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ENFORCEMENT RISK AND SCREENING IN RENTAL MARKETS: EVIDENCE FROM EVICTION MORATORIA

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ABSTRACT

We combine a hand-collected dataset on state eviction moratoria with a nationwide field experiment of more than 25,000 rental inquiries to study how contract enforcement affects screening in rental housing markets. Using staggered moratorium expirations, we show that property managers discriminated more against minority renters when eviction enforcement was suspended. Linking inquiries to tenant address histories, we find that non-responses translated into different move-in patterns and reduced property owners' returns through foregone leasing opportunities. A simple search model rationalizes these responses as property managers re-optimize when enforcement is suspended.

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1 Introduction

How does contract enforceability shape how intermediaries screen counterparties? Screening determines the risk and performance of financial assets, yet isolating the causal effect of enforcement on screening is difficult because changes in enforceability typically coincide with shifts in economic conditions or institutional environments. As a result, there is limited direct evidence on how enforcement risk affects intermediary behavior in market settings.

We address this challenge in rental housing, where eviction governs the enforcement of lease contracts and tenant selection is delegated to property managers acting on behalf of owners. We exploit the staggered expiration of state eviction moratoria in 2020 as a sharp and plausibly exogenous change in the credibility of enforcement. When eviction is not credibly enforceable, managers screen more selectively and more discriminatorily. We show that relative response rates for minority applicants, especially African American applicants, increase after moratoria end, implying that discrimination was more severe while enforcement was suspended. These results are consistent with two forces. First, weakened enforcement increases the expected cost of a bad match and therefore raises the value of screening along standard risk dimensions. Second, the same mechanism can amplify discriminatory screening, because the cost of an undesirable tenant match increases when eviction becomes more difficult. Both channels predict more selective screening, but they differ in their implications for how screening varies across markets.

Our empirical approach combines three elements that, to our knowledge, have not previously been brought together in this setting. First, we construct a novel, hand-collected dataset that traces each state eviction moratorium to its legal source and codes its effective start and end dates, providing precise variation in enforcement credibility. Second, we use the largest nationwide correspondence experiment in rental housing, comprising more than 25,000 inquiries (Christensen et al., 2021), which allows us to observe screening decisions directly. Third, we link rental listings to residential address history data, enabling us to connect inquiry-stage responses to realized tenant move-in patterns. This combination allows us to identify how an exogenous enforcement shock affects screening behavior and to trace those effects through to realized market outcomes.

Our central finding is that weakened enforcement leads to more selective and more discriminatory screening. Using staggered moratorium expirations, we show that relative response rates for African American applicants increase after moratoria end, implying that discrimination was more severe when eviction was not enforceable. The effects are robust across event windows, alternative control groups, alternative staggered difference-in-differences estimators, and bootstrap inference.

These screening distortions have implications for both allocation and returns. Linking the experimental listings to tenant address histories, we show that non-response during moratoria translates into systematically different realized move-in patterns. Units are less likely to be occupied by tenants whose race matches that of the fictitious minority inquirer when managers fail to respond during the moratorium period, and this gap narrows once enforcement is restored. We further show, using a calibration based on the observed response-rate effects, that the implied losses exceed \$100,000 per property manager in foregone rental income and asset value. Interpreting this calculation requires an assumption about the mapping from inquiries to leases, but it suggests that discriminatory screening can leave money on the table, consistent with a role for taste-based discrimination.

To interpret these findings, we develop a dynamic search model in which a property manager chooses whether to respond to inquiries when tenant default is possible and eviction may be temporarily suspended. Eviction moratoria increase the expected cost of a bad tenant match and therefore raise the value of screening. The model allows for both statistical discrimination, through group-specific beliefs about default risk, and taste-based discrimination, through group-specific utility costs. Both channels predict more selective screening when enforcement is weakened, but they generate different cross-sectional implications.

The data are more consistent with taste-based than with statistical discrimination, although both channels may be operative. We do not find larger treatment effects in higher-rent markets, where a statistical discrimination channel based on loss given default would predict stronger responses. By contrast, the increase in discriminatory screening is stronger in locations with higher measured racial animus (Stephens-Davidowitz, 2013) and in states with stronger baseline tenant protections (Coulson et al., 2025). Interpreting these patterns requires an assumption that group-level default risk does not vary systematically with rent levels in a way that offsets differences in loss given default. We therefore view these results as suggestive rather than definitive evidence on the relative importance of the two mechanisms.

A key identification concern is that states may have ended moratoria in response to changing public health or economic conditions. We address this concern in several ways.

First, event study estimates show no evidence of differential pre-trends. Second, moratorium expiration does not predict discontinuous changes in contemporaneous COVID-19 case dynamics. Third, pre-determined state characteristics, including demographic composition and political affiliation, do not systematically predict expiration timing. The results are also robust to matching on COVID-19 case rates, alternative control group definitions, and sample splits around major national events. We further show that the findings are not driven by federal eviction protections under the CARES Act, consistent with our emphasis on the credibility of enforcement rather than the formal existence of statutory protections (Benfer et al., 2023, 2021a; Hatamyar and Parmeter, 2023).

Our paper contributes to several strands of finance and economics. First, we contribute to a literature on contract enforcement and intermediary incentives. Closest to our setting are studies showing how limited enforceability shapes screening and monitoring decisions when agents act on behalf of principals (Lerner and Schoar, 2005; Arellano et al., 2012; Quintin, 2008). We bring this logic to rental housing, where lease enforcement operates through eviction and screening is delegated to property managers acting on behalf of owners. Relative to this literature, our setting provides a plausibly exogenous shock to enforceability, a direct measure of screening behavior at the micro level, and evidence on how screening distortions translate into realized outcomes.

We also contribute to a related literature in mortgage and housing finance that studies how enforcement institutions and intermediary exposure shape ex ante screening and outcomes. A first strand exploits differences between judicial and nonjudicial foreclosure regimes to show that weaker enforcement affects credit supply, foreclosure behavior, and borrower outcomes (Pence, 2006; Dagher and Sun, 2016). A second strand shows that the ability to sell or delay the disposition of originated loans affects screening incentives (Keys et al., 2010; Adelino et al., 2025). We study the same underlying force, variation in intermediary exposure to downside risk, but in a different institutional margin. Existing work varies exposure through contract design at origination or through the ability to transfer risk after origination. We instead vary the credibility of enforcement at the time screening decisions are made, by exploiting changes in eviction policy that affect the ability to enforce the contract after a tenant is selected. This distinction allows us to study how enforcement affects delegated screening directly and to document its implications for both allocation and discrimination in rental markets.

Third, we contribute to the literature on discrimination in housing markets. Prior work documents unequal treatment in rental and mortgage markets (Yinger, 1995; Galster and Godfrey, 2005; Hanson and Hawley, 2011; Christensen et al., 2021; Ambrose et al., 2020; Bartlett et al., 2022; Frame et al., 2023), but provides limited evidence on how legal and contractual environments shape the intensity of discrimination. We show that weakening enforcement amplifies discriminatory screening by intermediaries, particularly at stages that are difficult to regulate under existing fair housing laws. By combining a field experiment with policy variation and realized move-in outcomes, we trace this mechanism from inquiry-stage responses to actual occupancy patterns, linking discriminatory behavior to market-level allocation rather than communication outcomes alone.

Finally, we contribute to the literature on eviction policy and landlord behavior. Existing work emphasizes the protection that moratoria provide to incumbent tenants (Desmond, 2016; Collinson et al., 2023) and, more recently, the equilibrium effects of eviction restrictions on rents, defaults, and welfare (Abramson, 2025; Corbae et al., 2024). We highlight a distinct margin: access to vacant units. By increasing the cost of a bad match, eviction restrictions induce more selective and more discriminatory screening, reducing access for prospective tenants. Together with the resulting losses from foregone matches, these effects imply that welfare evaluations of eviction policy must incorporate intermediary behavior, market access, and asset-performance effects, in addition to the direct protection of incumbent tenants.

2 Theoretical Framework

We develop a dynamic screening model in which a property manager chooses whether to respond to rental inquiries when tenant default is possible and eviction enforcement may be temporarily suspended. The model allows for both group-specific beliefs about default risk (statistical discrimination) and group-specific utility costs (taste-based discrimination), and yields a simple testable prediction: by suspending enforcement, moratoria strengthen the influence of discriminatory priors on tenant selection decisions.

2.1 Setup

Consider a forward-looking property manager who discounts future payoffs at rate $\beta < 1$ and decides whether to respond to rental inquiries from two types, $i \in \{M, W\}$, of applicants: a minority applicant ($i = M$) and a white applicant ($i = W$). Applicants accept any lease offer, after which they occupy the unit as tenants. Each period, a tenant of type i pays rent $R > 0$ with probability π_i and defaults with probability $1 - \pi_i$. The parameter π_i captures the landlord's subjective belief about tenant reliability and may differ across groups. We interpret $\pi_M < \pi_W$ as evidence of *statistical discrimination.*

In addition to pecuniary payoffs, the property manager may incur a non-pecuniary flow cost κ_i from leasing to type-i applicants, reflecting taste-based preferences. We normalize $\kappa_W = 0$ and let $\kappa_M \equiv \kappa \geq 0$. A positive value of κ captures *taste-based discrimination.*

Tenants who default are evicted unless a moratorium is in place. If the property becomes vacant, the manager can list it for sale and start responding to inquiries about renting this unit. Let ψ denote an idiosyncratic shock to the relative payoff from responding to a minority rather than a white applicant. We assume $\mathbb{E}[\psi] = 0$, with cumulative distribution function $F(\cdot)$ and density $f(\cdot)$. We impose the technical condition $\lim_{\psi \to \psi_{\min}} \psi F(\psi) = 0$ to ensure well-behaved limits. Importantly, shock ψ captures idiosyncratic variation in search costs and is not itself a source of discrimination.

2.2 Property Manager's Problem

Let u_i denote the expected continuation value of leasing to an applicant of type i when eviction is enforceable

$$u_i = \pi_i(R + \beta u_i) + (1 - \pi_i)\beta V - \kappa_i, \tag{1}$$

where V denotes the option value of keeping the unit vacant and continuing to search.

Upon observing inquiries from applicants, the manager compares the payoff from responding to a minority applicant with the payoff from responding to a white applicant. The manager responds to the minority applicant whenever $u_M - \psi > u_W$, so the probability of response is:

$$P_M^{\text{Response}} = \mathbb{P}(u_M - \psi > u_W) = F(u_M - u_W). \tag{2}$$

Defining $\Delta u \equiv u_M - u_W$, the option value of a vacant unit satisfies:

$$V = \mathbb{E}[\max\{u_M - \psi, u_W\}] = u_W + \int_{\psi_{\min}}^{\Delta u} F(\psi)\, d\psi. \tag{3}$$

An equilibrium is a fixed point $\{u_M^*, u_W^*, V^*\}$ that solves the above equations.

2.3 Eviction Moratorium

We model an eviction moratorium as a temporary, one-period suspension of eviction following tenant default. During the moratorium period, the manager continues to incur the utility cost κ_i associated with tenant i and cannot re-list the unit. The continuation value under a moratorium becomes:

$$u_i = \pi_i(R + \beta u_i) + (1 - \pi_i)(-\beta\kappa_i + \beta^2 V) - \kappa_i. \tag{4}$$

Relative to the previous expression, the moratorium increases the expected cost of default by introducing both a time delay and an additional utility penalty. These effects interact directly with the manager's beliefs (π_i) and preferences (κ_i) regarding different applicant types.

2.4 Discrimination Effects of the Moratorium

Let $\Delta u^* \equiv u_M^* - u_W^*$ denote the cross-type value difference before the moratorium, and let $\Delta \bar{u} \equiv \bar{u}_M - \bar{u}_W$ denote the same under the moratorium. The difference in this difference is:

$$\Delta^2 u = \Delta \bar{u} - \Delta u^*. \tag{5}$$

A negative value of $\Delta^2 u$ implies that the moratorium exacerbates pre-existing disparities in applicant response rates.

Taste-Based Discrimination. Suppose $\pi_M = \pi_W = \pi$ and $\kappa > 0$. Then:

$$\Delta^2 u = -\frac{\beta(1-\pi)}{1-\beta\pi}\kappa < 0. \tag{6}$$

The moratorium amplifies the effect of taste-based preferences by prolonging the period over which the utility loss from leasing to minority tenants is incurred.

Statistical Discrimination. Suppose $\pi_W > \pi_M$ and $\kappa = 0$. Then:

$$\Delta^2 u = -\frac{\beta(1-\beta)(\pi_W - \pi_M)}{1 - \beta\pi_M - \beta(\pi_W - \pi_M)F(\Delta u^*)}\bar{V} < 0. \tag{7}$$

Here, the moratorium increases expected losses from applicants perceived as higher risk, with the effect magnified in high-rent markets where the option value of the unit $\bar{V}$ is larger.

2.5 Empirical Implications

The model predicts that eviction moratoria intensify discriminatory selection, decreasing the likelihood that minority applicants receive responses. The mechanisms differ by discrimination type. Under taste-based discrimination, response gaps widen uniformly across markets. Under statistical discrimination, the impact is more pronounced in high-rent markets, where the opportunity cost of delayed re-leasing is higher. These heterogeneous implications motivate our empirical test on the source of discrimination in Section 7.2.

3 Data

Correspondence experiment. Our main dataset comes from the large-scale randomized correspondence study conducted by Christensen et al. (2021), which examines racial discrimination in online rental markets in the United States. The experiment was implemented between February 6 and July 31, 2020 using an automated software bot that sent rental inquiries to newly posted listings across the fifty largest U.S. metropolitan areas. Appendix Tables A3 and A4 list the metropolitan areas and states in the experiment. Each day the bot targeted six listings per metro area: three located in downtown zip codes (defined as the five percent closest to the city center) and three in suburban zip codes. One day after a listing appeared, the bot submitted inquiries under fictitious applicant identities in randomized order, ensuring that no property manager received more than one inquiry per day.

Table 1: First and Last Names of Identities Used in the Correspondence Study

African American	**Hispanic**	**White**
Nia Harris	Isabella Lopez	Aubrey Murphy
Jalen Jackson	Jorge Rodriguez	Caleb Peterson
Ebony James	Mariana Morales	Erica Cox
Lamar Williams	Pedro Sanchez	Charlie Myers
Shanice Thomas	Jimena Ramirez	Leslie Wood
DaQuan Robinson	Luis Torres	Ronnie Miller

Applicant identities were constructed using eighteen first-last name pairs designed to signal African American, Hispanic, or white racial identities (see Table 1). Identities were additionally stratified by gender and maternal education background to reduce potential correlations with unobservable socioeconomic characteristics (Guryan and Charles, 2013; Fryer Jr and Levitt, 2004). Each fictitious applicant was assigned a unique email address and a New York City-area phone number, and all inquiries used the same standardized platform-generated message requesting to schedule a viewing. Responses were coded as one if the property manager confirmed availability within seven days and zero otherwise; approximately 80% of responses occurred within the first 24 hours.

The unit of observation is an individual inquiry sent to a property listing. The final dataset contains 25,428 inquiry-listing interactions. For each inquiry, the data report the response indicator, the race and gender of the fictitious applicant, the maternal education category, the order of the inquiry within the listing, the county and state FIPS codes, the CBSA, and the date the inquiry was sent. This dataset have two important limitations. First, information about the property manager, including identity and race, is not available because the vast majority of listings are posted by limited liability companies (LLCs). Second, the dataset also does not report listing characteristics such as rent levels or property type (e.g., single-family versus multi-family). These limitations restrict our ability to conduct important heterogeneity analyses by the race and type of the property manager and the property type.

However, we study whether eviction moratoria changes the composition of rental listings using CoreLogic data, and do not find any effects.

In the empirical analysis, the response indicator serves as the main outcome variable, while applicant race and gender indicators (and their interactions) are the key explanatory variables. We construct time variables based on the inquiry date and drop observations from Virginia after June 8, 2020, when the state introduced a second eviction moratorium.

Eviction moratoria. We construct a hand-collected dataset that records the start and end dates of state-level eviction moratoria during the spring and summer of 2020. Each policy action is linked to its legal or administrative source, including executive orders, court rulings, legislative acts, and regulatory directives. Building on the database compiled by Benfer et al. (2023), we independently verify all entries using primary sources and reconcile discrepancies in policy timing or classification.

Our analysis focuses on states that implemented a moratorium during the sample period; six states that never adopted such policies are excluded. Appendix Figures A1a and A1b report the geographic distribution of moratoria and the timing of enactment. Section 6.1 shows that our results are robust to alternative control groups that include these never-treated states.

COVID-19 cases and deaths. We obtain daily state-level COVID-19 case and death counts from the *New York Times* COVID-19 dataset, available at `https://github.com/nytimes/covid-19-data`. The data provide cumulative counts of confirmed cases and deaths by state and day. We downloaded the dataset on March 3, 2024, and set case and death counts to zero for state–day cells prior to the first reported case. We convert case counts to COVID-19 cases per 100,000 residents using state population totals.

We use COVID-19 cases per 100,000 population for several purposes. First, we use this measure to assess potential endogeneity in the timing of eviction moratoria. Second, we test whether the expiration of eviction moratoria affects COVID-19 case rates, verifying that treatment is not associated with changes in pandemic dynamics. Third, we conduct robustness checks using propensity score matching on COVID-19 case levels to ensure that treated and control states face comparable pandemic conditions.

Rental listings data. We use rental listing data from the CoreLogic Multiple Listing Service (MLS). The raw dataset contains all MLS listings from January 2018 through August 2020. We restrict the sample to rental listings and apply a series of cleaning procedures to remove non-residential properties, foreclosures, short sales, duplicate listings, and observations with missing or invalid dates; the full sample construction is reported in Appendix Table A6. The final cleaned dataset contains approximately 1.06 million rental listings. The data report detailed unit characteristics, including square footage, number of bedrooms and bathrooms, and property age, as well as listing and lease timing. We use this data to verify that the expiration of eviction moratoria did not affects the number of rental listings or the observable characteristics of the units.

Racial animus index. We measure racial animus using the Google search-based index developed by Stephens-Davidowitz (2013), which captures the relative frequency of racially charged search terms across U.S. Designated Market Areas (DMAs) between 2004 and 2007. The DMA-level index is mapped to Census block groups and merged with the experimental addresses to examine heterogeneity in discriminatory responses across locations with higher versus lower levels of racial animus; the precise geographic crosswalk used in this merge cannot be disclosed due to Institutional Review Board confidentiality requirements.

Tenant protection index. Coulson, Le, Ortego-Marti, and Shen (2025) compile a state-year dataset of legal provisions governing landlord-tenant relationships in the United States for the period 1997-2016. Using these data, they construct a Tenant Rights Index (TRI) by extracting the first principal component from twelve provisions capturing the strength of tenant protections. We use the most recent available value of the index (2016) to measure cross-state differences in tenant protection in our heterogeneity analysis in Section 7.4.

Political Affiliation. We measure state-level partisan composition using 2019 data from the National Conference of State Legislatures (NCSL).[1] For each state, we compute the Republican seat share in the lower chamber, defined as the ratio of Republican seats to total seats. Legislative composition is measured in 2019, prior to our sample period, and is used to construct heterogeneity splits based on the median Republican share.

[1]National Conference of State Legislatures, State Partisan Composition, available at `https://www.ncsl.org/about-state-legislatures/state-partisan-composition`.

Zillow Observed Rent Index (ZORI). We obtain rental market data from the Zillow Observed Rent Index (ZORI), provided by Zillow Research. The ZORI series reports smoothed, seasonally adjusted rent levels for all homes plus multifamily units and is available at the CBSA-month level. We measure local rental market conditions using the latest ZORI value prior to the start of our sample period, February 2020, and use this rent level to classify markets into above- and below-median rent areas for heterogeneity analysis. To merge the data with the experimental sample, we harmonize CBSA names between Zillow's naming convention and the Office of Management and Budget definitions using a crosswalk.

Rental Vacancy Rates. We obtain state-level rental vacancy rates from the U.S. Census Bureau's Housing Vacancy Survey (HVS). The HVS reports the percentage of rental housing units that are vacant in each state and quarter. We measure local rental market slack using the rental vacancy rate in 2019:Q4, the last observation prior to the start of our sample period. We use this measure in heterogeneity analyses.

American Community Survey (ACS). We obtain state-level socioeconomic characteristics from the 2015—2019 American Community Survey (ACS) five-year estimates accessed through Social Explorer. The data include total population, population density, median household income, the share of residents aged 65 and older, racial and ethnic composition, the share of renters, education shares, the share of residents living in group quarters, the share of essential workers, the uninsured share, average rent, and commuting mode shares, including public transportation, carpooling, driving alone, motorcycle, bicycle, walking, and working from home. We construct all shares as ratios of the relevant subgroup count to its corresponding ACS group total. For regression use, we transform median household income using the natural logarithm and rescale population density by dividing by 1,000. These variables are used to test for potential endogeneity in the timing of moratorium expiration and to conduct heterogeneity analyses across states with different demographic and socioeconomic characteristics.

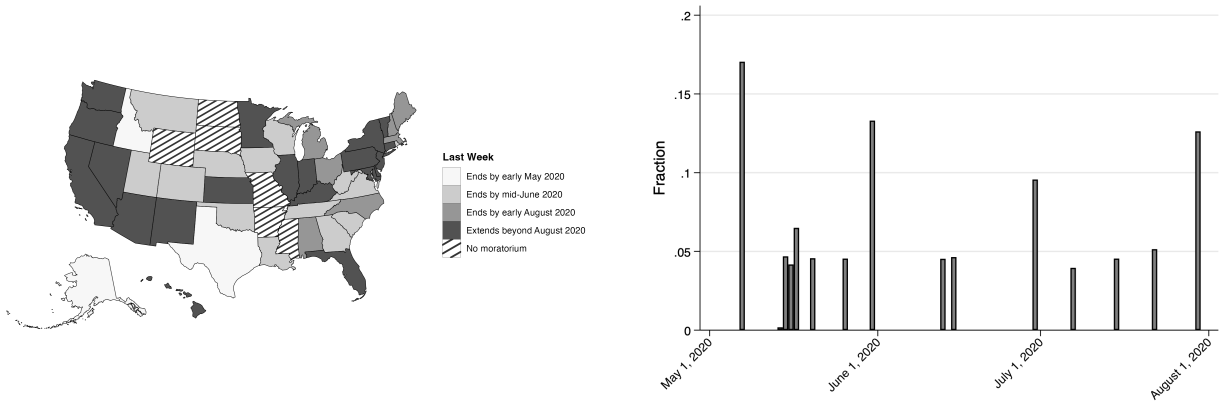
Air quality (fine particulate matter $PM_{2.5}$) data. To measure local air pollution exposure, we use satellite-derived estimates of fine particulate matter ($PM_{2.5}$) developed by van Donkelaar et al. (2021). These data combine satellite observations of aerosol optical depth with chemical transport models and ground monitoring stations in a geophysical–statistical framework to produce spatially continuous estimates of surface-level $PM_{2.5}$ concentrations. The dataset provides high-resolution measures of ambient air pollution that are widely used in environmental and health research, particularly in locations where ground monitoring networks are sparse. In the heterogeneity analysis, we classify areas into above- and below-median $PM_{2.5}$ exposure to test whether discrimination patterns differ between more and less polluted environments.

InfoUSA (Data Axle) residential data. To study whether discriminatory screening affects realized housing outcomes, we link the experimental rental listings to the residential address history database compiled by InfoUSA (now Data Axle). The database is a commercial marketing dataset that covers nearly the entire U.S. population and reports household-level information, including address histories, owner-renter status, ethnicity codes, and the length of residence at the current address.

We merge the 2020–2022 vintages of the database with the experimental addresses using several matching procedures described in Appendix D. Using the observation date and the reported length of residence, we infer the tenant's move-in date and retain households whose move-in occurs after the experimental inquiry date. Ethnicity codes are mapped into four race categories (White, Black, Hispanic, and Asian/Other) using vendor and manually constructed crosswalks. The resulting dataset provides a mapping between experimental rental listings and the race of the tenant who subsequently occupied the unit and forms the basis for our outcome test of discrimination in Section 8.1.

4 Results

4.1 Defining Treatment



(a) The Last Week of the Eviction Moratorium (b) The Last Date of Eviction Moratorium

Figure 1: Eviction Moratoria Expiration across U.S. States. Panel (a) shows the last week of the eviction moratorium across the United States. Panel (b) presents the distribution of moratorium expiration dates across states.

Because eviction moratoria were introduced within a narrow window at the onset of the COVID-19 pandemic, policy onset provides limited identifying variation across states. Moreover, the timing of enactment is unlikely to be exogenous: in our data, the start of a moratorium is associated with an increase of about 10.8 daily COVID-19 cases per 100,000 state residents, with a 95% confidence interval of 7.5 to 14.1, suggesting that states tended to adopt moratoria in response to worsening local pandemic conditions.[2] Expirations, in contrast, occurred in a staggered fashion and therefore provide substantially richer variation in the timing of restored eviction enforcement. Figure 1a depicts this variation in the final week of state-level moratoria across the United States. As we show in Section 4.2, expiration timing also does not appear to be systematically related to daily COVID-19 rates or to observable state characteristics, making it a more credible source of quasi-experimental variation. For these reasons, we define treatment based on the *expiration* of a moratorium rather than its enactment, in line with prior work that likewise studies the effects of morato-

[2]Estimated using the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021).

ria ending rather than their initial adoption (Benfer et al., 2021a; Hatamyar and Parmeter, 2023).

During our sample period, eviction restrictions also arose from the federal CARES Act, which imposed a temporary moratorium on evictions for properties with federally backed mortgages or participating in federal housing programs. In Section 6.2, we examine whether our results could be driven by this federal policy and show that the main findings are not explained by CARES Act coverage. In addition, the federal eviction moratorium issued by the Centers for Disease Control and Prevention began on September 4, 2020. Because our experimental sample ends on July 31, 2020, the analysis exploits only cross-state variation in the expiration of state-level moratoria that occurred prior to the CDC policy.

The correspondence experiment spans February 6 through July 31, 2020. To avoid conflating baseline discrimination with periods prior to the policy intervention, we restrict the sample to observations that occur after a state enacted its eviction moratorium. The resulting analysis sample begins on March 13, 2020, the earliest date on which a moratorium was active in any treated state. Figure 1b shows the distribution of moratorium expiration dates in the sample, which range from May 7 to July 30, 2020.

We define the treatment indicator $Treatment_{jt}$ as equal to one if inquiry j was sent after the expiration of the eviction moratorium in the corresponding state and zero otherwise. This design exploits plausibly exogenous cross-state variation in the timing of the restoration of eviction enforcement to identify its effect on property managers' screening behavior.

4.2 Potential Endogeneity of Treatment

Although eviction moratoria were enacted within a narrow time window across states, their expiration occurred at different points during the spring and summer of 2020. This staggered repeal raises a natural concern that states may have lifted moratoria in response to evolving public health conditions or pre-existing socioeconomic characteristics. If such factors are correlated with trends in discriminatory screening, estimates that exploit variation in moratorium expiration could be biased. Our identification strategy, therefore, requires that, absent repeal, discrimination trends would have evolved similarly across states.

Table 2: Predicting the Timing of Eviction Moratorium Expiration

	(1) Coefficient	(2) 95% Confidence Interval
First day of moratorium	6.57	(-16.97, 30.12)
Total population in 100k	0.87	(-0.80, 2.53)
Population density	-0.01	(-0.21, 0.19)
Log median income	-944.53	(-2793.84, 904.78)
Percent of people who are over 65 years old	-9.06	(-83.01, 64.88)
Percent of African Americans	-0.84	(-20.34, 18.65)
Percent of Asians	-17.19	(-64.03, 29.65)
Percent of American Indian	-9.70	(-81.46, 62.05)
Percent of Hispanics	-6.91	(-30.09, 16.26)
Percent of renters	16.08	(-30.13, 62.28)
Percent of people without high school degrees and below	-53.85	(-174.21, 66.52)
Percent of people with a college degree and above	-1709.28	(-7509.08, 4090.51)
Percent of people in group quarters	-100.86	(-450.12, 248.39)
Percent of essential workers	-65.88	(-163.94, 32.19)
Percent of people who are uninsured	-22.68	(-99.84, 54.48)
Percent of people who use public transportation	-185.58	(-610.11, 238.94)
Percent of people who carpool	-37.81	(-612.60, 536.99)
Percent of people who commute by driving alone	-189.05	(-617.60, 239.50)
Percent of people who commute using motorcycle	-311.11	(-3219.22, 2597.01)
Percent of people who commute using bicycle	-365.00	(-994.39, 264.40)
Percent of people who commute by walking	-193.25	(-764.43, 377.93)
Percent of people who work at home	-205.21	(-663.28, 252.87)
Observations	36	

Notes: The dependent variable is the date of eviction moratorium expiration at the state level. All covariates are predetermined and come from the 2015–2019 five-year American Community Survey. The specification controls for the moratorium start date to account for mechanical differences in policy duration. Reported coefficients are from a second-stage regression of state fixed effects obtained after partialling out contemporaneous COVID-19 conditions. The absence of systematic predictors indicates that moratorium repeal timing is orthogonal to observable state fundamentals. Ninety-five percent confidence intervals are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

We address this concern using three complementary approaches. First, we present event study evidence showing no differential pre-trends in discriminatory outcomes prior to moratorium expiration. Figure 2 shows that relative response rates for minority applicants evolve smoothly and in parallel across treated and not-yet-treated states in the pre-repeal period. This evidence supports the parallel trends assumption underlying our staggered difference-in-differences design.

Second, we assess whether moratorium repeal coincides with changes in contemporaneous public health conditions. In a placebo-style exercise, we estimate staggered difference-in-differences specifications in which the outcome is state-level daily COVID-19 case counts and the treatment is moratorium expiration. We find no evidence of discontinuous changes in COVID-19 dynamics around repeal dates. This finding is consistent with descriptive evidence in Benfer et al. (2021b), who document only a weak relationship between epidemiological conditions and the timing of moratorium rollback, noting that eviction protections were frequently lifted even as COVID-19 prevalence was increasing. We report these estimates in Section 5.2.

Importantly, we do not rely on conditioning on COVID-19 cases to identify our baseline effects. Adjusting for contemporaneous case counts in settings with staggered treatment adoption requires strong overlap in treatment propensities across cohorts, which is violated in our data. For transparency, Section 5.2 presents propensity score matched estimates based on contemporaneous COVID-19 incidence, but we treat these as robustness checks rather than as our preferred specification. Our baseline estimates instead rely on the absence of pre-trends and the quasi-random timing of moratorium repeal.

Finally, we examine whether the timing of moratorium termination is systematically related to pre-determined state characteristics. We implement a two-stage procedure. In the first stage, we regress the date of moratorium expiration on state fixed effects and measures of contemporaneous COVID-19 conditions. In the second stage, we regress the estimated state fixed effects on pre-pandemic socioeconomic characteristics from the 2015–2019 five-year American Community Survey, controlling for the moratorium start date to flexibly account for policy duration. As shown in Table **??**, none of these pre-determined covariates significantly predict the timing of repeal. We get similar results if we also consider political affiliation. This evidence suggests that variation in moratorium expiration was not systematically driven by underlying state-level fundamentals. Moreover, to the extent that such characteristics matter, they are absorbed by state fixed effects in our main specifications.

4.3 Staggered Differences-in-Differences

We examine how racial disparities in housing inquiry responses evolve following the expiration of state-level eviction moratoria. As shown in Appendix B, differential response rates across applicant racial identities are present throughout the sample period, consistent with the framework in Section 2. We therefore focus on whether the removal of eviction moratoria altered the intensity of discriminatory screening.

State eviction moratoria were lifted at different points in time, generating staggered treatment adoption. In such settings, conventional two-way fixed-effects Difference-in-Differences estimators combine comparisons across cohorts with heterogeneous treatment timing, which can lead to attenuation or sign distortions in the presence of dynamic or heterogeneous treatment effects. We therefore rely on an estimator that is robust to staggered adoption and explicitly accommodates treatment effect heterogeneity. For completeness, Appendix C reports conventional DiD estimates, which are directionally consistent with our main findings but statistically attenuated.

Our primary analysis uses the staggered Difference-in-Differences estimator developed by Callaway and Sant'Anna (2021). Because rental listings are not observed continuously over time and the set of active listings varies across states and days, we implement the estimator in two stages. First, we construct state-by-day measures of racial discrimination in inquiry responses. Second, we apply the Callaway–Sant'Anna Difference-in-Differences (CSDiD) procedure to these state-level outcomes to recover dynamic treatment effects associated with moratorium expiration.

Stage 1: Estimating Daily Discrimination Coefficients for Each State

In the first stage, we estimate the degree of racial and ethnic discrimination for each state s and day $\tau \in \{1, \dots, 177\}$, spanning February 6 to July 31, 2020. Specifically, we model the probability of a response to a rental inquiry as a function of race and identity characteristics:

$$Response_{ijst} = \beta^{AA}_{s\tau} \cdot African\ American_j + \beta^{H}_{s\tau} \cdot Hispanic_j + X_j'\theta_{s\tau} + u_{ijst}, \tag{8}$$

where i indexes rental properties, j indexes fictitious identities, s indexes states, and t is the date of inquiry. The outcome variable $Response_{ijst}$ equals one if identity j receives a response from property i in state s on day t. The regressors include indicator variables for race and a vector X_j of identity-level covariates (gender, maternal education, and inquiry order). Each regression is estimated via logit to ensure predicted probabilities remain bounded between zero and one.

To estimate $\beta^R_{s\tau}$, we use *all* observations from state s, applying a Gaussian kernel to weight observations by their temporal distance from day τ:

$$\omega_{ijst}^{\tau} = \frac{1}{h\sqrt{2\pi}} \exp\left(-\frac{1}{2}\left(\frac{t-\tau}{h}\right)^2\right), \tag{9}$$

where h is a smoothing parameter. This approach addresses the concern that sample sizes per state-day may be too small to yield reliable estimates. By pooling all available data for each state and upweighting observations close to τ, we recover smoothly varying state-day estimates of discrimination that reflect local trends without overfitting to daily noise. We use the coefficients only within a window around treatment and show robustness to different smoothing parameters h.

This estimation procedure gives us predicted discrimination coefficients $\{\beta_{s\tau}^{AA}, \beta_{s\tau}^{H}\}$ for each state s and day τ for African American (AA) and Hispanic (H) identities. Because the discrimination coefficients $\{\beta_{s\tau}^{AA}, \beta_{s\tau}^{H}\}$ are estimated with sampling error, we report a bootstrap robustness check in Section 6.4 that accounts for this uncertainty. The bootstrap results confirm that our main inferences are not sensitive to estimation error in Stage 1.

Stage 2: Estimating Treatment Effects

We use a sequence of discrimination estimates $\{\beta_{s\tau}^{AA}, \beta_{s\tau}^{H}\}$ for each state s and day τ from Stage 1 to compute predicted response probabilities for representative applicants of each racial group — specifically, males with low maternal education who send the first message. Let $\rho_{s\tau}^{R}$ denote the predicted probability of a response for race $R \in \{AA, H\}$. We construct the natural logarithm of the relative response ratio:

$$\log(RR_{s\tau}) \equiv \log\left(\frac{\rho_{s\tau}^{R}}{\rho_{s\tau}^{W}}\right),$$

which serves as the outcome variable in the second-stage CSDiD estimation. Our main results are robust to using the relative response ratio as the outcome variable (Table A5).

Prior to applying the estimator, we restrict the sample in two ways. First, we exclude all observations from before a state enacted its moratorium and all τ for which the state has fewer than 100 inquiries to estimate $\beta_{s\tau}^{R}$ reliably. Second, we define a symmetric event window $|\tau - \tau_s^*| \leq \hat{\tau}$, where τ_s^* is the day the moratorium ends in state s, and $\hat{\tau} \in \{30, 45, 60\}$

determines the window size.

We follow Callaway and Sant'Anna (2021)'s methodology and define a state-day observation as treated on day τ if the state ended its moratorium before or on this day, and not treated if the state did not end its moratorium by that time. Our baseline control group is not-yet-treated states, but we show robustness to using not-treated states in the next section. To estimate the average treatment effect on states treated in period g for different periods t, $ATT(g,t)$, we first keep observations for states that had a moratorium on day g or have not yet had the moratorium by time t. We then use these observations to run the regression

$$\log(RR_{s\tau}) = \alpha_0^g + \alpha_1^g \cdot TREAT_s^g + \alpha_2^g \cdot \mathbb{1}\{\tau = t\} + \alpha_3^{g,t} \cdot TREAT_s^g \times \mathbb{1}\{\tau = t\} + \nu_{s\tau}, \quad (10)$$

where $TREAT_s^g$ indicates whether state s was treated on day g. The interaction coefficient $\alpha_3^{g,t}$ identifies the group-time average treatment effect $ATT(g,t)$, which is aggregated across groups and periods using the CS weighting scheme.

The estimates from equation (10) identify the causal effect of eviction moratorium expiration on relative response ratios under a conditional parallel trends assumption. Specifically, absent changes in eviction enforcement, the evolution of responses to minority applicants relative to white applicants would have followed similar trends across states with different moratorium expiration dates. This assumption allows for arbitrary cross state differences in levels and for common time-varying shocks, but rules out state-specific shocks coinciding with moratorium expiration that differentially affect property managers' screening behavior by race through channels other than eviction enforcement.

As in all difference-in-differences designs, the parallel trends assumption is not directly testable. Its plausibility is supported by the absence of differential pre-trends in relative response ratios prior to moratorium expiration. Appendix Figure A4 shows no evidence of pre-treatment trends in the raw relative response ratios estimated in Stage 1, and the group-time average treatment effects from the CSDiD event study analysis discussed below similarly exhibit flat pre-treatment dynamics.

Results

Figure 2 presents event study estimates for African American applicants using a smoothing parameter of $h = 10$ and an event window of $\hat{\tau} = 45$ days. Relative response ratios increase sharply following moratorium expiration, indicating a reduction in racial discrimination once eviction enforcement is reinstated. There is no evidence of differential pre-trends prior to expiration. The timing of the response aligns closely with the legal expiration date of the moratorium, consistent with a discrete change in enforcement rather than gradual shifts in market conditions.

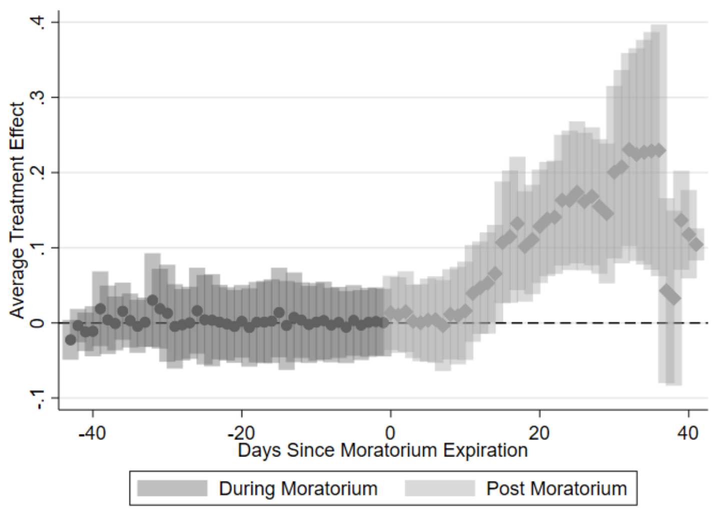


Figure 2: The Event Study Estimates of the Effect of the End of a Moratorium on the Natural Logarithm of the Relative Response Ratios for an African American Identity Relative to a White Identity from Callaway and Sant'Anna (2021)'s Staggered DiD with the Smoothing Parameter $h = 10$ and $\hat{\tau} = 45$ Days around Treatment

Under this design, potential threats to identification must involve state-specific, race-differential shocks that vary at the daily frequency and coincide precisely with moratorium expiration. Appendix Figure A2 shows that these patterns are robust across alternative smoothing parameters h and event-window lengths $\hat{\tau}$.

Table 3: Staggered DiD Estimates of the Effect of the End of Moratorium on the Logarithm of the Relative Response Ratio Relative to White Applicants

	African American			Hispanic		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
Panel A: 30 days around treatment						
ATT	0.033	0.041**	0.034**	0.008	0.012	0.010
	(-0.010, 0.077)	(0.003, 0.079)	(0.002, 0.066)	(-0.025, 0.041)	(-0.017, 0.042)	(-0.016, 0.037)
Number of Observations	770	770	770	770	770	770
Panel B: 45 days around treatment						
ATT	0.095***	0.085***	0.063***	0.043*	0.027	0.008
	(0.051, 0.139)	(0.049, 0.121)	(0.033, 0.092)	(-0.004, 0.090)	(-0.009, 0.064)	(-0.022, 0.037)
Number of Observations	1217	1217	1217	1217	1217	1217
Panel C: 60 days around treatment						
ATT	0.102***	0.090***	0.065***	0.059**	0.040*	0.014
	(0.052, 0.152)	(0.051, 0.128)	(0.034, 0.095)	(0.007, 0.112)	(-0.004, 0.083)	(-0.022, 0.050)
Number of Observations	1652	1652	1652	1652	1652	1652

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 3 reports average treatment effects on the treated across alternative smoothing parameters and event windows. For African American applicants, the estimated effects are positive and stable across specifications. Our preferred specification corresponds to a 45-60 day event window with moderate smoothing, $h = 7$ or $h = 10$, and implies a 0.09 log-point increase in the African American-White relative response ratio following moratorium expiration. In contrast, estimated effects for Hispanic applicants are smaller in magnitude and substantially less precisely estimated across specifications. Accordingly, the remainder of the analysis focuses on African American applicants, for whom the treatment effects are consistently robust and economically meaningful.

To assess the economic significance, recall that white applicants in our sample received responses 59.8% of the time during moratoria, while African American applicants received responses at a rate of 54.5%, implying a relative response ratio of 0.911. Our ATT estimates suggest that the log of this ratio increases by 0.09 for African American identities. This translates into the increase in the relative response ratio RR by $100 \times (e^{0.09} - 1) = 9.4\%$, which significantly shinks the racial gap in response rates.

5 Alternative Explanations

This section examines whether factors other than the restoration of eviction enforcement could account for the changes in screening behavior documented above. In particular, we consider three potential confounding mechanisms. First, moratorium expiration could coincide with changes in rental market conditions, such as shifts in the number or characteristics of units listed for rent. Second, our estimates could reflect contemporaneous public health dynamics during the COVID-19 pandemic. Third, screening behavior may have changed in response to heightened public awareness of racial discrimination following the murder of George Floyd. Across these tests, the evidence consistently indicates that the observed changes in relative response rates are most consistent with changes in enforcement risk rather than with these alternative explanations.

5.1 Market Conditions and Rental Unit Characteristics

Table 4: Staggered DiD Estimates of the Effect of the End of Moratorium on Housing Characteristics

	# Listings	Sq. Ft.	Beds	Baths	Age
	Panel A: 30 days around treatment				
ATT	-0.124	-0.006	-0.017	-0.006	-0.157
	(-0.681, 0.433)	(-0.058, 0.047)	(-0.087, 0.053)	(-0.084, 0.072)	(-4.276, 3.962)
Number of Observations	1109	1031	1031	1031	1031
	Panel B: 45 days around treatment				
ATT	-0.298	-0.004	0.017	0.000	-1.925
	(-0.799, 0.203)	(-0.052, 0.045)	(-0.035, 0.068)	(-0.076, 0.076)	(-5.834, 1.984)
Number of Observations	1785	1648	1648	1648	1648
	Panel C: 60 days around treatment				
ATT	-0.363	-0.020	0.046	-0.059	0.426
	(-0.894, 0.167)	(-0.075, 0.034)	(-0.016, 0.109)	(-0.145, 0.027)	(-4.078, 4.930)
Number of Observations	2394	2212	2212	2212	2212

A potential alternative explanation for the observed changes in relative response ratios is that market conditions shifted around moratorium expiration, for example, through changes in the volume or composition of rental units listed for rent. To examine this possibility, we use CoreLogic MLS data to construct a comprehensive panel of rental listings from 2018–2020. Rental units with multiple listing spells are merged following the procedure in Garriga and Hedlund (2020).

Table 4 reports staggered DiD estimates of the effect of moratorium expiration on the number of rental listings and key unit characteristics, including square footage, number of bedrooms and bathrooms, and unit age. Across all event windows, the estimated effects are small in magnitude and statistically insignificant. These results indicate that neither the quantity of rental listings nor their observable characteristics change discontinuously at moratorium expiration, making it unlikely that shifts in the supply of rental units drive the changes in relative response rates documented above.

A related concern is that the composition of the pool of prospective tenants searching for rental housing may change around moratorium expiration, potentially in race-specific ways. Direct micro-level data on the racial composition of searching tenants are not available. However, such changes would threaten identification only under a restrictive set of conditions: applicant composition would need to shift differentially by race, differentially by state, and discontinuously at the moratorium expiration date, through channels unrelated to eviction enforcement. This conjunction of conditions is difficult to reconcile with the sharp timing of the estimated increase in the relative response rates and the lack of the pre-trends.

5.2 COVID-19 Dynamics

Table 5: Staggered DiD Estimates of the Effect of the End of Moratorium on the Logarithm of the Relative Response Ratio Relative to White Applicants With Propensity Score Matching on the Daily COVID-19 Cases in a State

	Main			Propensity Score Matching on the COVID-19 Cases		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment					
ATT	0.033	0.041**	0.034**	0.285***	0.189***	0.108***
	(-0.010, 0.077)	(0.003, 0.079)	(0.002, 0.066)	(0.233, 0.337)	(0.154, 0.224)	(0.088, 0.129)
Number of Observations	770	770	770	718	718	718
	Panel B: 45 days around treatment					
ATT	0.095***	0.085***	0.063***	0.082***	0.074***	0.058***
	(0.051, 0.139)	(0.049, 0.121)	(0.033, 0.092)	(0.056, 0.107)	(0.055, 0.094)	(0.040, 0.075)
Number of Observations	1217	1217	1217	1155	1155	1155
	Panel C: 60 days around treatment					
ATT	0.102***	0.090***	0.065***	0.018	0.028	0.023*
	(0.052, 0.152)	(0.051, 0.128)	(0.034, 0.095)	(-0.029, 0.065)	(-0.008, 0.063)	(-0.004, 0.050)
Number of Observations	1652	1652	1652	1625	1625	1625

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

To evaluate whether contemporaneous public health conditions confound our estimates, we re-estimate the staggered difference-in-differences specifications using propensity score matching on daily state-level COVID-19 case counts. The right panel of Table 5 reports these estimates, with the left panel reproducing our baseline results for comparison. Across event windows and smoothing parameters, the matched estimates are similar in magnitude to the baseline effects and remain statistically significant in most specifications, indicating that differential exposure to COVID-19 does not drive our results.

We do not, however, adopt the matched estimates as our preferred specification. Identification under propensity score matching with staggered adoption requires a strong overlap condition: the conditional probability that a state s ends a moratorium on day g, given the number of COVID-19 cases X_s, must be bounded away from zero and one, $\varepsilon < \Pr(G_s = g \mid X_s) < 1 - \varepsilon$, for all repeal dates g. This requirement is difficult to guarantee in settings with staggered policy adoption, where treatment timing is concentrated in relatively narrow windows (Baker et al., 2025).

Table 6: Staggered DiD Estimates of the Effect of the End of Moratorium on the Daily Number of the COVID-19 Cases in a State Per 100,000 People

	Days around treatment		
	30	45	60
ATT	61.053	4.464	-10.445
	(-59.926, 182.032)	(-107.839, 116.768)	(-120.802, 99.911)
Number of Observations	917	1437	1958

Notes: 1) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 2) 95% confidence intervals in parentheses.

A complementary placebo test in Table 6 shows that the expiration of eviction moratoria does not systematically predict short-run changes in COVID-19 case rates, further indicating that public health dynamics do not mechanically determine treatment timing. Taken together, these results suggest that conditioning on COVID-19 cases is conservative rather than essential for identification. We therefore retain the unmatched staggered DiD estimates as our baseline and view the matched results in Table 5 as a robustness check.

5.3 Black Lives Matter Movement and George Floyd Murder

Because the experiment was conducted during summer 2020, a potential concern is that heightened awareness of racial discrimination following the murder of George Floyd on May 25, 2020 may confound our estimates. If the intensification of the Black Lives Matter movement independently altered screening behavior, it could contribute to the observed changes in responses to African American applicants.

Table 7: Estimates from Sample Before and After George Floyd Murder

	Before			After		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment					
ATT	0.069***	0.062***	0.040**	-0.031	-0.009	0.006
	(0.020, 0.119)	(0.019, 0.106)	(0.000, 0.081)	(-0.092, 0.029)	(-0.062, 0.043)	(-0.038, 0.050)
Number of Observations	289	289	289	401	401	401
	Panel B: 45 days around treatment					
ATT	0.044*	0.036*	0.021	0.080**	0.074***	0.062***
	(-0.001, 0.089)	(-0.006, 0.079)	(-0.018, 0.059)	(0.018, 0.141)	(0.027, 0.120)	(0.025, 0.099)
Number of Observations	450	450	450	653	653	653
	Panel C: 60 days around treatment					
ATT	0.062***	0.050***	0.031*	0.097***	0.084***	0.062***
	(0.018, 0.107)	(0.013, 0.088)	(-0.001, 0.064)	(0.026, 0.168)	(0.028, 0.139)	(0.017, 0.106)
Number of Observations	621	621	621	843	843	843

To assess this possibility, we re-estimate our main specifications separately for subsamples before and after May 25, 2020. Table 7 reports the results. Although statistical precision declines due to smaller subsamples, particularly in shorter event windows, the estimated effects remain economically meaningful and of similar sign across periods. In the 45- and 60-day windows, discrimination following moratorium expiration remains statistically significant both before and after May 25.

Overall, the persistence of the estimated effects across subsamples suggests that contemporaneous changes in racial awareness are unlikely to account for our main results. Instead, the evidence is more consistent with changes in screening behavior associated with the reinstatement of eviction enforcement.

6 Robustness

To strengthen the credibility of our estimates, we conduct a series of robustness checks that examine sensitivity to the definition of the control group, the presence of the federal CARES Act moratorium, alternative staggered difference-in-differences estimators, and estimation uncertainty arising from the two-stage procedure.

6.1 Control Group

Table 8: Staggered DiD Estimates of the Effect of the End of Moratorium on the Natural Logarithm of the Relative Response Ratios for a African American Identity Relative to a White Identity Using Not Treated States

	Not Treated			Not Yet Treated + Not Treated		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
Panel A: 30 days around treatment						
ATT	0.024***	0.053***	0.064***	0.020	0.027	0.017
	(0.012, 0.036)	(0.042, 0.064)	(0.053, 0.075)	(-0.023, 0.063)	(-0.007, 0.062)	(-0.013, 0.047)
Number of Observations	985	985	985	986	986	986
Panel B: 45 days around treatment						
ATT	0.070***	0.095***	0.098***	0.080***	0.074***	0.052***
	(0.057, 0.082)	(0.084, 0.107)	(0.087, 0.109)	(0.035, 0.124)	(0.038, 0.109)	(0.022, 0.082)
Number of Observations	1441	1441	1441	1441	1441	1441
Panel C: 60 days around treatment						
ATT	0.083***	0.106***	0.108***	0.084***	0.078***	0.057***
	(0.070, 0.095)	(0.094, 0.117)	(0.097, 0.120)	(0.035, 0.133)	(0.040, 0.116)	(0.026, 0.087)
Number of Observations	1848	1848	1848	1850	1850	1850

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% bootstrapped bias-corrected confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

We assess the robustness of our findings to the definition of the control group by re-estimating the staggered Difference-in-Differences specifications using states that never lifted their eviction moratoria during the sample period as the control group. These states provide a natural benchmark, as they remained untreated throughout the study window. Table 8 presents the results using this alternative control group in addition to the not-yet treated states in the right panel or as the only control group in the left panel.

The estimated effects remain positive and are statistically significant in most specifications. Notably, the estimated effects are similar in the magnitudes and are more consistently significant relative to the baseline specification that uses not-yet-treated states as the control group, consistent with the presence of discriminatory behavior during the moratorium period.

These results reinforce our core conclusion: the expiration of eviction moratoria is associated with a meaningful increase in relative response rates to African American rental applicants. The consistency of this pattern across "not-yet-treated", "never-treated", and "not-yet-treated" and "never-treated" control groups strengthens the credibility of our findings and suggests that they are not an artifact of control group selection.

6.2 Federal Eviction Moratorium

Table 9: Staggered DiD Estimates of the Effect of the End of Moratorium on the Logarithm of the Relative Response Ratio to African Americans Relative to White Applicants

	Benchmark Estimates			During Federal Moratorium		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
Panel A: 30 days around treatment						
ATT	0.033	0.041**	0.034**	0.056**	0.057***	0.044***
	(-0.010, 0.077)	(0.003, 0.079)	(0.002, 0.066)	(0.009, 0.103)	(0.017, 0.098)	(0.011, 0.077)
Number of Observations	770	770	770	726	726	726
Panel B: 45 days around treatment						
ATT	0.095***	0.085***	0.063***	0.071***	0.069***	0.050***
	(0.051, 0.139)	(0.049, 0.121)	(0.033, 0.092)	(0.036, 0.106)	(0.039, 0.100)	(0.024, 0.076)
Number of Observations	1217	1217	1217	1137	1137	1137
Panel C: 60 days around treatment						
ATT	0.102***	0.090***	0.065***	0.084***	0.079***	0.058***
	(0.052, 0.152)	(0.051, 0.128)	(0.034, 0.095)	(0.045, 0.124)	(0.046, 0.112)	(0.031, 0.085)
Number of Observations	1652	1652	1652	1520	1520	1520

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

The federal CARES Act imposed a nationwide eviction moratorium from March 27 to July 24, 2020, covering properties with federally backed mortgages. Although the policy overlapped with many state moratoria, its institutional structure differed in a key dimension: enforcement. State moratoria operated through local courts and directly suspended eviction filings and proceedings. By contrast, the CARES Act did not create an independent federal enforcement mechanism and did not grant Freddie Mac authority to intervene in state eviction courts.

Contemporaneous oversight reports from the Federal Housing Finance Agency Office of Inspector General underscore this distinction. The report concludes that Freddie Mac "does not have the authority or ability to directly enforce" compliance with the CARES Act and further states that the Enterprise "does not actively monitor its borrowers' compliance with the tenant protections of the CARES Act," instead relying on servicers to investigate alleged violations (FHFA Office of Inspector General, 2022). In other words, the federal moratorium lacked direct monitoring and judicial enforcement.

This institutional contrast allows us to isolate the role of contractual frictions. In our framework, discrimination increases when eviction ceases to be a credible enforcement threat. What matters for screening incentives is the expected enforceability of lease contracts, not the formal existence of a statutory restriction.

We therefore use the CARES Act period as a cross-regime comparison. Table 9 reports staggered DiD estimates for the full benchmark sample and for the subsample restricted to the CARES Act window. Across our preferred specifications, the estimated effects are economically similar and statistically indistinguishable across regimes.

This pattern is consistent with the contractual-frictions mechanism. Because the federal moratorium did not materially alter the enforceability of eviction through local courts, it did not meaningfully change property managers' expected loss from tenant default. Screening behavior responds to changes in enforcement credibility, such as the expiration of state-level court-imposed moratoria, rather than to the mere presence of a statutory restriction without direct enforcement authority.

This cross-regime comparison reinforces the model's central prediction that discrimination intensifies when eviction ceases to function as a credible enforcement mechanism.

6.3 Other Staggered Difference-in-Difference Estimators

Table 10: Estimates the Effect of the End in the Moratorium on the Natural Logarithm of the Relative Response Ratios for an African American Identity Relative to a White Identity from Alternative Staggered Difference-in-Differences Estimators

	Borusyak et al. (2024)			Sun and Abraham (2021)			de Chaisemartin and D'Haultfœuille (2020)		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
Panel A: 30 days around treatment									
ATT	0.025**	0.038***	0.042***	0.173***	0.158***	0.128***	0.085*	0.088*	0.077***
	(0.003, 0.048)	(0.029, 0.048)	(0.037, 0.048)	(0.145, 0.201)	(0.140, 0.176)	(0.115, 0.140)	(-0.001, 0.170)	(-0.003, 0.178)	(0.028, 0.127)
# Obs.	1046	1046	1046	985	985	985	1468	1468	1468
Panel B: 45 days around treatment									
ATT	0.032**	0.042***	0.044***	0.189***	0.185***	0.159***	0.149**	0.141***	0.114***
	(0.000, 0.063)	(0.023, 0.061)	(0.030, 0.058)	(0.162, 0.217)	(0.165, 0.206)	(0.144, 0.173)	(0.029, 0.269)	(0.055, 0.227)	(0.063, 0.166)
# Obs.	1486	1486	1486	1441	1441	1441	2355	2355	2355
Panel C: 60 days around treatment									
ATT	0.020	0.034***	0.036***	0.164***	0.159***	0.149***	0.146***	0.136**	0.107**
	(-0.015, 0.054)	(0.009, 0.059)	(0.013, 0.058)	(0.139, 0.190)	(0.141, 0.178)	(0.137, 0.162)	(0.054, 0.238)	(0.031, 0.242)	(0.013, 0.202)
# Obs.	1888	1888	1888	1848	1848	1848	3496	3496	3496

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% bootstrapped bias-corrected confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

A standard concern in staggered DiD settings is that two-way fixed-effects style comparisons can be contaminated by comparisons between already-treated and later-treated units, which can attenuate or otherwise distort treatment effect estimates when effects are heterogeneous over event time. This concern is particularly relevant in our setting because states exited eviction moratoria at different dates. Table 10 therefore re-estimates the effect of moratorium expiration using three alternative estimators proposed to address these issues: Borusyak et al. (2024), Sun and Abraham (2021), and de Chaisemartin and D'Haultfœuille (2020).

Across all three approaches, the estimated effect for African American applicants remains positive in nearly every specification and statistically significant in the large majority of cases. This pattern holds across event windows of 30, 45, and 60 days around treatment and across alternative values of the smoothing parameter h. The consistency in sign across estimators is important because these procedures rely on different comparison sets and weighting schemes.

The fact that they all imply an improvement in the relative response rate of African American applicants after the end of the moratorium indicates that our core result is not driven by forbidden comparisons or by a particular implementation of staggered DiD.

At the same time, the magnitude of the estimates varies across estimators, which is expected given differences in estimands, sample composition, and weighting of cohort-time effects. We therefore do not emphasize any single point estimate from Table 10. Instead, we interpret the table as a robustness exercise showing that the qualitative conclusion is stable: discriminatory screening is stronger while eviction enforcement is suspended and declines once moratoria expire.

6.4 Errors from the Two-stage Procedure

Table 11: Bootstrapped Staggered DiD Estimates of the Effect of the End in the Moratorium on the Natural Logarithm of the Relative Response Ratios for an African American Identity Relative to a White Identity

	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment		
ATT	0.036	0.047	0.039
	(-0.099, 0.164)	(-0.064, 0.119)	(-0.041, 0.093)
Number of Observations	770	770	770
	Panel B: 45 days around treatment		
ATT	0.114*	0.099**	0.071**
	(-0.007, 0.254)	(0.020, 0.189)	(0.019, 0.133)
Number of Observations	1217	1217	1217
	Panel C: 60 days around treatment		
ATT	0.111*	0.097***	0.069***
	(-0.003, 0.225)	(0.023, 0.194)	(0.018, 0.150)
Number of Observations	1652	1652	1652

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% bootstrapped bias-corrected confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

To evaluate whether our estimated treatment effects are robust to uncertainty introduced by the first-stage estimation of state-by-day discrimination coefficients, we implement a bootstrap procedure that accounts for the two-stage structure of the staggered Difference-in-Differences design. The procedure is designed to propagate first-stage estimation uncertainty through to the second stage and ensure valid inference.

We generate each bootstrap replicate by resampling states with replacement. For each selected state, we retain all observations across the full sample period following its implementation of a moratorium. We then re-estimate the first-stage logit regressions using the original kernel-weighted procedure to recover smoothed estimates of daily relative response ratios. These estimates are subsequently used as inputs to the second-stage Callaway–Sant'Anna estimator to compute the Average Treatment Effect on the Treated (ATT).

This process is repeated 1,000 times, each time drawing a new bootstrap sample at the state level. From the resulting distribution of ATT estimates, we compute bias-corrected point estimates and confidence intervals.

Table 11 presents the bootstrap results for African American identities. Across all specifications, the estimated treatment effects remain consistently positive and statistically significant. These results confirm that our findings are robust to potential estimation error introduced by the first-stage discrimination coefficients.

7 Mechanisms and Heterogeneity

Having established that racial disparities in inquiry responses widened during eviction moratoria, we turn to the mechanisms driving this effect. The analysis in this section evaluates whether limits on contract enforceability, specifically the suspension of eviction, shifted property managers' screening decisions toward informal mechanisms. We examine heterogeneity by applicant gender, proxies for discriminatory preferences, tenant-protection laws, political affiliation, and neighborhood characteristics. These dimensions allow us to assess whether the observed patterns are more consistent with taste-based discrimination or with risk-based screening, and to evaluate the role of enforcement frictions in shaping discriminatory behavior.

7.1 Gender Discrimination

Table 12: Gender Heterogeneity in the Effect of Moratorium Expiration

	$h = 7$		$h = 10$		$h = 15$	
	Male	Female	Male	Female	Male	Female
Panel A: 30 days around treatment						
ATT	0.033	0.031	0.041**	0.027	0.034**	0.020
	(-0.010, 0.077)	(-0.026, 0.087)	(0.003, 0.079)	(-0.024, 0.077)	(0.002, 0.066)	(-0.023, 0.062)
Observations	770	770	770	770	770	770
Panel B: 45 days around treatment						
ATT	0.095***	0.058**	0.085***	0.051**	0.063***	0.037**
	(0.051, 0.139)	(0.012, 0.105)	(0.049, 0.121)	(0.010, 0.092)	(0.033, 0.092)	(0.004, 0.070)
Observations	1217	1217	1217	1217	1217	1217
Panel C: 60 days around treatment						
ATT	0.102***	0.072***	0.090***	0.062***	0.065***	0.044***
	(0.052, 0.152)	(0.028, 0.117)	(0.051, 0.128)	(0.025, 0.099)	(0.034, 0.095)	(0.014, 0.073)
Observations	1652	1652	1652	1652	1652	1652

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit. 3) 95% bootstrapped bias-corrected confidence intervals are reported in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 12 reports estimates separately by applicant gender. Across specifications, the estimated effects of moratorium expiration are positive for both male and female applicants, indicating higher relative response rates once enforcement is restored. Although the differences across genders are not statistically significant, point estimates are consistently larger for male applicants, particularly in specifications with wider event windows.

Consistent with prior evidence that discriminatory screening in housing markets is more severe for men of color, we focus on male applicants in the remainder of the paper.

7.2 Statistical or Taste-Based Discrimination?

Table 13: Staggered DiD Estimates of the Effect of the End of Moratorium on the Natural Logarithm of the Relative Response Ratio for African Americans Relative to White Applicants by Pre-Pandemic Rent

	Rent Above Median			Rent Below Median		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment					
ATT	0.147***	0.120***	0.072***	-0.012	-0.011	-0.010
	(0.096, 0.197)	(0.080, 0.160)	(0.047, 0.098)	(-0.051, 0.028)	(-0.044, 0.021)	(-0.038, 0.018)
Number of Observations	172	172	172	491	491	491
	Panel B: 45 days around treatment					
ATT	0.096***	0.095***	0.062***	0.076***	0.071***	0.055***
	(0.036, 0.157)	(0.041, 0.148)	(0.017, 0.108)	(0.031, 0.122)	(0.035, 0.108)	(0.025, 0.085)
Number of Observations	389	389	389	811	811	811
	Panel C: 60 days around treatment					
ATT	0.072	0.075	0.055	0.096***	0.086***	0.062***
	(-0.032, 0.176)	(-0.019, 0.170)	(-0.024, 0.134)	(0.050, 0.142)	(0.047, 0.124)	(0.029, 0.094)
Number of Observations	493	493	493	1085	1085	1085

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% bootstrapped bias-corrected confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

We next turn to the first test of the mechanism underlying the increase in discrimination documented above. The theoretical framework delivers a sharp distinction between statistical and taste-based discrimination. Under statistical discrimination, the tightening of screening following moratorium expiration should scale with the economic value of the unit, $\bar{V}$, which is increasing in rent (equation (7)). By contrast, under taste-based discrimination, the impact of enforcement constraints on screening need not vary systematically with $\bar{V}$ (equation (6)).

We test these predictions by splitting the sample based on pre-pandemic rents from Zillow and re-estimating our preferred staggered DiD specification separately for above- and below-median rent markets. Table 13 reports the results for African American applicants.

Across event windows, we find no systematic evidence that treatment effects are larger in higher-rent markets, as implied by statistical discrimination. In the 45-day window, treatment effects are positive and of similar magnitude across rent segments, with overlapping confidence intervals. In the 60-day window, effects persist and remain statistically significant in below-median rent markets, while estimates in higher-rent markets attenuate and lose precision. This pattern runs counter to the prediction that weakened enforcement should induce the strongest screening responses precisely where the financial stakes are highest.

Instead, the evidence is consistent with a taste-based mechanism, in which discrimination intensifies when the economic cost of excluding minority applicants is lower. Taken together, this first mechanism test suggests that the increase in discrimination during eviction moratoria reflects prejudice-driven screening responses rather than risk-based updating tied to unit value.

7.3 Racial Animus Index

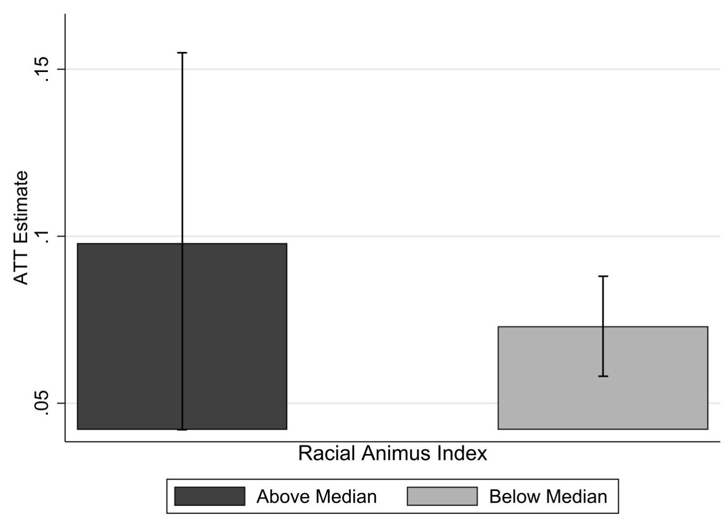


Figure 3: Discrimination Is Worse in States with High Racial Animus Index

As a complementary test of the taste-based discrimination mechanism, we examine whether the response to eviction moratoria varies with measures of racial animus. While the rent-based heterogeneity analysis isolates the role of economic incentives, this exercise instead probes variation in discriminatory preferences across locations.

Specifically, we use the Racial Animus Index developed by Stephens-Davidowitz (2013), which aggregates racially charged internet search activity to capture latent prejudice at the state level. Prior work shows that this index predicts a range of racially discriminatory behaviors, even after controlling for observable socioeconomic characteristics.

Figure 3 plots estimates separately for states with above and below median values of the animus index, with corresponding estimates reported in Appendix Table A7. Consistent with a taste-based mechanism, the increase in discriminatory screening during eviction moratoria is larger in states exhibiting higher measured racial animus. While the differences across animus groups are not statistically significant, the pattern of larger point estimates in high-animus states is directionally consistent with the theoretical prediction that enforcement constraints amplify pre-existing prejudicial preferences.

Taken together with the rent-based heterogeneity results, these patterns are consistent with enforcement constraints enabling preference-based screening, rather than reflecting purely risk-based updating that scales with unit value. The welfare implications are first-order. As we show in Section 8.2, moratoria materially increased financial losses for property owners through delayed and foregone rental cash flow. At the same time, by restricting market access, taste-based discrimination can delay housing search, limit neighborhood choice, and weaken access to jobs and amenities for minority renters. Over time, these constraints can generate persistent disparities in observed outcomes and feed back into beliefs, potentially creating the conditions under which statistical discrimination becomes self-reinforcing (Phelps, 1972) and creates long-term racial disparities.

7.4 Tenant Protections

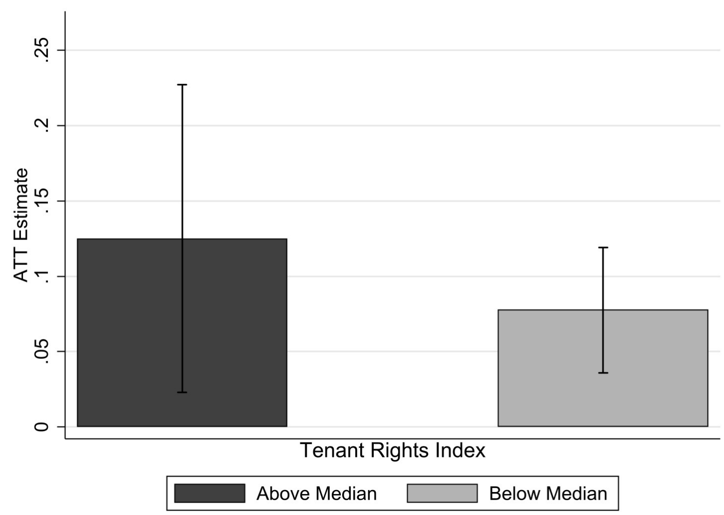


Figure 4: Estimates for States with Tenant Rights Index Above and Below Median

We next examine whether the response to eviction moratoria varies systematically with baseline legal constraints on landlords' ability to enforce rental contracts. To proxy for baseline constraints on formal enforcement, we use the tenant rights protection index developed by Coulson, Le, Ortego-Marti, and Shen (2025), which summarizes state-level legal provisions governing eviction procedures and tenant rights and is described in Section 3.

Figure 4 and Table A8 show the increase in discriminatory screening during eviction moratoria is larger in states with higher values of this index (the estimate on the left corresponds to a higher value of the index). At first glance, this pattern may appear counterintuitive, as tenant-protective legal environments are often associated with reduced landlord discretion. However, this heterogeneity is consistent with a contractual-enforcement mechanism. In states with stronger tenant protections, landlords already operate with limited scope for formal eviction and longer expected enforcement timelines. In tenant-protective environments, moratoria remove the last remaining formal enforcement option, forcing landlords to rely almost exclusively on ex ante screening. We interpret this pattern as suggestive evidence that discrimination operates as a substitute for weakened contract enforcement.

7.5 Political Affiliation

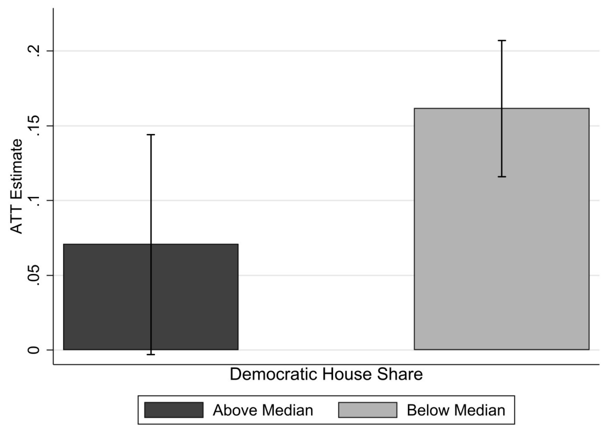


Figure 5: Heterogeneity by Political Affiliation

If the amplification of discrimination under eviction moratoria operates through weakened formal contract enforcement, the effect should be stronger in institutional environments where regulatory oversight of fair housing statutes is plausibly less stringent. We therefore examine heterogeneity in the treatment effect by state legislative partisan composition, measured by the Republican seat share in the lower chamber.

We split states at the median Republican seat share and re-estimate our CSDiD specification separately for above- and below-median groups. Figure 5 reports the average treatment effect on the treated (ATT) for the log relative response ratio in each subsample.

In states with above-median Republican representation, the ATT is positive and statistically significant following moratoria expiration, indicating that restoring eviction as a formal enforcement mechanism is associated with a reduction in the racial response gap. In states with below-median Republican representation, the corresponding ATTs are smaller and not statistically distinguishable from zero over the same post-expiration window.

These findings are consistent with a political economy channel in which the institutional environment shapes screening responses when eviction is temporarily suspended. If partisan composition proxies for differences in regulatory oversight or enforcement intensity, the larger response in Republican-leaning legislatures suggests that formal contract enforcement plays a relatively more important constraining role in those environments.

7.6 Neighborhood Characteristics

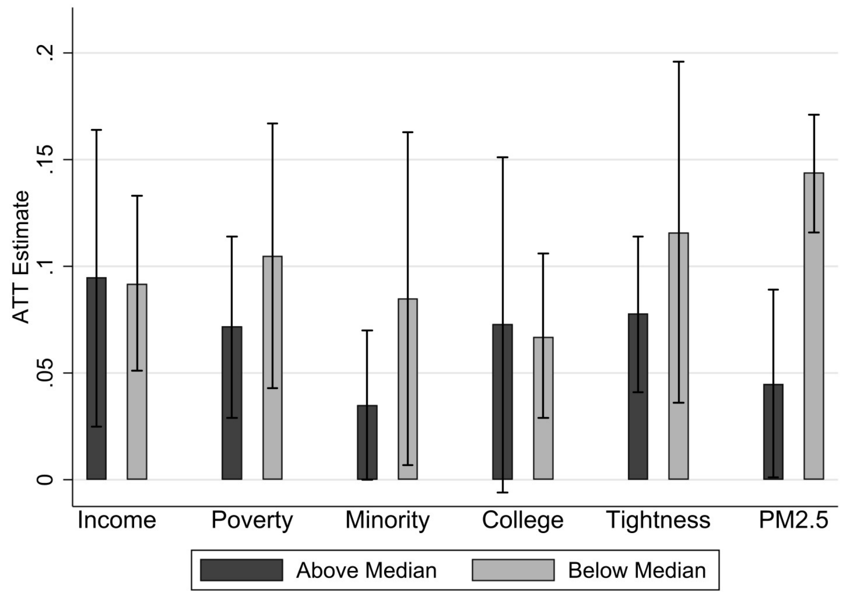


Figure 6: Heterogeneity by Neighborhood Characteristics

This section examines whether the increase in discriminatory screening during the eviction moratoria varies systematically with neighborhood characteristics. While our baseline estimates capture average effects across markets, heterogeneity by neighborhood context can shed light on the mechanisms through which weakened enforcement translates into differential treatment.

Figure 6 reports average treatment effects estimated separately for subsamples defined by whether neighborhoods lie above or below the median value of several pre-pandemic characteristics: median household income, poverty rate, minority population share, college-educated share, rental vacancy rate, and ambient air quality, measured by fine particulate matter concentrations (PM2.5). With the exception of air quality, we find little evidence that the magnitude of additional discrimination during the moratoria varies across these dimensions. In particular, the estimated effects are similar across neighborhoods that differ substantially in income, demographic composition, educational attainment, and baseline rental market tightness.

By contrast, we observe a pronounced difference along the environmental dimension. Neighborhoods with better air quality, defined by the PM2.5 concentrations below the median, exhibit a significantly larger increase in discriminatory responses during the eviction moratoria than neighborhoods with worse air quality. This pattern is consistent with an environmental-justice interpretation: areas that are relatively advantaged along environmental dimensions experience a sharper increase in exclusion when formal enforcement is suspended.

8 Consequences of Increased Discrimination

The preceding analysis shows that property managers changed their screening behavior when eviction enforcement was suspended. An important question is whether these screening decisions have real consequences for housing allocation and market outcomes. In this section, we examine two implications. First, we test whether differential responses in the correspondence experiment translate into systematic differences in who ultimately rents the units. Second, we evaluate the financial consequences of discriminatory screening for property owners, who rely on property managers as delegated agents. Together, these analyses link the screening behavior documented above to both allocation outcomes in the rental market and the economic returns to housing assets.

8.1 Housing Outcomes

To examine whether the discrimination observed in our correspondence experiment affects renters' subsequent housing outcomes, we link the experimental sample to the residential historical InfoUSA database, compiled by Data Axle. This dataset covers approximately 309 million individuals (effectively the entire U.S. population) and contains address histories along with demographic and economic attributes, including age, gender, race/ethnicity, marital status, number of children, estimated wealth and income, and length of residence at the current address. We locate 87.3% of the addresses from the rental listings in the experiment in the addresses from the InfoUSA database. Appendix D details the procedure.

Using information on race/ethnicity and residential tenure from InfoUSA, we identify newly moved-in renters with varied races including white, African-American, and Hispanic.

Specifically, we exclude homeowners and renters who moved-in before the inquiry date from the experiment or renters of "Other" race, yielding a final sample of 3,326 matched addresses. Appendix Table A9 reports the distribution of tenant race at these addresses. African American and Hispanic renters each account for roughly 12 percent of the sample, White renters comprise 55 percent, and 6.6 percent are of other races (e.g., Asian). The final 14 percent of matched addresses contain multiple renter households of different races (e.g., both White and African American families). These addresses are typically multi-family properties that advertise one address but offer multiple rental units for lease.

To examine whether actual move-in patterns are systematically aligned with the racial identity of the fictitious renter in the experiment, we implement an outcome test in the spirit of Becker (1957, 1993). Specifically, we construct the variable Same Race Share, which for each inquiry equals the fraction of tenants at the matched address whose race coincides with that of the fictitious renter. In most cases the measure takes values of zero or one, but for addresses with multiple renter households of different races, it can take intermediate values reflecting the share of tenants of the same race as the inquiry identity.

To assess whether property managers' screening decisions from the experiment affect the observed racial composition of tenants, we estimate the following equation for each state-day cell:

$$\text{Share Same Race}_{ijsm} = \alpha + \beta \text{Response}_{ijst} + X_j' \theta_{s\tau} + \varepsilon_{ijst},$$

where i indexes properties, j fictitious identities, s states, and t dates, consistent with (8). The left-hand side variable $\text{Share Same Race}_{ijsm}$ is measured at the subsequent move-in period $m > t$, which occurs after the inquiry date t. The parameter β describes the effect of a receiving a response from a property manager on the inquiry of a particular racial identity on the likelihood of an actual individual with that identity showing up in the apartment.

We estimate this specification using the quasi-maximum likelihood fractional logit procedure of Papke and Wooldridge (1996), weighted as in (9), to accommodate the bounded fractional outcome.

We use the estimated coefficients to predict the share of same-race tenants under two counterfactuals: when a manager responds to an inquiry ($\text{Response}_{ijst} = 1$) and when no response is sent ($\text{Response}_{ijst} = 0$). We then construct the natural logarithm of the relative response ratio for each state s and date t:

$$\ln(RR_{st}) = \ln(\text{Share Same Race}_{st} \big| \text{Response} = 0) - \ln(\text{Share Same Race}_{st} \big| \text{Response} = 1).$$

We take the natural log transformation and clamp predicted shares to the interval $(0.001, 0.999)$ to mitigate instability from extreme values. As a robustness check, we also re-estimate the specification using an alternative dependent variable—the risk difference, defined as the difference in predicted same-race shares between responses and non-responses, $(\text{Share Same Race}_{st} \big| \text{Response} = 0) - (\text{Share Same Race}_{st} \big| \text{Response} = 1)$, and obtain similar results, reported in Table A11 and Figure A3 in the Appendix.

In the second stage, we apply the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021) to the $\ln(RR_{st})$ series, exploiting the staggered expiration of eviction moratoria across states. This design allows us to test whether the likelihood that tenants match on race worsened during moratoria due to screening behavior.

Table 14: Staggered DiD Estimates of the Effect of the End of Moratorium on the Natural Logarithm of the Ratio of the Predicted Share of Same Race after No Response to the Predicted Share of Same Race after a Response

	(1)	(2)	(3)
	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment		
ATT	0.411	0.575**	0.285***
	(-0.194, 1.016)	(0.075, 1.076)	(0.082, 0.488)
Number of Observations	715	735	746
	Panel B: 45 days around treatment		
ATT	0.728**	0.676***	0.286***
	(0.149, 1.308)	(0.204, 1.148)	(0.102, 0.470)
Number of Observations	1122	1170	1192
	Panel C: 60 days around treatment		
ATT	0.754***	0.619***	0.283***
	(0.186, 1.323)	(0.163, 1.075)	(0.113, 0.454)
Number of Observations	1526	1582	1621

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weight in the quasi-maximum likelihood procedure with the logit link, proposed by Papke and Wooldridge (1996), see the text. 3) 95% confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 14 and Figure 7 present the results. We find that the expiration of eviction moratoria significantly increased the relative response ratio, indicating that racial disparities in property managers' screening decisions narrowed once enforcement authority was restored. The magnitudes are economically meaningful: for example, the estimate of 0.283 implies that the relative response ratio rose by approximately 33 percent ($100 \times (e^{0.283} - 1)$) following the end of a moratorium. In other words, property managers' non-responses during the moratorium disproportionately reduced the likelihood of same-race matches, and this gap diminished once enforcement constraints were lifted.

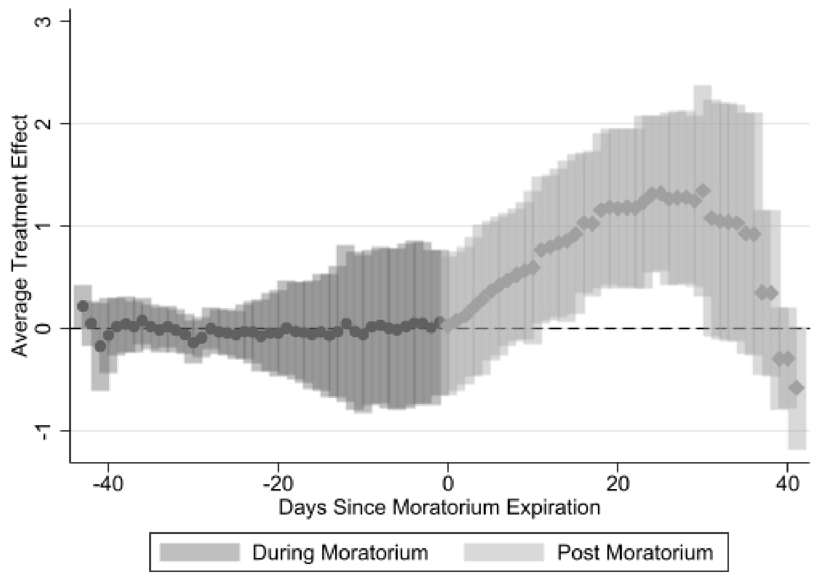


Figure 7: Event Study Coefficients from Callaway and Sant'Anna (2021)'s Estimator for the Natural Logarithm of the Relative Response Ratios for the Same Race without a Response Relative to the Share of Same Race with a Response with the Smoothing Parameter $h = 10$ and $\hat{\tau} = 45$ Days around Treatment

These findings from the outcome test align closely with our benchmark test based on response rates. As emphasized by Gaebler and Goel (2025), when both benchmark and outcome tests point in the same direction, the conclusion of discrimination is statistically robust under mild assumptions. In our setting, the convergence of evidence across both tests implies that property managers not only differentially screened minority applicants during eviction moratoria but that these decisions also translated into systematically different rental outcomes.

8.2 Financial Losses of Property Owners

The previous section discussed the implications of discriminatory screening for minority renters' access to housing. Because property managers act as delegated agents for property owners, these screening decisions also generate direct financial consequences for owners through foregone leasing opportunities and increased vacancy risk. In this section, we provide a back-of-the-envelope calculation of the implied financial losses.

Section 4.3 shows that during eviction moratoria, the relative response ratio for African American applicants relative to white applicants was 0.911. Our staggered difference-in-differences estimates imply that, absent the moratorium, the log relative response ratio would have been higher by approximately 0.09, corresponding to a counterfactual relative response ratio of $0.911 \times \exp(0.09) \approx 0.997$. Given that white applicants in our sample receive responses with probability 0.5978, this implies a counterfactual response probability for African American applicants of $0.997 \times 0.5978 = 0.5960$. By contrast, the observed response probability during moratoria is 0.5448, implying a gap of $0.5960 - 0.5448 = 0.0512$. Thus, eviction moratoria reduced the probability of responding to an inquiry from an African American applicant by approximately 5.1 percentage points relative to a counterfactual with normal eviction enforcement.

To translate this gap into economic losses, we make a set of conservative assumptions. Each rental listing receives between 22 and 30 inquiries over its listing period, and the inquiry-to-lease conversion rate is approximately 0.6%.[3] Interpreting a missed response as a foregone leasing opportunity at this margin, and assuming a monthly rent of \$2,000, the implied annual revenue loss per listing is $22 \times 0.006 \times \$2{,}000 \times 12 \times 0.0512 \approx \$162$.

We next scale this loss to the property manager level. Industry data suggest that approximately half of rental units are relisted each year and that a typical property manager oversees between 100 and 150 units.[4] Applying the conservative lower bound of 100 units

[3]The inquiry volume is based on industry reports summarized in ShowMojo's Q2 2025 market overview (https://hello.showmojo.com/blog/the-rental-market-in-q2-2025-its-a-great-time-to-double-down-on-efficiency/). The inquiry-to-lease conversion rate is a lower bound drawn from industry benchmarks reported by Follow Up Boss (https://www.followupboss.com/blog/real-estate-lead-conversion-rate).

[4]The annual relisting rate is drawn from industry turnover statistics reported by Nspire Experts (https://nspireexperts.com/apartment-turnover-rate/). The number of units managed per property

with 50 relistings per year implies an annual revenue loss of roughly \$8,100 per property manager.

Finally, to express this loss in asset value terms, we capitalize the annual flow loss using a 5% capitalization rate, consistent with prevailing multifamily benchmarks.[5] This yields an implied reduction in property value of approximately

$$\$162 \times 0.5 \times 100/0.05 = \$162{,}000$$

per property manager. While approximate, this calculation illustrates that discrimination induced by weakened eviction enforcement can generate economically meaningful losses for property owners, in addition to its distributional consequences for renters.

9 Conclusion

This paper studies how weakening contract enforcement in rental markets affects screening, allocation, and welfare. We combine a hand-collected dataset on state eviction moratoria with a nationwide correspondence experiment and exploit the staggered timing of moratorium expiration to identify how the temporary suspension of eviction enforcement changed property managers' behavior. We show that discrimination against minority applicants, especially African American applicants, increased when eviction was not enforceable and declined once enforcement authority was restored. These effects are robust across event windows, control-group definitions, alternative staggered DiD estimators, and bootstrap inference that accounts for the two-stage estimation procedure.

The evidence points to contract enforcement as the key channel. We find no evidence that moratorium expiration changed the number of rental listings or the observable characteristics of listed units, making other market dynamics or changes in housing supply composition an unlikely explanation. We also find that expiration timing is not systematically related to contemporaneous COVID-19 dynamics or pre-determined state characteristics, and the results are robust to matching on COVID-19 case rates and to splitting the sample before and after the murder of George Floyd. In addition, the estimates remain similar during the

manager is based on industry statistics compiled by DoorLoop from public and proprietary sources (https://www.doorloop.com/blog/property-management-industry-stats).

[5] The capitalization rate is taken from Fannie Mae Economic and Strategic Research Group (2020).

federal CARES Act period, consistent with our interpretation that what matters is not the formal existence of a moratorium per se, but whether eviction remains a credibly enforceable contract remedy.

Our mechanism tests are more consistent with taste-based discrimination than with statistical discrimination. In the model, statistical discrimination predicts stronger treatment effects in higher-rent markets, where the option value of the unit is larger. Empirically, we do not find this pattern. Instead, the increase in discriminatory screening during moratoria is not systematically larger in high-rent markets and is directionally stronger in places with greater racial animus. The effect is also larger in states with stronger baseline tenant protections, consistent with discrimination acting as a substitute for formal enforcement in environments where landlords already face tighter legal limits on eviction. Taken together, these findings suggest that moratoria amplified prejudice-driven screening rather than merely inducing landlords to respond mechanically to expected default risk.

The consequences extend beyond inquiry responses. Linking the experiment to residential address histories, we show that non-response during moratoria translated into systematically different realized tenant composition at matched addresses. The expiration of moratoria increased the relative likelihood of same-race tenant matches by economically meaningful amounts, implying that discriminatory screening affected who ultimately occupied rental units. Because both the benchmark screening test and the outcome test point in the same direction, the framework of Gaebler and Goel (2025) implies that the evidence of discrimination is statistically robust.

The welfare implications run in both directions. For incumbent tenants already in place, eviction moratoria can provide valuable short-run insurance against displacement. But for prospective tenants searching for housing, the same policies can tighten access by inducing property managers to screen more aggressively and, in our setting, more discriminatorily. These effects are especially important for minority renters, who face lower response rates during moratoria and therefore may experience longer search and fewer options. Our heterogeneity results further suggest that these access distortions may be particularly consequential in environmentally advantaged neighborhoods, where the increase in discrimination is larger. At the same time, discriminatory screening is not costless for owners. Our back-of-the-envelope calculations imply that foregone leasing opportunities can generate economically meaningful losses in rental income and property value, thereby reducing owners' expected returns, indicating that the behavioral response to weakened enforcement may harm principals as well as excluded tenants.

More broadly, our findings suggest that the welfare evaluation of eviction policy cannot focus only on tenants currently protected from removal. It must also account for equilibrium effects on access to vacant units, intermediary behavior, and the distribution of housing opportunities across groups. In this respect, our evidence complements the quantitative work of Corbae, Glover, and Nattinger (2023), who show in a calibrated general equilibrium search model with equilibrium evictions that a full moratorium is not an optimal policy even without incorporating discrimination. The discriminatory channel we document here provides an additional and previously unmeasured reason why broad suspensions of eviction enforcement may be socially costly.

These results point to two directions for policy. First, future research should quantify the full welfare trade-off in a structural model that jointly incorporates tenant insurance, landlord behavior, search frictions, and discrimination. Second, if policymakers seek to protect tenants during periods of distress, complementary market-design interventions may be needed to limit discriminatory screening at the front end of the rental search process. One possibility is to make initial inquiries more race-agnostic by concealing identifying information at the inquiry stage and restricting the visibility of personal details until later in the matching process, in closer alignment with Fair Housing principles and with platform designs that reduce direct exposure to identity signals. More generally, policies that preserve tenant protections while limiting the scope for discretionary, identity-based screening may perform better than blunt enforcement suspensions alone.

The central lesson of the paper is that contract enforcement shapes not only ex post landlord remedies but also ex ante market access. When eviction ceases to function as a credible enforcement mechanism, property managers substitute toward informal screening tools, and those tools can amplify racial discrimination. Any evaluation of tenant-protection policy should therefore take seriously not only the benefits for households already housed, but also the costs imposed on households still searching for a place to live.

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Appendix

A Proofs

The value of searching for a tenant for an empty unit is

$$\begin{aligned}
V &= \mathbb{E}\max\{u_M-\psi, u_W\} = \mathbb{E}[\mathbb{1}_{\{u_M-\psi\geq u_W\}}(u_M-\psi)+\mathbb{1}_{\{u_M-\psi<u_W\}}u_W] = \\
&= P(u_M-\psi\geq u_W)u_M - \mathbb{E}[\psi|u_M-\psi\geq u_W] + (1-P(u_M-\psi\geq u_W))u_W = \\
&= u_W + \Delta u F(\Delta u) - \int_{\psi_{\min}}^{\Delta u}\psi dF(\psi),
\end{aligned}$$

where $\Delta u \equiv u_M - u_W$. To simplify, use integration by parts to rewrite the last term as

$$\int_{\psi_{\min}}^{\Delta u}\psi dF(\psi) = \psi F(\psi)|_{\psi_{\min}}^{\Delta u} - \int_{\psi_{\min}}^{\Delta u}F(\psi)d\psi = \Delta u F(\Delta u) - \int_{\psi_{\min}}^{\Delta u}F(\psi)d\psi,$$

where $\lim_{\psi\to\psi_{\min}}\psi F(\psi)=0$ by assumption. Then the property manager's value of searching for a tenant for an empty unit is

$$V = u_W + \int_{\psi_{\min}}^{\Delta u}F(\psi)d\psi.$$

To derive the difference in the differences of the utilities, calculate the utility from leasing of an applicant i after and during the moratorium from (1) and (4) as

$$\begin{aligned}
\bar{u}_i &= \frac{\pi_i R}{1-\beta\pi_i} + \frac{(1-\pi_i)\beta^2}{1-\beta\pi_i}\bar{V} - \frac{((1-\pi_i)\beta+1)}{1-\beta\pi_i}\kappa_i, \\
u_i^* &= \frac{\pi_i R}{1-\beta\pi_i} + \frac{(1-\pi_i)\beta}{1-\beta\pi_i}V^* - \frac{1}{1-\beta\pi_i}\kappa_i.
\end{aligned}$$

To derive $\Delta^2 u = (\bar{u}_M - \bar{u}_W) - (u_M^* - u_W^*) = (\bar{u}_M - u_M^*) - (\bar{u}_W - u_W^*)$, we start with calculating $\bar{u}_i - u_i^*$:

$$\bar{u}_i - u_i^* = \beta\frac{(1-\pi_i)}{1-\beta\pi_i}(\beta\bar{V} - V^*) - \frac{\beta(1-\pi_i)}{1-\beta\pi_i}\kappa_i. \tag{11}$$

Then the difference in the differences of the utilities is

$$\begin{aligned}\Delta^2 u &= (\bar{u}_M - u_M^*) - (\bar{u}_W - u_W^*) = \beta(\frac{(1-\pi_M)}{1-\beta\pi_M} - \frac{(1-\pi_W)}{1-\beta\pi_W})(\beta\bar{V} - V^*) - \frac{\beta(1-\pi_M)}{1-\beta\pi_M}\kappa \\ &= \frac{\beta(1-\beta)(\pi_W - \pi_M)}{(1-\beta\pi_M)(1-\beta\pi_W)}(\beta\bar{V} - V^*) - \frac{\beta(1-\pi_M)}{1-\beta\pi_M}\kappa, \end{aligned} \tag{12}$$

where we the first equality uses normalization $\kappa_M = \kappa$ and $\kappa_W = 0$. The second equality uses

$$\begin{aligned}&\frac{(1-\pi_M)}{1-\beta\pi_M} - \frac{(1-\pi_W)}{1-\beta\pi_W} = \frac{1-\beta\pi_W - \pi_M + \pi_M\beta\pi_W - 1 + \beta\pi_M + \pi_W - \pi_W\beta\pi_M}{(1-\beta\pi_M)(1-\beta\pi_W)} \\ &= \frac{-\beta\pi_W - \pi_M + \beta\pi_M + \pi_W}{(1-\beta\pi_M)(1-\beta\pi_W)} = \frac{(1-\beta)(\pi_W - \pi_M)}{(1-\beta\pi_M)(1-\beta\pi_W)}.\end{aligned}$$

To access how the option value to lease changes, use

$$\begin{aligned}V &= u_W + \int_{\psi_{\min}}^{\Delta u} F(\psi)d\psi, \\ \bar{V} - V^* &= \bar{u}_W - u_W^* + \int_{\psi_{\min}}^{\Delta\bar{u}} F(\psi)d\psi - \int_{\psi_{\min}}^{\Delta u^*} F(\psi)d\psi.\end{aligned}$$

We use the first-order Taylor expansion to approximate $\int_{\psi_{\min}}^{\Delta\bar{u}} F(\psi)d\psi - \int_{\psi_{\min}}^{\Delta u^*} F(\psi)d\psi = (\Delta\bar{u} - \Delta u^*) \cdot F(\Delta u^*) = \Delta^2 u \cdot F(\Delta u^*)$ and $\bar{u}_W - u_W^*$ from (11):

$$\bar{V} - V^* = \beta\frac{(1-\pi_W)}{1-\beta\pi_W}(\beta\bar{V} - V^*) + F(\Delta u^*)\Delta^2 u.$$

Use the relationship above to find the option value to lease during the eviction moratorium:

$$\bar{V}(1 - \frac{\beta^2 - \beta^2\pi_W}{1-\beta\pi_W}) = (1 - \frac{\beta - \beta\pi_W}{1-\beta\pi_W})V^* + F(\Delta u^*)\Delta^2 u,$$

where $(1 - \beta\pi_W - \beta^2 + \beta^2\pi_W) = ((1-\beta^2) - \beta\pi_W(1-\beta)) = (1-\beta)(1+\beta-\beta\pi_W)$. Thus,

$$\begin{aligned}&(1-\beta)(1+\beta(1-\pi_W))\bar{V} = (1-\beta)V^* + (1-\beta\pi_W)F(\Delta u^*)\Delta^2 u, \\ &V^* = (1+\beta(1-\pi_W))\bar{V} - \frac{1-\beta\pi_W}{1-\beta}F(\Delta u^*)\Delta^2 u.\end{aligned}$$

To finish calculation of $\Delta^2 u$ from (12), we need $\beta\bar{V} - V^*$:

$$\beta\bar{V} - V^* = \beta\bar{V} - (1+\beta(1-\pi_W))\bar{V} + \frac{1-\beta\pi_W}{1-\beta}F(\Delta u^*)\Delta^2 u =$$
$$= (\beta - 1 - \beta + \beta\pi_W))\bar{V} + \frac{1-\beta\pi_W}{1-\beta}F(\Delta u^*)\Delta^2 u = -(1-\beta\pi_W)\bar{V} + \frac{1-\beta\pi_W}{1-\beta}F(\Delta u^*)\Delta^2 u.$$

Using the above change in the option value to lease in (12), we get

$$\Delta^2 u = \frac{\beta(1-\beta)(\pi_W-\pi_M)}{(1-\beta\pi_M)(1-\beta\pi_W)}(\frac{1-\beta\pi_W}{1-\beta}F(\Delta u^*)\Delta^2 u - (1-\beta\pi_W))\bar{V}) - \frac{\beta(1-\pi_M)}{1-\beta\pi_M}\kappa,$$
$$(1 - \frac{\beta(1-\beta)(\pi_W-\pi_M)}{(1-\beta\pi_M)(1-\beta\pi_W)}\frac{(1-\beta\pi_W)}{(1-\beta)}F(\Delta u^*))\Delta^2 u =$$
$$= -\frac{\beta(1-\beta)(\pi_W-\pi_M)}{(1-\beta\pi_M)(1-\beta\pi_W)}(1-\beta\pi_W)\bar{V} - \frac{\beta(1-\pi_M)}{1-\beta\pi_M}\kappa,$$
$$(1 - \frac{\beta(\pi_W-\pi_M)}{(1-\beta\pi_M)}F(\Delta u^*))\Delta^2 u = -\frac{\beta(1-\beta)(\pi_W-\pi_M)}{(1-\beta\pi_M)}\bar{V} - \frac{\beta(1-\pi_M)}{1-\beta\pi_M}\kappa.$$

We now can assess how the moratorium affects the difference in utilities, i.e. determine the sign of $\Delta^2 u$. The right-hand side is negative under discrimination of any type including a mix of taste-based and statistical discrimination. The multiplier of $\Delta^2 u$ is positive because $F(\Delta^* u) < 1$ and $\beta\pi_W - \beta\pi_M < 1 - \beta\pi_M$.

We can further analyze special cases. If we have statistical discrimination $\pi_W > \pi_M$ and $\kappa = 0$, the moratorium increases discrimination:

$$(1 - \frac{\beta(\pi_W-\pi_M)}{(1-\beta\pi_M)}F(\Delta u^*))\Delta^2 u = -\frac{\beta(1-\beta)(\pi_W-\pi_M)}{(1-\beta\pi_M)}\bar{V}.$$
$$\Delta^2 u = -\frac{\beta(1-\beta)(\pi_W-\pi_M)}{(1-\beta\pi_M-\beta(\pi_W-\pi_M)F(\Delta u^*))}\bar{V} < 0.$$

In a special case of taste-based discrimination, we have $\pi_M = \pi_W = \pi$, $\kappa > 0$, and

$$\Delta^2 u = -\frac{\beta(1-\pi)}{1-\beta\pi}\kappa < 0,$$

arriving at the same conclusion.

B Baseline Discrimination Specification

Table A1: Estimates from the Baseline Discrimination Specification on the Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Model	Linear	Linear	Linear	Linear	Probit	Logit
African American	-0.056***	-0.056***	-0.056***	-0.057***	-0.145***	-0.233***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.020)	(0.031)
Hispanic	-0.028***	-0.028***	-0.028***	-0.028***	-0.073***	-0.118***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.020)	(0.032)
Male		-0.035***	-0.035***	-0.035***	-0.089***	-0.143***
		(0.006)	(0.006)	(0.006)	(0.016)	(0.026)
Less Than High School			-0.000	-0.000	-0.000	-0.001
			(0.008)	(0.008)	(0.020)	(0.032)
High School Graduate			-0.032***	-0.031***	-0.080***	-0.129***
			(0.008)	(0.008)	(0.020)	(0.031)
Inquiry Order = 2				-0.029***	-0.075***	-0.120***
				(0.008)	(0.020)	(0.032)
Inquiry Order = 3				-0.050***	-0.129***	-0.207***
				(0.008)	(0.020)	(0.031)
Constant	0.605***	0.623***	0.633***	0.660***	0.407***	0.653***
	(0.005)	(0.006)	(0.008)	(0.009)	(0.023)	(0.037)
Observations	25,055	25,055	25,055	25,055	25,055	25,055
R-squared	0.002	0.003	0.004	0.006	-	-

Notes: 1) Table reports coefficients from a within-property linear regression model in columns (1)-(4), probit model in column (5), and logit model in column (6). 2) The outcome variable is an indicator of whether a response was received from the property manager. 3) The mean response to a white identity is 0.605 over the whole sample. 4) Standard errors in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

In this section, we document the presence of racial and ethnic discrimination in our sample. It is a necessary condition for such disparities to be exacerbated by eviction moratoria, according to our theoretical analysis in Section 2.

The experimental design described in Section 3 generates a series of binary decisions j for each property i, where the manager chooses whether to respond to an inquiry ($Response_{ij} = 1$) or not ($Response_{ij} = 0$), with $j = 1, 2, 3$. To quantify baseline disparities in manager responses, we estimate the following model with property fixed effects:

$$Response_{ij} = \delta_i + \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + X_j'\theta + \epsilon_{ij}, \tag{13}$$

where $African\ American_j$ and $Hispanic_j$ are indicator variables for the race or ethnicity of applicant j. The vector X_j includes identity-specific characteristics: gender, maternal education, and the order in which the inquiry was sent. Property fixed effects δ_i absorb all time-invariant listing-specific variation, ensuring identification from within-property differences in responses across applicants. Because fictitious names were randomly assigned and balanced across covariates, estimates of β^{AA} and β^{H} are robust to the inclusion or exclusion of X_j.

We estimate equation (13) using the full sample of inquiries across all weeks and states. The number of observations is slightly smaller than the original 25,428 due to the exclusion of some inquiries, e.g., prior to the start of a state's moratorium, as detailed in Section 4.1. Table A1, Columns (1)–(4), report results from the linear probability model. Columns (5) and (6) present marginal effects from corresponding Probit and Logit specifications.

Across all models, we find statistically significant evidence of lower response rates to African American and Hispanic applicants relative to white applicants, consistent with the findings in Christensen, Sarmiento-Barbieri, and Timmins (2022). These results confirm the presence of discriminatory behavior in our sample.

C Difference-in-Differences Specification

Table A2: Impact of an End of a Moratorium on Likelihood of Receiving a Response

	Dependent Variable: Response						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.050***	-0.066***	0.006	0.006	0.006	0.005	0.009
	(0.017)	(0.019)	(0.055)	(0.055)	(0.049)	(0.050)	(0.036)
African American	-0.054***	-0.055***	-0.053***	-0.053***	-0.053***	-0.055***	-0.060***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.012)	(0.011)	(0.013)
African American x Treatment	0.002	0.019	0.002	0.002	0.002	0.004	0.038
	(0.017)	(0.019)	(0.017)	(0.017)	(0.022)	(0.022)	(0.023)
Hispanic	-0.033***	-0.031***	-0.032***	-0.032***	-0.032***	-0.033***	-0.035***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)	(0.009)
Hispanic x Treatment	0.020	0.036*	0.022	0.022	0.022	0.023	0.034
	(0.017)	(0.019)	(0.017)	(0.017)	(0.022)	(0.022)	(0.021)
#Evictions in 2018, thousands		-0.001***					
		(0.000)					
Stringency Index	0.002***	0.002***					
	(0.001)	(0.001)					
COVID Cases per 1k				0.004	0.004	-0.003	0.003
				(0.024)	(0.026)	(0.026)	(0.027)
Constant	0.544***	0.550***	0.614***	0.593***	0.593***	0.674***	0.605***
	(0.039)	(0.042)	(0.017)	(0.125)	(0.134)	(0.126)	(0.151)
Observations	17,734	15,588	17,883	17,883	17,883	18,744	15,620
R-squared			0.025	0.025	0.025	0.025	0.024
Number of address_id	5,977	5,256	6,025	6,025	6,025	6,312	5,261
Gender	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educational Level	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Inquiry Order	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weekly FEs	No	No	Yes	Yes	Yes	Yes	Yes
Property FEs	No	No	Yes	Yes	Yes	Yes	Yes
Clustered at State-level	No	No	No	No	Yes	Yes	Yes
Control Group	Not yet	Not yet	Not yet	Not yet	Not yet	Not	Not yet
Moratoria Data							Benfer et al.

Notes: 1) The outcome variable is an indicator of whether a response was received from the property manager. 2) COVID Cases per 1k is the number of COVID-19 infections in a state on a day per 1,000 people. 3) Row "Control Group" specifies whether control group consists of not-yet-treated states ("Not yet") or not-yet-treated and not-treated states ("Not-treated"). 4) The last column shows the estimates when we use the eviction moratoria data from Benfer et al. (2023). 5) Standard errors in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

We estimate the following DiD specification:

$$\begin{aligned}Response_{ijt} = \delta_i + \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + \beta^{T} Treatment_{jt} \qquad (14)\\ + \beta^{AAT} Treatment_{jt} \times African\ American_j + \beta^{HT} Treatment_{jt} \times Hispanic_j + X_j'\theta + \epsilon_{ijt},\end{aligned}$$

where i indexes rental properties, j indexes applicant identities, and t indexes time (in days). The dependent variable $Response_{ijt}$ equals one if identity j received a response from property i on day t, and zero otherwise. The treatment variable $Treatment_{jt}$ equals one if the inquiry occurs after the expiration of the moratorium in the relevant state.

The variables $African\ American_j$ and $Hispanic_j$ are indicators for the racial or ethnic category of the inquiring identity. The vector X_j includes controls for gender, maternal education, and the order in which the inquiry was sent. Rental property fixed effects δ_i absorb all time-invariant listing-specific variation.

Table A2 presents the results. Columns (1) through (3) include controls for the local eviction policy stringency index (Hale et al. (2021)), historical eviction rates from 2018, and week fixed effects, but omit property fixed effects.[6] Column (4) introduces property fixed effects. Column (5) adds daily COVID-19 cases per 100,000 residents at the state level. Column (6) clusters standard errors by state – the level at which eviction moratoria are enacted and repealed – to account for within-state correlation in unobserved shocks. Column (7) retains the full specification and redefines the control group to include never-treated states. Column (8) shows estimates when we use the eviction moratoria data from Benfer et al. (2023) instead of our hand-collected dataset.

The estimated interaction terms—$Treatment_{jt} \times African\ American_j$ and $Treatment_{jt} \times Hispanic_j$—are positive but not statistically significant. The staggered timing of moratorium terminations across states during the spring and summer of 2020 may introduce bias into the canonical DiD estimator. When treatment varies across time, early-treated units serve as controls for later-treated units, potentially contaminating post-treatment comparisons and attenuating estimates toward zero (Goodman-Bacon, 2021). To address this concern, we use estimator that explicitly accounts for variation in treatment timing, as described in the main text.

[6]Historical eviction rates are obtained from Gromis et al. (2022), the most recent available data prior to the pandemic.

D Matching Experimental Addresses to the InfoUSA Database

We standardized all experimental rental addresses to a common format containing the house number, street name (including directional suffixes), and unit number when available. Six addresses were excluded from the procedure because they were too ambiguous to match (e.g., listings containing only a street name or anonymized house number). For the remaining addresses, we extracted the tracts containing experimental listings from the InfoUSA database and implemented a multi-step matching procedure, summarized in Table A10.

In the first step, we identified 6,077 exact matches between experimental addresses and InfoUSA records, requiring complete agreement on the full address, including directional suffixes. The second step relaxed this criterion by ignoring suffixes and matching on house number and street name, while requiring agreement on unit number when present, yielding an additional 1,199 matches. The third step expanded this search to all counties containing experimental tracts, adding 62 further matches.

Subsequent steps employed approximate matching techniques within the experimental tracts. First, we implemented a fuzzy string match between standardized experimental and InfoUSA addresses. Second, we conducted a spatial match using geocoded coordinates of the experimental listings, requiring agreement on the street name. These approaches were particularly useful for listings reported as intersections (e.g., "Main St and University Ave") rather than full addresses.

In the final step, we queried InfoUSA records directly on house number, street name, and zip code for any remaining unmatched addresses, obtaining 30 additional matches.

Altogether, we successfully matched 7,393 experimental listings (87.3%) to InfoUSA addresses.

E Tables and Figures

Table A3: Metropolitan Statistical Areas in the Experiment

Austin-Round Rock, TX	Nashville-Davidson-Murfreesboro-Franklin, TN
Baltimore-Columbia-Towson, MD	New Orleans-Metairie, LA
Birmingham-Hoover, AL	New York-Newark-Jersey City, NY-NJ-PA
Boston-Cambridge-Newton, MA-NH	Orlando-Kissimmee-Sanford, FL
Buffalo-Cheektowaga-Niagara Falls, NY	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
Charlotte-Concord-Gastonia, NC-SC	Phoenix-Mesa-Scottsdale, AZ
Chicago-Naperville-Elgin, IL-IN-WI	Pittsburgh, PA
Cincinnati, OH-KY-IN	Portland-Vancouver-Hillsboro, OR-WA
Dallas-Fort Worth-Arlington, TX	Providence-Warwick, RI-MA
Denver-Aurora-Lakewood, CO	Raleigh, NC
Detroit-Warren-Dearborn, MI	Richmond, VA
Hartford-West Hartford-East Hartford, CT	Riverside-San Bernardino-Ontario, CA
Houston-The Woodlands-Sugar Land, TX	Sacramento-Roseville-Arden-Arcade, CA
Indianapolis-Carmel-Anderson, IN	Salt Lake City, UT
Jacksonville, FL	San Antonio-New Braunfels, TX
Kansas City, MO-KS	San Diego-Carlsbad, CA
Las Vegas-Henderson-Paradise, NV	San Francisco-Oakland-Hayward, CA
Los Angeles-Long Beach-Anaheim, CA	San Jose-Sunnyvale-Santa Clara, CA
Louisville-Jefferson County, KY-IN	Seattle-Tacoma-Bellevue, WA
Memphis, TN-MS-AR	St. Louis, MO-IL
Miami-Fort Lauderdale-West Palm Beach, FL	Tampa-St. Petersburg-Clearwater, FL
Milwaukee-Waukesha-West Allis, WI	Virginia Beach-Norfolk-Newport News, VA-NC
Minneapolis-St. Paul-Bloomington, MN-WI	Washington-Arlington-Alexandria, DC-VA-MD-WV

Table A4: States in the Experiment

Alabama	Kentucky	North Carolina
Arizona	Louisiana	Oregon
California	Maryland	Pennsylvania
Colorado	Massachusetts	Rhode Island
Connecticut	Michigan	South Carolina
Delaware	Minnesota	Tennessee
District of Columbia	Mississippi	Texas
Florida	Nevada	Utah
Illinois	New Hampshire	Virginia
Indiana	New Jersey	Washington
Kansas	New York	Wisconsin

Table A5: Staggered DiD Estimates of the Effect of the End of Moratorium on the Relative Response Ratio Relative to White Applicants

	African American			Hispanic		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment					
ATT	0.044**	0.041**	0.031**	0.011	0.013	0.010
	(0.003, 0.084)	(0.007, 0.075)	(0.002, 0.060)	(-0.020, 0.043)	(-0.015, 0.041)	(-0.014, 0.035)
Number of Observations	770	770	770	770	770	770
	Panel B: 45 days around treatment					
ATT	0.088***	0.076***	0.054***	0.044**	0.030*	0.010
	(0.053, 0.122)	(0.046, 0.106)	(0.029, 0.080)	(0.006, 0.082)	(-0.002, 0.061)	(-0.017, 0.036)
Number of Observations	1217	1217	1217	1217	1217	1217
	Panel C: 60 days around treatment					
ATT	0.105***	0.084***	0.057***	0.066***	0.046**	0.020
	(0.068, 0.142)	(0.053, 0.115)	(0.032, 0.083)	(0.021, 0.110)	(0.007, 0.085)	(-0.013, 0.052)
Number of Observations	1652	1652	1652	1652	1652	1652

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A6: CoreLogic MLS Rental Listings: Sample Construction

	Sample	# Observations	# Dropped
1	All listings from January 2018 to August 2020	22,096,504	—
2	Rental listings	3,223,536	18,872,968
3	After removing real estate owned (REO)	3,217,397	6,139
4	After removing short sales	3,216,669	728
5	After removing foreclosures	3,212,948	3,721
6	After removing non-residential real estate properties	3,068,476	144,472
7	After removing outliers with extreme characteristics	2,998,364	70,112
8	After removing listings without unique identification #	2,415,059	583,305
9	After removing listings with days on market ≤ 1 or > 4 years	2,367,397	47,662
10	After dropping duplicated listings with the same lease date/price	2,090,292	277,105
11	After merging overlapping listings of the same rental property	1,937,337	152,955
12	After dropping missing or invalid dates	1,069,639	867,698
13	After removing duplicated listings	1,064,237	5,402

Table A7: Staggered DiD Estimates of the Effect of the End of Moratorium on the Natural Logarithm of the Relative Response Ratio for African Americans Relative to White Applicants by Racial Animus Index

	Racial Animus Index					
	Above Median			Below Median		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
Panel A: 30 days around treatment						
ATT	-0.027	-0.028	-0.021	0.141***	0.135***	0.087***
	(-0.070, 0.017)	(-0.063, 0.008)	(-0.052, 0.009)	(0.106, 0.176)	(0.105, 0.164)	(0.063, 0.112)
Number of Observations	444	444	444	232	232	232
Panel B: 45 days around treatment						
ATT	0.121***	0.098***	0.076***	0.071***	0.086***	0.057***
	(0.057, 0.184)	(0.052, 0.145)	(0.039, 0.113)	(0.051, 0.092)	(0.072, 0.100)	(0.048, 0.067)
Number of Observations	752	752	752	446	446	446
Panel C: 60 days around treatment						
ATT	0.121***	0.098***	0.071***	0.048***	0.073***	0.057***
	(0.046, 0.196)	(0.042, 0.155)	(0.026, 0.117)	(0.026, 0.070)	(0.058, 0.088)	(0.047, 0.067)
Number of Observations	1020	1020	1020	532	532	532

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% bootstrapped bias-corrected confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A8: Staggered DiD Estimates of the Effect of the End of Moratorium on the Natural Logarithm of the Relative Response Ratio for African Americans Relative to White Applicants by Tenant Rights Protection Index

	Tenant Rights Protection Index					
	Above Median			Below Median		
	$h = 7$	$h = 10$	$h = 15$	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment					
ATT	0.102***	0.091***	0.067***	0.086***	0.066***	0.038**
	(0.074, 0.131)	(0.062, 0.121)	(0.027, 0.108)	(0.034, 0.138)	(0.024, 0.109)	(0.004, 0.073)
Number of Observations	150	150	150	507	507	507
	Panel B: 45 days around treatment					
ATT	0.082**	0.111***	0.100***	0.072**	0.060**	0.037*
	(0.003, 0.162)	(0.057, 0.165)	(0.051, 0.149)	(0.017, 0.128)	(0.013, 0.108)	(-0.001, 0.075)
Number of Observations	287	287	287	787	787	787
	Panel C: 60 days around treatment					
ATT	0.145**	0.125**	0.092**	0.099***	0.078***	0.048***
	(0.028, 0.261)	(0.023, 0.227)	(0.006, 0.178)	(0.054, 0.145)	(0.036, 0.119)	(0.014, 0.081)
Number of Observations	519	519	519	1058	1058	1058

Notes: 1) ATT stands for the Average Treatment Effect on the Treated. 2) h is the smoothing parameter of the weighted logit, see the text. 3) 95% bootstrapped bias-corrected confidence intervals in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A9: Distribution of Tenant Races

Race	Number of Tenants	Percent
African-American	400	12.03
Hispanic	405	12.18
Other	219	6.58
White	1,835	55.17
Multiple	467	14.04
	3,326	100.00

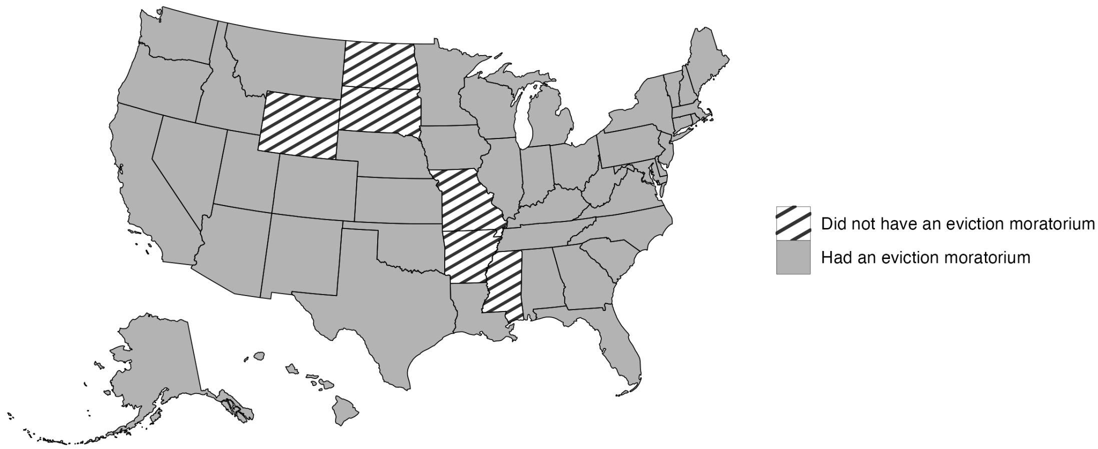
Table A10: Experiment to InfoUSA Address Match Rates

	Matching Step	New Matches	Total Matches	Share
1	Exact full-string address match in tracts from the experiment	6,077	6,077	69.2%
2	Exact house number + street name within a tract	+1,199	7,276	85.9%
3	Exact house number + street name within a county within tracts from the experiment	+62	7,338	86.6%
4	Fuzzy match on cleaned addresses in tracts from the experiment	+22	7,360	86.9%
5	Spatial match using lat/lon with street-name match	+12	7,372	87.0%
6	House number + street name + zip code	+30	7,393	87.3%
	Total matched		7,393 / 8,470	87.3%

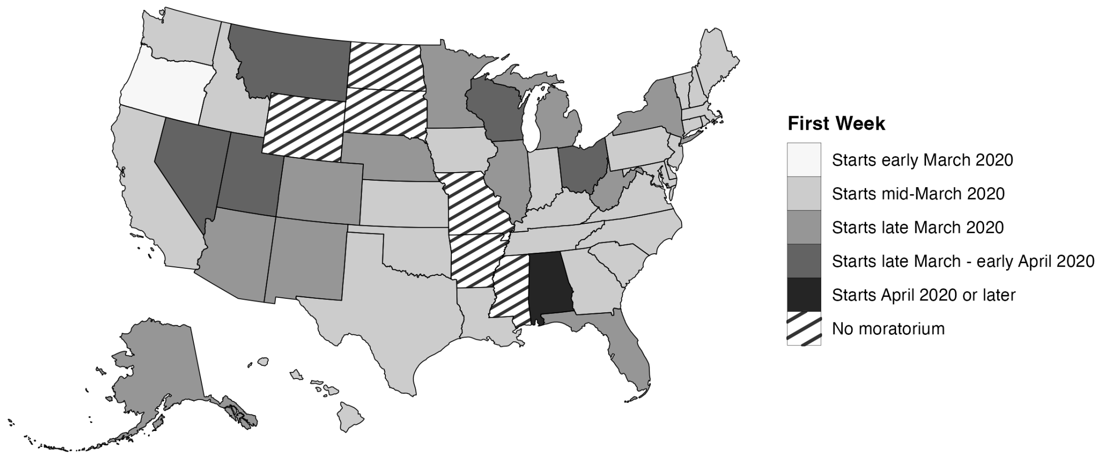
Notes: Six addresses were disqualified from the matching procedure and are excluded from all figures above. The original experimental address count is 8,476; after excluding the 6 disqualified addresses, the matching sample is 8,470.

Table A11: Staggered DiD Estimates of the Effect of the End of Moratorium on the Risk Difference: the Predicted Share of Same Race after No Response Minus the Predicted Share of Same Race after a Response

	(1)	(2)	(3)
	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment		
ATT	0.049**	0.051**	0.035
	(0.003, 0.094)	(0.002, 0.100)	(-0.008, 0.078)
Number of Observations	715	735	746
	Panel B: 45 days around treatment		
ATT	0.106***	0.074***	0.051**
	(0.051, 0.161)	(0.025, 0.124)	(0.011, 0.090)
Number of Observations	1122	1170	1192
	Panel C: 60 days around treatment		
ATT	0.085***	0.070***	0.053***
	(0.035, 0.135)	(0.026, 0.113)	(0.019, 0.088)
Number of Observations	1526	1582	1621

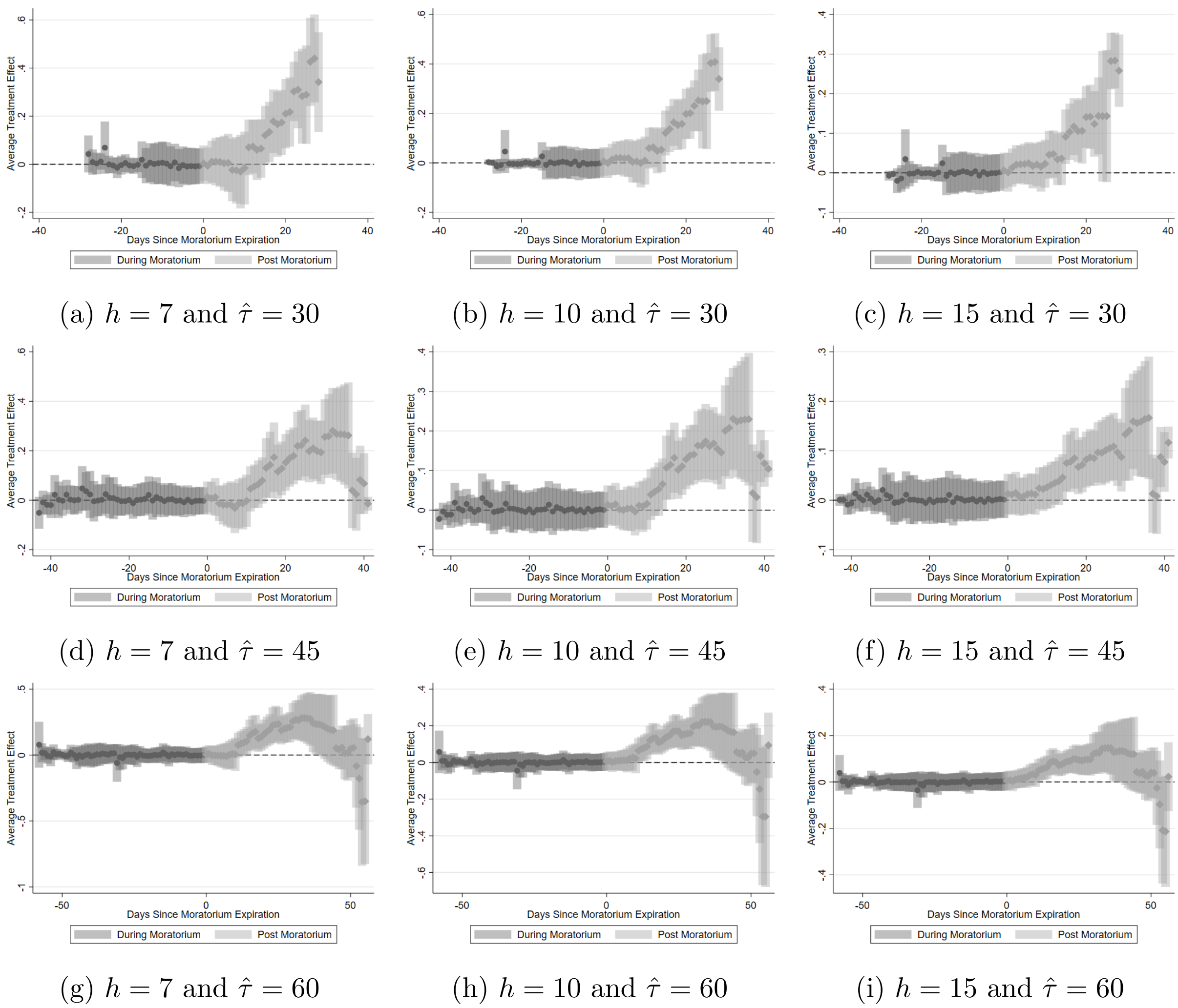


(a) States that Enacted Moratoria



(b) The First Week of the Eviction Moratorium across U.S.

Figure A1: Eviction Moratoria across the U.S.



(a) $h = 7$ and $\hat{\tau} = 30$ (b) $h = 10$ and $\hat{\tau} = 30$ (c) $h = 15$ and $\hat{\tau} = 30$

(d) $h = 7$ and $\hat{\tau} = 45$ (e) $h = 10$ and $\hat{\tau} = 45$ (f) $h = 15$ and $\hat{\tau} = 45$

(g) $h = 7$ and $\hat{\tau} = 60$ (h) $h = 10$ and $\hat{\tau} = 60$ (i) $h = 15$ and $\hat{\tau} = 60$

Figure A2: The Event Study Estimates of the Effect of the End of a Moratorium on the Natural Logarithm of the Relative Response Ratios for an African American Identity Relative to a White Identity from the Staggered DiD for Different Smoothing Parameters h and $\hat{\tau}$ Days Around Treatment

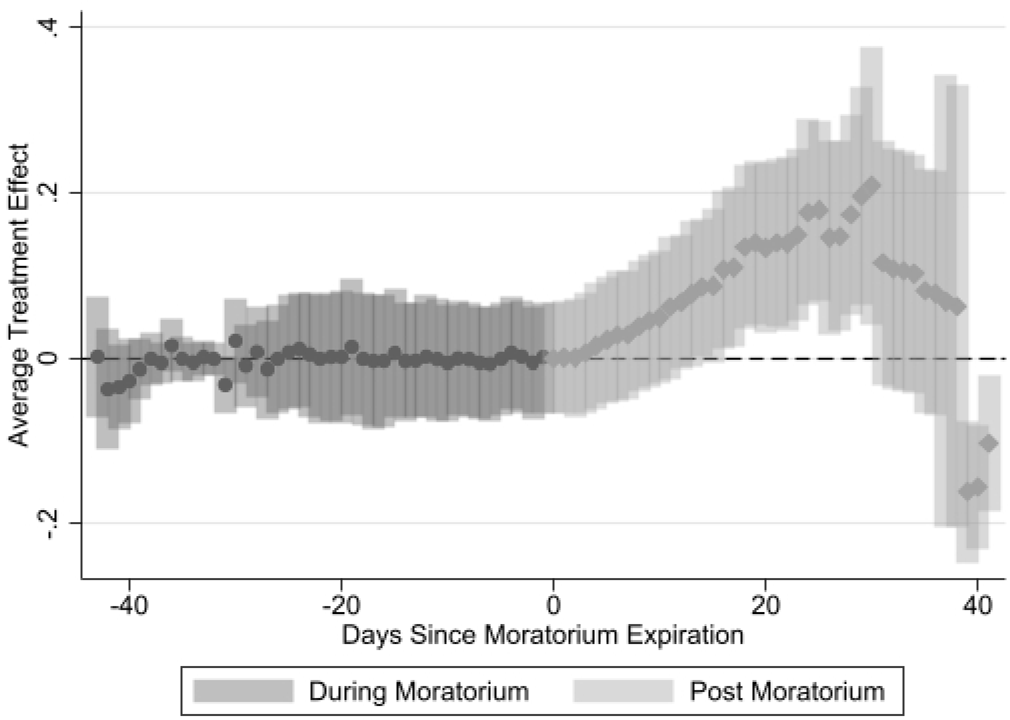


Figure A3: Event Study Coefficients from Callaway and Sant'Anna (2021)'s Estimator for the Risk Difference – the Predicted Share of Same Race after No Response Minus the Predicted Share of Same Race after a Response – with the Smoothing Parameter $h = 10$ and $\hat{\tau} = 45$ Days around Treatment

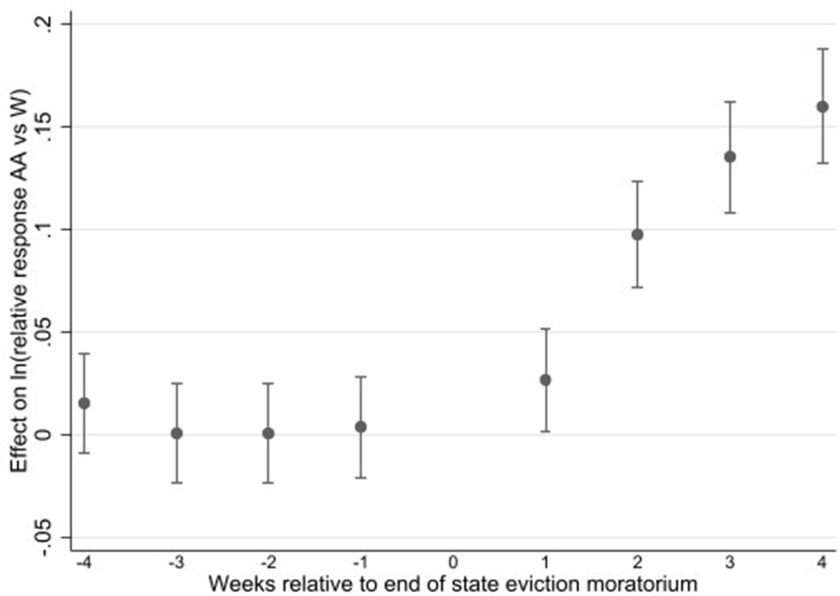


Figure A4: Stage 1 Response Rates: Event Study Coefficients