NBER WORKING PAPER SERIES

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Working Paper 32289
http://www.nber.org/papers/w32289

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2024, Revised March 2025

We thank Peter Christensen and Ignacio Sarmiento-Barbieri for sharing the experimental data from the correspondence study on housing discrimination and our discussants Nitzan Tzur-Ilan, Carlos Fernando Avenancio-León, Sofie R Waltl, and Matthew Freedman. The paper has benefited from the feedback of seminar and conference participants at UW-Madison, UCLA, University of Illinois Urbana-Champaign, Indiana University, the "Conference on Auctions, Firm Behavior, and Policy" at the University of Oklahoma, the NBER Summer Institute Real Estate Workshop, the Real Estate Finance and Investment Symposium at the University of Cambridge, and the UEA Conference. UW-Madison Institutional Review Board determined that this research is exempt from IRB review: submission ID number: 2024-0560. The authors do not have conflicts of interest to disclose. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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JEL No. J15, R31, R38

ABSTRACT

We show that pandemic-era eviction moratoria policies exacerbated discriminatory behavior by landlords in rental housing markets. We hand-collected data on state eviction moratoria start and end dates from government mandates and merged it with data from the largest correspondence study of the rental market, including over 25,000 landlord inquiries in the 50 largest U.S. cities during the spring and summer of 2020. Leveraging the staggered adoption of the eviction moratoria, we provide evidence that African Americans were disadvantaged in the search process while the moratorium was in place. We explain these findings through the lens of a housing search model.

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1 Introduction

The COVID-19 pandemic exacerbated housing precarity across the U.S. Widespread job losses increased financial distress among renters, raising the risk of eviction and foreclosure. At the same time, public health guidelines emphasized the necessity of stable housing to enable social distancing and reduce disease transmission. In response, governments at the federal, state, and local levels implemented a range of interventions to preserve housing stability, including direct rental assistance and eviction moratoria. The latter policies temporarily prohibited landlords from initiating eviction proceedings, effectively shutting down the eviction process in affected jurisdictions.

While eviction moratoria were designed to reduce displacement and curb the spread of COVID-19, their unintended consequences remain an open question. A growing literature examines their effectiveness in reducing evictions and mitigating public health risks (**???????**). However, these policies also restricted landlords' ability to replace tenants who defaulted on rent payments, potentially altering their incentives when selecting new renters. This constraint may have exacerbated discriminatory practices in rental markets, an unintended consequence that has received little empirical scrutiny.

We construct a novel dataset on state-level eviction moratoria by tracing each government mandate to its original source and coding the start and end dates. While prior work has documented pandemic-related eviction bans **?**, our data collection extends beyond COVID-19-specific measures to include moratoria implemented for other reasons, such as extreme weather events. This comprehensive approach allows us to separate pandemic-driven policies from broader eviction restrictions. We merge this dataset with the largest correspondence study of rental markets in the United States (Christensen et al., 2021), which comprises over 25,000 landlord inquiries across the 50 largest metropolitan areas during the spring and summer of 2020. The merged dataset allows us to analyze how eviction moratoria influenced racial disparities in rental market access. By leveraging the staggered timing of moratoria terminations, we isolate the impact of these policies on discrimination in the housing search process.

A potential concern in our empirical strategy is the endogenous timing of moratorium repeal. To address this, we show that socio-economic characteristics from the American Community Survey do not predict the timing of moratoria terminations, suggesting that their repeal was not systematically driven by underlying state-level demographic or economic factors. Additionally, we control for the number of daily COVID-19 infections to account for potential confounding effects related to public health conditions, and our results remain unchanged. Our findings are consistent with prior work suggesting that the end of eviction moratoria was often politically or administratively determined rather than driven by public health conditions (**?**).

We begin our analysis by developing a simple model in which landlords engage in forward-looking decision-making under an eviction moratorium. The model predicts that eviction restrictions exacerbate discriminatory behavior by limiting landlords' ability to evict tenants who default on rent. We test this prediction using a difference-in-differences framework that exploits variation in moratoria repeal across states. Our findings show that while eviction moratoria were in place, African American renters faced significantly higher rates of discrimination in rental inquiries. These results suggest that a policy designed to enhance housing security may have had the unintended consequence of making it more difficult for marginalized groups to access rental housing.

Our findings contribute to several literatures. First, we build on the extensive body of research documenting racial discrimination in a wide range of market activities. In the case of the housing market, racial discrimination can take place at various stages of the process, including home search (**??????**), negotiations over prices or rent (**?**), home appraisal (**?**), mortgage lending (**????**), and evictions (**??**). We extend this literature by documenting how eviction policy intensifies discrimination during the initial stage of the search process in the rental market. Discrimination that occurs at the initial search stage is particularly concerning because it could eliminate the possibility of a transaction for the minority home seeker before the rest of the process even has a chance to unfold.

Second, we contribute to research on the economic and social impacts of eviction policies. Prior studies explored who is most at risk of eviction (???) and highlight the consequences of eviction for tenants, including financial distress, homelessness, and adverse health outcomes (????????????). Other work examines the design and effectiveness of eviction-related policies, such as right-to-counsel programs (?) and tenant screening regulations (??). While most research focuses on the direct effects of eviction moratoria on displacement, we shift attention to their unintended consequences for discrimination in rental markets.

Finally, our study provides new insights into the broader debate on whether housing policies designed to protect vulnerable populations can have counterproductive effects. Prior work suggests that policies restricting landlord discretion-such as rent control and tenant screening regulations-may lead to lower housing supply or increased discrimination (??). Our findings highlight a similar dynamic, in which eviction restrictions may have unintended consequences for racial disparities in access to housing.

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 ?'s correspondence study. Section 4 presents our empirical strategy and baseline results. Section 5 addresses concerns about endogeneity and robustness and explores heterogeneity in the effects of eviction moratoria across demographic groups. Section 6 concludes.

2 Model

We use a simple search model to illustrate that discrimination increases with the implementation of an eviction moratorium. Assume there are two types of applicants for a rental property: a minority applicant with type $i = M$ and a white applicant with type $i = W$. Whenever an applicant is offered to lease a housing unit, the applicant accepts this offer and becomes a renter. The renter pays rent $R > 0$ every period with probability π_i and defaults with probability $1 - \pi_i$, where π_i is the probability of rent payment as perceived by a landlord which could differ by the type of applicant. If the landlord perceives that the creditworthiness of a minority applicant is lower than that of a white applicant, $\pi_M < \pi_W$, we interpret it as statistical discrimination. If the renter pays the rent, we assume that she stays in the rented unit. If the renter defaults on paying rent, her landlord evicts her and starts searching for another tenant. The landlord discounts future payoffs using a discount factor $\beta < 1$.

The landlord's per-period payoff includes the expected rent $\pi_i R$ net of a utility loss from leasing to an applicant of type i, $\kappa_i < R$. We normalize this utility loss from leasing to a white applicant to zero, $\kappa_W \equiv 0$, and denote $\kappa_M = \kappa$ to simplify notation. Whenever $\kappa > 0$, we interpret this as taste-based discrimination.

The landlord chooses which type of applicant to respond to with a lease offer. Each response is costly. Assume that the difference between the cost of calling a minority applicant and the cost of calling a white applicant is a random variable ψ that is zero in expectation but can be negative or positive. Denote the cumulative distribution function and probability density function of ψ as $F(.)$ and $f(.)$, respectively, and impose a technical assumption that $\lim_{\psi \to \psi_{\min}} \psi F(\psi) = 0$. ψ proxies for randomness in the search process and is not a source of discrimination in itself.

To solve the landlord's problem, denote the landlord's option value to lease an empty rental unit as V, and solve the problem backward. The value of a rental unit occupied by an applicant of type i for the landlord, u_i, is

$$u_i = \pi_i(R + \beta u_i) + (1 - \pi_i)(0 + \beta V) - \kappa_i, \tag{1}$$

The landlord gets rent R and a discounted value of releasing the unit to the current applicant of type i that if the tenant pays the rent that happens with probability π_i. With complementary probability, $1 - \pi_i$, the tenant defaults. Then the landlord evicts the current applicant and gets a discounted option value to search for a new tenant βV. The landlord incurs the utility loss κ_i from a current tenant of type i living in the unit.

To maximize the value of a vacant unit V, the landlord responds to a minority applicant if $u_M - \psi > u_W$, and to a white applicant otherwise. This optimal choice results in the probability of replying to an inquiry of a minority applicant of

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

Denote the difference in the utilities as $\Delta u = u_M - u_W$, then the value of searching for a tenant for an empty unit is

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

Appendix A includes all the proofs. The equilibrium is a triple $\{u_M^*, u_W^*, V^*\}$ that satisfies (1) and (3).

Eviction Moratorium. To consider the effect of the eviction moratorium, assume that the moratorium lasts for one period during which the landlord cannot evict a tenant even if the tenant defaults. However, the tenant can be evicted after the eviction moratorium ends. The value of the unit leased to an applicant of type i changes to

$$u_i = \pi_i(R + \beta u_i) + (1 - \pi_i)(0 + 0 - \beta\kappa_i + \beta^2 V) - \kappa_i. \tag{4}$$

The payoff in case of the tenant's default is now $-\beta\kappa_i + \beta^2 V$ because the landlord has to suffer a utility loss of κ_i for the duration of the moratorium and can release the unit only with a one-period delay which discounts his payoff to $\beta^2 V$. The option value of leasing a unit V is still determined by (3). The new equilibrium is a triple $\{\bar{u}_W, \bar{u}_W, \bar{V}\}$ that satisfies (3) and (4).

To study the effect of the moratorium on discrimination, notice from (2) that the effect of the moratorium on the probability of a minority applicant getting a response from a landlord is an increasing function of the difference between the value of a unit leased to a minority applicant and the value of a unit leased to a white applicant, $\Delta u = u_M - u_W$. The change in the probability that a minority applicant gets a response from the landlord after the moratorium ends depends on the difference in differences of these utilities:

$$\Delta^2 u \equiv \Delta\bar{u} - \Delta u^* = (\bar{u}_M - \bar{u}_W) - (u_M^* - u_W^*). \tag{5}$$

Consider a case of purely taste-based discrimination: $\pi_M = \pi_W = \pi$ and $\kappa > 0$. Then the difference between utilities from two applicants drops during the moratorium:

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

This is similar to a case of statistical discrimination, when we have $\pi_W > \pi_M$ and $\kappa = 0$ and

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

The model illustrates a simple intuition: when the eviction moratorium prohibits the landlord from evicting non-paying tenants, any original discriminatory behavior is amplified. In other words, the termination of the moratorium reduces discrimination. The extent to which this prediction plays-out in data is an empirical question that we address in the remainder of this paper.

3 Data

3.1 Correspondence Study

We test the predictions of this model using data collected as part of a correspondence study undertaken by **?** in the United States in the spring and summer of 2020. Christensen's team at the National Center for Supercomputing Applications developed a software bot that sent a randomized sequence of inquiries from African American, Hispanic, and white identities to 8,476 property managers across the fifty largest metropolitan housing markets in the United States on an online rental housing platform.[1]

Listings in downtown and suburban areas of each market were targeted on the day following the day on which each property was listed on the platform. Following the listing, a three-day sequence of inquiries was initiated by the bot, using identities drawn randomly from a set of 18 first/last name pairs summarized in Table 1.[2] Recognizing that names can encompass other unobservable traits like income (**??**), the bot refined its sampling of first names by incorporating gender and maternal educational attainment. Property managers never received inquiries from two different identities on the same day. Property manager responses were categorized as a positive response if they arrived within seven days and confirmed the availability of the property.

The final inquires dataset includes 25,428 interactions between property managers and fictitious renters who engaged in the initial stage of the search process that can be used to study patterns of discrimination encountered in at the initial stage of applying for a lease.

3.2 Development of an Eviction Moratoria Database

To analyze how responses of the landlords during the correspondence study were affected by enactment of the eviction moratoria, we collect the data on the start and end dates of moratoria. Our research builds upon the seminal work of **?**, whose analysis of COVID-19 eviction moratoria established a crucial foundation for understanding this policy response. Their work, which cataloged actions by governors, legislators, and other state-level authorities, serves as a valuable springboard for our broader study of housing stabilization policies across the United States.

[1] Metropolitan housing markets were delineated using Core-Based Statistical Areas (CBSAs) as defined by the US Census.

[2] 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.

We expand the scope of inquiry beyond COVID-specific moratoria to encompass all forms of eviction prevention measures. This includes policies enacted through legislative action, executive orders, or the discretionary enforcement decisions by local sheriffs, regardless of whether they were initiated due to COVID-19 or extreme weather conditions. Our aim is to create a comprehensive identification and characterization of eviction moratoria, encompassing all implementation mechanisms. To achieve this, we conducted a detailed review of each state's eviction moratoria policies, employing a wide range of sources to compile a robust and accurate dataset. The National Apartment Association's COVID-19 State and Local Eviction Moratorium Report was a pivotal resource, offering timely and in-depth insights into the policy landscape.

To guarantee the data's veracity and internal consistency, a co-author with legal expertise meticulously traced each finding back to its primary sources. This process ensured not only data accuracy but also contextualized them within the broader analytical framework, strengthening the study's overall rigor. Furthermore, our methodology included consideration of additional eviction protections, such as seasonal restrictions that halt evictions during cold weather months or other emergency conditions. Recognizing the importance of these measures in protecting vulnerable populations, we thoroughly documented instances where eviction protections were enhanced by such factors, thus providing a more nuanced view of tenant protections during the pandemic and beyond. By revisiting each state's strategy and adding data on cold weather eviction bans and other measures, we developed an independent database. While informed by the initial work of **?**, our database might show slight variations due to our broader criteria and source verification process. These differences underline our effort to capture the entire spectrum of eviction moratoria, including those prompted by weather-related and emergency conditions not explicitly addressed in the original database.

Our enhanced database aims to offer a comprehensive resource for understanding the complex nature of eviction moratoria during a significant public health and economic crisis. By incorporating additional protective measures and verifying our sources through rigorous legal scrutiny, we aspire to present a richer, more detailed portrait of the policies designed to prevent housing displacement and protect tenants across the United States.

4 Results

4.1 Baseline Discrimination Specification

Before we study the effect of the eviction moratoria, we demonstrate persistent discrimination of the African Americans and Hispanics in our sample.

The experimental design described in Section 3.1 involves a sequence of binary decisions j, where the manager of a given property i decides whether to respond ($Response_{ij} = 1$) or not ($Response_{ij} = 0$) with $j = 1, 2, 3$. We begin by estimating the magnitude of discriminatory constraints using the following linear probability model, which limits identifying variation to within-property differences in behavior:

$$Response_{ij} = \delta_i + \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + X_j^{'}\theta + \epsilon_{ij}, \tag{8}$$

where $African\ American_j$ and $Hispanic_j$ are indicator variables that take a value of one if the race group associated with the identity is either African American or Hispanic; and zero otherwise. X_j is a vector of identity-specific characteristics: gender, maternal education level, and the order in which the inquiry was sent. δ_i is a property-level fixed effect. Given that names are drawn randomly and balanced across gender, education level, and inquiry order, estimates of β should be robust to the inclusion/omission of X_j. **?** demonstrate that estimates are consistent when including/omitting control variables and when using a conditional logit vs. a linear probability model. We estimate (8) using all weeks and states. Columns (1)-(4) of Table 2 show the estimates from this linear probability model. Columns (5) and (6) of Table 2 show the estimates from the Probit and Logit models. The estimates confirm the presence of discrimination against renters of color.

4.2 Defining Treatment

Most moratoria that were initiated over a relatively short period of time near the start of the pandemic. Hence, instead of focusing on the beginning of a moratorium, we focus on its termination.[3] Moratoria ended at different times over the course of the summer of 2020 before the CARES Act put into place a national moratorium on September 4, 2020. Figure 1 shows last week of the eviction moratorium across different states. Figures 5a and 5b in the Appendix show which states did and did not implement an eviction moratorium, and, for those states that did, the week when the moratorium started.

To arrive at our analysis sample, we drop 8 states in which a moratorium was never enacted, and we drop all observations in each state before a moratorium.

Our correspondence study starts with the first inquiry on February 6, 2020, and ends with the last inquiry on July 31, 2020. Because we drop observations before the start of the moratorium, the earliest date of the inquiry in our analysis sample is March 13, 2020. Figure 2 shows when the moratoria were lifted in our sample. The earliest date when a state lifted the eviction moratorium is May 7, 2020, and the latest date when a state lifted the moratorium in our sample is July 30, 2020.

We define treatment as the end of an eviction moratorium that had previously been in place so that $Treatment_j$ is an indicator variable that takes a value of one if an inquiry was sent after the end of the moratorium.

4.3 Potential Endogeneity of Treatment

Eviction moratoria were started in different states at approximately the same time. However, the terminations of the moratoria were more spread out over time and could have been endogenously determined. The decision to end the moratorium could have been affected by COVID-19 infections. To account for this, we show that our results are robust to controlling

[3]Eviction moratoria expirations have been used elsewhere in the literature on policy impacts related to COVID-19. See **?** as an example.

for the number of daily COVID-19 cases in a state. This result is consistent with **?**, who suggest that the number of COVID-19 infections was not an important determinant of the termination date of the moratorium. In particular, they "find little to no evidence that public health conditions served as a meaningful predictor of the timing of moratoria predictions" and "eviction protections were very often rolled back even as the prevalence of COVID-19 was increasing in a given state".

To assess whether states selected the last day of the eviction moratorium based on other state-level characteristics, we employ a two-stage procedure. We first regress the last day of the eviction moratorium on the number of daily COVID-19 infections in a state and state fixed effects. Then we the regress the estimated state fixed effects from the first stage on socio-economic variables from the American Community Survey and the first day of the moratorium to control for the moratorium length. Table 3 shows that all the variables we considered are statistically insignificant in predicting the termination of the eviction moratoria. These state-level characteristics are absorbed by the property fixed effects in the Difference-in-Difference specification. The staggered Difference-in-Differences procedure that we implement by using csdid and csdid2 commands in Stata cannot estimate the effect with this many controls.[4] Hence, we proceed with only including the number of daily COVID-19 cases in a state as our control.

4.4 Difference-in-Differences Specification

To study of how the discriminatory behavior changed when moratoria ended, we start by estimating a Difference-in-Differences (DiD) specification:

$$\begin{aligned} Response_{ijt} = \delta_i &+ \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + \beta^{T} Treatment_{jt} \\ &+ \beta^{AAT} Treatment_{jt} \times African\ American_j \\ &+ \beta^{HT} Treatment_{jt} \times Hispanic_j + X_j'\theta + \epsilon_{ijt}, \end{aligned} \tag{9}$$

[4] The results is an empty matrix of the estimates.

where i is a rental property, j is an inquiring identity, t is a day. $Hispanic_j$ and $African$ $American_j$ are indicator variables that take a value of one if the race group associated with the identity is either African American or Hispanic, and zero otherwise. X_j are other attributes associated with identity j (gender, maternal education, and inquiry order). δ_i is a rental property fixed effect. $Response_{ijt}$ takes a value of one if inquiry by identity j to property i on day t yields a response, and zero otherwise.

Table 4 shows the results. Columns (1) through (3) include specifications that control for the index of the stringency of the eviction policies, the number of evictions in a county in 2018, and week fixed effects, but do not include property fixed effects.[5] Column (4) further includes the property fixed effects. Column (5) adds the number of daily COVID-19 infections per 100,000 population in a state. Column (6) further clusters the errors by state. The coefficient on the interaction between the African American and Hispanic indicators and the Treatment dummy is positive but not statistically significant.

The staggered nature of eviction moratoria endings across states during the spring and summer of 2020 introduces potential bias in the difference-in-differences estimates. Specifically, states where the eviction moratoria ended earlier serve as the control group for states that ended their moratoria later. As a result, the effect of the moratorium in the earlier group is subtracted from the effect in the later group, potentially contaminating the estimates (**?**). To address this issue, we employ an estimator that accounts for staggered treatment.

4.5 Staggered Differences-in-Differences

Eviction moratoria come to an end in different states over a span of two months, which makes the use of staggered treatment in a difference-in-differences framework (Callaway and Sant'Anna 2021) relevant for our analysis. Because listings are not observed at every point in time to use them as a unit of analysis, we implement staggered DiD by creating a panel of data describing discrimination across states and over time. To this end, we carry out the

[5] We use the data on the number of evictions from **?**, which is the latest available data prior to the pandemic.

? (CS) estimation using a two-stage procedure described below.

Stage #1: Discrimination Coefficients

We begin by modeling the level of discrimination in each state in our data on each day, denoted by $\tau = 1, ..., 177$, between February 6 and July 31, 2020 using a predicted probability to get a response to an inquiry from a logit estimator. We use a logit model to ensure that the estimated probability of response is between zero and one. To get these predicted probabilities, we estimate a separate logit regression for each state on each day using all of that state's observations weighted by how far they are in time from the day in question:

$$Response_{ijkt} = \beta^{AA}_{k\tau} African\ American_j + \beta^{H}_{k\tau} Hispanic_j + X'_j \theta_{k\tau} + u_{ijkt},$$

where i denotes a rental property, j is the inquiring identity, k is a state, and t is the day on which the inquiry took place. τ denotes the day to which the resulting regression coefficients correspond to. $African\ American_j$ and $Hispanic_j$ are indicator variables that take a value of one if the race group associated with the identity is either African American or Hispanic, and zero otherwise. X_j are other attributes associated with identity j in the experiment conducted on property i in state k on day t (gender, maternal education, and inquiry order). $Response_{ijkt}$ take a value of one if inquiry by identity j to property i in state k on day t yields a response, and zero otherwise. $\beta^{R}_{k\tau}$ is the coefficient describing the effect on the probability of a response on day τ in state k to an identity with associated race $R \in \{$African American (AA), Hispanic (H)$\}$ relative to a white (W) identity. We weight each observation using $\omega^{\tau}_{ijkt} = 1/(h\sqrt{2\pi}) \exp(-((t-\tau)/h)^2/2)$ to give observations on day t closer in time to τ more weight. The smoothing parameter h determines how much weight is given to inquiries made on nearby days.

Stage #2: Moratorium Effect

With the procedure described above, we recover $[\beta^R_{k\tau}]_{\tau=1,...,T}$ for each state k, day τ, and race $R \in \{\text{AA, H}\}$. We then calculate the predicted probability of a response to an inquiry from a male with a low maternal education who sent a message first (before the other two inquiries were sent) of each race $R \in \{\text{AA, H, W}\}$. These values are denoted as $\rho^R_{k\tau} \equiv P(Response_{ijk\tau} = 1|R_j = 1)$. We use these values to calculate the relative response ratio for an individual of race $R \in \{\text{AA, H}\}$ relative to a white individual $R = W$ on day τ in state k, $\rho^R_{k\tau}/\rho^W_{k\tau}$. These estimated relative response ratios $\rho^R_{k\tau}/\rho^W_{k\tau}$ become the data for the second stage of our estimation procedure, which applies the CS staggered difference-in-differences procedure.

Before implementing that procedure, we make two cuts to the sample of relative response ratios. First, we drop all observations before the start of the moratorium in state k and for which we have fewer than 100 inquiries to estimate $\beta^R_{k\tau}$. Second, we keep observations for which $|\tau - \tau^*_k| \leq \hat{\tau}$, where τ^*_k is the day on which treatment occurs (moratorium ends) in state k, and $\hat{\tau} = 30$, 45, or 60 days defines the window around treatment. Therefore, the second stage estimation procedure uses estimates of discrimination within the $\hat{\tau}$ window around the end of the moratorium in the state in question.

To illustrate the dynamics of the relative response ratios, we plot the event study coefficients from a regression of the relative response ratios for an African American identity, $\rho^R_{k\tau}/\rho^W_{k\tau}$, on indicators for whether the difference between the current day and the end of the moratorium is within a specific time window — $\tau - \tau^*_k \in [-60, -45)$, $[-45, -30)$, $[-30, -15)$, $[-15, 0)$, $[0, 15)$, $[15, 30)$, $[30, 45)$, $[45, 60)$ —and state fixed effects. Figure 3 shows these estimates with 90% confidence intervals. The relative response ratios rise after the end of the moratoria. We confirm these findings by performing the staggered CSDiD estimation.

We follow **?**'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. Hence, not-yet-treated states become the controls for the treated states.

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 and use these observations to run the regression

$$\frac{\rho^R_{k\tau}}{\rho^W_{k\tau}} = \alpha^g_0 + \alpha^g_1 TREAT^g_k + \alpha^g_2 \mathbb{1}\{\tau = t\} + \alpha^{g,t}_3 TREAT^g_k \times \mathbb{1}\{\tau = t\} + \nu_{k\tau}, \tag{10}$$

where the left-hand side variable is the relative response ratio for an individual of race $R \in (AA,\ H)$ relative to a white individual on day τ in state k. $TREAT^g_k$ takes a value of one if state k was treated on day g, and $\mathbb{1}\{\tau = t\}$ takes a value of one if this observation is for period $\tau = t$. The estimate of $\alpha^{g,t}_3$ is the estimate of the average treatment effect on day t for states that ended the eviction moratorium on day g, $ATT(g,t)$.[6] Our baseline specification does not incorporate any additional controls, but we check that the results are robust to controlling for the number of daily COVID-19 cases in a state. This yields an average treatment effect on the treated for states treated on day g. The CS procedure provides weights to combine these estimates into a single Average Treatment Effect of the Treated (ATT) that we report in tables.

Table 5 presents our results for (1) different smoothing parameters $h = 7, 10, 15$, (2) days around treatment, $\hat{\tau}$, of 30, 45, and 60 days, and (3) without any controls and with the number of daily COVID-19 cases in a state as a control. The estimates are positive, suggesting that the end of the moratorium increases the relative response ratio for African American identities. Hence, an eviction moratorium significantly disadvantages African American identities in the housing search process relative to their white counterparts, and racial discrimination intensified during the eviction moratorium.

Most of the estimates of the Average Treatment Effect on Treated (ATT) from Table 5 are within 0.06-0.10 range. To understand the magnitude of the estimated effect, note that white identities in our sample received a response 57.36% of the time during moratoria. African

[6] This follows Remark 3 in **?**.

American and Hispanic identities are less likely to receive a response compared to a white identity when a moratorium is in place. The estimated coefficient on $African\ American$ from the previous Section implies that an African American identity with the same maternal education, gender, and inquiry order would only receive a response 51.26% of the time. This implies a relative response ratio of 0.89 during a moratorium. When the moratorium expires, the response to an African American identity increases by an additional 0.06-0.10. This increases the post-moratoria relative response ratio for African American identities to 0.95-0.99, almost closing the gap in the response rates to African American and white identities.

Figure 4 shows the event study estimates from **?**'s estimator for the logit smoothing parameter $h = 10$ and $\hat{\tau} = 45$ days around the end of an eviction moratorium. There are no pre-trends in the relative response rates to African Americans prior to the treatment, but once the eviction moratorium ends the relative response ratios rise, indicating that the discrimination decreased once the moratorium was repealed. The event studies for other values of h and $\hat{\tau}$ are presented in Figure 6 in the Appendix and show consistent results.

5 Robustness and Treatment Heterogeneity

5.1 Bootstrap

Because the staggered DiD estimation from the previous section is a two-stage procedure, the error in the estimates of state × day discrimination coefficients $[\beta^R_{k\tau}]_{\tau=1,\ldots,T}$ from the first stage needs to be accounted for in the second stage of the procedure. Given the complicated properties of that error , we employ a bootstrap procedure. In particular, we generate a bootstrap sample clustering on states (i.e., take a random sample of states with replacement and use all of the days of data for those states following their implementation of a moratorium) of the first-stage state × day relative response ratio. Next, we use these estimates to run the CS multi-period differences-in-differences procedure, which yields an estimate of the overall treatment effect for this bootstrap draw. We repeat these steps 1,000 times, going back each time to a new bootstrap sample clustered on states. We then report bias-corrected estimates and confidence intervals.

Table 6 shows the estimates for the response to an African American identity relative to a white identity. Most of the estimates are positive and significant at a 1% significance level, suggesting that the end of the moratorium leads to increased responses to African American identities. Therefore, our results are robust to potential errors introduced by the estimation of the discrimination coefficients in the first stage of our two-stage staggered DiD procedure.

5.2 ? Eviction Moratoria Data

The first comprehensive database on pandemic eviction moratoriums was compiled by **?**. Using this database, we updated the start and end dates of the moratoriums for our analysis. However, our results remain robust to using the original data from **?**. To demonstrate this, we re-estimated our DiD and staggered DiD models with their data.

Tables 7 and 8 present these results. In Table 7, the interaction term between race and treatment dummies is positive and statistically significant. Table 8 shows a positive and significant estimate of the Average Treatment Effect on the Treated in specifications without controlling for the number of COVID-19 cases, but when we include them as a control. The differences in the estimates are due to updates of the start and end of the moratoria that we made to align with the legislative actions and broadened scope of the reason for initiating the moratoriaama, see Section 3.2. Overall, these findings confirm the intensified discrimination during eviction moratoriums that we identified in Section 4.

5.3 Heterogeneity

We examine results with respect to two forms of heterogeneity: gender identity and the level of rents, which allows us to perform a test for statistical discrimination. We do not find evidence of statistically significant differences by gender, but point estimates do suggest that males may face greater discrimination than females, and that this is particularly true for African Americans, see Appendix Tables 9 and 10.

With respect to the type of discrimination that we observe, the model predicts that the change in the response rate to minorities is sensitive to the value of a rental unit to the landlord $\bar{V}$ in case of statistical discrimination (equation (7)) but not in case of taste-based discrimination (equation (6)). Because the value of the rental unit depends positively on rents, we can test for the presence of statistical discrimination by comparing the size of the ATT in areas with high and low rents. Results in Appendix Tables 11 and 12 show that estimates for states with high rents fall in the range 0.03 - 0.15, which is higher than the estimates for states with low rents, 0-0.10. The differences in the estimates are not, however, statistically significant, and are therefore only suggestive of the presence of statistical discrimination.

6 Conclusion

While moratoria on evictions may have played a role in preventing the spread of disease during the COVID-19 pandemic and accompanying economic turmoil (**?**), they may have also exacerbated racial inequities by putting minorities at a disadvantage in the housing search process. Given the lack of affordable housing in many markets, increased discrimination in the housing search process can have important long-run implications. Using data collected as part of a correspondence study conducted by **?** during the pandemic, we show that this detrimental impact is particularly important for African American renters, especially men. While eviction moratoria may prove to be important policy tools in response to future public health emergencies, our results suggest that they need to be accompanied by stricter enforcement of fair housing laws that prohibit discriminatory practices.

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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: 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	-0.055***	-0.055***	-0.055***	-0.055***	-0.142***	-0.229***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.020)	(0.032)
Hispanic	-0.027***	-0.027***	-0.027***	-0.028***	-0.071***	-0.115***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.020)	(0.032)
Constant	0.605***	0.622***	0.633***	0.661***	0.410***	0.658***
	(0.005)	(0.006)	(0.008)	(0.009)	(0.023)	(0.037)
Observations	24,194	24,194	24,194	24,194	24,194	24,194
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.

Table 3: 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 4: Impact of an End of a Moratorium on Likelihood of Receiving a Response

	Dependent Variable: Response					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.050***	-0.066***	-0.048**	0.006	0.006	0.006
	(0.017)	(0.019)	(0.019)	(0.055)	(0.055)	(0.049)
African American	-0.054***	-0.055***	-0.054***	-0.053***	-0.053***	-0.053***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.012)
African American x Treatment	0.002	0.019	0.021	0.002	0.002	0.002
	(0.017)	(0.019)	(0.019)	(0.017)	(0.017)	(0.022)
Hispanic	-0.033***	-0.031***	-0.031***	-0.032***	-0.032***	-0.032***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)
Hispanic x Treatment	0.020	0.036*	0.036*	0.022	0.022	0.022
	(0.017)	(0.019)	(0.019)	(0.017)	(0.017)	(0.022)
#Evictions in 2018, thousands		-0.001***	-0.001***			
		(0.000)	(0.000)			
Stringency Index	0.002***	0.002***				
	(0.001)	(0.001)				
COVID Cases per 100k					0.000	0.000
					(0.000)	(0.000)
Constant	0.544***	0.550***	0.607***	0.614***	0.593***	0.593***
	(0.039)	(0.042)	(0.161)	(0.017)	(0.125)	(0.134)
Observations	17,734	15,588	15,737	17,883	17,883	17,883
R-squared				0.025	0.025	0.025
Number of addresses	5,977	5,256	5,304	6,025	6,025	6,025
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Educational Level	Yes	Yes	Yes	Yes	Yes	Yes
Inquiry Order	Yes	Yes	Yes	Yes	Yes	Yes
Weekly FEs	No	No	Yes	Yes	Yes	Yes
Property FEs	No	No	No	Yes	Yes	Yes
Clustered at State-level	No	No	No	No	No	Yes

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) Standard errors in parentheses. 4) ***, **, 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 for African American Applicants 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	0.076**	0.293***	0.069***	0.193***	0.050***	0.107***
	(0.011, 0.141)	(0.147, 0.440)	(0.021, 0.117)	(0.104, 0.282)	(0.014, 0.085)	(0.058, 0.155)
Number of Observations	718	770	718	770	718	770
	Panel B: 45 days around treatment					
ATT	0.100***	0.086***	0.083***	0.082***	0.056***	0.066***
	(0.051, 0.149)	(0.040, 0.133)	(0.046, 0.120)	(0.050, 0.115)	(0.025, 0.088)	(0.035, 0.097)
Number of Observations	1155	1217	1155	1217	1155	1217
	Panel C: 60 days around treatment					
ATT	0.122***	0.114***	0.096***	0.085***	0.062***	0.056***
	(0.060, 0.184)	(0.051, 0.177)	(0.048, 0.144)	(0.036, 0.135)	(0.024, 0.101)	(0.017, 0.094)
Number of Observations	1625	1652	1625	1652	1625	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) 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: Boostrapped Staggered DiD Estimates of the Effect of the End in the Moratorium on the Relative Response Ratio for African American Applicants 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	0.094	0.519***	0.083*	0.343***	0.059**	0.195***
	(-0.033, 0.289)	(0.254, 0.703)	(-0.014, 0.189)	(0.163, 0.469)	(0.003, 0.137)	(0.074, 0.319)
Number of Observations	770	718	770	718	770	718
	Panel B: 45 days around treatment					
ATT	0.113**	0.067	0.093**	0.071	0.063**	0.059
	(0.017, 0.198)	(-0.054, 1.045)	(0.018, 0.155)	(-0.031, 1.230)	(0.013, 0.108)	(-0.031, 1.301)
Number of Observations	1217	1155	1217	1155	1217	1155
	Panel C: 60 days around treatment					
ATT	0.134***	0.292**	0.102***	0.227**	0.067**	0.180*
	(0.039, 0.291)	(0.020, 1.068)	(0.026, 0.223)	(0.014, 0.751)	(0.011, 0.159)	(-0.002, 0.789)
Number of Observations	1652	1625	1652	1625	1652	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% bootstrapped bias-corrected confidence intervals in parentheses, 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 7: Difference-in-Difference Estimates Using ?'s Data

	Dependent Variable: Response					
	(1)	(2)	(3)	(4)	(5)	(6)
	Response	Response	Response	Response	Response	Response
Treatment	-0.053***	-0.054**	-0.031	0.038	0.039	0.039
	(0.019)	(0.021)	(0.021)	(0.059)	(0.060)	(0.046)
African American	-0.060***	-0.060***	-0.061***	-0.059***	-0.059***	-0.059***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.014)
African American x Treatment	0.040**	0.055***	0.059***	0.042**	0.042**	0.042*
	(0.019)	(0.021)	(0.020)	(0.019)	(0.019)	(0.024)
Hispanic	-0.035***	-0.032***	-0.032***	-0.035***	-0.035***	-0.035***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Hispanic x Treatment	0.039**	0.050**	0.054***	0.043**	0.043**	0.043*
	(0.019)	(0.021)	(0.020)	(0.019)	(0.019)	(0.022)
#Evictions in 2018, thousands		-0.001***	-0.001***			
		(0.000)	(0.000)			
Stringency Index	0.002***	0.002***				
	(0.001)	(0.001)				
COVID Cases per 100k					0.000	0.000
					(0.000)	(0.000)
Constant	0.499***	0.501***	0.787***	0.634***	0.603***	0.619***
	(0.047)	(0.051)	(0.047)	(0.020)	(0.146)	(0.160)
Observations	14,799	13,004	13,153	14,948	14,948	14,948
R-squared				0.025	0.025	0.025
Number of addresses	4,974	4,373	4,421	5,022	5,022	5,022
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Educational Level	Yes	Yes	Yes	Yes	Yes	Yes
Inquiry Order	Yes	Yes	Yes	Yes	Yes	Yes
Weekly FEs	No	No	Yes	Yes	Yes	Yes
Property FEs	No	No	No	Yes	Yes	Yes
Clustered at State-level	No	No	No	No	No	Yes

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

Table 8: Staggered DiD Estimates of the Effect of the End of Moratorium on the Relative Response Ratio for African American Applicants Relative to White Applicants Using **?**'s Data

	(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	0.112**	-0.349**	0.083**	-0.273*	0.060**	-0.240*
	(0.021, 0.203)	(-0.647, -0.051)	(0.012, 0.153)	(-0.557, 0.012)	(0.002, 0.118)	(-0.526, 0.046)
Number of Observations	706	838	706	838	706	838
			Panel B: 45 days around treatment			
ATT	0.097***	-0.415***	0.068**	-0.342**	0.043	-0.296*
	(0.027, 0.168)	(-0.715, -0.116)	(0.009, 0.127)	(-0.643, -0.041)	(-0.010, 0.096)	(-0.602, 0.010)
Number of Observations	1083	1241	1083	1241	1078	1236
			Panel C: 60 days around treatment			
ATT	0.100**	-0.615**	0.074**	-0.534*	0.048	-0.474
	(0.022, 0.178)	(-1.173, -0.056)	(0.007, 0.141)	(-1.158, 0.089)	(-0.012, 0.108)	(-1.115, 0.168)
Number of Observations	1511	1617	1511	1617	1488	1594

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 9: Estimates for Males

	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
			Panel A: 30 days around treatment			
ATT	0.076**	0.293***	0.069***	0.193***	0.050***	0.107***
	(0.011, 0.141)	(0.147, 0.440)	(0.021, 0.117)	(0.104, 0.282)	(0.014, 0.085)	(0.058, 0.155)
Number of Observations	718	770	718	770	718	770
			Panel B: 45 days around treatment			
ATT	0.100***	0.086***	0.083***	0.082***	0.056***	0.066***
	(0.051, 0.149)	(0.040, 0.133)	(0.046, 0.120)	(0.050, 0.115)	(0.025, 0.088)	(0.035, 0.097)
Number of Observations	1155	1217	1155	1217	1155	1217
			Panel C: 60 days around treatment			
ATT	0.122***	0.114***	0.096***	0.085***	0.062***	0.056***
	(0.060, 0.184)	(0.051, 0.177)	(0.048, 0.144)	(0.036, 0.135)	(0.024, 0.101)	(0.017, 0.094)
Number of Observations	1625	1652	1625	1652	1625	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.

Table 10: 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	0.073*	0.362***	0.056**	0.200***	0.038*	0.099***
	(-0.005, 0.151)	(0.137, 0.586)	(0.001, 0.112)	(0.088, 0.312)	(-0.004, 0.081)	(0.045, 0.154)
Number of Observations	718	770	718	770	718	770
			Panel B: 45 days around treatment			
ATT	0.065**	0.056**	0.053**	0.054***	0.034*	0.044***
	(0.008, 0.122)	(0.002, 0.109)	(0.010, 0.095)	(0.020, 0.088)	(-0.001, 0.069)	(0.015, 0.074)
Number of Observations	1155	1217	1155	1217	1155	1217
			Panel C: 60 days around treatment			
]hline ATT	0.086**	0.081**	0.066***	0.060**	0.041**	0.037**
	(0.019, 0.153)	(0.015, 0.148)	(0.016, 0.116)	(0.011, 0.109)	(0.003, 0.080)	(0.000, 0.074)
Number of Observations	1625	1652	1625	1652	1625	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.

Table 11: Estimates for Areas with Rent Above the Median

	(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	0.087**	0.184***	0.067*	0.147***	0.042*	0.090***
	(0.004, 0.171)	(0.170, 0.198)	(-0.002, 0.136)	(0.134, 0.160)	(-0.001, 0.084)	(0.082, 0.099)
Number of Obs.	42	109	42	109	42	109
			Panel B: 45 days around treatment			
ATT	0.110***	0.185***	0.070***	0.148***	0.030*	0.090***
	(0.030, 0.190)	(0.171, 0.198)	(0.018, 0.121)	(0.135, 0.160)	(-0.002, 0.061)	(0.082, 0.099)
Number of Obs.	48	315	48	315	48	315
			Panel C: 60 days around treatment			
ATT	0.088*	0.137***	0.054*	0.109***	0.025	0.063**
	(-0.002, 0.177)	(0.044, 0.231)	(-0.005, 0.112)	(0.037, 0.182)	(-0.015, 0.064)	(0.015, 0.111)
Number of Obs.	136	420	136	420	136	420

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: Estimates for Areas with Rent Below the Median

	(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	0.012	-0.026	0.006	-0.038	0.001	-0.044
	(-0.051, 0.074)	(-0.155, 0.103)	(-0.042, 0.053)	(-0.141, 0.064)	(-0.039, 0.040)	(-0.116, 0.028)
Number of Observations	402	540	402	540	402	540
	Panel B: 45 days around treatment					
ATT	0.082***	0.085***	0.074***	0.100***	0.052***	0.087***
	(0.034, 0.130)	(0.032, 0.139)	(0.037, 0.110)	(0.058, 0.142)	(0.022, 0.082)	(0.038, 0.136)
Number of Observations	402	540	402	540	402	540
	Panel C: 60 days around treatment					
ATT	0.125***	-0.007	0.105***	-0.006	0.072***	-0.020
	(0.047, 0.204)	(-0.065, 0.050)	(0.044, 0.167)	(-0.059, 0.046)	(0.023, 0.122)	(-0.065, 0.025)
Number of Observations	1046	1182	1046	1182	1046	1182

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.

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

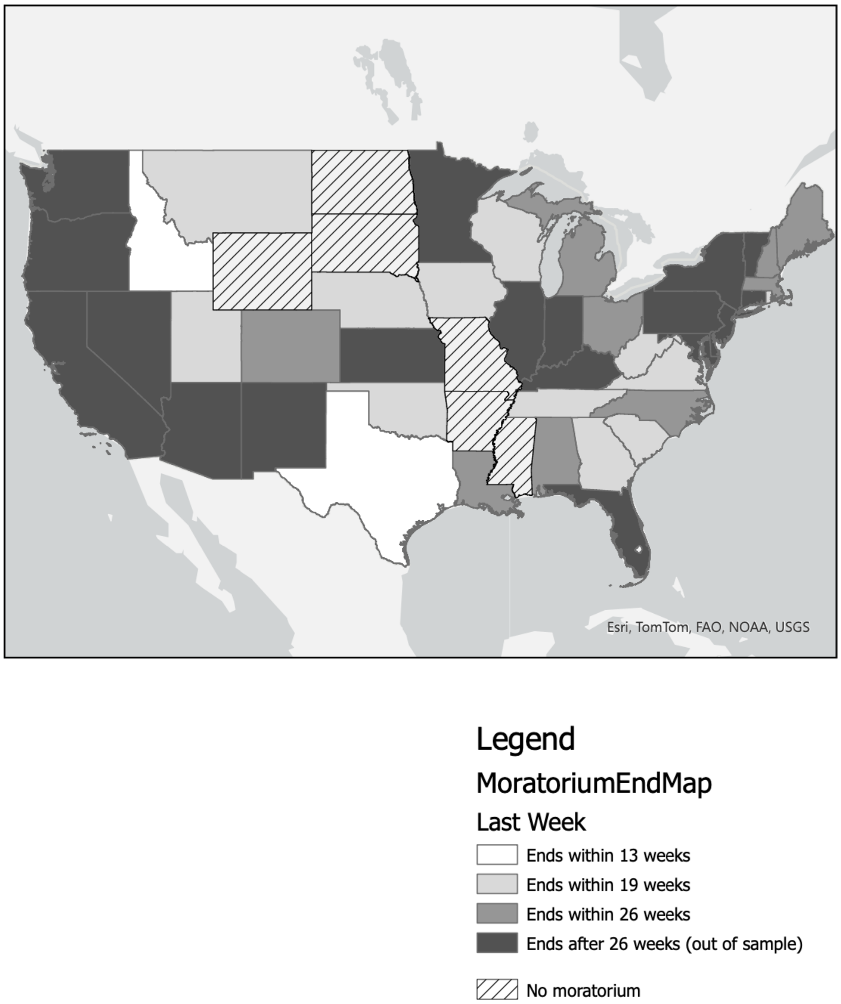
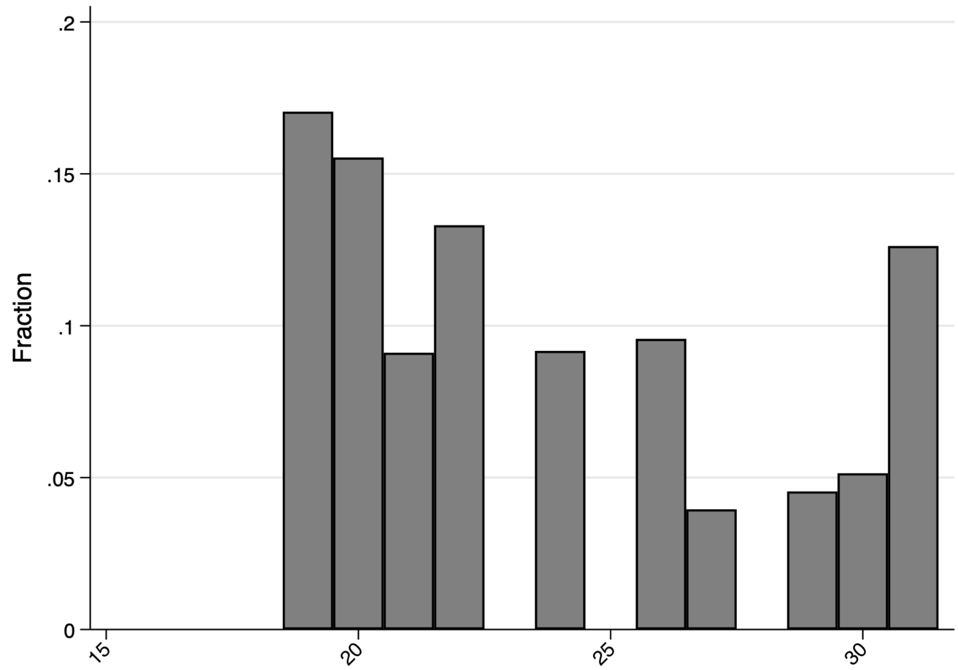
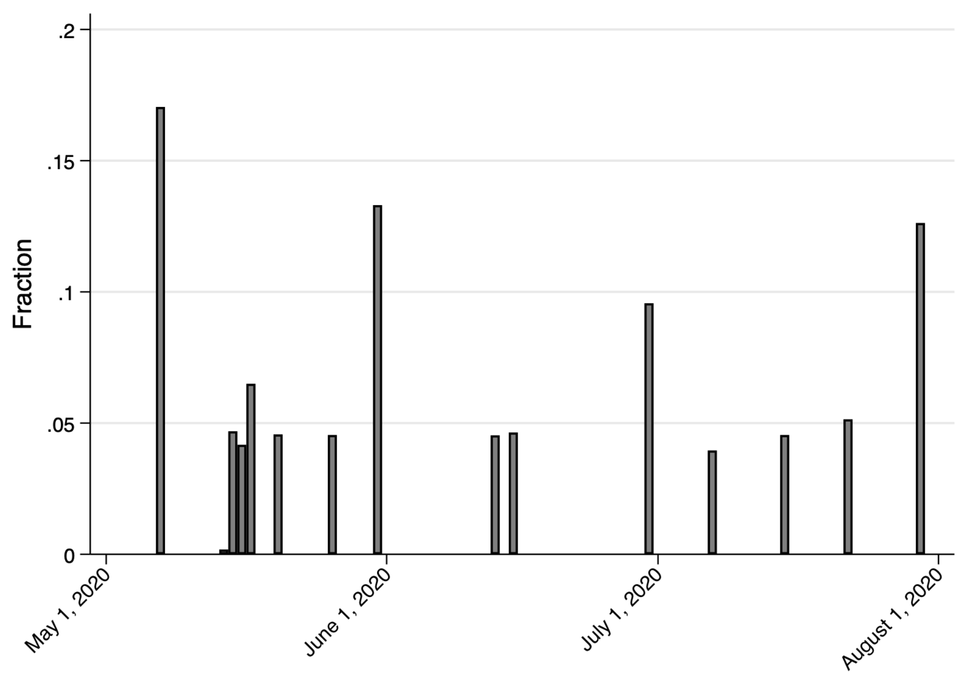


Figure 2: The Distribution of the Moratorium Expiration Dates



(a) Weeks



(b) Dates

Figure 3: Event Study Coefficients for the Relative Response Ratios for an African American Identity Relative to a White Identity with the Smoothing Parameter $h = 10$ and $\hat{\tau} = 45$ Days around Treatment

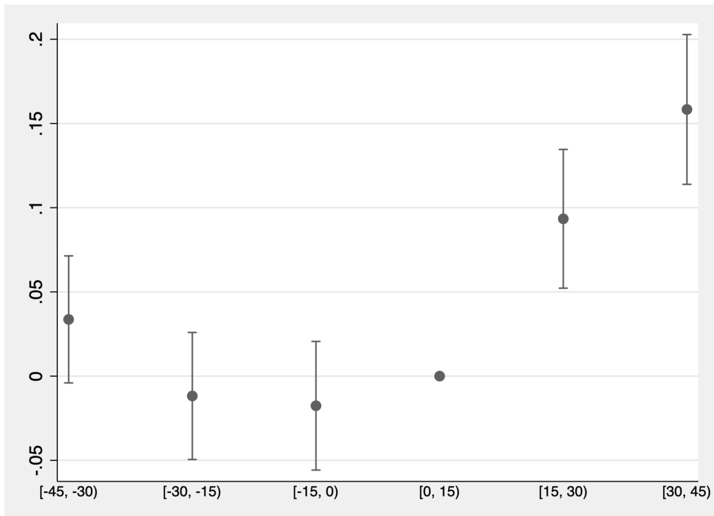
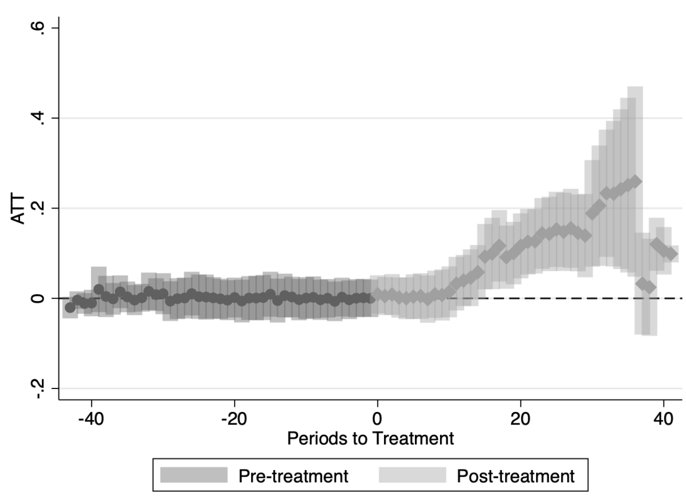


Figure 4: Event Study Coefficients from ?'s Estimator for the Relative Response Ratios for an African American Identity Relative to a White Identity with the Smoothing Parameter $h = 10$ and $\hat{\tau} = 45$ Days around Treatment



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 landlord'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

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

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

$$\frac{(1-\pi_M)}{1-\beta\pi_M} - \frac{(1-\pi_W)}{1-\beta\pi_W} = \frac{1-\beta\pi_W - \pi_M + \pi_M\beta\pi_W - 1 + \beta\pi_M + \pi_W - \pi_W\beta\pi_M}{(1-\beta\pi_M)(1-\beta\pi_W)}$$
$$= \frac{-\beta\pi_W - \pi_M + \beta\pi_M + \pi_W}{(1-\beta\pi_M)(1-\beta\pi_W)} = \frac{(1-\beta)(\pi_W - \pi_M)}{(1-\beta\pi_M)(1-\beta\pi_W)}.$$

To access how the option value to lease changes, use

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

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,

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

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 Tables and Figures

Figure 5: Eviction Moratoria across the U.S.

(a) States that Enacted Moratoria

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

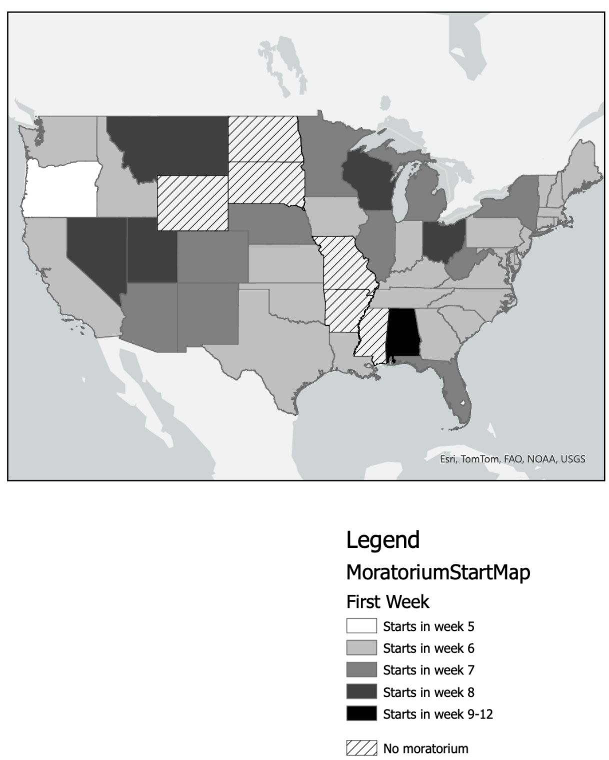
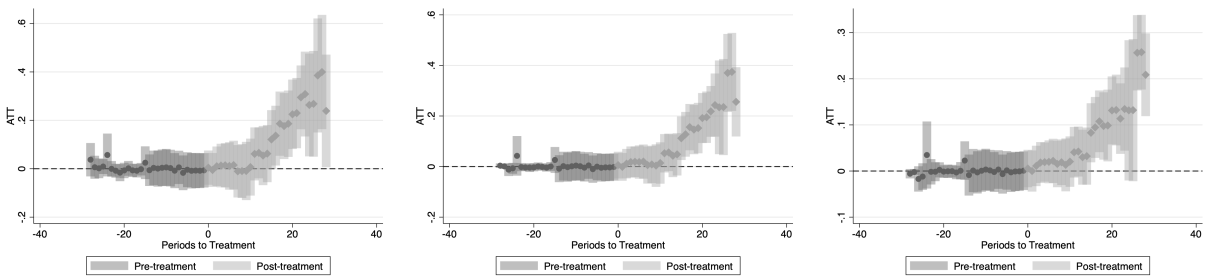
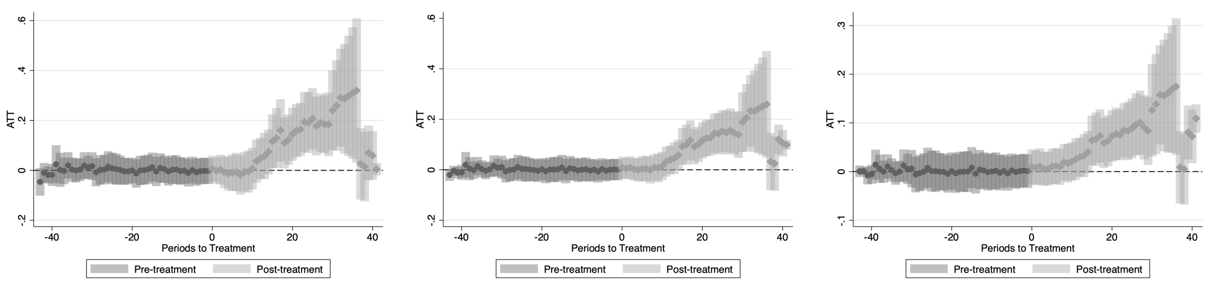


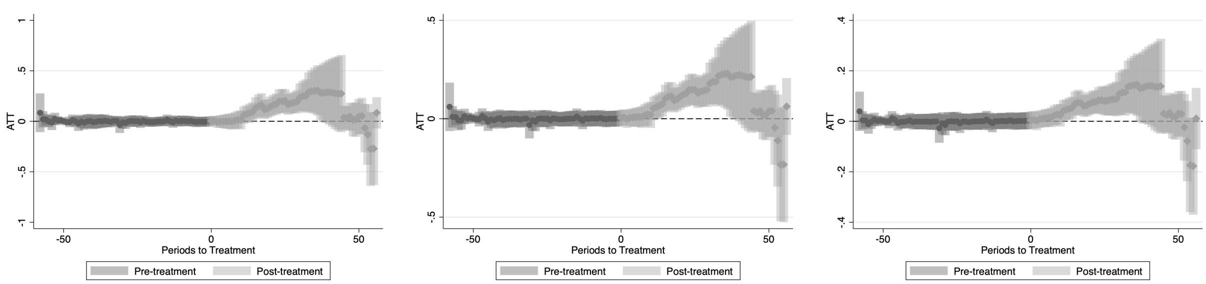
Figure 6: The Event Study Estimates from the Staggered DiD for Different Smoothing Parameters h and $\hat{\tau}$ Days Around Treatment



(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 $\hat{\tau} = 60$