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# THE DAMAGES AND DISTORTIONS FROM DISCRIMINATION IN THE RENTAL HOUSING MARKET

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**ABSTRACT**

By constraining an individual's choice during a search, housing discrimination distorts sorting decisions away from true preferences and results in a ceteris paribus reduction in welfare. This study combines a large-scale field experiment with a residential sorting model to derive utility-theoretic measures of renter welfare loss associated with the constraints imposed by discrimination in the rental housing market. Results from experiments conducted in five cities show that key neighborhood amenities are associated with higher levels of discrimination. Counterfactual simulations based on the sorting model suggest that discrimination imposes damages equivalent to 4.4% and 3.5% of the annual incomes for African American and Hispanic/ LatinX renters, respectively. Damages are increasing in income for African American renters, such that impacts become stronger for economically mobile households. Renters of color must make substantial investments in additional search to mitigate the costs of these constraints. We find that a naive model ignoring discrimination constraints yields biased estimates of willingness to pay for key neighborhood amenities.

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# 1 Introduction

By constraining an individual's choice during a search process, discrimination can distort decisions away from true preferences and result in a *ceteris paribus* reduction in the welfare of those that face it. Decades after the passage of the Fair Housing Act, evidence from both audit and correspondence studies indicates that housing market discrimination continues to constrain the choices of people of color in the United States and steer them into neighborhoods that confer disadvantage (Christensen and Timmins, 2022, Ewens, Tomlin, and Wang, 2014, Carlsson and Eriksson, 2014, Hanson and Hawley, 2011, Ahmed and Hammarstedt, 2008). However, inferences about the impacts of experimentally identified constraints are limited by the fact that researchers never observe how search constraints ultimately bind on the decisions of fictitious buyers and renters (Neumark, 2018, Guryan and Charles, 2013, Heckman, 1998).

The present study combines a large-scale field experiment with structural methods to examine the effects of discrimination in the housing market. A key innovation involves the integration of a correspondence experiment in a welfare-theoretic framework, which we use to estimate damages to African American and Hispanic/LatinX renters. The approach is motivated by the basic insight that the damages from discriminatory constraints depend on the value that an individual places on the set of choices that are made inaccessible as a consequence of discriminatory behavior. Since discrimination may differentially constrain access to choices with certain attributes or in certain neighborhoods, an estimate of the welfare cost to a renter or buyer requires understanding how households value the different attributes of each housing choice.

We develop a correspondence research design to measure within-property variation in housing choices that are made available (or not) by property managers in response to renter inquiries. Our experimental sample is obtained through interactions on a major online search platform and includes the entire set of listings for three-bedroom, two-bathroom rental units in each of five different major U.S. metropolitan housing markets:

Atlanta, GA; Houston, TX; Philadelphia, PA; Cleveland, OH; and San Jose, CA.[1] While online housing markets do not include all options available in the markets that we study, they have increasingly become the locus of rental housing search behavior and constitute an important channel for discriminatory behavior. A survey of rental search behavior reported that 72% of housing searches were initiated on online platforms in 2015 (Apartments.com, 2015). Reduced-form tests reveal that minority identities in our sample have an 8.3% lower likelihood of response indicating that a rental property is available for rent. Discriminatory constraints vary substantially by race group and across MSAs, with the lowest relative response rate for minority renters, defined as the ratio between the mean response rates for minority and white identities, found in Philadelphia (86.8%) and the highest found in Houston (95.0%).

Our reduced-form tests present four additional facts that are consistent with the patterns of damages revealed by the structural model. First, discriminatory constraints are positively correlated with neighborhood amenity levels. This result is consistent with recent findings on discriminatory steering by real estate agents in the buyer market, contributing to a mounting body of evidence across search settings and geographies that reveals that African American households face strong frictions when searching for housing in high-amenity neighborhoods (Christensen and Timmins, 2022).[2] In the rental markets that we study, discriminatory constraints are particularly strong for properties with access to higher school quality and lower toxic air concentrations, but the pattern holds for neighborhood-level murder rates and access to cafes, which proxy for a range of retail amenities (Glaeser, Kim, and Luca, 2018, Papachristos et al., 2011).

Second, we find that renters of color face less discrimination in neighborhoods with higher shares of African American or Hispanic/LatinX households, while they face greater discrimination in neighborhoods with higher shares of white households. This fact is con-

[1]Specifically, markets are defined using the Metropolitan Statistical Area definition and are sampled from the list of 28 metro areas used in recent paired-actor research by HUD/Urban Institute (Turner et al., 2013). Metropolitan Statistical Areas: Atlanta-Sandy Springs-Roswell, GA; Houston-The Woodlands-Sugar Land, TX; Philadelphia-Camden-Wilmington PA-NJ-DE-MD; Cleveland-Elyria, OH; and San Jose-Sunnyvale-Santa Clara, CA.

[2]Unlike discriminatory steering by real estate agents in the buyer market, however, choice constraints in the online rental market are mediated by direct interactions with property managers and cannot be circumvented by shifting to a different agent.

sistent with prior correspondence literature and suggests that the damages of discriminatory constraints may be somewhat lower for renters with strong homophily preferences. Third, discriminatory constraints are significantly higher among properties that have recently entered the market (listed for fewer than 3 days), indicating that constraints are stronger in neighborhoods with strong rental demand. Finally, discriminatory constraints facing minority identities become significantly stronger when a property manager receives inquiries from other identities in our sample, suggesting that discriminatory behavior in tight markets is exacerbated by competition among renters.

While the experimental results demonstrate reduced access to high amenity neighborhoods, which can be used to infer the sign of disparate impacts under reasonable conditions, the magnitude of damages depends on how they interact with the preferences and incomes of prospective renters, which are not observed in an audit experiment (Christensen and Timmins, 2022). This has been a key limitation in prior research on the impacts of housing discrimination (Yinger, 1995, Heckman, 1998). Building on recent applications of the consideration sets method (Abaluck and Adams, 2016, Gaynor, Propper, and Seiler, 2016) and a growing literature that combines experimental and structural approaches (Galiani, Murphy, and Pantano, 2015, Todd and Wolpin, 2020, Ferreira and Wong, 2020), we introduce a structural sorting model that uses experimentally identified variation in discriminatory behavior at the level of the census tract in combination with data from InfoUSA's Residential Historical Database on the location decisions of renting households in the five MSAs between 2016-2018.[3] Subject to the standard assumptions required in the sorting literature, our model recovers estimates of utility parameters that are statistically different in economically important ways from the estimates recovered from a naive model that ignores discriminatory constraints.

We use the estimated utility function to generate estimates of the impact of discriminatory search constraints on minority renters. For the average renter in our five cities,

[3]InfoUSA's Residential Historical Database tracks 120 million households, including renters, between 2006 and 2019. Data are compiled using 29 billion records from 100 sources including census statistics, billing statements, telephone directory listings, mail order purchases and magazine subscriptions. Data include information about gender, ethnicity, age, address, renter/owner status and estimated household income.

the estimated damages from discriminatory constraints – i.e., the constraints faced by a renter of color over and above those faced by a white renter with the same income – are equivalent to 4.4% and 3.5% of the annual incomes for African American and Hispanic/LatinX renters, respectively.[4] For African American renters, damage estimates increase substantially at higher levels of income – African American renters face damages of approximately 7% of income at income levels above $100,000 per year. This heterogeneity across the income distribution results from two interacting factors: (1) stronger discriminatory constraints in high amenity and high-price neighborhoods (clearly illustrated in our reduced form results) and (2) higher marginal utility from those amenities at higher levels of income. The first factor results in pronounced impacts on African American renters.

Discriminatory behavior has been shown to introduce group-specific search frictions in a wide variety of settings, including in the labor market (Kline, Rose, and Walters, 2022, Kline and Walters, 2021, Lang and Lehmann, 2012), housing and mortgage markets (Christensen, Sarmiento-Barbieri, and Timmins, 2022, Ouazad and Rancière, 2019, Hanson et al., 2016), and consumer markets (Edelman, Luca, and Svirsky, 2017, List, 2004). A recent correspondence experiment conducted across the 50 largest cities in the U.S. reveals that racial/ethnic discriminatory constraints affect the choices of African American and LatinX renters across all regions and documents a strong correlation between discrimination and the Black-white intergenerational income mobility gap at the city level (Christensen, Sarmiento, and Timmins, 2021).[5]

A mostly separate literature on the mechanisms underlying neighborhood effects has emphasized the role of housing search constraints in impeding moves to high-opportunity neighborhoods (Bergman et al., 2019, Aliprantis, Carroll, and Young, 2018, Ioannides, 2011). By directly affecting residential location decisions, housing discrimination could create a potential barrier to intergenerational income mobility (Chetty et al., 2018, Gra-

[4]Our damages estimates are necessarily subject to assumptions and limitations described in Section 5 and are not designed to capture impacts on property managers.

[5]The five MSAs studied in the current paper pertain to the set of the fifty largest MSAs in the U.S. and are also sampled as part of Christensen, Sarmiento, and Timmins (2021). However, the experiments were conducted independently, using distinct sampling designs and at different points in time.

ham, 2018). The present study uses a correspondence experiment to examine patterns of discriminatory constraints across neighborhoods, providing evidence that discrimination directly restricts access to the very neighborhoods that provide the greatest utility to economically mobile minority households. We use the structural model to examine the detrimental impacts of discriminatory behavior on renter welfare and describe the extent to which increased search activity can be used to offset these effects. Our results suggest that at moderate levels of search activity, renters of color need to expend approximately 10-30% more effort on search than their white counterparts to achieve the same level of utility.

The final section of the paper contributes to a long-standing literature on the relationship between preferences revealed by sorting in the housing market and the efficient allocation of local public goods (Epple, Filimon, and Romer, 1984, Tiebout, 1956). By constraining an individual's choice set, discrimination can drive a wedge between true amenity preferences and those revealed in a (constrained) search. We examine the effects of market distortions introduced by discrimination in the markets that we study. Allowing for racial heterogeneity in preferences, we find that a naive model that ignores constraints imposed by discrimination significantly understates African American and Hispanic/LatinX willingness-to-pay for retail amenities, proxied by nearby cafes. Although not statistically significant, point estimates suggest similar biases for the other neighborhood attributes that we study. This suggests that the discriminatory constraints can distort revealed preferences in ways that have important effects on the interpretation of revealed preference estimates and on the resulting provision of local public goods (Kuminoff, Smith, and Timmins, 2013, Bayer, Ferreira, and McMillan, 2007).

The paper proceeds as follows. Section 2 summarizes a number of relevant literatures on housing discrimination and location choice. Section 3 describes the correspondence research design and reports reduced-form experimental estimates. In Section 4, we develop a structural model of housing search and reports findings. Section 5 discusses study limitations, and Section 6 concludes.

## 2 The Impacts of Discriminatory Constraints

Mounting experimental evidence indicates that racial discrimination continues to constrain the choice sets of people of color during search in the rental or owner-occupied housing markets in the United States (Christensen, Sarmiento-Barbieri, and Timmins, 2022, Ewens, Tomlin, and Wang, 2014, Carlsson and Eriksson, 2014, Hanson and Hawley, 2011, Ahmed and Hammarstedt, 2008). Interactions between a household's search parameters and the discriminatory behavior that they face in any given market are critical for understanding when and how discriminatory constraints will bind, since this is the margin where households ultimately make rental or purchase decisions (Heckman, 1998). By constraining an individual's set of choices during a search, housing discrimination can distort the sorting outcome away from that associated with true preferences and result in a *ceteris paribus* reduction in welfare. Christensen and Timmins (2022) advance a framework that defines the conditions under which evidence from audit/correspondence research designs can reveal disparate impact and provide reduced form evidence that steering behavior in the buyer market constrains location decisions of buyers of color in a number of critical dimensions.

Reduced-form evidence from audit/correspondence experiments does not, by itself, provide a basis for quantifying the magnitude of damages from discriminatory constraints or understanding impacts on other equilibrium outcomes. To address this limitation, the present paper integrates the audit/correspondence framework within a residential choice model by formalizing the idea that a key outcome of discriminatory behavior in the rental housing market is a choice set constraint – an ex ante restriction on the housing choices in sets considered by renters. We estimate the impacts of constraints on renter choice sets using the consideration sets framework, which originally emerged to capture cognitive or other internal limits in a choice process (Caplin, Dean, and Leahy, 2019, Manzini and Mariotti, 2014, Masatlioglu, Nakajima, and Ozbay, 2012, Eliaz and Spiegler, 2011), but applies to a broad class of settings involving internally or externally imposed constraints on a choice set.

To identify the effects of constraints, empirical studies typically introduce an instrument that plausibly affects consumers' attention to different products without affecting the utility generated from consuming it (Goeree, 2008, Moraga-González, Sándor, and Wildenbeest, 2015, Koulayev, 2009) or an auxiliary data source that defines the actual choice sets, such as marketing surveys. Kennan and Walker (2011) apply the method to study migration behavior in the U.S. housing market, utilizing location histories of households as a measure of place-specific knowledge about the local labor market. Abaluck and Adams-Prassl (2021) advance an "alternative specific consideration model" (ASC), where constraints on the choice sets are identified from asymmetries in cross-price derivatives from a discrete choice demand system. Gaynor, Propper, and Seiler (2016) examine changes in the elasticity of demand with respect to the quality of health care in the wake of a reform that exogenously expanded patient choice sets.

Similar to the choice sets in Gaynor, Propper, and Seiler (2016), the choice set constraints that result from discriminatory behavior are directly measurable and are exogenous to buyer attention processes. Unlike most prior applications, identifying variation in our consideration sets model comes from a field experiment in the specific market that we study. In particular, the correspondence research design provides experimentally-identified differences in the probability that choices will be made available to renters corresponding to different race groups. Using data on renter choices made in the context of these choice set constraints, the discrete choice model identifies preference parameters that are conditional on observed levels of discriminatory behavior.

In Section 4.1, we define a consideration sets estimator that identifies preference parameters using differences in the probability that choices will be made available to renters in different race groups that are experimentally identified from a correspondence study. In particular, we explain how predicted response probabilities are generated for each census tract from our experimental data, and how those predicted probabilities are used to construct random consideration sets that are then used to estimate our model. This framework provides a general approach for integrating the results from audit/correspondence research to search and discrete choice models in a variety of settings.

# 3 Correspondence Experiment

## 3.1 Sampling Frame and Data Collection

Our study was executed using a bot designed to collect comprehensive, real-time data on rental housing listings on a major online realty platform while sending inquiries from racially distinct renter identities.[6] The bot compiled information for all 3 bedroom, 2 bathroom rental listings that appeared in five major Metropolitan Statistical Areas: Atlanta-Sandy Springs-Roswell, GA, Houston-The Woodlands-Sugar Land, TX, Philadelphia-Camden-Wilmington PA-NJ-DE, San Jose-Sunnyvale-Santa Clara CA, and Cleveland-Elyria OH. The cities were selected to capture a set of markets across different US regions having either a high representation of African American (Atlanta, Houston, Philadelphia, Cleveland) and/or a high representation of Hispanic/LatinX (Houston, San Jose) households. The sampling design ensures that estimates reflect differences across the full set of housing options advertised to prospective renters at the time of an experimental trial, simulating the set of options available to a prospective renter that is searching on the platform at that time.

We focus on this market segment as one that corresponds to the choices of renter families who are considering key neighborhood amenities such as pollution exposures (Currie et al., 2015) and school quality (Bayer, Ferreira, and McMillan, 2007). In addition to housing features such as monthly rent, square footage, house type, bedrooms, bathrooms, the bot collected neighborhood characteristics that are visible to renters on the search platform: average school quality (elementary, middle and high school), the number of local cafes, and an index of the number of recent murders.[7]

The bot also collected data on the ambient concentrations of chemical toxic pollution at the location of each listing as reported by the EPA's Risk-Screening Environmental In-

[6] The design was implemented with a software stack and compute infrastructure designed by Christensen's team at the National Center for Supercomputing Applications.

[7] We construct a single aggregate index of school quality using the geometric average of the elementary, middle and high school scores presented for each listing. This aggregate measure addresses collinearity between the three school quality measures. Similarly we use a single measure of crime (murders) rather than multiple measures (e.g., murders, burglaries, etc.) and a single measure of retail (cafes) rather than additional measures (e.g., groceries, nightlife) to avoid problems of collinearity. Murder rates have been shown in other work to capture the dominant and most salient form of crime in revealed preference studies of damages in the housing market Albouy, Christensen, and Sarmiento-Barbieri (2020).

dicators (RSEI) model, which uses chemical toxicity data, TRI release and transfer quantities, and the location of facilities to calculate toxic concentrations across a national grid. RSEI uses the American Meteorological Society/EPA Regulatory Model (AERMOD) and incorporates information about facilities (location, stack height, etc.), meteorology (wind, wind direction, and ambient temperature), and chemical-specific decay rates to account for differential releases, meteorological conditions such as wind speed and direction, decay rates, and other key characteristics of emissions that can affect exposures (EPA, 2018). Recent work provides evidence that minority renters face discriminatory constraints in neighborhoods with lower levels of concentration (Christensen, Sarmiento-Barbieri, and Timmins, 2022).

The top panel of Table 1 reports summary statistics for key characteristics for each rental listing. We find substantial variation in rent and neighborhood characteristics both between and across the five cities in the sample. The average monthly rent in San Jose, CA ($2,137 per month) is more than double the size of rents in Cleveland, OH ($995 per month). But even within San Jose, the standard deviation of rents ($513 per month) is substantial. There is similar heterogeneity across MSA's in air quality, with a mean RSEI concentration over 37,000 in Houston, while that in San Jose is only 114. As found in a larger set of rental housing markets with industrial facilities, there is substantial within-MSA heterogeneity in RSEI (Christensen, Sarmiento-Barbieri, and Timmins, 2022). Average school quality can range from 0 to 10. The mean value in San Jose (6.81) contrasts with that in Philadelphia (3.43). We see similar heterogeneity in the murders index, with a high of 298.89 (Philadelphia) and a low of 44.98 (San Jose), and cafes, with a high of 47.05 (Philadelphia) and a low of 5.63 (Houston).

In order to characterize the racial composition of each census tract in the study, we collect data from the 2013-17 five-year average ACS, a 1% sample of the total population (Ruggles et al., 2017). We limit the sample to data describing household heads who are renters from the five cities. White households constitute the largest group across tracts in each city, ranging from a high of 70% in Cleveland to 33% in Houston. The second largest group varies by city – Hispanic/LatinX at 35% and 22% in Houston and San Jose, respectively, and African Americans at 41%, 30% and 19% in Atlanta, Philadelphia and Cleveland.

### 3.2 Correspondence Design and Randomization

In a correspondence study, fictitious applicants correspond by mail or via online platform Bertrand and Duflo (2017). Correspondence studies have analyzed the role of race and ethnicity (Ewens, Tomlin, and Wang, 2014, Carlsson and Eriksson, 2014, Ahmed and Hammarstedt, 2008, Ahmed, Andersson, and Hammarstedt, 2010, Hanson and Hawley, 2011, Hanson, Hawley, and Taylor, 2011, Carpusor and Loges, 2006), LGBT status (Ahmed and Hammarstedt, 2009), and immigrant status (Baldini and Federici, 2011, Bosch, Carnero, and Farre, 2010) in rental housing markets.

Two recent experiments study the racial perceptions of names used in correspondence research by quantifying the congruence between the occurrence of distinctly African American, Hispanic/LatinX, and white names and the rate of identification (cognitive association with each group) among survey respondents in the United States (Gaddis, 2017, 2018).[8] Using the results from Gaddis (2017, 2018), we constructed 18 pairs of first and last names that have the highest probability of identification as belonging to each race group. The resulting set of fictitious renter identities consisted of 6 distinct first-last name pairs for each of the three groups. A question that has emerged in prior correspondence studies using racialized names is the possibility that any given name may signal race as well as other unobserved characteristics such as income (Guryan and Charles, 2013, Fryer Jr and Levitt, 2004). To test this empirically, we stratify the sample of first names using the statistical distribution of maternal educational attainment (low, medium, and high) and gender (male and female) reported in Gaddis (2017, 2018). The resulting name groups consist of three male and three female names, one drawn from each of three levels of maternal educational attainment (high/medium/low).

Each rental apartment received a sequence of three separate inquiries directly through the online platform in the course of an experimental trial. Names were drawn randomly

[8] See Appendix A for detail on name selection and the identification rates for each of the names in this study.

from the full set of six for each race group. Inquiries for the same listing were never sent from the same identity or from two different identities on the same day.[9] Randomization of the timing, sequence, and gender/maternal education associated with each inquiry should guarantee that these characteristics are balanced across the inquiries for each group. Differences in name pairs or timing could occur, for example, if a listing is taken offline in the midst of a trial. Appendix Table B1 reports balance statistics for the matched response dataset. We do not find any evidence of differences in the sequence of inquiries or the frequency of inquiries made from a given race-gender or race-education pair.

As it executed each experimental trial, the bot collected data on the exact location, sequence, and timing of responses. Responses to inquiries were coded using two criteria that determine whether or not a housing choice is made available: (1) a response was received within 7 days of the associated inquiry and (2) the response indicated that the property is available for rent.[10] Figure 1 maps raw response data for the listings in each of the five cities, illustrating the locations where a trial yielded responses to 0, 1, 2, or all 3 of the matched sets of inquiries for a given listing. Figure A.2 graphs the average response times for the different inquiries. We find that when property managers operating on this search platform respond, they generally do so within a day of receiving an inquiry. We received 82% of responses within the first 24 hours, 94% within the first three days, and 97% within the first 5 days of an inquiry.

### 3.3 Estimating Choice Constraints

The experimental design yields tests of discriminatory constraints using the differences in means for different groups, as in Bertrand and Mullainathan (2004). We report the magnitudes of constraints in terms of *relative response rates*, which measure the probability of response to an inquiry from an African American or Hispanic/LatinX identity relative to the probability of response to a comparison white identity for the same listing. This measure divides the percentage point difference in mean response rate for each of

[9]Balance tests are reported in Table B1.

[10]The 7-day cutoff was used to restrict responses that may be received weeks or months after an inquiry and are not counted as choices in the study.

the groups of color by the baseline response rate to inquiries from white identities and then adds 1. Equal response rates imply a relative response of 1.

As described in the previous section, each rental apartment receives an inquiry from each of the racial groups on three separate days. For example, on day one, the manager of the unit could receive an inquiry from a white identity, then from an African American identity on day two, and from a Hispanic/LatinX identity on day three. Based on this design, we construct a balanced set of observations of binomial decisions made by property managers in response to a series of inquiries. Assuming that names are drawn randomly and balanced across gender, education level, and inquiry order, relative rates estimates should be robust across specifications that include/omit these variables as controls. Results reported in Appendix Table B1 indicate that inquiries are balanced across these variables and Appendix Table B2 shows that relative rates estimates are not sensitive to the inclusion/omission of these variables as controls.[11]

## 3.4 Experimental Findings: Choice Constraints

**Experimental Findings: Choice Constraints**

Table 2 reports estimates for each of the five housing markets (MSAs) included in the study, as well as for the full sample. The top row reports estimates for minority identities, which combines both African American and Hispanic/LatinX identities. The following two rows report separate estimates for the two minority race groups. At the bottom of the table, the average response rate for white identities (comparison group) are reported, along with the total observations (inquiries) and number of observations associated with properties that yielded an asymmetric response.

These estimates reveal evidence of discriminatory constraints facing both minority

[11]Table B2 reports estimates with increasing controls for tester attributes (i.e. gender and education) in columns 1-4. Randomization of the inquiry process across the 18 identities in the sample ensures that the only difference between white and non-white testers is in the information conveyed by names. Estimates for minority identities indicate that estimates are robust across the sets of controls, including when broken out for African American or Hispanic testers. Attribute controls do increase the precision of estimates. A comparison of estimates on columns 4 and 5 indicates that within-listing estimates of relative response rates are slightly, but not significantly, different from response rates estimated from between-listing (using only first inquiries).

groups in each of the markets in the sample. We estimate that the relative response rate for minority identities is 91.7%, which corresponds to a 43.4% response rate for inquiries from white identities and a 3.6 percentage point lower mean response rate for inquiries from the minority group. Estimated relative response rates indicate that inquiries from African American renters receive a lower likelihood of response (89.9%) than those from Hispanic/LatinX renters (93.4%). In a correspondence experiment conducted using the same platform/name pairs but an entirely independent sample drawn from the 50 largest MSA's in the United States, Christensen, Sarmiento, and Timmins (2021) estimated relative response rates are 90.7% for African American renters and 95.4% for Hispanic/LatinX renters, suggesting that constraints facing these groups in the present sample are consistent with the magnitudes found in a national sample of large metropolitan areas.

Estimates reported in the remaining columns indicate heterogeneity both in baseline response rates and in discriminatory constraints across MSAs, with the lowest relative response rate (86.8%) found in Philadelphia and the highest found in Houston (95.0%), although differences are not statistically significant. We observe even greater heterogeneity in relative response rates by race group, with the lowest and highest values observed for inquiries from African American renters – 77.8% in Philadelphia and 98.2% in Houston. Three of the group-specific estimates are not statistically significantly different from 100% when broken out by minority group. In Houston and Atlanta, relative response rates are higher on average for African American than Hispanic/LatinX identities. In Philadelphia, Cleveland, San Jose, they are higher for Hispanic/LatinX identities.

#### Heterogeneity in Choice Constraints: Neighborhood Characteristics

The estimates above indicate that discriminatory constraints vary substantially across MSAs in the United States. In this section, we report the results of reduced form tests that examine within-MSA heterogeneity in constraints. In particular, estimates plotted in Figure 3 examine whether discriminatory constraints reduce the access of minority renters to property choices that confer higher levels of key neighborhood amenities.[12]

[12]Refer to figure C.1 for estimates of heterogeneous effects by neighborhood amenity levels for each race group.

Each subfigure plots a smoothed function describing the average difference between the response to a white renter minus that to a renter of color (stacking the data to consider both differences between white and African American renters and between white and Hispanic/LatinX renters) using the model:

$$g(x_0) = \frac{1}{2N} \sum_{i=1}^{N} \left[(W_i - AA_i) + (W_i - HL_i)\right] f\left(\frac{x_i - x_0}{h\sigma_x}\right) \tag{1}$$

where $W_i$, $AA_i$ and $HL_i$ take the value 1 or 0 depending upon whether the white, African American, or Hispanic/LatinX renter inquiring at property $i$ received a positive response or not. A function value at $x_0$ equal to 1 would indicate that, at that level of the amenity, white renters always received responses while renters of color never did. A value of -1 would indicate the opposite. Averages of this discrimination index are smoothed using a Gaussian kernal, $f(.)$ with a smoothing parameter $h$ equal to five-times Silverman's rule of thumb (Silverman, 1986). Bootstrapped 95% confidence intervals are displayed around each function.

While the study was not designed to guarantee statistically powered tests of differences in response rates at high versus low amenity levels, these estimates suggest that discriminatory constraints are stronger among properties that have higher rental prices, highly rated schools, fewer local point sources of chemical toxics (plants reporting emissions to the EPA Toxics Release Inventory), and lower murder rates in the neighborhood. On average, there is little evidence of differences in discriminatory constraints with high/low numbers of cafes, though the standard errors are very high in high cafe neighborhoods, suggesting that this may be driven by variation across the MSAs.

**Heterogeneity in Choice Constraints: Listing Age**

A possible explanation for the findings above is that discriminatory constraints are stronger in neighborhoods with strong demand. In markets with excess demand, models of animus-based, taste-based, and attention discrimination all predict that it could be more costly to respond uniformly to all applicants and the opportunity cost associated with losing a prospective applicant may be lower. Estimates reported in Table 3 compare response rates for properties that have been on the market for 0-3 days, 3-7 days, and 7+ days. For both minority groups, we find evidence of stronger discriminatory constraints when sending inquiries to recently listed properties. Among properties that have been listed for less than 3 days, the relative response rate for inquiries from minority identities is only 55.8%. It increases to 64.7% between 3-7 days and 96.5% after 7 days. The remainder of the table indicates that the patterns are similar across both of the minority race groups.

**Heterogeneity in Choice Constraints: Inquiry Order**

In Table 4, we further examine heterogeneity in response rates across trials where a minority identity sends the first, second, or third inquiry (these are each compared to the response rate for a white identity that sends a first inquiry). Estimates reported in column 1 indicate that relative response rates to all identities fall when preceded by another inquiry, though inquiries sent from minority identities fall faster – from 85.4% when first, to 78.2% when second, to 71.8% when third in the sequence. Columns 2 and 3 show that this pattern is consistent across neighborhoods with above-median shares of white and above-median shares of minority households, although discriminatory constraints are always stronger in neighborhoods with higher shares of white households. These results are consistent with the results on listing age and suggest that competition for housing could be a mechanism through which response rates for minority applicants diminish in tight housing markets.

# 4 Neighborhood Sorting with Choice Constraints

In this section, we integrate the experimentally identified estimates of discriminatory constraints with a structural sorting model, allowing us to study the extent to which the constraints imposed during the search process affect both the quantity and also the quality of options that end up in the renter's post-search choice set. There may be trade-offs between housing attributes that renters are considering in the context of discriminatory constraints. For example, a unit may provide a high level of public safety but poor schools. In order to study the cumulative impact of constraints on neighborhood choices, we require a set of utility weights. These are provided by the estimates from the residential sorting model.

## 4.1 Neighborhood Choice Model

We model residential location choices for renters who vary in income and preferences for key neighborhood attributes. Renters optimally choose a housing unit within a census tract based on individual incomes and preferences for neighborhood attributes that vary across tracts. This approach accounts for impacts of discrimination on multiple neighborhood characteristics that may be traded off for one another in the minds of renters.

While models of residential location choice (including property-value hedonic models) typically assume that individuals have the entire range of options available to choose from in a given market, the experimental evidence reported in Section 3.2 indicates that this assumption is violated in all five of the MSAs that we study. Figure 2 maps the response rates in each of the five MSAs across the set of census tracts for which renter moves were observed during 2018 in InfoUSA. We build constraints into our structural model directly, allowing experimental variation in choice probabilities across census tracts to differentially constrain the choice sets of the renters observed in the panel.

**Renter Location Data**

We estimate utility function parameters for neighborhood attributes using InfoUSA's residential historical dataset, which provides a large panel of the incomes and actual location decisions of households who moved into rental properties during 2018. InfoUSA's consumer database tracked 120 million households and 292 million individuals between 2006-2020, and is maintained using 29 billion records from 100 sources including census statistics, billing statements, telephone directory listings and mail order buyers/magazine subscriptions. Household-level identifiers provide information on the gender, race/ethnicity, age, address, renter/owner status and estimated household income of renters that made a move in 2018.

The bottom panel of Table 1 summarizes characteristics of renters identified in InfoUSA by MSA. White renters have the highest mean income in every city in the sample, though income gaps between white renters and renters of color differ greatly across cities.

The white-Black income gap ranges from 14% in San Jose to 71% in Philadephia. The white-LatinX income gap ranges from 9% in Atlanta to 31% in Philadephia. In addition, the ordering of mean income across groups differs by MSA, with African Americans having the lowest mean income in Atlanta, Houston and Philadelphia. Hispanic/LatinX renters have the lowest mean income in San Jose, and the two groups of color have roughly the same mean income in Cleveland. Renters of color spend a greater share of income on housing in all of the five cities, though this gap is consistently smaller than the same city's racial income gap. African American renters spend the highest share of income on housing, with shares ranging from 40-42%.

Compared to the Census ACS population shares, the InfoUSA sample of renters has a higher representation of white households, but otherwise follows the general demographic patterns observed across MSA's. Atlanta, Cleveland, and Philadelphia have the highest populations of African American renters, who represent approximately one-quarter of the renter populations in those MSAs. San Jose has the smallest population of African American renters, who represent just 2%. African Americans and Hispanic/LatinX renters both represent more than 20% of the renter population in Houston. Hispanic/LatinX renters represent a large share (22%) of the renter population in San Jose, but a relatively small share of the renter populations in Atlanta (6%), Philadelphia (6%), and Cleveland (5%).

**Predicted Response Probabilities**

We use experimental variation to estimate the probability that a renter who is a member of a particular race group $g$ will receive a response from listing $k$ in city $c$, where $\chi_{k,c,g} = 1$ if identity race $g$ at rental property $k$ in city $c$ received a positive response (= 0 otherwise). For race group $g$, we maximize the following log-likelihood function:

$$L_g = \sum_c \sum_{k \in c} \omega_{k,c} \big[ \chi_{k,c,g} ln\Phi \big[ Z'_{k,c}\beta_g + \gamma_{0,c,g} + \gamma_{1,c,g} lat_{k,c} + \gamma_{2,c,g} lon_{k,c} \big] + \big(1 - \chi_{k,c,g}\big) ln \big(1 - \Phi \big[ Z'_{k,c}\beta_g + \gamma_{0,c,g} + \gamma_{1,c,g} lat_{k,c} + \gamma_{2,c,g} lon_{k,c} \big] \big) \big] \quad (2)$$

where $Z_{k,c}. = [AvgSchoolQuality, Cafes, Murders, ln(RSEI), \%Race_g, \%Race_g^2]$ is a vector of explanatory variables that includes the neighborhood characteristics that are used below to define utility. We also include a separate intercept term for each city, and we interact city dummy variables with latitude and longitude ($lat_{k,c}$, $lon_{k,c}$). We weight the likelihood contributions from each listing by $\omega_{k,c} = 1/N_c$, where $N_c$ is the number of listings in city $c$. In this way, all cities enter equally into the prediction of response probabilities. We report estimates from Eq. 2 in Table 5. Standard errors are generated from 200 bootstrap draws that are taken to preserve the sample size contributed by each city.

Nearly all determinants of response probabilities are statistically significant for Hispanic/LatinX renters, including race and other neighborhood characteristics along with latitude and longitude across cities. The only exception are those latter variables in San Jose. The same is true for white and African American renters. For those groups, latitude and longitude are statistically significant determinants of response probability in Atlanta and Houston; longitude is significant for both groups in Philadelphia and for white renters in Cleveland. Amongst the other neighborhood attributes, $ln(RSEI)$ and $Cafes$ are also significant determinants of response probabilities for white and African American renters. We account for the fact that other variables do not enter significantly into the response probability equations in the calculation of standard errors for our utility function parameters, jointly bootstrapping both sets of parameters 200 times.

We use estimates from this equation to predict the probability of a response for a renter of race $g$ in any census tract $j \in c$ (location assumed to be at the tract centroid) as:

$$\rho_{j,c,g} = \Phi(Z'_{j,c}\hat{\beta}_g + \hat{\gamma}_{0,c,g} + \hat{\gamma}_{1,c,g} lat_{k,c} + \hat{\gamma}_{2,c,g} lon_{k,c}) \tag{3}$$

We randomly generate a collection of $N_s$ choice sets, $\{\Gamma_{i,s}\}_{s=1}^{N_s}$, for each renter $i$ observed in the InfoUSA database using estimates of $\hat{\rho}$ from Equation 3. The next section describes how those choice sets are generated and used to condition the expected probability in a likelihood function that characterizes the observed behavior of renters in our sample.

**Modeling Location Choice with Consideration Sets**

We turn next to the utility of renter $i$ choosing to live in tract $j$:

$$U_{i,j} = \alpha ln(I_i - R_j) + \xi_j + \beta_{1,g}\sigma_{g(i),j} + \beta_{2,g}\sigma^2_{g(i),j} + \epsilon_{i,j} \tag{4}$$

$$\xi_j = X_j'\gamma + \sum_c \delta_{j,c}(\psi_{1,c}lat_{j,c} + \psi_{2,c}lon_{j,c}) \tag{5}$$

where $I_i$ is the monthly income of renter $i$ (in \$1000's), $R_j$ is the monthly rent (in \$1000's) associated with housing units in tract $j$, $X_j$ captures other attributes of tract $j$ including $AvgSchoolQuality$, $Cafes$, $Murders$, and $ln(RSEI)$. $\sigma_{g(i),j}$ measures the share of tract $j$ population in group $g$ corresponding to race of renter $i$, and $\epsilon_{i,j}$ is an idiosyncratic utility shock for renter $i$ associated with census tract $j$. The $\sigma_{g(i),j}$ parameter captures the effect of race-specific preferences for local public goods and amenities that might determine differential sorting patterns. Importantly, the $\sigma_{g(i),j}$ parameter will capture any differences in unobserved neighborhood characteristics such as retail establishments, churches, and schools that may have an association with the sorting patterns of a particular racial/ethnic group in the sample. This could include homophily preferences – a preference to live near others who are of the same race. We include both $\sigma_{g(i),j}$ and $\sigma^2_{g(i),j}$. $\delta_{j,c}$ is defined as in equation (2). The final term allows for utility to vary with latitude and longitude differentially in each city.

Assuming that $\epsilon_{i,j} \sim$ $i.i.d.$ Type I Extreme Value, the probability that renter $i$ will choose location $j$ is given by:

$$P_{i,j} = \frac{exp\left[\alpha ln(I_i - R_j) + \xi_j + \beta_{1,g}\sigma_{g(i),j} + \beta_{2,g}\sigma^2_{g(i),j}\right]}{\sum_k exp\left[\alpha ln(I_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]} \tag{6}$$

where the denominator is summed over all tracts $k$ that the renter has to choose from. The typical approach is to include all tracts in the market in this choice set. In our approach, we denote a consideration set for renter $i$ (i.e., a restricted collection of tracts from which renter $i$ may choose) by $\Gamma_i$. The probability of choosing a particular tract $j$ given the consideration set $\Gamma_i$ is given by:

$$P_{i,j}|\Gamma_i = \frac{exp\left[\alpha ln(I_i - R_j) + \xi_j + \beta_{1,g}\sigma_{g(i),j} + \beta_{2,g}\sigma^2_{g(i),j}\right]}{\sum_{k\in\Gamma_i} exp\left[\alpha ln(I_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]} \tag{7}$$

where $\Gamma_i$ determines the set of tracts that comprise the denominator of the logit expression. Note that, for the probability to be well-defined, renter $i$'s choice $j$ must be an element of $\Gamma_i$.

Our experiment identifies choice sets probabilistically – with probability $\rho_{j,c,g}$, tract $j$ in city $c$ will appear in the choice set of a renter from group $g$. In order to calculate the expected probability that renter $i$ will choose tract $j$, we simulate $N_s$ consideration sets and take the associated expected probability:

$$E[P_{i,j}] = \sum_{s=1}^{N_s}\left(\frac{exp\left[\alpha ln(I_i - R_j) + \xi_j + \beta_{1,g}\sigma_{g(i),j} + \beta_{2,g}\sigma^2_{g(i),j}\right]}{\sum_{k\in\Gamma_{i,s}} exp\left[\alpha ln(I_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]}\right) W_{i,s} \tag{8}$$

where each consideration set is weighted by $W_{i,s}$

$$W_{i,s} = \frac{P(\Gamma_{i,s})}{\sum_{m=1}^{N_s} P(\Gamma_{i,m})} \tag{9}$$

and

$$P(\Gamma_{i,s}) = \Pi_{j=1}^{J}\rho_{j,c,g(i)}^{\zeta_{i,j,s}}(1-\rho_{j,c,g(i))})^{1-\zeta_{i,j,s}} \tag{10}$$

Each simulated consideration set is found by taking a uniform random draw from $[0,1]$; if that draw is less than $\rho_{j,c,g}$, the tract enters simulated consideration set $\Gamma_{i,s}$. If tract $j$ is included in the simulated choice set $s$ for renter $i$, then $\zeta_{i,j,s} = 1$. The weight $W_{i,s}$ then reflects the probability that consideration set $s$ with choice probability $\Gamma_{i,s}$ is available to renter $i$ (weights are normalized to sum to 1). We then maximize the log-likelihood function based on these probabilities:

$$L = \sum_{i=1}^{N} lnE[P_{i,j^*(i)}] \tag{11}$$

where $j^*(i)$ refers to the observed census tract choice of individual $i$. Our analysis pools data from the five different metropolitan areas in order to increase the external validity and to provide greater variation in the neighborhood attributes that individuals are choosing over. Importantly, while we estimate a common set of preference parameters across markets for neighborhood attributes other than latitude and longitude, we restrict the choice set available to an individual to only include the tracts in their associated city. Within a given city, likelihoods are implicitly weighted based on the population of renters from different groups as documented in the InfoUSA data. For instance, a smaller share of African American renters in San Jose will translate into smaller share of the sample that we use to identify the structural preference parameters in our model, giving less weight to their choices.

## 4.2 Structural Model Estimates

Table 6 reports the results of our model of residential location choice, incorporating the choice set constraints imposed by discrimination. We report bootstrapped standard errors that account for the estimation error arising from using fitted response probabilities from a first stage. In particular, we take 200 randomly generated data sets (with replacement) of our experimental data. These randomly generated data sets are each the same size within each city as the actual data set. For each, we generate a set of estimates and fitted probabilities associated with equations (4) and (5). Next, for each randomly generated first-stage data set, we generate a random data set for our second-stage estimation (data sets have the same sample size as the actual data set and are drawn with replacement). We estimate the second-stage consideration set model with that data set using the results from the first-stage response probabilities. We save these first- and second-stage results, and repeat this process 200 times. Finally, we use the standard deviation of the bootstrapped estimates as our standard errors. Column 1 reports parameter estimates and standard errors for all second-stage parameters. Aside from four of the latitude-longitude-city interaction parameters, all of the estimates are statistically significant at the 1% level. For each of the primary neighborhood attributes, Column 2 reports measures of marginal willingness-to-pay for a one-unit increase in the attribute, measured as a percentage of income. We begin with our utility function for individual $i$ living in census tract $j$:

$$U_{i,j} = C_i^{\alpha} e^{X_j'\gamma + \beta_{1,g}\sigma_{g(i),j} + \beta_{2,g}\sigma^2_{g(i),j} + \epsilon_{i,j}} \tag{12}$$

Recognizing that $C_i = I_i - R_j$ given the budget constraint for each renter $i$, the marginal willingness to pay (MWTP) for $X_j$ is given by the following expression:

$$MWTP = \frac{\frac{\partial U}{\partial X}}{\frac{\partial U}{\partial C}} = \frac{\gamma}{\alpha}(I_i - R_j) \tag{13}$$

Dividing by $I_i$ yields a convenient expression for MWTP as a share of income:

$$\frac{MWTP}{I_i} = \frac{\gamma}{\alpha}\frac{(I_i - R_j)}{I_i} = \frac{\gamma}{\alpha}(1 - s_H) \tag{14}$$

where $s_H$ is the share of household income spent on rent. This implies that marginal willingness to pay as a share of non-housing expenditures is given by:

$$\frac{MWTP}{I_i(1 - s_H)} = \frac{\gamma}{\alpha} \tag{15}$$

In the case of average school quality, $\alpha = 1.3117$ and $\gamma = 0.0350$ and the average standard deviation across our five MSAs is 1.684. This implies a willingness to pay of 2.70% of non-housing expenditures for a one-unit improvement in average school quality, or 4.55% for a one standard deviation improvement.[13] For a household that consumes 20% of income on rent, this would imply a willingness to pay of about 3.64% of total income for that one standard deviation improvement.[14]

## 4.3 Measuring the Effects of Discrimination on Renter Welfare

Discrimination in the online search environment directly affects a renter's choice set. The random utility choice framework simulates an actual search process and is well-suited for analyzing the impacts of alterations to an individual's choice set. We describe impacts in terms of equivalent variation in income by first measuring the expected utility associated with the full (unconstrained) set of all census tracts versus the constrained set.

$$EU_i = log\left(\sum_{k=1}^{J} exp\left[\alpha ln(I_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]\right) \tag{16}$$

[13] Standard errors for these WTP ratios are bootstrapped as well.
[14] Figure D.1 plots rent-to-income by income and race.

Alternatively, the expected utility associated with the constrained set of choices is given by:

$$\tilde{EU}_i = log\left(\sum_{s=1}^{N_s} W_{i,s} \sum_{k\in\Gamma_{i,s}} exp\left[\alpha ln(I_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]\right) \qquad (17)$$

We can therefore calculate the equivalent variation in income ($EV_i$) associated with choice set constraint from the following equation:

$$log\left(\sum_{k=1}^{J} exp\left[\alpha ln(I_i + EV_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]\right) = \qquad (18)$$

$$log\left(\sum_{s=1}^{N_s} W_{i,s} \sum_{k\in\Gamma_{i,s}} exp\left[\alpha ln(I_i - R_k) + \xi_k + \beta_{1,g}\sigma_{g(i),k} + \beta_{2,g}\sigma^2_{g(i),k}\right]\right)$$

We use Eq.18 to simulate changes in the $EV_i$ that an individual renter receives in counterfactual search environments. In particular, we simulate the search behavior of a set of 5,000 African American and 5,000 Hispanic/LatinX renters using random draws from actual race-specific income distributions in each city. For each renter, we compute the welfare effects associated with search when confronted with choice set constraints given by the response probabilities for their group. We then confront the same renter with the choice set constraints given by the response probabilities recovered for white identities. This simulation holds constant all aspects of search that are race-specific – in particular, those associated with income and potential homophily preferences. This allows us to: (i) isolate the effects of discriminatory constraints that affect the consumption of an array of neighborhood amenities and (ii) estimate their combined effects with a single welfare measure.

Figure 5 reports the distributions of equivalent income variation associated with discriminatory constraints as a share of annual income. For African Americans, the median value is -3.2%, compared to -1.5% for Hispanic/LatinX renters.[15] The mean effects for both groups are higher (-4.4% and -3.5%) as a result of left tails with damages of over -15%. These findings indicate that the discrimination incurred during the first stage of a search process can result in lost choices that both groups would be willing-to-pay significant sums to avoid. Furthermore, the distributions of damages for both groups are bimodal, suggesting important differences across different renters within each group. Figure 6 investigates this further by illustrating shifts in the distribution of damages across different renter income groups. These histograms demonstrate that the mass of welfare effects increases (moves to the left) as income rises for African Americans, with a median value of approximately -1.6% for those with annual incomes in the $0-30,000/year range. This increases to -3.3% for those in the $30,000-60,000/year range and continues to rise, exceeding -6.8% for those in the $120,000-150,000/year range. By contrast, damages for Hispanic/LatinX renters become smaller and the distribution more compressed at higher incomes.

Figure 7 provides a clear illustration of the way in which monetized damages from discriminatory constraints vary with the incomes of African American and Hispanic/LatinX renters. The results in this figure combine damages facing renters in all of the five markets, allowing us to examine heterogeneity in damages across a range of income levels, market characteristics, and levels of discriminatory behavior. We note that income levels differ systematically across the markets, such damages facing renters in high-income markets such as San Jose are disproportionately represented in the upper segment of the income distribution. Damages rise steadily with income for African Americans, approaching $10,000/year for households that earn $150,000/year. This contrasts with damages facing Hispanic/LatinX renters, which do not grow with respect to income. This reflects the stronger constraints facing African American renters that search for housing in high

[15]Figure 5 indicates that a small fraction of the African American and LatinX renter distributions experiences small welfare gains from discriminatory constraints, which reflect higher response rates to inquiries from each group than from white identities in certain neighborhoods. An analysis of response rates indicates that African American renter identities receive statistically higher response rates (than white identities) in 1.3% of tracts and that LatinX renter identities receive statistically higher response rates than white identities in 3.9% of tracts. As evidenced in Figures 3 and 4, these neighborhoods tend to have larger shares of African American/LatinX households but lower levels of public safety.

amenity tracts, which are consistent with stronger reduced form effects in high amenity neighborhoods reported in Appendix C. This difference sheds light on the implications of housing discrimination on the economic mobility of African American households, which is shown to differ systematically from that of Hispanic/LatinX households (Chetty and Hendren, 2018). Discriminatory constraints impose a higher cost of search on economically mobile African American households that would optimally invest an increasing fraction of income in high amenity neighborhoods, which are shown to be important for human capital accumulation.

## 4.4 Measuring the Effects of Discrimination on Housing Search

In the prior section, we found that renters of color face a considerable welfare impact associated with discriminatory constraints. A natural response might be to invest more heavily in search to mitigate the welfare effect. However, search is costly. In this section, we quantify the amount of additional search required for a renter of color to achieve the same utility outcome as an otherwise identical renter who faces white response probabilities in the rental market.

We consider 5,000 pairs of renters in each city, using either the median incomes for African American or Hispanic/LatinX renters from each of the five cities. The non-random component of utility for pair $i$ is defined by estimates of preferences for neighborhood characteristics in tract $j$.[16]

$$U_{i,j} = \alpha ln(I_i - R_j) + \xi_j + \beta_{1,g}\sigma_{g(i),j} + \beta_{2,g}\sigma^2_{g(i),j} \tag{19}$$

Using these preferences, we rank tracts from highest to lowest utility to define the order of search within a market.[17] As the search proceeds through the series of ranked tracts,

[16]We include 'same race' shares for African American and Hispanic/LatinX renters in this model in order to compare the search costs incurred by a minority renter from each group relative to those incurred by a counterfactual renter with identical homophily preferences who receives the response probability for the white comparison identity.

[17]We assume in this exercise that the search order is established prior to the realization of the random component of preferences. We consider that random component – the idiosyncratic preference shock that is the basis for the random utility model specification – to be the information that is learned upon visiting a residential location choice. This is realized conditional upon a successful inquiry about that tract. As such, each pair receives a common random draw from the Gumbel distribution ($F(x) = e^{-e^{-x}}$) for each tract conditional upon the response outcome.

the African American or Hispanic/LatinX identity in each pair receives draws from the appropriate experimental response distribution while the otherwise identical white member of the pair receives a draw from the white response distribution.[18] If the individual does not receive a response for a tract or if the tract exceeds the budget constraint for the pair, the draw yields a utility of $-\infty$. Otherwise, the individual records the utility (including the idiosyncratic preference shock) received from that tract. We simulate a search across the series of tracts where renters maximize utility according to the following procedure: if the utility from a given draw is higher than that received from prior draws in the search, it becomes the max utility. If lower than the max utility obtained from prior draws, then that tract is ignored. For each African American and white renter, we therefore obtain an estimate of the maximum level of utility achieved at every level of search.

We combine simulation results across cities by defining different levels of search intensity in terms of the percentage of the city's tracts.[19] We combine all simulated individuals' max utilities across cities and compute the median in each of 100 percentile bins over 25,000 different renter pairs. We then compute the additional search required by the median renter of color to obtain the same utility as the median white counterpart.

Figure 8 reports the additional investments required by African American and Hispanic/LatinX households to obtain the same utility as an equivalent white counterpart. Dashed lines in each figure describe the excess search required by the minority renter as a percentage of the search conducted up to that point. These estimates suggest that minority renters face non-trivial search costs. Minority renters searching in 5-10% of the tracts in a market would have to increase their search by 15-30% to achieve the same utility as a white renter. Minority renters searching 25% of the market would increase their search by approximately 10% (i.e., an additional 2.5% of tracts in the market). For both African American and Hispanic/LatinX renters, the results suggest increasing absolute levels of search required to achieve the same utility as a comparable white renter.

[18] In particular, the white counterfactual identity has the same preferences as the renter of color in the pair, but differs only in that they receive responses based on probabilities for white renters.

[19] For example, Atlanta has 129 tracts, so 1% of the city searched would be $INT(0.01 * 129) = 1$ tract, while 4% of Houston, with its 322 tracts, would be $INT(0.04 * 322) = 13$ tracts.

This is a result of the fact that the tracts searched later in the process are lower-amenity and provide less utility, so more of them are required.

Renters may send additional requests to properties in tracts that yield a high level of expected utility or strategically avoid tracts where they anticipate discriminatory constraints. Since the correspondence study does not test for effects on follow-up inquiries, the search simulation described above assumes that the relative response to a second request would be identical to that received for the first. However, the evidence from a prior correspondence study using the same design indicates that follow-up inquiries *reduce* the relative response rates of renters of color when subsequent inquiries are sent (Christensen, Sarmiento-Barbieri, and Timmins, 2022). Appendix G illustrates the effects of follow-up inquiries using an exercise that examines cumulative response rates to the average listing in the experimental sample from Christensen, Sarmiento-Barbieri, and Timmins (2022). While there may be heterogeneity in this difference across different types of neighborhoods, it suggests that our estimates likely provide a lower bound on constraints facing renters of color relative to white counterparts given the same investment in search.

In Figure 9, we construct a variant of the search simulation that assumes that minority renters can strategically avoid tracts where they are likely to face increased discrimination. This exercise allows white renters and renters of color to order tracts based on expected utility taken with respect to their different probabilities of receiving a response.[20] African American and Hispanic/LatinX renters will, therefore, avoid higher amenity tracts where we have found that the likelihood of discrimination is higher for these groups. We find some evidence that strategic avoidance can partially mitigate the additional search costs for African American renters, though resulting differences in search cost are largely consistent with those in the baseline model. The results suggest that even with information about discriminatory constraints, strategic avoidance does not fully mitigate the search costs of minority renters in the markets that we study. This is largely attributable to a key fact established by our reduced-form analysis: discriminatory constraints are correlated with higher neighborhood amenity levels, making it difficult to strategically avoid

[20] In particular, we multiply expected utility by the probability of a response, yielding expected utility. We then order the tracts for each group based on that value.

the former without also avoiding the latter.

### 4.5 Discrimination and the Bias in WTP Measurement

In this section, we examine the distortionary effects of discriminatory behavior on bias in estimates of revealed preference parameters underlying housing search behavior. This is important, as decisions in the housing market send powerful implicit signals about demand for local public goods and have become central to the valuation of key non-market goods and neighborhood amenities. These values are used to guide decisions about the allocation of public resources and to conduct cost-benefit analysis of regulatory policy. If biases in these estimated values are correlated with race, discrimination could have important distributional consequences.

The intuition underlying the bias hypothesis is straightforward. Housing markets provide valid revealed preference estimates of demand for local public goods, assuming that households have access to all available choices. Under that condition, households reveal their willingness-to-pay to live in a neighborhood with a marginally better attribute (e.g., lower crime rates) compared to an otherwise similar neighborhood with a marginally worse attribute (e.g., higher crime rates). Systematic exclusion from housing choices in neighborhoods with higher amenity levels would bias the preferences estimated for the excluded group. A naive model would assume that this group has low willingness-to-pay for those amenities. We construct a test for bias using the experimental data and consideration set model to estimate MWTP that incorporates choice set constraints. We then compare these to estimates from a naive model that ignores these constraints. In order to demonstrate the particularly important role that these biases might play, we re-estimate using a specification that allows for limited heterogeneity in MWTP for each amenity in our study based on race. In particular, we allow the coefficients on the log of income after paying rent and on all non-race tract amenities to be different for white renters versus renters of color (ROC).[21]

The top panel of Table 7 reports estimates from models with and without (naive model) consideration sets, based on our main specification. While the parameters in

[21] In our estimation results, renters of color combines the African American and Hispanic/LatinX groups.

Table 7 include race-specific heterogeneity in preference parameters, estimates from the consideration sets model are consistent in sign, magnitude, and significance level with those reported in Table 6.[22] The lower panel reports a measure of the relative upward bias in each willingness to pay estimate for white renters relative to renters of color when ignoring choice set constraints.[23] The measure is computed using the difference in the averages of the MWTP's for each amenity taken over all renters within each race group when incorporating consideration sets versus the naive model.[24] The difference in these differences reveals the extent to which an upward bias in the MWTP attributed to white renters from ignoring choice set constraints exceeds that attributed to renters of color.

In all cases, the point estimate of these differences is positive. In the case of cafes, a proxy for retail amenities, ignoring consideration sets raises this number for white renters by 0.0812 more than it raises it for renters of color. The difference is statistically significant at the 0.05 level.[25] We find similar results for each of the other three amenities that we study, though the differences are not statistically significant in our sample. These findings suggest that ignoring consideration sets could bias the allocation of investments in key amenities away from neighborhoods that are composed primarily of renters of color and toward those composed primarily of white renters.

# 5 Study Limitations

We identify four primary limitations of the current study that may be addressed in future work: (1) the correspondence framework does not measure the complete set of discriminatory behavior that could affect renter constraints, (2) the correspondence design captures discriminatory behavior on a particular search platform, (3) the current study is limited to a particular segment of the rental housing market, and (4) computational

[22]Standard errors are again generated from 200 random bootstrap draws that account for error introduced by the first-stage estimation of response probabilities.
[23]In the case of murders and RSEI pollution, we consider MWTP for reductions in their values, so they are also treated as amenities.
[24]Each renter's income net of housing expenditure is used to calculate MWTP and is measured at the individual's observed housing choice. Similarly, we use the value of RSEI at the individual's observed housing choice to calculate the MWTP for a reduction in that disamenity.
[25]Standard errors for the differences in ratios in the lower panel of Table 7 are also generated from 100 random bootstrap draws.

constraints associated with the estimation of consideration sets.

Discrimination in housing market transactions (as in any market) can occur at different points of contact and could express itself in different ways throughout the process. No single research design can capture the full set of behaviors, which could include: differential responses in communications, differential policies regarding income/credit/pets/smoking, differential levels of encouragement in response, differential responses to negotiation over rent or other terms of a lease, and differential in-person cues. The present study focuses on the most concrete channel through which discriminatory behavior constrains a choice set in the first point of contact. If access to a lease is constrained by a property manager in this initial contact, we assume that this constraint will bind on their search. Unlike in the case of steering by a real estate agent, where a household could potentially gain access to previously constrained parts of the market by finding a new agent, communication with a property manager represents the key point of access in this market. Little is known about how the effects of discriminatory responses can accumulate through the course of interactions about a rental property. By selecting traits that maximize the salience of racial/ethnic identity in the initial first contact, the correspondence study is designed to trigger a behavioral response (on choice constraints) that may otherwise unfold as a renter's identity is revealed through the course of a rental search process. To the extent that the impact of discriminatory responses on a prospective renter's choice set accumulates throughout the process with the same level of heterogeneity across the neighborhood amenities that we study, then the estimates from a correspondence experiment provide a lower bound on the magnitude of the damages. Damages could also be larger if discrimination enters a renter's utility function directly. If constraints at later stages deviate from the patterns observed in the correspondence study, then our damages estimates can be interpreted as those associated with constraints encountered in the first stage of a search.

Similarly, our study is limited to discrimination observed on a particular rental housing search platform. Higher or lower baseline response rates on the search platform could affect estimates of choice constraints and the equivalent variation for all groups in the considerations sets model. Since the structural estimates in our consideration sets model are estimated on the basis of differences relative to the comparison white identity, the key assumption is that the experimental estimates from the correspondence study identify differences in discriminatory constraints across neighborhoods. To the extent that the relationship between constraints and neighborhood amenities differs for search done outside the online platform, then their effects will be captured by tract-level controls for racial composition and our damages estimates can be interpreted as those associated with constraints on this platform alone.

Our study is limited to a specific segment of the rental markets in five large cities. Focusing on these cities allows us to incorporate constraints facing renters with a range of preferences who are searching in markets characterized by a range of amenities and conditions. By focusing on a narrow segment of the rental market (3 bedroom, 2 bath units), we are able to focus on search given heterogeneous preferences for neighborhood amenities while holding certain rental property characteristics constant. However, renters may encounter different levels of constraint in other cities or segments of the U.S. rental housing market. Variation in discriminatory behavior across neighborhood-level amenities may interact with variation in property characteristics. While the current study uses tract-level racial composition to control for unobserved characteristics that have shaped the revealed sorting patterns of different groups into the census tracts in our sample, a broader set of characteristics and interactions could be more explicitly studied in future work. This could also include listings from a broader set of search platforms, such as those used by voucher recipients.

Finally, we note the potential to expand on the features of the modeling approach introduced in this paper. For example, future applications could allow for a pair of normally distributed random effects that enter into the response probability and utility equations. Allowing these draws to be correlated would allow, for example, for neighborhoods that are attractive in an unobservable dimension to also be more discriminatory towards renters of color. Alternatively, one might introduce a richer form of heterogeneity in preferences, allowing for some form of random parameters.[26] The estimation algorithm in the current study integrates over potential choice sets in a computationally intensive process that requires calculating individual choice probabilities for a large number of random choice set draws. This, combined with bootstrapping for parameter inference, introduces a number of computational constraints. However, implementing these and other additions may be possible in other choice settings or with improvements in parallel computing that speed the estimation process.

# 6 Conclusion

The experimental literature on discrimination has focused largely on reduced form differences in discriminatory behavior. Estimates of impacts are limited by the fact that a researcher never observes how search constraints bind on the decisions of fictitious buyers/renters. This paper combines a correspondence study with a utility-based structural model of housing search, drawing upon estimation techniques developed in the consideration sets literature to estimate the structural parameters in the context of discriminatory constraints that restrict renter choice sets in five major metropolitan housing markets in the United States.

Under the assumptions of the structural model, this estimation approach recovers utility-theoretic measures of welfare cost associated with the choice set restrictions imposed by discrimination. Our estimates suggest that the damages from discriminatory constraints in the first stage of a search process are equivalent to 4.4% and 3.5% of income for the average African American and Hispanic/LatinX renters in our sample, respectively. These damages grow considerably with the level of income for African American renters. This is consistent with stronger discrimination rates found in high-amenity/high-rent neighborhoods and the higher marginal value of neighborhood amenities at higher incomes. In addition, we use our model to examine the extent to which search activity can be used to offset the detrimental effects of discrimination. At moderate levels of search, we find that renters of color are required to undertake approximately 10-30% more effort

[26]We thank a pair of anonymous referees for these suggestions.

on search to achieve the same level of utility as a white counterpart with the same income and preferences.

In a final section, we explore the effect of discriminatory constraints on estimates of the revealed willingness-to-pay for the amenities that we study. Findings from this analysis indicate that by driving a wedge between true amenity preferences and those revealed by a housing search, discrimination can distort estimates of willingness-to-pay derived using standard methods. The same distortion affects signals that the housing market sends to policymakers about the value of key local public goods.

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Table 1. Summary Statistics: Neighborhood Characteristics

| | Atlanta, GA | | Houston, TX | | Philadelphia, PA | | Cleveland, OH | | San Jose, CA | |
|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. |
| Rental Listings (Search Platform) | | | | | | | | | | |
| Monthly Rent | 1167.19 | 290.17 | 1120.00 | 355.13 | 1157.94 | 336.31 | 994.55 | 379.42 | 2137.11 | 513.53 |
| Ave School Quality | 5.07 | 1.46 | 4.57 | 1.82 | 3.43 | 1.24 | 4.93 | 2.17 | 6.81 | 1.73 |
| Cafes | 15.90 | 14.09 | 5.63 | 7.19 | 47.05 | 37.29 | 3.20 | 3.38 | 11.75 | 7.98 |
| Murder Index | 273.87 | 281.89 | 175.00 | 155.50 | 298.89 | 311.38 | 98.75 | 149.39 | 44.98 | 62.31 |
| Toxics Concentration (RSEI) | 638.30 | 466.76 | 37006.13 | 45389.93 | 4312.55 | 3266.31 | 9605.46 | 9523.51 | 114.36 | 133.18 |
| American Community Survey | | | | | | | | | | |
| African American Share | 0.41 | 0.34 | 0.21 | 0.22 | 0.30 | 0.31 | 0.19 | 0.25 | 0.02 | 0.02 |
| LatinX Share | 0.07 | 0.09 | 0.35 | 0.21 | 0.08 | 0.07 | 0.05 | 0.07 | 0.22 | 0.18 |
| White Share | 0.43 | 0.30 | 0.33 | 0.25 | 0.51 | 0.29 | 0.70 | 0.26 | 0.38 | 0.20 |
| InfoUSA Renter Dataset | | | | | | | | | | |
| Renter Income | | | | | | | | | | |
| African American | 36,390 | 16,460 | 36,970 | 20,660 | 36,730 | 24,380 | 28,470 | 14,540 | 79,480 | 47,440 |
| Hipanic/LatinX | 44,790 | 20,130 | 39,730 | 22,450 | 47,830 | 30,630 | 28,950 | 13,930 | 74,730 | 43,780 |
| White | 49,010 | 21,290 | 50,800 | 27,150 | 62,810 | 31,320 | 36,090 | 17,820 | 90,800 | 50,820 |
| Rent-to-Income Ratio | | | | | | | | | | |
| African American | 0.40 | 0.16 | 0.40 | 0.17 | 0.42 | 0.18 | 0.42 | 0.16 | 0.40 | 0.19 |
| Hipanic/LatinX | 0.39 | 0.16 | 0.39 | 0.17 | 0.39 | 0.18 | 0.38 | 0.13 | 0.40 | 0.19 |
| White | 0.37 | 0.16 | 0.36 | 0.18 | 0.34 | 0.17 | 0.35 | 0.15 | 0.36 | 0.19 |
| Population Share | | | | | | | | | | |
| African American | 0.25 | | 0.23 | | 0.21 | | 0.25 | | 0.02 | |
| Hipanic/LatinX | 0.06 | | 0.22 | | 0.06 | | 0.05 | | 0.22 | |
| White | 0.68 | | 0.55 | | 0.72 | | 0.71 | | 0.77 | |
| Census Tracts | n = 129 | | n = 322 | | n = 138 | | n = 92 | | n = 184 | |

Notes: Table reports mean and std. dev. for neighborhood characteristics (census tract level) from the following data sources: (1) rental listings on the search platform used in the experiment (toxics concentrations come from the EPA RSEI model), (2) American Community Survey (2013-2017), and (3) InfoUSA renter data. Rent-to-income ratios use renter incomes from InfoUSA data and rental prices from the 2013-2017 American Community Survey for each MSA. Rental listing characteristics are first calculated for each census tract using ACS reported rent and characteristics associated with all rental listings scraped by our bot in each tract that can be merged with units that were seen to be newly occupied in 2017. Tract means are the values used for estimation. The first panel in the table reports the means and standard deviations taken across these census tract values. Tract race shares are taken from the ACS (DP05-2017) using percentages defined as white alone (not Hispanic or Latino), Black or African American alone (not Hispanic or Latino), and Hispanic or Latino (of any race). InfoUSA summary statistics are based on a random sample of 10,000 renters in each city drawn from the set of likely renters (defined as having a value of 1-4 out of 10 on InfoUSA's renter/owner prediction scale) who are observed to be moving into a unit in 2017 in one of the census tracts that was part of our experiment. Before taking the random samples, we drop individuals with incomes in the top 5% of the distribution in each city in order to avoid errors in income imputation.

# Table 2. Evidence of Discrimination on Housing Choice by City

| | All Cities | Atlanta | Houston | Philadelphia | Cleveland | San Jose |
|---|---|---|---|---|---|---|
| *Panel A: Relative Response* | | | | | | |
| Minority | 0.9168*** | 0.8715*** | 0.9502 | 0.8684*** | 0.9240*** | 0.9261*** |
| | (0.8982,0.9354) | (0.8090,0.9340) | (0.8795,1.0209) | (0.7930,0.9438) | (0.8852,0.9628) | (0.9035,0.9487) |
| African American | 0.8992*** | 0.8966** | 0.9815 | 0.7778*** | 0.9078*** | 0.8937*** |
| | (0.8768,0.9216) | (0.8243,0.9690) | (0.9012,1.0619) | (0.6875,0.8680) | (0.8604,0.9552) | (0.8655,0.9219) |
| Hispanic | 0.9344*** | 0.8464*** | 0.9188 | 0.9591 | 0.9401** | 0.9585*** |
| | (0.9135,0.9554) | (0.7739,0.9188) | (0.8370,1.0007) | (0.8724,1.0458) | (0.8966,0.9836) | (0.9339,0.9831) |
| *Panel B: Difference in Means* | | | | | | |
| Minority | -0.0361*** | -0.0446*** | -0.0086 | -0.0694*** | -0.0349*** | -0.0511*** |
| | (-0.0442,-0.0280) | (-0.0663,-0.0229) | (-0.0208,0.0036) | (-0.1094,-0.0295) | (-0.0527,-0.0171) | (-0.0668,-0.0355) |
| African American | -0.0437*** | -0.0359** | -0.0032 | -0.1173*** | -0.0423*** | -0.0736*** |
| | (-0.0534,-0.0340) | (-0.0610,-0.0107) | (-0.0171,0.0107) | (-0.1650,-0.0695) | (-0.0641,-0.0205) | (-0.0931,-0.0540) |
| Hispanic | -0.0284*** | -0.0533*** | -0.0140 | -0.0216 | -0.0275** | -0.0287*** |
| | (-0.0375,-0.0193) | (-0.0785,-0.0282) | (-0.0282,0.0001) | (-0.0675,0.0243) | (-0.0475,-0.0075) | (-0.0457,-0.0117) |
| *Panel C: Mean Response Rates* | | | | | | |
| Minority | 0.3975 | 0.3026 | 0.1640 | 0.4583 | 0.4242 | 0.6408 |
| African American | 0.3899 | 0.3113 | 0.1694 | 0.4105 | 0.4168 | 0.6184 |
| Hispanic | 0.4052 | 0.2939 | 0.1586 | 0.5062 | 0.4316 | 0.6633 |
| White | 0.4336 | 0.3472 | 0.1726 | 0.5278 | 0.4591 | 0.6920 |
| Matched Listings | 6,015 | 1,031 | 1,570 | 324 | 1,418 | 1,672 |
| Total Observations | 18,045 | 3,093 | 4,710 | 972 | 4,254 | 5,016 |

Notes: Panel A reports relative response rates, which divide the estimate of mean difference for each group in Panel B by the mean response rate to inquiries from white identities in Panel C and then add 1. Standard errors are clustered by listing. Column 1 of Panel A reports relative response rates for the full sample of listings across all cities. Columns 2-5 of Panel A report estimates of the relative response rates by city. Panel B reports differences in means as in Bertrand and Mullainathan (2004), with standard errors clustered by listing. Panel C reports the mean response rates for each group. 90% confidence intervals are reported in parentheses. Sample sizes are reported for matched inquiry sets (a set includes inquiries sent from all three identities) and total observations where a fully matched set was not obtained. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Discriminatory Constraint by Days on Market

| Group | Relative Response Rates | Difference in Means |
|---|---|---|
| Minority: 0-3 Days | 0.5581*** | -0.1916*** |
| | (0.4528,0.6634) | (-0.2373,-0.1459) |
| Minority: 3-7 Days | 0.6474*** | -0.1529*** |
| | (0.5890,0.7057) | (-0.1782,-0.1276) |
| Minority: 7+ Days | 0.9647*** | -0.0153*** |
| | (0.9441,0.9853) | (-0.0242,-0.0064) |
| Hispanic: 0-3 Days | 0.5519*** | -0.1943*** |
| | (0.4324,0.6713) | (-0.2461,-0.1425) |
| Hispanic: 3-7 Days | 0.6578*** | -0.1484*** |
| | (0.5917,0.7239) | (-0.1770,-0.1197) |
| Hispanic: 7+ Days | 0.9841 | -0.0069 |
| | (0.9608,1.0075) | (-0.0170,0.0032) |
| Black: 0-3 Days | 0.5644*** | -0.1889*** |
| | (0.4439,0.6849) | (-0.2411,-0.1366) |
| Black: 3-7 Days | 0.6369*** | -0.1574*** |
| | (0.5707,0.7032) | (-0.1861,-0.1287) |
| Black: 7+ Days | 0.9453*** | -0.0237*** |
| | (0.9208,0.9698) | (-0.0344,-0.0131) |
| *Mean Response Rates* | | |
| Hispanic: 0-3 Days | 0.2391 | 0.2391 |
| Hispanic: 3-7 Days | 0.2851 | 0.2851 |
| Hispanic: 7+ Days | 0.4266 | 0.4266 |
| Black: 0-3 Days | 0.2446 | 0.2446 |
| Black: 3-7 Days | 0.2760 | 0.2760 |
| Black: 7+ Days | 0.4097 | 0.4097 |
| White: 0-3 Days | 0.3043 | 0.3043 |
| White: 3-7 Days | 0.3167 | 0.3167 |
| White: 7+ Days | 0.4533 | 0.4533 |
| Mean White (Overall) | 0.4336 | 0.4336 |
| Matched Listings | 6,015 | 6,015 |
| Total Observations | 18,045 | 18,045 |

Notes: Left column reports relative response rates, which divide the estimate of the difference in mean response in the right column by the overall mean response rate to inquiries from the white identity (reported below) and then adds 1. Right Column reports differences in means as in Bertrand and Mullainathan (2004), with standard errors clustered by listing. 90% confidence intervals are reported in parentheses. Lower panel reports mean response rates for each group. Sample sizes are reported for matched inquiry sets (a set includes inquiries sent from all three identities) and total observations where a fully matched set was not obtained. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Discriminatory Constraint by Inquiry Sequence

| | Full Sample | White Neighborhoods | Minority Neighborhoods |
|---|---|---|---|
| *Panel A: Relative Response* | | | |
| Minority-1st | 0.8535*** | 0.8418*** | 0.8563*** |
| | (0.8018,0.9052) | (0.7638,0.9199) | (0.7877,0.9249) |
| Minority-2nd | 0.7821*** | 0.7794*** | 0.7805*** |
| | (0.7419,0.8222) | (0.7190,0.8397) | (0.7269,0.8341) |
| Minority-3rd | 0.7177*** | 0.6850*** | 0.7423*** |
| | (0.6778,0.7576) | (0.6246,0.7455) | (0.6894,0.7951) |
| White-2nd | 0.8206*** | 0.8115*** | 0.8215*** |
| | (0.7613,0.8799) | (0.7218,0.9011) | (0.7429,0.9001) |
| White-3rd | 0.7812*** | 0.7653*** | 0.7876*** |
| | (0.7218,0.8406) | (0.6757,0.8550) | (0.7088,0.8663) |
| *Panel B: Difference in Means* | | | |
| Minority-1st | -0.0635*** | -0.0633*** | -0.0671*** |
| | (-0.0859,-0.0411) | (-0.0946,-0.0321) | (-0.0991,-0.0351) |
| Minority-2nd | -0.0945*** | -0.0884*** | -0.1024*** |
| | (-0.1119,-0.0771) | (-0.1126,-0.0642) | (-0.1274,-0.0774) |
| Minority-3rd | -0.1224*** | -0.1262*** | -0.1203*** |
| | (-0.1397,-0.1051) | (-0.1504,-0.1019) | (-0.1449,-0.0956) |
| White-2nd | -0.0778*** | -0.0755*** | -0.0833*** |
| | (-0.1035,-0.0521) | (-0.1114,-0.0396) | (-0.1200,-0.0466) |
| White-3rd | -0.0949*** | -0.0940*** | -0.0991*** |
| | (-0.1206,-0.0691) | (-0.1299,-0.0581) | (-0.1359,-0.0624) |
| *Panel C: Mean Response Rates* | | | |
| Minority-1st | 0.4275 | 0.3927 | 0.4612 |
| Minority-2nd | 0.3965 | 0.3677 | 0.4258 |
| Minority-3rd | 0.3686 | 0.3299 | 0.4080 |
| White-1st | 0.4910 | 0.4560 | 0.5283 |
| White-2nd | 0.4132 | 0.3805 | 0.4450 |
| White-3rd | 0.3962 | 0.3621 | 0.4291 |
| Mean White (Overall) | 0.4336 | 0.4005 | 0.4666 |
| Matched Listings | 6,015 | 3,006 | 3,009 |
| Total Observations | 18,045 | 9,018 | 9,027 |

Notes: Panel A reports relative response rates, which divide the estimate of mean difference for each group in Panel B by the mean response rate to inquiries from white identities in Panel C and add 1. Standard errors are clustered by listing. Column 1 of Panel A reports relative response rates for the full sample of listings. Estimates in Columns 2-3 split the sample into listings in census block groups where the share of white households is above or below the median within the MSA. Panel B reports differences in means as in Bertrand and Mullainathan (2004), with standard errors clustered by listing. Panel C reports the mean response rates for each group. 90% confidence intervals are reported in parentheses. Sample sizes are reported for matched inquiry sets (a set includes inquiries sent from all three identities) and total observations where a fully matched set was not obtained. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Response Prediction: Probit Estimates

| Parameter | White | African American | Hispanic/LatinX |
|---|---|---|---|
| Avg School Quality | 0.0072 | −0.0046 | −00077** |
| | (0.0116) | (0.0078) | (0.0036) |
| Cafes | 0.0037*** | 0.0019*** | 0.0016*** |
| | (0.0008) | (0.0004) | (0.0002) |
| Murders | 0.0000 | 0.0002 | −0.0002*** |
| | (0.0000) | (.0001) | (.0000) |
| ln(RSEI) | 0.1418*** | 0.1679*** | 0.1931*** |
| | (0.0362) | (.0285) | (.0172) |
| % Own Race | 0.2867 | 0.1218 | −0.5528*** |
| | (0.4005) | (.2013) | (.1037) |
| % Own Race$^2$ | −0.2192 | −0.5498*** | 0.8065*** |
| | (0.3688) | (.2128) | (.1527) |
| HOU | 24.9737 | 0.9134*** | 19.7755*** |
| | (16.3197) | (0.3137) | (2.3415) |
| PHL | 15.4763 | 82.6539*** | 71.1384*** |
| | (26.3105) | (27.4140) | (17.5649) |
| CLE | −115.7315 | 56.0436 | −31.5169*** |
| | (71.4859) | (34.8997) | (9.8585) |
| SJC | 126.1653** | 136.1588*** | 63.9292*** |
| | (63.6383) | (46.6048) | (20.5339) |
| Latitude (ATL) | −0.2441* | −0.3241*** | 0.0111*** |
| | (0.1390) | (0.0975) | (0.0024) |
| Longitude (ATL) | −0.7088*** | −0.6155*** | −0.9415*** |
| | (0.1227) | (0.0965) | (0.0428) |
| Latitude (HOU) | −0.3440*** | −0.4312*** | −0.3600*** |
| | (0.1003) | (0.0621) | (0.0242) |
| Longitude (HOU) | −0.3755*** | −0.5433*** | −0.7301*** |
| | (0.1407) | (0.0900) | (0.0542) |
| Latitude (PHL) | −0.2006 | 0.1458 | 1.5317*** |
| | (0.5744) | (0.3722) | (0.2335) |
| Longitude (PHL) | −0.5871* | −0.6335** | 0.6973*** |
| | (0.3028) | (0.2620) | (0.1204) |
| Latitude (CLE) | 2.1861** | 0.1885 | 1.9266*** |
| | (0.9726) | (0.5052) | (0.1300) |
| Longitude (CLE) | −0.9392** | 0.2800 | −0.3816*** |
| | (0.4715) | (0.2613) | (0.0888) |
| Latitude (SJC) | 0.5761 | 0.9592 | −0.2291 |
| | (1.0231) | (0.5978) | (0.1539) |
| Longitude (SJC) | 0.7770 | 1.0644** | −0.2136 |
| | (0.7142) | (0.4669) | (0.1492) |
| Constant | −53.0164*** | −42.5127*** | −81.5116*** |
| | (12.0950) | (8.3109) | (3.6730) |

Notes: Table reports parameter estimates from first stage probit model from Eq. 2. Columns 1, 2, 3 report coefficient estimates for African American, Hispanic/LatinX, and white response rates, respectively. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6. Parameter Estimates

| Parameter | Consideration Sets | WTP/Non-Housing Exp |
|---|---|---|
| ln(I-R) | 1.3117*** | |
| | (0.0211) | |
| Avg School Quality | 0.0350*** | 0.0270*** |
| | (0.013) | (0.0099) |
| Cafes | 0.0092*** | 0.0070*** |
| | (0.0008) | (0.0006) |
| Murders | −0.0015*** | −.0011*** |
| | (0.0001) | (.0001) |
| ln(RSEI) | −0.3366*** | −0.2566*** |
| | (0.0403) | (0.0305) |
| White $*$% White | 0.0836*** | |
| | (0.0060) | |
| White $*$% White$^2$ | −0.0007*** | |
| | (0.0001) | |
| African American $*$% African American | 0.1035*** | |
| | (0.0045) | |
| African American $*$% African American$^2$ | −0.0006*** | |
| | (0.0001) | |
| LatinX $*$% LatinX | 0.0805*** | |
| | (0.0035) | |
| LatinX $*$% LatinX$^2$ | −0.0008*** | |
| | (0.0001) | |
| ATL $*$Latitude | 2.4804*** | |
| | (0.2468) | |
| ATL $*$Longitude | −4.0436*** | |
| | (0.3387) | |
| HOU $*$Latitude | 1.2489*** | |
| | (0.1463) | |
| HOU $*$Longitude | 0.6214*** | |
| | (0.1778) | |
| PHL $*$Latitude | 0.6213 | |
| | (0.6266) | |
| PHL $*$Longitude | −0.4489 | |
| | (0.3606) | |
| CLE $*$Latitude | −3.2345** | |
| | (1.3534) | |
| CLE $*$Longitude | 1.2826** | |
| | (0.6192) | |
| SJC $*$Latitude | 2.3260*** | |
| | (0.4715) | |
| SJC $*$Longitude | −1.2245*** | |
| | (0.3367) | |

Notes: Table reports parameter estimates from baseline model specification with consideration sets. Column 1 reports coefficient estimates. Column 2 reports estimates of willingness-to-pay as a share of non-housing expenditures. Standard errors in both columns are generated using 200 random bootstrap draws of both the first-stage (i.e., predicted response probabilities) and second- (i.e., residential location choice) stage models. *** p<0.01, ** p<0.05, * p<0.1.

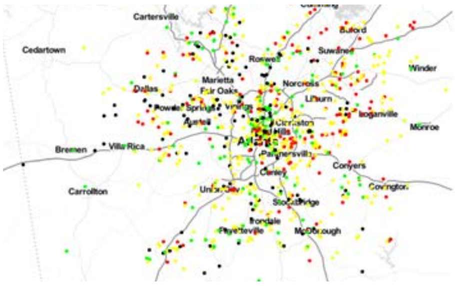
Table 7. Bias in Willingness to Pay

| Variable | Consideration Sets | No Consideration Sets |
|---|---|---|
| ln(I-R) | 1.3124*** | 1.2784*** |
| | (0.0266) | (0.0236) |
| Avg School Quality | 0.0452*** | 0.0575*** |
| | (0.0148) | (0.0039) |
| Cafes | 0.0099*** | 0.0141*** |
| | (0.0010) | (0.0003) |
| Murders | −0.0009*** | −0.0008*** |
| | (0.0001) | (0.0001) |
| ln(RSEI) | −0.3228** | −0.1021*** |
| | (0.0.0513) | (0.0070) |
| ln(I-R) x ROC | 0.0804* | 0.0856** |
| | (0.0431) | (0.0401) |
| Avg School Quality x ROC | −0.0173 | −0.0279*** |
| | (0.0178) | (0.0082) |
| Cafes x ROC | −0.0028** | −0.0040*** |
| | (0.0013) | (0.0007) |
| Murders x ROC | −0.0010*** | −0.0008*** |
| | (0.0002) | (0.0001) |
| ln(RSEI) x ROC | −0.0746 | −0.0005 |
| | (0.0589) | (0.0178) |

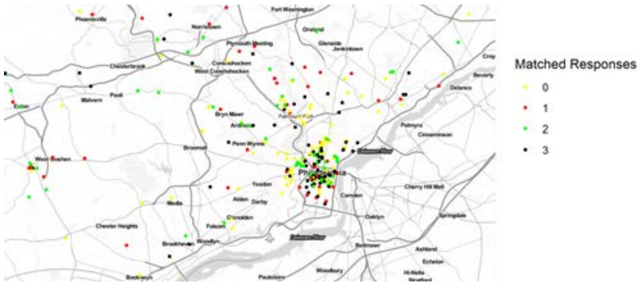
| | White[WTP(NCS)-WTP(CS)]-ROC[WTP(NCS)-WTP(CS)] |
|---|---|
| Avg School Quality | 0.3907 |
| | (0.4811) |
| Cafes | 0.0812** |
| | (0.0317) |
| Murders | 0.0023 |
| | (0.0048) |
| ln(RSEI) | 0.0110 |
| | (0.0118) |

Notes: Table reports differences in parameter estimates and estimated willingness to pay from model specifications that include/omit consideration sets using experimentally identified discriminatory constraints and allow for heterogeneity by race (i.e., white renters v. renters of color). Upper panel reports parameter estimates from model with consideration sets (left column) and without consideration sets (right column), with standard errors generated from 100 random bootstrap draws of both the first- (i.e., predicted response probabilities) and second- (i.e., residential location choice) stage models. Parameter estimates for the full set of control variables are reported in Appendix H. Lower panel reports differences in differences in willingnesses to pay across race (white and renters of color) and without (NCS) and with (CS) consideration sets. Willingness to pay for school quality, cafes, and murders is calculated as the ratio of utility parameters multiplied by income net of housing expenditures at each individual's observed housing choice. WTP calculation for $ln(RSEI)$ divides this product by the $RSEI$ level at the individual's observed housing choice. Standard errors for the difference in ratios are generated from the boostrapped parameter values using the following procedure: (1) for each bootstrap draw, the average difference for white renters across all cities is calculated along with the average difference for renters of color, (2) the difference in those two average values is calculated, (3) the mean of this difference is calculated for the full set of bootstrap draws. The reported standard error is the standard deviation taken over the set of bootstrap draws. *** p<0.01, ** p<0.05, * p<0.1.

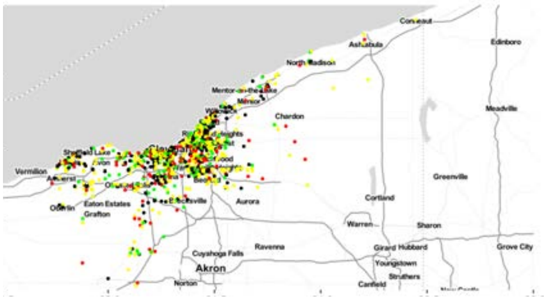
Figure 1. Within-Listing Response Differential by MSA



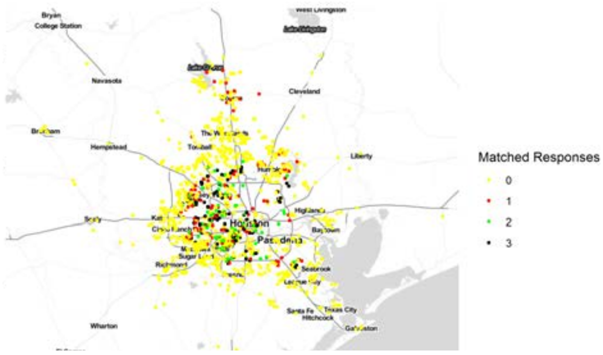
(a) Atlanta, GA (n=3,093)



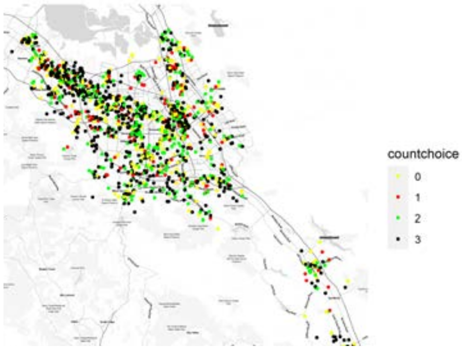

(b) Philadelphia, PA (n=972)



(c) Cleveland, OH (n=4,254)



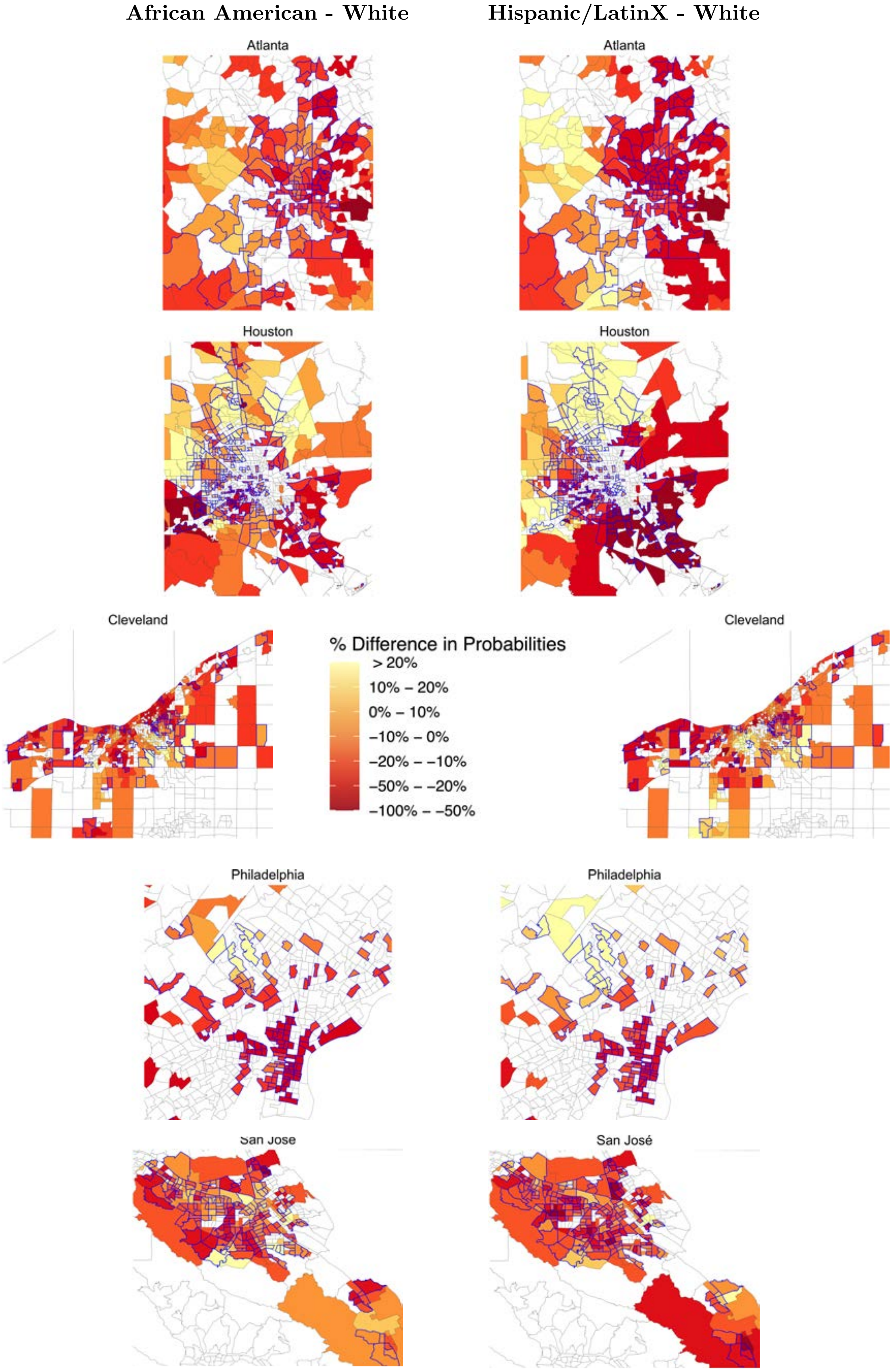

(d) Houston, TX (n=4,710)




(e) San Jose, CA (n=5,016)

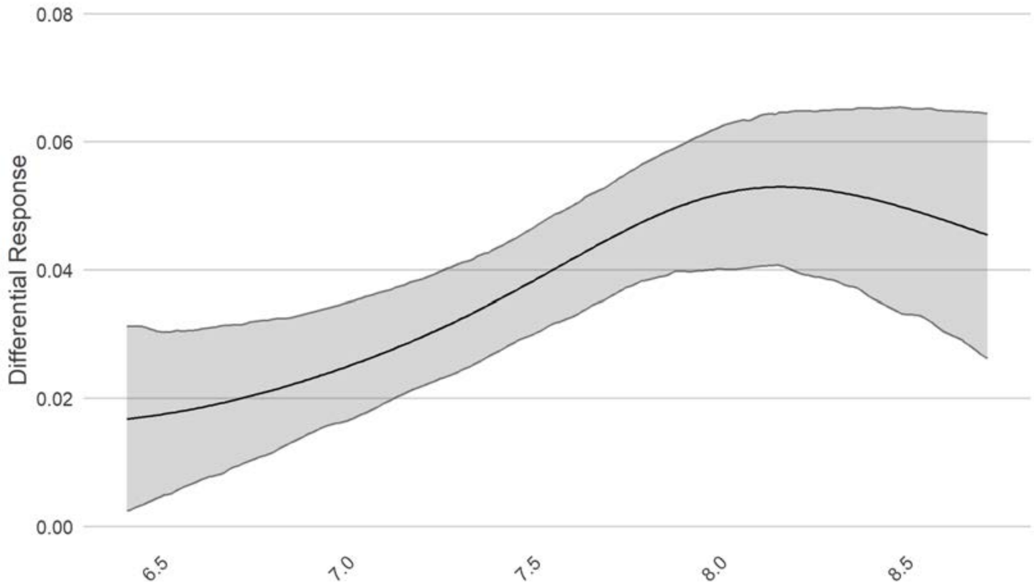
Notes: Figures map the raw data from matched response sets for the five Metropolitan Statistical Areas in the experiment. Matched responses refer to the number of responses returned from a single property over the course of the 3-day trial.

Figure 2. Differential Responses by Tract

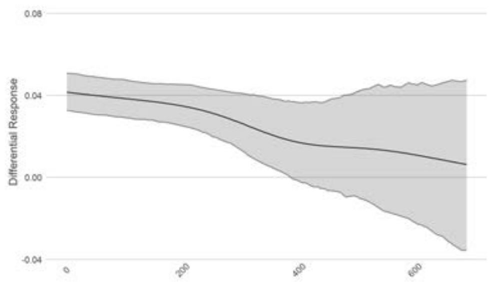



Notes: Figures map the average difference in response rates to African American and Hispanic/LatinX identities (relative to white) in each census tract for the five Metropolitan Statistical Areas in the experiment. Census tracts where renters are observed to move in 2018 are included in the sample and colored. Census tracts that contain information from the experimental trials are shown with blue borders. All other tracts do not contain data on renter moves/response rates and are omitted from the structural model.

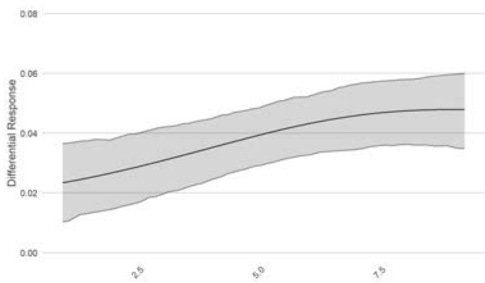
Figure 3. Discriminatory Constraints by Attribute (White - Renter of Color)



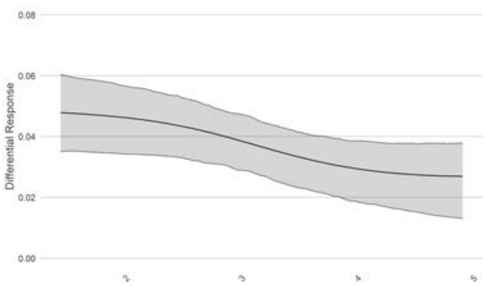

**A: Rental Price**



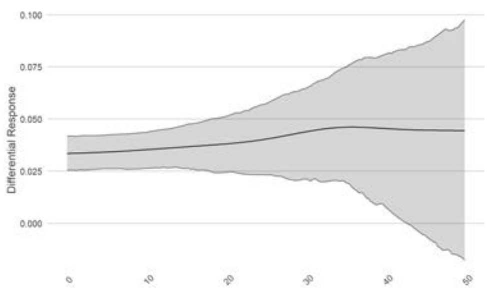

B: Murder Index




C: Average School Rating




D: Toxic Concentrations (RSEI)

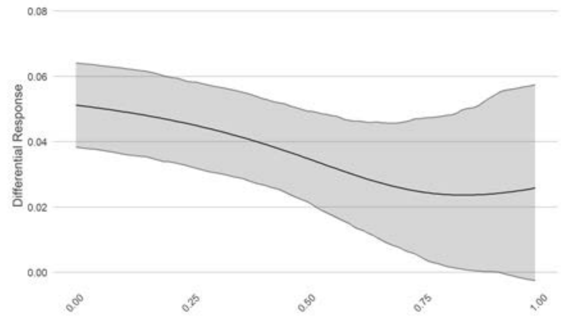



E: Cafes

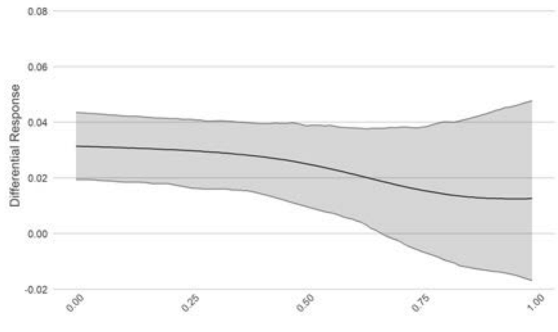
Notes: Figures plot differential constraints for renters of color (African American and Hispanic/LatinX) relative to white by attributes observed in rental listings collected in the experiment using estimates from Eq. 1. Increases in differential response correspond to increases in discriminatory constraints. Rental prices and other characteristics are observed at the property level (listings). Toxics concentrations come from the EPA RSEI model and are provided at the location of each property.

Figure 4. Response Rates by Neighborhood Demographic Shares

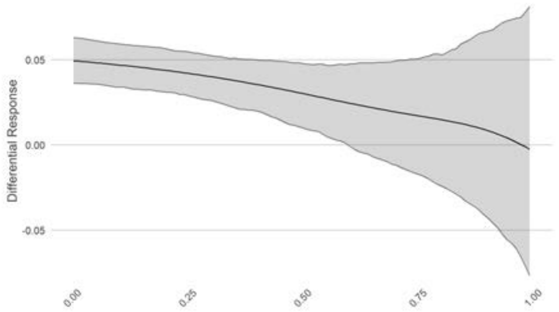
**African American** **Hispanic/LatinX**



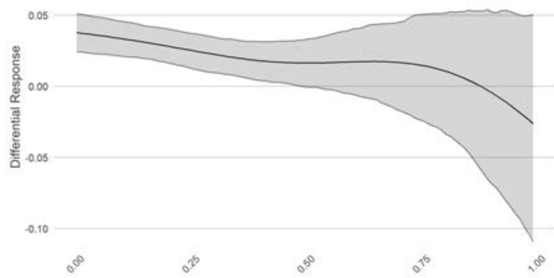

(a) African American Share



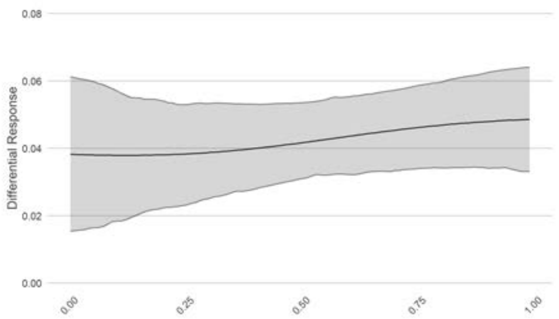

(b) African American Share



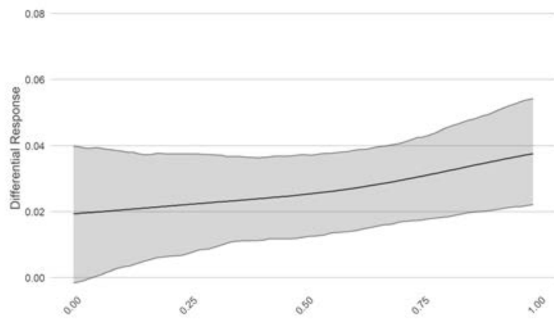

(c) Hispanic/LatinX Share




(d) Hispanic/LatinX Share



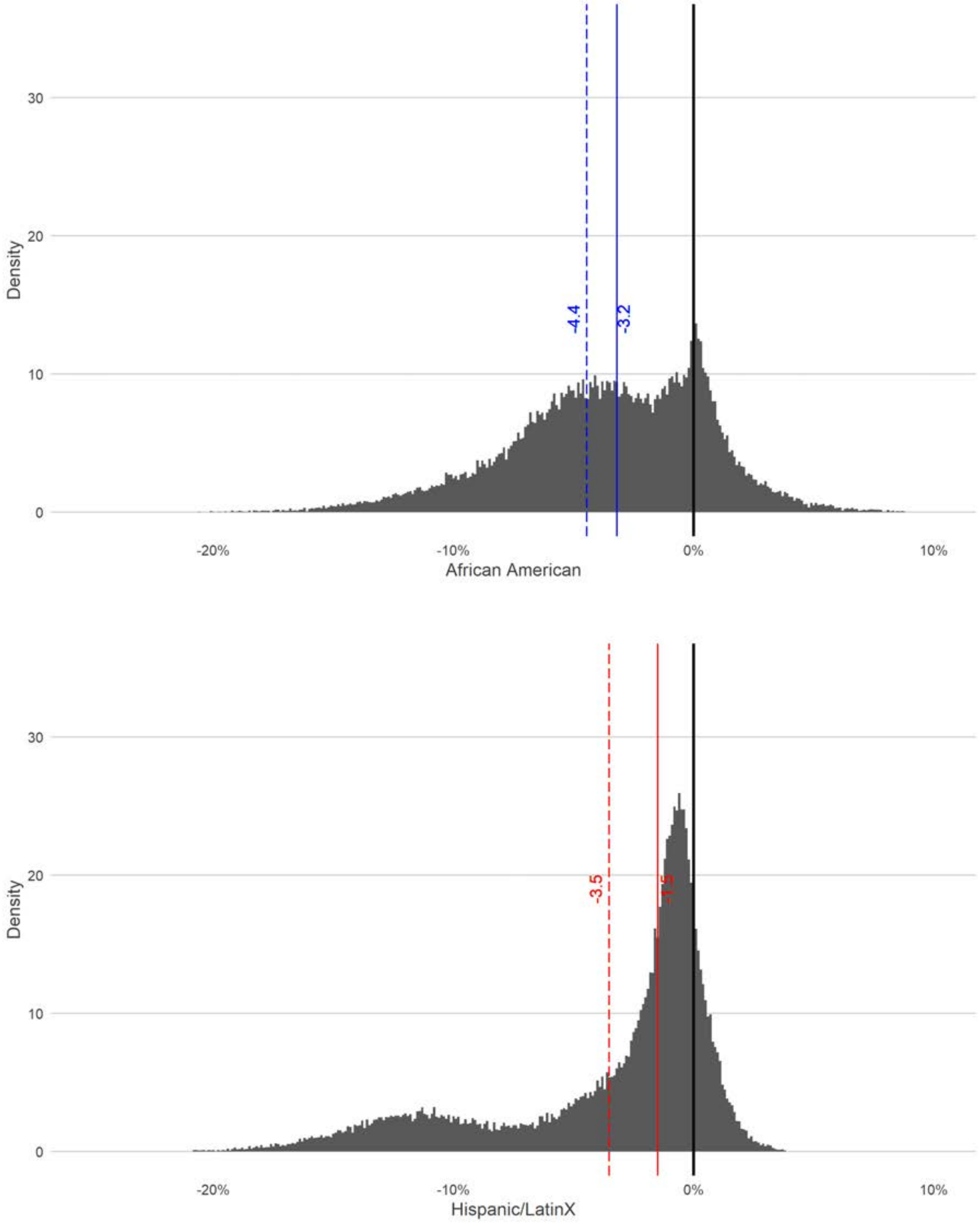

(e) White Share




(f) White Share

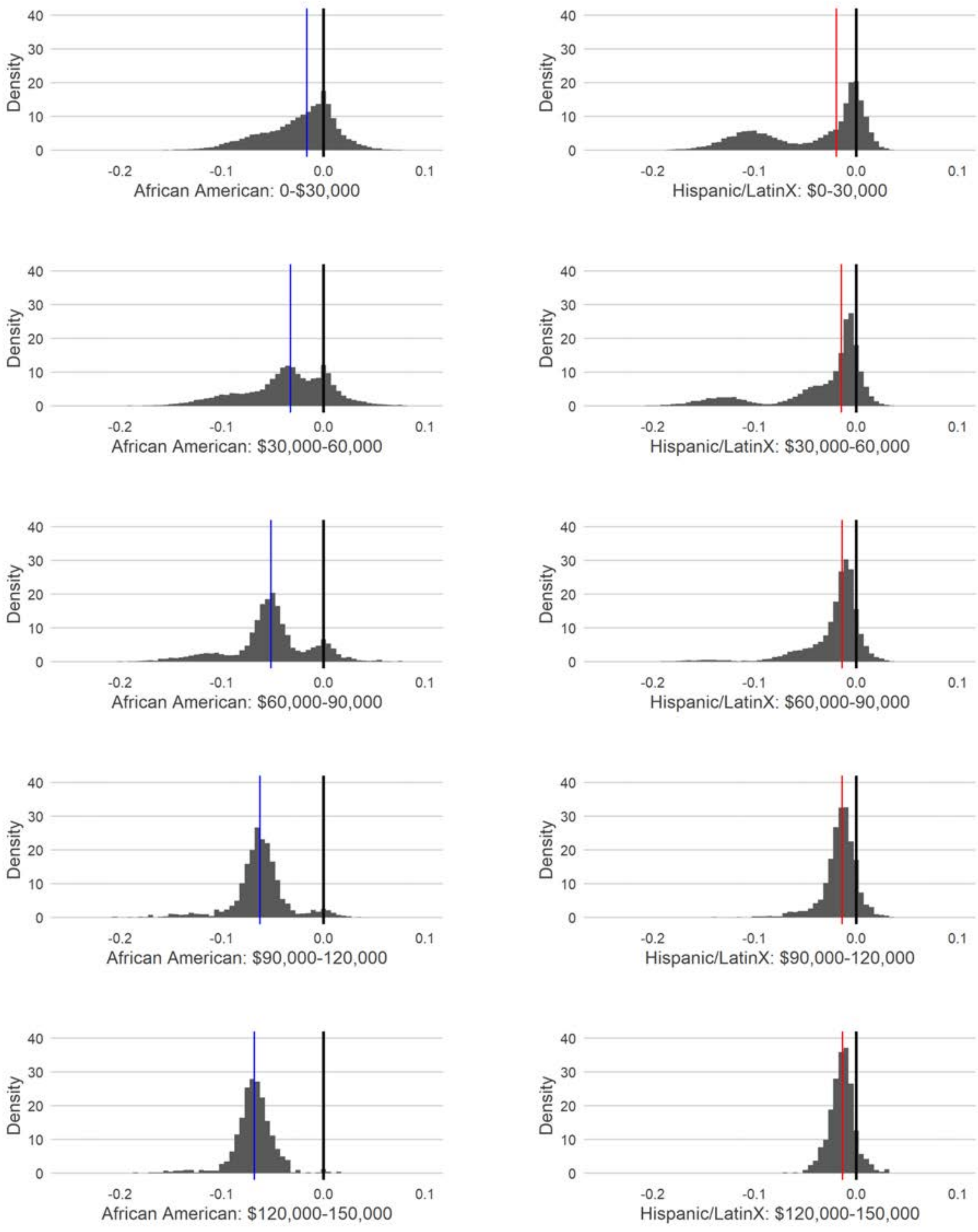
Notes: Figures plot differential constraints for renters of color (African American and Hispanic/LatinX) relative to white renters by ethnic/racial composition using estimates from Eq. 1. Increases in differential response correspond to increases in discriminatory constraints. Racial composition is observed at the block group level using the 2013-2017 American Community Survey.

Figure 5. Annualized Damages as a Share of Annual Income (Equivalent Variation)



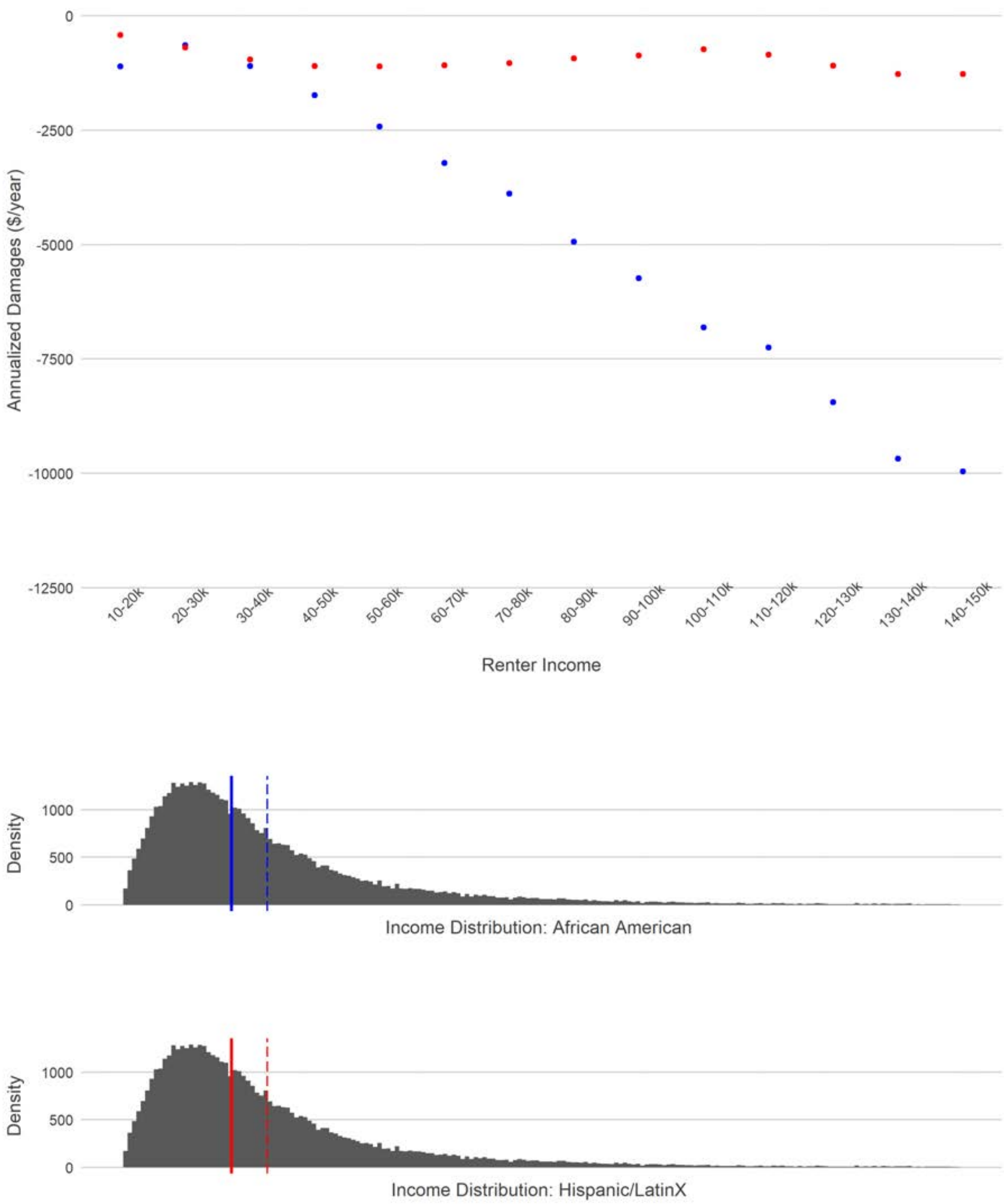

Note: The figure graphs the distribution of renter welfare effects (equivalent variation as a share of renter income) resulting from discriminatory constraints as measured by the correspondence experiment. Plots illustrate damages to African American renters (top) and Hispanic/LatinX renters (bottom), with solid vertical lines denoting the median and dashed vertical lines denoting the mean estimates.

Figure 6. Annualized Damages by Income Group (Equivalent Variation)




Note: Graphs plot the distributions of renter welfare effects (equivalent variation as a share of renter income) at different levels of renter income. Each plot illustrates a distribution of effects for the following renter income groups: $0-30,000, $30,000-60,000, $60,000-90,000, $90,000-120,000, $120,000-150,000. Blue and red vertical lines denote median effects for African American renters and Hispanic/LatinX renters, respectively.

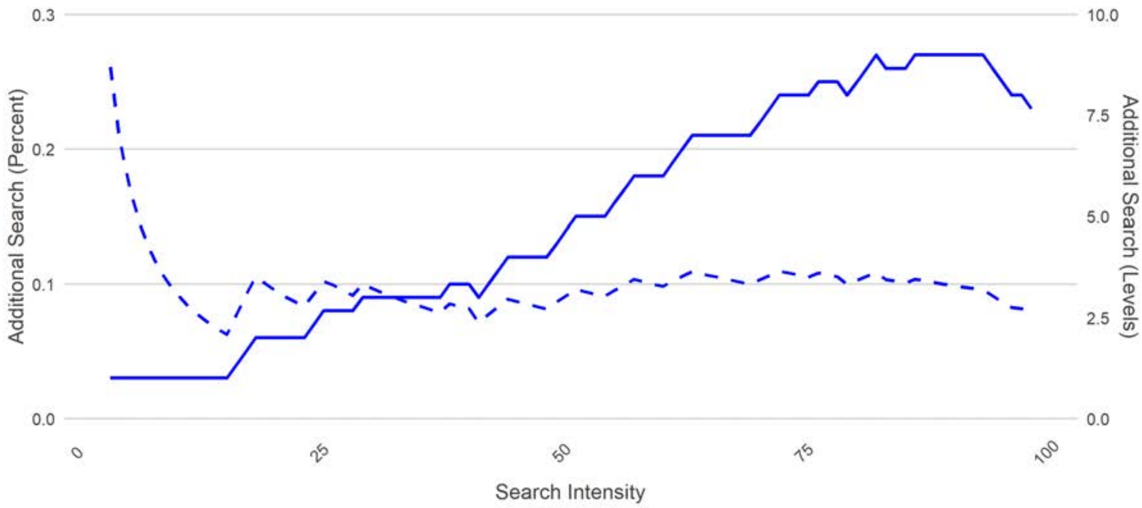
Figure 7. Annualized Damages by Income (Equivalent Variation)



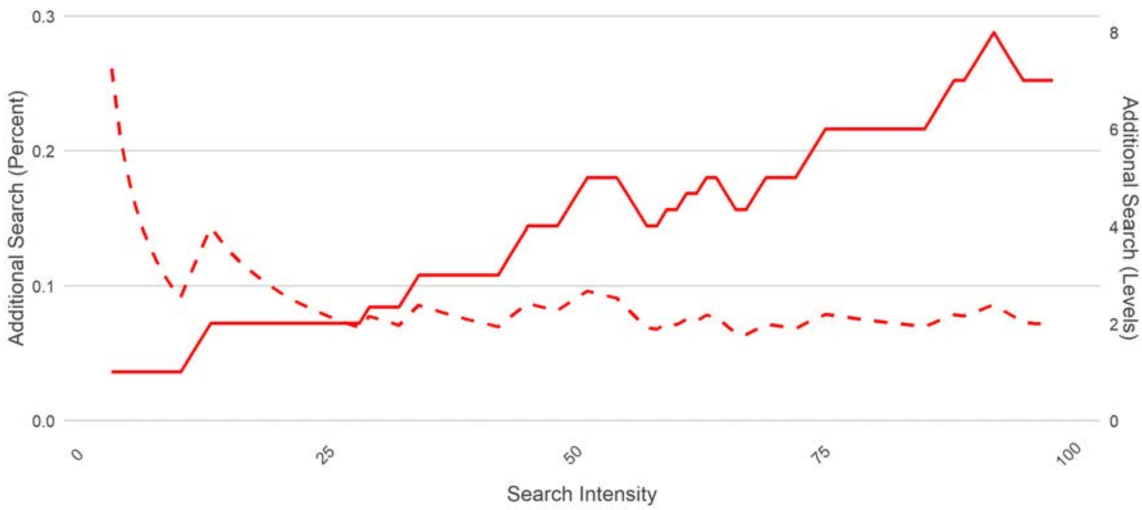

Note: Top panel graphs the distribution of renter welfare effects (equivalent variation in dollars per year) resulting from discriminatory constraints as measured by the correspondence experiment. Blue points show effects for African American renters and red points plot effects for Hispanic/LatinX renters. Bottom panels graph the distribution of renter incomes from the InfoUSA renter sample.

Figure 8. Search Intensity

(a) African American



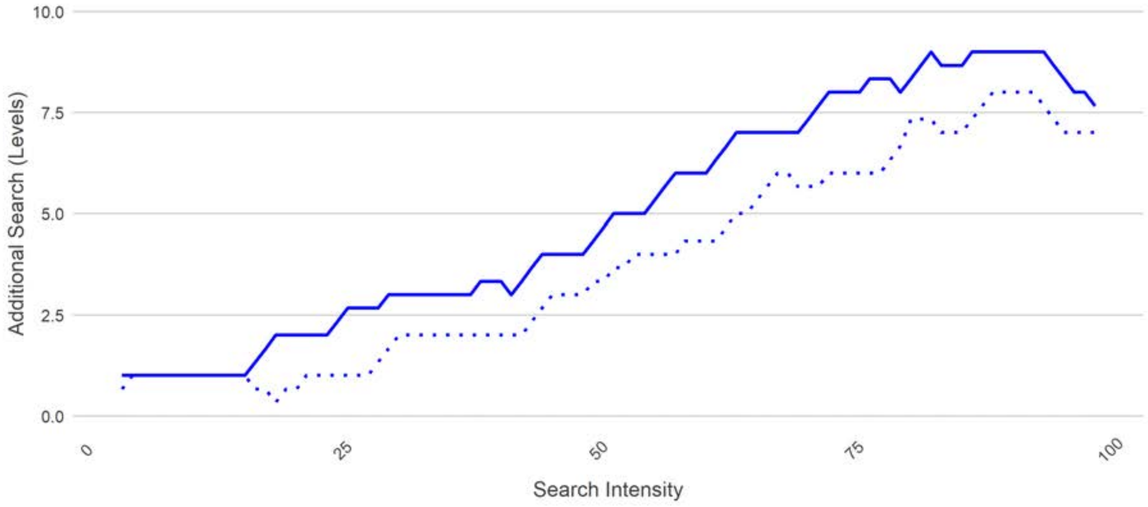

(b) Hispanic/LatinX



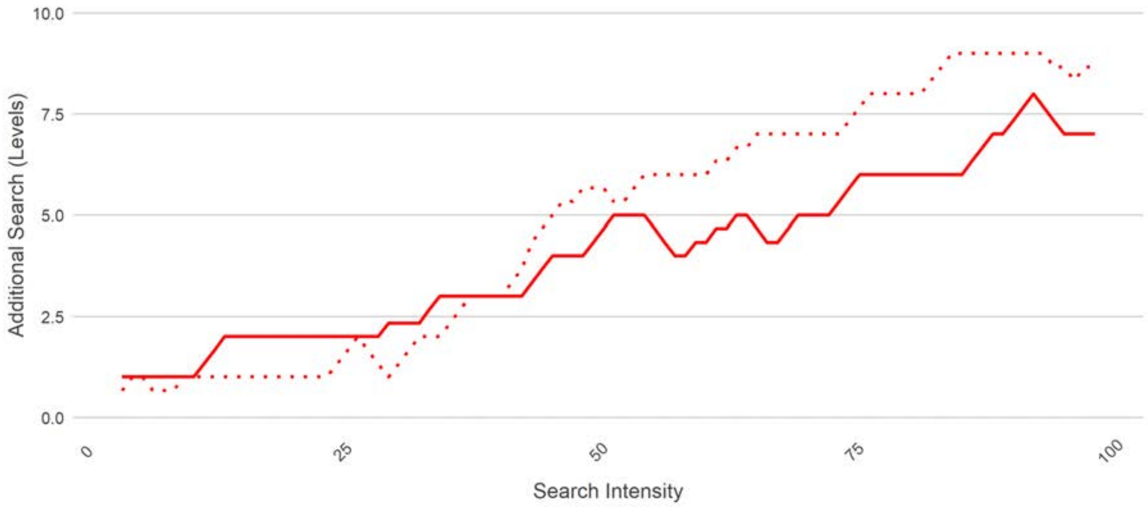

Note: Graph plots results of 25,000 simulations of additional search required by an African American (top panel) or Hispanic/LatinX (bottom panel) renter to obtain the same expected utility as an equivalent white renter at different levels of search intensity. Search intensity is normalized across the 5 cities using binned percentiles of the number of tracts in each market (x-axis). For example, Atlanta has 129 tracts, so 1% of the city searched would be $INT(0.01 * 129) = 1$ tract, while 4% of Houston, with its 322 tracts, would be $INT(0.04 * 322) = 13$ tracts. Dashed line measures the median search cost differential in percent terms (left axis) and solid line measures the median search cost differential in level (right axis).

Figure 9. Search Intensity: Strategic Avoidance

(a) African American




(b) Hispanic/LatinX




Note: Graph plots results of 25,000 simulations of additional search required by an African American (top panel) or Hispanic/LatinX (bottom panel) renter to obtain the same expected utility as an equivalent white renter at different levels of search intensity. Search intensity is normalized across the 5 cities using binned percentiles of the number of tracts in each market (x-axis). For example, Atlanta has 129 tracts, so 1% of the city searched would be $INT(0.01 * 129) = 1$ tract, while 4% of Houston, with its 322 tracts, would be $INT(0.04 * 322) = 13$ tracts. Solid lines reproduce the median search cost differential in levels (right axis) for a renter with average income in each city from Figure 8. Dotted lines provide the same measure from a simulation that assumes minority renters can strategically avoid discrimination by searching in tracts that maximize expected utility conditional on the probability of a response.

# Appendix:
# The Damages and Distortions from Discrimination in the Rental Housing Market

Peter Christensen and Christopher Timmins

December 14, 2022

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# Appendix

## Table of Contents

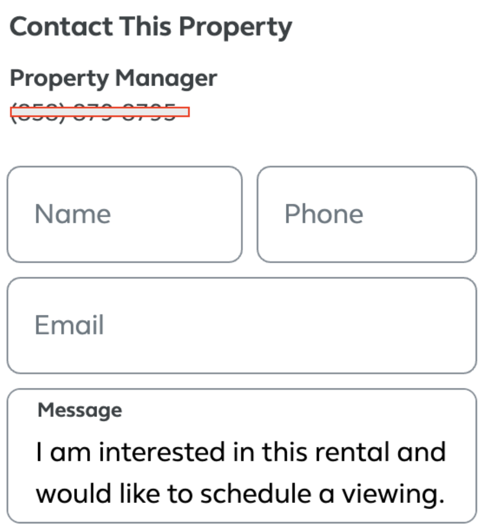
# A Experimental Design

In a correspondence experiment, a researcher elicits racialized perceptions in a trial by constructing fictitious identities and experimentally varying a single trait (Bertrand and Duflo, 2017). The literature in the housing market has generated several different correspondence research designs. In this section, we describe key differences and their implications for response rates and detection.

## Research Design: Inquiries

The platform used in the present study transmits automated inquiries using standardized fillable forms, which has become the default for sending inquiries on many digital housing search platforms.

Figure A.1. Examplar Inquiry Form




In the present study, inquiries are sent using the fillable form above, where prospective renters submit 3 pieces of information: (1) prospective renter's name, (2) prospective renter's phone number, (3) prospective renter's email address, (4) a message.

### Name

1 of 16 randomly assigned first-last name pairs is assigned in any given inquiry (see Appendix A for details on name selection).

### Phone Number

Each first-last name pair is associated with a single phone number. The full set of phone numbers comes from a small set of area codes associated with the NYC metro area, which is distinct from the metro areas in the sample. This is done to maximize the anonymity of identities in the study and reduce the likelihood of any differential signal extracted from specific area codes.

### Phone Number

Each first-last name pair is associated with a single gmail address. The full set of gmail addresses were constructed from first-last name pairs plus a series of randomly selected additional alphanumeric characters to guarantee availability.

### Message

All inquiries were sent with the default message provided on the site: "I am interested in this rental and would like to schedule a viewing."

## Research Design: Forms vs. Email Correspondence

The digital housing search market has evolved considerably over the past decade. The use of default fillable forms differs from correspondence studies that were designed during the first generation of research on digital platforms. At that time, email correspondence was the primary form of inquiry available to renters searching for housing on digital platforms (i.e. Craigslist) and therefore the primary option available to a correspondence researcher. Researchers primarily used emails with researcher-specified standardized messages. For instance, a prominent study by Hanson and Hawley (2011) generated different types of messages that were designed to imply class status:

<u>High-Class Message</u>

Hi there,

I'm interested in the rental you posted on Craigslist, would you tell me if it is still available? If you need them, I have good references and I could also send a recent credit report.

Thanks for your time.

Sincerely,
First and last name of home-seeker

<u>Low-Class Message</u>

Hi,

I saw the place on the internet. Is the place still avialbe? Do you need references or credit scores? I can send those if you want.

C U Later,
First and last name of home-seeker

The language chosen in researcher-specified messages has implications for the interpretation of estimates from correspondence studies, since any given choice of words will become part of the information signalled about renter attributes or qualifications. In the absence of information of the linguistic patterns commonly used by a given group on any given site, the internal validity of studies using researcher-specified messages depends on the extent to which a standardized message conveys equivalent information about each of the groups in the study *or* that any differences are controlled by a researcher.

A second challenge involves the potential likelihood of detection by property managers who receive more than one inquiry with the same language in a message. Detection risk with any researcher-specified message limits the scope for research designs that use repeat inquiries to capture discriminatory behavior at the property level.

The use of default fillable forms allows a researcher to minimize potential salience effects from specific language used in researcher-formulated messages. However, the researcher must consider whether using a fillable form itself signals something about renter attributes/qualifications. In particular, a correspondence researcher must assume that an inquiry submitted using a fillable form sends and equivalent signal when received by each group in a study. This assumption may be the most plausible on platforms where fillable forms are widely/commonly used, minimizing the likelihood the choice will convey information to a property manager.

The use of a more anonymous channel could also have important effects on baseline response rates since it involves a lower level of commitment on the part of the prospective renter. Indeed, the baseline response rate to inquiries sent from white identities in our study (43.4%) is lower than the response rate to white inquiries from personalized email inquiries – (57.12%) in Hanson and Hawley (2011) and (58.1%) in Ewens, Tomlin, and Wang (2014).

## Detection

Correspondence studies rely on the assumption that online search tools will protect the anonymity of fictitious identities. We highlight the several features of the current design that were implemented to avoid detection:

1. Fillable forms: Our study was implemented using communication (inquiries) through fillable forms on one of the largest housing search platforms in the United States. The forms do not rely on researcher-specified messages and provide 3 pieces of information to property managers: name, email, phone number.

2. Repeat Interactions: Different properties on the platform may be managed by same property manager, increasing the likelihood that a manager will receive multiple inquiries from the same identity. In the present study, inquiries sent to the same property were also sent on separate days. There are three different factors that interact to determine the likelihood that repeat-interactions result in detection and affect the interpretation of estimates in a correspondence study: (A) the probability that a property manager receives multiple inquiries from the same identity (name/email/phone number), (B) conditional on (A), the probability that this event becomes salient in the mind of the property manager, (C) conditional on A-B, the probability that this alters the property manager's likelihood of response, (D) conditional on A-C, the probability that this this alters the property manager's likelihood of response differently for one group relative to another (discriminatory detection). We discuss the each of these factors vis-a-vis the features of our design and the marketplace that we study:

(A) Consistent with other studies in the correspondence literature, we randomly assign inquiries from a set of 6 names per race/ethnic group, such that the probability that a property manager who is managing 2 simultaneous listings has a 16.7% probability of interacting with the same identity. The likelihood that a property manager that manages 2 simultaneous listings interacts with the repeat names from 2 different identity groups is 8.3%. In Table B2, we compare the relative response rates obtained using the full sample with within-listing variation to the relative response rates obtained using only the sample of responses from the 1st inquiry made to a given listing, which are less likely to be subject to repeat-interactions. We do not find a statistical difference in these estimates.

(B) The probability of repeat-interactions with the same identity is relatively low in the present study, though it is not zero. Even so, it is not clear that interacting with the same identity would be highly salient event in the mind of a property manager in the online rental housing marketplace. Prospective renters are routinely making many inquiries to several different properties in the same search session/day/week. As a result, it is likely that property managers with multiple listings in the same neighborhood or with similar characteristics receive repeat-inquiries from the same (authentic) prospective renter. Repeat-interactions may become more salient in geographies or market segment where certain groups are less well-represented.

(C) In the case where a property manager did wonder about the authenticity of an inquiry, they would presumably evaluate the payoff from responding to a fictitious applicant against the cost of not responding to an inquiry that was thought to be fictitious but turned out to be authentic (false positive).

(D) If A-C occur, this could result in a reduction in response rates to all groups. If A-C occur differentially for certain groups relative to others, then detection would bias estimates of relative response rates. In other words, detection could be discriminatory or the behavioral response to detection could be discriminatory.

3. Online Research: One concern that arises in both audit and correspondence studies is the potential for those being audited to check the online profile of the tester or fictitious applicant. Our correspondence design focuses on the first contact in a housing search, where the returns to learning about a respondent are low. We might expect online research to occur in later stages of contact or in other markets such as the labor market, where hiring managers may have more information and may be more inclined to conduct research on an applicant.

4. Distinct Names: Our study utilizes names that are sampled from the highest percentiles of the distribution of each of three racial groups. By construction, these very common names will be linked to many possible online identities. For example, if a property manager were to conduct a google search of one of our fictitious identities, they would retrieve results like: this example. It is likely that a large fraction of the renter population also has a weak online presence. We assume that the likelihood that property managers will be affected by (the absence of) identifiable online information is low.

## Name Selection in the Current Study

### Correspondence Design and the InfoUSA Renter Population

The correspondence research design allows a researcher to experimentally vary a single, specific trait in an interaction. In work on racial/ethnic discrimination, the researcher varies information to elicits a cognitive association with a given racial/ethnic group (Bertrand and Duflo, 2017). The researcher then measures the impact of the cognitive bias triggered by variation in the trait on an outcome of interest, such as a housing choice. Most correspondence studies in housing and labor markets use racially distinct names, a the trait that is a typical component of correspondence in a digital market transaction been repeatedly shown to elicit racialized perceptions. While there are limitations associated with the use of any given trait, the consistent use of this trait has enabled researchers to learn about racial perceptions of names across markets, research designs, and in the general population. Multiple randomized experiments have focused exclusively on measuring the congruence between perceived associations with an ethnic/racial group and self-identified racial identity (Crabtree and Chykina, 2018, Gaddis, 2017, 2018).

The internal validity of a correspondence design depends crucially on the ability to trigger a cognitive associations with a given racial/ethnic identity using variation in a single trait, such that any resulting differences in behavior can be attributed to racialized perceptions. A researcher therefore maximizes the internal validity of a correspondence study by selecting names that are strongly associated with racialized perceptions. Racially distinct names are (by construction) not representative of the group-specific distribution of names found in the population. More general inferences about discriminatory behavior in the associated market depend on the following assumptions: (1) the behavioral response to a randomly varied trait in a correspondence study is determined by an underlying system of social-cognitive categories that a subject associates with racial/ethnic identities and (2) the behavioral response triggered by racial categories (stereotyping) is not specific to the particular trait that triggered it. In other words, the relationship between cognitive bias and the resulting behavioral response is not assumed to operate through any specific trait in particular, but rather through deeper cognitive structures that respond to a broad class of racialized traits or characteristics.

While these assumptions have not been tested in the context of correspondence research, they are consistent with a large body of research on racial bias in social and cognitive psychology. A long-standing literature studying implicit bias using instruments such as the implicit association tests (IAT) has found that a range of racial cues elicit automatic social categorization, irrespective of the relevance of categories for a given task (Yamaguchi and Beattie, 2020). Through steroetyping, implicit attitudes and biases that are triggered by trait-based cues linked to salient racial categories predict behavioral responses in inter-group interactions (Maina et al., 2018), selection tasks in laboratory studies (Beattie, Cohen, and McGuire, 2013) and impacts on economic outcomes such as worker productivity, student performance, and legal decisions (Carlana, 2019, Arnold, Dobbie, and Yang, 2018, Glover, Pallais, and Pariente, 2017).

Can estimates from a correspondence study be used to make inferences about constraints facing the median renter in a given group? The assumption made when integrating the results from a correspondence study with data on observed outcomes is that the discriminatory behavior measured using the correspondence method will capture the behavioral responses that result from implicit/explicit categorization through stereotyping. While the correspondence design triggers racialized perceptions by revealing a specific trait in a single initial communication, the assumption is that a consistent behavioral response would occur at the stage in a market transaction where cues about racial identity are revealed.

### Name Selection in the Current Study

First names are taken from the work of Gaddis (2017, 2018), which experimentally tests for congruence between the statistical distribution in birth records and the probability of external classification by survey respondents. Gaddis describes the selection procedure:

> "I selected names for this study using New York state birth record data for all births from 1994 to 2012 obtained from the New York State Department of Health to examine population-level race and SES characteristics. These data separately list the total number of births by (1) name and mother's race and (2) name and mother's education. This data structure allowed me, for example, to choose two names similar in terms of mother's race but different in terms of mother's education-in other words, a black lower-SES name and a black middle- to upper-SES name. Two examples used in this study are DaQuan and Jabari; 91.8 percent of children named DaQuan and 92.1 percent of children named Jabari are born to black mothers. These names are equal in blackness but vary by mother's education; only 12.8 percent of mothers who name their child DaQuan have some college or more education, whereas 56.8 percent of mothers who name their child Jabari have some college or more education. Additionally, when possible, I selected names that were used in previous or ongoing audit studies from different disciplines (e.g., Bertrand and Mullainathan 2004; Gaddis 2015; Milkman et al. 2012)."

Gaddis finds congruence rates of 75%, 75.5%, and 87.3%, respectively, for African American, Hispanic, and white first names. When last names are included, Gaddis finds that congruence rates increase to 82.5%, 97.3%, and 92.4% for African American, Hispanic, and white first-last name pairs, respectively. Gaddis also shows that congruence rates for all groups decline when first names are (mis)matched with last names from a different group. Based on this evidence, we select first-last name pairs that are shown to have a high probability of eliciting racially congruent perceptions. Panel A of Table A1 reports the identification rates from Gaddis (2017, 2018) for the specific subset of first names that we use in the present study. In the study, we use the following first-last name pairs: Nia Harris, Jalen Jackson, Ebony James, Lamar Williams, Shanice Thomas, DaQuan Robinson, Isabella Lopez, Jorge Rodriguez, Mariana Morales, Pedro Sanchez, Jimena Ramirez, Luis Torres, Aubrey Murphy, Caleb Peterson, Erica Cox, Charlie Myers, Leslie Wood, Ronnie Miller. In every case, congruence rates increase with the inclusion of a correctly matched last name.

## Table A1. Identification Rates for First Names and Last Name Frequencies

| Panel A. Identification Rates from Gaddis (2017, 2018) (%) | | | | |
|---|---|---|---|---|
| Race | First Name | No Last Name | Last Name Included | Quartile mother's education |
| African American | Nia | 41 | 65 | High |
| African American | Jalen | 63 | 71 | High |
| African American | Ebony | 91 | 95 | Med |
| African American | Lamar | 88 | 94 | Med |
| African American | Shanice | 93 | 92 | Low |
| African American | DaQuan | 91 | 96 | Low |
| Hispanic/LatinX | Isabella | 48 | 98 | High |
| Hispanic/LatinX | Jorge | 86 | 98 | High |
| Hispanic/LatinX | Mariana | 78 | 99 | Med |
| Hispanic/LatinX | Pedro | 98 | 99 | Med |
| Hispanic/LatinX | Jimena | 49 | 97 | Low |
| Hispanic/LatinX | Luis | 83 | 99 | Low |
| White | Aubrey | 90 | 93 | High |
| White | Caleb | 77 | 84 | High |
| White | Erica | 82 | 93 | Med |
| White | Charlie | 86 | 91 | Med |
| White | Leslie | 72 | 93 | Low |
| White | Ronnie | 71 | 89 | Low |

| Panel B. Last Names Frequency of Occurrence in 2010 Census (%) | | | | |
|---|---|---|---|---|
| Race | Last Name | African American | Hispanic/LatinX | White |
| African American | Harris | 42.4 | 2.3 | 51.4 |
| African American | Jackson | 53.0 | 2.5 | 39.9 |
| African American | James | 38.9 | 3.1 | 51.6 |
| African American | Williams | 47.7 | 2.5 | 45.8 |
| African American | Thomas | 38.8 | 2.5 | 52.6 |
| African American | Robinson | 44.9 | 2.6 | 48.7 |
| Hispanic/LatinX | Lopez | 0.6 | 92.9 | 4.9 |
| Hispanic/LatinX | Rodriguez | 0.5 | 93.8 | 4.8 |
| Hispanic/LatinX | Morales | 0.6 | 93.2 | 4.6 |
| Hispanic/LatinX | Sanchez | 0.5 | 93.0 | 5.0 |
| Hispanic/LatinX | Ramirez | 0.3 | 94.5 | 3.9 |
| Hispanic/LatinX | Torres | 0.6 | 92.2 | 5.4 |
| White | Murphy | 11.5 | 2.3 | 83.1 |
| White | Peterson | 10.1 | 2.4 | 84.4 |
| White | Cox | 12.1 | 2.3 | 82.6 |
| White | Myers | 10.5 | 2.1 | 84.5 |
| White | Wood | 5.6 | 2.4 | 88.7 |
| White | Miller | 10.8 | 2.2 | 84.1 |

Notes: In the study, we use the following first-last name pairs; Nia Harris, Jalen Jackson, Ebony James, Lamar Williams, Shanice Thomas, DaQuan Robinson, Isabella Lopez, Jorge Rodriguez, Mariana Morales, Pedro Sanchez, Jimena Ramirez, Luis Torres, Aubrey Murphy, Caleb Peterson, Erica Cox, Charlie Myers, Leslie Wood, Ronnie Miller.

Panel B reports the set of last names used in our study and examined in Gaddis (2017, 2018), which were generated using the distribution from the 2010 Census. We note that imperfect (< 100%) name-race congruence shown by Gaddis has implications for the interpretation of our results since names with lower levels of congruence will be less likely to induce discriminatory behavior. The fact that African American names are associated with lower congruence than LatinX names suggests that our results may understate discriminatory constraints facing the African American group relative to the LatinX group. We also note that heterogeneity in congruence by maternal education (lower congruence for the low maternal education group) may mean that our estimates understate constraints for renters with low maternal education.

The birth record data used in Gaddis (2017) cover the years 1994 to 2012, making them relevant for renters under age 25 as of the time of our study. Gaddis (2017) explains the choice to use the full set of NY birth data in his study, rather than constrain the dataset to an age range that is more likely to have entered the rental housing market or labor market (i.e. 18-25). Gaddis (2017) does not provide an analysis of differences in the frequency of occurrence of names in early years (i.e. 1994-2001) and later years (i.e. 2002-2012) of birth records. Given that this study is designed to guide correspondence research, we assume that differences are not substantial. Gaddis (2017) also discusses potential heterogeneity in names used across regions: "Although racial and SES-based naming practices may vary somewhat across regions, the question of importance is whether racial perceptions from names vary across regions. In supplemental analyses, I test whether respondents from New York vary from respondents in the rest of the United States. I find no substantive differences in these analyses, suggesting that the use of New York data likely has no significant bearing on the results (footnote 4, pp. 484-485)."

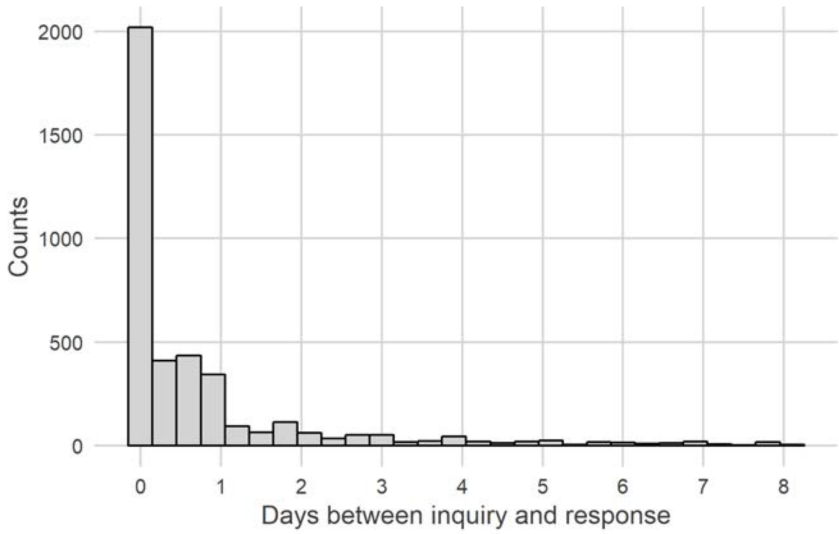
## Randomization Protocol and Response Coding

The research design simulates a housing search using all available listings in a ZIP code at a given time and is therefore reflective of the true set of options available in the given online market. By generating within-property estimates of response for each racial group, we can more directly examine the effect of discriminatory constraints on each choice set in the sample.

Immediately following the compilation of the relevant listings in a given market, a name is randomly drawn and assigned from each of three racial groups. Each rental apartment, therefore, receives a sequence of three separate inquiries in the course of an experimental trial (one from each group). The sequence of inquiries from the different race groups is randomized, and inquiries for the same listing are never sent from two race groups on the same day. Responses from property managers are transmitted via email (gmail address associated with each name), phone messages (individual phone numbers associated with each name), and text messages. The content of phone, text, and email responses from property managers are recorded by a team of human coders to ensure the quality of the data. They are coded using two criteria that determine whether or not a response indicates that a housing choice is made available to a prospective renter: (1) a response is received within 7 days of the associated inquiry and (2) the response indicates that the property is available for rent.

Figure A.2 plots the distribution of inquiry response time in the sample: 52% of responses are received within the first 8 hours of an inquiry, 74% are received within 24 hours and 98% are received within 5 days. The 7-day cutoff is used to restrict responses that may be received weeks or months after an inquiry and are not counted as choices in the study. Discriminatory constraints are expressed in terms of relative response rates, which measure the within-property difference in access to a housing choice. Relative response rates are estimated relative to an inquiry made to the same property from a white identity.

Figure A.2. Response Time




Note: Figure plots times elapsed between inquiries and responses in the sample using the timestamp given at the moment that an inquiry is sent and the timestamp given on the phone, email, or text response.

As discussed above, we operationally define choice set constraints on the basis of whether a property manager makes a housing choice available upon receiving a request. However, discriminatory behavior by property managers may also operate through additional screening requirements that may make it more challenging for renters from a given group to access a choice. Here we provide a basic test for evidence of discriminatory screening in a response. In particular, we generate a measure of whether a response contains any of the following terms: (1) "income," (2) "credit," (3) "references," (4) any of 1-3. We then use the primary estimating equation defined in Section 3.3 to estimate the relative rate of appearance of screening terms.

Table A2 reports the results of this test. We find that the average response to an inquiry sent from a white identity has a 6.7% likelihood of containing one of the three screening terms. The term "credit" is the most likely to appear, followed by "income" and then "references." We do not find any evidence of differences in screening terms appearing in responses to inquiries sent from LatinX/Hispanic. However, these results suggest that the likelihood that a screening term is used increases by 65% in inquiries from African American renters. The likelihood that "income" is mentioned increases by 52%. This test suggests that screening terms may be used on a discriminatory basis and while the impacts of screening language appearing in a responses likely vary across the renter population, this evidence suggests that the estimates of impacts likely provide a lower bound on the impacts of discriminatory behavior on renter choice sets.

Table A2. Relative Use of Screening Terms

| Race Group | Any Screening Term | Income | References | Credit |
|---|---|---|---|---|
| *Panel A: Relative Response Rates* | | | | |
| Hispanic | 1.1346 | 1.1707 | 1.1250 | 1.0638 |
| | (0.8915,1.3777) | (0.8898,1.4517) | (0.6648,1.5852) | (0.8186,1.3090) |
| Black | 1.3077** | 1.2927* | 1.5000 | 1.1702 |
| | (1.0631,1.5523) | (1.0092,1.5761) | (0.9562,2.0438) | (0.9130,1.4275) |
| *Panel B: Difference in Means* | | | | |
| Hispanic | 0.0090 | 0.0090 | 0.0026 | 0.0039 |
| | (-0.0073,0.0253) | (-0.0058,0.0239) | (-0.0069,0.0121) | (-0.0110,0.0187) |
| Black | 0.0206** | 0.0154* | 0.0103 | 0.0103 |
| | (0.0042,0.0370) | (0.0005,0.0304) | (-0.0009,0.0215) | (-0.0053,0.0259) |
| Observations | 2,331 | 2,331 | 2,331 | 2,331 |
| Group Mean (White) | 0.0669 | 0.0528 | 0.0206 | 0.0605 |

Notes: Column 1 reports estimates of the relative prevalence of any screening terms in responses to inquiries. Estimates in columns 2-4 report the relative prevalence of specific screening terms. The prevalence of screening terms are estimated relative to the prevalence in response to an inquiry from a white identity, which is reported in the bottom row of the table. Panel A reports differences in relative rates of responses containing screening terms, which divide the estimate for each group in Panel B by the mean rate for to inquiries from the white group (bottom row) and add 1. Panel B presents the raw differences in means as in Bertrand and Mullainathan (2004). 90% confidence intervals are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

# B Balance Tests and Robustness

## Balance Tests

Table B1 reports balance statistics for inquiries in our experimental sample. We note that some differences in name pairs or timing can occur if a listing is taken offline during a trial. We do not find any evidence of differences in the frequency of names associated with a given race-gender pair, or the frequency of inquiries associated with different gender or levels of maternal education. These variables are used as controls in our tests to increase statistical power, though they do not affect the magnitude of our estimated relative response rates (see Table B2 below).

Table B1. Balance Statistics

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Panel A: Inquiry Order* | | | | | |
| | First | Second | Third | | |
| Hispanic | 0.0025 | -0.0065 | 0.0040 | | |
| | (-0.0149,0.0199) | (-0.0238,0.0108) | (-0.0133,0.0213) | | |
| African American | -0.0040 | -0.0025 | 0.0065 | | |
| | (-0.0213,0.0133) | (-0.0199,0.0149) | (-0.0108,0.0238) | | |
| *Panel B: Gender and Mother's Education Level* | | | | | |
| | Gender | | Mother's Education | | |
| | Male | Female | Low | Medium | High |
| Hispanic | -0.0048 | 0.0048 | -0.0062 | 0.0095 | -0.0033 |
| | (-0.0196,0.0099) | (-0.0099,0.0196) | (-0.0202,0.0079) | (-0.0046,0.0235) | (-0.0175,0.0108) |
| African American | -0.0037 | 0.0037 | -0.0062 | 0.0022 | 0.0040 |
| | (-0.0187,0.0114) | (-0.0114,0.0187) | (-0.0207,0.0084) | (-0.0120,0.0163) | (-0.0103,0.0183) |
| Observations | 18,045 | 18,045 | 18,045 | 18,045 | 18,045 |

Notes: Table reports balance statistics for the experimental data set using a linear regression. In Panel A, the dependent variable takes a value 0 or 1 depending on whether it was the first, second, or third inquiry in the series, i.e. in Column (1) the dependent variable is 1 if the inquiry was sent first and 0 otherwise. In Panel B, the dependent variable takes a value of 0 or 1 depending on the gender of the name and level of maternal education. Standard errors are clustered by listing. *** p<0.01, ** p<0.05, * p<0.1

## Robustness to Controls

Columns 1-4 of Table B2 report results with successive sets of controls and indicate that there is no difference in estimates that include or omit the maternal education or other controls. Phillips (2016) shows that matched-inquiry experimental designs can affect estimates of discriminatory response in competitive markets. Column 5 reports estimates from a model that considers differences in first inquiries only, which reflect random assignment of identities to listings but do not control for within-listing characteristics. The ratios of means estimates from the preferred model in columns 1-4 are larger than those in column 5, though the estimates are not statistically different. This is consistent with evidence reported in Table 4, which indicates lower relative rates when minority identities are assigned to later inquiries.

Table B2. Robustness to Controls

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Panel A: Conditional Relative Response* | | | | | |
| Minority | 0.9168*** | 0.9165*** | 0.9170*** | 0.9174*** | 0.8700*** |
| | (0.8982,0.9354) | (0.8979,0.9351) | (0.8984,0.9356) | (0.8989,0.9358) | (0.8244,0.9156) |
| African American | 0.8992*** | 0.8989*** | 0.8992*** | 0.9000*** | 0.8599*** |
| | (0.8768,0.9216) | (0.8765,0.9213) | (0.8768,0.9215) | (0.8778,0.9222) | (0.8073,0.9125) |
| Hispanic | 0.9344*** | 0.9341*** | 0.9348*** | 0.9347*** | 0.8799*** |
| | (0.9135,0.9554) | (0.9131,0.9551) | (0.9138,0.9558) | (0.9139,0.9555) | (0.8274,0.9325) |
| *Panel B: Linear Probability Model* | | | | | |
| Minority | -0.0361*** | -0.0362*** | -0.0360*** | -0.0358*** | -0.0638*** |
| | (-0.0442,-0.0280) | (-0.0443,-0.0281) | (-0.0441,-0.0279) | (-0.0438,-0.0278) | (-0.0862,-0.0414) |
| Black | -0.0437*** | -0.0438*** | -0.0437*** | -0.0434*** | -0.0688*** |
| | (-0.0534,-0.0340) | (-0.0535,-0.0341) | (-0.0534,-0.0340) | (-0.0530,-0.0337) | (-0.0947,-0.0429) |
| Hispanic | -0.0284*** | -0.0286*** | -0.0283*** | -0.0283*** | -0.0590*** |
| | (-0.0375,-0.0193) | (-0.0377,-0.0195) | (-0.0374,-0.0192) | (-0.0373,-0.0193) | (-0.0847,-0.0332) |
| Observations | 18,045 | 18,045 | 18,045 | 18,045 | 6,015 |
| Mean White (Overall) | 0.4336 | 0.4336 | 0.4336 | 0.4336 | 0.4910 |
| Gender | No | Yes | Yes | Yes | Yes |
| Education | No | No | Yes | Yes | Yes |
| Inquiry Order | No | No | No | Yes | Yes |

Notes: Columns 1-4 report estimates of relative response rates for the full sample of listings across all cities, progressively adding controls to a linear probability model. Panel A reports relative response rates, which divide the estimate for each group in Panel B by the mean response rate to inquiries from white identities (below) and add 1. All regressions include listing fixed effects and standard errors are clustered at the MSA level. Estimates in Column 5 report estimates of relative response rates from a model that tests for differences in first inquiries. Response rates in the linear probability model (Panel B) are estimated relative to responses to inquiries sent from a white identity (the omitted category). The average response rate for inquiries sent from white identities are reported for each sample. 90% confidence intervals are reported in parentheses. Sample sizes are reported for matched inquiry sets (a set includes inquiries sent from all three identities) and total observations (where a fully matched set was not obtained). *** p<0.01, ** p<0.05, * p<0.1

## Unit Characteristics

Columns 1-2 of Table B3 report conditional relative response rates for the primary sample of 3 bedroom, 2 bathroom units used in the analysis relative to a subset of other units that were listed simultaneously. We do not find significant differences in response rates for the minority group for 3 bedroom, 2 bathroom units relative to the broader sample of listings. We do find a marginally significant difference for the African American group, suggesting that if anything, the sample of 3 bedroom, 2 bathroom units may understate the magnitude of discriminatory constraints facing African American renters in other market segments.

Table B3. Discriminatory Constraints by Unit Characteristics

| Group | Conditional Relative Response | Linear Probability Model |
|---|---|---|
| Minority | 0.9047*** | -0.0413*** |
| | (0.8711,0.9383) | (-0.0559,-0.0267) |
| Minority3bed | 1.0223 | 0.0097 |
| | (0.9784,1.0662) | (-0.0094,0.0288) |
| Hispanic | 0.9392** | -0.0264** |
| | (0.8933,0.9851) | (-0.0463,-0.0064) |
| Hispanic3bed | 0.9920 | -0.0035 |
| | (0.9350,1.0490) | (-0.0282,0.0213) |
| Black | 0.8702*** | -0.0563*** |
| | (0.8368,0.9037) | (-0.0708,-0.0417) |
| Black3bed | 1.0526* | 0.0228* |
| | (1.0046,1.1005) | (0.0020,0.0436) |
| Mean White (Overall) | 0.4336 | 0.4336 |
| Matched Listings | 6,015 | 6,015 |
| Observations | 18,045 | 18,045 |

Notes: Table reports estimates of conditional relative response rates for all units relative to the differential rate within the subset of 3 bedroom 2 bathroom units. Relative response rate estimates in Column A divide the estimated difference in response rates for each group in Column B by the mean response rate to inquiries from white identities (below) and add 1. Estimates in Column B are estimated using a linear probability model that includes listing fixed effects, gender, education and inquiry order controls (white identity is the omitted category). All standard errors are clustered at the MSA level. The average response rate for inquiries sent from white identities is reported for each sample. 90% confidence intervals are reported in parentheses. Sample sizes are reported for matched inquiry sets (a set includes inquiries sent from all three identities) and total observations (where a fully matched set was not obtained). *** p<0.01, ** p<0.05, * p<0.1

## Maternal Education

Table B4 reports mean response rate for each of the 18 race-gender-education combinations and a test of the null hypothesis that names within the same race group exhibit the same response rates by gender and mother's education, similar to Kline, Rose, and Walters (2022). We find that names at all education levels receive lower relative response rates than the associated white comparison identity. We find some evidence of difference between African American names, though we do not find evidence that response rates vary monotonically with maternal education. Results in Table B2 indicate that estimates of response rates are not sensitive to the inclusion of a control for maternal education.

Table B4. Discriminatory Constraints by Maternal Education

| | African American | | Hispanic | | White | |
|---|---|---|---|---|---|---|
| Mother's Education | Male | Female | Male | Female | Male | Female |
| High | 0.3876 | 0.4393 | 0.3921 | 0.4079 | 0.4260 | 0.4263 |
| | (0.3622,0.4130) | (0.4136,0.4650) | (0.3664,0.4177) | (0.3821,0.4336) | (0.4001,0.4518) | (0.4005,0.4521) |
| Medium | 0.3229 | 0.3826 | 0.3667 | 0.4039 | 0.4327 | 0.4321 |
| | (0.2985,0.3474) | (0.3573,0.4079) | (0.3417,0.3917) | (0.3787,0.4291) | (0.4070,0.4584) | (0.4059,0.4583) |
| Low | 0.3733 | 0.4323 | 0.4144 | 0.4459 | 0.4390 | 0.4450 |
| | (0.3433,0.4033) | (0.4065,0.4580) | (0.3887,0.4401) | (0.4201,0.4717) | (0.4133,0.4646) | (0.4194,0.4705) |
| F-stat | 5.1036 | 3.9992 | 2.4118 | 2.2126 | 0.1728 | 0.3776 |
| P-value | 0.0061 | 0.0184 | 0.0897 | 0.1095 | 0.8414 | 0.6855 |
| Mean Response Rate | 0.3899 | | 0.4052 | | 0.4336 | |

Notes: Table reports mean response rates for each of the 18 race-gender-education combinations used in the study. Standard errors are clustered by listing. For each race group, we report a test of the null hypothesis that names within the same race group exhibit the same response rates by gender and mother's education. We also report the mean response rate by race. 90% confidence intervals are reported in parentheses.

## Gender

Columns 1-2 of Table B5 report relative response rates by the gender associated with a name pair. Consistent with the results of prior correspondence work in other markets Christensen, Sarmiento-Barbieri, and Timmins (2022), we find some evidence of lower response rates to inquiries from African American men.

Table B5. Discriminatory Constraints by Gender

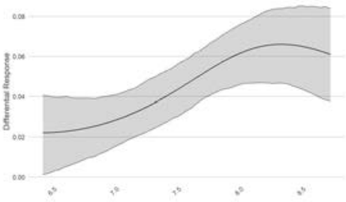
| Group | Relative Response | Difference in Means |
|---|---|---|
| Minority: Female Name | 0.9656** | -0.0149** |
| | (0.9407,0.9905) | (-0.0257,-0.0041) |
| Minority: Male Name | 0.8675*** | -0.0574*** |
| | (0.8418,0.8932) | (-0.0686,-0.0463) |
| Hispanic: Female Name | 0.9668* | -0.0144* |
| | (0.9352,0.9983) | (-0.0281,-0.0007) |
| Hispanic: Male Name | 0.9017*** | -0.0426*** |
| | (0.8695,0.9339) | (-0.0566,-0.0287) |
| Black: Female Name | 0.9644* | -0.0154* |
| | (0.9323,0.9966) | (-0.0294,-0.0015) |
| Black: Male Name | 0.8334*** | -0.0722*** |
| | (0.8002,0.8666) | (-0.0866,-0.0578) |
| Mean White (Overall) | 0.4336 | 0.4336 |
| Matched Listings | 6,015 | 6,015 |
| Observations | 18,045 | 18,045 |

Notes: Table reports estimates of relative response rates for female versus male first names. Relative response rate estimates in left column divide the differences in means estimates for each group in right column by the mean response rate to inquiries from white identities (below) and add 1. Right column reports differences in means as in Bertrand and Mullainathan (2004), with standard errors clustered by listing. 90% confidence intervals are reported in parentheses. Sample sizes are reported for matched inquiry sets (a set includes inquiries sent from all three identities) and total observations (where a fully matched set was not obtained). *** p<0.01, ** p<0.05, * p<0.1

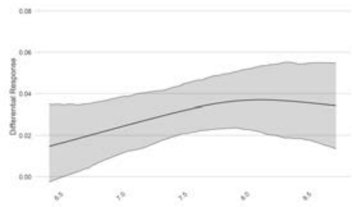
# C Heterogeneity by Housing/Neighborhood Attributes

Figure C.1. Response Rates by Housing/Neighborhood Attribute

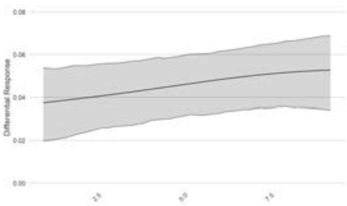
**African American** | **Hispanic/LatinX**



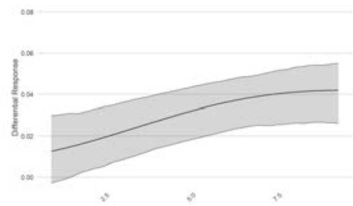
(a) Rental Price



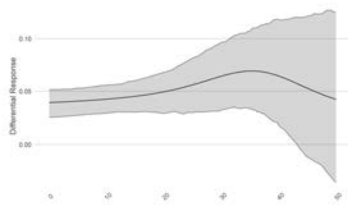
(b) Rental Price



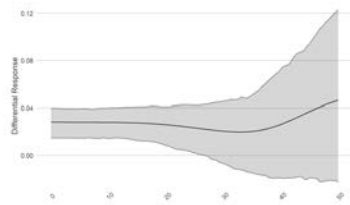
(c) School Rating



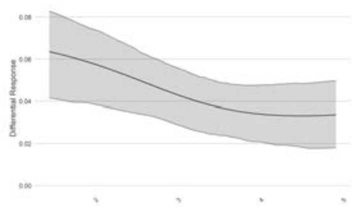
(d) School Rating



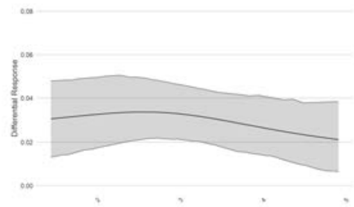
(e) Cafes



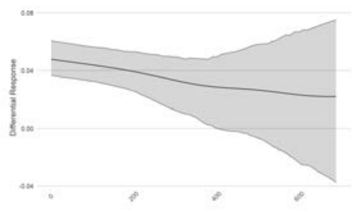
(f) Cafes



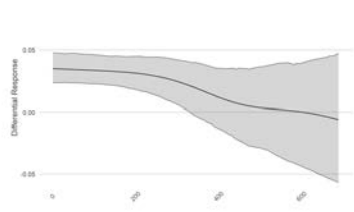
(g) Toxic Concentration



(h) Toxic Concentration



(i) Murders

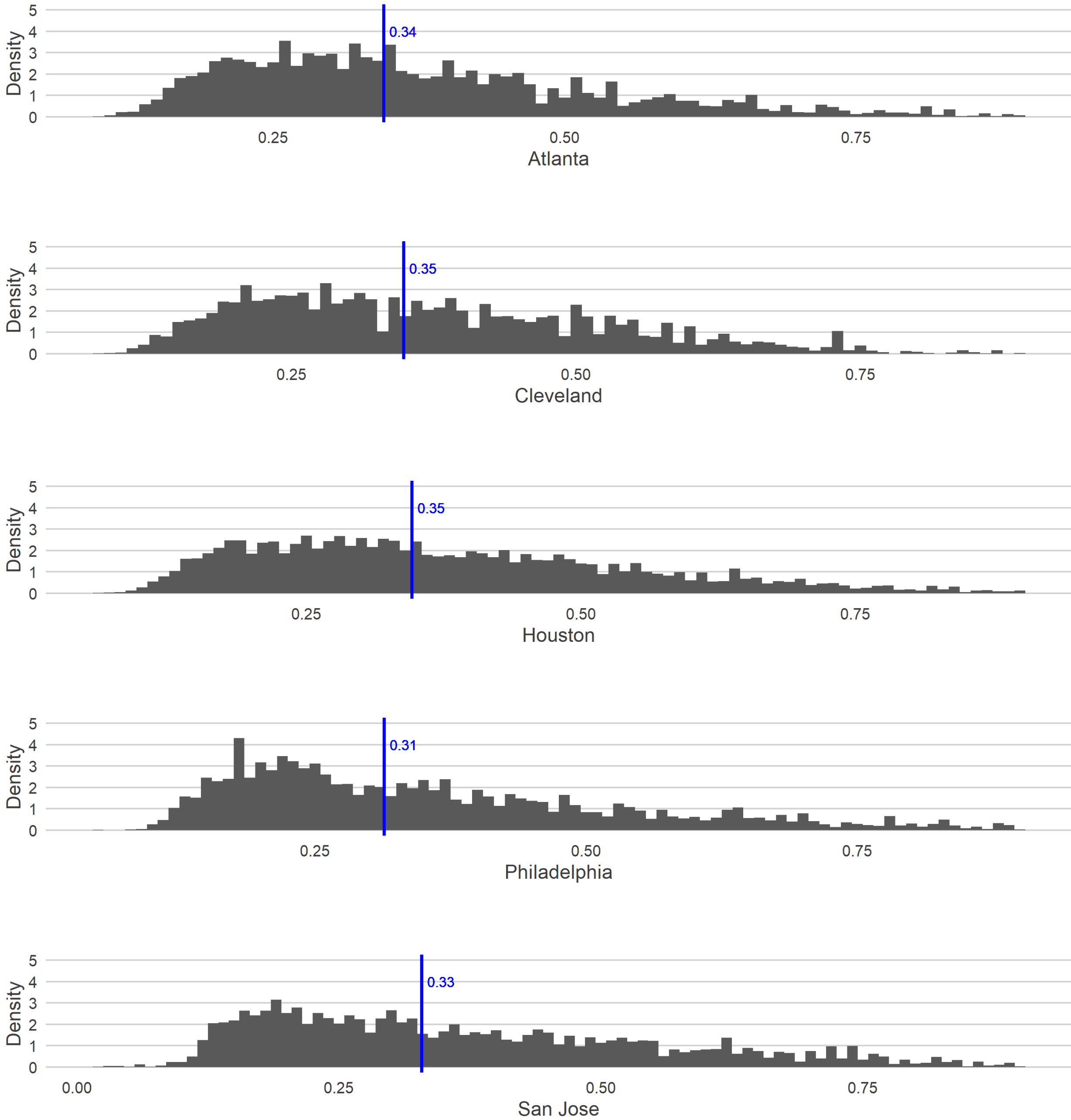


(j) Murders

Notes: Figures plot differential constraints for renters of color (African American and Hispanic/LatinX) relative to white by attributes observed in rental listings collected in the experiment using estimates from Eq. 1. Increases in differential response correspond to increases in discriminatory constraints.

# D Appendix: Rent-to-Income Ratio

Figure D.1. Rent-to-Income Ratio by City




Note: Graphs plot the distributions of rent-to-income ratios using renter incomes from InfoUSA data and rental prices from from the 2013-2017 American Community Survey for each MSA. Blue vertical lines identify the mean for each distribution.

# E Appendix: Damages Including/Omitting Houston

Our consideration sets model and damages simulations are both designed to identify the effects of differential response rates. Our reduced form estimates indicate the presence of differential response rates in Houston, suggesting that this market contains information about discriminatory constraints. However, the baseline response rate in Houston is lower than in the other markets in our study and differences in relative rates are not statistically significant. In this section, we evaluate the sensitivity of our results to the inclusion/omission of Houston from our sample. Estimates reported in Table E1 indicate that the signs and magnitudes of structural parameter estimates are highly similar across the two samples. We note that omitting the data from the Houston market does alter the sample and results in the interpretation of estimates as being representative of a different set of cities.

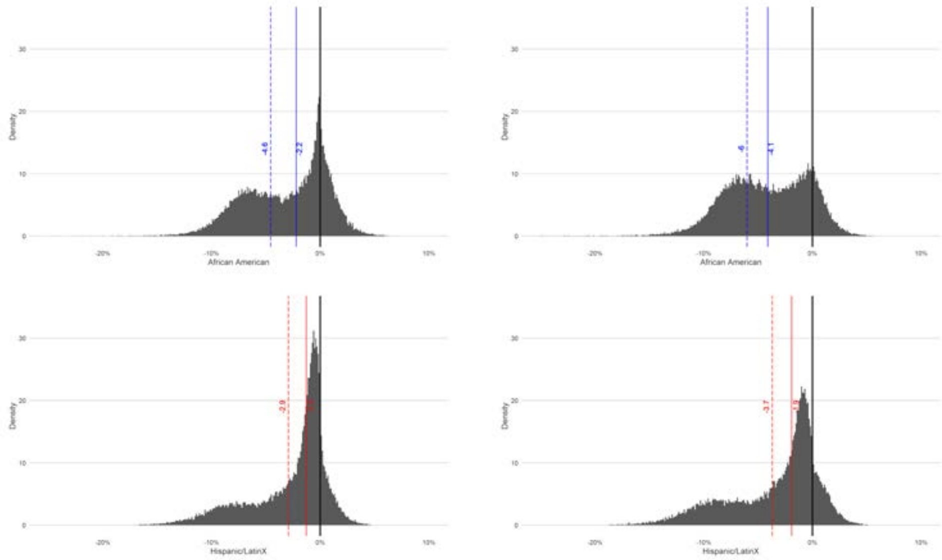
Table E1. Parameter Estimates (excluding Houston)

| Parameter | Consideration Sets: All Cities | Exl. Houston |
|---|---|---|
| ln(I-R) | 1.3117*** | 1.3620*** |
| | (0.0211) | (0.0256) |
| Avg School Quality | 0.0350*** | 0.0138 |
| | (0.0130) | (0.0102) |
| Cafes | 0.0092*** | 0.0088*** |
| | (0.0008) | (0.0007) |
| Murders | −0.0015*** | −0.0016*** |
| | (0.0001) | (0.0002) |
| ln(RSEI) | −0.3366*** | −0.1908*** |
| | (0.0403) | (0.0390) |
| White ∗% White | 0.0836*** | 0.0804*** |
| | (0.0060) | (0.0046) |
| White ∗% White$^2$ | −0.0007*** | −0.0006*** |
| | (0.0060) | (0.0000) |
| African American ∗% African American | 0.1035*** | 0.1023*** |
| | (0.0045) | (0.0088) |
| African American ∗% African American$^2$ | −0.0006*** | −0.0006*** |
| | (0.0001) | (0.0001) |
| LatinX ∗% LatinX | 0.0805*** | 0.1125*** |
| | (0.0035) | (0.0041) |
| LatinX ∗% LatinX$^2$ | −0.0008*** | −0.0013*** |
| | (0.0001) | (0.0001) |
| ATL∗Latitude | 2.4804*** | 2.4477*** |
| | (0.2468) | (0.2710) |
| ATL∗Longitude | −4.0436*** | −3.7879*** |
| | (0.3387) | (0.3381) |
| HOU∗Latitude | 1.2489*** | |
| | (0.1463) | |
| HOU∗Longitude | 0.6214*** | |
| | (0.1778) | |
| PHL∗Latitude | 0.6213 | 0.5197 |
| | (0.6266) | (0.5775) |
| PHL∗Longitude | −0.4489 | −0.2464 |
| | (0.3606) | (0.3557) |
| CLE∗Latitude | −3.2345** | −2.7480** |
| | (1.3534) | (1.2002) |
| CLE∗Longitude | 1.2826** | 1.0131*** |
| | (0.6192) | (0.3858) |
| SJC∗Latitude | 2.3260*** | 1.6648*** |
| | (0.4715) | (0.4528) |
| SJC∗Longitude | −1.2245*** | −1.1376*** |
| | (0.3367) | (0.2934) |

Notes: Table reports parameter estimates from baseline model specification with consideration sets. Column 1 reports coefficient estimates from the full sample of cities. Column 2 reports coefficient estimates excluding data from Houston. Standard errors are generated from 100 random bootstrap draws of both the first- (i.e., predicted response probabilities) and second- (i.e., residential location choice) stage models. *** p<0.01, ** p<0.05, * p<0.1.

We then use the parameters from the model that omits Houston to evaluate the sensitivity of our damages estimates to the inclusion/omission of Houston. Results reported in Figure E.1 indicate that the omission of Houston, where our reduced form results suggest that African American and LatinX renters face smaller discriminatory constraints, results in larger damages estimates than obtained from the full sample. The overall patterns of our findings are consistent.

Figure E.1. Annualized Damages as a Share of Annual Income

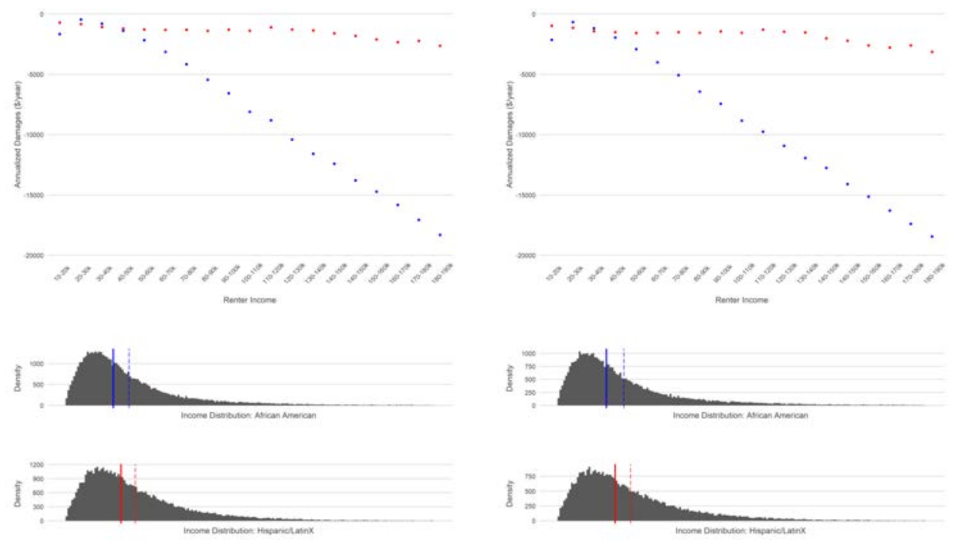



(a) All Cities (b) Excluding Houston

Note: The figure graphs the distribution of renter welfare effects (compensating variation as a share of renter income) resulting from discriminatory constraints as measured by the correspondence experiment. Plots illustrate damages to African American renters (top) and Hispanic/LatinX renters (bottom), with solid vertical lines denoting the median and dashed vertical lines denoting the mean estimates. Panel (a) reports estimates of damages to the two renter groups using the full sample of cities. Panel (b) reports estimates of damages to the two renter groups excluding the sample of data from Houston.

Results reported in Figure E.2 indicate that the omission of Houston, where our reduced form results suggest that African American and Hispanic/LatinX renters face smaller discriminatory constraints, results in larger damages estimates than obtained from the full sample. The overall patterns of our findings are consistent.

Figure E.2. Annualized Damages by Income

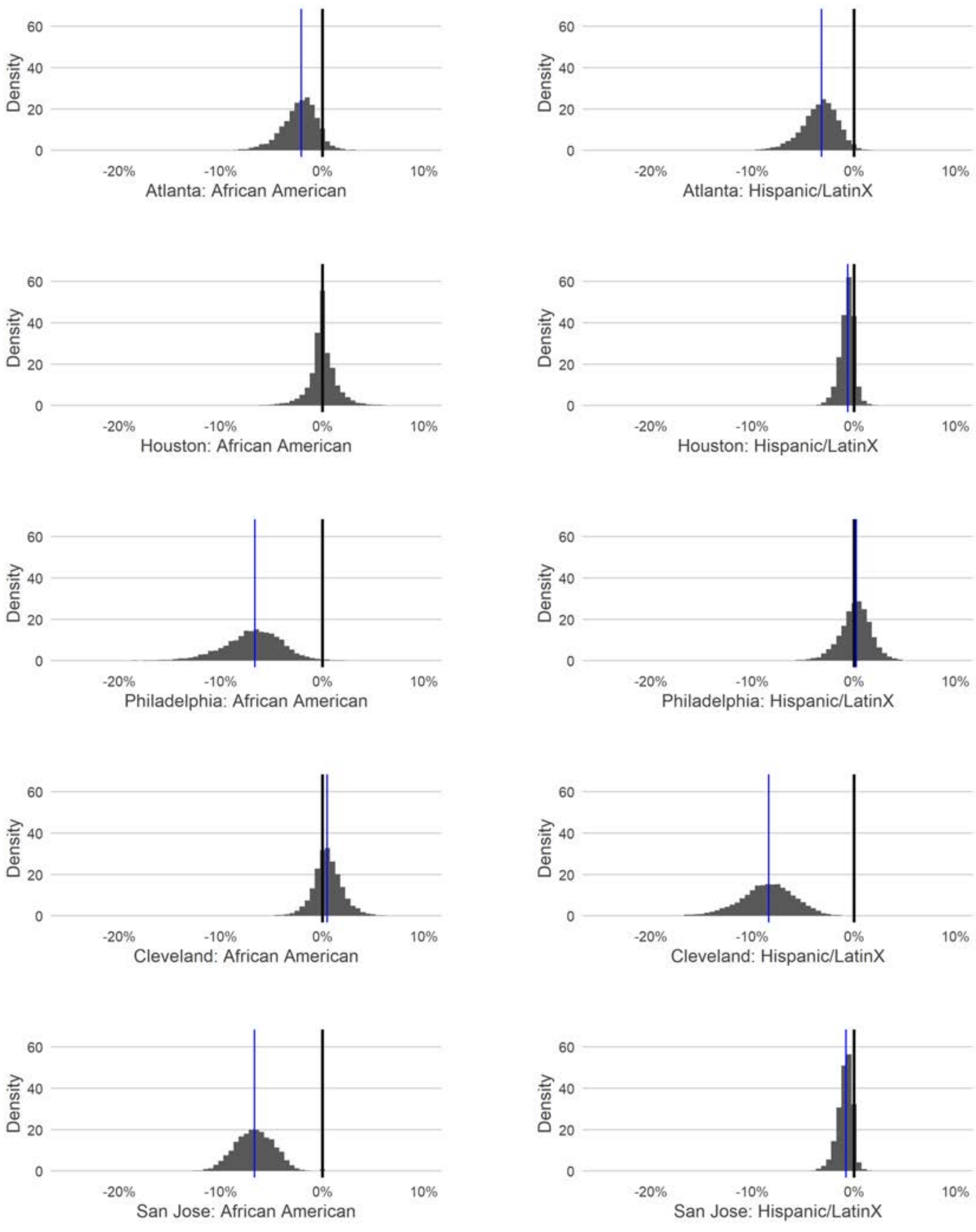



(a) All Cities (b) Excluding Houston

Note: Top panel graphs the distribution of renter welfare effects (compensating variation in dollars per year) resulting from discriminatory constraints as measured by the correspondence experiment. Blue points show effects for African American renters and red points plot effects for Hispanic/LatinX renters. Bottom panels graph the distribution of renter incomes from the InfoUSA renter sample. Panel (a) reports estimates of damages to the two renter groups using the full sample of cities. Panel (b) reports estimates of damages to the two renter groups excluding the sample of data from Houston.

# F Appendix: Damages by City

Figure F.1. Annualized Damages by City (Equivalent Variation)




Note: Graphs plot the distributions of renter welfare effects (equivalent variation as a share of renter income) at different levels of renter income. Each plot illustrates a distribution of effects for the following renter income groups: $0-30,000, $30,000-60,000, $60,000-90,000, $90,000-120,000, $120,000-150,000. Blue vertical lines denote median effects for African American renters and Hispanic/LatinX renters, respectively. Effects were estimated using the baseline model with all preferences.

# G Appendix: Search Intensity

## Follow-Up Inquiries

This section discusses the potential for renters of color to attenuate the impacts of discriminatory constraints using follow-up inquiries to the same listing. The current study does not measure responses to follow-up inquiries. We therefore report evidence on follow-up inquiries using data from from Christensen, Sarmiento-Barbieri, and Timmins (2022), which deployed the same correspondence design to examine discriminatory constraints in 2,918 listings from 19 ZIP codes drawn at random from all ZIP codes surrounding major point sources of airborne chemical toxics in the US. Table G1 reports estimates of relative response using the sample from Christensen, Sarmiento-Barbieri, and Timmins (2022) and odds ratios as reported in that study.

Table G1. Overall Discrimination Rates
Properties with Two Inquiries

| | *Dependent variable:* *Response* | |
|---|---|---|
| | (1) | (2) |
| Minority First Inquiry | 0.6006 | |
| | (0.3107,1.1613) | |
| Minority Second Inquiry | 0.4549*** | |
| | (0.4101,0.5046) | |
| Second Inquiry | 0.7049 | 0.7027 |
| | (0.2392,2.0774) | (0.2369,2.0842) |
| African American First Inquiry | | 0.4169* |
| | | (0.1994,0.8719) |
| African American Second Inquiry | | 0.3419*** |
| | | (0.2589,0.4516) |
| Hispanic/LatinX First Inquiry | | 0.8837 |
| | | (0.4029,1.9383) |
| Hispanic/LatinX Second Inquiry | | 0.5921*** |
| | | (0.5116,0.6852) |
| Mean Response (White) First Round | 0.48 | 0.48 |
| Gender | Yes | Yes |
| Education Level | Yes | Yes |
| Inquiry Order | Yes | Yes |
| Observations | 1,572 | 1,572 |
| Listings | 524 | 524 |
| % w. diff. response | 0.61 | 0.61 |

Notes: Table reports odd ratios relative to the white identity. Odds ratios are estimated using a within-property conditional logit model including controls for gender, education and order the inquiry was sent. Standard errors are clustered at the ZIP code level using a score wild bootstrap proposed Kline and Santos (2012) to account for the small number of clusters. 90% confidence intervals reported in parentheses.*$P < 10\%$ level, **$P < 5\%$ level, ***$P < 1\%$ level.

Relative to a mean response rate of 48% to initial (first round) inquiries from a white identity, the relative response rate to an inquiry from a minority identity is 0.6 in the first round, implying an absolute first-round response rate of 29% to minority identities. If the minority and their counterfactual white identity send a second follow-up inquiry, the odds of response are 0.45 for the minority renter and 0.70 for the comparison identity, implying absolute response rates of 22% for minority identities and 34% for white identities in the second round. Follow-up inquiries therefore increase the response rate for to minority identities from 29% in a first round to a cumulative rate of 51% across both rounds. They increase response rates to comparison white identities by a larger amount, from 48% in the first round to to a cumulative rate of 82% across both rounds. These estimates suggest that in the context of non-response from a property manager, the impacts of discriminatory constraints among renters of color may become larger (on average) relative to a white counterfactual who behaves in an identical manner.

However, if the likelihood of sending a follow-up inquiry depends upon the likelihood of receiving an initial response, then the estimates in Table G1 indicate that lower first round responses to renters of color will also induce a higher rate of follow-up to the average listing. This higher rate of follow-up would add additional search cost to renters of color relative to a counterfactual, which we do not quantify in the current study.

How does this higher rate of follow-up affect absolute response rates? We examine the cumulative response rates to an initial and a follow-up inquiry across a range of levels of follow-up effort for a renter of color and their white counterfactual. Column 1 in Table G2 lists a range of follow-up effort, which we define as the probability that a renter will send a second follow-up inquiry in the event of non-response to a first inquiry (ex. 0% follow-up effort signifies that the probability of a second inquiry is 0%, whereas 100% implies that a renter is certain to send a follow-up inquiry). Columns 2 and 3 report follow-up probabilities for each group, which are simply the product of follow-up effort and the probability of non-response to a first inquiry for each group from Table G1. The fact that this increases faster for minority identities follows from the lower expected rate of response to an initial inquiry from that group.

Table G2. Follow-Up Inquiries

| | p(2nd inquiry) | | Cum. Response Rate | | Difference |
|---|---|---|---|---|---|
| Follow-up Effort | White | Minority | White | Minority | |
| 0% | 0.00 | 0.00 | 0.48 | 0.29 | 0.19 |
| 10% | 0.05 | 0.07 | 0.50 | 0.30 | 0.20 |
| 20% | 0.10 | 0.14 | 0.51 | 0.31 | 0.20 |
| 30% | 0.16 | 0.21 | 0.53 | 0.32 | 0.21 |
| 40% | 0.21 | 0.28 | 0.55 | 0.33 | 0.22 |
| 50% | 0.26 | 0.36 | 0.57 | 0.34 | 0.22 |
| 60% | 0.31 | 0.43 | 0.58 | 0.36 | 0.23 |
| 70% | 0.36 | 0.50 | 0.60 | 0.37 | 0.24 |
| 80% | 0.42 | 0.57 | 0.62 | 0.38 | 0.24 |
| 90% | 0.47 | 0.64 | 0.64 | 0.39 | 0.25 |
| 100% | 0.52 | 0.71 | 0.65 | 0.40 | 0.25 |

Notes: Table reports the results of post-estimation calculations using estimates of response rates to the average inquiry from each group in Table G1. The first column defines a range of follow-up effort from 0-100%, which is the probability that a renter will send a follow-up inquiry when they receive no response to a first inquiry. The 2nd and 3rd columns report the probability of sending a follow-up inquiry for white and minority renters, which is the product of follow-up effort times the group-specific likelihood of a response to the first inquiry from Table G1. The 4th and 5th columns report cumulative response rates for each group, which are the sum of group-specific likelihoods of response to an initial inquiry and a second inquiry. The 6th column reports the difference between the cumulative response rates to inquiries from white identities (column 4) minus cumulative response rates to inquiries from minority identities (column 5).

Columns 4 and 5 report the cumulative response probabilities (inquiry rounds 1 and 2) using the group-specific ranges of follow-up probabilities and estimates of response rates to first and second inquiries from Table G1. Column 6 reports the difference in cumulative response probabilities at each level of follow-up effort. The baseline damages estimates in the paper effectively assume a baseline follow-up probability of zero (no second inquiries), which are included as the top row. We find that the gap in discriminatory constraint *increases* at increasing levels of follow-up effort, indicating that to the extent that a renter's likelihood of following up depends on the probability of response to an initial inquiry, renters of color that expend larger amounts of effort in follow-up inquiries to the same listing will likely absorb additional search cost while also facing larger choice constraints relative to a white counterfactual with identical search behavior.[1]

[1]We note that the sample from Christensen, Sarmiento-Barbieri, and Timmins (2022) is different from the sample in the current study, though the estimates of relative response rates in the two studies are consistent. These results are illustrative of the impact of follow-up inquiries on choice constraints, though we are not able to examine differences in the specific cities or across the different levels of amenities studied in the current paper.

# H Appendix: Bias in Willingness to Pay

Table I1. Bias in Willingness to Pay: Full Model

| Variable | Consideration Sets | No Consideration Sets |
|---|---|---|
| ln(I-R) | $1.3124^{***}$ | $1.2784^{***}$ |
| | (0.0266) | (0.0236) |
| Avg School Quality | $0.0452^{***}$ | $0.0575^{***}$ |
| | (0.0148) | (0.0039) |
| Cafes | $0.0099^{***}$ | $0.0141^{***}$ |
| | (0.0010) | (0.0003) |
| Murders | $-0.0009^{***}$ | $-0.0008^{***}$ |
| | (0.0001) | (0.0001) |
| ln(RSEI) | $-0.3228^{**}$ | $-0.1021^{***}$ |
| | (0.0.0513) | (0.0070) |
| ATL∗Latitude | $4.6333^{***}$ | $4.0132^{***}$ |
| | (0.3604) | (0.2005) |
| ATL∗Longitude | $-2.2236^{***}$ | $-3.3801^{***}$ |
| | (0.3606) | (0.3024) |
| HOU∗Latitude | $1.3359^{***}$ | $0.7544^{***}$ |
| | (0.2305) | (0.0646) |
| HOU∗Longitude | $0.8169^{***}$ | $0.2788^{***}$ |
| | (0.2689) | (0.0769) |
| PHL∗Latitude | 0.9316 | $0.5564^{***}$ |
| | (0.7507) | (0.1433) |
| PHL∗Longitude | −0.4789 | $-1.2544^{***}$ |
| | (0.4436) | (0.0947) |
| CLE∗Latitude | $-3.5566^{**}$ | $0.5705^{***}$ |
| | (1.6302) | (0.1107) |
| CLE∗Longitude | $1.3710^{*}$ | $-0.2739^{***}$ |
| | (0.7415) | (0.0584) |
| SJC∗Latitude | $3.8144^{***}$ | $3.9672^{***}$ |
| | (0.6520) | (0.2790) |
| SJC∗Longitude | −0.1622 | $0.8901^{***}$ |
| | (0.4510) | (0.1690) |
| ln(I-R) x ROC | $0.0804^{*}$ | $0.0856^{**}$ |
| | (0.0431) | (0.0401) |
| Avg School Quality x ROC | −0.0173 | $-0.0279^{***}$ |
| | (0.0178) | (0.0082) |
| Cafes x ROC | $-0.0028^{**}$ | $-0.0040^{***}$ |
| | (0.0013) | (0.0007) |
| Murders x ROC | $-0.0010^{***}$ | $-0.0008^{***}$ |
| | (0.0002) | (0.0001) |
| ln(RSEI) x ROC | −0.0746 | −0.0005 |
| | (0.0589) | (0.0178) |
| White ∗% White | $0.0857^{***}$ | $0.0918^{***}$ |
| | (0.0057) | (0.0013) |
| White ∗% White$^2$ | $-0.0007^{***}$ | $-0.0007^{***}$ |
| | (0.0001) | $(1.0x10^{-5})$ |
| African American ∗% African American | $0.1061^{***}$ | $0.1080^{***}$ |
| | (0.0049) | (0.0014) |
| African American ∗% African American$^2$ | $-0.0007^{***}$ | $-0.0007^{***}$ |
| | (0.0001) | $(2.0 \text{ x } 10^{-5})$ |
| LatinX ∗% LatinX | $0.0828^{***}$ | $0.0790^{***}$ |
| | (0.0030) | (0.0024) |
| LatinX ∗% LatinX$^2$ | $-0.0009^{***}$ | $-0.0008^{***}$ |
| | $(4.0 \text{ x } 10^{-5})$ | $(3.0 \text{ x } 10^{-5})$ |
| ATL∗Latitude x ROC | $-4.0195^{***}$ | $-3.6380^{***}$ |
| | (0.4874) | (0.3238) |
| ATL∗Longitude x ROC | $-2.7886^{***}$ | $-2.6032^{***}$ |
| | (0.5431) | (0.3996) |
| HOU∗Latitude x ROC | −0.0934 | −0.0597 |
| | (0.2461) | (0.1098) |
| HOU∗Longitude x ROC | −0.2189 | $-0.6336^{***}$ |
| | (0.3299) | (0.1359) |
| PHL∗Latitude x ROC | -1.1796 | 0.1319 |
| | (0.8634) | (0.3872) |
| PHL∗Longitude x ROC | −0.1922 | $1.6491^{***}$ |
| | (0.5755) | (0.2778) |
| CLE∗Latitude x ROC | 1.9328 | $-0.8235^{***}$ |
| | (1.6421) | (0.3130) |
| CLE∗Longitude x ROC | -0.6915 | $1.1140^{***}$ |
| | (0.7704) | (0.1353) |
| SJC∗Latitude x ROC | $-4.0603^{***}$ | $-5.2159^{***}$ |
| | (0.7656) | (0.5063) |
| SJC∗Longitude x ROC | $-3.1736^{***}$ | $-3.9601^{***}$ |
| | (0.5293) | (0.2835) |

Notes: Table reports differences in parameter estimates and estimated willingness to pay from model specifications that include/omit consideration sets using experimentally identified discriminatory constraints and allow for heterogeneity by race (i.e., white renters v. renters of color). Table reports parameter estimates from model with consideration sets (left column) and without consideration sets (right column), with standard errors generated from 100 random bootstrap draws of both the first- (i.e., predicted response probabilities) and second- (i.e., residential location choice) stage models. *** p<0.01, ** p<0.05, * p<0.1.

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