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DISCRIMINATION DURING EVICTION MORATORIA

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ABSTRACT

We merge a hand-collected dataset on state-level eviction policies with a nationwide field experiment of over 25,000 rental inquiries to study how enforcement constraints affect screening in rental markets. Exploiting plausibly exogenous variation from the staggered repeal of moratoria, we show that property managers discriminated more against minority renters when eviction—the primary enforcement mechanism—was suspended. Linking the experiment to tenant address histories, we find that nonresponses during moratoria translated into systematically different move-in patterns, shaping rental asset performance and market access. A simple search model explains these responses as landlords re-optimizing when enforcement is suspended.

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1 Introduction

Contractual frictions – such as limits on the enforcement of lease agreements – can erode investor returns by distorting the incentives of asset managers through the principal-agent channel. These frictions may affect how agents screen tenants and manage assets, especially in settings where performance relies on enforceable contracts. Yet identifying the causal impact of contractual frictions and disentangling the mechanisms through which they affect investor outcomes remains empirically challenging, largely due to a lack of plausibly exogenous variation in enforcement constraints.

We exploit a natural experiment that temporarily constrained landlords' ability to enforce lease contracts: eviction moratoria enacted across U.S. states in response to the COVID-19 pandemic and other acute events (e.g., hurricanes, extreme weather). We construct a novel, hand-collected dataset that traces each state-level moratorium to its legal source and codes the start and end dates. Importantly, our data extend beyond COVID-19-specific policies, allowing us to isolate the effect of eviction restrictions from the broader public health crisis. We merge these data with data from the largest randomized control trial study of the U.S. rental market (Christensen et al., 2021), which comprises over 25,000 inquiries from fictitious tenants sent to property managers across the 50 largest metropolitan areas.

Leveraging the staggered timing of eviction moratoria, we estimate the causal impact of contract enforcement constraints on property managers' screening behavior. We find that these constraints significantly increased discriminatory behavior against minority renters, consistent with managers responding to elevated default risk under weakened enforcement by tightening their screening standards. Such behavior can result in qualified renters being passed over due to prejudice (taste-based discrimination) rather than accurate risk assessment (statistical discrimination), potentially leaving units vacant longer or occupied by less suitable tenants, thereby reducing landlords' returns. To assess whether these screening decisions translated into actual market outcomes, we complement this analysis with an outcome test that links the experimental listings to a national database of tenant address histories. This analysis shows that property managers' non-responses during moratoria systematically reduced the share of tenants whose race matched that of the fictitious inquirer, indicating that discriminatory screening distorted realized move-in patterns.

A key identification concern is that the timing of moratoria terminations may be endogenous. We address this in several ways. First, we show that pre-determined socio-economic characteristics from the American Community Survey do not predict the repeal of moratoria, suggesting that terminations were not systematically driven by underlying state-level fundamentals. Second, we control for contemporaneous COVID-19 daily case counts to account for potential confounding from public health dynamics; our results are robust to these controls. Our findings are consistent with prior work suggesting that moratorium policies were largely shaped by political and administrative factors rather than economic conditions (Benfer et al., 2023).

To probe the underlying mechanism, we develop a forward-looking search model in which property managers re-optimize their screening strategy when eviction is no longer an enforceable threat. Landlords face uncertainty about tenant default and incur a utility loss when delinquent tenants cannot be removed. Under normal conditions, the threat of eviction mitigates this risk ex post. But when moratoria suspend that mechanism, landlords anticipate greater exposure to losses and respond by tightening their screening ex ante. If landlords hold discriminatory priors – whether due to taste-based preferences or biased beliefs about group-level default risk – these frictions can amplify disparities in access. This behavioral response is consistent with a principal-agent framework, where weaker enforcement shifts incentives toward more conservative and potentially biased screening practices. In this setting, tenant protections may introduce a trade-off between social equity and asset performance, as distorted screening can result in higher vacancies, underutilized properties, and reduced investor returns. The outcome test strengthens the case that discrimination is not just in communication, but also in actual housing outcomes.

The model yields a key testable implication: under statistical discrimination, the tightening of screening should vary with the economic value of the rental unit, as landlords have more to lose when enforcement risk is high. In contrast, taste-based discrimination should be unrelated to property value. We leverage this distinction empirically by comparing treatment effects across high- and low-rent markets. For African American applicants, the response gap narrows more sharply in high-rent areas, consistent with statistical discrimination. For Hispanic applicants, treatment effects are more uniform across markets, suggesting taste-based behavior. These results indicate that while eviction moratoria exacerbate disparities for both groups, the underlying mechanisms differ-and that understanding these differences is crucial for designing policies that promote both equity and efficiency in housing markets.

Related Literature. This paper makes several contributions at the intersection of contract theory, delegated monitoring, and real estate asset management. We extend foundational insights from the literature on legal enforcement and financial contracting into U.S. rental housing markets, a setting where empirical identification has been limited by a lack of plausibly exogenous variation in enforcement. Prior work shows that weak contract enforcement prompts investors to redesign contracts and shift monitoring strategies – for instance, relying less on covenants and more on control rights (Lerner and Schoar, 2005), or restricting access to financing for riskier borrowers in low-enforcement environments (Arellano et al., 2012; Quintin, 2008). We bring these insights to rental housing by treating eviction moratoria – temporary constraints on the primary enforcement mechanism in lease contracts – as natural experiments in enforcement risk. When eviction protections bind, property managers, similarly to other asset managers, respond by tightening screening criteria and monitoring practices. This response is consistent with a principal-agent framework in which the manager re-optimizes under heightened risk of tenant default, paralleling behavior observed in other financial contracting contexts.

Our findings also complement recent research on real estate asset management and institutional landlords. Recent work highlights how large landlords exercise market power to raise rents and increase evictions, shaping both asset performance and market access (Gurun et al., 2022; Austin, 2024). We complement these findings by showing that contractual frictions can induce property managers to re-optimize screening practices, with significant implications for returns and market access.

Second, our work contributes to the literature on racial discrimination in housing. Prior studies document persistent discrimination in various housing market stages (Yinger, 1995; Galster and Godfrey, 2005; Ambrose et al., 2020; Hanson and Hawley, 2011; Christensen and Timmins, 2022; Bartlett et al., 2022; Frame et al., 2023). We extend this work by showing that enforcement frictions amplify discriminatory behavior. During eviction moratoria, landlords become more likely to reject applicants from demographics perceived as higher-risk. This behavior reflects a shift in landlords' risk calculus, consistent with principal-agent theory: when formal enforcement tools weaken, principals rely more on informal discrimination to protect returns. Our results highlight a new mechanism by which enforcement frictions interact with taste-based or statistical discrimination, contributing to racial disparities in market access.

We also contribute to a growing finance literature on eviction policies and housing markets. Recent theoretical work has modeled how eviction constraints shift equilibrium rents, default rates, and investor welfare (Abramson, 2025; Corbae et al., 2024). These models predict trade-offs between tenant protection and landlord risk: stronger eviction protections can reduce evictions but increase default risk, prompting higher rents and tighter ex-ante screening. Our empirical findings complement these models by providing direct evidence of landlord behavior change in response to real-world eviction moratoria. We leverage a unique hand-collected dataset of local COVID and non-COVID eviction moratoria, merged with a large-scale field experiment in U.S. rental markets, to causally identify how investors respond when enforcement is suspended. We document that landlords raise screening standards when evictions are off the table.

Finally, we contribute to the literature on the broader economic and policy effects of eviction regulation. Prior research emphasizes the harms of eviction for tenants (Desmond, 2016; Collinson et al., 2023), while some recent studies evaluate policies such as rental assistance and right-to-counsel (Abramson, 2025). We provide a complementary perspective by analyzing the behavioral responses of property managers and landlords. Our evidence suggests that policies constraining contract enforcement may produce unintended consequences by altering landlord decision-making. These results illustrate an important equilibrium trade-off: protections for vulnerable renters can lead landlords to tighten screening criteria, potentially exacerbating inequality. By formalizing this mechanism in a simple search model and testing its predictions, we show how enforcement frictions translate into investor behavior – a contribution with direct implications for asset pricing and market structure in the real estate sector.

The remainder of the paper proceeds as follows. Section 2 presents a theoretical model illustrating how eviction moratoria can increase discrimination in rental markets. Section 3 describes the novel eviction moratoria dataset and Christensen et al. (2021)'s field experiment. Section 4 outlines our empirical strategy and presents the baseline results. Section 5 addresses endogeneity concerns, reports robustness checks, and explores heterogeneity in the effects of eviction moratoria across demographic groups. Section 6 links screening decisions to subsequent tenant outcomes using address histories. Section 7 concludes.

2 Theoretical Framework

We develop a dynamic search model to formalize how eviction moratoria affect property managers' screening incentives in a context of pre-existing racial and ethnic disparities. The model incorporates both statistical and taste-based discrimination and yields a central 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 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 concerns, the property manager may experience a per-period non-monetary utility cost κ_i from leasing to type-i applicants, reflecting taste-based preferences. We normalize $\kappa_W = 0$ and define $\kappa_M \equiv \kappa \geq 0$. When $\kappa > 0$, the manager exhibits *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. Each decision to respond to an inquiry incurs a cost. Let ψ denote the difference in the cost of responding to a minority versus 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, 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 in the absence of a moratorium:

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

where V denotes the option value of a vacant unit.

The property manager responds to a minority applicant if $u_M - \psi > u_W$, 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 value of vacancy 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 strategy in Section 5.3.

3 Data

3.1 Correspondence Study

We evaluate the model's predictions using data from a large-scale randomized correspondence study conducted by Christensen et al. (2021) in the United States during spring–summer 2020. A software bot developed by Christensen's team at the National Center for Supercomputing Applications submitted inquiries under eighteen African American, Hispanic/Latinx, and white identities to 8,476 property managers in the fifty largest U.S. metropolitan areas (see Tables A2–A3 in the Appendix). Each day the bot targeted three downtown listings and three suburban listings in each metro area (downtown defined as the 5% of zip codes closest to the city center; all others classified as suburban). One day after a listing appeared, the bot sent one inquiry per racial identity in randomized order, ensuring no manager received more than one identity's inquiry per day.

Names were drawn from eighteen first–last pairs designed to signal distinct racial/ethnic categories, and sampling was stratified by gender and maternal education to mitigate confounding with unobservable traits such as income (Guryan and Charles, 2013; Fryer Jr and Levitt, 2004).[1] Each fictitious identity was assigned a unique email address and a New York City–area phone number. All inquiries carried the standardized message, auto filled by the rental platform: "I am interested in this rental and would like to schedule a viewing."

Responses were coded as one if a property manager confirmed availability within seven days and zero otherwise; approximately 80% of responses arrived within 24 hours. The final dataset comprises 25,428 inquiry–manager interactions at the initial stage of the rental application process.

3.2 Development of an Eviction Moratoria Database

We construct an original, hand-collected database that traces the start and end dates of eviction moratoria across U.S. states, linking each policy action directly to its legal or administrative source. Building on foundational work by Benfer et al. (2023), we independently verify and document each moratorium using primary sources-executive orders, court

[1] In adherence to the protocols outlined in the literature on correspondence studies, pairs of names were carefully selected to evoke cognitive associations with specific racial/ethnic categories.

rulings, legislative acts, and regulatory directives. This process enhances transparency and ensures that each policy entry can be directly traced to its underlying authority.

Our database extends beyond the COVID-19 context to include a broader class of eviction prevention policies. Specifically, we incorporate emergency protections unrelated to the pandemic, such as cold-weather bans and utility disconnection moratoria triggered by extreme weather conditions. By capturing both COVID-specific and non-COVID emergency measures, we provide a more comprehensive picture of how eviction enforcement was suspended across the United States.

Our database actively cross-validates entries against those in Benfer et al. (2023), noting and explaining any discrepancies in coverage, timing, or interpretation. These differences often arise from our broader inclusion criteria and our strict insistence on linking each policy to a verifiable source.

Taken together, these improvements provide researchers with a more accurate and legally grounded timeline of enforcement constraints during the pandemic and other emergencies. By capturing the full range of eviction-related moratoria-including those that fall outside the scope of COVID-specific policies-we offer a dataset better suited to identify the causal effects of eviction enforcement on housing market behavior.

4 Results

4.1 Defining Treatment

While most eviction moratoria were enacted rapidly in the early weeks of the pandemic, their expiration occurred in a staggered fashion over the summer of 2020. This variation in termination timing allows us to isolate changes in property manager behavior as jurisdictions restored enforcement authority. Accordingly, we define treatment based on the *end* of a moratorium, rather than its initiation.[2] The federal CARES Act established a nationwide eviction moratorium beginning on September 4, 2020, but our sample ends before this date, capturing variation in state-level expirations that occurred independently.

Figure 1 depicts the final week of state-level moratoria across the United States. Ap-

[2]This approach follows other studies examining policy effects in the COVID-19 context. See Benfer et al. (2021a) for a related application.

pendix Figures A1a and A1b provide additional detail, showing which states implemented a moratorium and, for those that did, the week of enactment.

Our correspondence experiment began on February 6, 2020, and concluded on July 31, 2020. To avoid conflating pre-treatment periods with baseline discrimination, we exclude all observations preceding a state's moratorium. The resulting analysis sample begins on March 13, 2020—the earliest date after a moratorium was enacted in a treated state. Figure 2 shows the distribution of moratorium termination dates in our sample, which range from May 7 to July 30, 2020.

We define the treatment variable $Treatment_j$ as an indicator equal to one if an inquiry was sent *after* the expiration of a state's eviction moratorium. This design allows us to exploit plausibly exogenous variation in enforcement constraints to identify their impact on property manager behavior.

4.2 Potential Endogeneity of Treatment

While the initiation of eviction moratoria occurred in close temporal proximity across states, the timing of their repeal varied substantially. This staggered expiration raises potential concerns about endogeneity—specifically, that states may have lifted their moratoria in response to evolving public health conditions or underlying socioeconomic factors. To assess the plausibility of such selection, we examine whether the timing of policy termination is systematically related to contemporaneous COVID-19 incidence or pre-pandemic state characteristics.

To account for the potential influence of COVID-19 dynamics on policy timing, we include daily state-level COVID-19 case counts as a control in our main empirical specification. Our results remain robust to this adjustment, suggesting that variation in moratorium timing is not solely driven by public health conditions. This conclusion is consistent with the descriptive findings of Benfer et al. (2021b), who report that "public health conditions served as a meaningful predictor of the timing of moratoria" only weakly, noting that "eviction protections were very often rolled back even as the prevalence of COVID-19 was increasing in a given state."

To further probe the possibility of selection on socioeconomic fundamentals, we implement a two-stage procedure. In the first stage, we regress the date of moratorium termination on daily COVID-19 case counts and state fixed effects. In the second stage, we regress the estimated state fixed effects from the first stage on pre-pandemic characteristics drawn from the American Community Survey, along with the moratorium start date to flexibly control for duration. As shown in Table 2, none of these covariates significantly predict the timing of moratorium repeal.

Because these state-level characteristics are absorbed by property fixed effects in our main Difference-in-Differences specification, we omit them from the baseline model. Moreover, computational limitations inherent in the staggered Difference-in-Differences estimator—implemented via the `csdid` and `csdid2` commands in Stata—preclude inclusion of the full covariate set.[3] We therefore retain only daily COVID-19 case counts as a control variable in our preferred specification.

4.3 Difference-in-Differences Specification

Before analyzing how the repeal of eviction moratoria interacts with property managers' screening behavior, we first confirm the presence of discriminatory outcomes in our sample (see Appendix B). This step is essential given the theoretical framework in Section 2, which predicts that eviction moratoria can amplify pre-existing disparities in property manager response rates.

Having established baseline discrimination, we proceed by examining how this behavior changes following the expiration of state-level eviction moratoria using a standard Difference-in-Differences (DiD) framework. Our analysis focuses on states that implemented a moratorium during the sample period. We exclude eight states that never adopted such policies, though robustness checks confirm that our results are not sensitive to their inclusion.

We estimate the following DiD specification:

$$\begin{aligned} Response_{ijt} = {} & \delta_i + \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + \beta^{T} Treatment_{jt} \\ & + \beta^{AAT} Treatment_{jt} \times African\ American_j + \beta^{HT} Treatment_{jt} \times Hispanic_j + X_j'\theta + \epsilon_{ijt}, \end{aligned} \tag{8}$$

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

[3] Including these covariates yields an empty matrix of estimates.

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 3 presents the results. Columns (1) through (3) include controls for the local eviction policy stringency index, historical eviction rates from 2018, and week fixed effects, but omit property fixed effects.[4] 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 turn to an estimator that explicitly accounts for variation in treatment timing, as described in the following section.

4.4 Staggered Differences-in-Differences

Because eviction moratoria were lifted at different times across states, we implement a staggered Difference-in-Differences (DiD) framework using the estimator developed by Callaway and Sant'Anna (2021). This approach accommodates heterogeneity in treatment timing and allows us to isolate the causal effect of moratorium expiration on racial disparities in housing inquiry responses. Since rental listings are not observed continuously over time,

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

we apply the Callaway–Sant'Anna Difference-in-Differences (CSDiD) estimator in two stages. First, we construct a panel of state-by-day discrimination estimates; second, we apply the CSDiD procedure to evaluate the dynamic treatment effects.

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 $\tau \in \{1, \ldots, 177\}$, 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:

$$Response_{ijst} = \beta^{AA}_{s\tau} \cdot African\ American_j + \beta^{H}_{s\tau} \cdot Hispanic_j + X'_j \theta_{s\tau} + u_{ijst}, \tag{9}$$

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:

$$\omega^{\tau}_{ijst} = \frac{1}{h\sqrt{2\pi}} \exp\left(-\frac{1}{2}\left(\frac{t-\tau}{h}\right)^2\right), \tag{10}$$

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 $\{\beta^{AA}_{s\tau}, \beta^{H}_{s\tau}\}$ for each state s and day τ for African American (AA) and Hispanic (H) identities. Because the discrimination coefficients $\{\beta^{AA}_{s\tau}, \beta^{H}_{s\tau}\}$ are estimated with sampling error, we report a bootstrap robustness check in Section 5 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 5).

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

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

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.

Results

Figure 3 shows the event-study estimates for African American applicants using $h = 10$ and $\hat{\tau} = 45$. There is no evidence of pre-trends prior to the moratorium's expiration. The relative response ratio increases after treatment, indicating a reduction in racial discrimination once eviction enforcement is reinstated. Appendix Figure A2 shows qualitatively similar results for other values of h and $\hat{\tau}$.

Table 4 reports ATT estimates across a range of specifications. Panel A presents results for African American identities, with estimates consistently positive across all parameter settings. Panel B reports estimates for Hispanic applicants, which are generally smaller in magnitude and less statistically robust.

To assess the economic significance, recall that white applicants in our sample received responses 57.36% of the time during moratoria, while African American applicants received responses at a rate of 51.26%, implying a relative response ratio of 0.89. Our ATT estimates suggest that the log of this ratio increases by 0.143 for African American identities, which is the median of the estimates across specifications from Panel A of Table 4. This translates into the increase in the relative response ratio RR by $100 \times (e^{0.143} - 1) = 15.38\%$, which eliminates the racial gap in response rates.

5 Robustness and Treatment Heterogeneity

5.1 Bootstrap

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 6 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.

5.2 Not Treated States as a Control Group

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 7 presents the results under this alternative control group.

For African American applicants, the estimated effects remain positive and are statistically significant at the 1% level across specifications. Notably, the estimated effects for Hispanic applicants are larger in magnitude and 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 minority rental applicants. The consistency of this pattern across both "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.

5.3 Heterogeneity

We examine heterogeneity in treatment effects along two dimensions: applicant gender and local rental market conditions, the latter providing a test for statistical versus taste-based discrimination.

While the differences in the estimates by gender are not statistically significant, point estimates consistently indicate that male applicants—particularly African American males — experience larger declines in relative response rates during the moratoria (Tables 8 and 9). These patterns are consistent with prior findings that discrimination in housing markets may be more severe for men of color.

Our theoretical framework predicts that under statistical discrimination, the change in response rates to minority applicants should vary with the economic value of the rental unit, $\bar{V}$ (equation (7)), which is increasing in the rent. In contrast, under taste-based discrimination, the effect of the moratorium should not vary with $\bar{V}$ (equation (6)). We therefore test for statistical discrimination by comparing ATT estimates across markets with above- and below-median rents.

To implement this, we use Zillow's Observed Rent Index (ZORI) at the metro area level to proxy for rent and re-estimate our preferred specification—column (4) of Table 3—separately for high- and low-rent subsamples. Table 10 presents the results for African American applicants. The estimated treatment effects are larger and more statistically significant in high-rent areas, with ATTs ranging from 0.062-0.318 across specifications. The null hypothesis that these effects are equal across rent segments is rejected at conventional significance levels in most cases.[5] These results support the presence of statistical discrimination: property managers appear more sensitive to moratoria when the economic value of the property is higher.

We conduct a parallel analysis for Hispanic applicants. As shown in Table 11, the difference in ATT estimates between high- and low-rent areas is not statistically significant in most specifications. This suggests that the observed disparities for Hispanic applicants may stem primarily from taste-based discrimination, rather than beliefs about applicant type varying with unit value.

[5]Specifically, the null is rejected at the 10% level or below for all three bandwidths ($h = 7$, 10, and 15) without COVID-19 controls in the 30-day window, and for $h = 10$ and $h = 15$ with COVID-19 controls in the 45- and 60-day windows.

6 Consequences for 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 C 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. Table 13 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 (9).

The left-hand side variable 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 (10), 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 A5 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 12 and Figure 4 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 0.287 implies that the relative response ratio rose by approximately 33 percent ($100 \times (e^{0.287} - 1)$) 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

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.

7 Conclusion

This paper examines how contractual frictions-specifically, eviction moratoria-affect racial disparities in access to rental housing. Using a nationwide correspondence experiment and plausibly exogenous variation in the timing of moratorium expiration, we find that African American and Hispanic applicants were less likely to receive responses from property managers during periods when eviction was prohibited. These gaps narrowed following the reinstatement of enforcement authority, consistent with the view that eviction moratoria altered screening behavior in a manner that disadvantaged minority renters.

We interpret these results through the lens of a simple search model in which moratoria reduce property managers' ability to remove non-paying tenants. In this setting, property managers strengthen their screening criteria to mitigate default risk. When managers hold biased priors-whether due to taste or beliefs about average group-level risk-this re-optimization can amplify discriminatory behavior. Our empirical results suggest that these mechanisms differ by racial and ethnic group. For African American applicants, the treatment effects are larger in higher-rent markets-where the stakes of tenant default are greater-supporting the presence of statistical discrimination. For Hispanic applicants, treatment effects are more uniform across market segments, consistent with taste-based discrimination that is less sensitive to property value.

Our outcome test links property managers' responses in the experiment to realized tenant composition at matched addresses. We find that non-responses during eviction moratoria translated into lower shares of tenants whose race matched that of the fictitious inquirer, consistent with discriminatory screening distorting actual move-in patterns. Applying the same staggered DiD framework to the log relative response ratio, we show that the expiration of moratoria raised the likelihood of same-race tenant matches by roughly one-third, underscoring the economic significance of these screening effects. Importantly, because both our benchmark test of screening decisions and our outcome test of realized tenant composition point to the same conclusion, the framework of Gaebler and Goel (2025) implies that our evidence of discrimination is statistically robust. Taken together, these results demonstrate that contractual frictions not only shape property managers' immediate communication behavior but also leave measurable imprints on the racial composition of households that ultimately occupy rental units.

These findings highlight the importance of enforcement institutions in shaping intermediary behavior in asset markets. When contract enforcement is weakened, intermediaries respond by altering screening in ways that may not align with social goals or investor interests. In the context of rental housing, policies designed to prevent displacement can unintentionally restrict market access for minority renters, particularly where enforcement risk is high. Understanding this trade-off-between equity and performance-can inform the design of more effective housing policies that protect tenants without reinforcing discriminatory barriers.

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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

Table 2: Predicting the End of Moratorium

	(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 motocycle	-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: 1) The dependent variable is the last day of the moratorium. 2) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 3: 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 100k				0.000	0.000	-0.000	0.000
				(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.544***	0.550***	0.614***	0.593***	0.593***	0.675***	0.636***
	(0.039)	(0.042)	(0.017)	(0.125)	(0.134)	(0.127)	(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 100k is the number of COVID-19 infections in a state on a day per 100,000 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.

Table 4: Staggered DiD Estimates of the Effect of the End of Moratorium on on the Natural Logarithm of the Relative Response Ratios for a Minority Identity Relative to a White Identity

	(1)	(2)	(3)	(4)	(5)	(6)
	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
			Panel A: 30 days around treatment			
ATT	0.055*	0.288***	0.050**	0.228***	0.030	0.168***
	(-0.005, 0.115)	(0.181, 0.394)	(0.004, 0.097)	(0.141, 0.315)	(-0.008, 0.068)	(0.097, 0.240)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.106***	0.524***	0.092***	0.708***	0.061***	0.683***
	(0.050, 0.163)	(0.308, 0.740)	(0.046, 0.138)	(0.374, 1.043)	(0.021, 0.100)	(0.357, 1.009)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.110***	0.155***	0.094***	0.143***	0.064***	0.127***
	(0.053, 0.168)	(0.072, 0.238)	(0.047, 0.141)	(0.077, 0.209)	(0.024, 0.103)	(0.070, 0.184)
Number of Observations	1850	1823	1850	1823	1850	1823
			Hispanic			
			Panel A: 30 days around treatment			
ATT	0.038*	0.059**	0.030*	0.047**	0.020	0.027**
	(-0.007, 0.082)	(0.013, 0.105)	(-0.005, 0.065)	(0.011, 0.082)	(-0.007, 0.047)	(0.002, 0.052)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.041	−0.189***	0.033	-0.024	0.015	-0.046
	(-0.013, 0.096)	(-0.325, -0.052)	(-0.013, 0.078)	(-0.142, 0.093)	(-0.021, 0.051)	(-0.135, 0.043)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.032	-0.002	0.022	-0.002	0.006	-0.001
	(-0.025, 0.089)	(-0.064, 0.061)	(-0.028, 0.073)	(-0.056, 0.052)	(-0.036, 0.047)	(-0.043, 0.042)
Number of Observations	1850	1823	1850	1823	1850	1823

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) COVID Cases per 100k is the number of COVID-19 infections in a state on a day per 100,000 people. 4) 95% confidence intervals in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 5: Staggered DiD Estimates of the Effect of the End of Moratorium on the Relative Response Ratio Relative to White Applicants

	(1)	(2)	(3)	(4)	(5)	(6)
	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
			Panel A: 30 days around treatment			
ATT	0.057**	0.258***	0.047**	0.200***	0.027	0.146***
	(0.002, 0.111)	(0.156, 0.359)	(0.006, 0.089)	(0.122, 0.279)	(-0.007, 0.061)	(0.082, 0.209)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.114***	0.491***	0.092***	0.656***	0.058***	0.630***
	(0.050, 0.178)	(0.296, 0.686)	(0.043, 0.142)	(0.350, 0.963)	(0.018, 0.098)	(0.329, 0.932)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.113***	0.158***	0.090***	0.134***	0.058***	0.113***
	(0.049, 0.177)	(0.069, 0.247)	(0.040, 0.139)	(0.067, 0.202)	(0.019, 0.097)	(0.058, 0.168)
Number of Observations	1850	1823	1850	1823	1850	1823
			Hispanic			
			Panel A: 30 days around treatment			
ATT	0.044*	0.069***	0.033*	0.052***	0.021	0.030**
	(-0.001, 0.088)	(0.020, 0.117)	(-0.002, 0.067)	(0.014, 0.089)	(-0.004, 0.047)	(0.005, 0.056)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.052*	−0.142**	0.040*	0.001	0.019	-0.026
	(-0.002, 0.105)	(-0.262, -0.022)	(-0.005, 0.085)	(-0.100, 0.102)	(-0.016, 0.054)	(-0.103, 0.050)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.038	0.019	0.026	0.014	0.009	0.009
	(-0.018, 0.094)	(-0.039, 0.077)	(-0.024, 0.076)	(-0.036, 0.064)	(-0.032, 0.050)	(-0.030, 0.048)
Number of Observations	1850	1823	1850	1823	1850	1823

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) COVID Cases per 100k is the number of COVID-19 infections in a state on a day per 100,000 people. 4) 95% confidence intervals in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 6: 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

	(1)	(2)	(3)	(4)	(5)	(6)
	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
			Panel A: 30 days around treatment			
ATT	0.055	0.493***	0.049	0.391***	0.023	0.297***
95% Confidence Interval	(-0.090, 0.182)	(0.304, 0.646)	(-0.042, 0.148)	(0.231, 0.355)	(-0.034, 0.095)	(0.182, 0.252)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.114**	0.862***	0.098**	1.206***	0.062*	1.181***
95% Confidence Interval	(0.003, 0.226)	(0.333, 1.206)	(0.015, 0.189)	(0.416, 2.130)	(-0.001, 0.134)	(0.452, 2.056)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.113**	0.961*	0.094**	0.810**	0.062**	0.667***
95% Confidence Interval	(0.012, 0.214)	(-0.006, 0.485)	(0.014, 0.187)	(0.048, 0.690)	(0.002, 0.139)	(0.056, 3.155)
Number of Observations	1850	1823	1850	1823	1850	1823

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

	(1)	(2)	(3)	(4)	(5)	(6)
			African American			
			Panel A: 30 days around treatment			
ATT	0.080***	0.076	0.088***	0.112**	0.084***	0.115***
	(0.029, 0.130)	(-0.063, 0.215)	(0.051, 0.124)	(0.015, 0.209)	(0.056, 0.111)	(0.063, 0.166)
Number of Observations	985	175	985	175	985	175
			Panel B: 45 days around treatment			
ATT	0.158***	0.186***	0.158***	0.204***	0.138***	0.184***
	(0.098, 0.219)	(0.074, 0.298)	(0.110, 0.207)	(0.121, 0.287)	(0.100, 0.176)	(0.136, 0.231)
Number of Observations	1441	325	1441	325	1441	325
			Panel C: 60 days around treatment			
ATT	0.184***	0.223***	0.180***	0.226***	0.156***	0.199***
	(0.129, 0.240)	(0.133, 0.314)	(0.135, 0.225)	(0.162, 0.291)	(0.120, 0.193)	(0.160, 0.238)
Number of Observations	1848	482	1848	482	1848	482
			Hispanic			
			Panel A: 30 days around treatment			
ATT	0.024	0.057	0.026	0.056	0.029**	0.057***
	(-0.020, 0.068)	(-0.047, 0.161)	(-0.007, 0.059)	(-0.014, 0.127)	(0.005, 0.052)	(0.025, 0.089)
Number of Observations	985	175	985	175	985	175
			Panel B: 45 days around treatment			
ATT	0.040	0.070*	0.043**	0.075**	0.039**	0.064***
	(-0.009, 0.088)	(-0.011, 0.151)	(0.002, 0.084)	(0.013, 0.137)	(0.008, 0.071)	(0.030, 0.098)
Number of Observations	1441	325	1441	325	1441	325
			Panel C: 60 days around treatment			
ATT	0.026	0.043	0.029	0.046*	0.028	0.042**
	(-0.024, 0.077)	(-0.025, 0.111)	(-0.015, 0.073)	(-0.009, 0.102)	(-0.007, 0.063)	(0.005, 0.080)
Number of Observations	1848	482	1848	482	1848	482

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 8: Estimates for Males

	(1)	(2)	(3)	(4)	(5)	(6)
	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
			Panel A: 30 days around treatment			
ATT	0.055*	0.288***	0.050**	0.228***	0.030	0.168***
	(-0.005, 0.115)	(0.181, 0.394)	(0.004, 0.097)	(0.141, 0.315)	(-0.008, 0.068)	(0.097, 0.240)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.106***	0.524***	0.092***	0.708***	0.061***	0.683***
	(0.050, 0.163)	(0.308, 0.740)	(0.046, 0.138)	(0.374, 1.043)	(0.021, 0.100)	(0.357, 1.009)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.110***	0.155***	0.094***	0.143***	0.064***	0.127***
	(0.053, 0.168)	(0.072, 0.238)	(0.047, 0.141)	(0.077, 0.209)	(0.024, 0.103)	(0.070, 0.184)
Number of Observations	1850	1823	1850	1823	1850	1823

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 9: Estimates for Females

	(1)	(2)	(3)	(4)	(5)	(6)
	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
			Panel A: 30 days around treatment			
ATT	0.047	0.260***	0.036	0.192***	0.017	0.138***
	(-0.015, 0.108)	(0.137, 0.383)	(-0.013, 0.084)	(0.102, 0.282)	(-0.024, 0.058)	(0.067, 0.209)
Number of Observations	986	932	986	932	986	932
			Panel B: 45 days around treatment			
ATT	0.071**	0.424***	0.057**	0.593***	0.034	0.598***
	(0.013, 0.129)	(0.245, 0.603)	(0.009, 0.105)	(0.307, 0.880)	(-0.008, 0.075)	(0.304, 0.892)
Number of Observations	1441	1397	1441	1397	1441	1397
			Panel C: 60 days around treatment			
ATT	0.076**	0.145***	0.058**	0.122***	0.036*	0.104***
	(0.017, 0.134)	(0.060, 0.231)	(0.010, 0.107)	(0.056, 0.188)	(-0.005, 0.077)	(0.048, 0.161)
Number of Observations	1850	1823	1850	1823	1850	1823

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 10: 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

	(1)	(2)	(3)	(4)	(5)	(6)
			Rent Above Median			
			Panel A: 30 days around treatment			
ATT	0.214***	0.309***	0.175***	0.248***	0.105***	0.147***
	(0.134, 0.293)	(0.227, 0.391)	(0.107, 0.242)	(0.170, 0.326)	(0.064, 0.146)	(0.101, 0.193)
Number of Observations	172	66	172	66	172	66
			Panel B: 45 days around treatment			
ATT	0.147***	0.318***	0.113***	0.253***	0.062***	0.147***
	(0.070, 0.225)	(0.225, 0.410)	(0.058, 0.168)	(0.170, 0.336)	(0.026, 0.097)	(0.101, 0.193)
Number of Observations	389	78	389	78	389	78
			Panel C: 60 days around treatment			
ATT	0.130***	0.252***	0.101***	0.206***	0.060***	0.117***
	(0.042, 0.218)	(0.124, 0.381)	(0.037, 0.165)	(0.105, 0.306)	(0.015, 0.105)	(0.056, 0.177)
Number of Observations	493	185	493	185	493	185
			Rent Below Median			
			Panel A: 30 days around treatment			
ATT	0.025	0.320***	0.018	0.234***	0.005	0.152**
	(-0.036, 0.086)	(0.123, 0.517)	(-0.036, 0.072)	(0.072, 0.395)	(-0.044, 0.053)	(0.027, 0.277)
Number of Observations	699	575	699	575	699	575
			Panel B: 45 days around treatment			
ATT	0.119***	0.880***	0.109***	1.231***	0.077***	1.200***
	(0.058, 0.180)	(0.513, 1.247)	(0.052, 0.165)	(0.653, 1.809)	(0.025, 0.129)	(0.638, 1.761)
Number of Observations	1001	847	1001	847	1001	847
			Panel C: 60 days around treatment			
ATT	0.112***	0.071*	0.098***	0.065*	0.067**	0.033
	(0.046, 0.178)	(-0.000, 0.143)	(0.038, 0.157)	(-0.004, 0.135)	(0.014, 0.121)	(-0.029, 0.096)
Number of Observations	1288	1152	1288	1152	1288	1152

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 11: Staggered DiD Estimates of the Effect of the End of Moratorium on the Natural Logarithm of the Relative Response Ratio for Hispanic Applicants by Pre-Pandemic Rent Level

	(1)	(2)	(3)	(4)	(5)	(6)
			Rent Above Median			
			Panel A: 30 days around treatment			
ATT	0.089***	0.163***	0.050***	0.087***	0.023***	0.036***
	(0.053, 0.125)	(0.151, 0.176)	(0.029, 0.071)	(0.080, 0.093)	(0.013, 0.033)	(0.031, 0.042)
Number of Observations	172	66	172	66	172	66
			Panel B: 45 days around treatment			
ATT	0.097***	0.166***	0.054***	0.087***	0.010	0.036***
	(0.056, 0.137)	(0.153, 0.178)	(0.019, 0.089)	(0.081, 0.094)	(-0.017, 0.037)	(0.030, 0.042)
Number of Observations	389	78	389	78	389	78
			Panel C: 60 days around treatment			
ATT	0.068***	0.148***	0.037*	0.072***	0.010	0.028***
	(0.018, 0.117)	(0.106, 0.190)	(-0.006, 0.081)	(0.040, 0.104)	(-0.022, 0.041)	(0.010, 0.045)
Number of Observations	493	185	493	185	493	185
			Rent Below Median			
			Panel A: 30 days around treatment			
ATT	0.075**	0.178***	0.056**	0.136***	0.036*	0.076***
	(0.018, 0.133)	(0.100, 0.257)	(0.009, 0.103)	(0.073, 0.198)	(-0.001, 0.073)	(0.033, 0.118)
Number of Observations	699	575	699	575	699	575
			Panel B: 45 days around treatment			
ATT	0.075**	-0.347***	0.060*	-0.069	0.030	-0.123
	(0.001, 0.150)	(-0.587, -0.108)	(-0.004, 0.123)	(-0.285, 0.147)	(-0.023, 0.082)	(-0.288, 0.041)
Number of Observations	1001	847	1001	847	1001	847
			Panel C: 60 days around treatment			
ATT	0.071*	0.085*	0.053	0.076*	0.028	0.064*
	(-0.004, 0.147)	(-0.005, 0.174)	(-0.016, 0.122)	(-0.005, 0.157)	(-0.032, 0.088)	(-0.003, 0.131)
Number of Observations	1288	1152	1288	1152	1288	1152

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 12: 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	0.431	0.656***	0.287***
	(-0.123, 0.985)	(0.179, 1.133)	(0.072, 0.503)
Number of Observations	715	735	746
	Panel B: 45 days around treatment		
ATT	0.845**	0.795**	0.330***
	(0.052, 1.639)	(0.165, 1.425)	(0.091, 0.570)
Number of Observations	1122	1170	1192
	Panel C: 60 days around treatment		
ATT	0.908**	0.804**	0.369***
	(0.099, 1.717)	(0.155, 1.454)	(0.123, 0.614)
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 13: 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

Figure 1: The Last Week of the Eviction Moratorium across the U.S.

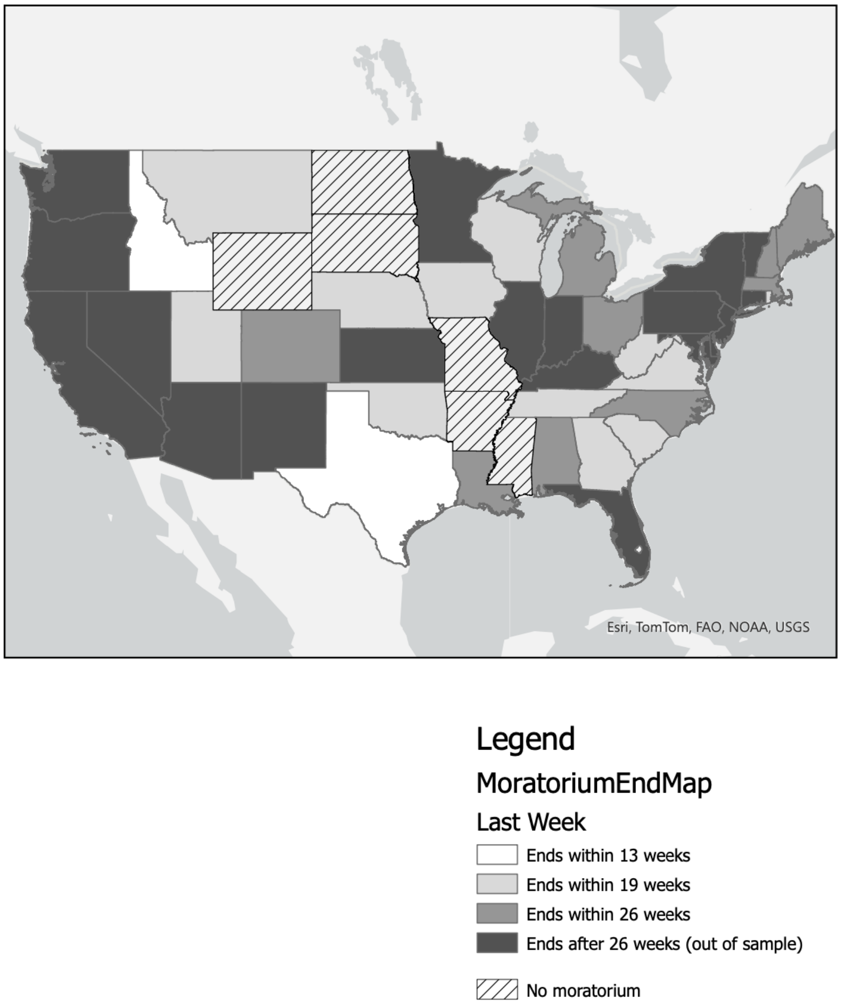
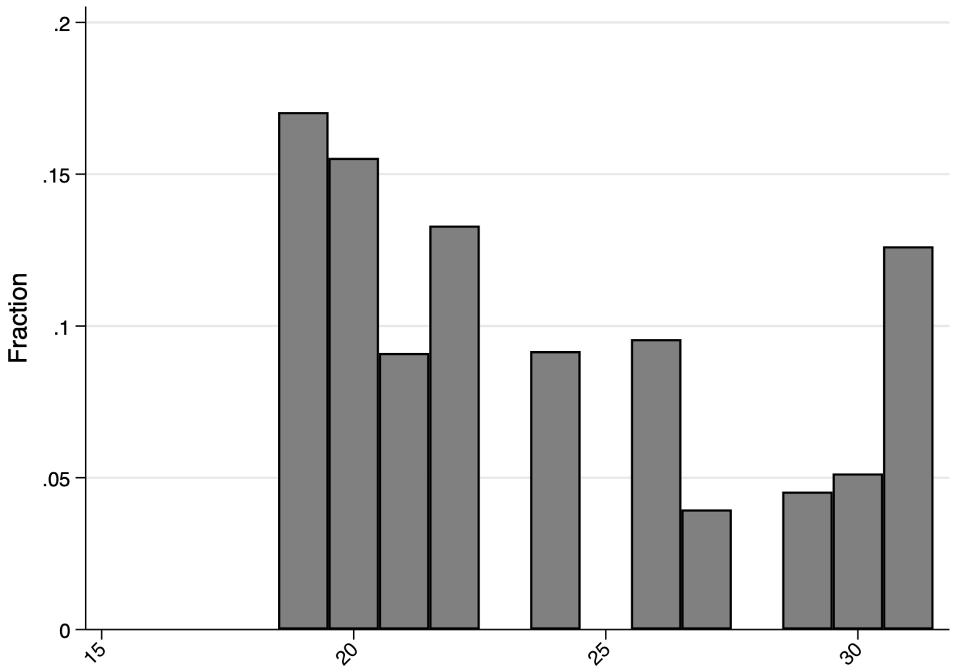
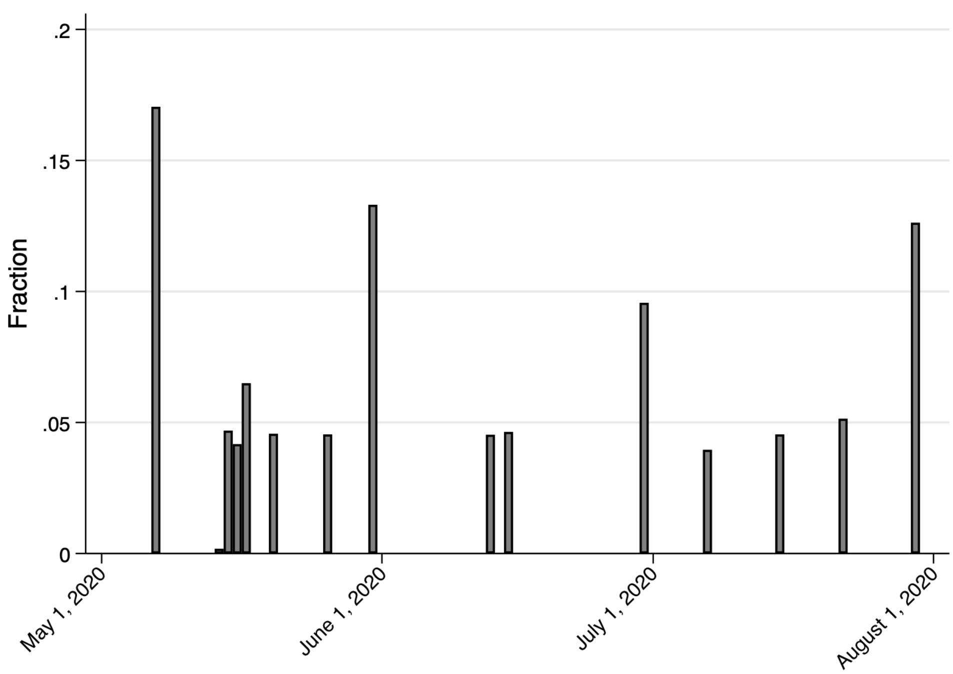


Figure 2: The Distribution of the Moratorium Expiration Dates



(a) Weeks



(b) Dates

Figure 3: 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

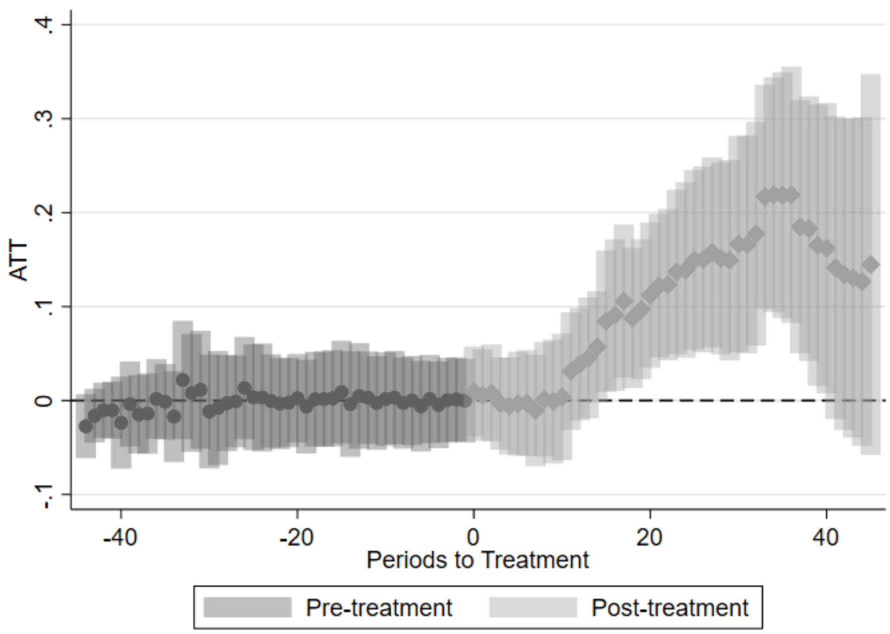
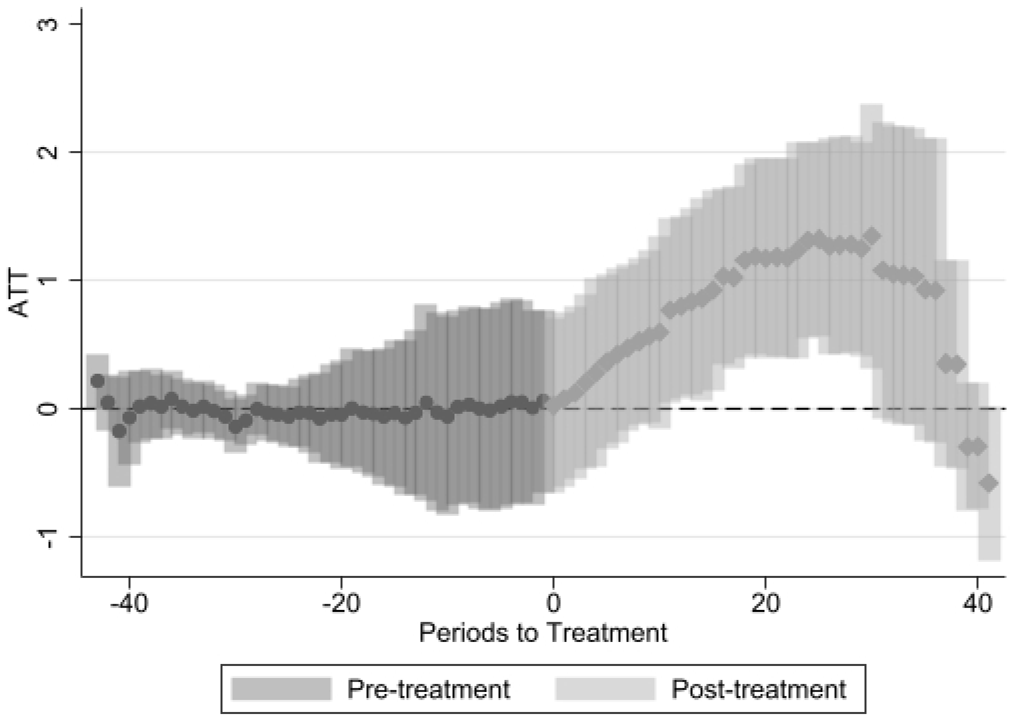


Figure 4: Event Study Coefficients from Callaway and Sant'Anna (2021)'s Estimator for the Natural Logarithm of the Relative Response Ratios for the Same Race without a Response Relative to the Share of Same Race with a Response with the Smoothing Parameter $h = 10$ and $\hat{\tau} = 45$ Days around Treatment



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 (**??**) and (**??**) 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

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

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.

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

Model	(1) Linear	(2) Linear	(3) Linear	(4) Linear	(5) Probit	(6) 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)
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	-	-
Gender	No	Yes	Yes	Yes	Yes	Yes
Educational Level	No	No	Yes	Yes	Yes	Yes
Inquiry Order	No	No	No	Yes	Yes	Yes

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 0.5736. 4) Standard errors in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

B Baseline Discrimination Specification

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 3.1 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 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 A4.

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.

D Tables and Figures

Table A2: 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 A3: 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

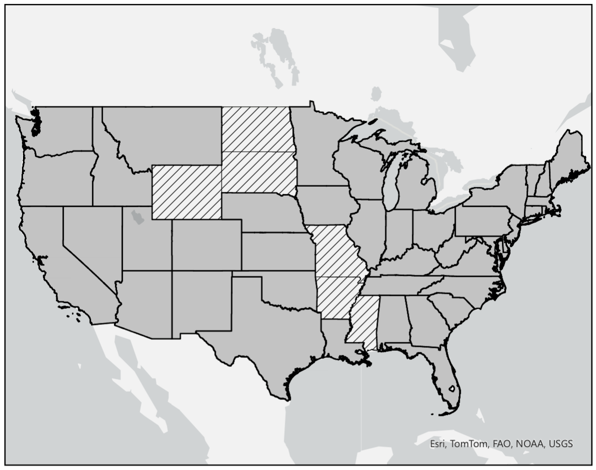
Table A4: Experiment to InfoUSA Address Match Rates

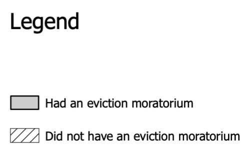
	Matching Step	New Matches	Total Matches	Share
1	Exact full-string address match in tracts from the experiment	6,077	6,077	69.2%
2	Exact house number + street name within a tract	+1,199	7,276	85.9%
3	Exact house number + street name within a county within tracts from the experiment	+62	7,338	86.6%
4	Fuzzy 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.

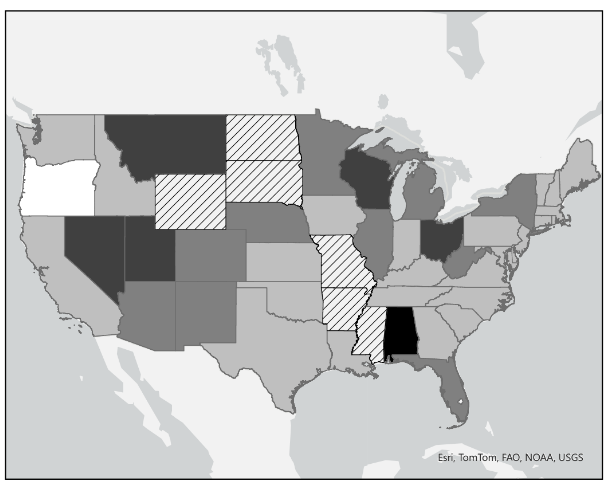
Figure A1: Eviction Moratoria across the U.S.

(a) States that Enacted Moratoria





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



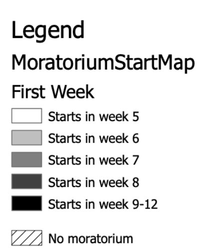
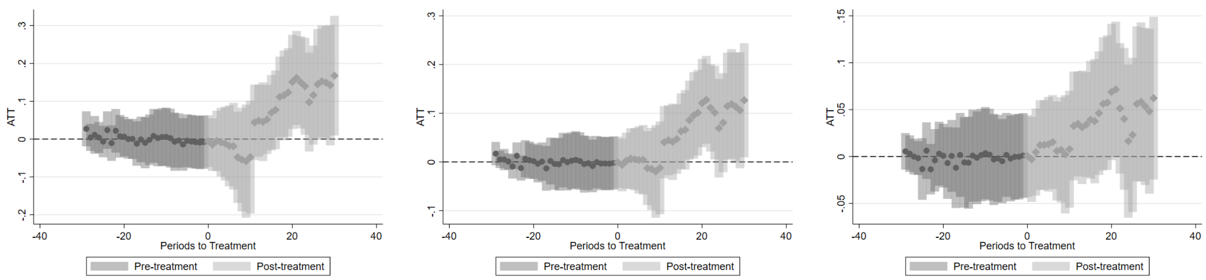
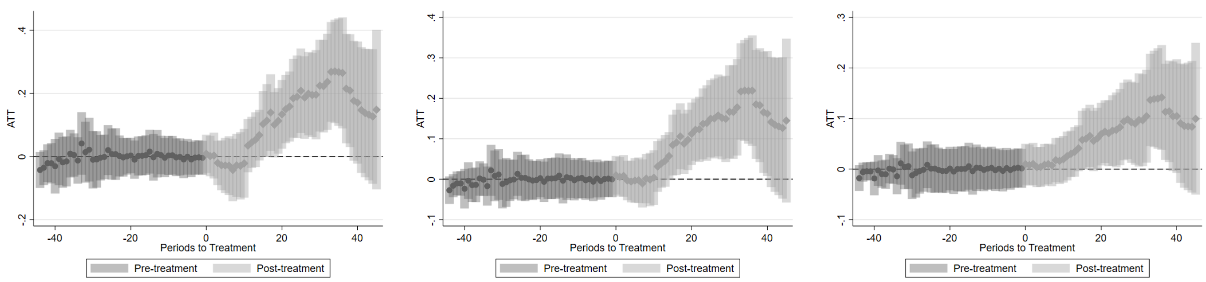


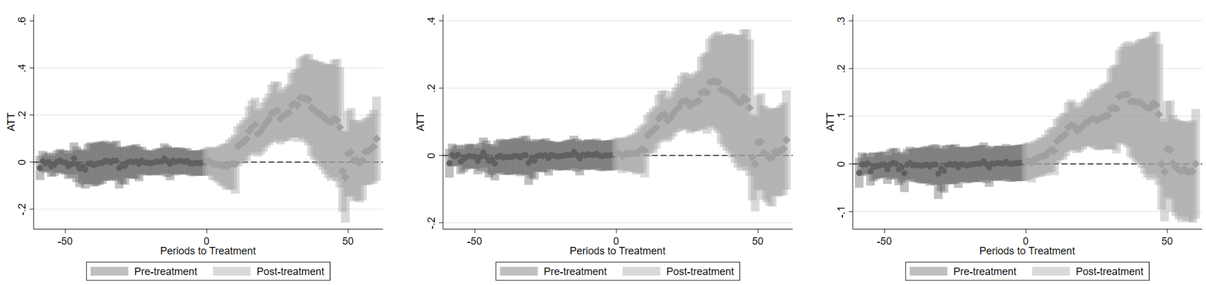
Figure A2: The Event Study Estimates of the Effect of the End of a Moratorium on the Natural Logarithm of the Relative Response Ratios for an African American Identity Relative to a White Identity from the Staggered DiD for Different Smoothing Parameters h and $\hat{\tau}$ Days Around Treatment



(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 $\hat{\tau} = 60$

Table A5: 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	0.067**	0.057**	0.036*
	(0.012, 0.121)	(0.009, 0.106)	(-0.005, 0.077)
Number of Observations	715	735	746
	Panel B: 45 days around treatment		
ATT	0.124***	0.084***	0.057**
	(0.041, 0.206)	(0.022, 0.146)	(0.009, 0.104)
Number of Observations	1122	1170	1192
	Panel C: 60 days around treatment		
ATT	0.098***	0.086***	0.068***
	(0.029, 0.166)	(0.031, 0.141)	(0.023, 0.113)
Number of Observations	1526	1582	1621

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