Housing Search Frictions: Evidence from Detailed Search Data and a Field Experiment

Peter Bergman Eric Chan Adam Kapor*

September 4, 2019

PRELIMINARY RESULTS

Abstract

This paper shows that imperfect information about school quality causes low-income families to live in neighborhoods with lower-performing, more segregated schools. We randomized the addition of school quality information onto a nationwide provider of online housing listings for families with housing vouchers. We find that this information causes families to move to areas with higher-performing, more racially disparate schools. To understand the value of this information and its implications for models of neighborhood choice, we develop a dynamic model of households' search and location choices that incorporates subjective beliefs and imperfect information about school quality. We use the model to estimate how much families value school quality and how much they would appear to value it if we ignored information frictions. If we had ignored information frictions, control group families would appear to value school quality half as much, relative to their commute to downtown, as treatment group families. Moreover, we show that control-group households have biased beliefs that underestimate school quality and mispredict it as a function of other neighborhood characteristics.

*Bergman: Teachers College, Columbia University, bergman@tc.columbia.edu; Chan: Babson College, echan1@babson.edu; Kapor: Princeton University and NBER, akapor@princeton.edu. The authors would like to thank Dan Ackerberg, Magdalena Bennett, Eric Chyn, Tom Vogl, and seminar and conference participants at HUD, UT Austin, ASU, UVA, NWU, Princeton, Harvard (Opportunity Insights), Brookings, and AEFP for useful feedback. We are especially grateful to Greg Sullivan and Mirav Rathod from GoSection8, Lynn Rodgers and Veronica Helms from HUD, and Sam Olivieri and Wousie Hough from GreatSchools for data provision and assistance. This project could not be possible without funding from the Walton Family Foundation, the Laura and John Arnold Foundation and J-PAL. The experimental portion of this study was pre-registered on OSF, which can be accessed here. All errors are our own.

1 Introduction

School quality varies substantially both within and across neighborhoods. This geographic variation matters because high-quality schools improve children's long-run outcomes, and, for most of the United States, where families choose to live determines where their child goes to school (Reardon, 2018; Deming et al., 2014; Angrist et al., 2013; Chetty et al., 2011, 2014; Dobbie and Fryer, 2015). Nevertheless, many low-income families move to neighborhoods zoned to low-performing schools. This is particularly true for the recipients of housing vouchers (Section 8), a federal program with a goal to use housing as a "platform for educational opportunity" for low-income families (Ellen et al., 2016; HUD, 2011).

This paper shows that information frictions cause low-income families to live in neighborhoods with lower-performing, more segregated schools. We randomized the addition of school quality information onto the largest provider of online housing listings for families with housing vouchers. We show that providing information about zoned schools and their performance affects households' search behavior and moving decisions. Using data and variation from this experiment, we estimate a dynamic model of households' search and location choices that incorporates subjective beliefs and imperfect information about school quality. To understand the value of information and its implications for models of neighborhood choice, we use the model to estimate how much families value school quality and how much they would appear to value it if we ignored information frictions. We also show what beliefs about the distribution of school quality and other neighborhood characteristics can rationalize the choices of households who do not see the additional school quality information, and how these compare to expectations based on neighborhood characteristics.

To implement our design, we assembled a unique dataset on low-income households' search behavior. 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 and then track detailed information on users' search behavior. We obtained nationwide data on school proficiency ratings from a non-profit organization. We also used a nationwide dataset on school attendance zones to

assign units to their neighborhood schools. To estimate the impact of this information on users' search behaviors and residential choice, we then merged individual-level data from the listings provider with the universe of residential data on voucher recipients from the Department of Housing and Urban Development (HUD).

We find that the addition of school quality information has meaningful impacts on how families search for housing and where they choose to live. The additional information 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 find that families in the treatment group live in areas assigned to schools that have 0.10 standard deviation higher ratings than schools assigned to the locations of control group families. Families also appear to target the quality of the "next-level" of school for their child. If a family has a child less than age 5, the largest treatment effect is on the quality of elementary school; similarly for families with a child aged 5 to 10, the largest effect is on the quality of middle school.

The size of these effects depends on households' informed preferences and beliefs as well as the supply of desirable apartment units. The model allows us to measure demand and information 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 28 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 understate this valuation by more than 50%; families would trade 13 minutes of commute time for a 10 percentile point increase in school quality.

The experimental variation allows us to estimate a model in which households not only receive noisy signals of school characteristics, but also interpret these noisy signals according to a prior belief about the distribution of quality and neighborhood amenities that may depart from rational expectations. We show that families do not use observable neighborhood characteristics well to predict school quality. 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 a misspecified prior. We find that, if households were misinformed only in the sense of having unbiased noise, we would not find a positive treatment effect on the quality of zoned schools from our informational intervention. This result suggests that allowing for departures from rational expectations based on neighborhood characteristics may be important in models of search for housing.

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 when they are young (Chetty et al., 2016; Chyn, 2018). However, voucher-induced moves result in substantially less changes in school characteristics than neighborhood characteristics (Jacob, 2004; Sanbonmatsu et al., 2011; Gennetian et al., 2012; Jacob et al., 2014). More recently, Bergman et al. (2019) show that multiple barriers impede moves to neighborhoods that promote upward mobility. 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 as well as housing characteristics. The former typically show that distance to the school and its racial composition are strong determinants of families' choices (Hastings et al., 2009; Glazerman and Dotter, 2017). Families alter 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). Lastly, several papers show that test scores are capitalized into housing prices (Black, 1999; Figlio and Lucas, 2004; Bayer et al., 2007). While 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 market. Lastly, Waldinger (2018) and van Dijk (2019) use observational data from centralized assignment mechanisms, which have sources of randomness in assignment, to estimate demand

for housing characteristics in low-income populations. We contribute to this research by testing how low-income families respond to information in settings where schooling decisions and residential decisions are simultaneously determined, and incorporate imperfect information into a model of residential choice.

This paper first provides the empirical setting in Section 2 to describe relevant background on housing choice vouchers, study partners, the intervention, and subject recruitment. We then describe the data in Section 3, experimental results in Section 4, describe the model in Section 5, estimation in Section 6, and model results 7. We conclude in Section 8.

2 Background and Experimental Design

2.1 Housing Choice Vouchers

The Housing Choice Voucher (HCV) program is a Department of Housing and Urban Development (HUD) program for assisting low-income families to afford housing in the private market. Vouchers are administered locally by Public Housing Authorities (PHA). Qualified families who receive a voucher are 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 different between the rent charged by the landlord and the amount subsidized by the program.

Families are eligible for a voucher based on their total annual gross income, family size, and citizenship status.[1] Income eligibility cannot exceed fifty percent of the median income for the county or metropolitan area where the family chooses to reside. However, PHAs are required to provide 75 percent of vouchers to very low income families, which are those whose income do not exceed 30 percent of the area's median income. Families may apply with their local PHA, who collects information on income, assets and family composition. The information is verified with banks, employers and local agencies. After verification, the family is generally placed on a waiting list. The length of time spent on the wait lists ranges widely between

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

PHAs.

HUD provides standards for fair market rents (FMR), which are the amounts that determine how much rent can be covered for voucher-holders in each market. Acknlowledging that there could be regional deviation from estimated FMRs, PHAs then are allowed to use fair market rents to determine payment standards, which determine the monthly subsidies that will be covered for voucher holders.[2] Families who receive a voucher are free to search and choose housing that both meets the requirements of the program and their own needs and desires, provided that the unit owner is willing to accept the voucher. As part of the program, voucher-holders are generally required to pay 30% of their adjusted gross income each month for rent and utilities.[3] An important implication of this rule is that the typical voucher-holder faces no price variation within the set of units that will accept a voucher.

2.2 Study Partners

The implementation of the intervention relies on two study partners, GreatSchools and GoSection8. GreatSchools is a non-profit organization providing free and accessible school quality ratings to families via a web-based platform on 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, is developed to assist families in comparing schools within states. Ratings are whole numbers between one and ten [1-10] and are categorized as below average [1-3], average [4-7], and above average [8-10]. These ratings were color-coded on GreatSchools' web site, with green representing above average scores, orange-yellow representing average scores, and red representing below average scores. Our goal is to assess families' response to this measure, which is used by large-market share real-estate websites (i.e. Zillow, Trulia, Redfin).

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

[2] Payment standards must be between 90-110% of fair market rents.

[3] It is possible to lease a unit over the payment standard, but the family is required to pay more than 30%. The maximum a family can pay is up to 40% of its adjusted gross income, usually only allowed in the first year of lease up. We find that leases rarely exceed 30%.

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 allows both landlords and housing authorities to post listings. Additionally, a service provided to local and government housing authorities is the ability to develop an area-specific internet portal integrated into housing authorities' websites. For example, the New York City Housing Authority has a specific portal created by GS8 that shows only listings specific to the five NYC boroughs. In this manner, the GS8 database has become a primary source of rental listings for many local housing authorities. Overall, the site receives approximately 400,000 unique visitors per month and registers 11,000-13,000 prospective tenants per month, as of the beginning of this study in 2015.

GS8's interface is similar to that of Zillow or other housing search platforms. The platform solicits listings from public housing authorities' lists of voucher-friendly landlords, as well as from landlords directly. Upon arriving to the site, a potential tenant logs in, then types in a search query, such as a city or zip code. This query results in a set of listing titles shown to the user. The listing results displays the address, rent and the number of bedrooms and baths for each of a group of listings. Clicking on a listing brings up a 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 listing page contains contact information for 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.

2.3 Subject Recruitment

The intervention was implemented in late May 2015 and ran through February 2017. Users to the site received a pop-up requesting whether the user would like to sign up for the study (see Figure 2) via the landing page (see Figure 1). If the user selects "no," they then navigate the website as a non-participant of the study without school-quality information. If the user instead selects "yes," the user is taken to a page

with the consent agreement and asked to fill out an initial survey (see Figure 3). This is our initial attempt at collecting pertinent data, including the registrant's name and contact information, along with the head of household's name and date of birth, whether the family has children, current zip code, and voucher status/housing agency. The information obtained for the head of household is used as baseline data and provides necessary information on which to merge to HUD's database of voucher users to obtain outcome data. Upon completion of the survey, the user is registered both as a tenant registrant on the website and as a participant of our study.

Randomization into treatment and control is then automated by the website at the moment of survey completion, and any subsequent browsing, searching, and page-viewing are tracked via IP addresses and cookies. Treatment assignment is stratified by voucher status.

The recruited participants totaled 5,743. Of these, 3,012 self-reported that they hold a voucher. Of the voucher holding participants, we were able to match 1,965 heads of households with stated vouchers to HUD data, or about two-thirds[4]. Table 1 shows the balance between the analytical sample (those matched to HUD data) and those who were unmatched. This comparison depicts the differences between GS8 users found in the HUD database (which we refer to as the *matched* sample) and the more general user base of GS8 (which we refer to as *unmatched*). Variables are primarily derived from the baseline survey completed at time of participation agreement, which asks users regarding their childrens' age, time to move, and reason for moving. We find some differences between the two groups. While the percentage of users reporting at least one child is almost identical (93%), matched participants less often report to have children below four but more often report to have children eleven and above. Reasons for moving also differ. Those who matched are more likely to state jobs, family, or transportation as the primary reason why they desire to move, whereas the broader sample is more likely to move due to marital status, schools, apartment

[4]There are various reasons why not all families who reported that they have vouchers in hand were matched to HUD. Some of these reasons may be self-reporting error regarding voucher status, error - whether intentional or otherwise - in reporting the accurate first and last names or date of birth of the head of household, or the fact that PHAs vary greatly in their efforts to collect data on every household that is offered a voucher yet never use it. In speaking with a HUD representative, the latter reason is one reason that there may be error in matches from their perspective.

issues, or failed inspections. Unmatched participants also reported wishing to move within three months substantially more than the matched sample.

2.4 Intervention

A user randomized into treatment can immediately view the GreatSchools Ratings module within each property listing's profile (see Figures 4 and 5). 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.[5] The school listing below the map presents the rating, the school name, school type (all are public), grades served, distance from the property, and a check mark if the school is the assigned school for that particular property. Ratings are displayed within a colored circle, with green representing an above average rating, orange an average rating, and red a below average rating.[6] The treatment is only preceded by the general property details and description first seen when the end user clicks on the webpage (See Figure 4).

Both treatment and control group users can view a property's profile without 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 include rental price, number of beds and baths, and an occasional short description written by a landlord.

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, provides some of the baseline and school characteristics and

[5] If an assigned school is not available for the unit's location, users are shown the closest elementary, middle, or high school.
[6] Clicking on a school 'details' link immediately takes the user to a web page on GreatSchools.org where they can view additional information for the specific school.

ratings used for the intervention.

3.1 GoSection8 Data

From GoSection8, we obtained data on tenant study registration. Tenant registration files contain data on randomized treatment status, initial sign up date, and self-reported results from the baseline survey. The baseline survey includes questions regarding the head of household name and birth dates, intended move dates, voucher status, and the age of children in the household. We use self-reported name 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 landlord via a "contact the landlord" section 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 property.

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," with the next most popular reasons being "to get better transportation" [14%] and eviction [13%]. Given the relevance of schooling as the reason for moving and the prevalence of school-aged children in our sample, there are indications that the population of GS8 users can realize educational benefits from our treatment.

3.2 GreatSchools Data

GreatSchools provided school quality ratings, geocoded school locations, district boundaries, and assigned school data. Geocoded school location, district boundaries, and school assignment data was specifically purchased from the commercial product Maponics. This data was provided to both the research team and GoSection8 for module implementation. 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 as measured by high school graduation rates, participation and performance on college entrance exams and/or Advanced Placement data, academic rigor of a school based on enrollment into advanced courses, equity as based on performance of disadvantaged students, test scores for low-income students, and discipline and attendance data. At the time of the pilot intervention, however, ratings were primarily based on standardized test score proficiency rates due to data limitations. GreatSchools did also incorporate student progress and college readiness data whenever available by state. Scores are calculated, and so are only comparable, within each state.

The distribution of the universe of school ratings are shown in figures 6 for elementary, 7 for middle, and 8 for high schools respectively, in the dark blue bars. Ratings tend to peak at six or seven and overall tend to be distributed fairly normally around a mean of 5.5 and standard deviation of 2.5. However, the highest ratings on average are from elementary schools and the lowest are from high schools, which also have the lowest share of schools rated above average. There is also strong variation in the ratings within metro areas.

As can also be seen in figures 6 through 8 in the clear bars with red outlines, properties listed on GoSection8 have significantly lower ratings on average, with its ratings distribution heavily skewed to the right. The implication here is that the distribution of schools associated with the rental supply to HCV holders are rated lower than the schools associated to the residential properties of the general population. Further, choice is restricted by the low variation in ratings within metropolitan areas.

For example, 88% of the top 100 cities by number of GS8 rental property listings has a elementary

school rating standard deviation of greater than 2.5, which is the standard deviation of the universe of schools. Additionally, the three cities with the most listings on GoSection8 all show significantly worse schooling options for its low-income properties. In Los Angeles, the universe of elementary schools average a 4.6 rating, whereas the schools associated with listed properties average 3.3. In Baltimore, it is 4.0 and 2.9, and in Chicago, it is 4.0 and 2.5, respectively.

3.3 HUD Data

HUD provided quarterly data on the universe of voucher recipients, which includes household member's demographics, income, property bedrooms, property rent and required payments, and property geocoded locations. We linked this data to GoSection8 tenant participant data based on first name, middle name, and date of birth of the head of household.[7]

As part of the study on GoSection8, we recruited 2,968 eligible participants who self-reported that their household had a voucher. Of these participants, we were able to match 1,965 heads of households (66%)[8] to HUD data based on name and date of birth using data from 2013 through 2018. The latest HUD data spanned about 2 years after the end of the experiment. In speaking with HUD, 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. However, HUD administrators also noted that PHAs do a poor job of keeping track of those who are granted a voucher but never moved as a result.[9] When a family receives a voucher, there is a limited amount of time (often 60-90 days) for the user to locate a property owner that accepts the voucher.

Table 3 compares a 5% sample of heads of voucher-holding households between 2013 and 2017 to our sample of GoSection8 head of household participants who were matched to HUD data. There are substantial

[7] We merged data from GoSection8 to HUD based on fuzzy merging Stata package dtalink. Prior to the merge, we also used the Stata package soundex to transform names into an alphanumerical form understood by Stata.

[8] There were no significant differences in treatment status between matched and unmatched users.

[9] As a result, there is the possibility of matching some voucher holders who never used the voucher and are in the data, but not matching others since they may not be in the data.

differences between the matched participants from GoSection8 and the sample of HUD voucher holders in terms of gender, disability status, race, household size, and rental/utility payments. For the most part, our sample contains slightly more females, less disabled head of households, less Hispanic, and higher rent than the average voucher-holding families. However, much of these differences may be due largely to the sample size differential. It may also reflect the differences between a user of GoSection8 and the general low-income population, such as differences in Internet access or ability to freely use the Internet, such as the case for some disabled users.

Table 4 compares demographic characteristics of census tracts[10] of listed properties on GoSection8.com to census tracts of residential locations of our "matched sample". In general, neighborhoods are similar in poverty rates and education levels, but differ somewhat in racial composition. In particular, ther neighborhoods listed on GoSection8 have higher populations of white families. At a neighborhood level, there is mixed evidence of observable differences.

Table 5 compares the universe of US elementary schools to the set of schools zoned to GoSection8 property listings, schools zoned to HUD listings, and to the schools at which our study sample lives at baseline conditional on baseline voucher use. The average US primary school has a GreatSchools rating of 5.78 out of 10. The mean of the GoSection8 listings are about half a point lower. The mean of a 5% random sample of HUD listings is roughly an additional point lower. The mean among our study sample is more than an additional point lower. Share receiving free or reduced-price lunch (Share FRPL) rates at zoned schools, a common proxy for poverty, are 84% in our sample at baseline, higher than the 71% rate HUD units' zoned schools and 63% at GS8 schools, which in turn is higher than the U.S. average. Despite only minor differences in observable census tract characteristics, there is substantial differences in the schools associated with the properties that our study sample is residing in. Specifically, the GreatSchools rating difference between the HUD 5% sample and our study sample is equivalent to about 0.4 standard deviation.

[10]We used American Community Survey (5 year estimates, 2013-2017) census tracts instead of the smaller and more locationally-precise census blocks because census blocks contain a lot of missing data for our sample. By its nature, census blocks contain a smaller number of households. The data for small n's are often not reported for privacy purposes.

We show baseline sample characteristics and treatment-control group balance in Table 2 by regressing an indicator for treatment assignment on baseline covariates. The regression to check for balance is as follows:

$$\text{baseline variable}_i = \beta_0 + \beta_1 \text{Treatment}_i + \varepsilon_i,$$

where Y is the baseline variable of interest for family i and β 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 the user being in the Moving to Work (MTW) program. However, an omnibus test for treatment-control differences is not significant (p-value=0.19).

Empirical Strategy

The main empirical analyses estimates an intent-to-treat (ITT) effect. To measure the effect of treatments on various outcomes, we estimate the following:

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

where Y is the outcome of interest for family i, and β is the coefficient of interest for the ITT estimator resulting from the treatment. As long as the randomization is implemented correctly, this allows us to estimate the treatment-control differences without any additional covariates. In the following section, we provide the estimated impacts of the intervention.

4 Impacts

Figure 9 shows the cumulative number of properties families view each week relative to their intended move date, which is normalized to 0. Weeks prior to the intended move date are therefore negative and weeks

after are positive. 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 2 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 steadily declines. There is no significant difference in the patterns across group. We show this formally in the regression tables below.

There are also no significant differences in the school quality of listings viewed by families. Panel (a) of figure 10 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. The lack of differences in the quality of views between the treatment and control group is not entirely surprising; the school quality information is not shown to a user until *after* they click and view a particular listing. Thus, at the time a user clicks on a listing, they may not know the quality of schools associated with it. However, families may infer and learn over time about which neighborhoods have good schools concentrate their searches accordingly. We test this further below.

After a family views a unit they may choose to make inquiry to the landlord about that unit. We recorded if the family sends an inquiry to the landlord via the platform, though we cannot observe whether families called landlords directly. Figure 9 traces out the average number of inquiries made per 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 spike in inquiries in the week prior to when they would like to move, and slightly higher inquiries before then. As we test below, the treatment group makes significantly more total inquiries than the control group.

Once a 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 10, panel (c), shows that there is an increase in the school quality associated with the units families inquire about. Most

of the increase inquiries is concentrated among units with average school quality ratings between 3 and 5.

Table 6 presents these and other search-related regression results. Families make, on average, 33 views. Nearly 80% make at least one view and families view properties in 14 different census tracts. There are no significant effects on these outcomes.

Even though families reach out to landlords in areas with better schools, this may not translate into impacts on the quality of schools assigned to where a family lives. Using address information from HUD, we test for impacts on the quality of schools assigned to where families actually live. Table 9 shows the results across several measures of quality. School quality averaged across the elementary, middle and high school ratings associated with a families address improves by 0.29 points, which is statistically significant. If we instead used the maximum or minimum school quality across the elementary, middle and high school assigned to an address, the impacts are 0.34 and 0.20, respectively, and remain significant. Figure 11 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. Overall, the effect on average school quality translates into a 1.5 percentage point increase in the share of students marked "proficient" or above on state exams.

Breaking these results down further, the effects are slightly larger for the middle school quality than the elementary school quality assigned to a unit. There also impacts on the average demographics of the schools assigned to where families live. Schools assigned to treatment group families have 2 percentage points fewer students receiving free or reduced-priced lunch and students who are Black or Hispanic. This result is not surprising given that school demographics are highly correlated with school performance.

We show that effects are concentrated in schools which parents may expect to send their children in the future. We define a "next school" measure based on the age of the youngest child. If the child is 0-4[11], the "next school" is the zoned elementary school. If the child is 5-10, the next school is the middle school.

[11] We break down the child's age in terms of ranges because when asking the participant the age of their children on the baseline survey, one requirement from the website was to make it as easy as possible. We decided to break it down by ranges that correlate to the age range of school levels. For example, children tend to be in elementary school when they are between 5-10 years of age, and in middle school from 11-13, and so on.

Otherwise, it is the zoned high school. Table 10 shows that families target the next school, with impacts on primary schools for households with young children and on middle schools for those with children 5-10.

Table 11 shows whether the additional school quality information resulted in families choosing neighborhoods with different characteristics. In general, this is not the case. There are minor differences in the racial composition of neighborhoods, none of which are statistically significant. There is evidence in a slight—1 percentage point—decrease in poverty rates, and an increase in moves to low-poverty ($<10\%$) neighborhoods. Figure 12 shows that there is a slight but broad-based shift leftward in the neighborhood poverty rates of treatment families. For context, the MTO experiment induced moves to neighborhoods with 12 percentage point lower poverty rates (Kling, Liebman, Katz).[12] While the demographic composition of families' neighborhood choices do not change tremendously, there are larger, more significant effects on their distance from downtown and the "walkscore" of the neighborhood. The latter reflects what local amenities and public transportation are nearby. Commute to downtown increases by 2 minutes, which implies families are moving slightly farther out from the city center to live in areas with better schools.

One question is whether families appear to value school quality not correlated with surrounding neighborhood's characteristics. We regress the school-quality measure on the neighborhood characteristics listed in the table, and then regress the residual on the treatment indicator. The effect on residual school quality, shown in the bottom row of the table, is smaller but still more than 60% of the original, unresidualized coefficient and statistically significant.

Lastly, HUD tracks only limited data on characteristics of the units families live in. Table 8 shows the effect of the information treatment on unit rent, the number of bedrooms, and beds per household member. However, the design of the voucher program implies that there is unlikely to be an affect 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 precisely 30% of their income toward rent. Moreover, families are eligible for a number of bedrooms

[12] 12 percentage points is the Intent-to-Treat Effect.

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 not impact on the rent paid and the number of bedrooms. Instead, as shown above, families appear to trade off unit location—distance from downtown—for additional school quality.

We explore heterogeneity by baseline voucher status. To do so, we consider those households who are already present in HUD data at baseline. These households are currently using a voucher, and are typically not forced to move. We find significant effects on inquiries for this population, as shown in table 14.

5 Model

We now estimate a model of households' search behavior and moving decisions, recovering households' preferences, their costs of moving, and their prior beliefs about the joint distribution of school quality and other chacteristics. The model allows us to compare households' beliefs to rational-expectations chances and to measure informed households' willingness to trade off school quality for other amenities.

The model is designed to capture the following features of the market. First, if a household fails to use its voucher within a given period, the household loses the voucher. In contrast, once a household signs a lease, it commits to pay rent at that location for a year. Therefore, 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, but we assume no recall across periods.[13] Third, not all search is conducted online. Potential tenants may discover other apartments through social networks or by chance. Our model therefore allows for off-platform search as well as on-platform.

[13] 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 out of 100 landlords responded to online inquiries, typically with a lag of a day or two.

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. There is no discounting; however, if the household fails to move by time T, it receives an outside-option payoff which is normalized to zero.[14]

The set of available apartments in market m is denoted $\mathcal{J}_m$. Household i's indirect utility conditional on moving to unit $j \in \mathcal{J}_m$ is given by $u_{ij}(x_{ij}, q_{ij}, \epsilon_{ij})$. It depends on school quality of the unit's zoned schools $q_{ij} \in \mathbb{R}$, other unit and neighborhood characteristics $x_{ij} \in \mathbb{R}^K$, and a preference shock $\epsilon_{ij} \in \mathbb{R}$.

At beginning of period t, household i in market m receives a cost draw

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

iid across periods. i observes c_{it}^{search}, then chooses whether to use the search platform. If so, i pays c_i^{search} and draws a set of listings $J_{it}^{on} \subset \mathcal{J}_{m(i)}$ from a joint distribution over sets of apartments,

$$F_{go8}(|J_{it}|, \{q_{ij}, x_{ij}, \epsilon_{ij}\}_{j \in J_{it}^{on}}; m, w_i)$$

where $m(i)$ denotes i's market and w_i represents characteristics of household i.[15]

Regardless of its on-platform search decision, the household draws a set of additional units

$$J_{it}^{off} \sim F_{off}(|J_{it}^{off}|, \{q_{ij}, x_{ij}, \epsilon_{ij}\}_{j \in J_{it}^{off}}; m, w_i)$$

each period.[16] We let $J_{it} = J_{it}^{on} \cup J_{it}^{off}$ denote the set of all properties viewed by i in period t.

[14] Normalizing the outside option is without loss. We allow households who currently hold a voucher or otherwise have more attractive options to have lower relative preferences for the "inside" goods.

[15] For example, the relevent notion of school quality, "next school quality," depends on the ages of the children in household i.

[16] Regardless of the household's search decision, the household may discover additional apartments offline by walking past "for rent" signs or via word of mouth.

The household observes characteristics $\{x_{ij}, \hat{q}_{ij}, \epsilon_{ij}\}_{j \in J_{it}}$ for each property in J_{it}. These characteristics include covariates x_{ij} and the household's signal of quality $\hat{q}_{ij}$, which need not equal the true quality q_{ij}. Household i then draws a moving cost

$$c_{it}^{move} \sim F(c^{move}|c_{it}^{search}),$$

and chooses a set of units to submit inquiries to,

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

An inquiry succeeds with probability α_j and fails with complementary probability. A failure means that the unit is unavailable, or is unacceptable to the tenant in some way.

Next, household i simultaneously learns the set of properties at which her inquiries were successful, $J_{it}^{choice} \subseteq J_{it}^{inquiry}$, then chooses whether to accept (i.e. move to) any of the properties in this set. If household i moves to unit j in week t, The household incurs a cost c_{it}^{move}. Households cannot move to units at which their inquiries were unsuccessful. If i accepts some unit, j, it receives utility, net of moving costs, given by $u_{ij} - c_{it}^{move}$, and becomes inactive. Otherwise, i continues to the next period.

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 for on-platform listings. Treated households may have imperfect signals of quality for off-platform listings, $\hat{q}_{ij} \sim G_{\text{off}}(\hat{q}_{ij}|q_{ij})$.

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 prior belief over the conditional distribution of q_{ij} given characteristics x_{ij}.

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

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

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 $\hat{u}_{ij} - c_{it}^{move} > V_{i,t+1}$.

We assume that inquiry costs are small, and consider the limit in which they approach zero from above.[17] In this limit, i submits an inquiry to j at time t if and only if $u_{ij} - c_{it}^{move} > V_{i,t+1}$.

The payoff from on-platform search is given by

$$V_{it}^1 = E\left(\max\left\{V_{i,t+1}, \max\{\hat{u}_{ij} - c_{it}^{move}|j \in J_{it}^{choice}\}\right\}\right),$$

where the expectation is over the sets $J_{it} = J_{it}^{off} \cup J_{it}^{on}$, the moving cost c_{it}^{move}, and the outcomes of inquiries.

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

i chooses to search in period t if

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

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

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

The model delivers the following qualitative predictions. First, agents are less likely to use the platform early in their search process. This prediction follows from two properties of the model. First, V_{it} is falling

[17] Submitting an inquiry consists of typing a message into a text box or calling the landlord. We believe the costs of these activities are small relative to those of signing a lease and paying a deposit.

in time. In particular, the agent has the option not search in period t, moving only if an off-platform unit offers greater utility than waiting, then playing optimally in all subsequent periods. Under this strategy, $V_{i,t+1} < V_{it}$. Second, the probability of on-platform search is decreasing in V_{it}. In particular, household i searches with probability $F_{c^search}(V_{it}^1 - V_{it}^0)$. To see that $(V_{it}^1 - V_{it}^0)$ is decreasing in $V_{i,t+1}$, consider the event that the best acceptable on-platform unit in period t offers utility u^1 to agent i net of the moving cost while the best acceptable off-platform unit in period t offers utility $u^0 < u^1$. We have $V_{it}^1 = \max\{u^1, V_{i,t+1}\}$ and $V_{it}^0 = \max\{u^0, V_{i,t+1}\}$. Therefore

$$V_{it}^1 - V_{it}^0 = \int_{u^1 > \max\{V_{i,t+1}, u^0\}} u^1 - \max\{V_{i,t+1}, u^0\} dF_{u^1,u^0;\theta}(u^1, u^0|\theta),$$

where $F_{u^1,u^0;\theta}(\cdot)$ is the distribution of highest on- and off-platform utilities of units with successful inquiries.

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 costless off-platform search in the future. With a short horizon, search is more valuable.

A second model prediction is that measurement error in q means that agents' inquiries and moves will be less sensitive to variation in q conditional on x. Agents' apparent preferences for other characteristics will depend on their true preferences as well as their subjective prior.

6 Identification and Estimation

6.1 Empirical specification

In our empirical specification we make the following functional-form assumptions.

$$
\begin{aligned}
u_{ij} &= x_{ij}\beta^x + q_{ij}\beta^q + \epsilon_{ij} \\
c_{ijt}^{\text{search}} &\sim N(\mu_{\text{search}}, \sigma^2_{\text{search}}) \\
c_{ijt}^{\text{move}} &\sim N(0, \sigma^2_{\text{move}})
\end{aligned}
$$

Here, q_{ij} is "next school" quality. X includes the following unit and neighborhood characteristics: racial composition, poverty rate, walk score, distance from downtown. In addition, we include interactions between racial composition and own race. We allow the constant term β_0 to vary with the household's race as well. The restriction $\mu_{move} = 0$ without loss of generality, as c_{move} enters utility additively with β_0.

Households share a prior according to which $E(q|x) = x'\gamma$. The signal $\hat{q}$ is the sum of the true quality q and additive measurement error which is jointly normal q. That is, for variance parameters σ^2_η and σ^2_q, for untreated households the signal distribution is given by

$$
\begin{pmatrix} q \\ q+\eta \end{pmatrix} | x \sim N\left(\begin{pmatrix} x'\gamma \\ x'\gamma \end{pmatrix}, \begin{pmatrix} \sigma_2^q & \\ \sigma_2^q & \sigma_2^q + \sigma_2^\eta \end{pmatrix} \right).
$$

Treated households' errors about off-platform units are analogous, but with parameter $\sigma^2_{q_{\text{off}}}$ instead of σ^2_q. Paralleling our treatment of preference heterogeneity, we include interactions between racial composition and own race in the prior belief mean γ, and allow the mean belief about quality γ_0 to depend on race as well.[18]

On-platform listings are sampled from empirical distribution of $\{J_{it}\}_{i \in I_m,\ t=1,\ldots,T}$ by market and treat-

[18] We wish to use experimental variation to pin down γ. Given that race is a preference shifter, we do not want to assume that it is excluded from beliefs.

ment status. We assume off-platform units are drawn from the same distribution as on-platform units, and that the number of off-platform units drawn in a period is distributed $Poisson(\lambda)$. We allow for a linear time trend in off-platform arrival rates,

$$\lambda = \lambda_0 + \lambda_t t.$$

6.2 Identification and testable restrictions

We first discuss identification of belief parameters within the context of our parametric model. We then provide a more general intuition.

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

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

The econometrician observes q and x but not $\hat{q}$ or ϵ. From our point of view,

$$\hat{u}_{ij} \equiv E(u_{ij}|q_{ij})$$

is a random variable with

$$\hat{u}|x, q \sim N(q\left(s\beta_q\right) + x(\beta + (1-s)\beta_q\gamma), \sigma_\epsilon^2 + s^2\beta_q^2\sigma_\eta^2).$$

The derivation for off-platform properties is analogous.

We now use this result to show that, for each $(\beta, \gamma, \sigma_\eta^2, \sigma_q^2, \mu_{search}, \sigma_{search}^2, \sigma_{move}^2)$, there is an observationally equivalent "full-information" model in which q is observed without error at on-platform properties. Define

$$\sigma = \sqrt{1 + s^2 \beta_q^2 \sigma_\eta^2}.$$

Define other parameters as follows:

$$\begin{aligned}
\tilde{\alpha} &= \alpha \\
\tilde{\lambda} &= \lambda \\
\tilde{\beta}_x &= \sigma^{-1} \left(\beta_x + (1-s)\beta_q \gamma \right) \qquad (1) \\
\tilde{\beta}_q &= \sigma^{-1} s \beta_q \qquad (2)
\end{aligned}$$

Define rescaled shocks as follows:

$$\begin{aligned}
\tilde{c}_{it}^{search} &= \sigma^{-1} c_{it}^{search} \\
\tilde{c}_{it}^{move} &= \sigma^{-1} c_{it}^{move} \text{ for some } \tilde{k} > 0 \\
\tilde{\epsilon}_{ij} &= \sigma^{-1} \left(\epsilon_{ij} + s \beta_q \eta_{ij} \right).
\end{aligned}$$

Define

$$\tilde{u} = x\tilde{\beta} + q\tilde{\beta}_q + \tilde{\epsilon}.$$

Then $\tilde{\epsilon} \sim N(0,1)$. Moreover, $(\tilde{u}, \tilde{c})$ is sufficient for the agent's decisions, and hence for the likelihood.

One may estimate the full-information model separately by treatment status, recover parameters $\tilde{\alpha}, \tilde{\beta}, \tilde{\mu}_{search}, \tilde{\sigma}_{search}$ and obtain "as if" willingness-to-travel measures $\frac{\tilde{\beta}_q}{\tilde{\beta}_{commute}}$ relating taste for quality to distaste for distance from city center.

Cross-equation restrictions on the parameters of the "equivalent models" for the treatment and control

groups then pin down the subjective prior terms γ and $s = \frac{\sigma_q^2}{\sigma_\eta^2+\sigma_q^2}$. In particular, under the maintained assumptions that the treatment is purely informative and agents are Bayesian, we can find the subjective belief parameters (γ, σ, s) that best reconcile the estimates from treatment and control groups.

If the true γ is known, we can test hypothesis of Bayesian updating under maintained assumption that treatment operates only via information channel, vla a LMT or Wald test. 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 x.

Our parametric model gives convenient functional forms for the "as-if-fully informed" preferences of the control group $\tilde{\beta}$ as a function of β, γ, and σ (equations 2 and 1, or equivalently for γ and σ given estimates of the "full-information equivalent model" parameters for both groups. However, the intutition for identification of beliefs is more general. A Bayesian agent who receives noisy signals of quality q will shrink these signals toward a prior belief. The greater the noise, the more weight the agent places on her prior. If the agent believes that some characteristic, x, is positively associated with quality, the agent will have greater demand for units with high values of x. When this agent is informed about quality, she will appear to value x less. Thus, prior beliefs about the relationship between, say, walkscores and school quality are revealed by treatment effects of information provision about school quality on demand for walkscores. This intuition does not rely on normality or linearity.

6.3 Estimation procedure

We first recover the preference, cost and information parameters:

$$(\alpha, \beta, \mu_{search}^{treat}, \mu_{search}^{control}, \sigma_{search}^{treat}, \sigma_{search}^{control}, \lambda_0, \lambda_t, \sigma_\eta^{control}, \sigma_\eta^{treat*offplatform}),$$

where $\sigma_\eta^{2,control}$ and $\sigma_\eta^{2,treat*offplatform}$ denote the variance of measurement error on quality for untreated households, and for treated households' off-platform views respectively.

In order to restrict λ to be identical across treatment status, we estimate the model jointly on treatment

and control groups. We use the method of simulated moments. We solve the model separately for each of several types defined by treatment status, race, baseline housing status, and market. We then form moments 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 are observables[19] and y are the following moments: 1(search) in period t, $t = 1, \ldots, T$; Number of inquiries in t, $t = 1, \ldots, T$; 1(match to any HUD); 1(match to viewed listing); 1(match to direct inquiry); mean inquiry characteristics; mean match characteristics.

We fix σ_q^2 at its rational-expectations value.[20] Having recovered these parameters, we use equations 2 and 1 to recover the preferences we would have obtained had we ignored information frictions. We obtain standard errors via the delta method.

6.4 Computation

We estimate the model using a dynamic version of the fixed-grid approach due to Ackerberg, 2009. An outline of the procedure is as follows:

1. Fix grids and compute optimal policies:
    - Fix a grid of on- and off-platform draws of size (M, T), where M is the number of draws per period and T is the number of periods. For each grid point, draw recentered utilities $u^* = u - \beta_0$.
    - Fix a grid of recentered moving costs: $c^* = c^{move} + V - \beta_0$
    - given c^* and u^*, the optimal policy is fixed: inquire iff $u^* > c^*$.
    - For each grid point (u^*, c^*, t), store moment contributions and payoff contribution

$$E\left((u^* - c^*)1(\text{move})\right) \equiv E((u - c^{accept} - V_{t+1})1(\text{move})).$$

[19] $(\mathbf{1}, treatment_i, black_i)$

[20] We regress q on x in the set of GoSection8 units, and estimate the residual variance. Our estimation procedure reveals the posterior variance, which depends on σ_q^2 and σ_s^2 via the precision term s.

This step does not depend on parameters and can be done once.

2. Obtain weights at trial parameter vector θ.

- Start in period T. Fix $V_{T+1}(\theta) = 0$. Compute weight on each grid point (u^*, c^*, T). At a given parameter vector, we have:

$$\epsilon(\theta) = u^* - (0, \beta_1, \ldots, \beta_K) \sim N(0, 1)$$
$$c^{move}(\theta) = c^* - V_{T+1} + \beta_0 \sim N(0, \sigma^2_{c^{accept}}).$$

  - The likelihood of a utility grid point (m, t) is given by

$$w_u[m, t] = \frac{1}{p_0[m, t]} Pr(|\mathcal{J}^{\text{off}}_{mt}||\lambda) \prod_{j \in \mathcal{J}_{mt}} \phi(\epsilon; 0, 1 + s^2 \beta_q^2 \sigma^2_\eta(\text{off;on})).$$

    Here σ^2_η(off;on) is the variance of η for the property, which depends on the household's treatment status and whether the property is on- or off-platform, $\mathcal{J}_{mt}$ is the set of properties in draw (m, t), $Pr(J|\lambda)$ is probability that a Poisson(λ) equals J, and $p_0[m, t]$ is the probability according to which the cell was drawn in step 1.

  - The likelihood of a cost grid point (c, t) is given by

$$w_c[c, t] = \phi(c^{move}(\theta); 0, \sigma^2_{c^{move}}).$$

  - A point (m, c, t) has weight $w_c[c, t] * w_u[u, t]$.

- Compute weighted averages of payoff contributions: $V^1_T(\theta)$, $V^0_T(\theta)$. Let $\Delta V_T = V^1_T(\theta) - V^0_T(\theta)$.
- Update $V_T(\theta) = V_{T+1}(\theta) + V^0_T(\theta) + E((\Delta V_T - c^{\text{search}})1(\Delta V_T > c^{\text{search}}))$.
- Recurse backwards.

In practice we solve a sequence of penalized problems with penalties proportional to $\sum_{m,t} w_u^2$, taking

the penalty term to zero. After each problem in the sequence we redraw the original grid. This procedure allows us pick grids that approximate the density well.

To match the time-varying moments (i.e. the number of views in each period $t = 0, \ldots, T$, and number of inquiries in each period,) we interact the data and model predictions with basis functions

$$b_k(t) = \cos(k\pi t/T)$$

for $k = 0, 1, 2, 3$, giving four moments to match for the pattern of views, and four moments for inquiries, before interacting with observables w_i. This allows us to fit time patterns of activity with fewer moments. These functions are shown in figure 13.

7 Results

We present preliminary parameter estimates in table 15. We allow for heterogeneous outside options as a function of race. We find that households have tastes for higher quality and lower commute distance to city center. A treated household is willing to trade off commute distance and mean school quality at a rate $\beta_q/\beta_{\text{commute}} = -0.951$, or nearly an hour per GreatSchools point.[21]

In contrast, calculating the as-if preference parameters $\tilde{\beta}$ for the control group implied by our estimates of $\beta, \gamma, \sigma_q^2$, and σ_η^2, we find $\tilde{\beta}_q = -0.0117$, and $\tilde{\beta}_q/\tilde{\beta}_{\text{commute}} = -0.098$, or 6 minutes per point, implying that, had we estimated the model ignoring information frictions, we would have found a substantially lower willingness to travel for school quality.

In table 16 we compare our belief parameters (γ, σ_η) to those of an OLS regression of mean quality on characteristics in our sample of housing units. A Wald test of equality of $\gamma = \gamma^{\text{OLS}}$ massively rejects this hypothesis ($p < 0.0001$) for households whose head-of-household is not Black, and is marginal ($p = 0.0664$) for Black households.

[21] Not all households work in the city center, so distaste for commute to work location is likely larger than β_{commute}.

We plot the implied distributions of residual quality $q - E(q|x)$ in figure 14. This residual is the object that our information treatment provides to households when their signal η is completely uninformative. Subjective belief residuals place greater weight on positive values, indicating that our treatment may provide "surprise" information about the presence of high-quality schools in certain locations.

Table 17 compares the informed preference parameters reported in 15 to the full-information equivalent preferences for the control group. Black and nonblack households differ in the constant terms and in the taste for share Black within the neighborhood. Both treatment and control groups put positive weight on quality and negative weight on commute. The informed preferences place a larger negative weight on commute than do the full-information equivalent preferences of the control group. We obtain standard errors on the equivalent preferences via the delta method. Estimates of the constant term are noisy; model estimates indicate that the variance of the moving-cost shock is large, making it difficult to pin down the mean utility level.

The first two columns of table 18 show model fit. The first column, labeled "data", reports treatment effect estimates. We report coefficients on treatment status from a regression of total inquiries (1st row) on treatment status and race, and from a regression of endline "next-school" quality (2nd row) on treatment status and race. 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 estimate for inquiries closely, cannot reject matching treatment impacts on endline quality as well. Thus the model seems to fit key causal-impact estimates well. Figure 15 shows model-predicted vs observed differences in number of inquiries by treatment status per week, indicating good fit to noisy data as well.

The final column of table 18 shows the results of a counterfactual in which households are endowed with the rational-expectations prior shown in table 16, but control-group households, and treated households viewing off-platform units, continue to observe q with normal measurement error with variance σ_η^2 as

estimated. In this counterfactual, we find that treatment would have a large impact on the number of inquiries, as it reduces uncertainty about where the good schools are, but the impact on endline quality, which was significant and positive, goes from roughly 3 to 4 percentiles in the data to -1 percentile, with a confidence interval that covers zero. We conclude that our intervention had positive impacts on endline quality in part because of uninformed households' systematically incorrect beliefs about school quality at locations at which they do not currently live.

8 Conclusions

In this paper we conducted a randomized trial of an information intervention. We showed that providing information about school quality to households at a key point in their search process led them to move to areas with higher-quality zoned schools. 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.

Moreover, information is not the only friction in low-income housing markets. There are serious financial and hassle costs associated with moving. Low-income households may find it difficult to pay security deposits and application fees. Moreover, lack of supply may incentivize households to accept units even if the school quality is low. Policies to ameliorate these frictions include subsidies or loans to tenants to address moving costs among liquidity-constrained population, as well landlord incentives, construction incentives and regulatory changes, or increases in rent formulas in order to increase the supply of units with good schools.

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 complement to these more-expensive policies.

References

Abdulkadiroglu, Atila, Parag A Pathak, Jonathan Schellenberg, and Christopher R Walters, "Do Parents Value School Effectiveness?," Working Paper 23912, National Bureau of Economic Research October 2017.

Ackerberg, Daniel A., "A new use of importance sampling to reduce computational burden in simulation estimation," *QME*, Dec 2009, *7* (4), 343–376.

Allende, Claudia, Francisco Gallego, and Christopher Neilson, "The Equilibrium Effects of Informed School Choice," Working Paper 2018.

Angrist, Joshua D., Parag A. Pathak, and Christopher R. Walters, "Explaining Charter School Effectiveness," *American Economic Journal: Applied Economics*, October 2013, *5* (4), 1–27.

Bayer, Patrick, Fernando Ferreira, and Robert McMillan, "A unified framework for measuring preferences for schools and neighborhoods," *Journal of political economy*, 2007, *115* (4), 588–638.

Bergman, Peter, Raj Chetty, Stefanie DeLuca, Nathaniel Hendren, Lawrence F Katz, and Christopher Palmer, "Creating Moves to Opportunity: Experimental Evidence on Barriers to Neighborhood Choice," Technical Report, National Bureau of Economic Research 2019.

Black, Sandra E., "Do Better Schools Matter? Parental Valuation of Elementary Education," *The Quarterly Journal of Economics*, 1999, *114* (2), 577–599.

Chetty, Raj, John N. Friedman, and Jonah E. Rockoff, "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood," *American Economic Review*, 2014, *104* (9), 2633–79.

_ , _ , Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan, "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star," *The Quarterly Journal of Economics*, 2011, *126* (4), 1593–1660.

_ , **Nathaniel Hendren, and Lawrence F. Katz**, "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment," *American Economic Review*, 2016, *106* (4), 855–902.

Chyn, Eric, "Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Children," *American Economic Review*, October 2018.

Clampet-Lundquist, Susan and Douglas S. Massey, "Neighborhood Effects on Economic Self Sufficiency: A Reconsideration of the Moving to Opportunity Experiment," *American Journal of Sociology 114.1: 107-143*, 2008.

Corcoran, Sean P, Jennifer L Jennings, Sarah R Cohodes, and Carolyn Sattin-Bajaj, "Leveling the Playing Field for High School Choice: Results from a Field Experiment of Informational Interventions," Working Paper 24471, National Bureau of Economic Research March 2018.

Deming, David J., Justine S. Hastings, Thomas J. Kane, and Douglas O. Staiger, "School Choice, School Quality, and Postsecondary Attainment," *The American Economic Review*, 2014, *104* (3), 991–1013.

Dobbie, Will and Roland G. Fryer, "The Medium-Term Impacts of High-Achieving Charter Schools," *Journal of Political Economy*, 2015, *123* (5), 985–1037.

Ellen, Ingrid Gould, Keren Mertens Horn, and Amy Ellen Schwartz, "Why don't housing choice voucher recipients live near better schools? Insights from Big Data," *Journal of Policy Analysis and Management*, 2016, *35* (4), 884–905.

Figlio, David N and Maurice E Lucas, "What's in a grade? School report cards and the housing market," *American Economic Review*, 2004, *94* (3), 591–604.

Gennetian, Lisa, Matthew Sciandra, Lisa Sanbonmatsu, Jens Ludwig, Lawrence Katz, Greg Duncan, Jeffrey Kling, and Ronald Kessler, "The Long-Term Effects of Moving to Opportunity on

Youth Outcomes," *Cityscape*, 2012, *14* (2).

Glazerman, Steven and Dallas Dotter, "Market Signals: Evidence on the Determinants and Consequences of School Choice From a Citywide Lottery," *Educational Evaluation and Policy Analysis*, 2017, *39* (4), 593–619.

Hastings, Justine, Thomas Kane, and Douglas Staiger, "Heterogeneous Preferences and the Efficacy of Public School Choice," Working Paper June 2009.

HUD, "HUD, Greatschools Team up to give public housing, voucher families tools to make informed school choices," 2011.

Jacob, Brian A., "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago," *American Economic Review, 94(1): 233-258*, 2004, *94* (1), 233–258.

_ , Jens Ludwig, and Max Kapustin, " The Impact of Housing Assistance on Child Outcomes: Evidence from a Randomized Housing Lottery *," *The Quarterly Journal of Economics*, 11 2014, *130* (1), 465–506.

Katz, Lawrence F., Jeffrey B. Liebman, and Jeffrey R. Kling, "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment," *Quarterly Journal of Economics*, 2001, *116* (2), 607–54.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz, "Experimental Analysis of Neighborhood Effects," *Econometrica 75 (1): 83-119*, 2007, *75* (1), 83–119.

Ludwig, Jens, Greg J. Duncan, Lisa A. Gennetian, Lawrence F. Katz, Ronald C. Kessler, Jeffrey R. Kling, and Lisa Sanbonmatsu, "Long-Term Neighborhood Effects on Low-Income Families: Evidence from Moving to Opportunity," *American Economic Review Papers and Proceedings 103(3): 226-31*, 2013.

Reardon, Sean F, "Educational Opportunity in Early and Middle Childhood: Variation by Place and Age," CEPA Working Paper 17-12, Center for Education Policy Analysis, Stanford University March

2018.

Sanbonmatsu, Lisa, Jens Ludwig, Lawrence F. Katz, Lisa A. Gennetian, Greg J. Duncan, Ronald C. Kessler, Emma Adam, Thomas McDade, and Stacy Tessler Lindau, *Moving to Opportunity for Fair Housing Demonstration Program: Final Impacts Evaluation*, Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research, 2011.

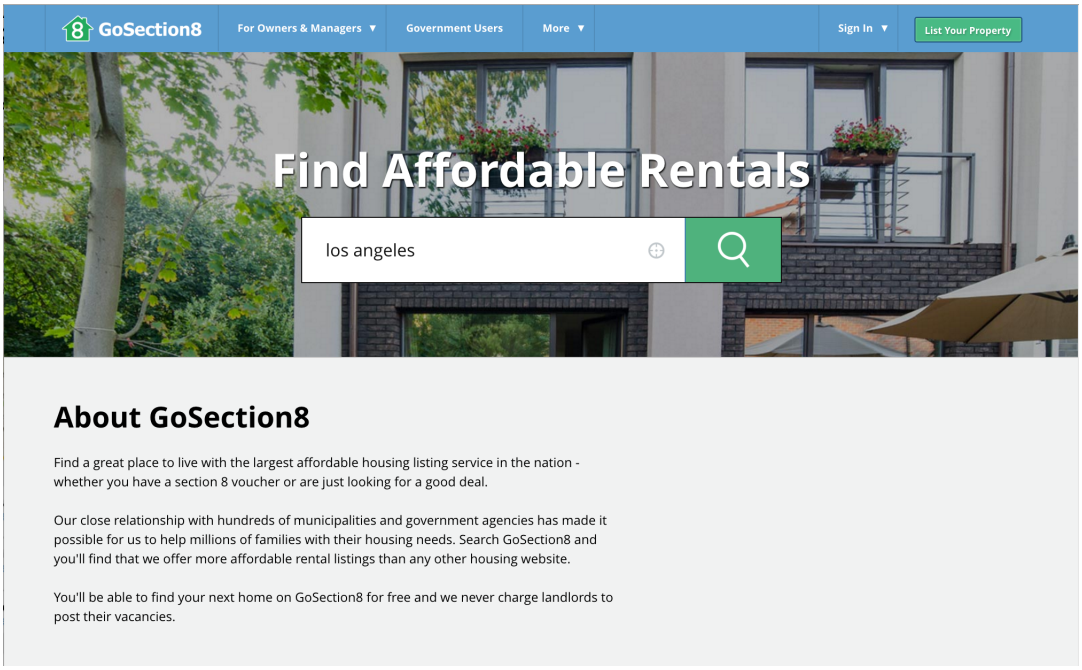
van Dijk, Winnie, "The Socio-Economic Consequences of Housing Assistance," Working Paper 2019.

Waldinger, Daniel, "Targeting In-Kind Transfers Through Market Design: A Revealed Preference Analysis of Public Housing Allocation," Working Paper 2018.

Weinstein, Jeffrey M. and Justine S. Hastings, "Information, School Choice, and Academic Achievement: Evidence from Two Experiments*," *The Quarterly Journal of Economics*, 11 2008, *123* (4), 1373–1414.

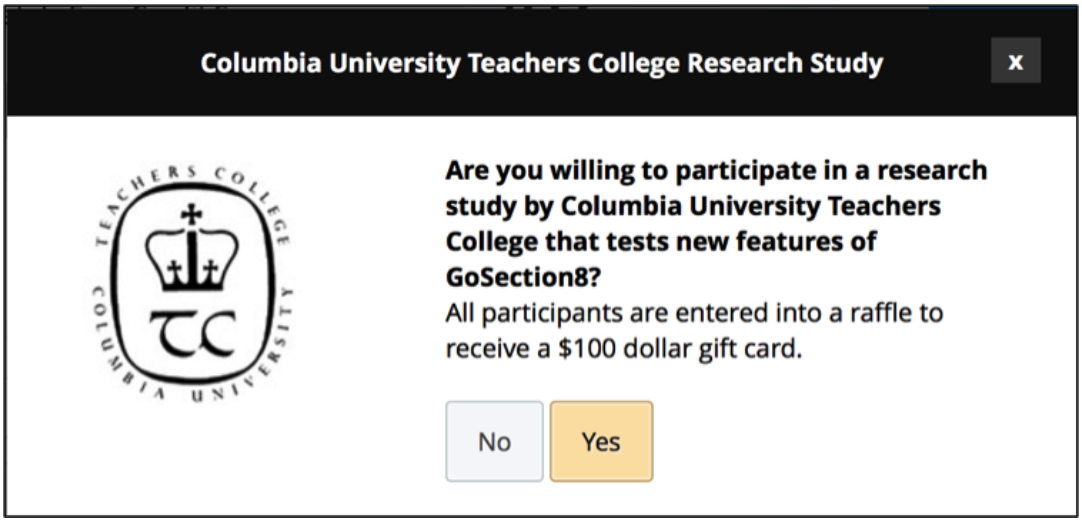
Figures

Figure 1: Landing page



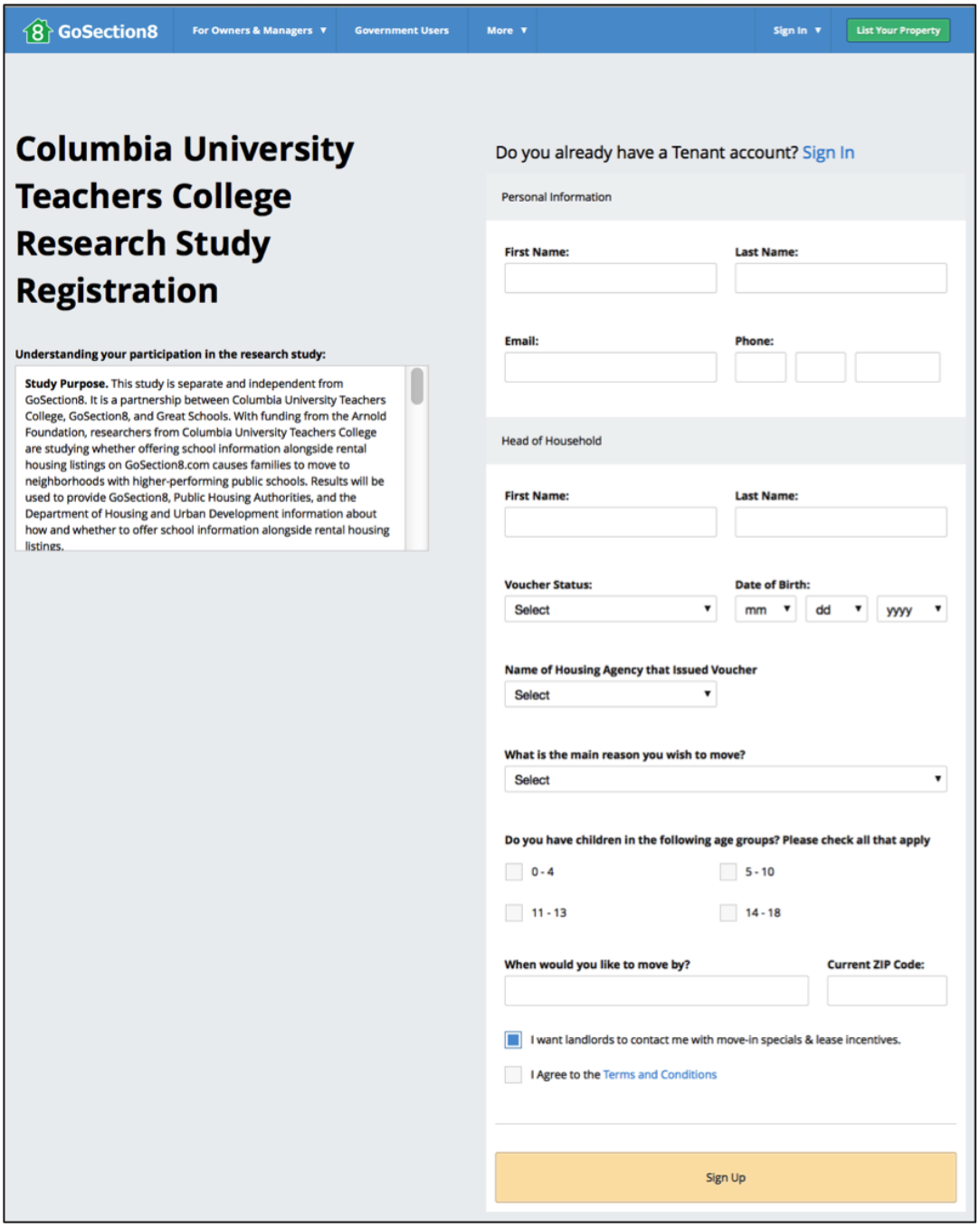
This figure shows the landing page on GoSection8.com, where users can search for rental properties by zip code, city, or county.

Figure 2: Consent pop up



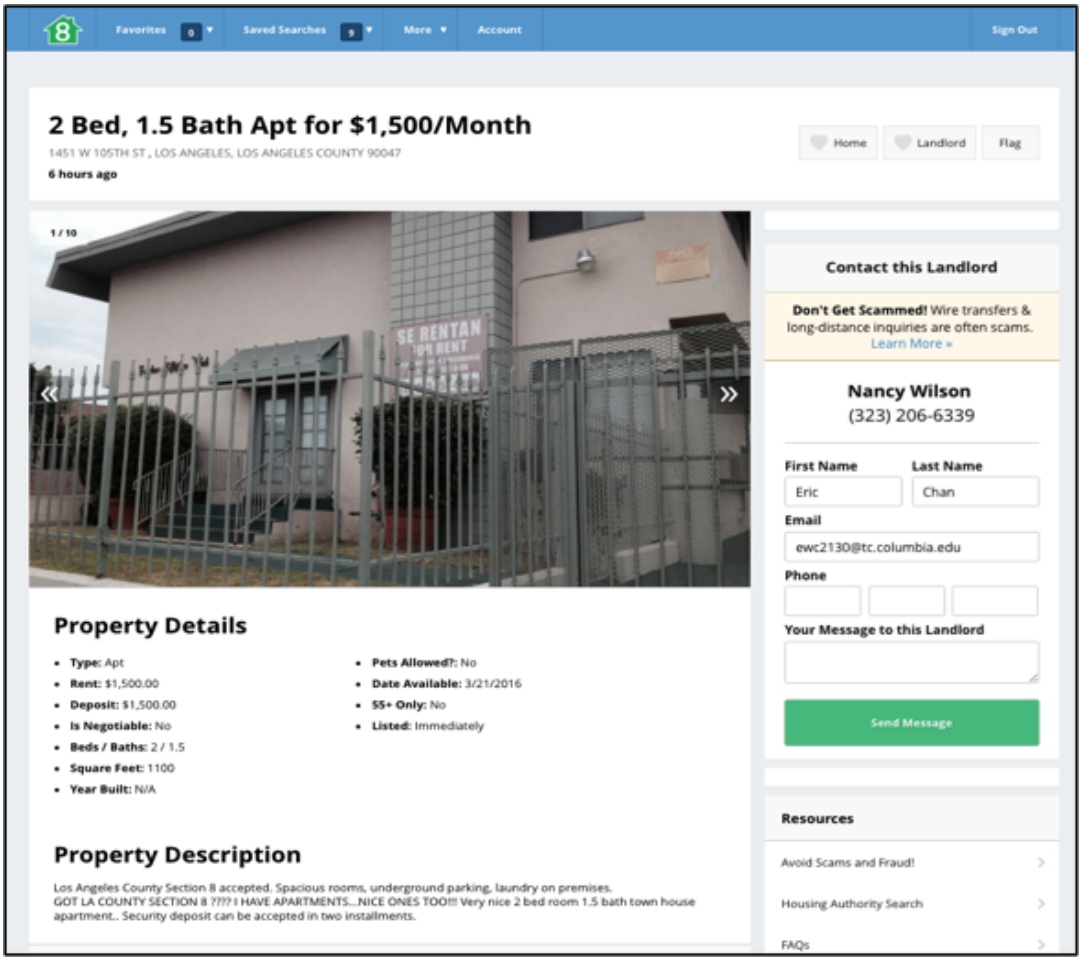
This figure shows the consent pop up when users first log into GoSection8.com.

Figure 3: Intake survey



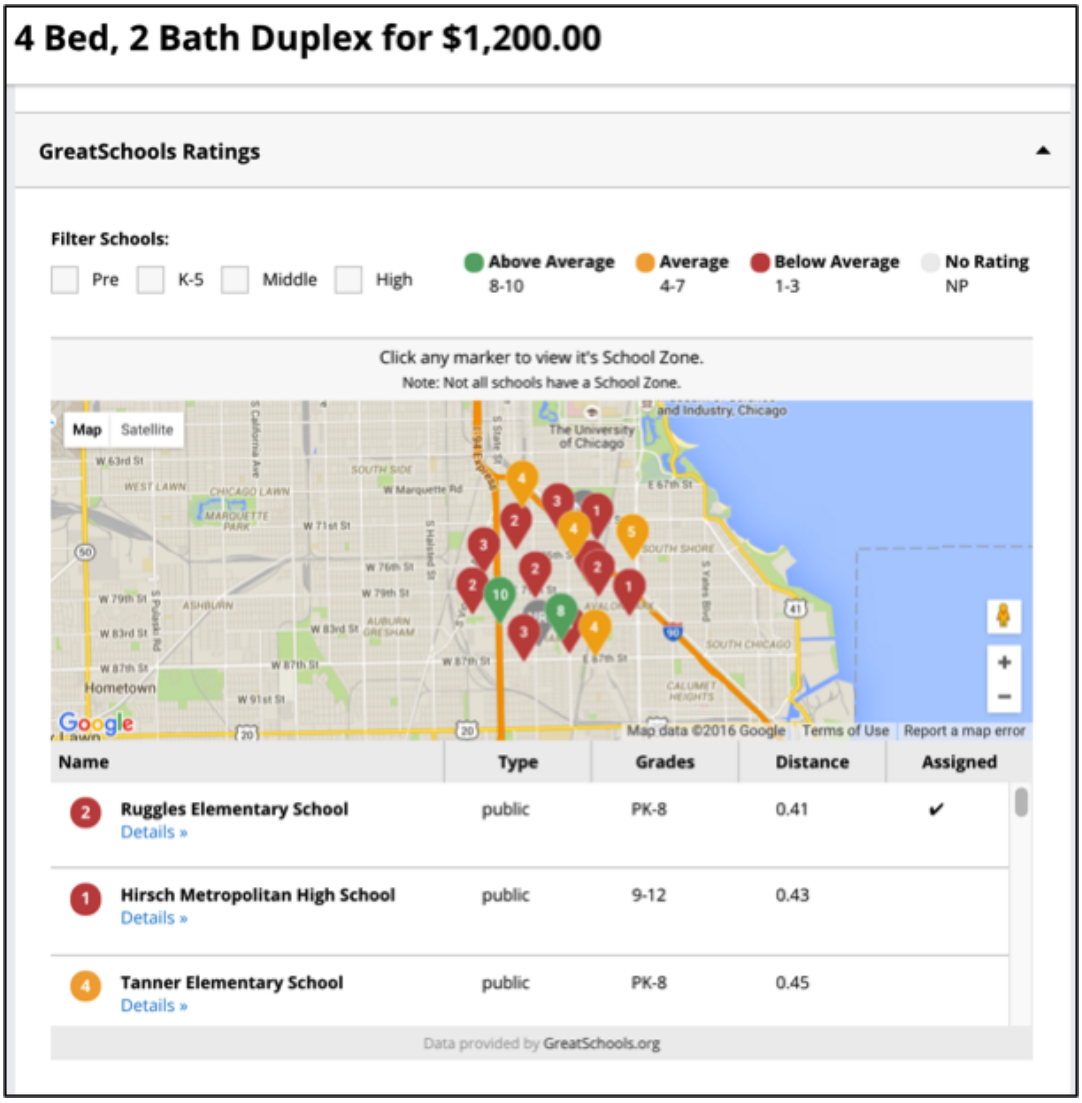
This figure shows the survey after users click 'yes' to the consent pop up on GoSection8.com.

Figure 4: Listing page



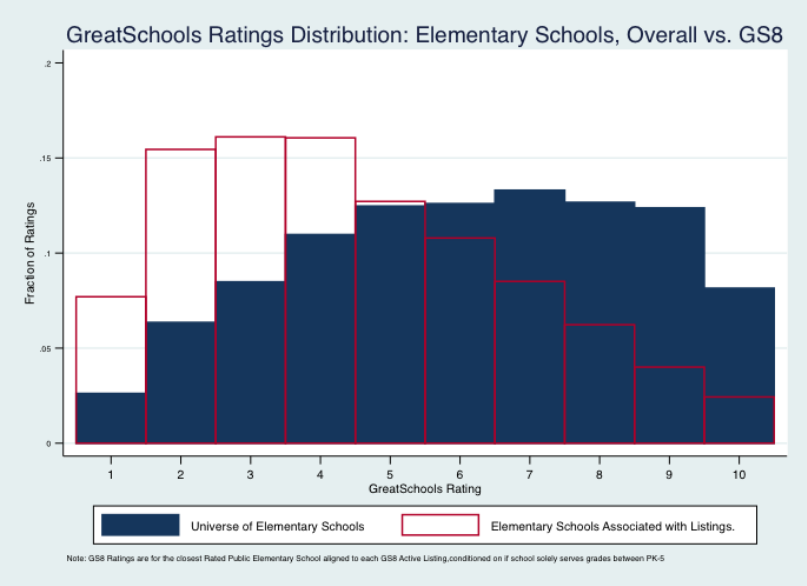
This figure shows the property listing page on GoSection8.com. The intervention module is directly underneath the shown area. See next figure.

Figure 5: Greatschools module



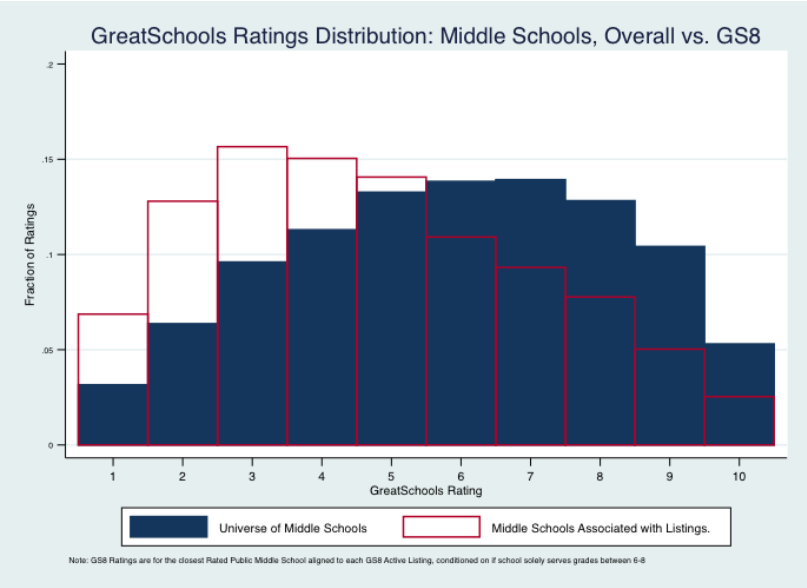
This figure shows the intervention module with school quality information on each of GoSection8's property listing page. It includes a map, the assigned (or closest school if no assigned schools) schools at each school level, and school quality information.

Figure 6: Distribution of GreatSchools Public School Ratings Nationally vs. GoSection8 Properties



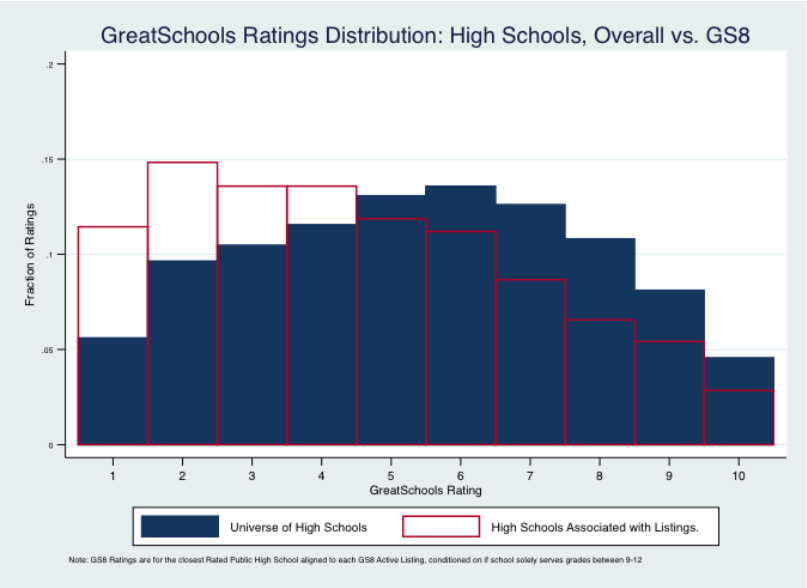
This figure compares the GreatSchools Public School Ratings for the universe of elementary schools vs. the elementary schools associated with the GoSection8 properties listed on its website. Data from GreatSchools and GoSection8.

Figure 7: Distribution of GreatSchools Public School Ratings Nationally vs. GoSection8 Properties



This figure compares the GreatSchools Public School Ratings for the universe of middle schools vs. the middle schools associated with the GoSection8 properties listed on its website. Data from GreatSchools and GoSection8.

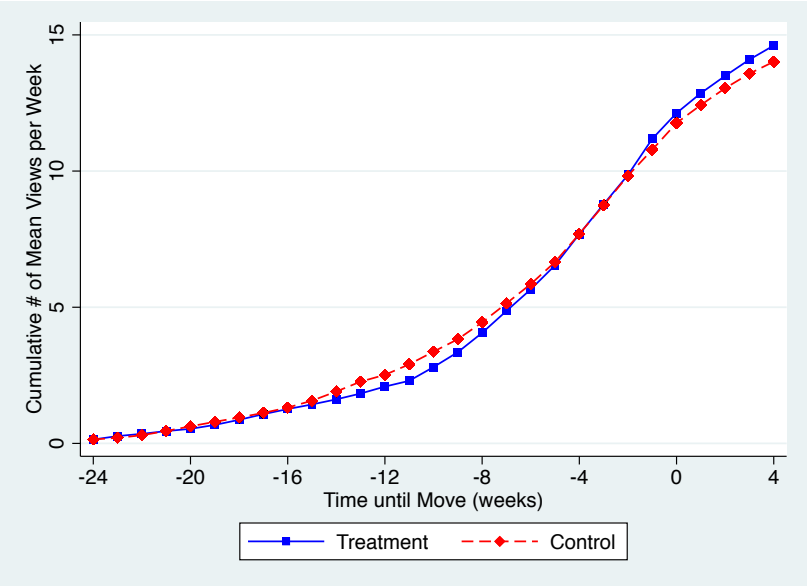
Figure 8: Distribution of GreatSchools Public School Ratings Nationally vs. GoSection8 Properties



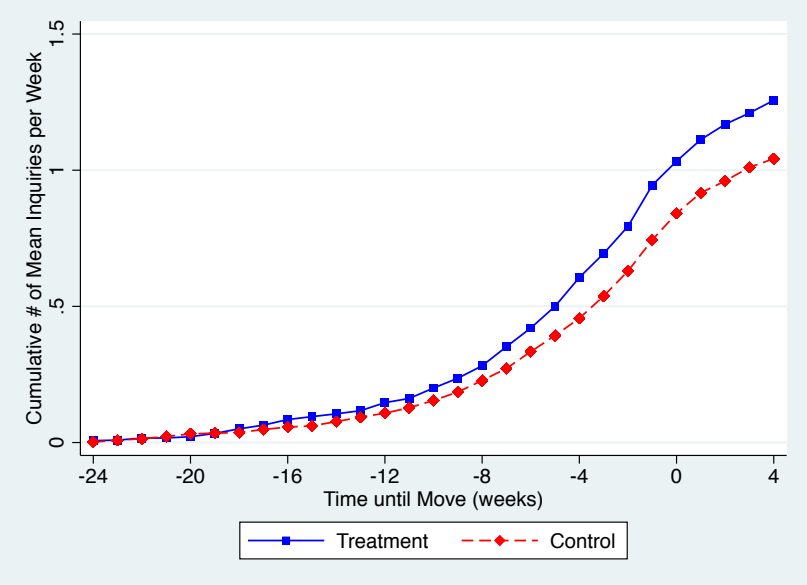
This figure compares the GreatSchools Public School Ratings for the universe of middle schools vs. the middle schools associated with the GoSection8 properties listed on its website. Data from GreatSchools and GoSection8.

Figure 9: Number of Views and Inquiries Made Relative to Move Date

(a) Cumulative views



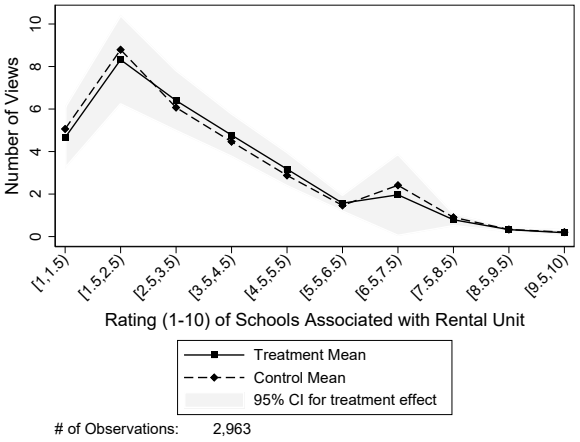
(b) Cumulative inquiries



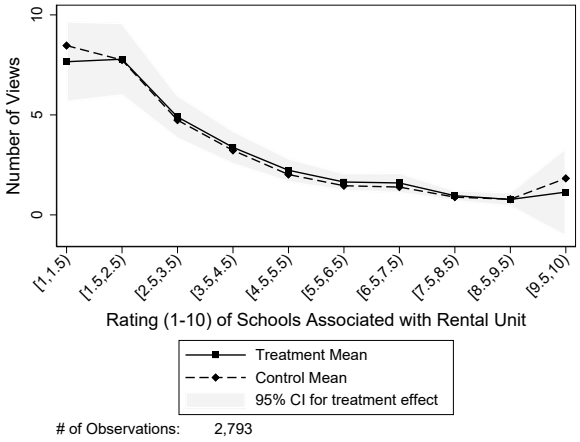
This figure compares the GreatSchools Public School Ratings for the universe of middle schools vs. the middle schools associated with the GoSection8 properties listed on its website. Data from GreatSchools and GoSection8.

Figure 10: Searches and Inquiries by School Quality

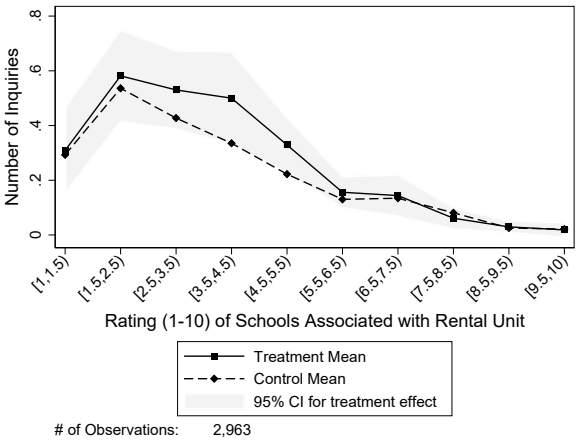
(a) Number of Views by Mean School Quality



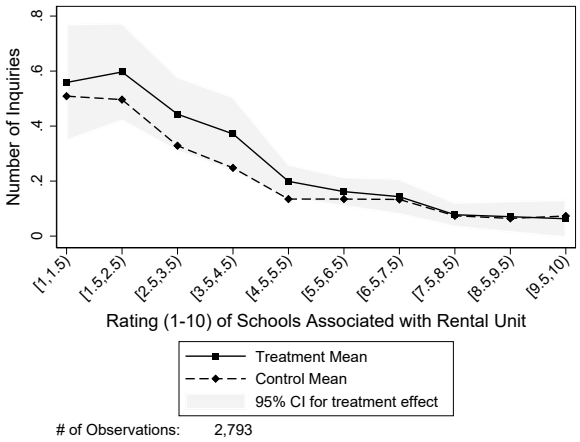
(b) Number of Views by "Next" School Quality



(c) Number of Inquiries by Mean School Quality



(d) Number of Inquiries by "Next" School Quality



These figures show the number of views and inquiries by mean school quality [(a) and (c)], and the number of view by next school quality. "Next" school is defined as the subsequent school level that a child would attend in the future. For example, if a family had a child in the age 5-10 range during which they are attending elementary school, their next school level would be mildde school.

Figure 11: Density of "Average" Ratings

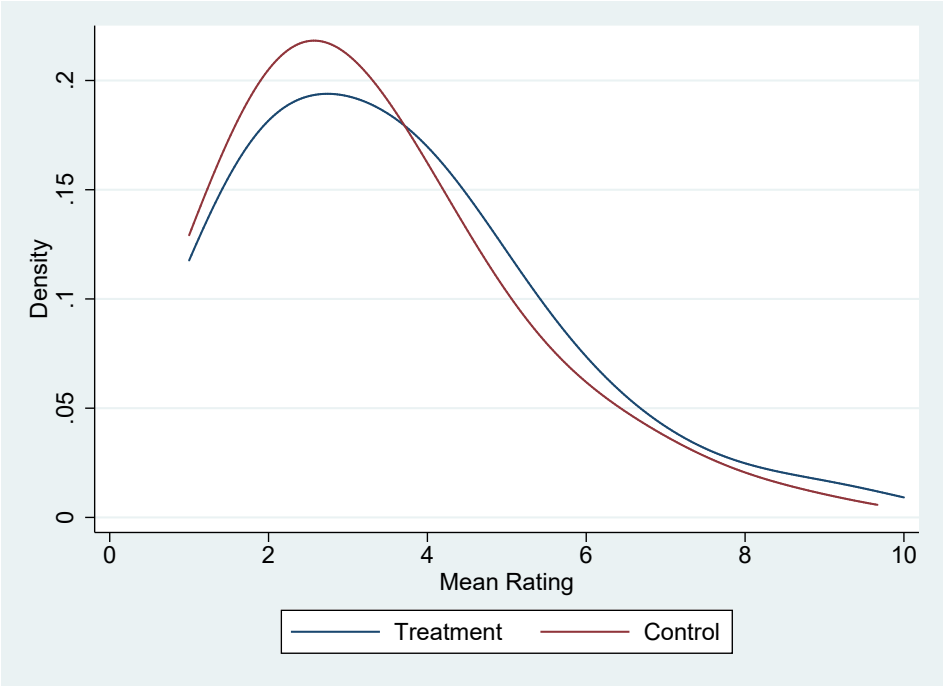


Figure 12: Poverty Density

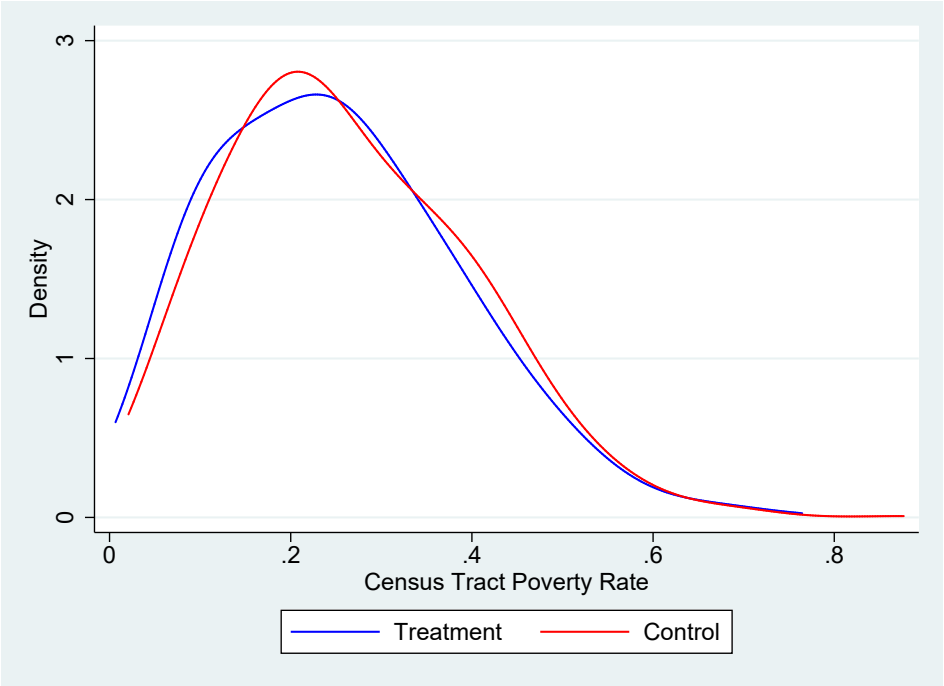


Figure 13: Basis functions for time-varying moments

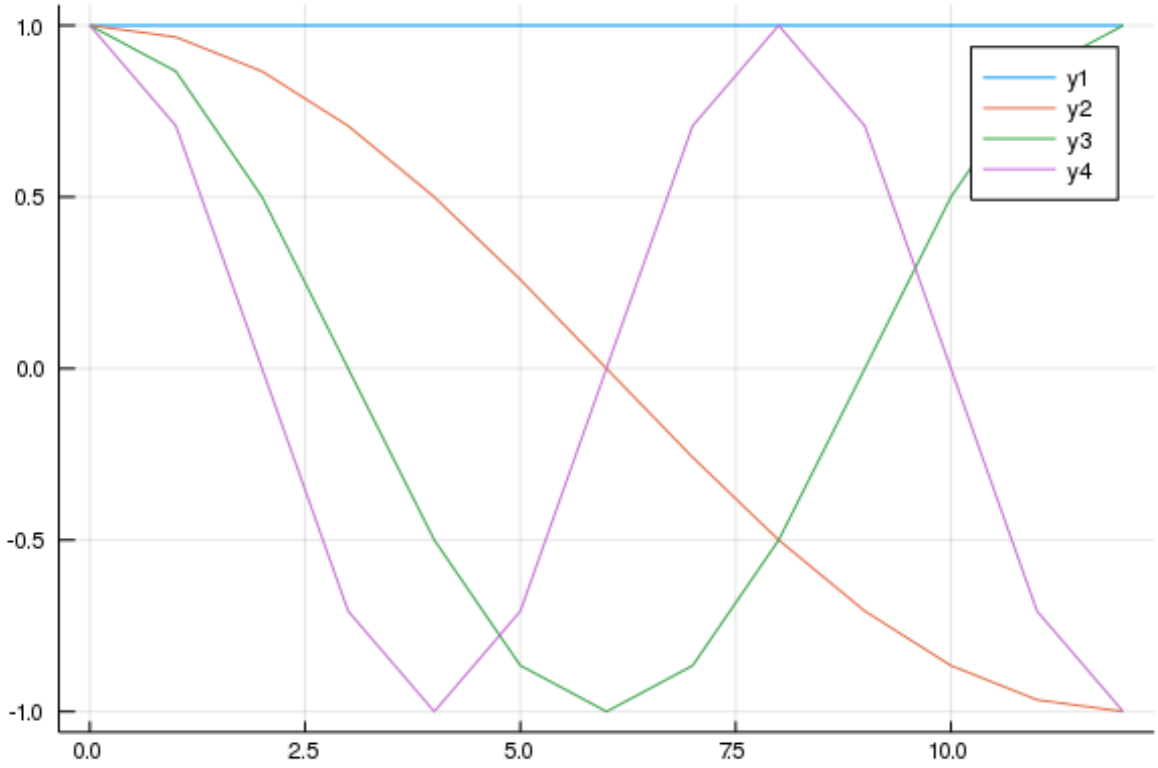


Figure 14: Kernel density of residuals $q - E(q|x)$

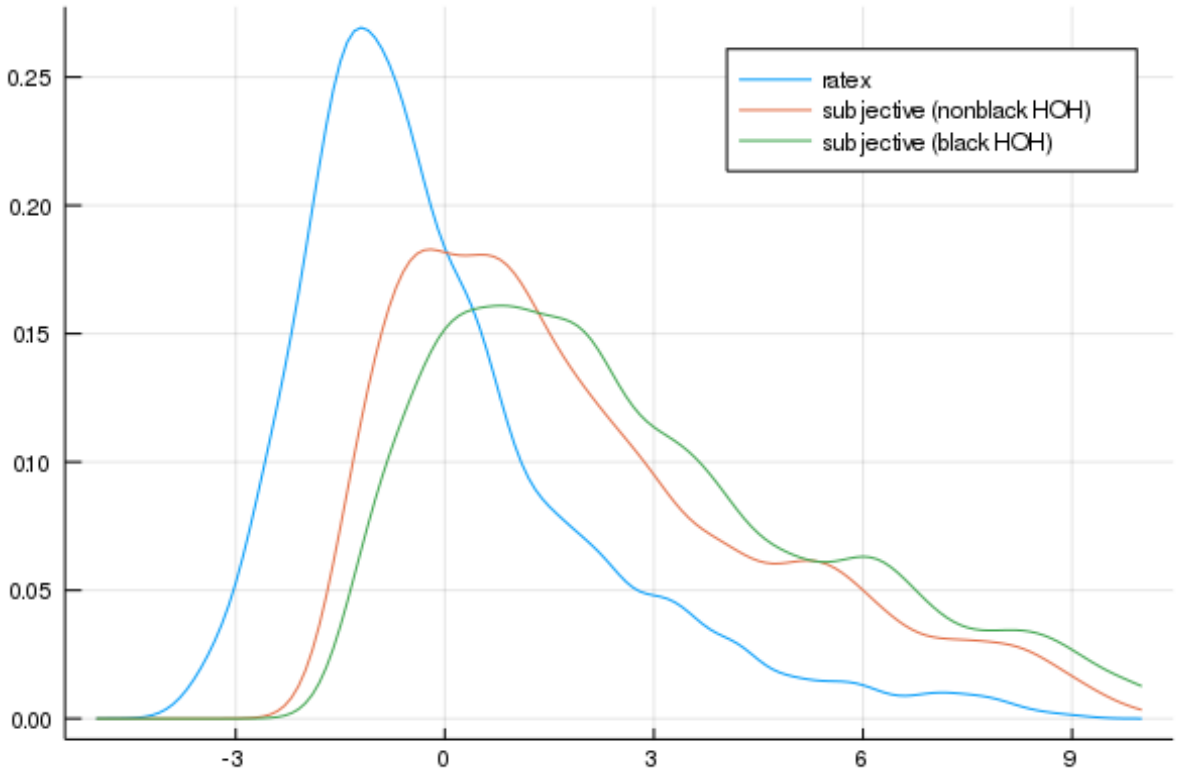
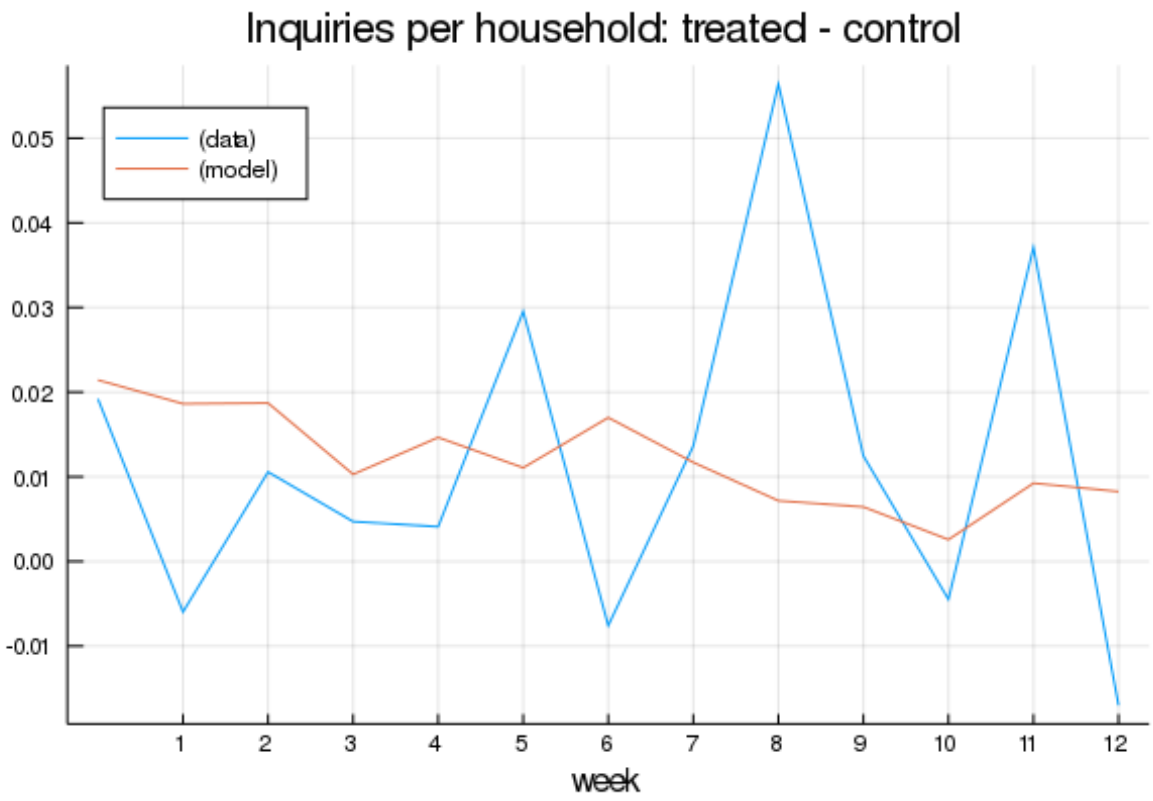


Figure 15: Model fit: treatment effect on inquiries per week



Tables

Table 1: Matched vs. Unmatched Sample Comparison

Variable	Unmatched Mean	Matched-Unmatched Difference	P-value	N
treatment group	0.51	−0.01	0.31	5,743
intend to move within 3 months	0.78	−0.11***	0.00	5,743
<u>children</u>				
has child indicator	0.93	−0.01	0.22	5,743
child 0-4 age 8	0.45	−0.13***	0.00	5,743
child 5-10 age	0.18	0.01	0.23	5,743
child 11-13 age	0.09	0.03***	0.00	5,743
listed child under 13	0.73	−0.09***	0.00	5,743
listed child over 13	0.23	0.09***	0.00	5,743
<u>move reason</u>				
marital status	0.10	−0.03***	0.00	5,743
job	0.08	0.02**	0.03	5,743
family	0.07	0.03***	0.00	5,743
transportation	0.12	0.05***	0.00	5,743
schools	0.36	−0.03**	0.02	5,743
apartment	0.06	−0.05***	0.00	5,743
safer	0.01	−0.00	0.45	5,743
evicted	0.13	0.00	0.69	5,743
inspection	0.06	−0.03***	0.00	5,743
other	0.01	0.03***	0.00	5,743

Notes: All data from baseline survey of GoSection8. The number of observations is the number of GoSection8 users who chose to participate in the study.

Table 2: 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 18 and under children in HH	1.84	−0.06	0.43	1,932
intend to move within 3 months	0.66	0.01	0.65	1,928
moving for schools	0.31	0.02	0.29	1,932
HUD user matched to go8 user	0.66	−0.01	0.49	2,968
address info N/A	0.07	−0.01	0.50	1,932
Omnibus Test P-value: 0.19				

Notes: All data from HUD, with the exception of intention to move within three months and moving for schools, which are derived from baseline survey data on GoSection8. The abbreviate "hh" stands for 'household'. *** p<0.01, ** p<0.05, * p<0.10

Table 3: Demographic Characteristics: HUD (5% Random Sample) and Matched Participant Sample Comparison

Variable	go8 (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 bedrooms	2.43	0.38***	0.00	347,544	1,954
total annual income	14634.54	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
tenant rent	269.00	−16.58	0.29	342,830	429
utility allowance	138.63	28.52***	0.00	347,548	1,954

Notes: All data from HUD. Data is 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 only those who matched with the HUD database of voucher holders. The abbreviate "hh" stands for "household." *** p<0.01, ** p<0.05, * p<0.10

Table 4: Neighborhood characteristics: Go8 Listings and Matched Participant Sample Comparison

Census Tracts of Go8 Listings vs. GoSection8 Matched Addresses Sample						
	Mean (go8)	Mean (matched)	Difference	p-value	obs (go8 listings)	obs (matched addr.)
Share White	0.56	0.49	0.07***	0.00	21,867	1,932
Share Black	0.28	0.34	-0.06***	0.00	21,867	1,932
Share Asian	0.04	0.05	-0.01***	0.00	21,867	1,932
Share Hispanic	0.25	0.24	0.01**	0.05	21,867	1,932
High School +	0.78	0.79	0.01*	0.09	21,867	1,932
Bachelors +	0.17	0.19	-0.02**	0.02	21,867	1,932
Poverty	0.27	0.26	0.01	0.21	21,865	1,932
Food Stamps	0.58	0.57	0.02*	0.06	21,785	1,932

This table shows the differences in means for neighborhood characteristics of census tracts associated with GoSection8 listings and participants' addresses who were matched to HUD data. The observations are based on number of unique addresses for each group. Note that census tracts may be counted multiple times as they are matched to addresses, so these can be interpreted as weighted means of characteristics. *** p<0.01, ** p<0.05, * p<0.10

Table 5: School Characteristic Means: A Comparison of US Elementary Schools, GoSection8 Property Listings, HUD sample, and Matched Study Sample

	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, 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 6: 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: All outcome data from GoSection8.
*** p<0.01, ** p<0.05, * p<0.10

Table 7: 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: All outcome data from GoSection8.
*** p<0.01, ** p<0.05, * p<0.10

Table 8: Mean "School Quality" Views and Inquiries

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Mean Quality Views (Next Rate)	3.53	−0.08	0.08	0.31	2,210
Mean Quality Inquiries (Next Rate)	3.51	0.02	0.13	0.87	1,011
Mean Quality Views (Mean Rate)	2.91	−0.12	0.08	0.12	2,793
Mean Quality Inquiries (Mean Rate)	3.58	0.08	0.11	0.48	1,038

Notes: All outcome data from GoSection8.
*** p<0.01, ** p<0.05, * p<0.10

Table 9: School Characteristics

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Average School Quality	3.65	0.29***	0.09	0.00	1,935
Maximum School Quality	4.82	0.34***	0.11	0.00	1,935
Minimum School Quality	2.64	0.20**	0.08	0.02	1,935
Next School Level Quality	3.65	0.31***	0.12	0.01	1,586
High School Rating	4.01	0.23**	0.11	0.04	1,833
Middle School Rating	3.51	0.40***	0.11	0.00	1,900
Primary School Rating	3.47	0.25**	0.11	0.02	1,877
Mean share FRPL	0.72	−0.02**	0.01	0.05	1,876
Fraction Black or Hispanic	0.66	−0.02*	0.01	0.08	1,876

Notes: All outcome data from HUD merged to school quality data.
*** p<0.01, ** p<0.05, * p<0.10

Table 10: School Characteristics by Subgroup

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Has Child 0-4					
Primary School Rating	3.26	0.35*	0.19	0.06	585
Middle School Rating	3.43	0.27	0.19	0.17	591
High School Rating	3.98	0.19	0.21	0.37	568
Has Child 5-10					
Primary School Rating	3.51	0.31	0.24	0.21	392
Middle School Rating	3.40	0.80***	0.25	0.00	389
High School Rating	3.84	0.48**	0.24	0.05	382
Has Child 11-13					
Primary School Rating	3.37	0.12	0.30	0.70	238
Middle School Rating	3.61	−0.15	0.30	0.62	244
High School Rating	3.93	−0.05	0.30	0.88	239
Has Child 14-18					
Primary School Rating	3.65	0.13	0.24	0.60	382
Middle School Rating	3.62	0.32	0.23	0.17	393
High School Rating	4.35	−0.03	0.25	0.90	373

Notes: All outcome data from HUD merged to school quality data.
*** p<0.01, ** p<0.05, * p<0.10

Table 11: Neighborhood Characteristics

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Percent Hispanic	0.23	0.00	0.01	0.79	1,903
Percent White	0.50	0.01	0.01	0.25	1,903
Percent Black	0.34	−0.02*	0.01	0.08	1,903
Percent Asian	0.04	0.00	0.00	0.30	1,903
Percent H.S. Graduates	0.79	0.00	0.00	0.35	1,903
Percent B.A. Graduates	0.19	0.00	0.01	0.49	1,903
Median Earnings	30666.44	996.03	913.29	0.28	383
Percent in Poverty	0.26	−0.01*	0.01	0.09	1,903
Share Poverty<10%	0.10	0.03**	0.01	0.02	1,903
Percent on SNAP	0.56	0.00	0.01	0.98	1,902
Walkscore	49.96	−3.19***	1.12	0.00	1,938
Commute to dwtn	15.35	1.86***	0.69	0.01	1,922

Notes: All outcome data from HUD merged to school quality data.
*** p<0.01, ** p<0.05, * p<0.10

Table 12: Unit Characteristics (HUD Data)

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
gross rent	1151.70	4.88	18.97	0.80	1,929
number bedrooms	2.47	−0.02	0.04	0.69	1,929
Beds per Household Member	0.90	−0.01	0.02	0.79	1,915

Notes: All outcome data from HUD.
*** p<0.01, ** p<0.05, * p<0.10

Table 13: Property Views, in HUD at baseline

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Total views made post treatment	30.43	6.93	4.37	0.11	1,673
Number of Different Tracts Views Made In	13.66	1.41	1.32	0.29	1,673
Number of Views Rated 1	4.56	0.28	0.94	0.76	1,673
Number of Views Rated 2	8.01	1.31	1.48	0.38	1,673
Number of Views Rated 3	6.10	1.40	1.09	0.20	1,673
Number of Views Rated 4	4.32	1.24*	0.76	0.10	1,673
Number of Views Rated 5	2.80	0.87	0.59	0.14	1,673
Number of Views Rated 6	1.41	0.30	0.26	0.26	1,673
Number of Views Rated 7	1.42	1.27**	0.56	0.02	1,673
Number of Views Rated 8	0.98	−0.16	0.19	0.40	1,673
Number of Views Rated 9	0.37	0.01	0.08	0.95	1,673
Number of Views Rated 10	0.11	0.14**	0.06	0.03	1,673

Notes: All outcome data from GoSection8.
*** p<0.01, ** p<0.05, * p<0.10

Table 14: Property Inquiries, in HUD at baseline

Variable	Control Mean	Treatment Effect	Std Error	P-value	N
Total inquiries made post treatment	2.07	0.91**	0.46	0.05	1,673
Number of Different Tracts Inquiries Made In	1.51	0.39*	0.24	0.10	1,673
Number of Inquiries Rated 1	0.23	0.07	0.12	0.52	1,673
Number of Inquiries Rated 2	0.49	0.11	0.12	0.35	1,673
Number of Inquiries Rated 3	0.43	0.15	0.11	0.16	1,673
Number of Inquiries Rated 4	0.31	0.30**	0.13	0.03	1,673
Number of Inquiries Rated 5	0.22	0.17**	0.08	0.03	1,673
Number of Inquiries Rated 6	0.13	0.06	0.05	0.18	1,673
Number of Inquiries Rated 7	0.10	0.07	0.05	0.15	1,673
Number of Inquiries Rated 8	0.09	−0.03	0.03	0.24	1,673
Number of Inquiries Rated 9	0.02	−0.00	0.01	0.89	1,673
Number of Inquiries Rated 10	0.01	0.01	0.01	0.20	1,673

Notes: All outcome data from GoSection8.
*** p<0.01, ** p<0.05, * p<0.10

Table 15: Parameter Estimates

Variable	Estimate	SE
α	0.0293	(0.004)
β(constant)	4.3817	(67.7512)
β(poverty)	-2.8868	(3.085)
β(black)	4.5288	(1.5795)
β(walkscore)	-1.921	(1.965)
β(commute)	-2.3156	(2.5986)
β(quality)	2.2016	(0.1522)
μ_{search}	5.0776	(2.3668)
σ_c(search)	4.0001	(2.1146)
σ_c(move)	40.0568	(325.5887)
λ_0	2.7016	(1.3907)
λ_t	-0.0019	(0.2202)
σ_η(control)	5.2449	(1.611)
γ(constant)	-0.0729	(3.1365)
γ(poverty)	0.9465	(2.9249)
γ(black)	1.275	(1.9899)
γ(walkscore)	1.3991	(1.6732)
γ(commute)	-0.5003	(2.3854)
γ(constant $*$ 1(HOH black))	-1.0033	(4.9517)
γ(black $*$ 1(HOH black))	0.8184	(1.8674)
σ_η(treated $*$ off-platform)	1.3789	(0.8439)
Pr(inquiry observed)	0.1329	(0.0203)
β(constant $*$ 1(HOH black))	0.6659	(46.4588)
β(black $*$ 1(HOH black))	-2.931	(1.7345)

Table 16: Subjective Beliefs vs Rational Expectations

	Next School Quality		
	(RatEx)	(Subjective (white HOH))	(Subjective (black HOH))
γ(constant)	5.5592	−0.0729	−1.0761
	(0.0509)	(3.1365)	(5.089)
γ(poverty)	−4.0368	0.9465	
	(0.1197)	(2.9249)	
γ(black)	−1.9442	1.275	2.0934
	(0.0487)	(1.9899)	(2.0241)
γ(walkscore)	−0.4437	1.3991	
	(0.0638)	(1.6732)	
γ(commute)	−0.2246	−0.5003	
	(0.0731)	(2.3854)	
resid. variance	4.6647	4.6647	
$\sigma_\eta^2/(\sigma_\eta^2+\sigma_q^2)$		0.145	

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

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

Wald test $\gamma_{ols} == \gamma$: $\chi^2 = 30.6947$, $p = 0.0$

Wald test $\gamma_{ols} == \gamma_{\text{black}}$: $\chi^2 = 10.3299$, $p = 0.0664$

Table 17: preferences in equivalent full info model

	"as if full information" preferences			
	(treated, black)	(control, black)	(treated, nonblack)	(control, nonblack)
β(constant)	5.0476	1.5496	4.3817	2.1766
	(56.4527)	(29.2567)	(65.7605)	(35.1172)
β(poverty)	−2.8868	−0.5666	−2.8868	−0.5666
	(3.0762)	(2.9033)	(3.2896)	(2.6121)
β(black)	1.5979	2.8401	4.5288	3.5531
	(1.8335)	(2.0673)	(1.5859)	(2.1251)
β(walkscore)	−1.921	0.3654	−1.921	0.3654
	(1.9106)	(1.3489)	(2.028)	(1.3749)
β(commute)	−2.3156	−1.6703	−2.3156	−1.6703
	(2.741)	(1.8939)	(2.6159)	(1.8421)
β(quality)	2.2016	0.1637	2.2016	0.1637
	(0.1538)	(0.0986)	(0.1578)	(0.087)

Table 18: Counterfactual: fix $\gamma = \gamma_{OLS}$

	(Data)	(Model)	(Counterfactual)
inquiries	0.1541	0.1571	0.4059
	(0.1795)	(0.051)	(0.1843)
endline quality | move	0.3466	0.4598	−0.1413
	(0.164)	(0.0885)	(0.2191)