Hidden Biases: Selective Advertising in the Rental Housing Market*

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Abstract

While audit and correspondence studies have documented persistent racial discrimination in rental housing, these methods are not designed to capture subtler forms of bias that can occur without renters ever realizing it. This paper examines one such overlooked practice: selective unadvertising, where landlords withhold available units from online listings and instead fill them through informal channels that disproportionately benefit white renters. We introduce a novel, data-driven approach that links unit-level rental listings from a major online platform with detailed tenant turnover records across 27 U.S. metro areas. By comparing the racial composition of occupants in listed versus "hidden" units and controlling for alternative factors that might account for racial sorting, we assess the extent of discrimination through selective advertising. We find that Black renters are more likely to occupy listed units than comparable white renters, with disparities especially pronounced in neighborhoods offering better amenities, environmental quality, and upward mobility. Moreover, the burden of selective advertising falls disproportionately on lower-income renters and families with children. Together, these findings reveal how seemingly invisible listing practices can reinforce residential segregation and unequal access to neighborhood resources by shaping how information is supplied.

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> "Apart from misrepresenting the availability of apartments to African American testers, the manager also confided to a White tester that the landlord does not advertise available apartments because 'if you run ads, you get all kinds of things.'"
>
> — Freiberg and Squires (2015)

1 Introduction

Housing discrimination is deeply harmful: it not only denies individuals a place to live but also limits their access to schools, amenities, and economic opportunities that a neighborhood offers. Although the Fair Housing Act of 1968 and its 1988 amendments prohibit discrimination in the sale or rental of housing on the basis of race, color, religion, sex, family status, and disability, bias persists at nearly every stage of the housing transaction – from the initial search (Christensen and Timmins, 2022; Ewens et al., 2014; Hanson and Hawley, 2011), to price negotiations (Bayer et al., 2017), appraisals (Ambrose et al., 2022), and mortgage lending (Aaronson et al., 2021; Ambrose et al., 2021; Zhao et al., 2006; Frame et al., 2021). Among these practices, "discriminatory steering" during home search is especially concerning because it can block access to better-resourced areas before the rest of the transaction process unfolds (Dymski, 2006; Galster and Godfrey, 2005; Yinger, 1995).

Much of the existing work on steering comes from audit and correspondence studies that use matched white and minority testers or fictitious online inquiries to detect racial gaps in landlords' response rates and the units shown to prospective renters. Yet growing evidence from enforcement agencies suggests that these methods often miss subtler forms of discrimination that occur before renters even have the chance to contact landlords. As more urban residents search for housing online, digital platforms can either bridge or widen these divides by shaping how information is supplied. With selective advertising, renters may be exposed to fundamentally different availability of listings depending on the socioeconomic makeup of the neighborhoods they search. While legal actions, most notably HUD's landmark 2019 settlement with Facebook, have prohibited explicit ad-targeting based on protected traits, they

cannot restore opportunities that were never offered. As the Fair Housing Justice Center documents, many landlords intent on controlling tenant composition "never publicly advertise," instead relying on word of mouth, referrals, or selective broker networks. In some cases, they advertise only a limited subset of units to attract an initial pool of potential renters, then assign some to unlisted units as desired. Such strategies largely evade detection by audit and correspondence studies, leaving a blind spot in fair housing enforcement (Freiberg and Squires, 2015).

In this paper, we introduce a novel, data-driven method for detecting selective unadvertising in rental markets. We define a landlord as engaging in selective unadvertising if they list some available units while leaving others unlisted AND if those unlisted units are disproportionately occupied by white renters, after accounting for factors that influence residential sorting. Unlike experimental studies that rely on matched testers or fictitious inquiries, our approach circumvents the need for costly experiments and uncovers discrimination that takes place before prospective renters can even become aware of these opportunities, offering fresh insight into hidden bias in rental markets. By identifying where these practices are most prevalent and which renters are most affected, our findings shed light on how they may shape access to resources and opportunities for vulnerable groups, while also offering actionable guidance for designing more effective monitoring and enforcement tools.[1]

Measuring selective unadvertising is challenging because it demands a setting in which we can credibly compare tenant composition in advertised versus otherwise comparable, unadvertised units. To meet this challenge, we construct a novel dataset by merging two unique and complementary sources: individual-level turnover records from Data Axle's (formerly InfoUSA) Residential Historical Files, which track millions of residents' race, ethnicity, gender, age, family, and rental status from 2006 – 2024; and granular rental listing data from

[1]The enforcement literature (e.g., Freiberg and Squires, 2015) notes that more complex testing procedures can be developed to uncover evidence of these alternative tools for intentional discrimination. This may involve deploying multiple testers who interact with property managers on several occasions. Given the substantial resources required to implement such tests, having an indication of where selective advertising may be most prevalent, and which kind of renters it may be targeting most directly, will be useful.

Dwellsy, an online platform focused on professionally managed rental housing, that provides unit-level information including address, rent, listing dates, and property manager identities from 2021 – 2024. Using supplemental data from the Census, rental registration records, CoStar database, and web-scraped listings, we validate that our rental-listing and tenant-turnover datasets reliably capture nearly the universe of the rental market segment we study.

The merged dataset offers three distinctive advantages. First, it combines never-before-linked information on unit-level tenure status, race, income, family composition, rents, and property attributes with a public-listing indicator, allowing us to to compare tenants in units that were advertised with those in units that were not, while controlling for building characteristics, neighborhood attributes, and manager-specific factors that could otherwise confound the relationship. Second, its granularity at the individual unit level enables us to compare listed and unlisted units within the same building or under the same manager, thereby isolating selective advertising for other variations in listing practices. Third, the dataset is large, containing 1,179,163 unique rental units across 27 HUD-selected metropolitan areas, providing the statistical power and external validity necessary to detect systematic selective unadvertising.[2]

A unit is classified as unlisted if it undergoes tenant turnover without having been advertised beforehand. We restrict our sample, listed and unlisted units, to buildings where at least one unit appears on the Dwellsy platform in the same year. This ensures that an unlisted unit is unlikely to reflect a landlord's fixed cost of adopting the platform; once a property manager has listed on Dwellsy, the marginal cost of listing additional units is minimal. While this conservative approach means we will not capture cases where entire buildings or neighborhoods are left unlisted on Dwellsy –possibly due in part to selective advertising – it ensures that every "unlisted" unit we identify stems from a deliberate landlord choice rather than platform constraints.

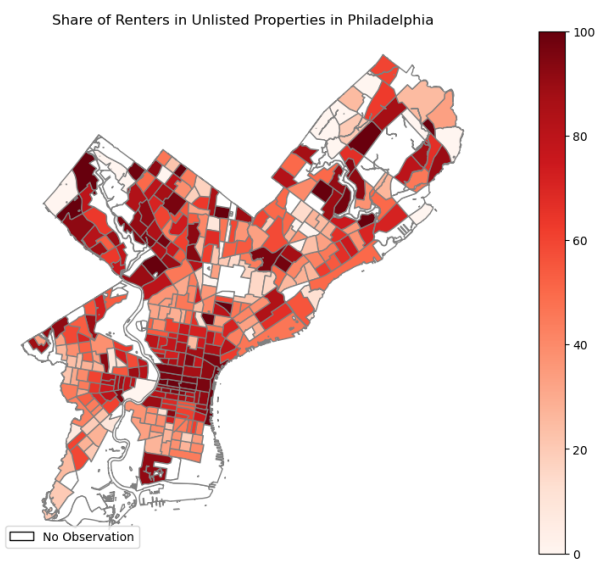
[2]The U.S. Department of Housing and Urban Development's Housing Discrimination Studies selected 28 cities based on the role of race in local housing markets; we exclude Richmond, Virginia, due to missing data.

Figure 1 (a) -(c) provides a heuristic illustration of our approach using Philadelphia as an example. Panels (a) plots the share of Black renters, while panel (b) shows the spatial distribution of unlisted units. We observe that neighborhoods with higher concentrations of unlisted properties tend to have fewer Black residents, consistent with landlords channeling white renters through informal networks such as word-of-mouth referrals. Panel (c) shows that unlisted units are more commonly found in areas with lower poverty rates, indicating that unadvertising is more likely to occur in neighborhoods that offer better local amenities.

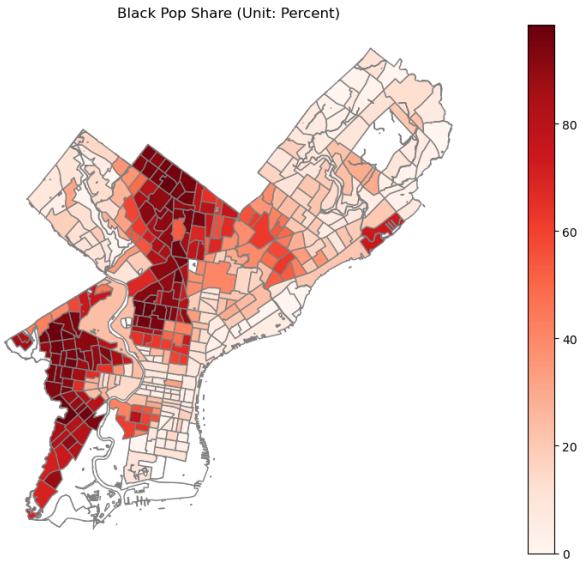
These patterns are not unique to Philadelphia. Across the 27 HUD-studied metropolitan areas, Black tenants are, on average, 10 percent more likely than white tenants to occupy listed units. Similarly, unlisted properties disproportionately appear in neighborhoods with better amenities, lower poverty, more educated populations, and superior environmental quality. Although descriptive, this evidence highlights the potential role of rental listing practice in reinforcing racial disparities in access to desirable local amenities.

Our empirical strategy starts with a multinomial logit model testing whether listed units are disproportionately occupied by Black or Hispanic tenants, controlling for a rich set of tract-level covariates, including quality-adjusted rent indices, the share of renters, the share of college graduates, local poverty rates, racial composition, air quality, school quality, alongside building quality and unit counts, as well as city and year fixed effects, all of which help account for race-based sorting unrelated to landlords' advertising choices. We find that, all else equal, listed units are 4.4 percent more likely to be rented to Black tenants than to white tenants, implying that unadvertised units are more likely to be channeled to white renter quietly.

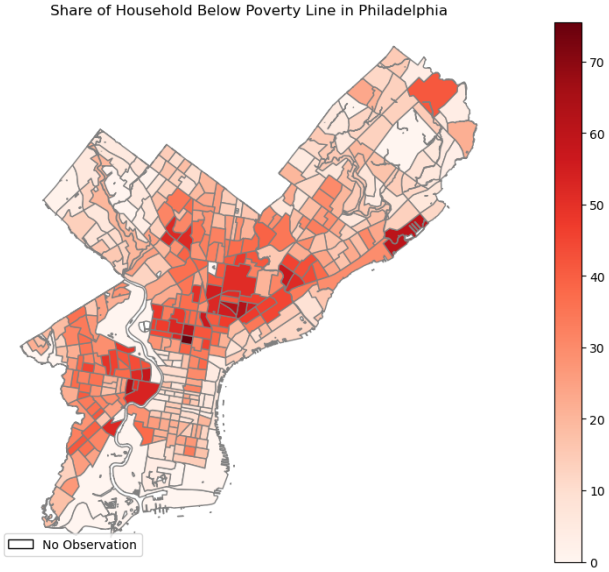
To strengthen identification even further, we estimate the multinomial logit model within the Berry–Levinsohn–Pakes (BLP) framework, allowing for granular fixed effects at the manager, tract, and even building level. This refinement takes an additional step to address potential sorting driven by unobservables specific to neighborhoods, managers, or individual buildings. For example, with manager fixed effects, identification relies on comparing listed



(a) Share of renters in unlisted units



(b) Black share of population



(c) Share of population in povery

Figure 1: Tract-level demographic statistics for the City of Philadelphia

and unlisted units operated by the same manager in buildings already active on Dwellsy. We view this specification as conservative because it cannot capture cases where some managers rely entirely on offline channels to attract preferred tenants. Nevertheless, even under this demanding specification, we find that Black renters are 3.8 percent more likely than white renters to occupy listed units. This estimate remains robust when we include tract or building fixed effects. Taken together, these findings reinforce the evidence that selective

advertising steers minority households away from unadvertised properties.

This 3.8 percent average gap masks considerable heterogeneity across neighborhoods. Using quality-adjusted zipcode rental indices (ZORI) as a proxy for local amenities, we find that in neighborhoods where ZORI is above the MSA average, listed units are 7.5 percent more likely to house Black tenants than white tenants, whereas below-average-ZORI neighborhoods show no such disparity. Moreover, selective advertising is especially pronounced in neighborhoods with above-average air and water quality, as measured by the EPA's Risk Screening Environmental Indicators (RSEI): in these areas, listed units are 10.1 percent more likely to house Black renters. The disparity is even larger in neighborhoods with above-average opportunity scores (Chetty et al., 2014), where listed units are 15.9 percent more likely to house Black tenants.

Thus, selective advertising not only limits Black renters' housing consumption options but, even more concerning, steers them away from neighborhoods with better amenities and resources. This is consistent with a report from the 2007 American Housing survey, which shows that Black households are 30 percent more likely than white households to report safety concerns with their drinking water, 60 percent more likely to report a serious crime in their neighborhood, and twice as likely to report being dissatisfied with the neighborhood elementary school. Our results suggest that at least part of this disparity could stem from discrimination that begins at the very start of the rental search process.

Beyond race alone, we find that selective advertising also disproportionately affects lower-income renters and families with children. Else equal, unlisted units are more likely to house higher-income tenants than lower-income ones, and households without children more often occupy unlisted units than those with children. While these patterns suggest that income and family status shape access to unadvertised housing, striking racial disparities persist even after accounting for both factors. Among lower-income renters, Black and Hispanic households are still 3.2 and 4.7 percent more likely, respectively, to occupy advertised units than comparable white renters, a gap that largely vanishes for higher-income renters.

For families with children, the disadvantage is even more pronounced: Black households with children are twice as likely as white families with children to live in advertised units, and even Black renters without children are 5 percent more likely than their white peers. Taken together, we find that lower-income Black families with children face the steepest barriers to accessing unadvertised rental units, showing how landlords' selective withholding of listings can reinforce disadvantage precisely where fair access is most needed.

Compared to traditional forms of discrimination in rental markets, such as fewer showings or uneven landlord callbacks, selective advertising operates quietly, almost invisibly, yet may inflict even greater harm by constraining housing availability at the very outset of a home search process. We find that the degree of selective advertising is greater in areas where more blatant forms of discrimination (e.g., landlords' response rates to email or phone inquiries) are more pronounced. This co-occurrence suggests that traditional audit-based measures of bias may capture only the "tip of the iceberg." By the time a prospective renter ever makes contact, selective advertising has often already diverted them away from many available units. Since most urban renters today start their search online, enforcement must extend beyond interactive correspondence or audit studies to include systematic oversight of digital ad placements – bridging the online listing divide and ensuring fair information access to the full supply of housing.

2 Literature Review

2.1 Discriminatory Use of Information in Online Housing Markets

Our work is related to a literature that has explored the use of information in housing markets to achieve discriminatory outcomes. Prior to the Internet age, Galster et al. (1987) examine data on the listing behavior from a single large real estate company in Milwaukee, WI and find that sellers in Black neighborhoods receive less advertising activity (fewer/shorter ads and fewer open houses) compared to those in predominantly white neighborhoods. More recently,

Boeing (2020) analyzes the over- versus under-representation of vacant units on Craigslist, and Adu and Delmelle (2022) examine the differential use of exclusionary language depending upon the racial makeup of the neighborhood where a unit is located. Besbris et al. (2021) focus on how advertisement language on Craigslist either emphasizes unit and neighborhood attributes or focuses on application criteria and logistics depending upon the neighborhood where the unit is located. Most directly related to our work, Humber and Matthews (2020) consider National Fair Housing Alliance v. Facebook, a case in which plaintiffs accused Facebook of allowing housing advertisers to target users based on protected characteristics such as age, sex, race, or zipcode.

2.2 Experimental Approaches to Measuring Discrimination

Extensive research has been conducted on field experiment methods to reveal discrimination. Bertrand and Duflo (2017) offer a comprehensive overview, highlighting the differences between audit and correspondence studies. In an audit study, two testers – differing only by a specific characteristic like race – perform real-world tasks (such as inquiring about property availability) in person. These studies have been used to explore various markets, including bargaining at car dealerships (Ayres and Siegelman, 1995), gender discrimination in restaurant hiring (Neumark et al., 1996), and the combined effects of race and criminal records on employment (Pager, 2003). By contrast, correspondence studies involve fictitious applicants who interact solely via mail or online.

These methods were developed as alternatives to simple outcome studies, where discrimination is measured by regressing economic outcomes (e.g., wages) on productivity indicators and race. Such outcome studies may be biased by unobservable factors related to productivity and race. Audit and correspondence studies, with their controlled settings, are designed to mitigate this issue.

2.3 Paired-Tester Audit Studies

A significant body of research has examined discrimination in the process of searching for rental and sales properties, especially at the stage when potential buyers or renters inquire about advertised units and receive recommendations or viewings. This phase is crucial as it helps shape the choice set available to the prospective tenant or buyer. Historical data from HDS audits conducted in 1977, 1989, 2000, and 2012 shows a dramatic decrease in overt forms of housing discrimination, such as not showing an advertised property. However, it remains unclear whether more subtle forms of discrimination, like the number of properties shown, have also declined. Research in this field often focuses on the likelihood that majority versus minority testers are given property viewings and opportunities to inspect. To understand the underlying mechanisms, studies typically test three hypotheses:

(i) Taste-based discrimination, where greater bias is expected from realtors with personal attributes that may make them more prejudiced;

(ii) Catering to white customers, where realtors may discriminate to avoid offending a prejudiced White clientele, especially in neighborhoods at risk of gentrification or where white customers have concerns about school integration; and

(iii) Statistical discrimination, where realtors base their behavior not on the individual characteristics of a minority customer but on the average attributes associated with that customer's race or class group.

Research in this area has largely focused on paired-tester audits, with notable studies including those conducted in Boston in 1981, as well as the HDS audits in 1989, 2000, and 2012. These studies share a common goal: measuring discrimination at the stage when realtors suggest rental units or homes for purchase and when customers are invited to inspect properties. However, they vary in their measures of discrimination and the minority groups they examine. Yinger (1986), using data from the 1981 Boston audit, examined discrimination between white and Black testers. He advocated for the use of tester-pair fixed effects to

account for unobservable factors specific to each audit pair (i.e., the two testers visiting a given realtor), a method that has since become standard in the field. Yinger's findings revealed that, on average, Black testers were informed of roughly 30 percent fewer rental units than their White counterparts, and White testers were invited to inspect 57 percent more apartments. Among the three hypotheses commonly explored in this literature, Yinger's study provided evidence supporting discrimination aimed at catering to a white customer base. Page (1995) builds upon Yinger (1986) by using a Poisson model to account for the discrete and non-negative nature of the number of houses shown, as well as the heteroskedastic variance dependent on the total number of units available. Analyzing data from the 1989 HDS audit, Page finds that Black and Hispanic testers are shown 80 percent and 90 percent of the number of units shown to White testers, respectively. These results are consistent across both rental and sales properties, though the disparity is more pronounced in some cases. The findings are best explained by either white customer or statistical discrimination mechanisms, particularly for Black testers. Analysis across HUD's Housing Discrimination Studies indicates that one of the most enduring forms of discrimination is the steering of minority households into minority neighborhoods during the search stage (Dymski, 2006; Galster and Godfrey, 2005; Yinger, 1995). Christensen and Timmins (2022) observe significant differences in neighborhood characteristics—such as pollution, crime, poverty, and the skill levels of local residents—of properties shown by realtors to white, African American, Hispanic, and Asian testers in the 2012 HDS. Further, Christensen and Timmins (2023) use a correspondence study, involving online interactions with racially distinct names to assess rental markets in five major cities. They find that discrimination results in average welfare costs – defined as an equivalent variation in income, or the amount of income that you would need to take away from individuals in the absence of discrimination to induce the same loss of welfare as caused by discriminatory constraints, to be 3.5 to 4.4 percent of annual income for renters of color, with African American renters experiencing higher welfare costs as their incomes increase. There is a literature describing the limitations of audit studies. For exam-

ple, it may be difficult to test for particular types of bias, such as bias against individuals with certain disabilities. It is also unlikely that testers will be identical in all respects except for the attribute of interest. Moreover, testers are aware of their role and may act in such a way as to try to sway the results toward or against finding evidence of discrimination. While sacrificing some richness in the portrayal of a racial identity, the investigator retains more control in a correspondence study where identities are typically conveyed by a name on an application or resume and can therefore be more easily controlled.

2.4 Correspondence Studies

Correspondence studies have been employed to examine labor market discrimination based on race and ethnicity. One of the most well-known studies used fictitious resumes sent in response to job advertisements in Boston and Chicago newspapers, varying only by the racialized names (Bertrand and Mullainathan, 2004). The findings revealed that resumes with white-sounding names received 50 percent more callbacks, and that stronger observable resume attributes mitigated the negative impact of Black names, indicating statistical discrimination. Following this, correspondence studies have explored discrimination in various other labor market contexts, including race and ethnicity (McGinnity et al., 2009; Baert et al., 2015; Booth et al., 2012; Galarza et al., 2014), gender (Carlsson, 2011; Booth and Leigh, 2010), caste and religion (Banerjee et al., 2009; Wright et al., 2013), previous unemployment spells (Eriksson and Rooth, 2014; Ghayad, 2013), and sexual orientation and appearance (Ahmed et al., 2013; Patacchini et al., 2015; Bailey et al., 2013; Rooth, 2009). In housing rental markets, correspondence studies have analyzed the role of race and ethnicity (Carlsson and Eriksson, 2014; Ahmed and Hammarstedt, 2008; Ahmed et al., 2010; Ewens et al., 2014; Hanson and Hawley, 2011; Carpusor and Loges, 2006), sexual orientation (Ahmed and Hammarstedt, 2009), and immigrant status (Baldini and Federici, 2011; Bosch et al., 2010). One problem that might arise in both audit and correspondence studies, specific to the internet age, is the potential for those being audited to check the online profile of the

tester or fictitious applicant. To address this problem, Acquisti and Fong (2015) develop an online presence for their fictitious applicants in a correspondence study of labor market discrimination. Similarly, Bartoš et al. (2016) create websites for applicants and monitor their access rates. A separate set of problems may arise if those being audited actively pursue strategies to avoid detection.

2.5 Enforcement

Fair housing enforcement groups have uncovered ample evidence that landlords and property managers are aware of the audit and correspondence studies described above and actively undertake strategies to avoid detection.[3] Freiberg and Squires (2015) describe how property managers use word-of-mouth, referrals from existing tenants, real estate agents, and online platforms to find prospective applicants without listing vacant units. This allows them to screen applicants before making properties available. In one example, Freiberg and Squires (2015) note, "Although housing providers may have many reasons for using fewer public sources to reach prospective renters or buyers, discrimination is more likely to occur when providers restrict knowledge of, or access to, available housing by limiting advertising primarily to favored populations." They go on to say that "Recent enforcement testing suggests that contacts by testers to housing providers, as part of initial visits by matched paired testers, may not always capture the housing provider's practices in a way that adequately discloses or confirms whether fair and equal treatment is being provided." While enforcement groups have developed strategies to document this sort of discriminatory behavior, these approaches are time-consuming and labor-intensive. The alternative strategy that we describe below takes a data-intensive approach to finding broad evidence of selective advertising and to learning about the contexts in which it is most likely to take place.

[3]For example, see https://fairhousingjustice.org/about-fhjc/our-history/.

3 Data

Our primary data sources are Dwellsy and InfoUSA. Dwellsy provides rental listings, including USPS-verified addresses, unit numbers (when applicable), asking rents, listing and delisting dates, and the name of the property manager. As a listing platform that contracts with property management software providers, Dwellsy does not charge per listing. Property managers using such software can advertise units on Dwellsy at no additional cost.

Individual and household characteristics are from InfoUSA's Residential Historical Database, which tracks individuals' addresses using a wide range of public and proprietary sources. This dataset includes USPS-verified addresses and imputed demographic and economic variables, such as household income, wealth, age, gender, and race/ethnicity, for a large panel of U.S. residents.

We provide details on Dwellsy and InfoUSA in Sections 3.1 and 3.2, and describe our matching procedure and the measure of a unit being (un)listed in Section 3.3. We then introduce the ACS data in Section 3.4 and report descriptive statistics from the merged sample in Section 3.5.

3.1 Dwellsy

Our data on rental listing come from Dwellsy, a rental listing platform. When a rental unit becomes available, the decision of whether to advertise it falls to the property manager, who may or may not also be the owner of the unit. The most common form of advertising is listing available properties on websites that aggregate listings across managers, such as Apartments.com and Dwellsy. Professional property managers tend to use management software both to track their availability internally and to efficiently list on multiple websites at once. The number of listing websites available depends on whether those websites have contracted with the company that provides the management software, so the property manager need not have adopted software that is specific to Dwellsy or even have had knowledge

of Dwellsy prior to the listing decision in order to enter our sample.

Pricing for management software varies by firm as well as the array of services provided, the number of units under management, and contract specifics such as whether billing is annual or monthly. Unlike most listing platforms, Dwellsy does not charge a per-listing fee. Thus, if a unit in a building – where the manager has listed other units on Dwellsy within the same year – remains unlisted prior to tenant move-in, it is likely to have been deliberately withheld.

The Dwellsy data span from January 2021 through the end of 2024 and cover 48 major U.S. metropolitan areas, including 27 HUD-selected metros. Across these 27 metro areas, Dwellsy records a total of 1,179,163 unique rental units. We begin by focusing on the 27 central cities within these metropolitan areas. Before matching to InfoUSA, we observe 550,623 unique listed rental units across these cities.

Each listing includes the name of the listing company, which typically reflects the name of the software account holder, often an LLC that appears to be a property management firm. While some large management companies operate across multiple markets, we identify managers separately within each metro to avoid misclassifying firms with common names (e.g., "Midtown Management" or "Total Property Management," or in some cases, simply first names) as multi-market entities.

A key advantage of the Dwellsy data relative to other online rental listing platforms is its accuracy, which stems from two key features of how the data are collected and maintained. First, Dwellsy obtains listing information directly from professional property managers via software integrations, eliminating the need for web scraping and reducing recording errors. In contrast, many existing studies rely on scraped data from rental websites, which are often subject to incomplete captures, duplication, and outdated listings.

Second, on some online platforms, only the number of available apartment units by type (e.g., two-bedroom and one-bathroom) is advertised. Some units are listed as teaser units to keep the apartment building visible on the market, while some rental listings remain active

long after being leased out. These practices obscure the true availability of rental units and hinder accurate matching of tenants to listings, both of which are central to our identification strategy. Dwellsy avoids these pitfalls by requiring professional managers to truthfully report which apartment units are available in the buildings they manage and to regularly verifyand update these units' status.

While Dwellsy primarily covers professionally managed buildings, and thus does not capture the full rental market, its accuracy in reporting and monitoring listings, ensured by professional managers, makes it well suited to our empirical analysis. To assess the platform's coverage and benchmark it against the broader rental stock, we draw on three complementary data sources: (1) rental registry records from the City of Seattle, (2) building-level listings from CoStar, and (3) scraped listings from ten major online rental platforms.

Comparison with Rental Registry Records We begin by comparing the Dwellsy data to administrative rental records from the City of Seattle. Specifically, we obtained the full registry of rental properties as of year-end 2024 from the Seattle Department of Construction and Inspections (SDCI), which has required all rental properties to be registered since 2017. In addition to property addresses, the registry includes the number of rental units in each building and the designated "contact" for the property, typically the name of the property management company.

Table 1 summarizes Dwellsy's coverage of the Seattle rental market. Among properties registered with the city, 24.7 percent of buildings listed at least one unit on Dwellsy during our sample period. These buildings account for 35 percent of all registered rental units in the city. Furthermore, 44 percent of registered buildings, and 51.5 percent of registered units, share a property manager contact with at least one building that listed on Dwellsy.

It is important to note that the rental registry reflects the full stock of rental units, whereas Dwellsy captures only units actively listed between January 2021 and June 2025. Given Dwellsy's focus on professionally managed properties and the fact that not all units

	Coverage Statistic	
	% Rental Buildings	% Rental Units
Building in Dwellsy	24.7	35.0
Property manager in Dwellsy	44.0	51.5

Table 1: Dwellsy Rental Market Coverage in the City of Seattle

Notes: A building is determined to be in Dwellsy if a unit in it was listed on Dwellsy between January 2020 and June 2025. A property manager is determined to be in Dwellsy if they are listed as a contact of a building that had a unit listed on Dwellsy during the same period. The first column displays the share of buildings that are registered as rental properties in the city of Seattle in 2024 that are either in Dwellsy or have a property manager in Dwellsy. The second column displays the share of units in buildings that are registered as rental properties in 2024 that are either in Dwellsy or have a property manager in Dwellsy.

turn over during the sample period, the observed 35 percent rental-unit-level coverage and 51.5 percent manager-level overlap provide strong support for the representativeness of the Dwellsy data within the professionally managed segment of the Seattle rental market.

Comparison with CoStar Listings As a second benchmark, we draw on data from CoStar Group, a commercial real estate analytics firm, covering all multifamily properties in five cities during July and August 2023. Unlike the Seattle rental registry, the CoStar data include detailed vacancy information at the building level. CoStar compiles its data from a range of sources, including Apartments.com (a subsidiary), public records, commercial mortgage-backed securities (CMBS) pools, property manager interviews, and other online listing platforms. As a result, CoStar's database provides a comprehensive view of the rental stock, both listed and unlisted, for the properties it monitors.

For each city, we adopt CoStar's "market" definition, which encompasses an area larger than the city boundary but smaller than the full metropolitan statistical area (MSA). To assess Dwellsy's coverage within these markets, we begin with all listings from Dwellsy across the entire census-defined metropolitan area and retain only those that match to a property in the CoStar sample. As in the Seattle comparison, we match buildings based on street address and consider a property covered by Dwellsy if it had at least one 2023 listing that corresponds to a CoStar building with an active vacancy.

Table 2 reports the resulting coverage statistics. Across the give cities, Dwellsy captures

	Buildings with Vacancies		Units in Buildings with Vacancies	
City	#	% Covered	#	% Covered
Chicago	233	32.19	24,031	55.31
DC	1,116	11.47	134,414	16.54
Minneapolis	903	26.25	63,510	16.79
Philadelphia	1,158	20.98	98,305	14.73
Pittsburgh	556	26.26	49,165	14.37

Table 2: Dwellsy Coverage in the Costar Multifamily Sample

Notes: The geographic area for each city is determined by CoStar "markets", which are generally larger than a central city but smaller than a metropolitan area. A building in the CoStar sample is considered to have a vacancy if it had a nonzero count of housing units marked as available when the full multifamily sample for its city was downloaded between July and August of 2023. A building is considered covered by the Dwellsy sample if any of its units were listed on Dwellsy in 2023.

between 11.5 and 32.2 percent of buildings with vacancies in CoStar. In terms of units, coverage ranges from 14.5 to 55.3 percent. It is important to emphasize that CoStar's data include all known rental vacancies, regardless of whether they are listed online. In contrast, Dwellsy reflects only the subset of available units that are actively advertised on its platform, with a focus on professionally managed residential properties. Given this narrower scope, Dwellsy's average coverage of approximately one quarter across cities appears both reasonable and consistent with its platform design.

Comparison with Scraped Listings Thus far, we have benchmarked the Dwellsy data against the full residential rental *stock* in Seattle (via the city's rental registry) and the vacant rental *stock* in selected cities (via CoStar). In both cases, Dwellsy listings represent a substantial share of the market. To further assess Dwellsy's *listing* coverage, we collected rental listings from ten major online platforms between July and August 2023 in six cities: Atlanta, Boston, Washington, DC, Minneapolis, Richmond, and Seattle. These platforms include both nationally recognized sites and lesser-known websites that tend to serve lower-income households.

For each listing, we recorded the address, listing date, asking rent, and basic unit characteristics. After geocoding the listings, we compared them to Dwellsy listings over the same

time period, restricting attention to buildings with at least one unit listed on Dwellsy. The goal was to see how many of these listings appeared on other rental websites but not on Dwellsy.

Table 3 reports the results. Panel A presents city-level comparisons of the average number of units listed per building on Dwellsy versus the scraped platforms. Across the six cities, the average number of listings is generally similar across sources, with the notable exception of Washington, DC. The discrepancy in DC is driven by a single building that had 167 more units listed on another platform than on Dwellsy. Upon closer inspection, we find that this reflects a reporting error on the external platform. To mitigate the influence of such outliers, the right-hand panel reports differences in the number of listings per building at the 25th, 50th, and 75th percentiles. These differences are near zero across all cities, including Washington, DC.

Panel B further breaks down the comparison by source platform of the scraped data. When compared to each alternative rental platform, the number of units listed per building on Dwellsy is similar, with the difference again close to zero at the 25th, 50th, and 75th percentiles. On average, Dwellsy lists approximately one fewer unit per building. This slight shortfall likely reflects Dwellsy's emphasis on professionally managed properties and its systematic efforts to verify listings, eliminate duplicates, and remove off-market units.

Overall, this comparison gives us high confidence that Dwellsy's coverage of listings in Dwellsy-covered buildings is nearly comprehensive. This aligns with the fact that Dwellsy does not charge per listing fees, allowing managers to advertise their listings at no additional cost beyond the initial software integration.

3.2 InfoUSA

We obtain individual and household characteristics, along with information on residential turnover, from Data Axle's InfoUSA Residential Historical Database, an annual panel constructed from a wide range of public sources (e.g., voter rolls, land records) and proprietary

	Mean Listed Units		Dwellsy Listings - Scraped Listings		
	Dwellsy	Scraped	p25	p50	p75
Panel A: All scraped sources, by city					
Atlanta	2.2	2.4	0	0	0
Boston	1.7	1.9	0	0	0
DC	2.7	5.5	-1	0	0
Minneapolis	2.8	3.7	-1	0	0
Richmond	1.7	2.0	0	0	0
Seattle	2.4	3.0	0	0	0
Panel B: All cities, by scraped source					
Platform #1	2.1	3.2	-1	0	0
Platform #2	1.6	2.0	-1	0	0
Platform #3	1.6	1.9	0	0	0
Platform #4	12.4	16.8	-6	-4	-3
Platform #5	1.0	1.0	0	0	0
Platform #6	3.0	1.0	2	2	2
Platform #7	3.3	3.7	0	0	2
Platform #8	2.5	4.1	0	0	0
Platform #9	3.0	4.2	-2	-1	0
Platform #10	2.4	2.9	0	0	0
Total	**2.4**	**3.4**	**0**	**0**	**0**

Note: Platform names are withheld for confidentiality.

Table 3: Listing Coverage in Dwellsy-Covered Buildings

data (e.g., marketing files). The resulting coverage is broad and detailed.

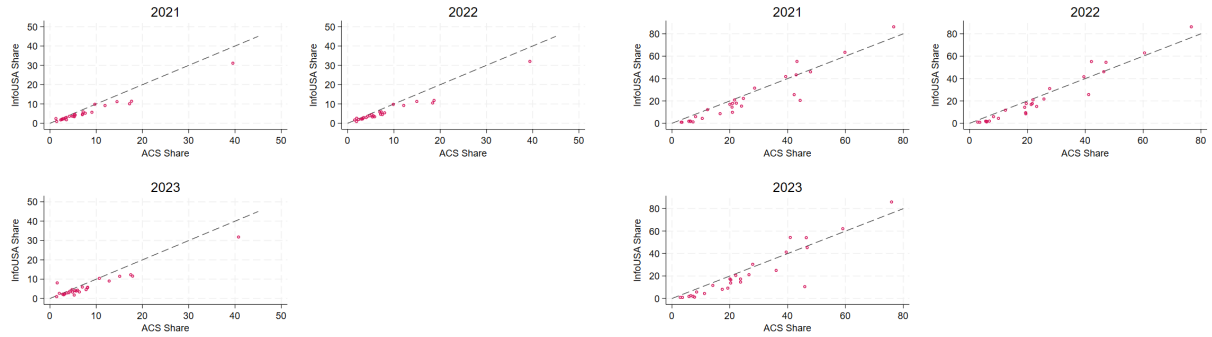
InfoUSA imputes racial and ethnic identity from an individual's full name and location. The algorithm first consults reference tables linking first names to ethnic backgrounds; when that yields no unique match, it sequentially considers the surname, expert linguistic rules (e.g., surnames ending in "oglu" are flagged as Turkish), and middle names. Ambiguities are further resolved using nine digit ZIP code demographics. Because race is imputed rather than observed, we later benchmark InfoUSA's city level racial composition against the American Community Survey (ACS) across our sample period.

We collapse InfoUSA's detailed ethnicity codes into five mutually exclusive groups: Black, White, Hispanic, Asian, and Other.[4] Movers in the InfoUSA panel are identified by comparing their address (including unit number) in a given year with the previoius year's address, or if InfoUSA indicates that they have lived in their unit for a year or less and they were most recently observed at their current address in the current year. Individuals are dropped from the InfoUSA sample if (i) they lack an address, (ii) they lack a unit number at a known multi unit address, or (iii) they have no address in both of the preceding two years.[5]

Comparison with the American Community Survey To ensure both the representativeness of the InfoUSA sample and that the ethnic codes in InfoUSA and our aggregation of the codes into race categories are consistent with socioeconomic conceptions of race, we

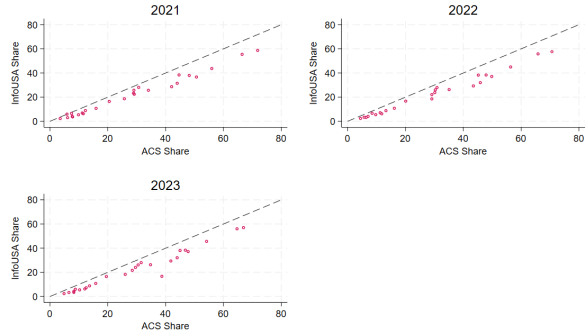
[4]White includes ethnicities classified as Western European, Scandinavian, Australian, New Zealander, Mediterranean, Jewish, and European Hispanic (i.e., Spanish and Portuguese). Black includes only African American. Asian includes all East Asian, South Asian, and Pacific ethnicities. Hispanic includes all non-European Hispanic ethnicities and Brazilians. Other includes all other categories, which in practice includes ethnicities classified as Caucasian (i.e., central Asian), Surinamese, Arab, and African (but not African American). To account for the possibility that Africans and African Americans experience comparable discrimination in the rental market, we test a racial coding that includes Africans in the Black designation and re-generate our primary results, which do not meaningfully change. InfoUSA includes missing ethnicity observations. These individuals are dropped from the sample. In cases where an individual has multiple ethnic designations, InfoUSA uses both first and last name to assign ethnicity (e.g., Nguyen O'Brien is coded as Vietnamese).

[5]It is not uncommon for an individual to be absent from a single year of the InfoUSA data. If the individual lived in the same address both the following and prior year, the address is imputed to have been unchanged in the missing year. If their address was missing in the prior year but was not missing and different two years prior, they are assumed to have moved in the current year. These cases are noted so that they can be dropped as a robustness check.

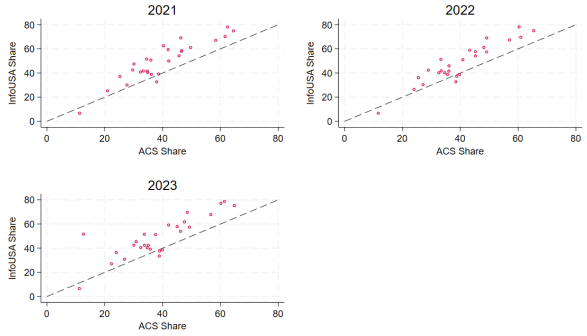


(a) Asian share

(b) Black share



(c) Hispanic share



(d) White share

Figure 2: Share of households by race, city, and year

aggregate the full InfoUSA sample to the city-year level, calculating shares by racial category and compare these shares to their corresponding one-year American Community Survey (ACS) estimates. We perform analogous checks for household income distributions and the share of households with children, variables that also enter our empirical specifications.

Figure 2 compares city-level racial composition estimates from InfoUSA (y-axis) and the American Community Survey (ACS, x-axis) for the years 2021–2023. Panels A through D correspond to the shares of the population identified as Asian, Black, Hispanic, and White, respectively. Panels A and C show that InfoUSA tends to slightly understate the Asian and Hispanic population shares relative to the ACS, while Panel D shows the opposite pattern for the White population share. Importantly, the estimates for the Black population share (Panel B) align closely between InfoUSA and the ACS across cities, providing reassuring validation given our focus on potential listing discrimination against Black renters. These



Figure 3: Comparison of the income distribution estimates for the city of Philadelphia between ACS and InfoUSA

patterns also suggest that some Asian and Hispanic individuals in the InfoUSA data may be misclassified as White.

Figure 3 compares the income distribution for the city of Philadelphia in each year of our sample as estimated by ACS and InfoUSA. Analogous comparisons for all cities in the sample are provided in Appendix B. As those distributions show, Philadelphia is broadly representative of the overall fit: although the match is weaker in some cities (e.g., Riverside, CA) and stronger in others (e.g., Fort Worth, TX), the two distributions generally align across the income range. InfoUSA tends to estimate a higher share of households in the lowest income brackets, while the ACS reports a greater share in the highest brackets, particularly in the top income bin. This pattern holds across cities in the sample, though it is more severe in some cities than others. Appendix B further compares the distribution of families with children as reported by ACS and InfoUSA.

3.3 Identifying Listed and Unlisted Status of Turnover Units

We merge Dwellsy listings with InfoUSA panels at the address level (including unit number) to obtain a dataset of renters in units that were either listed on Dwellsy or not. Figure 4 illustrates this process. Prior to turnover, a property manager decides whether to adopt listing software. If the manager does not use such software or subscribes one that does not contract with Dwellsy, the eventual tenant does not appear in our merged sample, regardless of whether the unit is advertised elsewhere. If the unit manager does subscribe to a listing software that contracts with Dwellsy, tenants enter our sample only if they are also observed in the InfoUSA panel.

If the unit rents to an individual in the InfoUSA panel, then the unit will be included in our panel in one of two cases: if the manager listed the unit on Dwellsy, then the unit is in our sample and coded as having been listed. If the manager did not list the unit on Dwellsy but within the same year listed other units, then the unit is included in our sample and coded as being unlisted.

To construct the sample, we first subset both datasets by Census metropolitan area and then merge on street address, zipcode, and unit number where available. The final sample includes 27 central cities within 25 metropolitan areas.[6] For units that were listed multiple times within the same calendar year, the most recent listing is retained.[7] A listing is matched to a mover if it is deactivated in the year that InfoUSA first records the tenant at that address, provided the move occurs within one year of the initial listing date.

Given the nature of Dwellsy's data generating process – managers may advertise a unit on Dwellsy's platform without cost as long as the listing is created in their software – we identify the unit inhabited by a mover in the InfoUSA panel as being unadvertised if it was

[6]The sample includes all observations from the cities and respective metropolitan areas of Albuquerque, Atlanta, Baltimore, Boston, Chicago, Cleveland, Columbia (South Carolina), Dallas, Washington (D.C.), Detroit, Fort Worth, Houston, Kansas City, Los Angeles, Miami, Minneapolis, Newark, New York, Philadelphia, Pittsburgh, Providence, Riverside, San Antonio, San Diego, San Jose, Seattle, and Tampa.

[7]These cases are overwhelmingly due to changes in listing details, with re-listings occurring subsequently in a short period of time. For example, a change in the asking price is registered in the data as a re-listing.

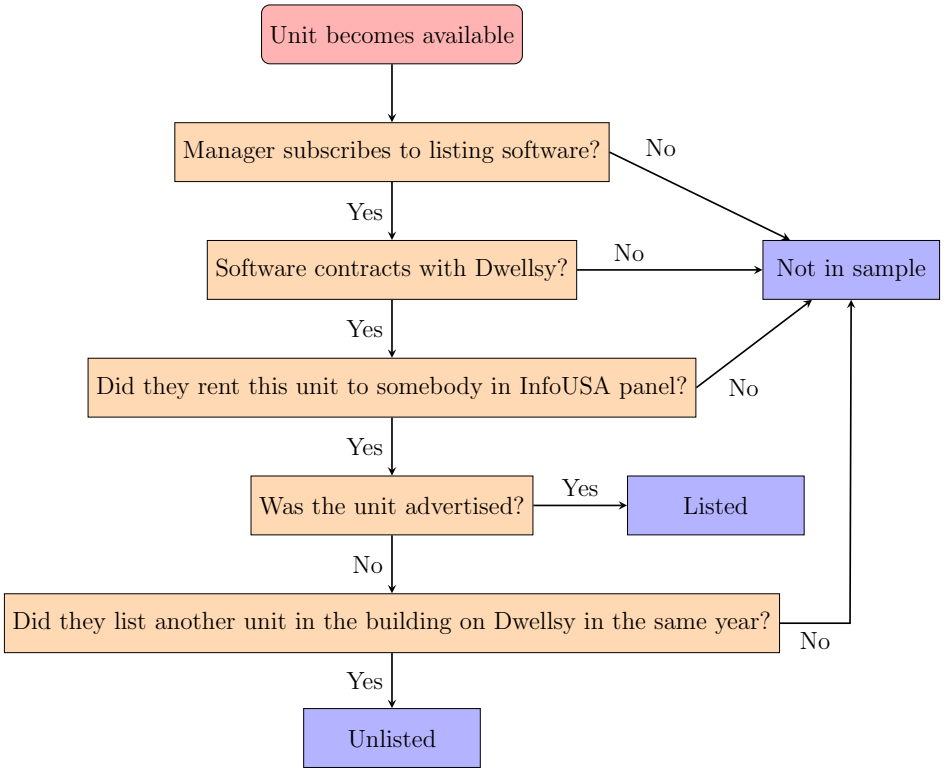


Figure 4: Process that determines whether a turnover in a unit is included in the sample and how it is coded

not listed on Dwellsy but is in a building that had other units listed on Dwellsy in the same year.

3.4 American Community Survey

To explore how listing decisions vary with neighborhood attributes and to control for the probability that potential tenants of a given racial category would apply for a given unit, we obtain tract-level sociodemographic characteristics from the five-year American Community Survey (ACS). Although our data span 2021 to 2024,[8] we use ACS data from the 2019 survey to avoid a five-year sample that includes the 2020 ACS data, which the U.S. Census Bureau chose not to release due to issues of potential non-response bias. In each tract, we retrieve the percentage of renters who are non-Hispanic white, Black, Hispanic, and Asian or Pacific Islander. We also obtain the percentage of households that are renters or have children and the percentage of the population that is in poverty or has a bachelor's degree or higher.

3.5 Sample Statistics

Table 4 presents neighborhood-level summary statistics for the merged Dwellsy-InfoUSA-ACS sample, disaggregated by tenants' race. Tenants tend to live in neighborhoods with a higher share of renters from their own racial group. Compared to white tenants, Black tenants are more likely to live in neighborhoods with higher poverty rates, greater exposure to pollution, fewer college-educated residents, and lower overall amenities as measured by the ZORI index.

Table A1 reports the share of renters living in listed units, broken down by race and by city. On average, 25.68 percent of tenants occupy units that were advertised on Dwellsy. In some cities, like Fort Worth, the share of renters in listed units reaches as high as 40 percent, while in median cities such as Los Angeles and San Diego, it is closer to 26 percent.

[8]The matched sample excludes 2021 despite each of our data sets including 2021 because the lack of 2020 Dwellsy data and the fact that most 2021 InfoUSA households were observed in the first half of the year induce a high risk of erroneous matches of listings to recently-moved residents.

	Asian	Black	Hispanic	Other	White	All
% Renters Black	11.81	55.38	14.60	15.71	12.20	16.82
% Renters White	52.34	26.58	42.07	51.26	56.77	51.06
% Renters Asian	16.41	4.76	9.40	11.75	10.33	10.37
% Renters Hispanic	16.69	11.58	31.72	18.68	18.00	19.22
% Population Renters	68.62	64.75	63.55	65.21	64.01	64.56
% Households w/ Children	33.87	40.02	39.56	36.27	35.73	36.51
% BA+	64.40	39.26	48.74	59.46	62.42	58.32
% in Poverty	14.45	22.94	15.47	14.44	13.07	14.57
ZORI Zip Index (MSA Index = 100)	109.53	92.77	101.39	106.95	108.02	105.77
Toxic Concentration (normalized)	0.14	0.04	-0.03	-0.02	0.04	0.04
PM 2.5	8.14	8.20	8.22	8.26	8.13	8.15

Table 4: Covariate Means by Race

Notes: Values display the correlation between the variable named in the first column and a dummy variable that equals 1 if the tenant is of the race listed in the column header and equals zero otherwise. The Zillow Observed Rent Index (ZORI) is a repeat-rent index of rental rates provided by listing company Zillow.com, released at both the zip code and metro area level each month. We divide the zip code–level index by the MSA-level index and multiply by 100 such that a value of 100 implies that the average rent in the zip code is exactly equal to the average rent in the metro area in a given month. PM 2.5 is the concentration of PM 2.5 pollution in the census tract, and toxic concentration is measured using the EPA's Risk Screening Environmental Indicators (RSEI) data, which combines TRI data on the quantity of toxic chemicals released or transferred off site with information about the chemical's toxicity and its transport through the environment, to calculate a geographically precise measure of relative risk. We normalize toxic concentration by subtracting the MSA-wide mean and dividing by the MSA-wide standard deviation. All variables listed in the first column are defined at the tract level other than ZORI, which is defined at the zipcode level.

One concern is that Dwellsy may not capture the full universe of online listings for a given property. However, as detailed in Section 3.1, we compare Dwellsy listings to rental data scraped from ten major online platforms across five major cities. The results indicate that Dwellsy provides near-complete coverage of online listings within buildings already active on its platform.

The relatively low share of listed units, despite the fact that the fixed cost of advertising has already been incurred for these buildings, suggests that, unlike in the owner-occupied housing market, a substantial fraction of rental units are leased without ever being advertised. At first glance, this may seem surprising. However, it reflects important institutional differences between rental and owner-occupied markets. Sales transactions for owner-occupied homes are formally recorded in public deed registries, making it possible to verify whether and when a transaction occurred. No equivalent system exists for rental transactions. More-

over, while roughly 90% of home sales are brokered by agents who are required to advertise through the Multiple Listing Service (MLS), advertising requirements in the rental market are far less standardized and less stringent. Together, these factors make rental activity more difficult to monitor and give landlords wide discretion over whether to list a unit at all.

Table A1 further reveals that, in general, Black renters are more likely to occupy listed units than white renters. For example, in Kansas City, 46 percent of Black tenants live in listed units compared to 36 percent of white tenants. In Philadelphia, these figures are 33 percent versus 21 percent; in Chicago, 25 percent versus 13 percent; and in Atlanta, 21 percent versus 12 percent, respectively.

Of course, a unit being leased without first being listed does not, on its own, imply selective advertising. Some degree of non-listing can arise from local institutional practices or rational managerial decisions. For instance, corporate relocation programs may involve tenant placements coordinated internally by organizations. Likewise, property managers may use word-of-mouth referrals to attract tenants they perceive as desirable. To test for potential discriminatory patterns, our main analysis later in this paper employs an econometric model to estimate whether unlisted units are *disproportionately* occupied by White renters, controlling for sorting based on neighborhood-, landlord- and building-level characteristics. For example, if word-of-mouth referrals typically come from tenants whom landlords already "favor", and those tenants tend to recommend friends of the same race, then the decision not to list would only be selective if landlords *disproportionately* favor white tenants in the first place, controlling for the racial composition of the existing local population.

With each renter in the sample classified as occupying either a listed or an unlisted unit, we can examine how different tract-level characteristics are distributed across neighborhoods with varying shares of unlisted units. Figure 5 shows histograms of key characteristics, along with the zipcode level Zillow Observed Rent Index (ZORI), which is normalized by the MSA-level ZORI. The ZORI captures changes in asking rents over time while controlling for shifts in the quality of the available rental stock. Table 5 further regresses the listed indicator

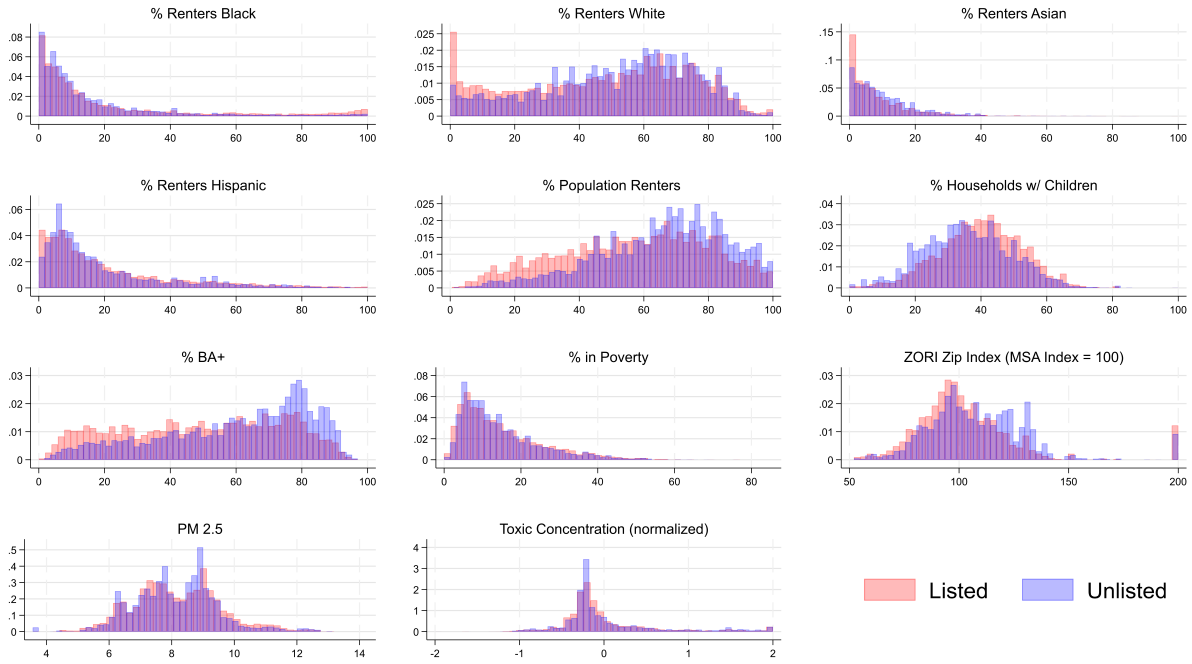


Figure 5: Distribution of Different Covariates Based on the Listed Status of Renters in the Sample

on the tract-level socio-demographic variables. Taken together, the evidence suggests that unlisted units are more prevalent in neighborhoods with higher ZORI values, larger shares of renters, and higher shares of residents with at least a four-year college degree. They are also more common in areas with a greater proportion of White or Asian renters, and less common in neighborhoods with higher shares of Black and Hispanic renters or households with children.

4 Estimation and Baseline Results

In this section, we describe our baseline empirical specification—a multinomial logit model estimating the probability that an individual from each racial group (White, Black, Asian, Hispanic, Other) occupies a given rental unit, with a focus on the role of the unit's listing status. Subsection 4.1 details the baseline model with a rich set of controls and city fixed effects, while Subsection 4.2 extends the analysis to a nested fixed point estimation framework

Outcome	Listed
Toxic Concentration	0.0032***
	(0.0008)
Poverty Share	0.0014***
	(0.0001)
ZORI	-0.0008***
	(0.0000)
% White	-0.0007***
	(0.0002)
% Black	0.0007***
	(0.0002)
% Asian	-0.0016***
	(0.0002)
% Hispanic	0.0010***
	(0.0002)
% Renters	-0.0041***
	(0.0000)
% Family with Chilren	0.0020***
	(0.0000)
MSA FE	✓
Year FE	✓
N	434,319
R^2	0.122

Table 5: Correlation Between Listed and Tract-Level Characteristics

that accommodates high-dimensional fixed effects for census tracts, property managers, and buildings.

4.1 Multinomial Logit

Our analysis undertakes a series of multinomial logit estimation procedures to explain the probability that unit i is occupied by a tenant of race $j \in \{\text{White}, \text{Black}, \text{Hispanic}, \text{Asian}, \text{Other}\}$. We posit a random-utility model in which the "utility" (or suitability) of unit i for a renter of race j is

$$U_{ijmt} = X_{it}^{'}\beta_j + \alpha_j \text{listed}_{it} + \phi_{jm} + e_{jt} + \epsilon_{ijmt} \tag{1}$$

where

- X_{it} are observed building and tract characteristics (building rent, size, neighborhood rent, demographics, etc.)

- listed_{it} indicates whether unit i was publicly listed

- β_j and α_j are race-specific parameters

- ϕ_{jm} is the fixed effect for race j in city m

- e_{jt} is the fixed effect for race j in year t

- ϵ_{ijmt} is an i.i.d. Type-I extreme-value error

Under the multnomial logit assumption, the probability that unit i in city m in year t is occupied by race j is

$$P(Y_{ijmt}) = \frac{exp\{\alpha_j \text{listed}_{it} + X'_{it}\beta_j + \phi_{jm} + e_{jt}\}}{\sum_{j=1,\ldots,5} exp\{\alpha_k \text{listed}_{it} + X'_{it}\beta_k + \phi_{jm} + e_{jt}\}} \tag{2}$$

where $Y_{ijmt} \in \{\text{White}, \text{Black}, \text{Hispanic}, \text{Asian}, \text{Other}\}$. We estimate $\{\beta_j, \alpha_j, \phi_{jm}, e_{jt}\}$ by maximizing the sample log-likelihood:

$$\sum_{i,m,t} \ln P_{i,j=k}(X_{it}, \text{listed}_{it}; \beta_j, \alpha_j, \phi_{jm}, e_{jt})$$

where $j = k$ denotes the observed race of tenant i, with non-Hispanic white as the reference category. The key parameter, α_j, captures how listing a unit shifts the log-odds that it will be occupied by race j rather than by white renters. Exponentiating α_j gives the relative-risk ratio, and subtracting one converts it to the percentage change in that relative probability when a unit is advertised.

To interpret α_j causally, we condition on a comprehensive set of building- and neighborhood-level covariates, X_{it}, including unit counts, property quality adjusted rent indices, zipcode level rent indices, racial composition, education attainment and amenity measures. These controls absorb any systematic differences, real or perceived, that might both influence landlords' listing decisions and attract certain racial groups. For example, a landlord may only advertise high-end units, which may disproportionately appeal to high-income renters, who may be disproportionately white. Such correlation, if present, would bias our estimate of

steering away from minority households downward. We mitigate this concern by including property quality indices and building size. Alternatively, in predominantly minority neighborhoods where vacancies are harder to fill, landlords may list every vacancy to help fill units quickly, leading advertised units there to house more minority renters. We address that possibility by controlling for local market conditions via quality-adjusted ZORI (and time-to-lease indices in some specifications) and the fraction of renters by race in each neighborhood. We further include race-by-city fixed effects ϕ_{jm} to allow every city to have its own baseline propensity for each racial group. These absorb city-level factors such as regional price differences, local demand by race, and the broad regulatory environment; and race–by–year effects e_{jt} to control for any shocks that affect all units' appeal to a given racial group in a given year. The underlying identification assumption is that with these rich controls, the remaining variation in whether a unit is listed or not "as good as random" with respect to unobserved factors that could differentially affect its appeal to different races. To put it another way, we assume

$$\mathrm{Cov}\big(\mathrm{listed}_{it},\, \varepsilon_{ijmt} \;\big|\; X_{it},\, \phi_{jm},\, e_{jt}\big) \;=\; 0 \tag{3}$$

We restrict the estimation sample only to buildings that have any units listed on Dwellsy, as we know that the property managers of these buildings will have incurred the fixed costs required to list on the platform and that the practical costs of listing any marginal units will be minimal. This approach may be conservative in detecting discrimination, depending on the racial composition of tenants in apartments outside Dwellsy's market coverage. Landlords might leave entire buildings unlisted to attract white tenants through referrals, which our method would miss. Conversely, if unlisted buildings were more frequently rented to Black tenants, our approach would overlook favorable treatment of minorities.

Baseline Multinomial Logit Estimates Results are presented in Table 6, where the dependent variables are Black, Hispanic, Asian, and other respectively, with non-Hispanic

white as the reference category. The specification controls for a comprehensive set of tract-level covariates, including zipcode level Zillow Observed Rent Index (ZORI) normalized by MSA-level ZORI, the share of renters, the shares of renters and of renters by race (White, Black, Asian, Hispanic), the share of college graduates, local poverty rates, and air-quality indices (RSEI), alongside building-level characteristics such as quality-adjusted rent indices and unit counts, as well as city and year fixed effects. We find that Black renters are about 4.4 percent more likely than their white counterparts to reside in a unit that was publicly listed, while Hispanic renters are 2.4 percently more likely; that is, they are less likely than white renters (i.e., the excluded group) to appear in unlisted units.

Turning to the other coefficients, we find that minority tenants are more likely to live in neighborhoods with a higher share of their own race. Minority renters are less likely than White renters to occupy units in neighorhoods with higher ZORI, clearner air and water and better opportunity scores. Black and Hispanic renters are less likely to be in buildings with higher quality-adjusted apartment rents. These patterns align with standard residential sorting dynamics, suggesting that our controls capture the typical drivers of racial sorting that might otherwise confounded the listing estimates.

4.2 Nested Fixed Point Estimation with Manager-City Fixed Effects

A potential threat to our identification is the presence of unobserved factors that could simultaneously affect whether a property is listed on Dwellsy and the demographic composition of those searching in the neighborhood, particularly the share of Black searchers. We address this by including finer fixed effects at the manager, tract, and building levels, respectively. Since our baseline already includes detailed tract-level covariates (rent indices, racial composition, income, education, amenities) and building attributes (quality-adjusted rents, size), our preferred specification extends the baseline MNL estimation to incorpo-

Outcome:	Asian	Black	Hispanic	Other
1(Listed)	-0.119***	0.043***	0.024*	-0.076***
	(0.0160)	(0.0178)	(0.0146)	(0.0252)
Rent Index	0.236***	-0.596***	-0.731***	0.125***
	(0.0206)	(0.0228)	(0.0187)	(0.0323)
log(Num. Units in Building)	0.069***	-0.014***	0.035***	0.057***
	(0.0037)	(0.0041)	(0.0033)	(0.0058)
ZORI Zip/Metro Index	-0.001***	-0.013***	-0.003***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.015***	-0.023***	-0.008***	-0.008***
	(0.0017)	(0.0019)	(0.0016)	(0.0027)
% Renters Black	0.002	0.021***	0.003**	0.008***
	(0.0017)	(0.0019)	(0.0016)	(0.0027)
% Renters Asian	0.024***	-0.013***	0.005***	0.005**
	(0.0018)	(0.0020)	(0.0016)	(0.0028)
% Renters Hispanic	-0.003**	-0.018***	0.010***	-0.011***
	(0.0017)	(0.0019)	(0.0015)	(0.0027)
% Population Renters	-0.001**	-0.001***	-0.003***	-0.004***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.006***	0.008***	-0.002***	0.008***
	(0.0008)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.003***	-0.007***	-0.003***	0.001**
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
% BA+	0.009***	0.001**	-0.004***	-0.002***
	(0.0005)	(0.0006)	(0.0005)	(0.0008)
RSEI (normalized)	0.082***	0.079***	0.047***	-0.039***
	(0.0078)	(0.0086)	(0.0071)	(0.0122)
Opportunity Score, 25th ptile income	-0.044***	-0.841***	-0.013	0.028*
	(0.0111)	(0.0123)	(0.0101)	(0.0175)
Listed Odds Ratio	0.887	1.044	1.024	0.927
Log-likelihood: -418279				
Observations: 428,513				
City and Year FE included				

Table 6: Multinomial logit model with City and Year FE

Outcome:	Asian	Black	Hispanic	Other
1(Listed)	-0.219***	0.037**	-0.016	-0.138***
	(0.0146)	(0.0160)	(0.0136)	(0.0234)
Rent Index	0.167***	-0.101***	-0.090***	-0.043*
	(0.0196)	(0.0216)	(0.0184)	(0.0316)
log(Num. Units in Building)	0.062***	-0.028***	0.020***	0.045***
	(0.0048)	(0.0053)	(0.0045)	(0.0077)
ZORI Zip/Metro Index	-0.001***	-0.013***	-0.005***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.021***	-0.046***	-0.011***	-0.008***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Renters Black	-0.001***	0.010***	0.002***	0.005***
	(0.0005)	(0.0006)	(0.0005)	(0.0009)
% Renters Asian	0.015***	-0.041***	0.003***	0.003**
	(0.0008)	(0.0009)	(0.0007)	(0.0013)
% Renters Hispanic	-0.010***	-0.039***	0.004***	-0.014***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Population Renters	0.002***	0.002***	-0.004***	-0.005***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.005***	0.005***	-0.001**	0.009***
	(0.0007)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.004***	-0.010***	-0.001***	0.000
	(0.0005)	(0.0005)	(0.0004)	(0.0008)
% BA+	0.008***	-0.002***	-0.009***	-0.005***
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
RSEI (normalized)	0.066***	0.116***	0.049***	0.005
	(0.0071)	(0.0078)	(0.0066)	(0.0114)
Listed Odds Ratio	0.803	1.038	0.984	0.871
Log-likelihood: -318852				
Observations: 358,077				
Manager and Year FE included				

Table 7: Nested fixed point MNL estimation with Manager FE

Outcome:	Other	Black	Hispanic	Asian
1(Listed)	-0.178***	0.030**	-0.044***	-0.269***
	(0.0222)	(0.0151)	(0.0126)	(0.0140)
Rent Index	0.084***	-0.106***	-0.087***	0.149***
	(0.0282)	(0.0192)	(0.0160)	(0.0178)
log(Num. Units in Building)	0.027***	-0.024***	0.005***	0.027***
	(0.0022)	(0.0015)	(0.0013)	(0.0014)
Listed Odds Ratio	0.837	1.030	0.957	0.764
Log-likelihood: -346301				
Observations: 389,359				
Tract and Year FE included				

Table 8: Nested fixed point MNL estimation with Tract and Year FE

rate manager fixed effects.[9] One important unobservable is the manager's listing practice. For example, some managers may post some units on Dwellsy yet still rely on traditional channels like community bulletin boards or church bulletins for other units. If those offline listings are randomly assigned across units, they won't bias our estimates: the unlisted units wouldn't systematically differ by renter race. However, if managers systematically reserve certain units, perhaps those likely to attract white tenants, for offline channels (e.g., bulletins with predominantly white readership) while advertising others online, that practice would mimic discrimination by selective advertising. By controlling manager fixed effects, we compare listed and unlisted units under the same management, thereby absorbing variations in managers' background, experience, habits, local ties (e.g., corporate relocation programs) and platform preferences. Because the units they manage are in buildings that have already advertised on Dwellsy at least once during that year, we can rule out the mere lack of access to the platform. Any remaining correlation between listing status and tenant race should therefore reflect deliberate choices rather than platform availability or managers' personal constraints.

Our estimation sample consists of 9,899 managers. Estimating a multinomial logit with thousands of manager dummies by maximum likelihood would suffer from the incidental-

[9]Specifications with tract and building fixed effects are presented as robustness checks.

parameter problem and be computationally prohibitive. Instead, we implement the nested fixed point (NFXP) approach proposed by Berry et al. (1995). The framework treats the vector of mean utilities, including the coefficients on our key "publicly listed" indicator and all observed controls, and a full set of manager dummies, as an implicit object to be recovered from observed choice shares via a fixed-point mapping, rather than estimated directly. Specifically, we write the utility (or suitability) of unit i under manager $mgr(i)$ for a renter of race j as the following:

$$U_{ijmt} = X_{it}^{'}\beta_j + \alpha_j \text{listed}_{it} + \eta_{j,mgr(i)} + e_{jt} + \epsilon_{ijmt} \tag{4}$$

where $\eta_{j,mgr(i)}$ is the race-specific fixed effect for the manager of unit i. Because each manager in our sample operates in only one city, these manager fixed effects also subsume the race–by–city fixed effects, ϕ_{jm}, from our baseline specification. For any candidate parameters $(\beta_j, \alpha_j, \{\eta_j\}, e_j)$, we compute the implied choice probabilities via the multinomial logit and then iterate

$$\delta_{jmt}^{new} = \delta_{jmt} + \ln s_{jmt}^{obs} - \ln s_{jmt}(\beta_j, \alpha_j, \{\eta_j\}, e_j) \tag{5}$$

until the predicted shares s_{jmt} to match the data s_{jmt}^{obs}. By inverting market shares to recover mean utility, the NFXP algorithm permits the inclusion of thousands of manager fixed effects via the contraction mapping without direct, high-dimensional likelihood estimation and hence delivering a consistent estimate of α_j, the causal effect of public listing on the probability of a tenant of a given race occupying a unit.

Nested Fixed Point Estimates Table 7 reports NFXP estimates that include manager-city and year fixed effects. Although adding manager effects reduces our sample by nearly a quarter relative to Table 6, the coefficients on tract- and building-level controls remain virtually unchanged in both sign and magnitude, suggesting that the assignment of managers is not systematically tied to particular neighborhoods or property types.

Most importantly, even after accounting for manager heterogeneity, Black renters remain 3.8 percent more likely than non-Hispanic whites to occupy publicly listed units, slightly smaller than the effect estimated from our baseline specification. On the other hand, the difference between Hispanic and white renters disappears. The attenuation is expected, since manager fixed effects absorb identifying variations driven by individual listing practices. To the extent that some managers may withhold their entire property portfolio from online platforms and recruit preferred tenants via word-of-mouth or personal networks, these forms of selective advertising would not be captured by our manager fixed effects specification. Thus, the estimated 3.8 percent difference should be viewed as a conservative lower bound on the true extent of selective advertising against Black renters.

Table 8 reports NFXP estimates with tract- and year-fixed effects. This is meant to control for any neighborhood level factors that could influence both landlords' listing decisions and tenants' sorting. For example, high-amenity, high-income tracts often face unusually strong demand; landlords there may not need to advertise given the long applicant queues, and those areas may also disproportionately attract white renters. By including tract fixed effects, we net out these demand-related correlations. Table 8 shows that, controlling for tract-level unobservables and building attributes, Black renters remain 3 percent more likely than non-Hispanic whites to occupy publicly listed units, comparable to the manager FE estimates. Of course, this specification still misses cases where entire neighborhoods rely exclusively on offline channels – potentially another form of selective advertising driven precisely by neighborhood demographics – but such effects would fall outside the within-tract variation and so remain undetected. Hence, the estimated 3 percent difference continues to serve as a conservative lower bound on the true extent of selective advertising.

In sum, even in the conservative specifications with manager or tract fixed effects, the evidence of selective advertising remains statistically significant and economically meaningful, confirming that discrimination at the listing stage is real. To assess its quantitative importance, we next explore heterogeneity in selective advertising across neighborhood types

and tenant types. Given its comprehensive manager, building, and tract-level controls and recognizing that its conservative estimates serve as lower bounds, we retain Equation (4) with manager-city and year fixed effects as the primary specification going forward.

5 Heterogeneity Across Tenant Types

So far, we have observed that Black renters are more likely to occupy listed units com- pared to white renters. Conversely, this implies that white renters are more likely to reside in unlisted units, suggesting that the practice of withholding listings may contribute to racial segregation. However, this form of listing discrimination may not be uniformly applied across different tenant types or neighborhoods. In this section and the next, we first examine how racial discrimination in listing interacts with other forms of renter heterogeneity, such as family status and income, and then identify which neighborhoods are more likely to use unlisted units to target white households.

5.1 Heterogeneity Among Renters With and Without Children

Selective advertising targets not only minorities but also families with children. To explore this further, we extend the estimation in Table 7 by expanding the dependent variables to include ten categories, distinguishing between renters with and without children within each racial group. Figure 6 displays the coefficients on *listed* for each racial group, both with and without children, using white families without children as the reference category.

Compared with Table 7, Figure 6 shows that the average Black–white listing odds gap (3.8 percent) masks striking heterogeneity by family type. Black renters without children are 3.9 percent more likely than childless white renters to occupy listed units, essentially matching the pooled estimate. Yet Black households with children are 55 percent more likely than childless white households to occupy listed units, implying that steering hits Black families with children especially hard. Notably, selective (un)advertising based on

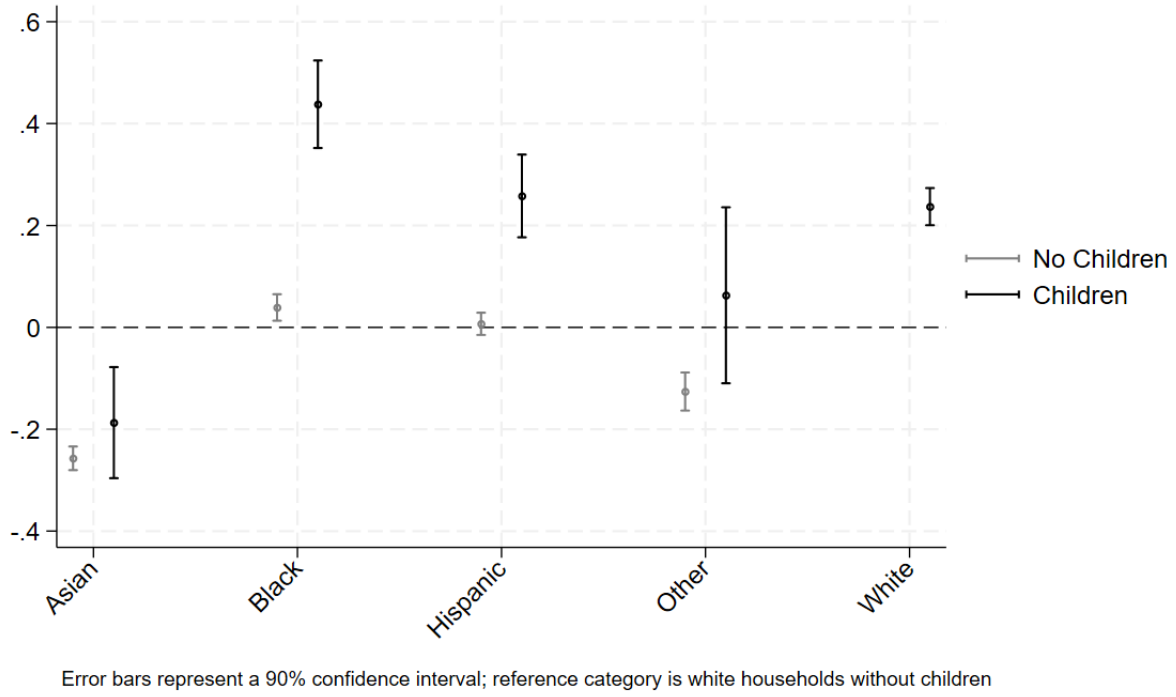


Figure 6: Listed Coefficient for Each Group (relative to white renters without children)

family status extends to other racial groups as well. Hispanic families with children are 29.4 percent more likely, and even white families with children are 26.7 percent more likely, to occupy listed units than white childless renters; by contrast, Asian and other-race renters seem to face no listing bias regardless of parental status. As Matthew Desmond notes in *Evicted*, "mothers often leave children with relatives while they chase down apartments" (Desmond, 2016). Our findings quantify the substantial barriers parents face in the rental market due to listing discrimination.

Importantly, racial disparities remain substantial even after accounting for family status: among families with children, Black households face twice the disadvantage of white households; and among family without children, unlisted units are still 4 percent more likely to be steered toward white tenants relative to Black tenants. Overall, Black families with children bear the greatest bias in listing, about half stemming from the added hurdle of having children, and the other half from racial bias.

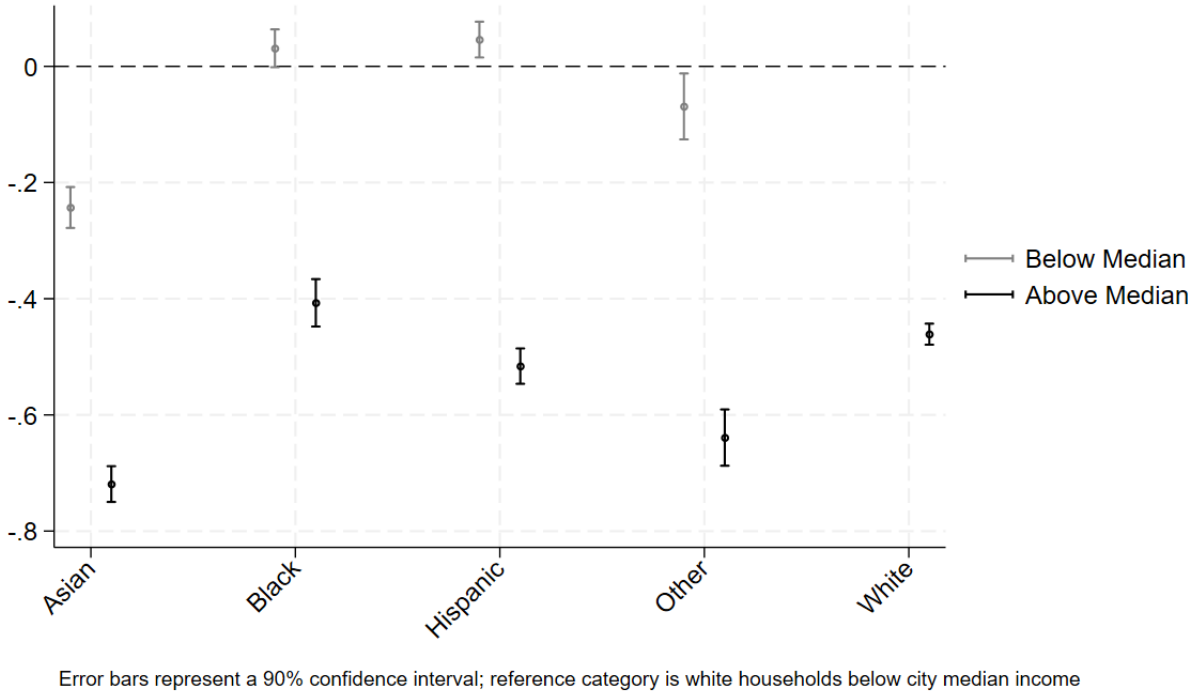


Figure 7: Listed Coefficient for Each Group (relative to white renters with below median income)

5.2 Income Heterogeneity in Selective Advertising

We further examine selective advertising heterogeneity with respect to tenant income. We extend the estimation in Table 7 by expanding the dependent variables to include ten categories, distinguishing between renters below and above the tract median income within each racial group.

Figure 7 displays the coefficients on *listed* for each racial group, both below and above median income, using white families below the median income as the reference category. Three findings emerge. First, income discrimination is substantial. Across all races, else equal, unlisted units are more likely to be occupied by renters with above-median income, compared with those with below-median income, even among white renters.

In addition, selective advertising against minority renters is concentrated among lower-income renters. Renters with above-median income, regardless of race, do not face selective advertising. Among lower-income renters, Black renters are 3.2 percent more likely and

Outcome:	Asian	Black	Hispanic	Other
1(Listed)*1(ZORI > 100)	0.088***	0.132***	-0.064***	-0.027
	(0.0266)	(0.0291)	(0.0248)	(0.0427)
1(Listed)	-0.221***	-0.029*	0.023	-0.108***
	(0.0195)	(0.0215)	(0.0183)	(0.0314)
Rent Index	0.170***	-0.102***	-0.090***	-0.042*
	(0.0196)	(0.0216)	(0.0184)	(0.0316)
log(Num. Units in Building)	0.062***	-0.028***	0.019***	0.045***
	(0.0048)	(0.0053)	(0.0045)	(0.0077)
ZORI Zip/Metro Index	-0.001***	-0.014***	-0.005***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.021***	-0.045***	-0.011***	-0.008***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Renters Black	-0.001***	0.011***	0.002***	0.005***
	(0.0005)	(0.0006)	(0.0005)	(0.0009)
% Renters Asian	0.015***	-0.040***	0.003***	0.003**
	(0.0008)	(0.0009)	(0.0007)	(0.0013)
% Renters Hispanic	-0.010***	-0.038***	0.004***	-0.014***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Population Renters	0.002***	0.002***	-0.004***	-0.005***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.004***	0.005***	-0.001**	0.009***
	(0.0007)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.004***	-0.010***	-0.001***	0.000
	(0.0005)	(0.0005)	(0.0004)	(0.0008)
% BA+	0.008***	-0.002***	-0.009***	-0.005***
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
RSEI (normalized)	0.067***	0.117***	0.049***	0.005
	(0.0071)	(0.0078)	(0.0066)	(0.0114)
Above-median Listed Odds Ratio	0.875	1.108	0.959	0.874
Below-median Listed Odds Ratio	0.801	0.971	1.023	0.898
Log-likelihood: -318720				
Observations: 358,077				
Manager and Year FE included				

Table 9: Heterogeneity in ZORI: NFXP estimation with Manager FE

Hispanic renters are 4.7 percent more likely than white renters to occupy listed units. This confirms that selective advertising disproportionately impacts lower-income minority renters.

Moreover, while the average Black–white listed odds ratio is 3.8 percent (Table 7), it remains largely unchanged at 3.2 percent when we restrict the sample to lower-income renters. This suggests that income discrimination accounts for only a small portion of the discrimination against Black renters observed in the pooled sample. On the other hand, the small and insignificant average listed Hispanic-white odds ratio understates the extent of listing discrimination faced by a subgroup of Hispanic renters: lower-income Hispanic renters are 4.7 percent more likely to occupy listed units than their lower-income white counterparts.

6 Heterogeneity Across Neighorhoods

In this section, we identify which neighborhoods are more likely to use unlisted units to target white households.

6.1 Heterogeneity in Neighborhoods with Varying ZORI

To assess differentiation in amenity levels, we introduce an interaction between the "listed" dummy variable and a dummy variable indicating whether the zip code-level ZORI is above its corresponding MSA-level ZORI. In a competitive market, neighborhood amenities are capitalized into local rents, so the ZORI dummy indicates neighborhoods with better-than-average local amenities. The results are presented in Table 9.

We find particularly strong steering in high-amenity neighborhoods. Even among buildings with the same Dwellsy platform access, under the same property management, in the same metro market, listed units in tracts with above-median quality-adjusted rents are 10.8 percent more likely to be occupied by Black tenants versus white tenants, while in lower-rent tracts the gap is statistically indistinguishable from zero. Thus, an average estimate of the Black–white difference - for example, the 3.8 percent gap reported earlier when pooling all

neighborhoods together - likely understates the true extent of steering. Moreover, the absence of Black-white gap in low-amenity neighborhoods confirms that steering away from Black renters is not a uniform practice but rather targeted at the most desirable neighborhoods.

One potential concern is that high ZORI neighborhoods tend to be in hot markets where landlords can simply fill vacancies via existing application queues and that those neighborhoods also tend to attract more white renters. Note that Table 9 has already included the ZORI index, racial share, income, education, and amenity measures as controls. We also include average time-to-lease in a robustness check. The pure "hot-market" demand effect is thus captured by those covariates, and our estimate of α_{black} is identified off the residual variation in listing status after conditioning on those market conditions. Moreover, if landlords simply relied on existing application queues, unlisted units would match Black and white applicants in proportion to their tract-level shares, exactly what our controls enforce. Together, these controls ensure that the estimated 10.8 percentage effect in better-amenity neighborhoods isn't just a byproduct of a hot market or white-renter demand, but rather reflects deliberate differential access to unadvertised units.

To control for changes in market conditions even further, Table 10 repeats the estimation in Table 9 but using building fixed effects. This presents the most demanding specification, since it compares listed and unlisted units within the same building. Consistent with our main findings, in high-amenity neighborhoods, unlisted units are 7.5 percent more likely to house white tenants, whereas this disparity vanishes in lower-amenity neighborhoods.

6.2 Heterogeneity in Neighborhoods with Varying Amenities

While ZORI captures overall neighborhood desirability, we next unpack two specific consumption amenities attached to rental units: environmental quality and economic opportunity: environmental quality and economic opportunity.

Outcome:	Asian	Black	Hispanic	Other
1(Listed)*1(ZORI > 100)	0.001	0.109***	0.020	0.049
	(0.0248)	(0.0269)	(0.0229)	(0.0397)
1(Listed)	-0.235***	-0.037**	-0.098***	-0.194***
	(0.0173)	(0.0188)	(0.0161)	(0.0278)
Above-median Listed Odds Ratio	0.791	1.075	0.925	0.865
Below-median Listed Odds Ratio	0.791	0.963	0.906	0.823
Log-likelihood: -219569				
Observations: 340,042				
Building and Year FE included				

Table 10: Heterogeneity in ZORI: NFXP estimation with Building and Year FE (2021-2023)

Environmental Quality We begin with neighborhood's air and water quality, using the EPA's Risk-Screening Environmental Indicators (RSEI) which estimate the concentration of airborne and waterborne toxins at the census-block level and translate them into potential human exposure. Table 11 repeats estimation from Table 9 but replaces ZORI with normalized RSEI. Here, "high" RSEI tracts are those whose RSEI scores exceed the MSA-wide average in a given year. Mirroring our ZORI findings, we find that selective unadvertising arises only where landlords choose to segment. Holding all else equal, listed units in below-average-RSEI neighborhoods are 10.1 percent more likely to house Black renters and 3.9 percent more likely to house Hispanic renters; these gaps disappear in above-average-RSEI tracts. In other words, disparities in the access to available rental units appear relevant only in cleaner and healthier neighborhoods. Notably, steering through selective advertising in better neighborhoods, measured by environmental quality, disadvantages not only Black renters but also Hispanic renters.

Economic Opportunity Next, we rank neighborhoods by the opportunity score, measured by the mean income in 2014 of children raised in each census tract by parents in the bottom income quartile (Chetty et al., 2018). This score conveniently summarizes local school quality, community networks, career opportunities, as well as other factors that shape children's future income prospects. Table 12 repeats the above estimation but uses normal-

Outcome:	Asian	Black	Hispanic	Other
1(Listed) x 1(High Normalized RSEI)	0.022	-0.183***	-0.101***	-0.173***
	(0.0260)	(0.0286)	(0.0243)	(0.0418)
1(Listed)	-0.182***	0.097***	0.038**	-0.051**
	(0.0182)	(0.0200)	(0.0171)	(0.0293)
Rent Index	0.170***	-0.101***	-0.088***	-0.041*
	(0.0197)	(0.0216)	(0.0184)	(0.0316)
log(Num. Units in Building)	0.062***	-0.029***	0.019***	0.045***
	(0.0048)	(0.0053)	(0.0045)	(0.0077)
ZORI Zip/Metro Index	-0.001***	-0.013***	-0.005***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.021***	-0.045***	-0.011***	-0.008***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Renters Black	-0.001**	0.011***	0.002***	0.005***
	(0.0005)	(0.0006)	(0.0005)	(0.0009)
% Renters Asian	0.015***	-0.040***	0.003***	0.003**
	(0.0008)	(0.0009)	(0.0007)	(0.0013)
% Renters Hispanic	-0.010***	-0.038***	0.004***	-0.014***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Population Renters	0.002***	0.002***	-0.004***	-0.005***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.004***	0.005***	-0.001**	0.009***
	(0.0007)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.004***	-0.010***	-0.001***	0.000
	(0.0005)	(0.0005)	(0.0004)	(0.0008)
% BA+	0.008***	-0.002***	-0.009***	-0.005***
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
RSEI (normalized)	0.067***	0.110***	0.047***	0.001
	(0.0071)	(0.0078)	(0.0067)	(0.0115)
High Listed Odds Ratio	0.852	0.918	0.939	0.799
Low Listed Odds Ratio	0.834	1.101	1.039	0.950
Log-likelihood: -318712				
Observations: 358,077				
Manager and Year FE included				

Table 11: Heterogeneity in RSEI: NFXP Estimation with Manager FE

Outcome:	Asian	Black	Hispanic	Other
1(Listed) x 1(High Opportunity, 25th ptile inc)	0.073***	0.137***	0.013	0.092**
	(0.0287)	(0.0315)	(0.0269)	(0.0462)
1(Listed)	-0.197***	0.010	-0.007	-0.153***
	(0.0174)	(0.0191)	(0.0163)	(0.0279)
Rent Index	0.167***	-0.101***	-0.087***	-0.042*
	(0.0197)	(0.0216)	(0.0184)	(0.0316)
log(Num. Units in Building)	0.062***	-0.031***	0.019***	0.045***
	(0.0048)	(0.0053)	(0.0045)	(0.0077)
ZORI Zip/Metro Index	-0.001***	-0.016***	-0.005***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.021***	-0.043***	-0.011***	-0.008***
	(0.0007)	(0.0008)	(0.0006)	(0.0011)
% Renters Black	-0.002***	0.005***	0.002***	0.005***
	(0.0006)	(0.0006)	(0.0005)	(0.0009)
% Renters Asian	0.015***	-0.029***	0.003***	0.003**
	(0.0009)	(0.0010)	(0.0009)	(0.0015)
% Renters Hispanic	-0.010***	-0.037***	0.004***	-0.014***
	(0.0007)	(0.0007)	(0.0006)	(0.0011)
% Population Renters	0.002***	-0.003***	-0.004***	-0.005***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.004***	0.002***	-0.001*	0.009***
	(0.0007)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.003***	-0.007***	-0.001***	0.000
	(0.0005)	(0.0005)	(0.0004)	(0.0008)
% BA+	0.008***	-0.003***	-0.009***	-0.005***
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
RSEI (normalized)	0.068***	0.145***	0.052***	0.006
	(0.0071)	(0.0078)	(0.0066)	(0.0114)
Opportunity Score, 25th ptile income	-0.042***	-0.838***	0.037***	0.004
	(0.0105)	(0.0115)	(0.0098)	(0.0168)
High Listed Odds Ratio	0.884	1.159	1.006	0.940
Low Listed Odds Ratio	0.821	1.010	0.993	0.858
Log-likelihood: -318164				
Observations: 358,077				
Manager and Year FE included				

Table 12: Heterogeneity in Opportunity Score: NFXP Estimation with Manager FE

ized opportunity score. Here, "high" opportunity score tracts are those whose opportunity score exceed the median across tracts within the same MSA. All else equal, in above-median-opportunity-score neighborhoods listed units are 15.9 percent more likely to house Black renters and 0.6 percent more likely to house Hispanic renters; in below-median-opportunity-score neighborhoods, the Black-white gap reduces to 1 percent, while the Hispanic-white gap disappears. These patterns results remain robust when we use the opportunity score measured for parents at either the median income or the 75th percentile.

Summary Unlike ZORI, environmental quality and economic opportunity are more than neighborhood luxuries; they are fundamental determinants of health, education, and long-term prosperity. RSEI captures exposure to airborne and waterborne toxins, directly shaping residents' well-being and life expectancy, while the opportunity score reflects the income trajectories that local schools, networks, and services provide for the next generation. Our findings reveal that rental units endowed with these vital consumption amenities are systematically steered away from Black, and to a less extent, Hispanic renters. When selective (un)advertising targets the cleaner, safer and opportunity-richer neighborhoods, minorities lose not only a shelter but also access to better health, stronger schools, and great economic mobility.

While disparities in neighborhood amenities have long been linked to income differences or sorting based on preferences for local public goods, our findings highlight a more direct and troubling mechanism: landlord steering. Recent literature (Christensen et al., 2022) documents how landlords disproportionately ignore rental inquiries from Black and Hispanic-identified callers in low-air-toxics areas yet readily respond to white callers. We add to this evidence by showing that discrimination begins even before inquiries are made: landlords' decisions about which units to advertise, or to leave off the market, create unequal access to homes with better amenities at the very start of the rental search process.

It is important to note that selective advertising does not fully block Black renters from

accessing desirable neighborhoods; rather, it suggests that Black renters are steered into a narrower slice of the housing market within those cleaner, higher-opporunity and nicer neighborhoods – specifically, the units landlords choose to advertise publicly, as shown in Tables 9–12.

If landlords tend to hold back higher-quality or premium units and fill them through private channels such as word-of-mouth or referrals, Black renters, who rely more heavily on public listings, may still reach better neighborhoods but may end up in more visible, but perhaps lower-tier units within them. In this sense, selective advertising may reinforce residential inequality in a subtle way: it shapes not only *where* Black renters can live but also *what* they can access once they get there.

Recall that Table 5 indicates that unlisted units tend to cluster in neighborhoods with stronger amenities overall. This pattern implies that the choice to withhold listings is more common where market demand and neighborhood quality are high, effectively narrowing the range of options available through public channels. Thus, even when Black renters gain entry to high-amenity areas, they may face hidden barriers to the best available units. In this sense, selective advertise narrows both the quantity and quality of housing choices precisely where choice matters most for access to good schools, environments, and upward mobility.

6.3 Heterogeneity with Other Forms of Discrimination

We next explore how selective advertising interacts with more overt forms of landlord discrimination—specifically, bias in email responses to rental inquiries. To measure this, we merge our data with the city-level average within-property relative response rate difference to email inquiries from a white identity compared to a Black or Hispanic identity for a given listing, as estimated by Christensen et al. (2021) in their correspondence study on a major online rental platform. In each city, a larger response-rate gap signals greater discrimination, with landlords being less responsive to Black and Hispanic home seekers compared to white seekers.

We augment our multinomial logit model by interacting the *listed* indicator with the correspondence-rate gap: the white-Black gap for the Black tenancy equation, the white-Hispanic gap for the Hispanic equation, and zero for all other races. Table 13 reports the estimates. As before, we find that Black and Hispanic renters remain more likely to occupy listed units. And importantly, this correlation grows in cities where email-response discrimination is stronger. In other words, listing-stage and inquiry-stage discrimination act as complements: jurisdictions with greater callback discrimination also exhibit more steering at the listing stage.

This complementarity has important implications for fair housing enforcement. On one hand, while audits and correspondence studies have rightly targeted the callback and showing bias, our findings show that discrimination at the listing stage demands equal attention. By the time renters reach out to landlords, many units may have already been withheld, so traditional audits systematically underestimate the full scope of discrimination in the rental market. Indeed, unadvertised units are unlikely to be detected in audit or correspondence studies, allowing landlords to minimize contact with unwanted populations by simply not publicizing vacancies. Effective enforcement must therefore extend upstream to monitor not only who gets shown units, but which units are advertised in the first place.

On the other hand, efforts to raise community awareness and curb blatant discrimination may also help dampen discrimination at the listing stage. Table C2 shows that selective unadvertising, though still present, is weaker in tracts with higher college completion rates, suggesting that an informed and engaged community might help deter rental discrimination, even at the listing stage.

7 External Validity

This section addresses a few concerns about the external validity of our results. The first concern about external validity is that within Dwellsy-covered buildings, landlords may use

Outcome:	Asian	Black	Hispanic	Other
1(Listed) x Correspdence Ratio Difference	0.000	107.889***	84.694***	0.000
	(0.0000)	(0.4428)	(0.3774)	(0.0000)
1(Listed)	-0.159***	-1.340***	-0.437***	-0.110***
	(0.0150)	(0.0174)	(0.0149)	(0.0241)
Rent Index	0.163***	-0.113***	-0.099***	-0.045*
	(0.0197)	(0.0220)	(0.0189)	(0.0316)
log(Num. Units in Building)	0.062***	-0.037***	0.022***	0.046***
	(0.0048)	(0.0054)	(0.0046)	(0.0078)
ZORI Zip/Metro Index	-0.001***	-0.016***	-0.005***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.021***	-0.049***	-0.014***	-0.008***
	(0.0007)	(0.0008)	(0.0007)	(0.0011)
% Renters Black	-0.002***	-0.001**	-0.001*	0.005***
	(0.0006)	(0.0007)	(0.0006)	(0.0009)
% Renters Asian	0.015***	-0.034***	0.000	0.002**
	(0.0009)	(0.0010)	(0.0009)	(0.0015)
% Renters Hispanic	-0.010***	-0.042***	0.002***	-0.014***
	(0.0007)	(0.0007)	(0.0006)	(0.0011)
% Population Renters	0.002***	-0.003***	-0.004***	-0.005***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.004***	0.000	-0.001*	0.009***
	(0.0007)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.003***	-0.007***	-0.002***	0.000
	(0.0005)	(0.0005)	(0.0005)	(0.0008)
% BA+	0.008***	-0.004***	-0.009***	-0.005***
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
RSEI (normalized)	0.069***	0.169***	0.069***	0.006
	(0.0071)	(0.0079)	(0.0068)	(0.0114)
Opportunity Score, 25th ptile income	-0.037***	-0.836***	0.030***	0.010
	(0.0102)	(0.0114)	(0.0097)	(0.0163)
Listed Odds Ratio at 25th Percentile CR Diff	0.85	1.47	0.33	0.90
Listed Odds Ratio at Mean CR Diff	0.85	173.40	3.75	0.90
Listed Odds Ratio at 75th Percentile CR Diff	0.85	3670.08	23.64	0.90
Log-likelihood: -300845				
Observations: 353,678				
Manager and Year FE included				

Table 13: Heterogeneity in Callback Response Differential: NFXP Estimation with Manager FE

Notes: The correspondence differences represent the difference in the share of rental applications responded to according to the apparent race of the applicant from Christensen and Timmins (2023).

multiple channels to advertise available units. Therefore, a property not listed on Dwellsy might still be advertised elsewhere. To address this, we compared Dwellsy listings with rental listings scraped from major online platforms in selected cities. Table 3 shows that in Dwellsy-covered buildings, the number of units listed on Dwellsy closely matches the total number of units scraped from other sites. The difference is zero at the 25th, 50th, and 75th percentiles across all cities where data was scraped. On average, Dwellsy lists one fewer unit, likely due to its focus on professionally managed buildings and diligent efforts to verify listings, avoid duplicates, and remove off-market units. Overall, this comparison gives us high confidence about Dwellsy's online coverage of listed units in these buildings. In other words, very few units listed on other platforms are missing from Dwellsy. This aligns with the fact that Dwellsy does not charge per listing fees, allowing managers to advertise at no additional cost once the initial software installation fee is paid.

Another concern about external validity is how our findings from Dwellsy-covered buildings translate to the broader housing market. As discussed in Section 3.1, Dwellsy focuses on professionally managed rental buildings. In Seattle, Dwellsy-covered active listings cover over one-third of the rental stock registered with the city. In cities like Chicago, DC, Minneapolis, Philadelphia and Pittsburgh, on average, Dwellsy listings cover nearly a quarter of the vacant rental stock recorded in CoStar. Both Seattle rental registry and CoStar database report rental stock, listed or not, regardless whether they are professionally managed.

The extent to which our findings from the Dwellsy sample apply to the broader housing market depends on how similar the landlords, housing units, and neighborhoods in the Dwellsy sample are to the rest of the rental market. Table 4 shows that, on average, in neighborhoods covered by Dwellsy, 14.5% of the population live below the poverty line, 59% have college degrees or higher, and 64% are renters. These renters are 51.5% white, 16.4% Black, and 19.3% Hispanic. While we can compare the characteristics of neighborhoods and housing units between Dwellsy and the broader rental market, it is important to recognize the differences between professionally managed and non-professionally managed rental markets.

We view our findings from Dwellsy as being most applicable to the professionally managed rental market, which constitutes a significant portion of the rental stock in large cities.

Finally, some renters may choose not to use online platforms when searching for rental housing, and thus may appear less affected by discrimination through selective advertising. However, needing to adapt search strategies to avoid discriminatory practices still imposes real costs, whether in time, effort, or restricted choice set, on renters. Moreover, when landlords strategically withhold certain units from online listings, they are likely to exercise similar discretion with offline channels as well, such as word-of-mouth referrals. In this way, the core barrier remains: information about available housing is selectively restricted, regardless of how prospective tenants conduct their search.

8 Conclusion

Audit and correspondence studies have played a key role in a growing body of research highlighting the ongoing issue of racial discrimination in rental housing markets. However, these methods may miss more subtle forms of discrimination. In our analysis, we uncover evidence that selective advertising can be a source of discrimination, where landlords choose not to advertise certain units, instead reserving them for prospective renters they have met in person after an initial screening or through other offline channels.

Detecting this practice is challenging, complicating fair housing enforcement efforts. We present a novel method for identifying selective advertising and examine its impact on minorities' access to neighborhoods with better amenities. Our approach leverages a large marketing dataset to track apartment turnovers across 27 major U.S. metropolitan areas. We match this data with a rental listings dataset to spot turnover units that were not publicly advertised. Our empirical design takes a conservative approach, using only units in buildings where an online listing platform with low marginal costs for additional listings was known to be present. Future research could extend this work to include other listing services, allowing

the analysis to incorporate a broader set of buildings, particularly smaller, non-professionally managed buildings.

By comparing the racial composition of actual occupants in listed versus "hidden" units, we evaluate the extent of discrimination through selective advertising. Our findings reveal discrimination against Black, and sometimes Hispanic renters, through selective advertising that is both significant in magnitude and especially pronounced in neighborhoods with better amenities and opportunities and where other forms of discrimination tend to be more severe. These disparities are concentrated among lower-income renters with children. This suggests that selective advertising not only reinforces existing inequalities but may further limit access to desirable neighborhoods for the very group that could benefit most.

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City	Asian	Black	Hispanic	Other	White	**Total**
Albuquerque	31.47	35.09	30.69	32.10	29.91	**30.32**
Atlanta	9.83	21.39	15.22	10.49	11.74	**15.69**
Baltimore	14.94	37.20	27.54	20.98	31.21	**32.51**
Boston	8.12	20.26	12.30	9.53	12.18	**11.70**
Chicago	7.44	24.19	14.57	11.97	12.99	**13.45**
Cleveland	23.44	37.88	37.80	21.62	30.14	**33.13**
Columbia	16.67	36.45	27.66	33.33	27.11	**28.43**
Dallas	7.16	20.66	16.21	8.85	11.79	**12.77**
DC	12.43	20.80	14.06	12.64	15.32	**16.53**
Detroit	20.26	38.11	25.15	20.12	26.23	**35.40**
Fort Worth	45.61	34.26	43.86	43.02	39.38	**39.94**
Kansas City	27.07	45.90	39.04	35.43	35.58	**37.82**
Los Angeles	20.06	32.16	27.21	22.87	26.33	**25.56**
Minneapolis	15.58	29.14	26.16	21.05	24.00	**23.90**
Newark	21.82	31.19	31.11	21.82	32.30	**30.59**
Philadelphia	16.58	33.03	27.59	24.52	21.00	**24.68**
Pittsburgh	24.17	36.43	27.72	37.11	34.91	**34.07**
Providence	9.92	2.56	26.62	18.64	20.84	**20.42**
Richmond	31.91	27.88	31.25	41.94	31.47	**30.93**
Riverside	17.78	29.79	36.11	18.75	34.64	**33.58**
San Antonio	15.83	19.76	22.52	18.97	21.11	**21.36**
San Diego	21.99	24.14	28.19	24.95	26.17	**26.06**
San Jose	29.18	23.53	39.57	34.38	38.10	**35.51**
Seattle	19.00	25.19	27.66	22.21	27.77	**26.22**
Tampa	13.29	29.03	25.00	22.14	16.88	**19.02**
Average	19.47	28.54	27.61	23.32	25.44	**25.68**
Median	17.78	29.03	27.21	21.62	26.17	**25.56**

Table A1: Share of Renters in Listed Units by Racial Group and City

APPENDIX

A Listed Share, by Race and by City

Table A1 reports the average share of renters living in listed units, broken down by race and by city.

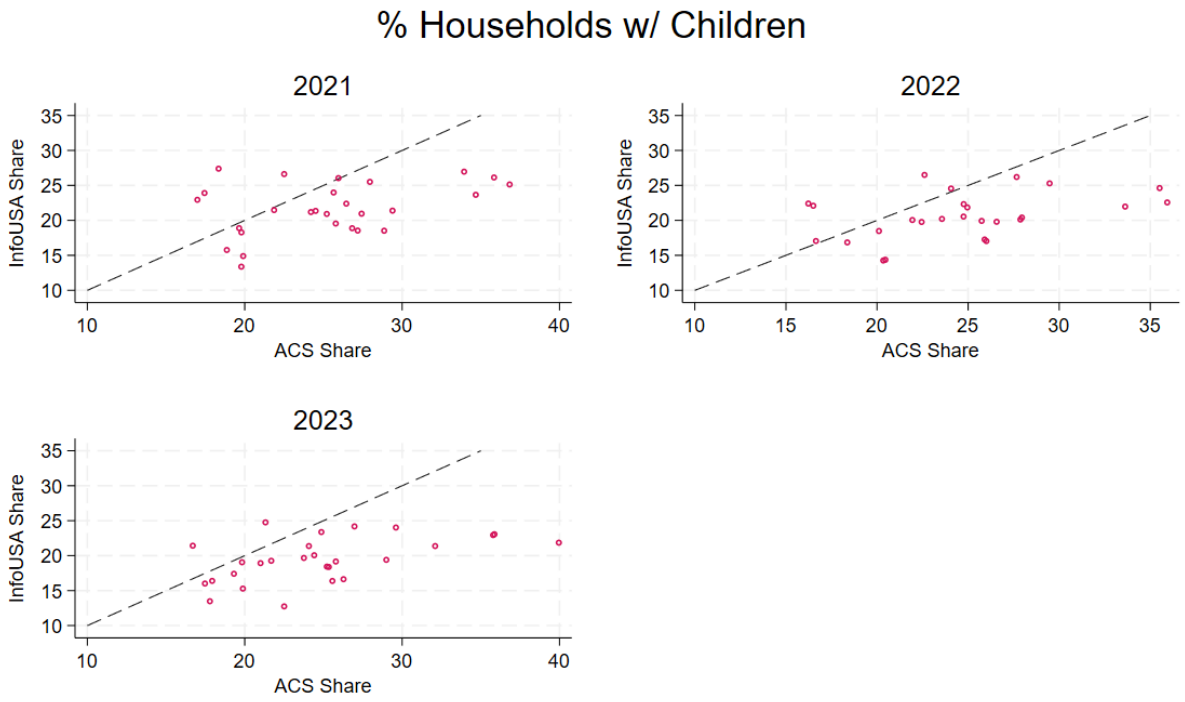


Figure B1: Comparison of the share of households in each city and year in the sample that have children

B ACS-InfoUSA City-level Income and Family Distribution Comparisons

Figure B1 displays the relationship, in each sample year, between the ACS and InfoUSA estimates for the share of households in each sample city that have children. Both ACS and InfoUSA estimates generally fall between 10% and 40% of households, but the relationship between the two estimates are noisy. While the InfoUSA estimate generally increases alongside the InfoUSA estimate, the relationship is weak, and the InfoUSA estimate understates the share of households with children in cities where the ACS estimates that 25% or more of households have children. Thus, it is possible that many individuals in our sample have children but are coded as not having children.

Figure B2-B20 display the histogram comparison, in each sample year, between the ACS and InfoUSA estimates for the household income distribution for each city.



(a) Albuquerque



(b) Atlanta

Figure B2: Comparison of income distribution estimates: Albuquerque and Atlanta



(a) Baltimore



(b) Boston

Figure B3: Comparison of income distribution estimates: Baltimore and Boston



(a) Chicago

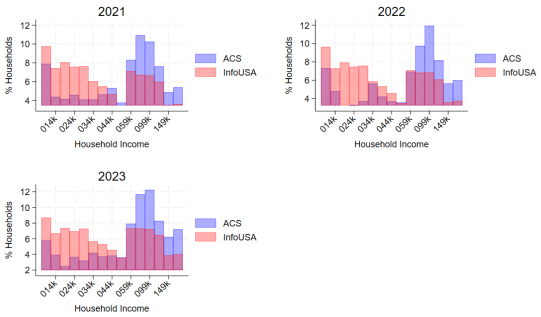


(b) Cleveland

Figure B4: Comparison of income distribution estimates: Chicago and Cleveland



(a) Columbia



(b) Dallas

Figure B5: Comparison of income distribution estimates: Columbia and Dallas



(a) Washington, D.C.



(b) Detroit

Figure B6: Comparison of income distribution estimates: Washington, D.C. and Detroit



(a) Fort Worth



(b) Houston

Figure B7: Comparison of income distribution estimates: Fort Worth and Houston



(a) Kansas City



(b) Los Angeles

Figure B8: Comparison of income distribution estimates: Kansas City and Los Angeles



Figure B9: Comparison of the income distribution estimates for Miami



Figure B10: Comparison of the income distribution estimates for Minneapolis

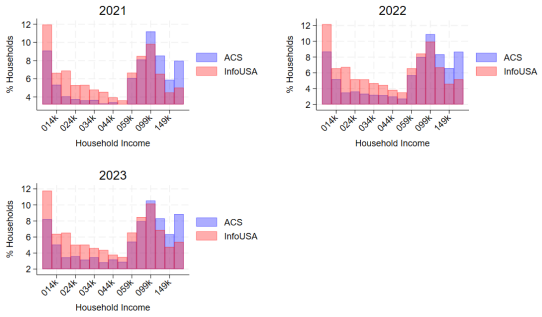


Figure B11: Comparison of the income distribution estimates for New York



Figure B12: Comparison of the income distribution estimates for Philadelphia

C Heterogeneity in College Graduates

Outcome:	Asian	Black	Hispanic	Other
1(Listed) x 1(High BA+ Share)	0.060**	-0.039	-0.166***	0.004
	(0.0311)	(0.0341)	(0.0290)	(0.0499)
1(Listed)	-0.224***	0.061**	0.117***	-0.122***
	(0.0280)	(0.0307)	(0.0262)	(0.0450)
Rent Index	0.166***	-0.101***	-0.089***	-0.042*
	(0.0197)	(0.0216)	(0.0184)	(0.0316)
log(Num. Units in Building)	0.062***	-0.031***	0.019***	0.045***
	(0.0048)	(0.0053)	(0.0045)	(0.0077)
ZORI Zip/Metro Index	-0.001***	-0.016***	-0.005***	-0.001**
	(0.0003)	(0.0003)	(0.0003)	(0.0005)
% Renters White	-0.021***	-0.043***	-0.011***	-0.008***
	(0.0007)	(0.0008)	(0.0006)	(0.0011)
% Renters Black	-0.002***	0.005***	0.002***	0.005***
	(0.0006)	(0.0006)	(0.0006)	(0.0009)
% Renters Asian	0.015***	-0.029***	0.003***	0.003**
	(0.0009)	(0.0010)	(0.0009)	(0.0015)
% Renters Hispanic	-0.010***	-0.037***	0.004***	-0.014***
	(0.0007)	(0.0007)	(0.0006)	(0.0011)
% Population Renters	0.002***	-0.003***	-0.004***	-0.005***
	(0.0003)	(0.0004)	(0.0003)	(0.0005)
% in Poverty	0.004***	0.002***	-0.001	0.009***
	(0.0007)	(0.0008)	(0.0007)	(0.0012)
% Households w/ Children	-0.003***	-0.007***	-0.001***	0.000
	(0.0005)	(0.0005)	(0.0004)	(0.0008)
% BA+	0.008***	-0.003***	-0.009***	-0.005***
	(0.0005)	(0.0005)	(0.0004)	(0.0007)
RSEI (normalized)	0.067***	0.144***	0.051***	0.005
	(0.0071)	(0.0078)	(0.0066)	(0.0114)
Opportunity Score, 25th ptile income	-0.037***	-0.826***	0.038***	0.012
	(0.0102)	(0.0112)	(0.0095)	(0.0164)
High Listed Odds Ratio	0.850	1.022	0.952	0.888
Low Listed Odds Ratio	0.800	1.063	1.125	0.885
Log-likelihood: -318152				
Observations: 358,077				
Manager and Year FE included				

Table C2: Heterogeneity in College Graduates: NFXP Estimation with Manager FE

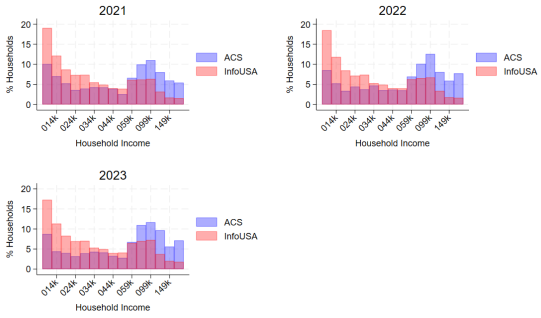


Figure B13: Comparison of the income distribution estimates for Pittsburgh

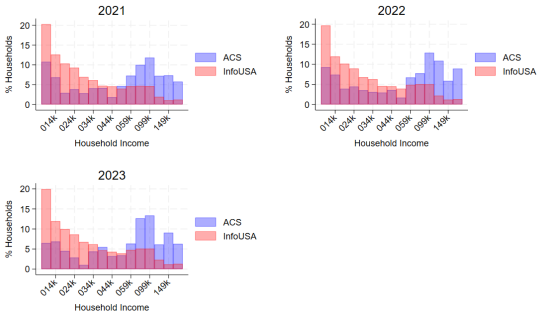


Figure B14: Comparison of the income distribution estimates for Providence



Figure B15: Comparison of the income distribution estimates for Riverside

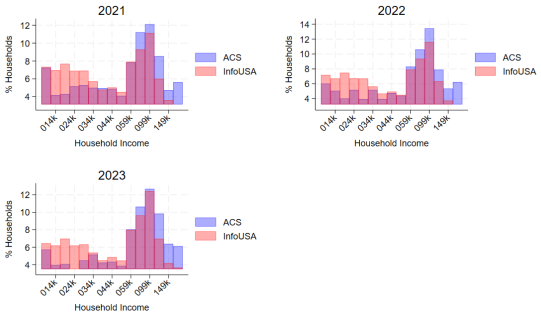


Figure B16: Comparison of the income distribution estimates for San Antonio



Figure B17: Comparison of the income distribution estimates for San Diego

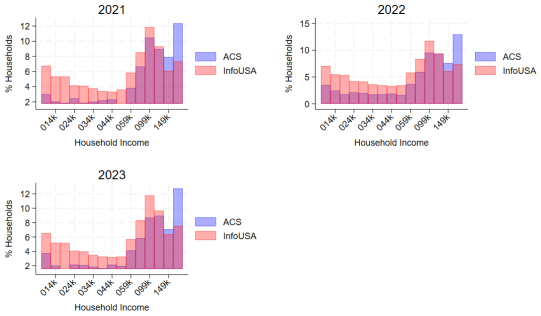


Figure B18: Comparison of the income distribution estimates for San Jose

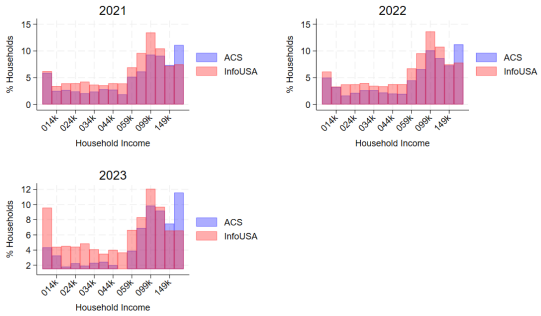


Figure B19: Comparison of the income distribution estimates for Seattle



Figure B20: Comparison of the income distribution estimates for Tampa