they are roughly the most popular ages to be in primary school (5-10), middle school (11-13), and high school (14-18). Heteroskedasticity-robust standard errors shown.

*** p<0.01, ** p<0.05, * p<0.10

Table A.7: School Characteristics by Subgroup: Average Next-Level Rating

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Avg. next level rating (one age range)	3.60	0.46***	0.16	0.00	931
Avg. next level rating (>1 age range)	3.53	0.31*	0.17	0.07	581

Notes: This table shows the results of a regression of mean GreatSchools ratings on an indicator for treatment (see text for exact specification). Each family's residence is assigned to the neighborhood-assigned elementary, middle and high school. The "average next level rating" variable is the average rating of the school level a family's child would attend next, and are broken out by families with child(ren) in a single age range and families with child(ren) in multiple age ranges. For example, if the family has children in the 0-4 age range, the next-level rating would be the rating of the primary school associated with their endline residential location. If a family have both a child in the 0-4 age range and the 11-13 age range, the primary school and high school ratings would be averaged to calculate the dependent variable. The possible age ranges are 0-4, 5-10, 11-13, and 14-18. Heteroskedasticity-robust standard errors shown.
*** p<0.01, ** p<0.05, * p<0.10

Table A.8: Heterogeneity by Moving for Schools

Variable	Avg. Schl. Quality	Schl. % FRPL	Schl. % Black or Hispanic	Nbhd. Poverty
Treatment	0.32***	-0.02*	-0.02	-0.01
	(0.11)	(0.01)	(0.02)	(0.01)
Treatment*Reason School	-0.10	0.00	0.00	-0.01
	(0.20)	(0.02)	(0.03)	(0.01)
Observations	1,935	1,876	1,876	1,919

Notes: This table shows the results of a regression of school characteristics and neighborhood poverty rates on an indicator for treatment (see text for exact specification) and its interaction with an indicator for families reporting the reason for moving as better schools. The sample is conditional on being matched to HUD's database of voucher holders. Each family's residence is assigned to the neighborhood-assigned elementary, middle and high school. Average school rating is the average the elementary, middle and high school rating from Greatschools. Mean share FRPL is the share of students receiving free or reduced-priced lunch (an indicator of low-income students at the school) at these three schools, and Fraction Black or Hispanic is the average share of Black and Hispanic students at the assigned elementary, middle and high schools. The latter two variables come from the National Center for Education Statistics (NCES) Common Core of Data. Poverty rate comes from the ACS. Heteroskedasticity-robust standard errors shown.
*** p<0.01, ** p<0.05, * p<0.10

Table A.9: Audit Study of GoSection8 Landlord Responses

			Replied		
Variable	(1)	(2)	(3)	(4)	(5)
Voucher	0.012			0.011	
	(0.021)			(0.021)	
Average School Rating		-0.007		-0.008	-0.010
		(0.007)		(0.007)	(0.010)
Neighborhood Rent ($100s)			0.001	0.002	-0.010
			(0.005)	(0.005)	(0.008)
Sample	All	All	All	All	Has a Voucher
Observations	1,417	1,417	1,417	1,417	684

Notes: This table shows the results of a landlord audit study we conducted of landlords on the GoSection8 platform. The dependent variable is whether or not a landlord replied. "Voucher" is randomly assigned to each inquiry to a (real) GoSection8 landlord sent by a fictitious tenant, and means that the message explicitly states the household has a voucher. Column (5) restricts the sample only to those messages explicitly stating the household has a voucher. Average school rating is the average school rating of the elementary, middle and high school assigned to the advertised unit. Neighborhood rent is the tract-level rent associated with the unit from the ACS data, and is measured in hundreds of dollars (i.e. the coefficients on rent are 100x larger than if rent were measured in dollars). Heteroskedasticity-robust standard errors shown in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.10$

11   Computational details

This section describes our model solution and estimation procedure in detail.

To approximate the model moments, we draw a grid of 4000 sets of properties per time period $t = 1, \ldots, 13$ per decision in {use platform, don't}. (We have found that using 2000 draws per period rather than 4000 does not affect our estimates). For each grid point (m, t), we draw a set of on-platform units, and a set of off-platform units whose number is drawn $Poisson(\lambda_0)$. For each decision $d \in$ {search, don't}, this gives us a matrix $x_{m,t,d}$ and vector $q_{m,t,d}$. In addition, we draw a vector of iid shocks $e_{0,m,t,d} \sim N(0, 1)$.

We use these grids to construct a differentiable approximation to the value function. To describe how we do this, we begin with the value of a set of inquiries, then work backwards.

Drop i subscripts for convenience, and fix $V_{T+1} = 0$. Let V_t denote the value at start of period t. The Gumbel distribution of ϵ^1 implies that, conditional on set of inquiries, a household accepts unit j in period t with probability

$$P_{ijt} = \frac{\exp(v_{ij} - V_{t+1})}{1 + \sum_{\text{inquired at } j} \exp(v_{ij} - V_{t+1})}.$$

Moreover, it implies that the value of a set of inquiries is given by:

$$V_{t+1} + \log \left(1 + \sum_{\text{inquired at } j} \exp(v_{ij} - V_{t+1}) \right).$$

To compute the value of discovering a set of properties with characteristics (x, q, e) given next-period value V_{t+1} and parameters θ which include the type-specific values of β_x, β_q, and σ_ϵ, we complete the following procedure:

1. Compute utilities
$$\{v_{ij}(\theta)\}_{j=1,\ldots,n_{m,t}} = x\beta_x + q\beta_q + \sigma_\epsilon e.$$

2. Sort them.

3. For each $k \leq n_{m,t}$ and decision d about platform use, compute the value of inquiring at the top k units:
$$U^k_{m,t,d}(\theta) = V_{t+1}(\theta) + \log \left(1 + \sum_{j=1,\ldots,k} \exp(v_{ij}(\theta) - V_{t+1}(\theta)) \right).$$

4. Find cutoff values of $c^{k,k+1}_{inquire}$ at which $U^k_{m,t}(\theta) = U^{k+1}_{m,t}(\theta)$.

5. Let $c^{n_{m,t}, n_{m,t}+1} = -\infty$.

6. The value of the choice set is given by:

$$U_{m,t,d}(\theta) = \sum_{k=1}^{n_{m,t,d}} \left(F_{inquire}(c^{k-1,k}(\theta)) - F_{inquire}(c^{k,k+1}(\theta)) \right) E \left(U^k_{m,t,d}(\theta) - c^{inquiry} | c^{inquiry} \in (c^{k,k+1}(\theta), c^{k-1,k}(\theta)) \right)$$

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In following these steps, we also compute the probability of inquiring at each set of k top units. We use these, together with the probabilities of accepting a unit, to compute differentiable expressions for the moment contributions of cell (m, t, d) conditional on accepting a unit, the moment contribution of this cell conditional on not accepting a unit, the probability of accepting a unit given draw (m, t, d), and the value $U_{m,t,d}$ of each draw of viewed properties (m, t) for $d \in 0, 1$, where 1 denotes platform use.

To compute the value in period t given platform-use decision d, we take the average of $U_{m,t,d}$ accounting for importance sampling in the number of off-platform listings discovered:

$$V_t^d(\theta) = \sum_m w_{m,t}(\theta) U_{m,t}^{(d)}(\theta), \quad w_{m,t} \propto F(n_{m,t}^{\text{off}}|\lambda)/F_0(n_{m,t}^{\text{off}}).$$

We use the same weights to aggregate the moment contributions from the various cells.

The value function at the start of period t is then given by:

$$V_t = V_t^0 + E(V_t^1 - V_t^0 - c_{search}|c_{search} < V_t^1 - V_t^0) F_{search}(c_{search}|\mu_{search}, \sigma_{search}^2).$$

When costs are lognormal, this expression has a (smooth) closed form. We induct backwards until we reach the initial period.

We start with an initial parameter vector θ_0, including an initial value of λ, denoted λ_0. We then draw the number of off-platform listings in each grid point according to

$$F_0(n_{m,t}^{\text{off}}) = Pr(Poisson(\lambda_0) = n_{m,t}^{\text{off}}).$$

We start with a diagonal weight matrix W_0, with diagonal elements proportional to the inverse variance of the corresponding moment in the data. (This choice effectively rescales all moments to have variance 1 in the data.)

We then obtain initial estimates θ_1, including an estimated arrival rate λ_1, which we use to redraw the grid. We estimate again, taking θ_1 as a starting value and again using weight matrix W_0, obtaining a new estimate θ_2 including λ_2.

We then redraw the grid using λ_2, and estimate the optimal weight matrix using the consistent estimate θ_2. Call this estimated weight matrix W_1. We compute the GMM estimator with weight matrix W_1, obtaining an estimate θ_3, including an arrival rate λ_3. We then recompute the optimal weight matrix (call this matrix W_2), redraw the grid using λ_3, and obtain an estimate, θ_4, which we report. (We find little difference between θ_3 and θ_4.)

To compute the GMM estimator in each step, we use the BOBYQA algorithm provided by the NLOpt software package. This is a gradient-free local optimization algorithm which uses past function evaluations to compute a local quadratic approximation to the objective function.