Discrimination During Eviction Moratoria*

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Abstract

We provide evidence of intensified discriminatory behavior by landlords in the rental housing market during the eviction moratoria instituted during the COVID-19 pandemic. Using data collected from an experiment that involved more than 25,000 inquiries of landlords in the 50 largest cities in the United States in the spring and summer of 2020, our analysis shows that the implementation of an eviction moratorium significantly disadvantaged African Americans in the housing search process. A housing search model explains this result, showing that discrimination is worsened when landlords cannot evict tenants for the duration of the eviction moratorium.

Keywords: Eviction, Discrimination, COVID-19

JEL Codes: J15, R31, R38

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1 Introduction

The topic of housing precarity was brought to the forefront by the COVID-19 pandemic. COVID-19 was both a health and an economic crisis. Economic shutdown resulted in many households facing job-loss over a short period of time, which increased the risk of eviction for renters and foreclosure for owners. Absence of stable housing made it difficult to follow stay-at-home orders along with a multitude of other recommended measures. In response, government policies were implemented as public health measures at the local, state, and federal levels to preserve housing stability. Included amongst these were rental assistance and eviction moratorium policies, the latter of which shut down the eviction process in the affected jurisdictions. Specifically, these moratoria prohibited landlords from evicting a tenant for the period when the policy was in place. If a tenant defaulted on rent, however, that rent would still be owed when the moratorium expired.

The main intent of eviction moratoria was to allow tenants to practice social distancing and comply with stay-at-home orders. A line of research has explored the impact these policies had on the spread of COVID-19 (Benfer et al., 2021a; Hepburn et al., 2021, 2023; Leifheit et al., 2021; Nande et al., 2021; Hatamyar and Parmeter, 2023). However, an additional consequence was to increase the tenant's expected tenure. The subsequent increase in the duration of the tenant's lease could intensify discriminatory practices in the process of applying for a lease as we illustrate with a simple model. If this is indeed the case, it may prove to be an unintended consequence of the moratorium.

Recent studies have shown that racial minorities face substantial discrimination in a wide range of market activities, from applying for a job to buying a car or renting an apartment. In the case of the housing market, racial discrimination can take place at various stages of the process, including home search (Christensen and Timmins, 2022; Ewens et al., 2014; Hanson and Hawley, 2011), negotiations over prices or rent (Bayer et al., 2017), home appraisal (Ambrose et al., 2021a), and mortgage lending (Aaronson et al., 2017; Zhao et al., 2006; Ambrose et al., 2021b; Frame et al., 2022). 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.

The distortions induced by discrimination at the search stage can be large and lead to significant equity and welfare concerns. Experimental work using actors pretending to be prospective home buyers or renters has sought to measure these costs. According to the comparative work done across the Department of Housing and Urban Development's Housing Discrimination Studies (HDS), the most persistent form of discrimination in the housing market has been discriminatory steering of minority households into minority neighborhoods at the search stage (Dymski, 2006; Galster and Godfrey, 2005; Yinger, 1995). Christensen and Timmins (2022) find significant differences in the characteristics of neighborhoods (e.g., pollution, crime, poverty, and skill-level of local residents) shown by realtors to white, African American, Hispanic, and Asian testers in the 2012 HDS study. Using a correspondence study (relying on online interactions using racialized names) of rental markets in five major markets, Christensen and Timmins (2023) find that discrimination imposes average welfare costs equivalent to between 3.5% and 4.4% of annual income for renters of color and search behavior results in greater welfare costs for African Americans as their incomes rise.

In this paper, we examine how discrimination in the rental housing search process interacted with policies intended to help renters secure more stable housing during the early stages of the pandemic. The moratoria placed on evictions during the spring and summer of 2020 provide variation in the constraints imposed on landlords with respect to how they might expect to deal with a tenant in default. We demonstrate how a moratorium can worsen or ease discrimination using a model of the forward-looking landlord's decision process. Because the effect could theoretically go either way, we test the predictions of the model using the outcomes of a large-scale correspondence study of the rental market conducted during the pandemic. Results accounting for the staggered repeal of moratoria across states show evidence that African Americans, in particular, faced significantly higher rates of discrimination when moratoria were in effect. A policy intended to help housing-insecure households may, therefore, have had the unintended consequence of making it harder for certain sub-groups to find housing during a critical juncture.

Our analysis speaks to a number of literatures in addition to those described above. During normal times, eviction has itself been a crisis confronting America's rental housing markets.[1] A large literature has explored who is most at risk of eviction (Desmond and Gershenson, 2017; Rutan and Desmond, 2021; Desmond, 2012) and what are the impacts on evicted tenants (Collinson et al., 2022; Humphries et al., 2019; Goplerud et al., 2021; Bullinger and Fong, 2021; Desmond and Kimbro, 2015; Kim et al., 2021; Schwartz et al., 2022; Himmelstein and Desmond, 2021; Hoke and Boen, 2021; Groves et al., 2021; Hatch and Yun, 2021). Another line of research has analyzed eviction policies including "Right-to-Counsel" (Abramson, 2021), rules governing the filing of eviction lawsuits (Gromis et al., 2022), and rent support and eviction moratoria (Corbae et al., 2023). Other work has examined the role of tenant screening in who can access housing (So, 2022; Rosen et al., 2021). While our paper studies the role of eviction policy on discrimination in the housing search process, there is research that studies the direct role of racial and ethnic discrimination in eviction decisions (Greenberg et al., 2016). There is also research on how policies intended to ensure decent and affordable housing may have the unintentional consequence of reducing housing access (Greif, 2018).

The remainder of the paper proceeds as follows. Section 2 describes a model that illustrates our empirical finding of an increase in discrimination during eviction moratoria. Section 3.1 describes a correspondence study conducted by Christensen et al. (2021), which provides the experimental evidence used to test the predictions of our model. Section 4 provides results of a simple baseline discrimination specification, confirming that results provided by Christensen et al. (2021), and Section 5 uses these data to carry out a difference-in-difference analysis that tests our model predictions. In Section 6, we show that our results with respect

[1] See https://www.economist.com/united-states/2021/05/13/in-america-a-million-evictions-take-place-in-a-normal-year. For comprehensive time-series data on eviction filings and threatened evictions at the county level, see https://evictionlab.org.

to African Americans are robust to using the staggered repeal of moratorium policies across states. Section 7 considers robustness of our results and treatment heterogeneity by gender, and Section 8 concludes.

2 Model

We illustrate that discrimination could increase or decrease with the implementation of an eviction moratorium with a simple search model. 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 and defaults with probability $1 - \pi$, where $F_i(\pi)$ is the distribution function of the probability of rent payment as perceived by a landlord which could differ by the type of applicant. We interpret a first-order stochastic dominance of the perceived distributions of the probability to pay $F_M(\pi) > F_W(\pi)$ as statistical discrimination. The probability of the rent payment π is realized when the landlord calls and interviews the renter. If the renter defaults, her landlord recovers a rent payment net of the collection costs that equals $L < R$. The landlord and the renter do not discount future payoffs, and the renter stays in the unit for the next period with the probability s.

The landlord's per-period payoff includes the expected rent $\pi_i R+(1-\pi_i)L$ net of a utility loss from leasing to an applicant of type i, κ_i, that satisfies $L < \kappa_i < R$. Whenever $\kappa_M > \kappa_W$, we interpret this as taste-based discrimination. Before leasing, the landlord chooses which type of applicant to call. Each call 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 can be positive or negative. Denote the cumulative distribution function and probability density function of ψ as $F_\psi(.)$ and $f_\psi(.)$, respectively. Assume that $f_\psi(.) > 0$ on its support.

The landlord decides which type of applicant to call. Once the landlord calls an applicant of type i, the perceived probability to repay rent $\pi \sim F_i(\pi)$ is realized. Based on the realization of π, the landlord decides whether or not to offer a lease to this applicant. If she offers a lease, the applicant accepts. If the landlord does not offer a lease, she starts the search over by choosing which applicant to call next. The optimal decision will be characterized by a threshold for the probability to repay p_i, such that the landlord makes a lease offer if $\pi > p_i$, and does not otherwise.

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

$$u_i = \mathbb{E}[\pi R + (1-\pi)L - \kappa_i + s u_i + (1-s)V | \pi \geq p_i],$$
$$u_i = V + \frac{1}{1-s}\left(L - \kappa_i + (R-L)\frac{\int_{p_i}^{1} \pi dF_i(\pi)}{1-F_i(p_i)}\right),$$

The value of calling an applicant of type i for the landlord is then

$$U_i = \max_{p_i}[\text{Prob}(\pi < p_i)V + \text{Prob}(\pi \geq p_i)u_i].$$

The first-order condition for maximizing this expected utility U_i over p_i is

$$-f_i(p_i)(L-\kappa_i) + (R-L)(-p_i f_i(p_i)) = 0.$$

with the solution $p_i^* = (\kappa_i - L)/(R-L)$. Because we assumed $L < \kappa_i < R$, this threshold is between zero and one: $p_i^* \in (0,1)$.

Let $p_i = p_i^*$, then the value of calling an applicant of type i is

$$U_i = V + \frac{1}{1-s}\{R \underbrace{\int_{p_i}^{1} \pi dF_i(\pi)}_{\text{Prob. of payment}} + L \underbrace{\int_{p_i}^{1} (1-\pi) dF_i(\pi)}_{\text{Prob. of default}} - \underbrace{(1-F_i(p_i))}_{\text{Prob. to lease}} \kappa_i\}, \tag{1}$$

and the value of an empty rental unit is $V = \mathbb{E}\max\{U_M - \psi, U_W\}$.

To maximize the value of a vacant unit V, the landlord calls a minority applicant if $U_M - \psi > U_W$, and a white applicant otherwise. This optimal choice results in the probability of calling a minority applicant of $P_M^{\text{Call}} = \text{Prob}(U_M - \psi > U_W) = F_\psi(U_M - U_W)$, where the difference in the values of calling a minority and white applicant on the right-hand side is

$$
\begin{aligned}
& U_M - U_W = \frac{1}{1-s}\{R(\int_{p_M}^1 \pi dF_M(\pi) - \int_{p_W}^1 \pi dF_W(\pi))+ \\
& + L(\int_{p_M}^1 (1-\pi)dF_M(\pi) - \int_{p_W}^1 (1-\pi)dF_W(\pi)) \\
& + (1 - F_W(p_W))\kappa_W) - (1 - F_M(p_M))\kappa_M\}.
\end{aligned}
$$

Comparative Statics: Eviction Moratoria

There are multiple ways to interpret the effect of the eviction moratorium on the problem of the landlord. We consider two of them to show that the eviction moratorium can either increase or decrease discrimination depending on the interpretation.

One way of interpreting the effect of the eviction moratorium in the model is a lower payoff for the landlord in case of the tenant's default, L. The eviction moratorium allowed renters to stay in rental units for the duration of the eviction moratorium even if they did not pay the rent. The accumulated rent together with any late fees was due at the end of the eviction moratorium. Because the rent and late fees are deferred further into the future during the eviction moratorium, we can interpret this as a decrease in L.

The second way of interpreting the eviction moratorium is viewing it as the increase in the expected tenure of the tenant, determined by the probability of staying in the unit, s. During the moratorium, this probability is elevated because the landlord cannot evict the tenant.

Our correspondence study measures discrimination as a differential response in the response rate of the landlord. Hence, we are interested in the change in the probability of the landlord responding to or calling a minority applicant. We show that this probability can increase or decrease during the moratorium depending on what the most salient features of the moratorium are for the landlord.

Eviction moratorium as a decrease in the landlord's payoff in case of the renter's default: $L \downarrow$**.** When the landlord's payoff L drops, she raises the optimal threshold on the probability of rent payment $p_i = (\kappa_i - L)/(R - L) = 1 + (\kappa_i - R)/(R - L)$: $\partial p_i/\partial L = (\kappa_i - R)/(R - L)^2 < 0$, where $\kappa_i - R < 0$ so that p_i increases when L drops.

To study the change in the observed call back/response rate, we need to know how this affects the difference in the payoff from leasing to a minority applicant relative to a white applicant:

$$\frac{dP_M^{\text{Call}}}{dL} = \frac{dF_\psi(U_M - U_W)}{dL} = \underbrace{f_\psi(U_M - U_W)}_{>0} \frac{d(U_M - U_W)}{dL}.$$

Because the landlord readjusts p_i to ensure $\partial U_i/\partial p_i = 0$, the envelope theorem implies

$$\frac{d(U_M - U_W)}{dL} = \frac{\partial(U_M - U_W)}{\partial L} = \frac{1}{1-s}[\int_{p_M}^{1}(1-\pi)dF_M(\pi) - \int_{p_W}^{1}(1-\pi)dF_W(\pi)].$$

The probability of calling a minority applicant rises if the right-hand side of the expression above is negative, which happens when the minority's conditional probability of default is less than the white's conditional probability of default, $\int_{p_M}^{1}(1-\pi)dF_M(\pi) < \int_{p_W}^{1}(1-\pi)dF_W(\pi)$.

In the case of purely taste-based discrimination, the perceived distributions of the probability to pay rent π are the same, $F_M(\pi) = F_W(\pi) = F(\pi)$, but disutility from a minority applicant is higher than from a white applicant, $\kappa_M > \kappa_W$. Because the landlord's utility loss from a minority tenant is higher than from a white, $\kappa_M > \kappa_W$, she requires a higher level of credibility p from the tenant: $p_M > p_W$. Then the probability of calling is negatively related to the landlord's payment in case of the tenant's default L:

$$\frac{dP_M^{\text{Call}}}{dL} = f_\psi(U_M - U_W)\frac{1}{1-s}[\int_{p_M}^1 (1-\pi)dF(\pi) - \int_{p_W}^1 (1-\pi)dF(\pi)] < 0.$$

In the case of purely taste-based discrimination with $\kappa_M = \kappa_W$ but $F_M(\pi) > F_W(\pi)$, the sign of the expression above could be negative or positive:

$$\begin{aligned}\frac{d(U_M - U_W)}{dL} &= \frac{1}{1-s}[\int_p^1 (1-\pi)dF_M(\pi) - \int_p^1 (1-\pi)dF_W(\pi)] = \\ &= \int_p^1 (F_M(\pi) - F_W(\pi))d\pi - (1-p)(F_M(p) - F_W(p)).\end{aligned}$$

If we assume that $F_M(\pi) - F_W(\pi)$ is decreasing for $\pi \geq p$, $\int_p^1 (F_M(\pi) - F_W(\pi))d\pi < (1-p)(F_M(p) - F_W(p))$ because we have taken the largest value that the integrand takes and multiplied it by the length of the interval that we integrate over. If this assumption holds, then $dP_M^{\text{Call}}/dL < 0$ as in the case of purely statistical discrimination. If $\int_{p_M}^1 (1-\pi)dF(\pi) - \int_{p_W}^1 (1-\pi)dF(\pi) > 0$ under alternative assumptions, we can get an opposite result.

If $dP_M^{\text{Call}}/dL < 0$ and the payoff of the landlord in case of tenant's default, L, is lower during the moratorium, the probability of calling a minority applicant P_M^{Call} increases. Hence, we would observe less discrimination during the moratorium, and the end of the moratorium would be associated with an increase in discrimination.

Intuitively, if the landlord sees the eviction moratorium as a reduction of her payoff in case of the tenant's default and uses a perceived distribution of the probability to pay rent to decide which applicant to call, then it is possible that the landlord would discriminate less during the eviction moratorium. This happens if a discriminating landlord believes that her selection process leads to a reduction in the minority's conditional probability of default relative to that of a white applicant: $\int_{p_M}^1 (1-\pi)dF_M(\pi) < \int_{p_W}^1 (1-\pi)dF_W(\pi)$. In this case, when the payoff in the case of a default decreases L, the landlord prefers to call minority applicants to hedge against default. However, the opposite results is possible as well.

Eviction moratorium as a higher probability to stay in the unit: $s \uparrow$. Another interpretation of the eviction moratorium is an increase in the duration of the renter's stay in the unit, s. To consider the effect of this change, use $\int_{p_i}^{1} \pi dF_i(\pi) = \pi F_i(\pi)|_{p_i}^{1} - \int_{p_i}^{1} F_i(\pi)d\pi = 1 - p_i F_i(p_i) - \int_{p_i}^{1} F_i(\pi)d\pi$ to rewrite (1) as

$$U_i = V + \frac{R-L}{(1-s)}\{1 - p_i - \int_{p_i}^{1} F_i(\pi)d\pi\}.$$

Because the optimal threshold on the probability to repay the rent $p_i = (\kappa_i - L)/(R-L)$ does not depend on the probability of the renter staying for another period s, we have

$$\frac{dU_i}{ds} = \frac{R-L}{(1-s)^2}\{1 - p_i - \int_{p_i}^{1} F_i(\pi)d\pi\}.$$

Thus, the change in the difference of the payoffs from the unit leased to a minority and a white applicant is

$$\frac{d(U_M - U_W)}{ds} = \frac{R-L}{(1-s)^2}\{-(p_M - p_W) - (\int_{p_M}^{1} F_M(\pi)d\pi - \int_{p_W}^{1} F_W(\pi)d\pi)\}.$$

In a case of pure taste-based discrimination with $F_M(\pi) = F_W(\pi) = F(\pi)$ and $\kappa_M > \kappa_W$, minority applicants are screened more severely than white applicants, $p_M > p_W$. Then we can use $-(p_M - p_W) = -\int_{p_W}^{p_M} d\pi$ and $-(\int_{p_M}^{1} F(\pi)d\pi - \int_{p_W}^{1} F(\pi)d\pi) = \int_{p_W}^{p_M} F(\pi)d\pi$ to show that minority applicants get fewer calls from landlords during the moratoria, or, put differently, the end of the moratorium reduces discrimination:

$$\frac{dP_M^{\text{Call}}}{dL} = -f_\psi(U_M - U_W)\frac{(R-L)}{(1-s)^2}\int_{p_W}^{p_M}(1 - F(\pi))d\pi < 0.$$

The conclusion is the same in the case of a pure statistical discrimination with $F_M(\pi) >$

$F_W(\pi)$ and $\kappa_M = \kappa_W$:

$$\frac{dP_M^{\text{Call}}}{dL} = -f_\psi(U_M - U_W)\frac{R-L}{(1-s)^2}(\int_p^1 F_M(\pi)d\pi - \int_p^1 F_W(\pi)d\pi) < 0.$$

To sum up, termination of the moratorium leads to less discrimination or more discrimination depending on the effect of the moratorium on the interaction of the renter and the landlord. Thus, whether or not eviction moratoria intensifies discrimination is an empirical question that we address using the corresponding study. Our empirical estimates suggest that the end of the moratorium reduces discrimination.

3 Data

3.1 Correspondence Study

We test the predictions of this model using data collected as part of a correspondence study undertaken by Christensen et al. (2021) 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 inquiries were 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 an online rental housing platform.[2]

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 then initiated by the bot, using identities drawn randomly from a set of 18 first/last name pairs summarized in Table 1.[3] Property managers never received inquiries from two different identities on the same day. Recognizing that names can

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

[3]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.

encompass other unobservable traits like income (Guryan and Charles, 2013; Fryer Jr and Levitt, 2004), the bot refined its sampling of first names by incorporating maternal educational attainment and gender. Property manager responses were categorized as such 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, revealing patterns of discrimination encountered in the initial stage of a search.

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 (Emily A. Benfer and Desmond, 2023), 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.

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 (Emily A. Benfer and Desmond, 2023), 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 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 the prior section involves a sequence of binomial 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:

$$Response_{ij} = \delta_i + \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + X_j^{'}\theta + \epsilon_{ij}, \tag{2}$$

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. Christensen et al. (2021) demonstrate that estimates are consistent when including/omitting control variables and when using a conditional logit vs. a linear probability model. Columns (1)-(4) of Table 2 show the estimates from this linear probability model using all weeks and states. 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.

5 Difference-in-Differences

5.1 Defining Treatment

Most moratoria that were put into place 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.[4] 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

[4]Eviction moratoria expirations have been used elsewhere in the literature on policy impacts related to COVID-19. See Benfer et al. (2021a) as an example.

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. The earliest date when a state lifted the eviction moratorium is May 15, 2020, and the latest date when a state lifted the moratorium in our sample is July 15, 2020. Figure 2 shows when the moratoria were lifted in our sample.

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.

5.2 Potential Endogeneity of Treatment

The eviction moratoria started approximately at the same time. However, the terminations of the moratoria were more spread out over time and could had been endogenous. 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 for the number of COVID-19 cases. This supports that the number of COVID-19 infections was not an important determinant of the termination date of the moratorium, consistent with Benfer et al. (2021b). 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 access whether the states selected the last day of the eviction moratorium based on other state-level characteristics, we employ a two-stage procedure. We first use a state by day panel with the number of daily COVID-19 cases in a state and the last day of the eviction moratorium to remove the impact of the COVID-19 infections. Specifically, we regress the last day of the eviction of the moratorium on the number of daily COVID-19 infections 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 length of the moratorium.

Table 3 shows the results. All variables are insignificant at a 5 percent significance level. Hence, we proceed with using the number of daily COVID-19 cases in a state as our control.

5.3 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:

$$\begin{aligned} Response_{ijt} = \delta_i &+ \beta^{AA} African\ American_j + \beta^{H} Hispanic_j + \beta^{T} Treatment_{jt} \\ &+ \beta^{AAT} Treatment_{jt} \times African\ American_j \\ &+ \beta^{HT} Treatment_{jt} \times Hispanic_j + X_j^{'}\theta + \epsilon_{ijt}, \end{aligned} \tag{3}$$

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). δ_i is a rental property fixed effect. $Response_{ijt}$ take 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 number of evictions in a county in 2018, the index of the stringency of the eviction policies in a county, 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 the COVID-19 infections per 100,000 population in a state. Column (6) further clusters the

[5]We use the data on the number of evictions from Gromis et al. (2022), which is the latest available data prior to the pandemic.

errors by state. White identities in our sample received a response 57.36% of the time during moratoria. African American and Hispanic identities are less likely to receive a response compared to a white identity when a moratorium is in place. The coefficient on $African\ American$ implies that an African American identity with the same 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 0.037. This increases the post-moratoria relative response ratio for African American identities to 0.96. Hence, an eviction moratorium significantly disadvantages African American identities in the housing search process relative to their white counterparts. While the direction of the effect is similar for Hispanic identities, the result is not statistically significant.

6 Staggered Differences-in-Differences

We are primarily interested in how an eviction moratorium affects discrimination. 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. Implementing staggered DiD requires a panel of data describing discrimination across states and over time. We carry out the Callaway and Sant'Anna (2021) (CS) estimation using a two-stage procedure described below.

6.1 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 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:

$$Response_{ijkt} = \beta_{k\tau}^{AA} African\ American_j + \beta_{k\tau}^{H} Hispanic_j + X_j'\theta_{k\tau} + u_{ijkt},$$

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. $African\ American_j$ 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. $\beta_{k\tau}^{R}$ is the coefficient describing the effect of an $R =$ [African American, Hispanic] identity on the probability of a response on day τ. We weight each observation using $\omega_{ijkt}^{\tau} = 1/(h\sqrt{2\pi})\exp(-((t-\tau)/h)^2/2)$. When estimating the extent of discrimination on day τ, observations on days t closer in time to τ receive more weight. The smoothing parameter h determines how much weight is given to inquiries made on nearby days.

6.2 Stage #2: Moratorium Effect

With the procedure described above, we recover $[\beta_{k\tau}^{R}]_{\tau=1,\ldots,T}$ for each state k, day τ, and race $R = [AA, H]$. 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 $R \in (AA,\ H,\ W)$. These values are denoted as $\rho_{k\tau}^{R} \equiv P(Response_{ijkt} = 1|R_j = 1)$. We use these values to calculate the relative response ratio for an individual of race $R \in (AA,\ H)$ relative to a white individual $R = W$ on day τ in state k, $\rho_{k\tau}^{R}/\rho_{k\tau}^{W}$. These estimated relative response ratios $\rho_{k\tau}^{R}/\rho_{k\tau}^{W}$ 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 where τ is less than the day on which the moratorium in state k begins and for which we have fewer than 100 observations 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 (i.e., moratorium ends) in state k, and $\hat{\tau} = 30$, 60, or 75 defines the window around treatment. Therefore, the second stage estimation procedure uses estimates of discrimination within the $\hat{\tau}$ window around the treatment date which is 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 the relative response ratio 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 that these estimates. The relative response ratios rise after the end of the moratoria. We confirm these findings by performing the staggered CSDiD estimation.

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 end its moratorium by that time. Hence, not-yet-treated states become the controls for the treated states. We then run a separate difference-in-differences regression for each day.

$$\frac{\rho^R_{k\tau}}{\rho^W_{k\tau}} = \alpha^g_0 + \alpha^g_1 TREAT_{k\tau} + \alpha^g_2 POST_{k\tau} + \alpha^g_3 TREAT_{k\tau} \times POST_{k\tau} + \nu_{k\tau}, \tag{4}$$

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. $TREAT_{k\tau}$ takes a value of one if state k was treated on day g, and $POST_{k\tau}$ takes a value of one if state k is post treatment on day τ. α_3^g describes the average treatment effect on the treated on day g. Our baseline specification does not incorporate any additional controls. 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, 60, and 75 days, and (3) with and without propensity score matching using the number of daily COVID-19 cases in a state. The estimates are positive, suggesting that the end of the moratorium increases the relative response ratio for African American identities. Therefore, racial discrimination intensified during the eviction moratorium.

7 Robustness and Treatment Heterogeneity

7.1 Bootstrap

Because the staggered DiD estimation from the previous section is a two-stage procedure, 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. Not knowing the 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 500 times, going back each time to a new bootstrap sample clustered on states. This creates a distribution of the estimates of the overall treatment effect. We use the 5th and 95th percentiles of this distribution as the 90% bootstrap confidence interval.

Table 6 shows the estimates for the response to an African American identity relative to a white identity. The estimates are positive and significant at the 10% significance level, suggesting that the end of the moratorium increases 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.

7.2 Heterogeneity by Gender

We showed that race discrimination declined after an eviction moratorium ended. We now explore the heterogeneity of this effect by gender. Table 8 in the Appendix and Figure 4 present the results from the DiD regression similar to specification (3), but with all interactions of the indicator variables for a Male identity, African American or Hispanic identity, and Treatment (a dummy variable for the end of the moratorium). The estimates show that racial discrimination against African Americans and Hispanics reduced significantly after the eviction moratorium was lifted specifically for males. Or, in other words, African American and Hispanic males were the targets of increased discrimination during the eviction moratoria.

8 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 (Benfer et al., 2021a), 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 Christensen et al. (2021) during the pandemic, we show that this detrimental impact is particularly important for

African American renters. While eviction moratoria may prove to be important policy tools in responses 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	-0.056***	-0.056***	-0.056***	-0.057***	-0.145***	-0.234***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.021)	(0.033)
Hispanic	-0.027***	-0.027***	-0.027***	-0.027***	-0.071***	-0.114***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.021)	(0.034)
Constant	0.601***	0.619***	0.631***	0.659***	0.405***	0.650***
	(0.006)	(0.007)	(0.008)	(0.009)	(0.024)	(0.039)
Observations	22,086	22,086	22,086	22,086	22,086	22,086
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

	Coefficient	95% Confidence Interval
First day of moratorium	0.08	(-4.88, 5.04)
Total population in 100k	0.87	(-0.46, 2.19)
Population density	-0.07	(-0.22, 0.09)
Log median income	482.03	(-996.03, 1960.09)
Percent of people who are over 65 years old	17.35	(-42.21, 76.91)
Percent of African Americans	-0.54	(-14.17, 13.09)
Percent of Asians	-27.91*	(-60.73, 4.90)
Percent of American Indian	10.03	(-38.30, 58.37)
Percent of Hispanics	-10.30	(-30.11, 9.50)
Percent of renters	-1.25	(-32.41, 29.91)
Percent of people without high school degrees and below	23.64	(-62.57, 109.85)
Percent of people with a college degree and above	879.70	(-3419.83, 5179.24)
Percent of people in group quarters	61.02	(-185.47, 307.50)
Percent of essential workers	-14.35	(-92.89, 64.19)
Percent of people who are uninsured	8.74	(-45.32, 62.79)
Percent of people who use public transportation	102.97	(-221.08, 427.03)
Percent of people who carpool	184.85	(-249.94, 619.64)
Percent of people who commute by driving alone	85.25	(-243.81, 414.30)
Percent of people who commute using motocycle	844.90	(-1431.58, 3121.38)
Percent of people who commute using motocycle	-53.61	(-517.43, 410.22)
Percent of people who commute by walking	132.68	(-314.41, 579.77)
Percent of people who work at home	45.28	(-308.43, 398.99)
Observations	39	

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	-0.095***	-0.109***	-0.055***	-0.017	-0.016	-0.016
	(0.018)	(0.020)	(0.020)	(0.089)	(0.089)	(0.102)
African American	-0.062***	-0.063***	-0.063***	-0.061***	-0.061***	-0.061***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.013)
African American x Treatment	0.038**	0.053***	0.053***	0.037**	0.037**	0.037*
	(0.018)	(0.020)	(0.020)	(0.018)	(0.018)	(0.021)
Hispanic	-0.033***	-0.031***	-0.030***	-0.033***	-0.033***	-0.033***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)
Hispanic x Treatment	0.027	0.034*	0.033*	0.026	0.026	0.026
	(0.018)	(0.020)	(0.019)	(0.018)	(0.018)	(0.020)
#Evictions in 2018, thousands		-0.001***	-0.001***			
		(0.000)	(0.000)			
Stringency Index	-0.000	-0.000				
	(0.000)	(0.000)				
COVID Cases per 100k					0.034	0.034
					(0.110)	(0.058)
Constant	0.672***	0.692***	0.825***	0.618***	0.596***	0.653***
	(0.032)	(0.033)	(0.051)	(0.021)	(0.074)	(0.046)
Observations	16,913	15,053	15,053	16,913	16,913	16,913
R-squared				0.026	0.026	0.026
Number of addresses	5,654	5,034	5,034	5,654	5,654	5,654
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 End of Moratorium on the Relative Response Ratio for African American Applicants Relative to White Applicants

	$h = 7$		$h = 10$		$h = 15$	
COVID-19 Cases per 100k	No	Yes	No	Yes	No	Yes
	Panel A: 30 days around treatment					
ATT	0.106**	0.602***	0.079**	0.530***	0.053**	0.432***
	(0.019, 0.193)	(0.198, 1.007)	(0.012, 0.145)	(0.240, 0.821)	(0.002, 0.104)	(0.212, 0.651)
Number of Obs.	500	619	500	619	500	604
	Panel B: 60 days around treatment					
ATT	0.050	0.149	0.048*	0.270***	0.039*	0.313***
	(-0.016, 0.117)	(-0.029, 0.328)	(-0.005, 0.101)	(0.116, 0.425)	(-0.006, 0.084)	(0.177, 0.450)
Number of Obs.	1193	1286	1193	1286	1179	1272
	Panel C: 75 days around treatment					
ATT	0.069*	0.307***	0.062**	0.350***	0.046*	0.341***
	(-0.005, 0.143)	(0.146, 0.468)	(0.003, 0.121)	(0.212, 0.488)	(-0.003, 0.095)	(0.220, 0.462)
Number of Obs.	1443	1534	1443	1534	1418	1513

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) 2) 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

	$h = 7$	$h = 10$	$h = 15$
	Panel A: 30 days around treatment		
ATT	0.103*	.053	.036
	(0.006, 0.232)	(-0.028, 0.151)	(-0.009, 0.100)
Number of Observations	553	619	619
	Panel B: 60 days around treatment		
ATT	0.081*	0.052*	0.041**
	(0.010, 0.177)	(0.001, 0.117)	(0.010, 0.078)
Number of Observations	553	1286	1286
	Panel C: 75 days around treatment		
ATT	0.099*	0.071*	0.052*
	(0.003, 0.234)	(0.003, 0.167)	(0.005, 0.123)
Number of Observations	553	1534	1534

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) 90% bootstrapped confidence intervals in parentheses, 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 7: Robustness of DiD Estimates to Controlling for COVID-19 Infections

	Dependent Variable: Response	
	(1)	(2)
Treatment	0.014	0.014
	(0.103)	(0.115)
African American	-0.061***	-0.061***
	(0.007)	(0.013)
African American x Treatment	-0.001	-0.001
	(0.024)	(0.022)
Hispanic	-0.033***	-0.033***
	(0.007)	(0.008)
Hispanic x Treatment	0.029	0.029
	(0.024)	(0.028)
COVID Cases per 100k	0.058	0.058
	(0.118)	(0.064)
COVID Cases per 100k x Treatment	-0.094	-0.094
	(0.227)	(0.110)
African American x COVID Cases per 100k x Treatment	0.048**	0.048***
	(0.020)	(0.008)
Hispanic x COVID Cases per 100k x Treatment	-0.004	-0.004
	(0.020)	(0.014)
Male	-0.039***	-0.039***
	(0.007)	(0.009)
Constant	0.588***	0.614***
	(0.074)	(0.043)
Observations	16,913	16,913
R-squared	0.026	0.026
Number of addresses	5,654	5,654
Weekly FEs	Yes	Yes
Property FEs	Yes	Yes
Educational Level	Yes	Yes
Inquiry Order	Yes	Yes
Clustered at State-level	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.

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

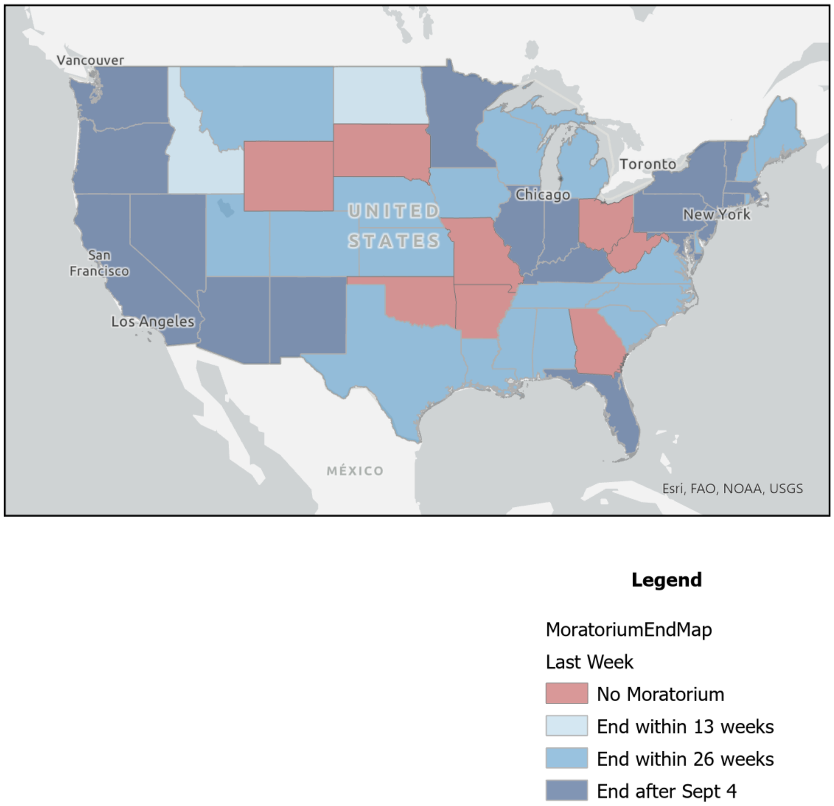
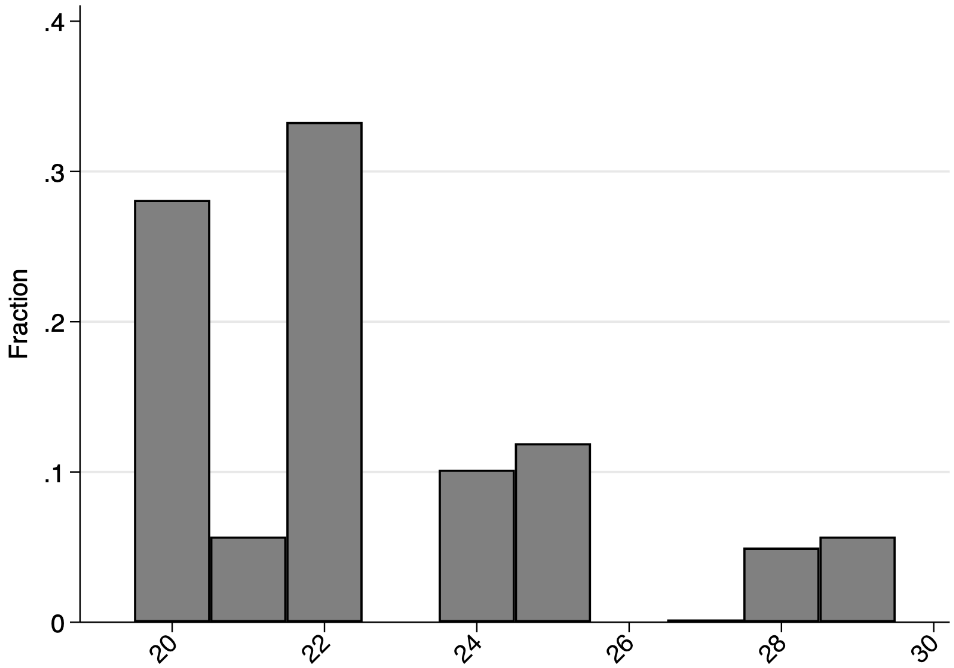
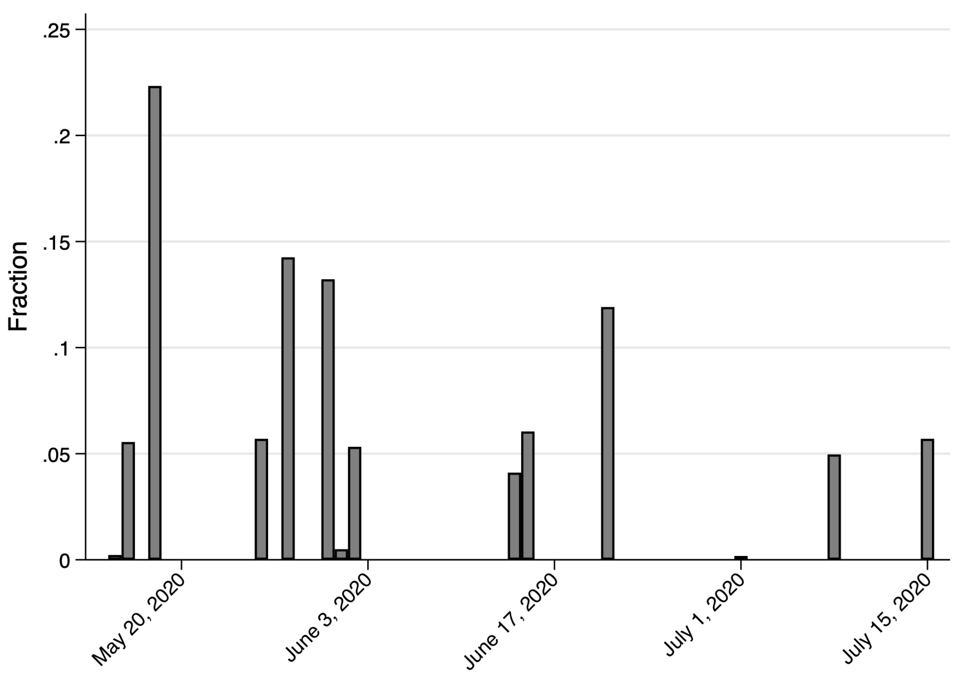


Figure 2: The Distribution of the Moratorium Expiration Dates



(a) Dates



(b) Weeks

Figure 3: Event Study Coefficients for the Relative Response Ratios for an African American identity relative to a white identity with the smoothing parameter $h = 7$ and $\hat{\tau} = 60$ days around treatment

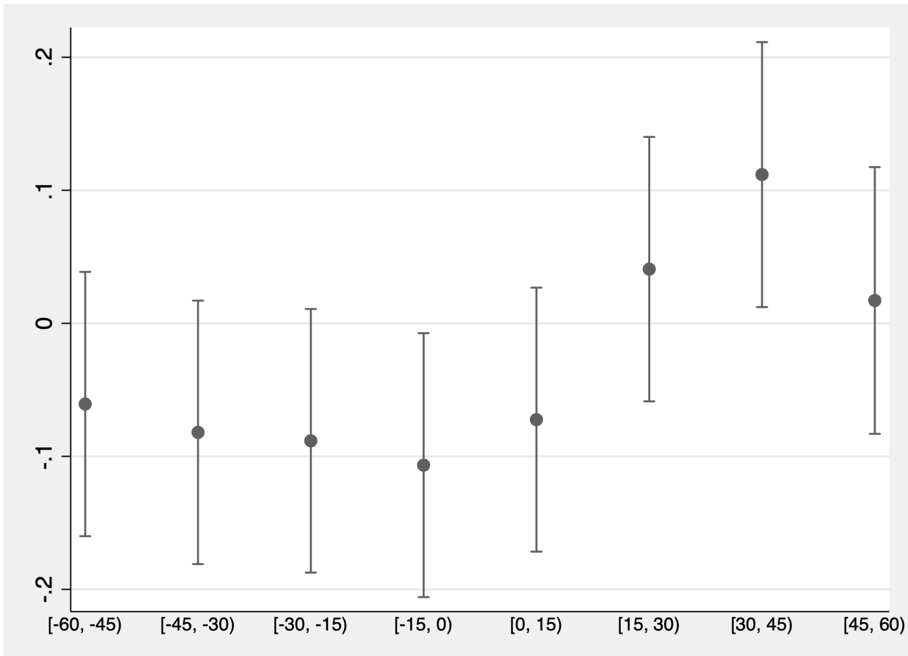
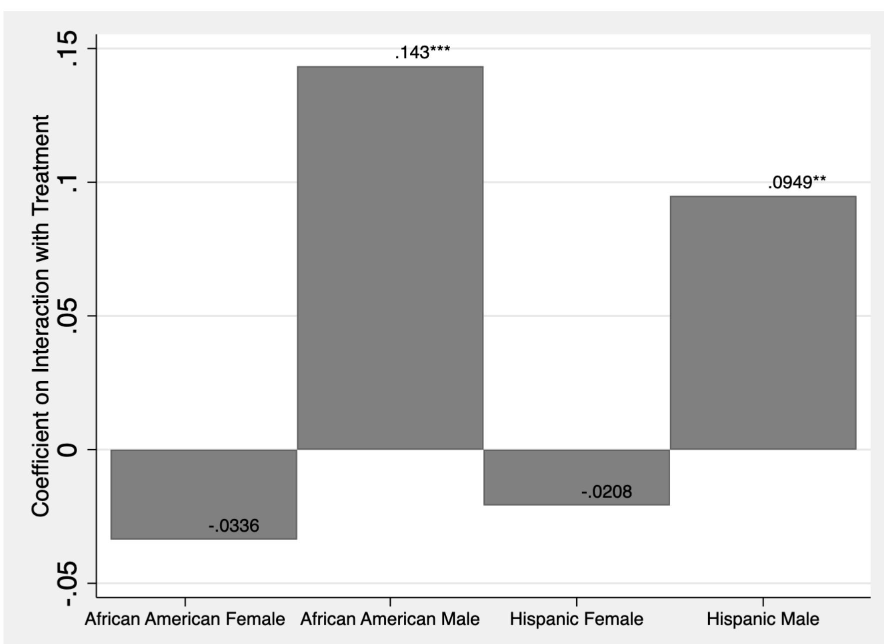


Figure 4: DiD Estimates on the Interaction of Treatment with Race and Gender Dummies



A Appendix

Tables and Figures

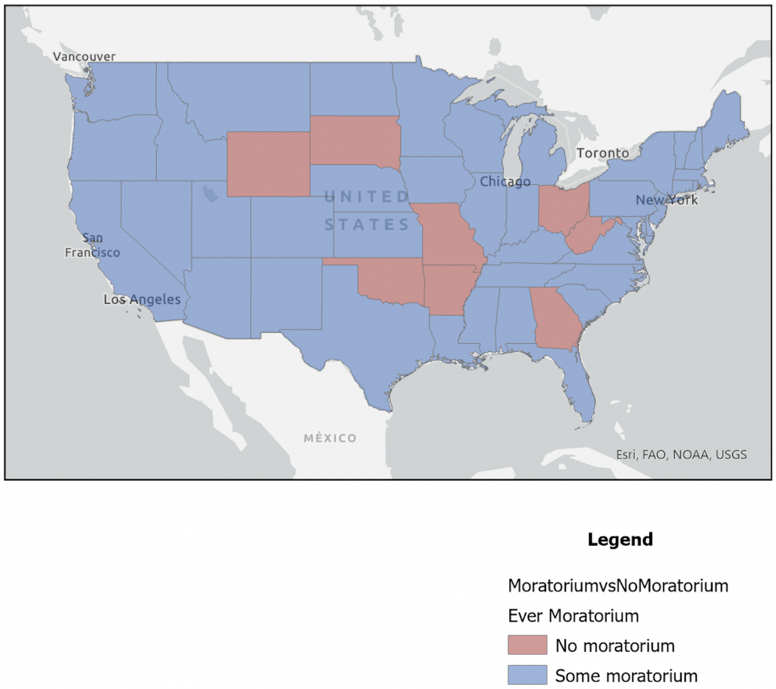
Table 8: Estimates by Gender

	Dependent Variable: Response				
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.070***	-0.071***	-0.076***	-0.018	0.006
	(0.023)	(0.023)	(0.025)	(0.026)	(0.090)
African American	-0.039***	-0.039***	-0.040***	-0.040***	-0.035***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)
African American x Treatment	-0.021	-0.021	-0.007	-0.008	-0.034
	(0.027)	(0.027)	(0.030)	(0.030)	(0.028)
Male	-0.040***	-0.040***	-0.043***	-0.044***	-0.040***
	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)
Male x Treatment	-0.050*	-0.050*	-0.069**	-0.069**	-0.056*
	(0.029)	(0.029)	(0.032)	(0.032)	(0.031)
African American X Male	-0.046***	-0.046***	-0.048***	-0.047***	-0.054***
	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)
African American x Male x Treatment	0.121***	0.121***	0.124***	0.125***	0.143***
	(0.042)	(0.042)	(0.045)	(0.045)	(0.044)
Hispanic	-0.054***	-0.054***	-0.055***	-0.056***	-0.055***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)
Hispanic x Treatment	-0.013	-0.013	-0.012	-0.013	-0.021
	(0.027)	(0.027)	(0.029)	(0.029)	(0.028)
Hispanic X Male	0.042**	0.042**	0.050***	0.051***	0.045**
	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)
Hispanic x Male x Treatment	0.082**	0.082**	0.093**	0.095**	0.095**
	(0.041)	(0.041)	(0.044)	(0.044)	(0.043)
#Evictions in 2018, thousands			-0.001***	-0.001***	
			(0.000)	(0.000)	
Stringency Index		-0.000	-0.000	0.001	
		(0.000)	(0.000)	(0.001)	
Constant	0.660***	0.673***	0.692***	0.814***	0.620***
	(0.011)	(0.032)	(0.034)	(0.052)	(0.022)
Observations	16,913	16,913	15,053	15,053	16,913
R-squared					0.030
Number of addresses	5,654	5,654	5,034	5,034	5,654
Weekly FEs	No	No	No	Yes	Yes
Property FEs	No	No	No	No	Yes
Educational Level	Yes	Yes	Yes	Yes	Yes
Inquiry Order	Yes	Yes	Yes	Yes	Yes

Notes: 1) Standard errors in parentheses. 2) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Figure 5: Eviction Moratoria across the U.S.

(a) States that Enacted Moratoria



(b) The First Week of the Eviction Moratoria Across U.S.