# Racial Discrimination in Eviction Filing

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#### Abstract

This paper develops and applies a novel framework to test whether landlords discriminate when filing eviction cases. Discrimination is measured by racial disparities in back rent owed at the time of filing, conditional on landlord and contract rent. Using rich administrative data from Philadelphia, I find that landlords discriminate by tolerating 4.9% less back rent from minorities before filing. To distinguish the sources of discrimination, I conduct a marginal outcome test and find evidence consistent with accurate statistical discrimination: landlords win cases at indistinguishable rates by tenant race, suggesting they accurately predict that successful minority cases require less back rent.

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# 1 Introduction

Each year, landlords file 3.6 million eviction cases in the U.S. (Gromis et al. 2022), placing families at risk of homelessness. Minority renters are disproportionately exposed to this risk as they are nearly three times as likely to face an eviction case as white renters (Graetz et al. 2023).<sup>1</sup> This disparity could be explained by racial differences in lease violations, racial differences in matching to strict landlords, or landlords discriminating by tolerating lease violations differently by race. This paper develops and applies new methods to measure such discrimination and understand its drivers.

When measuring racial discrimination, researchers must account for racial differences in the underlying "qualification" of individuals for a treatment. In most settings, "qualification" is at best partially unobserved, motivating strategies that leverage random assignment to decision-makers (Arnold et al. 2018; Dobbie et al. 2021; Arnold et al. 2022; Baron et al. 2023), structural models of decision-making (Knowles et al. 2001; Anwar and Fang 2006), control groups of non-discriminatory decision-makers (Goncalves and Mello 2021), or the ability to experimentally vary fictitious individuals' race (Bertrand and Mullainathan 2004; Ewens et al. 2014; Kline et al. 2022). The context of eviction, however, is uniquely well-suited to study discrimination given that a tenant's "qualification" for an eviction filing (caused only by non-payment) is fully summarized by the amount of back rent owed at the time of the filing. Using detailed eviction filing data from Philadelphia, I develop a new test for discrimination that leverages this comprehensive and observable measure of "qualification."

I consider a simple framework in which a landlord discriminates by tolerating nonpayment of rent differently by tenant race before filing an eviction case over non-payment. This notion of discrimination is rooted in the law: specifically, the Fair Housing Act which prohibits landlords from selectively evicting minority tenants even if the eviction would otherwise be lawful—for example, due to non-payment of rent (HUD 2021a). Formally, this concept of racial discrimination equates to a landlord using different filing thresholds of back rent by tenant race and aligns with the notion of "total discrimination" in Bohren et al. (2022) as it conditions on a natural definition of eviction "qualification." Total discrimination in eviction filing may be driven by indirect discrimination arising from differences in tenants' non-race characteristics (Bohren et al. 2022), accurate statistical discrimination arising from actual racial differences in the landlord's payoff from filing an eviction case (Phelps 1972; Arrow 1974; Aigner and Cain 1977), or landlords having racially biased beliefs or preferences

<sup>&</sup>lt;sup>1</sup>While an estimated 50.5% of individuals living in rental housing in the U.S. are white and 49.5% are racial or ethnic minorities, 26.3% of individuals living in households facing eviction filings are white and 73.7% are racial or ethnic minorities (Graetz et al. 2023). This equates to a minority eviction filing rate that is 2.9 times higher than the white eviction filing rate.

#### (Becker 1957; Bordalo et al. 2016; Bohren et al. 2019).

To derive my test for total racial discrimination, I leverage the assumption that tenants gradually fall behind on rent until the amount of back rent reaches their landlord's threshold for filing an eviction case. Total racial discrimination is identified by racial disparities in back rent owed at the time of an eviction filing, restricting to cases caused only by non-payment of rent. To allow a landlord's filing threshold of back rent to be measured in units of dollars owed or months of back rent owed, I condition these disparities on landlord and monthly contract rent.<sup>2</sup>

I apply this framework to the universe of residential eviction cases filed in Philadelphia between 2006 and 2019. Since tenant race is not reported in eviction cases, I impute tenant race using first names, last names, and census blocks. I find evidence of racial discrimination: on average, landlords tolerate 4.9% (\$121) less back rent from minority tenants than from white tenants before filing an eviction case. This result is robust to using numerous alternative imputation-based estimators of racial disparities. My estimates imply that at least 18% of minority eviction filings in Philadelphia (1,908 filings per year) occur systematically sooner than comparable white filings. Racial discrimination is not driven by a small number of landlords and instead appears highly prevalent, with at least 49% of landlords in my sample exhibiting racial discrimination.

Next, I extend the framework to distinguish between the sources of total discrimination: indirect discrimination, accurate statistical discrimination, or racial bias. To examine the role of indirect discrimination, I introduce controls for observable non-race characteristics of tenants. Importantly, I include multiple proxy measures of rent burden: individual contract rent as a share of median household income by tract among renters and as a share of median household income by tract and race. These controls are intended to account for racial and spatial disparities in rent affordability which could be an important channel of indirect discrimination if landlords incorporate a tenant's ability to afford rent going forward into eviction filing decisions. After controlling for these measures of rent burden, other nonrace tenant characteristics, and neighborhood factors, the discrimination estimate does not substantially change, implying limited scope for indirect discrimination.

I then consider whether the remaining discrimination reflects accurate statistical discrimination or racial bias. Landlords may exhibit accurate statistical discrimination if they observe that winning repossession of the property in court from minority tenants tends to require less back rent than cases against white tenants. This racial disparity in eviction court

 $<sup>^{2}</sup>$ I consider months of back rent owed as the ratio of back rent to monthly contract rent. Given that distressed renters often make partial rent payments (Manville et al. 2023), a tenant may, for example, accrue two months of back rent over a longer period of time than two months.

may reflect racial differences in whether tenants appear in court, unequal access to legal representation, racial disparities in bargaining power during landlord-tenant negotiations, discrimination by judges, or some other factor. Regardless of the source of the disparity, if landlords exhibit accurate statistical discrimination, then they should be equally likely to win repossession in marginal cases against white and minority tenants. Any deviation from equal repossession rates reflects racial bias in eviction filing.

I formalize this logic with a marginal outcome test for racial bias in eviction filing (Canay et al. 2020; Hull 2021), which follows recent evaluations of bias in other settings such as vehicle searches, capital sentencing, parole, bail, lending, and foster care (Knowles et al. 2001; Persico and Todd 2006; Alesina and La Ferrara 2014; Anwar and Fang 2015; Arnold et al. 2018; Marx 2021; Dobbie et al. 2021; Baron et al. 2023). I conduct my test by comparing a landlord's rate of winning repossession in white and minority cases with the same contract rent. The test is valid when landlords file eviction cases at their threshold of back rent (marginal outcomes) and a landlord's true payoff from winning repossession of the property does not vary with tenant race conditional on control variables (no omitted payoff bias).

I find evidence consistent with accurate statistical discrimination: landlords achieve statistically indistinguishable rates of winning repossession in court against white and minority tenants. This finding reflects that landlords account for racial disparities at later stages of the eviction process when deciding to file a case. An implication of this result is that policies to eliminate racial disparities in eviction court should reduce earlier discrimination by landlords filing eviction cases. Further upstream, however, it is possible that landlords would adjust how they screen potential tenants for rental units which could disproportionately harm minority renters.

While an extensive literature on housing market discrimination provides evidence of discrimination during the housing search process, this paper contributes evidence of discrimination in a new and high-stakes context: housing loss. Minority renters and homebuyers are known to face discrimination when accessing housing. Correspondence studies that send fictitious rental applications to landlords generally find lower callback rates for applicant names with a high likelihood of association with a minority group (Carpusor and Loges 2006; Ahmed and Hammarstedt 2008; Hanson and Hawley 2011; Ewens et al. 2014; Christensen et al. 2021).<sup>3</sup> Other research finds that minority homebuyers face discrimination when seeking mortgage loans (Munnell et al. 1996; Ladd 1998; Turner and Skidmore 1999; Ross et al. 2008; Hanson et al. 2016). Housing access, however, is a lower-stakes decision than housing loss, which is known to cause significant adverse effects (Collinson et al. 2024; Diamond et al.

<sup>&</sup>lt;sup>3</sup>A related literature uses in-person audits to test for discrimination against prospective homebuyers (Yinger 1986; Page 1995).

2020). I contribute the first known evidence of racial discrimination in high-stakes eviction filing decisions, indicating that minority renters face higher exposure to housing loss than white renters.

Methodologically, this paper contributes to a recent literature developing new tools to measure discrimination. While correspondence studies estimate discrimination by experimentally varying fictitious applicant names that are distinctly associated with different groups (Bertrand and Mullainathan 2004; Jacquemet and Yannelis 2012; Ewens et al. 2014; Edelman et al. 2017; Agan and Starr 2018; Kline et al. 2022), measuring discrimination is generally more challenging in observational data on high-stakes decisions such as eviction. The central challenge requires researchers to account for unobserved racial differences in the "qualification" of individuals for a treatment. One strand of literature overcomes this problem using structural models of decision-making (Knowles et al. 2001; Anwar and Fang 2006). More recent work leverages quasi-experimental institutional features such as random assignment to decision-makers (Arnold et al. 2018; Dobbie et al. 2021; Arnold et al. 2022; Baron et al. 2023), an identifiable control group of non-discriminatory decision-makers (Goncalves and Mello 2021), or observable outcomes from both random and algorithmic decision-making (Elzayn et al. 2023). Across this literature, these methods have overwhelmingly been developed for static decisions such as traffic stops (Knowles et al. 2001; Anwar and Fang 2006; Goncalves and Mello 2021), bail (Arnold et al. 2018, 2022), hiring (Bertrand and Mullainathan 2004), and lending (Dobbie et al. 2021), where the decision-maker first encounters the individual subject to the decision at the time treatment is assigned.

This paper contributes a novel method for measuring discrimination in dynamic settings characterized by a decision-maker observing the subject of a decision for a period of time prior to treatment. To measure total discrimination, I require this type of dynamic setting to have two key features. First, the measure of an individual's "qualification" (e.g. back rent) for a given treatment (e.g. eviction filing) must accrue gradually over time. Second, the relevant measure of "qualification" must be observable at the time of treatment. Since treatment is assigned the moment an individual's "qualification" crosses the decision-maker's threshold, total discrimination equates to racial differences in "qualification" at the time of treatment. In settings where the outcome of the treatment decision (e.g. winning court-ordered repossession) is also observable, it is possible to distinguish the sources of total discrimination using a marginal outcome test. This implementation of the marginal outcome test avoids the common "infra-marginality" problem of outcome tests (Ayres 2002) since the gradual accrual of an individual's "qualification" up to the decision-maker's threshold means that all individuals are treated at the margin.<sup>4</sup>

<sup>&</sup>lt;sup>4</sup>The "infra-marginality" problem refers to the fact that racial differences in average outcomes cannot

An advantage of this approach is that only data on treated units (i.e. filed eviction cases) is required to measure discrimination. Additionally, it does not require random assignment to decision-makers which allows for applications to observational data from wide range of important markets such as rental housing where landlords and tenants rarely match randomly. Besides eviction filing, other high-stakes settings where these new tools may be applied include worsening symptoms before a physician begins medical treatment, school absenteeism before an educator imposes disciplinary action, debt accrual before a creditor sells to a collection agency, or workplace misconduct leading up to an employee being fired.

The remainder of this paper is organized as follows. Section 2 details the legal framework governing discrimination in eviction and the Philadelphia eviction process. Section 3 describes the Philadelphia eviction court records used in the analysis. Section 4 models a landlord's eviction filing decision and demonstrates how discrimination enters. Section 5 outlines the empirical approach to measure total discrimination. Section 6 reports estimates of total discrimination. Section 7 distinguishes between the sources of total discrimination. Section 8 concludes.

# 2 Institutional Context

### 2.1 Fair Housing Act

The foundation of anti-discrimination law in the U.S. housing market is the Fair Housing Act. It prohibits discrimination by housing providers, such as landlords, whose discriminatory practices make housing unavailable on the basis of race, religion, sex, national origin, family status, or disability. When considering evictions in particular, the Department of Housing and Urban Development (HUD) states that "Selective evictions because of protected class (such as race or national origin) violate the Fair Housing Act even when the eviction might otherwise have been lawful" (HUD 2021a). More explicitly, HUD provides guidance to tenants that "Even if you are behind on your rent and subject to eviction, a landlord may not pick and choose which tenants to evict based on any protected characteristic" (HUD 2021b). They offer the following example of discrimination: "A landlord evicts a Black tenant for unpaid rent but does not evict a White tenant who has unpaid rent" (HUD 2021b).

inform differences in marginal outcomes if the racial groups have different underlying risk distributions. The intuition of my marginal outcome test is most similar to Anwar and Fang (2015) which tests for racial bias in parole decisions under the assumption that all inmates released by a Parole Board are released at the moment their potential recidivism rate is lowered enough via time spent incarcerated to hit the Parole Board's threshold for release.

I interpret these statements as the Fair Housing Act prohibiting a landlord from tolerating lease violations differently by tenant race before filing an eviction case.<sup>5</sup> When non-payment of rent is the only lease violation that has occurred, this equates to a landlord discriminating if they tolerate non-payment of rent differently by tenant race before filing an eviction case. I formalize this concept of discrimination as a landlord using different filing thresholds for white and minority tenants. The threshold concept is intuitive in cases caused only by non-payment of rent, where it is simply a measure of back rent. Since landlords may reasonably measure back rent in dollars or months owed, I define discrimination as racial disparities in a landlord's filing threshold of back rent among tenants with the same monthly contract rent.<sup>6</sup>

I argue that tenants equally behind on rent are equally qualified for an eviction filing, absent any other lease violations. This means that racial disparities in a landlord's filing threshold conditional on contract rent generates selective filings against minority tenants compared to equally qualified white tenants. This measure of discrimination is thus total discrimination—racial disparities in treatment among equally qualified individuals (Bohren et al. 2022)—which can arise from three potential sources. First, indirect discrimination can generate racial disparities in thresholds if landlords incorporate non-race characteristics correlated with race into filing decisions (Bohren et al. 2022). Second, accurate statistical discrimination can lead to racial disparities in thresholds if landlords account for actual racial differences in their payoff from filing (Phelps 1972; Arrow 1974; Aigner and Cain 1977). Third, racial bias can generate racial disparities in thresholds stemming from landlords' biased preferences or beliefs (Becker 1957; Bohren et al. 2019; Bordalo et al. 2016). I formalize these sources of discrimination and demonstrate how they can generate patterns of total discrimination in Section 4.

A benefit of estimating total discrimination in eviction filing is that it closely relates to the legal doctrine of disparate impact (Arnold et al. 2022; Bohren et al. 2022). The Fair Housing Act protects not only against selective evictions driven by racially discriminatory intent, but also against selective evictions that have a disparate impact on racial minorities if they were not necessary to achieve a legitimate interest for the landlord (HUD 2023). While selective evictions driven by discriminatory intent reflect only direct discrimination on the basis of race (accurate statistical discrimination or racial bias), the broader concept of disparate impact also includes indirect discrimination. The total discrimination measure

<sup>&</sup>lt;sup>5</sup>HUD has issued discrimination charges consistent with this interpretation. In 2012, HUD charged a landlord with discrimination in violation of the Fair Housing Act for attempting to evict a Black tenant in response to a disorderly conduct incident, alleging that the landlord did not attempt to evict white tenants with equally or more serious lease violations. See United States v. Altoona Housing Authority (W.D. Pa.) for details.

<sup>&</sup>lt;sup>6</sup>In Section 6.2, I demonstrate robustness to allowing landlords to consider non-payment of rent in an alternative unit of measure: back rent relative to the tenant's tenure in the rental property.

I estimate in this paper aligns with unlawful disparate impact if there exists no legitimate interest for a landlord to selectively file non-payment-based eviction cases against tenants equally behind on rent.

A potential inconsistency between my measure of total discrimination and unlawful disparate impact stems from the possibility that landlords may selectively evict low-income tenants who owe less back rent than their higher-income counterparts because landlords predict low-income tenants are less likely to pay rent in the future. To the extent income is correlated with race, I consider this behavior a form of indirect discrimination via non-race characteristics and include it under my definition of total discrimination because it leads landlords to evict minority tenants over less back rent than white tenants. However, one could argue that this behavior does not meet the legal standard to be considered unlawful disparate impact if a landlord's prediction about future rental earnings coming solely from observed non-race characteristics of tenants is considered a legitimate reason to tolerate less back rent from minority tenants.<sup>7</sup> In practice, I find no evidence of this potentially disputable type of indirect discrimination in Section 7.1, further supporting the interpretation that my estimates of total discrimination capture unlawful discrimination under the Fair Housing Act.

## 2.2 Eviction Filing Process in Philadelphia

Eviction filings are the first publicly documented step in the eviction process, although tenants may receive a short period of notice beforehand.<sup>8</sup> When filing an eviction case in Philadelphia, landlords indicate all causes of the filing, selecting from the following choices: non-payment of rent, breach of a condition of the lease, and/or termination of the lease term.<sup>9</sup> Note that they may indicate multiple causes. In the filing, the landlord also reports the amount of back rent, fees, and physical damages owed by the tenant, information on any other lease violations, and details of the lease agreement. After submitting this information, the landlord and tenant are notified of the scheduled hearing date.

<sup>&</sup>lt;sup>7</sup>It is important to note this behavior would be unlawful if tolerating less back rent from minority tenants was even in part based on tenant race or if landlords use tenant race as a signal for income.

<sup>&</sup>lt;sup>8</sup>Philadelphia landlords are mandated to provide tenants at least ten days notice prior to filing eviction cases over non-payment, but leases often waive this notice requirement in practice. Hoffman and Strephnev (2022) find that 67% of unsubsidized lease agreements associated with Philadelphia eviction cases waive or reduce the right to notice.

<sup>&</sup>lt;sup>9</sup>During the study period (2006-2019), Philadelphia landlords could terminate a lease for any reason except for in retaliation for a tenant reporting a complaint, joining a tenant organization, or being a victim of domestic violence. However, most filings (94%) involved non-payment of rent. A 2019 Philadelphia law introduced the requirement of "good cause" for filing an eviction case with allowable causes restricted to non-payment of rent, breach of lease, property damage, and other violations. For details, see Unfair Rental Practices, Philadelphia Code § 9-804.

### 2.3 Eviction Court Process in Philadelphia

After a landlord files an eviction case, the eviction case could proceed in a number of ways. A landlord may withdraw the case prior to the scheduled hearing date, typically in response to payment of the past due rent, the tenant voluntarily moving out, or a private settlement agreement. Barring a withdrawal, if the tenant does not appear in housing court for the scheduled hearing, the case results in a default judgment in favor of the landlord. In the relatively rare occurrence that the landlord does not appear in court for the scheduled hearing, the case results in a default judgment in favor of the scheduled hearing, the case results in a default judgment in court for the scheduled hearing, the case results in a default judgment in favor of the scheduled hearing.

If both the landlord and tenant appear in court, policy in Philadelphia requires the parties to attempt to reach an agreement before seeing a judge. This negotiation occurs between the landlord and tenant and any attorneys present.<sup>10</sup> If the parties reach an agreement, a staff member from the Municipal Court Mediation Unit reviews the agreement and confirms that both parties understand the agreement. Both parties then sign the agreement, and it is entered into the court record as a judgment by agreement (JBA) without any involvement of a judge (Reinvestment Fund 2020).<sup>11</sup> If the parties cannot reach an agreement, they contest the case before a judge who decides the case. Conditional on both parties appearing in court, 87% of cases result in a JBA, meaning that judges preside over much fewer eviction cases in Philadelphia (less than 5% of all filings) compared to other jurisdictions.<sup>12</sup>

Any judgment that is entered via default, a JBA, or decided by a judge may include a judgment for possession of the property and/or a judgment for money. After any judgment for possession in favor of the landlord is entered, the landlord may file for a writ of possession which will then be served to the tenant before the Landlord and Tenant Office or the Sheriff's Office executes the eviction.

# 3 Data

### 3.1 Eviction Court Records

I analyze the universe of residential eviction cases filed in Philadelphia between 2006 and 2019. These court records contain information on the filing date, defendant (tenant) names, plaintiff (landlord) names and addresses, property address, monthly contract rent,

 $<sup>^{10}</sup>$ Over 80% of landlords have legal representation, but less than 8% of tenants are represented. If neither party has an attorney, a volunteer mediator is provided.

<sup>&</sup>lt;sup>11</sup>The Philadelphia JBA system came about as a way to process high volumes of cases by allowing cases to proceed without any judge involvement (Reinvestment Fund 2020).

<sup>&</sup>lt;sup>12</sup>Unlike the JBA process in Philadelphia, judges in Cook County, Illinois and New York City approve settlement agreements, thus influencing a larger share of eviction filings (Collinson et al. 2024).

cause(s) of the case filing, itemized claim amount (including back rent, physical damages, etc.), judgments, and whether the landlord filed for a writ of possession. I construct landlord identifiers by linking eviction filings that share plaintiff names or plaintiff addresses. This linkage approach for identifying landlords is meant to account for inconsistent spellings and abbreviations, and the fact that many landlords owning multiple buildings list property-specific LLCs as plaintiff names on eviction filings. See Appendix B.1 for details.

The itemized amounts claimed are a unique feature of these data that are rarely observable in eviction case records from other jurisdictions. This is essential for testing for discrimination as the amounts provide a continuous measure of a landlord's grievances against a tenant at the time of an eviction filing. Only certain lease violations related to, for example, noise complaints or unapproved guests may not be reflected in the claimed amounts. For this reason, I focus on cases caused only by non-payment of rent for which the amount of back rent claimed should be a comprehensive measure of a tenant's "qualification" for the eviction filing.

### 3.2 Measuring Tenant Race

I impute the race of tenants using their names and property addresses listed on the eviction filing. To do so, I utilize the fully Bayesian Improved Surname Geocoding (fBISG) algorithm developed by Imai et al. (2022). The fBISG approach estimates the conditional posterior probability that tenant i belongs to racial group r given the i's first name, last name, and census block. See Appendix B.2 for more details on the fBISG methodology.

I conduct all analysis at the case filing level and compare landlords' treatment of white and minority tenants, where a minority tenant is defined as Black, Hispanic, Asian, or other. Since a filing may name multiple tenants who live in the same rental unit, I classify a filing as white if  $Pr(\text{all } i \text{ are white}) \geq 80\%$ . I classify a filing as minority if  $Pr(\text{any } i \text{ is a} \text{ minority}) \geq 80\%$ . The analysis sample excludes 14.62% of filings (N=44,824) that cannot be classified as white or minority with at least 80% certainty.<sup>13</sup>

Table 1 reports the racial composition of Philadelphia eviction filings based on imputed tenant race. For comparison, I also report the racial composition of the population of renters in Philadelphia from the 2010 decennial census. The vast majority of eviction cases are filed against non-white tenants. Black tenants appear especially over-represented while white tenants appear especially under-represented in eviction filings.

<sup>&</sup>lt;sup>13</sup>In Section 6.2.1, I demonstrate that the results are robust to including these cases with less certain race predictions to estimate alternative imputation-based estimators: a linear estimator using the continuous race probabilities and a probabilistic estimator that weights filings by the race probabilities.

	Eviction Filings	Renter Population
Asian	1.40%	5.46%
Black	76.97%	44.36%
Hispanic	7.69%	10.92%
White	9.06%	36.83%
Other/Mixed	4.88%	2.43%
N	261,738	

Table 1: Racial Composition of Eviction Filings

This table reports the racial composition of eviction filings and renters in Philadelphia. The eviction filings sample includes filings that could be classified as white or minority with at least 80% certainty. A minority filing is further classified as detailed race r if the most likely racial group of each individual tenant named in the filing is r. A minority filing is classified as mixed if the most likely racial group of each individual tenant named in the filing differ. The racial composition of renters in Philadelphia is sourced from the 2010 decennial census.

### **3.3 Descriptive Statistics**

Philadelphia eviction records offer unique insight into the circumstances under which landlords file eviction cases, revealing that many tenants face eviction cases over strikingly little unpaid rent. Among cases filed only due to non-payment of rent, tenants owe less than one month of contract rent in 10.6% of cases (1,433 cases per year) and owe less than \$600 in 17.7% of cases (2,389 cases per year). Moreover, minority renters appear particularly vulnerable to eviction cases. Not only are they more likely to face an eviction filing (Table 1), but the average claim amount tenants owe at the time of the filing is over \$600 lower for minority tenants than white tenants (Table A1).<sup>14</sup> Considering only the portions of the claim for which tenants are directly responsible—back rent, physical damages, and late fees,—it remains that minority tenants owe less on average than white tenants (Table A1).

Figure 1a plots the full distribution of claim amounts by tenant race and illustrates more clearly that minority tenants owe less than white tenants when landlords file eviction cases. Among cases caused only by non-payment of rent, the minority distribution of back rent owed at filing remains visibly lower than the white distribution of back rent (Figure 1b). Since minority tenants with eviction filings also tend to have lower monthly contract rents than white tenants (Table A1), minority tenants could theoretically owe equal months of back rent at the time of the filing as white tenants. However, this does not appear to be

<sup>&</sup>lt;sup>14</sup>The claim amount captures the total amount the landlord claims the tenant owed at the time the landlord filed the eviction case. The amount claimed on eviction filings tends to be mostly composed of back rent, but landlords also claim physical damages, late fees, attorney fees, and other fees.



### Figure 1: Distribution of Amounts Owed at Filing

Subfigure (a) plots the claim amount distribution separately for Philadelphia eviction filings against white and minority tenants. Subfigure (b) plots the back rent distribution of filings separately for filings against white and minority tenants that were caused only by non-payment of rent. Both subfigures exclude filings that could not be classified as white or minority with at least 80% certainty based on tenant race imputations. When plotting the distributions, amounts are top-coded at \$15,000.

the case in Figure A1a, which shows that minority tenants also appear to owe fewer months of back rent at the time of filing. This pattern is even clearer among tenants with below median contract rents (Figure A1b).

The outcomes of eviction filings also appear to be related to tenant race. Figure 2 plots the relationship between the amount of back rent owed at filing and whether the landlord won repossession of the property—measured as filing for a writ of possession—separately for white and minority cases. It is first important to note that back rent appears positively correlated with the likelihood of the landlord winning repossession. This is consistent with landlords more easily wining repossession from tenants with greater lease violations. The second key takeaway is that landlords tend to win repossession from minority tenants at a higher rate than in cases against white tenants owing similar back rent. This pattern is visible across the entire distribution of back rent and suggests that it requires less back rent for landlords to win repossession from minority tenants.



Figure 2: Racial Differences in Writ of Possession

This figure plots a bin scatter of the relationship between back rent and landlords filing for a writ of possession. Each point reflects 500 eviction cases against tenants belonging to a given racial group. The sample includes Philadelphia eviction filings caused only by non-payment of rent and excludes filings that could not be classified as white or minority with at least 80% certainty based on tenant race imputations. The x-axis is the median back rent among cases in the cell. The y-axis is the rate of landlords filing for a writ of possession among cases in the cell.

# 4 Conceptual Framework

To understand how racial disparities in the amount owed at filing could be explained by racial discrimination, I provide a framework to conceptualize how discrimination may enter a landlord's eviction filing decision. In this section, I first provide a model of a landlord's threshold rule for filing eviction cases. I then use this framework to demonstrate how discrimination can arise from three potential sources—indirect discrimination, accurate statistical discrimination, or racial bias—and discuss the testable implications of each source.

### 4.1 Landlord Filing Decision

Consider all tenants of a single landlord. Let  $R_i$  denote tenant race and let  $Z_i$  capture all characteristics of tenant *i* observable to the landlord besides race.  $Z_i$  may include individual

characteristics such as income and also characteristics of the rental property including neighborhood factors. Each month, tenant *i* agrees to pay the landlord the contract rent amount  $Y_i^0$ . If the tenant falls behind on rent, they accrue some amount of back rent,  $X_i(t)$ , owed to the landlord. For simplicity, assume that the tenant commits no other lease violations besides non-payment of rent. At any time *t* such that  $X_i(t) > 0$ , the landlord can legally file an eviction case over non-payment. Let  $D_i(t) = 1$  denote this eviction filing action taken by the landlord. Filing an eviction case is costly, however. The landlord pays a fixed cost *c* to file an eviction case, which reflects any unrecoverable court filing costs and legal fees.<sup>15</sup>

If the landlord chooses to file an eviction case against i, the case could either result in the landlord winning repossession of the property to rent to a new tenant  $(Y_i^* = 1)$  or the current tenant remaining in the property  $(Y_i^* = 0)$ . The true expected probability that the landlord wins repossession of the property is:

$$\tilde{p}(x, y, z, r) = \mathbb{E}[Y_i^* = 1 | X_i(t) = x, Y_i^0 = y, Z_i = z, R_i = r]$$
(1)

I assume  $\tilde{p}(x, y, z, r)$  is weakly increasing in back rent, x, which is consistent with the positive relationship between repossession rates and back rent in Figure 2. The likelihood of winning repossession also depends on  $Y_i^0$  since the ongoing contract rent amount informs the severity of the amount of back rent.  $Z_i$  enters  $\tilde{p}$  given that tenants with lower incomes or greater childcare responsibilities, for example, may have different likelihoods of appearing in eviction court as scheduled, affecting the likelihood of the landlord winning repossession.  $R_i$  enters  $\tilde{p}$  to allow for the possibility that minority tenants face unequal access to court resources or the eviction court process systematically favors white tenants, as suggested by Figure 2.

The landlord may have biased perceptions about the probability of winning repossession of the property. I define the landlord's perceived probability of winning repossession as:

$$p(x, y, z, r) = \tilde{p}(x, y, z, r) + b^p(r)$$
(2)

The bias term  $b^p(r)$  allows for landlords to have racially biased beliefs about the likelihood of winning repossession from a given tenant (Bohren et al. 2019; Bordalo et al. 2016).

The landlord's true payoff from winning repossession of the property from tenant i is  $\tilde{v}(y,z)$  for  $Y_i^0 = y$  and  $Z_i = z$ . Intuitively,  $\tilde{v}$  captures the difference in expected future rental income from replacing tenant i with some unknown new tenant. Non-race characteristics,  $Z_i$ , enter  $\tilde{v}$  since i's factors such as income may predict the likelihood that i pays rent in

<sup>&</sup>lt;sup>15</sup>I allow c, as well as the  $\tilde{p}$  and  $\tilde{v}$  functions defined below, to vary across landlords when I bring the model to data. Here, however, I simplify the model's notation by considering the behavior of a single landlord.

the future, affecting the landlord's payoff from replacing them.<sup>16</sup> Contract rent,  $Y_i^0$ , enters  $\tilde{v}$  since it captures the rental value of the property that the landlord could earn from a paying tenant. I assume that race cannot impact the true payoff  $\tilde{v}$ , holding fixed  $Z_i$  and  $Y_i^0$ .

I model the landlord's perceived payoff from winning repossession as:

$$v(y, z, r) = \tilde{v}(y, z) + b^v(r) \tag{3}$$

The perceived payoff v additionally varies with tenant race r to allow for the possibility that the landlord exhibits racial bias,  $b^v(r)$ . The bias term,  $b^v(r)$ , may reflect taste-based discrimination (Becker 1957) if the landlord values removing minority tenants from the property differently from removing white tenants, conditional on  $\tilde{v}$ . Alternatively,  $b^v(r)$  may reflect racially biased beliefs or stereotypes (Bohren et al. 2019; Bordalo et al. 2016) about the likelihood of the current tenant paying rent in the future.

The landlord files an eviction case against tenant i if the perceived payoff weighted by the perceived probability of winning repossession exceeds the fixed cost of filing:<sup>17</sup>

$$D_i(t) = \mathbb{1}\{p(X_i(t), Y_i^0, Z_i, R_i) v(Y_i^0, Z_i, R_i) \ge c\}$$
(4)

By monotonicity of p(x, y, z, r) over x, this is equivalent to a threshold rule for filing an eviction case:

$$D_i(t) = \mathbb{1}\{X_i(t) \ge \tau_i\}\tag{5}$$

where

$$\tau_i = p^{-1} \left( \frac{c}{v(Y_i^0, Z_i, R_i)}, Y_i^0, Z_i, R_i \right) = \tilde{p}^{-1} \left( \frac{c}{\tilde{v}(Y_i^0, Z_i) + b^v(R_i)} - b^p(R_i), Y_i^0, Z_i, R_i \right)$$
(6)

From this threshold rule, it is apparent that the landlord's threshold  $\tau_i$  varies with the tenant's non-race characteristics, contract rent, and race.

<sup>&</sup>lt;sup>16</sup>It is also possible that back rent,  $X_i(t)$ , predicts the likelihood *i* pays rent in the future. This would not affect the ability to formulate the landlord's filing decision as a threshold rule since  $\tilde{v}$  would also be increasing in back rent. As such, the test for total discrimination described in Section 5 does not require excluding  $X_i(t)$  from  $\tilde{v}$ . Any dependence of  $\tilde{v}$  on  $X_i(t)$  would only be a potential concern when applying Propositions 1 and 2 discussed below to the data. I address this concern in Section 7.2, where I find that the results of the empirical application of Propositions 1 and 2 are robust to allowing the landlord's true payoff from repossession to incorporate back rent owed by the current tenant.

<sup>&</sup>lt;sup>17</sup>This follows from a landlord maximizing utility  $U_0 + D_i(t)(p(X_i(t), Y_i^0, Z_i, R_i)v(Y_i^0, Z_i, R_i) - c)$  for some  $U_0$ .

### 4.2 Discrimination Definition and Sources

From Section 2.1, a landlord exhibits total racial discrimination if they use different filing thresholds,  $\tau_i$ , by tenant race conditional on contract rent:

$$\mathbb{E}[\tau_i|Y_i^0 = y, R_i = minority] \neq \mathbb{E}[\tau_i|Y_i^0 = y, R_i = white]$$
(7)

Racial discrimination against minority (white) tenants is the specific case of lower thresholds for minority (white) tenants. Given that  $\tau_i$  may vary by a tenant's race and non-race characteristics (Equation 6), total discrimination (Equation 7) can arise from three potential sources: indirect discrimination, accurate statistical discrimination, or racial bias.

First, indirect discrimination arises from components of  $Z_i$  that are correlated with race. Such variation in  $Z_i$  can generate racial differences in  $\tau_i$  even if neither p nor v depend on  $R_i$ . If indirect discrimination alone explains total racial discrimination, then thresholds should be equal across race holding fixed  $Z_i$ :

$$\mathbb{E}[\tau_i|Y_i^0 = y, Z_i = z, R_i = minority] = \mathbb{E}[\tau_i|Y_i^0 = y, Z_i = z, R_i = white]$$
(8)

If instead the landlord exhibits direct discrimination on tenant race (accurate statistical discrimination or racial bias), then thresholds would vary by tenant race even after holding fixed  $Z_i$ .

Second, a landlord exhibits accurate statistical discrimination if  $b^p(minority) = b^p(white)$ and  $b^v(minority) = b^v(white)$ . Accurate statistical discrimination can generate patterns of total discrimination due to the dependence of  $\tilde{p}$  on  $R_i$ . In essence, a landlord's accurate predictions about any racial differences in the likelihood of winning repossession in court results in their filing threshold varying by tenant race. To test this source, I can compare the marginal outcomes of filings across tenant race:

**Proposition 1.** If a landlord exhibits accurate statistical discrimination, then  $\mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = minority] = \mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = white].$ 

The intuition of Proposition 1 is that a landlord accurately accounting for any racial differences in the likelihood of winning repossession in court should achieve equal rates of winning repossession across tenant race at the margin, holding fixed non-race characteristics and contract rent. For the proof of Proposition 1, see Appendix C.1.

Third, racial bias arises from the dependence of  $\tau_i$  on  $b^p(R_i)$  and  $b^v(R_i)$ . A landlord exhibits racial bias if  $b^p(minority) \neq b^p(white)$  or  $b^v(minority) \neq b^v(white)$ . Holding fixed  $Z_i$ , racially biased beliefs or preferences about the probability of repossession or the payoff from repossession can generate racial differences in filing thresholds even if  $\tilde{p}$  does not actually depend on  $R_i$ . A testable implication of racial bias is that landlords deviate from equal rates of winning repossession across race at the margin, holding fixed non-race characteristics and contract rent:

**Proposition 2.** If  $\mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = minority] \neq \mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = white]$ , then a landlord exhibits racial bias.

The intuition of Proposition 2 is that a landlord tolerating a lower expected repossession rate from one racial group must reflect racially biased preferences or beliefs. Racial bias against minority (white) tenants is the specific case where the expected repossession rate is lower for minority (white) tenants. For the proof of Proposition 2, see Appendix C.2. Propositions 1 and 2 constitute a marginal outcome test for racial bias in eviction filing which is consistent with Canay et al. (2020) and follows the same intuition of outcome-based tests for racial bias conducted in other settings (Knowles et al. 2001; Persico and Todd 2006; Alesina and La Ferrara 2014; Anwar and Fang 2015; Arnold et al. 2018; Marx 2021; Dobbie et al. 2021; Baron et al. 2023).<sup>18</sup>

# 5 Empirical Approach

I next describe my empirical test for total racial discrimination in eviction filing. I return to the question of which sources of total discrimination landlords exhibit in Section 7. For a single landlord, total discrimination at contract rent y is:

$$\mathbb{E}[\tau_i|Y_i^0 = y, R_i = minority] - \mathbb{E}[\tau_i|Y_i^0 = y, R_i = white]$$
(9)

No discrimination is the case in which Equation 9 is zero. Total discrimination against minority (white) tenants is the case where Equation 9 is negative (positive).

To estimate Equation 9, I leverage the notion that tenants gradually accrue back rent until back rent reaches the landlord's eviction filing threshold. I observe the amount of back rent tenants owe upon crossing this threshold, which is the time the landlord files an eviction case. I denote this observable value  $X_i(t^*)$ , where  $t^* = \min(t)$  such that  $D_i(t) = 1$ . My empirical approach utilizes observable racial differences in  $X_i(t^*)$  to estimate racial differences in  $\tau_i$ , conditional on landlord and contract rent.

To build intuition for this approach, suppose that back rent evolves continuously over time. Continuous  $X_i(t)$  implies that back rent owed at the time of an eviction filing is

 $<sup>^{18}</sup>$ Propositions 1 and 2 are a stricter version of the marginal outcome test in Hull (2021) which allows bias to arise via non-race characteristics.

exactly the landlord's threshold:  $X_i(t^*) = \tau_i$ . Therefore, total discrimination for a particular landlord at contract rent y is:

$$\mathbb{E}[X_i(t^*)|Y_i^0 = y, R_i = minority] - \mathbb{E}[X_i(t^*)|Y_i^0 = y, R_i = white]$$
(10)

In reality, back rent may evolve discretely over time  $t \in \{0, 1, 2, 3, ...\}$ . Discrete changes in back rent, however, are bounded due to the monthly payment schedule of rent. Tenant *i* cannot owe more than one additional month of contract rent than in the previous month:

$$X_i(t) \le X_i(t-1) + Y_i^0 \tag{11}$$

In practice, distressed renters often make partial rent payments (Manville et al. 2023), suggesting that discrete changes in back rent may be smaller than one month of contract rent in many cases. Discrete changes in back rent may place back rent owed at time  $t^*$  above the landlord's threshold, but the distance above is bounded by the value of contract rent:

$$X_i(t^*) \in \left[\tau_i, \tau_i + Y_i^0\right) \tag{12}$$

I assume that back rent owed at filing exceeds the threshold by the same amount across white and minority tenants who have the same landlord and contract rent. Formally, this assumption is as follows for tenants of a single landlord with contract rent y:

$$\mathbb{E}[X_i(t^*) - \tau_i | Y_i^0 = y, R_i = minority] = \mathbb{E}[X_i(t^*) - \tau_i | Y_i^0 = y, R_i = white]$$
(13)

In essence, this assumption imposes that back rent owed at filing by white and minority tenants "overshoots" the filing threshold by the same amount on average. From this assumption, it follows that any racial differences in back rent equate to racial differences in thresholds conditional on landlord and contract rent, meaning that total discrimination in the discrete case is identical to the continuous case (Equation 10). The assumption would only be violated if the size of the last discrete change in back rent before the filing is statistically different by race among tenants with the same landlord and contract rent. See Appendix D for a discussion of bias if this assumption is violated.

To implement the test for total discrimination, I estimate the following specification for eviction filings caused only by non-payment of rent against tenant(s) i by landlord j on filing date t:

$$Ln(Back \ Rent)_{ijt} = \beta Minority_i + \alpha_{j,y(ijt)} + \varepsilon_{ijt}$$
(14)

Back Rent is the dollars of back rent owed at the time of the eviction filing. Minority is

an indicator equal to one if the filing is at least 80% likely to be against a minority tenant based on imputed tenant race. I include landlord-by-contract rent fixed effects,  $\alpha_{j,y(ijt)}$ , to condition on landlord j and contract rent y. I cluster standard errors at the landlord level.

For estimates of total discrimination,  $\beta$ , based on imputed race to be unbiased, I require the assumption that race is conditionally independent of the amount of back rent owed at filing given the imputation inputs—an individual's first name, last name, and census block. I discuss potential violations of this assumption in Section 6.2. It is also important to note that  $\beta$  is only identified for all j, y such that  $Filings_{j,y,white} > 0$  and  $Filings_{j,y,minority} > 0$ , meaning that  $\beta$  reflects average discrimination among this selected sample.

# 6 Discrimination Results

### 6.1 Main Results

In Table 2, I present the results of estimating Equation 14. Column 1 reports the raw racial disparity in back rent owed at the time of eviction filings, and column 2 estimates the disparity that is attributed to racial discrimination. The discrimination estimate is interpreted as minority tenants owing 4.9% less back rent at the time of a filing than white tenants with the same landlord and contract rent. This is evidence that landlords discriminate by using 4.9% lower filing thresholds for minority tenants. Comparing columns 1 and 2, discrimination explains about 11% of the raw racial disparity in back rent owed at filing. The difference between these coefficients reflects tenants sorting to landlords and contract rents differently by race. Some of this sorting may reflect earlier discrimination that occurs when landlords consider rental applications (Hanson and Hawley 2011; Ewens et al. 2014; Christensen et al. 2021). This means that the discrimination coefficient reported in column 2 isolates discrimination in eviction filing from any earlier housing market discrimination, providing evidence that racial discrimination persists beyond entry into rental housing.

The magnitude of the racial discrimination estimate is economically meaningful. Landlords using 4.9% lower filing thresholds for minority tenants equates to tolerating \$121 fewer dollars of back rent (column 3) or 0.13 fewer months of back rent (column 4) from minority tenants. To better appreciate the magnitude, I calculate how many minority eviction filings occur sooner than white eviction filings due to discrimination. I simulate a nondiscriminatory threshold for minority tenants equal to the mean back rent owed at filing among white tenants with the same landlord and contract rent. Each year, an average of 1,908 minority filings have lower back rent than the simulated threshold, meaning that they occur systematically sooner (over less back rent) than cases against white tenants.

	Back Rent				
	Log Dollars       (1)     (2)		Dollars	Months	
			(3)	(4)	
Minority Tenant	$-0.4500^{**}$ (0.2274)	$-0.0490^{***}$ (0.0167)	$-120.8471^{***} \\ (39.8827)$	-0.1336*** (0.0420)	
$\begin{array}{c} \text{Observations} \\ \text{R}^2 \end{array}$	$159,799 \\ 0.0176$	$120,328 \\ 0.6442$	$120,328 \\ 0.2079$	$119,766 \\ 0.6000$	
Mean White Outcome Landlord $\times$ Contract Rent FE	7.4654	7.3686 ✓	2,000.17 ✓	2.2698 ✓	

Table 2: Total Discrimination

p < 0.1, p < 0.05, p < 0.01. This table reports the results from estimating Equation 14. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Columns 2 through 4 exclude singleton landlord-by-contract rent observations. Contract rent fixed effects refer to the exact dollar value of contract rent. The dependent variable in column 4 is the dollar value of back rent divided by contract rent. Standard errors are clustered at the landlord level.

This is 53.05% of minority filings with a white counterpart in the same landlord-by-contract rent cell and 18.22% of all minority filings in the analysis sample. Whether these minority tenants would continue to fall behind on rent and still receive a filing at the higher, nondiscriminatory threshold requires an assumption about how many minority tenants would catch up on rent. In other settings, even small amounts of credit can reduce eviction filings (Lodermeier 2024), suggesting that informal credit from landlords raising minority thresholds by 4.9% may allow a substantial share of minority tenants to avoid a filing entirely.

## 6.2 Robustness Checks

Concerns about the discrimination estimate may arise due to tenant race being imputed, potential omitted lease violations, other ways landlords may measure non-payment, responses to eviction notices, the construction of landlord identifiers, the selected sample of landlords that identify discrimination, or the possibility of race-varying discrete changes in back rent. To address these concerns, I first demonstrate robustness to alternative imputation-based estimators of racial disparities, excluding cases with possible unobserved lease violations, allowing non-payment thresholds to be measured in back rent relative to tenure, restricting to cases where tenants received little notice, and using more conservative landlord definitions. I then broaden the set of landlords that identify discrimination by using coarser contract rent fixed effects. Lastly, I consider the magnitude of any bias from different accrual of back rent across race and validate that the main results cannot be explained by any such bias.

	Log Back Rent					
	70% Cutoff	90% Cutoff	Linear	Probabilistic		
	(1)	(2)	(3)	(4)		
Minority Tenant	$-0.0397^{***}$ (0.0143)	$-0.0579^{***}$ (0.0220)	$-0.0365^{**}$ (0.0174)	$-0.0474^{***}$ (0.0080)		
$\begin{array}{l} \text{Observations} \\ \text{Mean White Back Rent} \\ \text{Landlord} \times \text{Contract Rent FE} \end{array}$	$128,108 \\ 1,589.49 \\ \checkmark$	$108,605 \\ 1,604.54 \\ \checkmark$	140,294 1,104.18 ✓	$140,294 \\ 1,119.71 \\ \checkmark$		

Table 3: Total Discrimination Using Alternative Race Imputation Approaches

\*p< 0.1, \*\*p< 0.05, \*\*\*p< 0.01. This table reports the total discrimination estimates under various race imputation methods. Columns 1 and 2 classify filings as white or minority based on 70% and 90% imputation cutoff rules. Column 3 regresses log back rent on the continuous probability that the filing is against at least one non-white tenant. Column 4 reports a probabilistic estimator that is equal to the minority-white difference in probability-weighted average log back rent. Average minority log back rent is calculated by weighting all filings by the probability that the filing is against at least one non-white tenant. Average white log back rent is calculated by weighting all filings by the probability that the filing is against at least one non-white tenant. Average white log back rent is calculated by weighting all filings by the probability that the filing is against entirely white tenants. To condition on landlord and contract rent, I regress log back rent on landlord-by-contract rent intercepts using the race probabilities as weights and calculate the mean of the intercepts. The sample in columns 1 and 2 includes Philadelphia eviction cases filed only due to non-payment of rent that could be classified as white or minority with 70% or 90% certainty. The sample in columns 3 and 4 includes all Philadelphia eviction cases filed only due to non-payment of rent. All specifications exclude singleton landlord-by-contract rent observations. Contract rent fixed effects refer to the exact dollar value of contract rent. Standard errors in columns 1 through 3 are clustered at the landlord level. Standard errors in column 4 are derived from the asymptotic distribution of the probabilistic estimator described in Elzayn et al. (2023).

#### 6.2.1 Race Imputation

My estimate of discrimination using imputation-based race classifications is unbiased if race is conditionally independent of the amount of back rent owed at filing given an individual's name and census block. If this assumption is violated, the estimated racial disparities in back rent using cutoff-based race classifications may be biased in either direction (Chen et al. 2019). For example, I may understate discrimination (in absolute value) if conditional on the imputed probability a tenant is a racial minority, landlords tolerate less back rent from true minority renters than true white renters. I may overstate discrimination if racial disparities in back rent among the unclassified cases that fall below the 80% cutoff and are excluded from the analysis are smaller than disparities among the classified cases.

To address concerns about these potential biases, I first verify that the choice of cutoff (80%) for classifying filings by race is not driving the results. I find significant evidence of discrimination using 70% or 90% cutoffs in columns 1 and 2 of Table 3. Next, I find similar evidence of discrimination in column 3 when using a linear estimator based on the continuous probability that the filing is against at least one non-white tenant. In column 4, I report a

probabilistic estimator (Chen et al. 2019; Elzayn et al. 2023) that weights each filing by the probability it is white or minority to calculate race-specific average back rents, and again find strikingly similar evidence of discrimination. Note that in other settings, the linear and probabilistic estimators have been shown to bound the true racial disparity (Elzayn et al. 2023).<sup>19</sup>

### 6.2.2 Omitted Lease Violations

By focusing on eviction cases caused only by non-payment of rent, back rent owed at the time of filing should fully capture how qualified a tenant is for an eviction filing. In Table A1, however, some landlords indicate the cause of the filing is only non-payment but also claim physical damages or other fees in the filing. This may indicate some unobserved lease violations in the sample of cases appearing to be caused only by non-payment of rent. To ensure unobserved lease violations are not biasing my estimate of total discrimination, I further restrict the sample to cases caused only by non-payment of rent with \$0 of physical damages or other fees claimed in the filing. Estimates of total discrimination are robust to using this further restricted sample (column 1 of Table A2). I also find similar evidence of discrimination when comparing the sum of back rent and late fees owed at filing in this sample (column 2 of Table A2). Since other fees claimed in the eviction filing may include missed utility payments which accrue similarly to back rent, I also compare the sum of back rent, late fees, and other fees owed at the time of filing in column 3 of Table A2, and again estimate a nearly identical magnitude of discrimination.

#### 6.2.3 Measuring Non-Payment of Rent

The discrimination test allows for equal thresholds across race measured in either dollars or months of back rent to be considered non-discriminatory behavior. However, an additional unit of measure to consider for thresholds of non-payment is the ratio of back rent to a tenant's tenure in the rental property.<sup>20</sup> For example, a landlord may make different filing

<sup>&</sup>lt;sup>19</sup>The linear and probabilistic estimators bound the true racial disparity in x when the following covariance terms have the same sign:  $\mathbb{E}[\operatorname{Cov}(x, M|m)]$  and  $\mathbb{E}[\operatorname{Cov}(x, m|M)]$ , where M is an indicator for true minority status and m is the imputed minority probability. Note that these covariance terms must also condition on landlord and contract rent when considering the bounding assumptions for the discrimination measure in this paper.

<sup>&</sup>lt;sup>20</sup>Note that this ratio introduces a linear assumption about the accrual of back rent over time. This may be unrealistic if, for instance, tenants pay rent on time for some period of time and only begin to fall behind after an income shock. Since I do not observe the path of back rent prior to the filing, I cannot examine the validity of the linear assumption. To the extent that back rent indeed accrues non-linearly but at a similar rate among white and minority tenants with the same landlord and contract rent, then the main discrimination results in Table 2 already allow for non-discriminatory landlords to measure non-payment by the ratio of back rent to the time of accrual.

decisions for a new tenant who failed to pay their first rental payment and a longstanding tenant who gradually accrued an equivalent amount of back rent. One could argue that this form of selective eviction filing behavior is non-discriminatory if it is reasonable for a landlord to measure non-payment of rent in terms of back rent relative to tenure. I allow for landlords to utilize this additional measure of non-payment by including landlord-by-contract rent-by-tenure fixed effects in the total discrimination test in Table A3.<sup>21</sup> I find similar and if anything, larger magnitudes of discrimination when also conditioning on tenure. This suggests that discrimination remains even if landlords using equal filing thresholds of back rent divided by tenure is considered non-discriminatory behavior.

#### 6.2.4 Notice Period

Philadelphia law mandates that landlords provide tenants with ten days notice prior to filing eviction cases over non-payment of rent. After receiving an eviction notice, some tenants may quickly pay back rent or vacate the property to avoid an eviction filing. If such responses to a notice vary by race or if landlords provide different amounts of notice by race, my estimates of discrimination could include selection bias. In practice, however, many eviction cases are filed with substantially less than ten days notice or no notice at all (Hoffman and Strezhnev 2022).<sup>22</sup> To verify that my discrimination test is not driven by different responses to notices, I re-estimate discrimination among tenants who received little notice in Table A4 and find similar evidence of discrimination.<sup>23</sup> I also find similar results when controlling for the number of days of notice a tenant received prior to the filing.

#### 6.2.5 Landlord Identifiers

The landlord identifiers based on linked networks of shared plaintiff names and plaintiff addresses are intended to group inconsistent spellings corresponding to a single landlord and property-specific LLCs that may be managed by a single owner or property manager. When constructing these landlord identifiers, the linkage algorithm finds five landlords that account for 39% of all residential eviction filings. While this may reflect true eviction filing volumes by some large landlords, it is also possible that rental properties that switch management over the analysis period could generate false links between otherwise separate landlords. To account for this potential concern, I estimate discrimination using more conservative landlord

<sup>&</sup>lt;sup>21</sup>Due to power concerns from the high-dimensionality of these fixed effects and since tenure is only observable for cases filed after February 2011, I use \$25 bins of contract rent for this test.

 $<sup>^{22}</sup>$ In the eviction filing data, I observe if and when the landlord provided a notice to the tenant. Almost one in four cases in the analysis sample were filed with less than two days notice.

<sup>&</sup>lt;sup>23</sup>Due to power concerns with this much smaller sample of filings, I use \$25 bins of contract rent instead of the dollar value of contract rent.

definitions in Table A5. First, I identify potentially problematic landlord identifiers: those that filed at least 25 eviction cases over the analysis period and the most common plaintiff address associated with the landlord was used in less than 50% of their filings. I separate these potentially problematic landlords into sub-groups based on plaintiff addresses. Second, I define landlords as simply plaintiff addresses or plaintiff names. Under each of these more conservative landlord definitions, I still find significant evidence of discrimination.

#### 6.2.6 Selected Sample of Landlords Identifying Discrimination

Measuring discrimination requires landlords to have filed multiple eviction cases against tenants with the same contract rent that belong to different racial groups. The effective sample that identifies discrimination is 35% (N = 56,526) of cases in the analysis sample that are filed by 486 unique landlords.<sup>24</sup> Since my discrimination estimate is only representative of this effective sample, it may not reflect average discrimination in Philadelphia. To broaden the set of landlords reflected in the discrimination estimate, I use coarser fixed effects in Table A6. Using additive landlord and contract rent fixed effects yields a nearly identical estimate of discrimination, as does grouping contract rent into \$100, \$50, and \$25 bins. The samples that identify discrimination with these coarser fixed effects reflect many more unique landlords (up to 1,274). The stability of the discrimination estimate in these larger and more representative samples is consistent with my main discrimination estimate reflecting average discrimination in Philadelphia.

### 6.2.7 Discrete Back Rent Assumption

Measuring discrimination also requires the assumption that the amount by which back rent exceeds the landlord's threshold at the time of filing is race-invariant conditional on landlord and contract rent (Equation 13). If instead the last discrete change in back rent prior to the filing is statistically smaller for minority tenants, then white tenants' back rent would be further above the landlord's threshold at the time of the filing than that owed by minority tenants. This would negatively bias my estimate of discrimination. In Appendix D, I demonstrate that while minority tenants appear to have slightly smaller discrete changes in back rent in the data, the magnitude could only explain up to 9% of my discrimination estimate.

 $<sup>^{24}</sup>$ This is smaller than the 120,328 cases reported in column 2 of Table 2 because the sample of 120,328 cases includes landlord-by-contract rent cells with multiple filings, but some of these cells only include a single race.

### 6.3 Landlord-Specific Discrimination

The estimate of racial discrimination thus far reflects average discrimination across landlords. This could be driven by a small number of highly-discriminatory landlords or reflect widespread discrimination among Philadelphia landlords. To better understand the prevalence of discrimination in eviction filing, I estimate landlord-specific discrimination coefficients. To do so, I estimate an alternative version of Equation 14 that allows for the discrimination parameter to vary by landlord:

$$Ln(Back \ Rent)_{ijt} = \beta_j Minority_i + \alpha_{j,y(ijt)} + \varepsilon_{ijt}$$
(15)

Estimates of  $\beta_j$  are most informative for landlords with a sufficient number of filings against white and minority tenants with the same contract rent, but many landlords file few eviction cases. Therefore, I adopt three different approaches to estimate  $\beta_j$ , varying the filing volume of landlords required for inclusion.

First, I restrict to cases that are found in landlord-by-contract rent cells with at least two non-payment of rent filings against white tenants and minority tenants. Due to the small size of many landlord-by-contract rent cells, this method can only identify landlord-specific discrimination coefficients for 127 unique landlords, but these landlords are responsible for 51% of Philadelphia eviction cases. As a second approach, I estimate  $\beta_j$  parameters from a regression with landlord and contract rent fixed effects included separately instead of interacted, and I again restrict to landlords that filed at least two non-payment-based filings against each racial group. This second approach allows me to estimate  $\beta_j$  coefficients for 520 unique landlords who are responsible for 59% of Philadelphia eviction cases. Finally, as a third approach, I again estimate  $\beta_j$  parameters using additive landlord and contract rent fixed effects but remove the restriction to landlords that filed at least two non-payment cases against each racial group. This third approach allows me to estimate  $\beta_j$  coefficients for 1,274 unique landlords who are responsible for 64% of all Philadelphia eviction cases.

I plot the distributions of these landlord-specific discrimination coefficients in Figure 3. The median  $\hat{\beta}_j$  from the three methods are -0.0683, -0.0205, and -0.0267, respectively, suggesting that the median landlord exhibits discrimination in eviction filing. From Figure 3, however, it remains unclear what share of landlords exhibit discrimination. The standard errors of each individual  $\beta_j$  estimated by Equation 15 are quite imprecise due to the often small number of eviction cases filed by a single landlord.<sup>25</sup>

To account for noise in the  $\hat{\beta}_j$ s and estimate the share of landlords exhibiting racial discrimination, I follow Goncalves and Mello (2021) by modeling the estimation error in  $\hat{\beta}_j$ 

 $<sup>^{25}\</sup>text{The}$  means of the standard errors of the  $\hat{\beta}_j\text{s}$  plotted in Figure 3 are 0.24, 0.25, and 0.19, respectively.

Figure 3: Landlord-Specific Discrimination Estimates



This figure plots the distribution of landlord-specific discrimination coefficients from estimating Equation 15. The sample restricts to eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. The estimates plotted in the solid red line are controlled for landlord-by-contract rent fixed effects and estimated from a sample of cases found in landlord-by-contract rent cells with at least two cases against both racial groups. The estimates plotted in the green dashed line are controlled for landlord and contract rent fixed effects separately and restrict to landlords with at least two non-payment cases filed against both racial groups. The estimates plotted in the blue dashed line are controlled for landlord and contract rent fixed effects separately and restrict to landlords with at least two non-payment cases filed against both racial groups.

as normal with variances taken from the estimated standard errors. I then estimate the true distribution of landlord discrimination by maximum likelihood and report the shares of discriminatory landlords ( $\beta_j < 0$ ) and reverse-discriminatory landlords ( $\beta_j > 0$ ) in Table A7. I estimate that at least 49% of landlords in the sample exhibit any discrimination against minority tenants, and at least 40% of landlords exhibit substantial discrimination ( $\beta_j < -0.02$ ). Across all approaches, I find that a higher share of landlords discriminate against minority tenants than reverse-discriminate against white tenants. It is important to note that these estimated discriminatory shares reflect only the selected sample of landlords where  $\hat{\beta}_j$  is identified and are estimated with considerably more noise than is achieved by Goncalves and Mello (2021) given that many landlords file a small number of eviction cases.<sup>26</sup>

 $<sup>^{26}</sup>$ Goncalves and Mello (2021) develop this method for estimating the share of discriminatory agents for





This figure plots the discrimination coefficients by landlord type. The outcome variable is log back rent, and the coefficients are controlled for landlordby-contract rent fixed effects. The sample is restricted to Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Standard errors are clustered at the landlord level.

### 6.4 Heterogeneity

I also explore heterogeneity by landlord, tenant, rent, and neighborhood characteristics to understand which tenants are most vulnerable to discrimination in eviction filing. Figure 4 plots estimates of racial discrimination separately for various types of landlords: corporate landlords, large property managers, white and minority individual landlords, and other uncategorized landlords.<sup>27</sup> Discrimination appears to be driven by non-minority landlords,

the setting of traffic stops. Given that police officers stop a much higher number of drivers than landlords file eviction cases, their results are more precise than the results in this paper.

<sup>&</sup>lt;sup>27</sup>I classify landlords as large property managers if no plaintiff name is used in at least 50% of their filings and the landlord filed at least 25 cases. For non-property managers, I classify landlords as corporate if the most common plaintiff name contains "LLC", "LP", "LTD", "INC", or similar strings. For the remaining landlords, I impute their race using the fBISG methodology given the first name, last name, and ZCTA of the most common plaintiff name and address associated with the landlord. Based on these classifications, large property managers file 36.28% of cases, corporate landlords file 17.46% of cases, white individual landlords file 8.20% of cases, minority individual landlords file 16.03% of cases, and unclassified landlords file 22.03% of cases.

while minority landlords if anything may exhibit reverse-discrimination against white tenants.<sup>28</sup> In Section 7.2, I discuss the likely drivers of this pattern of heterogeneity. Examining individual tenant, rent, and neighborhood characteristics in Figure A2, I find no clear patterns of heterogeneous discrimination by gender, tenure in the rental property, contract rent, rent affordability, tract racial composition, or tract poverty rates.

# 7 Sources of Discrimination

## 7.1 Direct vs. Indirect Discrimination

Having found evidence of racial discrimination in eviction filing, I next turn to examine the possible sources of discrimination. If indirect discrimination alone explains total discrimination, then filing thresholds should be equal across race holding fixed non-race characteristics  $Z_i$  among tenants with the same landlord and contract rent (Equation 8). Rejecting equal thresholds conditional on  $Z_i$  is evidence of direct discrimination on the basis of tenant race.

I do not observe the full vector of non-race tenant characteristics  $Z_i$  observable to the landlord but test for direct discrimination by controlling for the subset of  $Z_i$  in the data. These characteristics include an imputed measure of gender, the number of tenants named in the filing, two proxy measures of rent burden, and census tract fixed effects. I construct rent burden as individual-level contract rent listed in the filing as a share of median household income among similar households in the American Community Survey (ACS). The first measure of rent burden uses median household income among renters in the census tract and year of the filing. The second measure uses median household income in the census tract and year of the filing among householders with the same detailed race as the imputed race of the first tenant listed on the filing.<sup>29</sup> For this test be informative, these control variables must vary with tenant race after conditioning on landlord and monthly contract rent in order for there to be scope for landlords to exhibit indirect discrimination based on these characteristics. Table A8 confirms tenant race is a highly significant predictor of each of the observable non-race characteristics of tenants, conditional on landlord and contract rent.

Table 4 provides evidence of direct discrimination. Column 1 re-states the previous estimate of total discrimination for reference, and columns 2 through 4 estimate direct dis-

 $<sup>^{28}</sup>$ I can reject that discrimination by individual minority landlords is equal to discrimination by all other landlords (p-value = 0.029).

<sup>&</sup>lt;sup>29</sup>I match median household income by tract, year, and race if the most likely race—Asian, Black, Hispanic, white, or other—of the first tenant listed on the filing is imputed with at least 80% certainty.

	Log Back Rent				
	(1)	(2)	(3)	(4)	
Minority Tenant	$-0.0490^{***}$ (0.0167)	$-0.0523^{***}$ (0.0150)	$-0.0606^{***}$ (0.0157)	$-0.0467^{***}$ (0.0169)	
$\begin{array}{c} \text{Observations} \\ \text{R}^2 \end{array}$	$120,328 \\ 0.6442$	$120,328 \\ 0.6451$	$120,328 \\ 0.6456$	$120,328 \\ 0.6567$	
Mean White Back Rent Landlord $\times$ Contract Rent FE	1,585.38 ✓	1,585.38 ✓	1,585.38 ✓	1,585.38 ✓	
Tenant Controls Rent Burden FE (Tract $\times$ Renter Inc.)		$\checkmark$	$\checkmark$	$\checkmark$	
Rent Burden FE (Tract $\times$ Race Inc.) Census Tract FE			$\checkmark$	$\checkmark$	
Bias-adj coef.				-0.0403	

Table 4: Direct vs. Indirect Discrimination

\*p < 0.1, \*\*p < 0.05, \*\*\*p < 0.01. This table reports the results from estimating Equation 14 with controls. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Column 1 restates the estimate of total discrimination from Table 2. Columns 2 through 4 introduce control variables. Tenant controls include the imputed gender of first tenant named and fixed effects for the number of tenants listed on the filing. Rent burden fixed effects are 2-percentage point bins of the ratio of individual-level contract rent to median household income among similar individuals in the ACS. The first set of rent burden fixed effects uses median household income among renters in the census tract around the year of the filing. The second set of rent burden fixed effects uses median household income by household head race and census tract around the year of the filing. Household head race in the eviction filing is defined based on the imputed race of the first tenant listed if the imputation is at least 80% likely. Median household income measures are sourced from 5-year ACS estimates, where the midpoint of the 5-year ACS period is matched to the case filing year. All control variables include a category for missing values. All specifications exclude singleton observations for the fixed effects used. Contract rent fixed effects refer to the exact dollar value of contract rent. Standard errors are clustered at the landlord level. The bias-adjusted coefficient is estimated following Oster (2019) and sets  $R_{max}^2 = R_{Col.4}^2 + (R_{Col.4}^2 - R_{Col.2}^2).$ 

crimination by including flexible controls for gender, the number of tenants, rent burden, and neighborhood characteristics. Controlling for these characteristics does not substantially change the discrimination coefficient, which is consistent with total discrimination entirely reflecting direct discrimination.

To the extent that landlords selectively file eviction cases according to other tenant characteristics not captured here, the estimates of direct discrimination in Table 4 could include omitted variable bias. In particular, I do not observe individual-level income which landlords may observe and incorporate into filing decisions if income predicts a tenant's ability to pay the contract rent going forward and thus the landlord's payoff from eviction  $(\tilde{v})$ . It is important to note, however, that landlords likely only observe at best a lagged measure of tenant income from when they initially screened the tenant for the unit.

This potential omitted variable does not appear to be a substantial issue for a number of reasons. First, the two proxy measures of rent burden and the inclusion of census tract fixed effects should account for a considerable portion of any racial disparities in rent affordability that landlords observe. In particular, the second rent burden proxy used in columns 3 and 4 directly incorporates racial disparities in income within census tract and year. If unobserved variation in tenant income was biasing the direct discrimination estimate in column 4, I would expect the inclusion of these controls to shift the direct discrimination coefficient at least somewhat toward zero relative to the total discrimination estimate in column 1. Instead, the magnitude of discrimination increases in columns 2 and 3 and is barely attenuated in column 4. Second, I implement the approach in Oster (2019) to estimate bias-adjusted coefficients for direct discrimination. Assuming that controlling for any remaining unobservables would add as much to the  $\mathbb{R}^2$  as the observables, I estimate a bias-adjusted estimate of direct discrimination of -0.0403 which falls within the 95% confidence interval of both the total and direct discrimination estimates.

Overall, the results in Table 4 suggest limited scope for indirect discrimination. Racial discrimination in eviction filing appears to mostly reflect landlords discriminating directly on the basis of tenant race. This provides stronger evidence that landlord discrimination in eviction filing is in violation of the Fair Housing Act.<sup>30</sup>

### 7.2 Accurate Statistical Discrimination vs. Racial Bias

Direct racial discrimination exhibited by landlords may reflect accurate statistical discrimination or racial bias. From Section 4.2, accurate statistical discrimination may arise if landlords account for any true dependence of the probability of winning repossession on tenant race. Alternatively, landlords may exhibit racial bias if they deviate from accurate statistical discrimination and tolerate different true payoffs from filing eviction cases by tenant race.

In practice, winning repossession appears to depend on tenant race (Figure 2), motivating the possibility of accurate statistical discrimination. The data suggest that the pattern could be driven by landlords withdrawing fewer cases against minority tenants and that minority tenants are less likely to have legal representation (Figure A3).<sup>31</sup> However, these

<sup>&</sup>lt;sup>30</sup>From Section 2.1, total racial discrimination in eviction filing estimated in Section 6 violates the Fair Housing Act under the assumption that there exists no legitimate interest for a landlord to selectively file non-payment of rent eviction cases against tenants equally behind on rent. If this assumption is violated, then only direct discrimination would necessarily equate to a violation of the Fair Housing Act.

<sup>&</sup>lt;sup>31</sup>Differential rates of court appearance does not seem to explain Figure 2 since minority tenants tend to appear in court more often than white tenants conditional on no case withdrawal.

potential explanations are suggestive as I am unable to investigate other possible channels such as differences in bargaining power during private landlord-tenant negotiations, access to information, or discrimination by judges in the small number of cases they decide.<sup>32</sup> Regardless of the explanation for Figure 2, if landlords account for the observable pattern that it seemingly requires less back rent to win repossession from minority tenants, then accurate statistical discrimination may explain total discrimination in eviction filing.

A testable implication of accurate statistical discrimination is that a landlord should win repossession of the property at the same rate against a white and minority tenant at the margin conditional on non-race characteristics and contract rent (Proposition 1). Any deviation from equal repossession rates reflects racial bias (Proposition 2). In practice, I do not observe all possible non-race characteristics that the landlord may observe and thus conduct the marginal outcome test controlling for the same observable tenant characteristics as in Section 7.1. I conduct this marginal outcome test using the following specification for filings against tenant(s) i by landlord j on filing date t:

$$Repossession_{ijt} = \theta Minority_i + \gamma_{j,y(ijt)} + X'_{ijt}\lambda + v_{ijt}$$
(16)

An estimate of  $\theta \neq 0$  is evidence of racial bias under two assumptions: marginal outcomes and no omitted payoff bias (Hull 2021; Kleinberg et al. 2018).<sup>33</sup> First, the test requires that all outcomes are marginal. This assumption is satisfied if back rent evolves continuously, ensuring that back rent owed at filing is equal to the landlord's filing threshold for tenant *i*. Second, no omitted payoff bias requires that a landlord's true payoff from winning repossession does not vary with tenant race after including landlord-by-contract rent fixed effects,  $\gamma_{j,y(ijt)}$ , and control variables,  $X_{ijt}$ , which include imputed gender, the number of tenants listed, two proxy measures of rent burden, and census tract fixed effects. The inclusion of rent burden controls and census tract fixed effects, in particular, should limit concerns about omitted payoff bias as they proxy for the likelihood of future payment by tenant *i*.

The marginal outcome test also depends on accurately measuring the outcome—the landlord winning repossession of the rental property from the tenant during the court process.<sup>34</sup>

 $<sup>^{32}</sup>$ It is important to highlight that compared to other jurisdictions where judges may play a larger role in eviction court outcomes (Collinson et al. 2024), Philadelphia eviction court is characterized by judges deciding less than 5% of eviction filings. This means that it is unlikely that the racial disparities in eviction court anticipated by landlords are driven in large part by judge behavior.

<sup>&</sup>lt;sup>33</sup>A negative (positive) estimate of  $\theta$  implies racial bias against minority (white) tenants.

<sup>&</sup>lt;sup>34</sup>It is possible that landlords may have other goals when filing an eviction case, such as filing as a threat to induce tenants to pay back rent (Garboden and Rosen 2019). A marginal outcome test that uses a landlord's future rental earnings from the rental unit as the outcome variable could account for the various aims of landlords when filing eviction cases. Given that landlords' rental earnings are unobservable, I simplify the setting and assume that landlords intend to win repossession of the unit.

My preferred measure for *Repossession* is whether the landlord filed for a writ of possession, which aligns with the definition of an eviction order used by Collinson et al. (2024) in New York City eviction cases. One drawback of this measure is that it may miss informal repossessions. For example, after winning a judgment, a landlord may not file for a writ of possession if the tenant has already vacated the property.<sup>35</sup> If unobserved repossessions vary by race, this represents another form of omitted payoff bias. To address this, I repeat the marginal outcome test using two broader measures of repossession: including JBAs in which the tenant agrees to vacate or already vacated the property, and additionally including any judgment in favor of the landlord regardless of whether a writ of possession was filed. While each of these measures of repossession has drawbacks, consistent results across these measures should support the interpretation of the outcome test.

Table 5 reports the results of the marginal outcome test. Landlords win repossession of the property at statistically indistinguishable rates across tenant race, which is consistent with accurate statistical discrimination. The results are somewhat imprecise, but I can reject that landlords tolerate more than 1.05 percentage point (2.41%) lower repossession rates from minority tenants. Since concerns about omitted payoff bias could arise if the amount of back rent a tenant owes at the time of the filing predicts future payment, I demonstrate that the finding in Table 5 is robust to using as outcomes the repossession measures scaled by the log value of back rent owed at filing (Table A9).

Evidence that landlords use lower filing thresholds for minority tenants but achieve indistinguishable repossession rates across tenant race implies that landlords accurately anticipate that winning repossession from minority tenants requires less back rent than winning repossession from white tenants. Table 6 quantifies this underlying racial disparity in repossession that landlords appear to accurately predict.<sup>36</sup> By conditioning on landlord and back rent owed at filing, these estimates can be interpreted as counterfactual repossession disparities if landlords were to exhibit no discrimination at filing. The magnitudes of the underlying racial disparities in repossession (Table 6) are consistently larger than the realized repossession disparities after landlords discriminate at filing (Table 5). This pattern further supports the finding that landlords exhibit accurate statistical discrimination: by filing eviction cases at lower thresholds of back rent for minority tenants, landlords appear to equalize the otherwise-unequal repossession rate.

Recall from Figure 4 that minority landlords appear to differ from other landlords by

<sup>&</sup>lt;sup>35</sup>In the data, a substantial number of cases with a judgment in favor of the landlord have no writ of possession filed (Figure A4), suggesting that the writ of possession measure may indeed underestimate repossession. However, some of these cases could reflect a landlord reaching a private agreement with a tenant post-judgment that allows the tenant to remain in the property.

<sup>&</sup>lt;sup>36</sup>In essence, the estimates in Table 6 more precisely quantify the disparity visualized in Figure 2.

	Writ of Possessionor Agree to Leave via JBA		or Landlord Won Judgment
	(1)	(2)	(3)
Minority Tenant	$0.0171 \\ (0.0141)$	0.0138 (0.0138)	0.0092 (0.0095)
Observations	120,328	120,328	120,328
$\mathbb{R}^2$	0.1851	0.1895	0.2151
White Outcome Mean	0.4380	0.4791	0.6478
Landlord $\times$ Contract Rent FE	$\checkmark$	$\checkmark$	$\checkmark$
Tenant Controls	$\checkmark$	$\checkmark$	$\checkmark$
Census Tract FE	$\checkmark$	$\checkmark$	$\checkmark$

 Table 5: Marginal Outcome Test

p < 0.1, p < 0.05, p < 0.05, p < 0.01. This table reports the results from estimating Equation 16. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Contract rent fixed effects refer to the exact dollar value of contract rent. Tenant controls include the imputed gender of first tenant named, fixed effects for the number of tenants in the property listed on the filing, and fixed effects for two measures of rent burden each divided into 2-percentage point bins. The first measure of rent burden is the ratio of contract rent to median household income among renters in the census tract. The second measure of rent burden is the ratio of contract rent to median household income by race and census tract. Median household income by race and tract is defined based on the imputed race of the first tenant listed if the imputation is at least 80% likely. All control variables include a category for missing values. The outcome variable in column 1 is an indicator for whether a landlord filed for a writ of possession. The outcome variable in column 2 additionally includes cases where the tenant agreed to vacate the property as part of a judgment by agreement (JBA). The outcome variable in column 3 additionally includes any case in which the landlord won a default judgment or won a contested judgment (decided by the judge) regardless of filing for a writ of possession. All specifications exclude singleton observations for the fixed effects used. Standard errors are clustered at the landlord level.

exhibiting reverse-discrimination against white tenants. This pattern of heterogeneity can be rationalized with the finding of accurate statistical discrimination if the underlying racial disparity in repossession rates varies by landlord type. For example, it is possible that minority landlords can achieve more favorable outcomes when negotiating with white tenants, while white landlords can achieve more favorable outcomes when negotiating with minority tenants. Indeed, Table A10 shows that the underlying repossession disparity has the opposite sign for minority landlords. This is consistent with all landlords exhibiting accurate statistical discrimination: most landlords tolerate less back rent from minority tenants because they accurately predict that less back rent is required for them to win against minority tenants, but minority landlords tolerate less back rent from white tenants because they accurately predict that less back rent is required for them to win against minority tenants, but minority landlords tolerate less back rent from white tenants because they accurately predict that less back rent is required for them to win against minority tenants, but minority landlords tolerate less back rent from white tenants because they accurately predict that less back rent is required for them to win against white tenants.

Taken together, the evidence suggests that landlords account for racial disparities at

	Writ of Possession		or Agree to Leave via JBA		or Landlord Won Judgment	
	(1)	(2)	(3)	(4)	(5)	(6)
Minority Tenant	$\begin{array}{c} 0.0571^{***} \\ (0.0218) \end{array}$	$\begin{array}{c} 0.0483^{***} \\ (0.0178) \end{array}$	$\begin{array}{c} 0.0626^{***} \\ (0.0205) \end{array}$	$\begin{array}{c} 0.0527^{***} \\ (0.0159) \end{array}$	$\begin{array}{c} 0.0527^{**} \\ (0.0211) \end{array}$	$\begin{array}{c} 0.0421^{***} \\ (0.0163) \end{array}$
Observations R <sup>2</sup> White Outcome Mean Landlord × Back Bent (\$) FE	116,370 0.1291 0.4179	$\begin{array}{c} 127,355\\ 0.1232\\ 0.4216\end{array}$	116,370 0.1370 0.4527	$\begin{array}{c} 127,355 \\ 0.1309 \\ 0.4601 \end{array}$	116,370 0.1598 0.6254	$\begin{array}{c} 127,355\\ 0.1533\\ 0.6349\end{array}$
Landlord $\times$ Back Rent ( $\oplus$ ) FE	·	$\checkmark$	·	$\checkmark$	·	$\checkmark$

Table 6: Underlying Racial Disparities in Repossession

p < 0.1, p < 0.05, p < 0.05, p < 0.01. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Back rent fixed effects in dollars are bins defined at \$50 increments. Back rent fixed effects in months owed are bins of back rent divided by contract rent defined at increments of 0.25 months. The outcome variable in columns 1 and 2 is an indicator for whether a landlord filed for a writ of possession. The outcome variable in columns 3 and 4 additionally includes cases where the tenant agreed to vacate the property as part of a judgment by agreement (JBA). The outcome variable in columns 5 and 6 additionally includes any case in which the landlord won a default judgment or won a contested judgment (decided by the judge) regardless of filing for a writ of possession. All specifications exclude singleton landlord-by-back rent observations. Standard errors are clustered at the landlord level.

later stages of the eviction process when deciding to file a case. This implies that policy responses to eliminate the racial disparities in eviction court that affect the repossession rate should reduce discrimination by landlords at the margin of eviction filing. Further upstream, however, landlords could also change how they select tenants for rental units in response to reduced disparities in eviction court. With the data analyzed in this paper, I am unable to investigate this potential margin of adjustment, meaning that welfare implications of eliminating racial disparities in eviction court are unclear.

# 8 Conclusion

In this paper, I develop novel methods for measuring discrimination in dynamic settings and find evidence of racial discrimination when landlords file eviction cases. On average, landlords tolerate 4.9% less back rent from minority tenants compared to white tenants with the same contract rent. At least 49% of landlords in my sample exhibit racial discrimination. This discrimination appears to be fully driven by direct discrimination on the basis of race and not other characteristics, which potentially violates the Fair Housing Act.

Evidence of racial discrimination at the margin of housing loss reveals that minorities face significant discrimination in the housing market even after entry into housing. Racial discrimination in this context is particularly troubling given the severe consequences of eviction. Regardless of the case outcome, eviction filings generate a public record that is visible on tenant screening reports for seven years. Anecdotal evidence suggests this public record restricts future access to quality housing (Goldstein 2021; Lake and Tupper 2021). Higher exposure to eviction filings may disproportionately expose minority tenants to the adverse effects of housing displacement (Collinson et al. 2024). Landlords tolerating less back rent from minority tenants equates to less informal credit provided by landlords, amplifying known racial disparities in formal credit access (Cohen-Cole 2011; Weller 2009).

My results suggest that racial discrimination in eviction filing is explained by landlords exhibiting accurate statistical discrimination. Landlords observe that winning repossession from minority tenants requires less back rent. By filing eviction cases at lower thresholds against minority tenants, landlords equalize the repossession rate. This finding implies that policy responses to reduce racial disparities in eviction court may also reduce racial discrimination at the earlier stage of eviction filing. Right-to-counsel laws for tenants in eviction court and eviction diversion programs could theoretically impact the scope for statistical discrimination by reducing racial gaps in the repossession rate. Other policies that directly regulate when landlords can file eviction cases—for example, "good cause" laws or back rent floors—may also reduce discrimination in eviction filing. However, these types of policy interventions that make evicting minority tenants more difficult could also induce landlords to adopt stricter tenant screening than could negatively impact minority renters (Arefeva et al. 2024). Evaluating the welfare impacts of recent eviction policies that include any changes to discrimination in eviction filing remains an important avenue for future work.

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# Appendix A Supplemental Figures and Tables

## A.1 Supplemental Figures



Figure A1: Months of Back Rent Owed at Filing

This figure plots the months of back rent distribution of Philadelphia eviction filings separately for filings against white and minority tenants. Months of back rent is equal to dollars of back rent divided by monthly contract rent. The sample in both subfigures restricts to Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Subfigure (b) further restricts to tenants with contracts rents below the sample median in the year of the eviction filing. When plotting the distributions, months owed are top-coded at 12 months.





This figure plots the discrimination coefficients estimated from various heterogeneous versions of Equation 14. Subfigure (a) estimates discrimination by the imputed gender of the first tenant named in the filing based on the tenant's first name. Coefficients are controlled for landlord-by-contract rent fixed effects interacted with gender. Subfigure (b) estimates discrimination by the years the tenant has lived in the rental property prior to the eviction filing. Coefficients are controlled for landlord-by-contract rent fixed effects interacted with the tenure bins. Subfigure (c) estimates discrimination by \$200 contract rent bins. Coefficients are controlled for landlord-by-contract rent fixed effects. Subfigure (d) estimates discrimination by the ratio of contract rent to median household income among renters in the census tract. Median household income is sourced from 5-year ACS estimates, where the midpoint of the 5-year ACS period is matched to the case filing year. Coefficients are controlled for landlord-by-contract rent fixed effects interacted with the rent burden bins. Subfigure (e) estimates discrimination by the minority population share of the census tract. Coefficients are controlled for landlord-by-contract rent fixed effects interacted with the minority share bins. Subfigure (f) estimates discrimination by the poverty rate among renters in the census tract. Coefficients are controlled for landlord-by-contract rent fixed effects interacted with the poverty rate bins. In all subfigures, the outcome variable is log back rent and the sample is restricted to Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Standard errors are clustered at the landlord level.



Figure A3: Sources of the Underlying Racial Disparity in Repossession

## (a) Case Withdrawn

(b) Appear | No Withdrawal

This figure plots bin scatters of the relationship between back rent and various measures of progress through the eviction court process. Each point reflects 500 eviction cases against tenants belonging to a given racial group. The x-axis in each subfigure is the median back rent owed among cases in each cell. Subfigure (a) uses a sample of Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. The y-axis in subfigure (a) is the rate of landlords withdrawing cases prior to the scheduled hearing date. In subfigure (b), the sample is further restricted to non-withdrawn cases, and the y-axis is the rate at which tenants appear in court when scheduled. In subfigure (c), the sample is further restricted to cases in which the tenant appeared in court, and the y-axis is the rate at which tenants had legal representation.



Figure A4: Philadelphia Eviction Case Outcomes

This figure plots the frequency of Philadelphia eviction case outcomes. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Judgment for tenant includes contested cases where the judge ruled in favor of the tenant and default judgments in favor of the tenant after the landlord did not appear in court. Cases with unclear outcomes have missing outcome variables or multiple occurrences of judgments or withdrawals.

# A.2 Supplemental Tables

	All causes		Non-paymen	t of rent only
	White	Minority	White	Minority
Claim Details				
Total Claim Amount	$2,\!698.87$	2,058.15	2,832.26	2,029.01
	(3,287.23)	(12, 025.93)	(3, 308.67)	(3, 434.49)
Back Rent	$2,\!122.17$	$1,\!615.41$	$2,\!295.13$	$1,\!639.91$
	(2,914.62)	(11, 229.63)	(3,012.16)	(3, 237.58)
Late Fees	89.27	69.21	97.60	74.62
	(242.96)	(160.33)	(212.98)	(149.97)
Physical Damages	3.29	1.97	0.78	0.76
	(93.86)	(99.79)	(47.40)	(72.63)
Attorney Fees	277.26	209.56	292.34	202.39
	(357.46)	(267.89)	(396.13)	(250.76)
Other Fees	154.88	123.45	145.60	110.67
	(757.63)	(756.14)	(667.11)	(785.49)
Other Characteristics				
Contract Rent	887.04	774.99	953.49	811.51
	(563.18)	(28, 316.92)	(573.79)	(30, 635.91)
N	23,704	238,034	14,511	146,655

Table A1: Descriptive Statistics

This table reports the means and standard deviations of variables in the analysis sample of Philadelphia eviction cases. The analysis sample includes eviction filings that could be classified as white or minority with at least 80% certainty based on tenant race imputations and have non-missing claim amounts and back rent. The total claim amount is the sum of back rent, late fees, physical damages, attorney fees, and other fees that the landlord claims the tenant owes at the time of the case filing. Contract rent is the amount of rent owed monthly.

	Log Back Rent	$egin{array}{llllllllllllllllllllllllllllllllllll$	$egin{array}{llllllllllllllllllllllllllllllllllll$
	(1)	(2)	(3)
Minority Tenant	-0.0494**	-0.0467**	-0.0467***
	(0.0227)	(0.0217)	(0.0164)
Observations	85,810	85,847	120,349
$\mathbb{R}^2$	0.6604	0.6637	0.6477
Mean White Outcome \$ Amount	1,534.75	$1,\!610.13$	$1,\!657.51$
Landlord $\times$ Contract Rent FE	$\checkmark$	$\checkmark$	$\checkmark$
Damages = 0	$\checkmark$	$\checkmark$	$\checkmark$
Other Fees $= 0$	$\checkmark$	$\checkmark$	

Table A2: Robustness Checks for Omitted Lease Violations

p < 0.1, p < 0.05, p < 0.01. This table reports the results from estimating Equation 14 using alternative definitions of cases caused only by non-payment of rent and incorporating other components of the claim amount. The sample in all columns includes Philadelphia eviction cases filed only due to non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations and had \$0 of physical damages claimed. Columns 1 and 2 additionally restrict to cases with \$0 of other fees claimed in the filing. All specifications exclude singleton landlord-by-contract rent observations. Contract rent fixed effects refer to the exact dollar value of contract rent. Standard errors are clustered at the landlord level.

	Log Back Rent		
	(1)	(2)	(3)
Minority Tenant	-0.0486** (0.0237)	$-0.0570^{**}$ (0.0260)	$-0.0574^{**}$ (0.0254)
$\begin{array}{c} \text{Observations} \\ \text{R}^2 \end{array}$	$75,759 \\ 0.6550$	$69,047 \\ 0.6652$	$62,202 \\ 0.6779$
Mean White Back Rent Landlord $\times$ Contract Rent (\$25 bins) $\times$ Tenure FE	1,567.96	1,538.60 ✓	1,504.09 ✓
Tenure Bin Size	12  months	6 months	3  months

Table A3: Tenure Robustness Checks

p < 0.1, p < 0.05, p < 0.05, p < 0.01. This table reports the results from estimating Equation 14 with landlordby-contract rent-by-tenure fixed effects The sample includes Philadelphia eviction filings caused only by nonpayment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Contract rent fixed effects are \$25 bins of contract rent. Tenure fixed effects are 12 month, 6 month, or 3 month bins of tenure in the rental property, which is defined as the difference between the lease start date and the eviction filing date. All specifications exclude singleton landlord-by-contract rent-by tenure observations. Standard errors are clustered at the landlord level.

	Log Back Rent				
	$\leq 1$ Days Notice $\leq 5$ Days Notice		All Cases		
	(1)	(2)	(3)		
Minority Tenant	$-0.0340^{**}$ (0.0150)	$-0.0234^{**}$ (0.0114)	$-0.0465^{***}$ (0.0133)		
Observations $R^2$ Mean White Back Rent Landlord × Contract Rent (\$25 bins) FE Days of Notice FE	27,147 0.4088 1,559.50 $\checkmark$	47,114 0.4147 1,544.20 $\checkmark$	130,319 0.6260 1,595.58 $\checkmark$		

### Table A4: Notice Period Robustness Checks

\*p< 0.1, \*\*p< 0.05, \*\*\*p< 0.01. This table reports the results from estimating Equation 14 in subsets of the data with little notice and adding controls for notice periods. The baseline sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Column 1 restricts the sample to eviction filings with at most 1 day of notice prior to the eviction filing date. Column 2 restricts the sample to eviction filings with at most 5 days of notice prior to the eviction filing date. Column 3 does not restrict the sample and includes fixed effects for the number of days of notice. All specifications exclude singleton landlord-by-contract rent observations. Days of notice is defined as the number of days between the notice date and the filing date, and filings with no notice are coded as 0 days of notice. Contract rent fixed effects are \$25 bins of contract rent. Standard errors are clustered at the landlord level.

	Log Back Rent			
	(1)	(2)	(3)	
Minority Tenant	$-0.0278^{**}$ (0.0132)	$-0.0327^{***}$ (0.0109)	$-0.0285^{*}$ (0.0150)	
Observations R <sup>2</sup>	107,663 0.6861	96,749 0.7213	94,021 0.7034	
Mean White Back Rent Alternate Landlord $\times$ Contract Rent FE Plaintiff Address $\times$ Contract Rent FE	1,541.91 ✓	1,523.76 ✓	1,528.62	
Plaintiff Name $\times$ Contract Rent FE			$\checkmark$	

Table A5: Robustness Checks for Alternative Landlord Identifiers

\*p< 0.1, \*\*p< 0.05, \*\*\*p< 0.01. This table reports the results from estimating Equation 14 using samples of Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Alternative landlord fixed effects used in column 1 separate the original landlord identifiers into sub-groups based on plaintiff address strings in cases where the original landlord identifier filed at least 25 cases and the most common plaintiff address was used in less than 50% of the filings. For cases in which plaintiff addresses appear to contain attorney addresses, the alternative landlord sub-groups in column 1 and the plaintiff address fixed effects in column 2 correspond to the plaintiff names rather than plaintiff addresses. All specifications exclude singleton landlord-by-contract rent observations for the relevant landlord definition. Contract rent fixed effects refer to the exact dollar value of contract rent. Standard errors are clustered at the landlord level.

	Log Back Rent				
	(1)	(2)	(3)	(4)	
Minority Tenant	$\begin{array}{c} -0.0461^{***} \\ (0.0133) \end{array}$	$\begin{array}{c} -0.0477^{***} \\ (0.0134) \end{array}$	$\begin{array}{c} -0.0473^{***} \\ (0.0135) \end{array}$	$-0.0497^{***}$ (0.0141)	
Observations	146,632	138,291	134,142	130,432	
$\mathbb{R}^2$	0.5991	0.6019	0.6082	0.6142	
Mean White Back Rent	$1,\!651.47$	$1,\!614.54$	$1,\!601.78$	$1,\!596.07$	
Landlord FE	$\checkmark$				
Contract Rent FE	$\checkmark$				
Landlord $\times$ Contract Rent (\$100 bins) FE		$\checkmark$			
Landlord $\times$ Contract Rent (\$50 bins) FE			$\checkmark$		
Landlord $\times$ Contract Rent (\$25 bins) FE				$\checkmark$	

Table A6: Discrimination Estimates Using Coarser Fixed Effects

p < 0.1, p < 0.05, p < 0.05, p < 0.01. This table reports the results from estimating Equation 14 with coarser fixed effects. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. All specifications exclude singleton observations for the fixed effects used. Contract rent fixed effects used in column 1 refer to the exact dollar value of contract rent. Contract rent fixed effects used in columns 2 through 4 use \$100, \$50, and \$25 dollar bins. Standard errors are clustered at the landlord level.

	Method 1	Method 2	Method 3
Any discrimination	n		
Share $\beta_j < 0$	0.8864	0.7300	0.4869
·	(0.0963)	(0.1001)	(0.0420)
Share $\beta_j > 0$	0.1136	0.2700	0.3833
	(0.0963)	(0.0961)	(0.0350)
Substantial discrimination			
Share $\beta_i < -0.02$	0.4038	0.5040	0.4319
U	(0.2356)	(0.1543)	(0.0379)
Share $\beta_j > 0.02$	0.1136	0.2700	0.3667
	(0.0873)	(0.0479)	(0.0223)
Median $\hat{\beta}_i$	-0.0683	-0.0205	-0.0267
Ν	127	520	1274
FE Type	Landlord $\times$	Landlord $+$	Landlord $+$
	Contract Rent	Contract Rent	Contract Rent
Sample Restriction	$N_{jyr} \ge 2$	$N_{jr} \ge 2$	$N_{jr} \ge 1$

Table A7: Share of Discriminatory Landlords

This table reports the shares of discriminatory landlords ( $\beta_j < 0$ ) and reverse-discriminatory landlords ( $\beta_j > 0$ ). Method 1 uses  $\hat{\beta}_j$ s controlled for landlord-by-contract rent fixed effects and estimated from sample of cases in landlord-by-contract rent cells with at least 2 nonpayment filings against white tenants and at least 2 non-payment filings against minority tenants. Method 2 uses  $\hat{\beta}_j$ s controlled for landlord and contract rent fixed effects separately and estimated from a sample of cases by landlords who file at least 2 non-payment filings against white tenants and at least 2 non-payment filings against minority tenants. Method 3 uses  $\hat{\beta}_j$ s controlled for landlord and contract rent fixed effects separately and estimated from a sample of cases by landlords who file at least 1 non-payment filing against a white tenants and at least 1 non-payment filing against a minority tenants. Bootstrapped standard errors are reported in parentheses.

	Female	Tenants	Rent Burden (Tract-Renter Inc)	Rent Burden (Tract-Race Inc)
-	(1)	(2)	(3)	(4)
Minority Tenant	$\begin{array}{c} 0.0970^{***} \\ (0.0110) \end{array}$	$\begin{array}{c} 0.1523^{***} \\ (0.0155) \end{array}$	$\begin{array}{c} 0.0724^{***} \\ (0.0146) \end{array}$	$0.0770^{***}$ (0.0198)
$\begin{array}{c} \text{Observations} \\ \text{R}^2 \end{array}$	$131,705 \\ 0.1839$	$121,322 \\ 0.3686$	$110,856 \\ 0.6334$	$96,370 \\ 0.6271$
Mean White Outcome Landlord $\times$ Contract Rent FE	0.4776 ✓	$\begin{array}{c} 1.2573 \\ \checkmark \end{array}$	0.3283 ✓	0.2404 <hr/>

Table A8: Relationship Between Tenant Race and Non-Race Characteristics

\*p<0.1, \*\*p<0.05, \*\*\*p<0.01. This table reports the results from regressing observable non-race characteristics of tenants on tenant race conditional on landlord and monthly contract rent. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. The female outcome used in column 1 is based on the imputed gender of the first tenant named on the filing. The tenants outcome used in column 2 refers to the number of tenants listed on the filing. The rent burden outcome used in column 3 is the ratio of individual-level contract rent to median household income among renters in the census tract around the year of the filing. The rent burden outcome used in column 4 is the ratio of individual-level contract rent to median household by household head race and census tract around the year of the filing. Household head race of the first tenant listed if the imputation is at least 80% likely. Median household income measures are sourced from 5-year ACS estimates, where the midpoint of the 5-year ACS period is matched to the case filing year. Both rent burden measures are top-coded at 1.5. All specifications exclude singleton landlord-by-contract rent observations. Contract rent fixed effects refer to the exact dollar value of contract rent. Standard errors are clustered at the landlord level.

	Writ of Possession	or Agree to Leave via JBA	or Landlord Won Judgment	
	(1)	(2)	(3)	
Minority Tenant	$0.1122 \\ (0.1083)$	$0.0809 \\ (0.1062)$	$0.0478 \\ (0.0746)$	
Observations	120,328	120,328	120,328	
$\mathbb{R}^2$	0.2122	0.2212	0.2688	
White Outcome Mean	3.2576	3.5652	4.8041	
Landlord $\times$ Contract Rent FE	$\checkmark$	$\checkmark$	$\checkmark$	
Tenant Controls	$\checkmark$	$\checkmark$	$\checkmark$	
Census Tract FE	$\checkmark$	$\checkmark$	$\checkmark$	

Table A9: Marginal Outcome Test with Scaled Outcomes

p < 0.1, p < 0.05, p < 0.01. This table reports the results from estimating Equation 16 using scaled outcome measures of repossession. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Contract rent fixed effects refer to the exact dollar value of contract rent. Tenant controls include the imputed gender of first tenant named, fixed effects for the number of tenants in the property listed on the filing, and fixed effects for two measures of rent burden each divided into 2-percentage point bins. The first measure of rent burden is the ratio of contract rent to median household income among renters in the census tract. The second measure of rent burden is the ratio of contract rent to median household income by race and census tract. Median household income by race and tract is defined based on the imputed race of the first tenant listed if the imputation is at least 80%likely. All control variables include a category for missing values. The outcome variables are various measures of repossession scaled by the log of the amount of back rent owed at the time of the filing. The measure of repossession in columns 1 is an indicator for whether a landlord filed for a writ of possession. The measure of repossession in column 2 additionally includes cases where the tenant agreed to vacate the property as part of a judgment by agreement (JBA). The measure of repossession in columns 3 additionally includes any case in which the landlord won a default judgment or won a contested judgment (decided by the judge) regardless of filing for a writ of possession. All specifications exclude singleton observations for the fixed effects used. Standard errors are clustered at the landlord level.

	Writ of Possession		or Agree to Leave via JBA		or Landlord Won Judgment	
	(1)	(2)	(3)	(4)	(5)	(6)
Minority Tenant $\times$ Minority Landlord	-0.1832**	-0.1144**	-0.2364***	-0.1125**	$-0.1287^{*}$	-0.1034**
	(0.0803)	(0.0546)	(0.0806)	(0.0532)	(0.0684)	(0.0444)
Minority Tenant $\times$ Non-Minority Landlord	$0.0585^{***}$	$0.0505^{***}$	$0.0643^{***}$	$0.0549^{***}$	$0.0538^{**}$	$0.0440^{***}$
	(0.0216)	(0.0176)	(0.0202)	(0.0157)	(0.0210)	(0.0161)
Observations	116,370	$127,\!355$	116,370	127,355	116,370	$127,\!355$
$\mathbb{R}^2$	0.1291	0.1232	0.1371	0.1309	0.1598	0.1533
White Outcome Mean	0.4179	0.4216	0.4527	0.4601	0.6254	0.6349
Landlord $\times$ Back Rent (\$) FE	$\checkmark$		$\checkmark$		$\checkmark$	
Landlord $\times$ Back Rent (Mths) FE		$\checkmark$		$\checkmark$		$\checkmark$

Table A10: Underlying Racial Disparities in Repossession by Landlord Type

p < 0.1, p < 0.05, p < 0.05, p < 0.01. The sample includes Philadelphia eviction filings caused only by non-payment of rent that could be classified as white or minority with at least 80% certainty based on tenant race imputations. Back rent fixed effects in dollars are bins defined at \$50 increments. Back rent fixed effects in months owed are bins of back rent divided by contract rent defined at increments of 0.25 months. The outcome variable in columns 1 and 2 is an indicator for whether a landlord filed for a writ of possession. The outcome variable in columns 3 and 4 additionally includes cases where the tenant agreed to vacate the property as part of a judgment by agreement (JBA). The outcome variable in columns 5 and 6 additionally includes any case in which the landlord won a default judgment or won a contested judgment (decided by the judge) regardless of filing for a writ of possession. All specifications exclude singleton landlord-by-back rent observations. Standard errors are clustered at the landlord level.

# Appendix B Data Construction

## B.1 Landlord Linking Methodology

I construct landlord identifiers using plaintiff names and plaintiff addresses listed in Philadelphia eviction records. This method is intended to account for inconsistent spellings and the fact that many corporate landlords use property-specific LLCs. I implement the following procedure to generate unique landlord identifiers. For a given landlord name, I observe all landlord address variations associated with that name. For each of those address variations, I then observe all landlord name variations associated with that address. I repeat this process until no additional landlord name or address variations can be found. Then, I assign this linked set of landlord aliases a unique landlord identifier. I repeat this process for the entire sample of Philadelphia eviction cases.<sup>37</sup>

In 7.6% of cases, however, the plaintiff address field appears to contain the address of the plaintiff's attorney instead of the landlord's address. Because of this, the linking algorithm may mistakenly group filings with shared attorneys under a single landlord. I prevent this by manually identifying the law firm addresses of all attorneys that represent the plaintiff in at least 50 filings and do not input any plaintiff addresses matching these attorney addresses into the linking algorithm. All filings with these attorney addresses are assigned landlord identifiers based on linkages derived from the plaintiff names alone. In the end, from 65,438 unique landlord names and 52,389 unique landlord addresses, I generate 38,347 unique landlord identifiers.

## **B.2** Race Imputation Methodology

I impute tenant race based on tenants' first names, last names, and census blocks of rental properties using the fBISG methodology outlined in Imai et al. (2022). Incorporating first names and granular geolocation measures greatly improves the accuracy of race imputations compared using last names alone (Voicu 2018; Imai and Khanna 2016).

The standard BISG approach estimates the conditional probability that an individual i belongs to a racial group given the individual's name and geolocation. However, this approach can have substantial measurement error problems when using granular geolocation measures. Since the prior distribution  $Pr(Race_i|Geolocation_i)$  comes from decennial census counts, some census blocks may have zero counts of individuals in certain racial groups which imposes a 0% probability of belonging to a racial group even if a name is distinctly associated

 $<sup>^{37}</sup>$ I construct landlord identifiers prior to dropping non-residential eviction filings in order to maximize linkages between plaintiff name and address aliases.

with that racial group. The prior distribution is also decreasing in accuracy in time since the decennial census. Both of these census data problems are more significant for rapidly growing minority groups.

I utilize the improved fBISG method from Imai et al. (2022) which allows for gaining the accuracy from granular geolocation information while addressing these census data problems. This method uses a measurement error model for the observed census counts to ensure that zero counts do not necessarily imply 0% probability of belonging to that racial group. I use an fBISG model based on the tenant's first name, last name, and census block to generate the conditional posterior probability that tenant *i* belongs to each potential racial group (White, Black, Hispanic, Asian, other). These predictions incorporate 2010 decennial census counts by race and census block, the 2010 census surname list which reports the racial distribution of surnames appearing at least 100 times, and the racial distribution of first names from voter registration data compiled by Imai et al. (2022). See Imai et al. (2022) for more detail on the fBISG race imputation methodology.

# Appendix C Proof of Marginal Outcome Test

## C.1 Proof of Proposition 1

An eviction filing against tenant *i* at the margin  $(X_i(t) = \tau_i)$  satisfies:

$$\left(\tilde{p}(\tau_i, Y_i^0, Z_i, R_i) + b^p(R_i)\right) \left(\tilde{v}(Y_i^0, Z_i) + b^v(R_i)\right) = c$$
(17)

Since  $\tilde{p}(x, y, z, r) = \mathbb{E}[Y_i^* | X_i(t) = x, Y_i^0 = y, Z_i = z, R_i = r]$ , this is equivalent to:

$$\mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = r] = \frac{c}{\tilde{v}(y, z) + b^v(r)} - b^p(r)$$
(18)

For  $r \in \{white, minority\}$ , it thus follows from the law of iterated expectations that:

$$\mathbb{E}[Y_{i}^{*}|X_{i}(t) = \tau_{i}, Y_{i}^{0} = y, Z_{i} = z, R_{i} = r]$$

$$=\mathbb{E}\left[Y_{i}^{*}\Big|\mathbb{E}[Y_{i}^{*}|X_{i}(t) = \tau_{i}, Y_{i}^{0} = y, Z_{i} = z, R_{i} = r] = \frac{c}{\tilde{v}(y, z) + b^{v}(r)} - b^{p}(r), Y_{i}^{0} = y, Z_{i} = z, R_{i} = r\right]$$

$$=\mathbb{E}\left[\mathbb{E}[Y_{i}^{*}|X_{i}(t) = \tau_{i}, Y_{i}^{0} = y, Z_{i} = z, R_{i} = r]\right]$$

$$\mathbb{E}[Y_{i}^{*}|X_{i}(t) = \tau_{i}, Y_{i}^{0} = y, Z_{i} = z, R_{i} = r] = \frac{c}{\tilde{v}(y, z) + b^{v}(r)} - b^{p}(r), Y_{i}^{0} = y, Z_{i} = z, R_{i} = r\right]$$

$$=\frac{c}{\tilde{v}(y, z) + b^{v}(r)} - b^{p}(r)$$
(19)

Under no racial bias  $(b^v(white) = b^v(minority) \text{ and } b^p(white) = b^p(minority))$ :

$$\mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = minority] = \mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = white]$$
(20)

## C.2 Proof of Proposition 2

This proof is by contradiction. Suppose a landlord exhibits no racial bias (accurate statistical discrimination). Then,  $b^v(white) = b^v(minority) = b^v$  and  $b^p(white) = b^p(minority) = b^p$ . It follows from Equation 19 by the law of iterated expectations that:

$$\mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = r] = \frac{c}{\tilde{v}(y, z) + b^v} - b^p$$
(21)

$$\mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = minority] - \mathbb{E}[Y_i^*|X_i(t) = \tau_i, Y_i^0 = y, Z_i = z, R_i = white]$$
  
=  $\frac{c}{\tilde{v}(y, z) + b^v} - b^p - \frac{c}{\tilde{v}(y, z) + b^v} - b^p$   
= 0

# Appendix D Bias from Discrete Back Rent

For my test for racial discrimination to be valid, I assume that the amount by which back rent may exceed the threshold at the time of filing is the same for white and minority tenants who have the same landlord and contract rent. If this assumption is violated, my estimate of discrimination includes bias from differential discrete accrual of back rent. Considering all tenants with the same landlord, this bias term for contract rent y is:

$$\mathbb{E}[X_i(t^*) - \tau_i | Y_i^0 = y, R_i = minority] - \mathbb{E}[X_i(t^*) - \tau_i | Y_i^0 = y, R_i = white]$$
(23)

Define the last discrete change in back rent prior to the eviction filing as:

$$\Delta_i = X_i(t^*) - X_i(t^* - 1) \tag{24}$$

Note that  $X_i(t^* - 1)$  must be less than the threshold  $\tau_i$ , implying that  $X_i(t^*) - \tau_i \in [0, \Delta_i)$ . Assume for simplicity that the tenant distribution of  $X_i(t^*) - \tau_i$  has uniform density between 0 and  $\Delta_i$ .

Suppose that for some share of tenants, back rent accrues at discrete increments equal to contract rent ( $\Delta_i = Y_i^0$ ). For the remaining tenants, assume the most extreme alternative where back rent accrues continuously ( $\Delta_i = 0$ ). Let  $\alpha^m$  be the share of minority tenants with  $\Delta_i = Y_i^0$  and  $\alpha^w$  be the share of white tenants with  $\Delta_i = Y_i^0$ . Then, the bias term for contract rent y is equal to:

$$(\alpha^m - \alpha^w)\frac{y}{2} \tag{25}$$

I next turn to the data to investigate whether it is a valid concern that any racial differences in the last discrete change in back rent prior to the filing could explain my estimate of discrimination. Given back rent owed at the time of the filing and contract rent, I observe whether tenants owe integer or non-integer months of back rent at the time of an eviction filing. It is a reasonable assumption that tenants owing integer months of back rent at the time of the filing have  $\Delta_i = Y_i^0$ . For the remaining tenants in the data that owe non-integer months of back rent at the time of the case filing, it is likely that  $\Delta_i < Y_i^0$  and in the most extreme case,  $\Delta_i = 0$ .

My discrimination estimate is -0.1336y (Table 2). For bias from differential discrete accrual of back rent to fully explain this result, it must be the case that  $\alpha^m - \alpha^w = -0.2672$ . In other words, among tenants with the same landlord and contract rent, the share of minority tenants owing integer months of back rent must be 26.72 percentage points lower than the share of white tenants owing integer months of back rent. In the data, minority tenants are only 2.32 percentage points less likely to owe integer months of back rent than white tenants with the same landlord and contract rent. This could only generate bias equal to -0.0116y, which is 8.68% of my true discrimination estimate. As such, any bias from differential discrete accrual of back rent cannot come close to fully explaining my estimate of total discrimination.