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Marina Mileo Gorzig
Mathematica

Deborah Rho
University of St. Thomas

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The Impact of Renter Protection Policies on Rental Housing Discrimination

Marina Mileo Gorzig* Deborah Rho†

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Abstract

Policies that reduce information on applicants have mixed results in the labor market. However, little is known about their impact in the housing market. We submitted fictitious email inquiries to publicly advertised rentals using names manipulated on perceived race and ethnicity before and after a policy that restricted the use of background checks, eviction history, income minimums, and credit history in rental housing applications in Minneapolis. After the policy was implemented, discrimination against African American and Somali American men increased. Triple difference analysis shows that discrimination increased in Minneapolis relative to St. Paul after the policy.

Keywords: Discrimination | Race/ethnicity | Immigration | Housing
JEL codes: J68, J15, R31

* Mathematica. Email: MGorzig@mathematica-mpr.com ORCID: 0000-0002-6294-3659
† University of St. Thomas. Department of Economics. Saint Paul, MN. Email: deborah.rho@stthomas.edu ORCID:0000-0001-9928-5066

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People of color, immigrants, and individuals with a criminal record face well-documented and significant discrimination in housing (Hanson and Santas 2014; Turner et al. 2013; Bosch et al. 2015; Hogan and Berry 2011; Bosch et. al. 2010; Roman and Travis 2004; Baldini and Federici 2011; Dion 2002). Discrimination in housing has adverse effects on wellbeing, including limited employment opportunities (Phillips 2018; Boeri et al. 2015), poor health outcomes (LaVeist 2003), racial and ethnic segregation (Bosch et al. 2015), and increased recidivism (Fontaine and Biess 2012). In an effort to increase access to housing for lower income workers, people of color, and formerly incarcerated people, numerous cities in the United States have passed, or are considering passing, renter protection policies that restrict the use of background checks, eviction history, strict income minimums, and credit history in the rental housing application process (Racial Equity Alliance 2017).

Previous research has found that similar policies have mixed results in the labor market. For example, restricting the use of pre-employment credit checks appears to reduce a Black job seeker's match with a job (Bartik and Nelson 2019). Likewise, policies that limit the use of background checks early in the employment application process have sometimes been found to increase discrimination against young Black and Latino men (Doleac and Hansen 2020; Agan and Starr 2017). Policies that *increase* information about job seekers can improve outcomes; for example, legislation that allows drug testing increases Black employment (Wozniak 2015).

However, other studies find that policies that restrict the use of criminal history in the job application process reduce discrimination against formerly incarcerated people (Agan and Starr 2018) particularly in the public sector (Craigie 2019; Shoag and Veuger 2021). Similarly, expungement of criminal records also improves employment outcomes, especially for Black applicants (Prescott and Starr 2020).

There is little to no research on how these types of policies impact the housing market, despite their increasing popularity in housing policy. In this paper we examine the impact of a set of policies that reduce information about rental applicants. We measure the change in rental housing discrimination against African American and Somali American applicants after Minneapolis implemented a new set of policies that limit the use of background checks, eviction history, and credit score. Minneapolis has a long history of public policies that intentionally created segregated neighborhoods including racially restrictive covenants; we additionally examine if the new policy had a different impact in Census tracts with more residents who are Black, foreign born, or Somali American.

To examine these questions, we implemented a correspondence study where we submitted fictitious email inquiries to publicly advertised rental units using names that are manipulated on perceived race and ethnicity (Somali American, African American, and white American). We began data collection prior to the implementation of the policy and continued after the policy took effect. We first examine the change in discrimination in

Minneapolis (where the policy was implemented) and then compare the changes in Minneapolis to changes in neighboring St. Paul and surrounding suburbs (where the new policy was not implemented). This approach identifies the impact of the new policy on rental housing discrimination.

Because we submit inquiries randomly, this approach identifies the change in the average level of discrimination in the Minneapolis housing market. However, this is not the experience of the average rental housing seeker, because housing applicants do not apply randomly. For example, white applicants may apply more often in neighborhoods with more white residents. To examine if the experience of housing seekers varies by neighborhood, we examine whether the impact of the policy varied by Census-tract level demographics. We test whether the impact of the new policy is different in neighborhoods with more Black residents, Somali American residents, and foreign-born residents.

We find evidence that the new policy worsened discrimination against Somali American and African American rental applicants. After implementing a policy that restricts the use of background checks, credit history, and eviction history, the difference in positive responses to African American and Somali American applicants compared to white American applicants in Minneapolis increased by over 10 percentage points for both groups. This increase was largest for units that were 2 or more bedrooms and among emails sent from male names. We find the largest increase in discrimination occurred in Census tracts with more Black residents. We then use St. Paul and nearby

suburbs as a counterfactual to account for secular changes in the housing market and implement a triple-difference analysis. We find that the policy in Minneapolis was associated with a large increase in discrimination against Somali American and African American rental applicants relative to St. Paul and the suburbs.

Because we are comparing changes in Minneapolis relative to St. Paul, events that occurred in Minneapolis at the same time as the implementation of the policy could be driving our results. There are two potential threats to identification: the protests following George Floyd's murder by the Minneapolis Police Department and the COVID-19 pandemic. We find similar results when we leave out south Minneapolis, where most property damage was located, suggesting that the protests were not driving our results. COVID-19 had similar impacts in both cities so the triple difference analysis accounts for any broad changes like COVID-19; additionally, we test if changes in data collection due to COVID-19 could have impacted our results and find no evidence of this.

Why applicant protection policies may inadvertently impact discrimination

Upstream biases in education, the labor market, and the criminal justice system mean that Black and Latino people have lower average income (Chetty et al. 2019), lower average credit scores (Leonhardt 2021), higher

incarceration rates (Raphael 2014), and higher rates of eviction (Hepburn et al. 2020) than other groups. It is intuitive that a policy that removes information on income, credit score, and criminal history in employment or housing applications would benefit Black and Latino applicants. When Minneapolis passed a broad set of renter protections, City Council Member Jeremiah Ellison stated, "The intention of the [renter protection] ordinance is to reduce unnecessary financial and screening barriers that block people who are ready to enter the rental housing market from doing so" (Desmond 2019).

However, previous research has found that policies that reduce information on applicants have mixed results in the labor market. At the center of the controversy are policies that restrict employers from asking about criminal history on initial job applications. Agan and Starr (2018) raised concerns about potential unintended consequences of these polices. They submitted fictitious resumes to employers before and after the implementation of a policy limiting background checks in initial employment applications. Prior to the policy, there was a 7% gap between employers contacting resumes that appeared to be from young white men and those that appeared to be from young African American men; this increased to 43% after ban the box was implemented. Similarly, Doleac and Hansen (2020) found that ban-the-box policies increased discrimination based on race and ethnicity.

This finding is not universal – for example Burton and Wasser (2022) note that the Doleac and Hansen (2020) findings are sensitive to the model specification and Rose (2021) finds negligible impacts of restricting the use of

criminal history in the early stages of the hiring process on the labor market outcomes of ex-offenders. Craigie (2019) and Shoag and Veuger (2021) found that ban-the-box policies benefited formerly incarcerated people seeking jobs in the public sector and found little evidence of increased statistical discrimination. Likewise, expungement of criminal records also improves employment outcomes for those with criminal records, especially for Black people (Prescott and Starr 2020).

While a great deal of recent research has focused on policies limiting the use of criminal history, policies intended to protect applicants in the labor market are much broader. For example, restricting the use of pre-employment credit checks reduces a Black job seeker's match with a job (Bartik and Nelson 2019), and legislation that allows drug testing increases Black employment (Wozniak 2015). The observed increase in discrimination in the labor market after the implementation of policies that reduce potential negative information about applicants suggests that when an applicant's individual information is not available, employers may make decisions based on the perceived group average – increasing discrimination based on race and ethnicity.

Much of the research on the unintended consequences of policies that attempt to protect marginalized applicants has focused on the labor market. However, similar policies are becoming increasingly popular in the housing market. It remains unclear if the findings from the labor market extend to the rental housing market. In some ways the two markets function similarly – in

both contexts, decision makers have incomplete information and are selecting among multiple applicants. However, there are also important differences. Employers may be seeking the applicant who will be the most productive, whereas landlords may be searching for someone who is above a certain bar (e.g., will pay rent and will not harm the property). Bartos et al. (2016) shows that employers spend less time evaluating applications from marginalized groups whereas landlords spend more time on these applications. It is not clear if a policy that attempts to protect applicants by removing information will function the same in the housing market as it does in the labor market. In this paper, we directly address this gap by evaluating the impact of a set of renter protections on discrimination in the rental housing market.

Heterogeneity by neighborhood demographics

Discrimination in rental housing varies by neighborhood demographics, regional characteristics, housing unit characteristics, and landlord characteristics (Christensen and Timmins 2021; Carlsson and Eriksson 2014; Ewens et al. 2014; Hanson and Hawley 2011). Christensen and Timmins (2021) implemented a large audit study across the U.S. and found that property managers discriminated more against Black and Latinx rental applicants in neighborhoods with higher school quality, lower air pollution, higher percent white, and more rental demand. Likewise, landlords in

neighborhoods near racial "tipping points" show more discrimination (Hanson and Hawley 2011).

The impact of applicant *information* also varies by neighborhood demographics. Ewens et al. (2014) shows that positive signals from Black applicants close the racial gap in positive contacts among landlords in neighborhoods with a higher proportion of Black residents. This is consistent with their model of landlord statistical discrimination – signals are weighted more heavily when landlords are more familiar with a group of applicants. Likewise, Bartoš et al. (2016) notes that signals from culturally similar groups will convey more precise information on the individual applicant – the signal will have a higher variance for applicants from a cultural group the decision maker is more familiar with (Cornell and Welch 1996). In contrast, Agan and Starr (2020) find that employers in neighborhoods with fewer Black residents appear more likely to stereotype Black applicants as having a criminal record when they do have information on specific applicants.

If the impacts of the policy are different in neighborhoods with more Black and Somali American residents, the overall impact of the policy will not be equal to the impact of the policy on the average housing seeker. African American and Somali American apartment seekers likely apply more often to apartments in these neighborhoods. If the impact is larger in these neighborhoods, the average estimate of a change in discrimination from applying randomly (as in a correspondence study) will understate the change experienced by the average African American or Somali American apartment

seeker. To examine this, we test whether the impact of the new policy is different in neighborhoods with a higher proportion of Black residents, Somali American residents, and foreign-born residents.

Population and setting

Minneapolis (population 425,403) and St. Paul (population 307,695) are consistently ranked among the cities with the highest racial disparities in income, education, and home ownership (Beaumont 2020; Furst and Webster 2019; Buchta 2017). Like many Northern cities, Minneapolis and St. Paul have a long history of using racial covenants to restrict the sale of homes to people of color – a pattern termed the "Jim Crow of the North" (PBS 2019). While these racial covenants were banned by the Minnesota Legislature in 1953 and by the Federal Fair Housing Act of 1968, their presence is palpable in the low homeownership rate among Black residents and highly segregated neighborhoods (Mapping Prejudice 2020; Sood et al. 2019; Minnesota State Demographic Center 2018).

FIGURE 1 HERE

Minneapolis and St. Paul are home to the largest Somali American diaspora in the United States, allowing us to examine if rental housing discrimination and policies to reduce discrimination function differently for Black refugees and native-born African Americans. Beginning in the early 1990s, the U.S. began receiving refugees from the civil war in Somalia.

Minnesota, and particularly the Twin Cities area, served as a major destination for refugees. Using IPUMS ACS data (Ruggles et al. 2015), we estimate that in 2015, over 35% of all people in America who identified as Somali lived in Minnesota. In 2015, approximately 24,256 Somali Americans lived in Minneapolis and St. Paul, comprising an estimated 3.4% of the Twin Cities population (Gorzig and Rho 2022). Somali Americans comprise a large and important ethnic group within Minnesota, particularly the metropolitan area. Somali Americans have established neighborhoods south of downtown; the Riverside Plaza is a well-known apartment complex housing recent immigrants and is known as "Little Somalia." However, overall foreign-born Black Americans are less segregated in Minneapolis than U.S.-born Black Americans (Crowell and Fossett 2020).

Minneapolis Renter Protection Policy

In September 2019, the Minneapolis City Council passed a law restricting the use of background checks, eviction histories, and credit scores in rental housing applications; the new policy went into effect in June 2020 for landlords with more than 15 units and in December 2020 for landlords with fewer units (Evans 2019). The law bars landlords from considering misdemeanors that are older than three years and felonies older than seven years. For certain felonies, landlords cannot consider convictions that are older than 10 years.

Additionally, landlords cannot legally consider evictions older than three or more years from the date of application. While landlords are permitted to consider information on a credit report that is relevant to an applicant's ability to pay, the use of credit score to screen applicants is prohibited. Landlords are also not legally permitted to strictly require monthly income three or more times the rent and security deposits are capped at one month's rent.

Some aspects of the policy are harder to enforce than others. For example, landlords are still able to do background checks; they are not legally permitted to consider most older convictions. Likewise, the landlord is allowed to view an applicant's credit report but is not legally permitted to screen on a specific credit score. However, neither the applicant nor city enforcement can easily observe what the landlord is considering after completing the background or credit check. These aspects of the policy make screening more difficult for the landlord – they are no longer legally able to state in an ad that they are using a credit score cutoff or will not rent to anyone with a conviction, which is a low-cost way to screen applicants. While the policy is not able to prevent screening on credit score or conviction history, it makes it more costly for the landlord to do so. In contrast, requiring a certain income or security deposit can easily be viewed by tenants and therefore violations are more easily reported.

Data

To evaluate the impact of this set of policies, we sent email inquiries from fictitious applicants to real landlords in Minneapolis, St. Paul, and contiguous suburbs who posted vacancies for their rental units online. We manipulate the name in the email and email address to indicate the potential tenant's gender and race/ethnicity. We began data collection in January 2020 – six months prior to the implementation of the new policy – and collected data until June 2021. We track which applicants the landlords respond to positively (e.g., offers to show the property). This style of study is known as a "correspondence study" or an "audit study" and is a commonly used method to test discrimination in the labor market (Bertrand and Mullainathan 2004) and the housing market (Andersson et al. 2012; Ahmed et al. 2010; Friedman et al.2013). We pre-registered this experiment with the AEA RCT Registry (#5240).

We manipulate the name in the emails and email addresses to indicate the potential tenant's gender and whether the applicant is Somali American, African American, or white American. The names used in the experiment are the ones we used in Gorzig and Rho (2022), in which we conducted a labor market experiment in Minnesota. The Somali American names were selected from the CDC's list of popular Somali first names. The Somali American

names we use are Aasha Waabberi, Fathia Hassan, Khalid Bahdoon, and Abdullah Abukar.[1]

The African American and white American names are racially distinct and were pre-tested to select names that clearly signal race and do not signal different socioeconomic status (Levitt and Dubner 2005; Gaddis 2015). In Gorzig and Rho (2022), we evaluated potential names using Amazon Mechanical Turk, an online labor market where people perform piece-rate tasks such as surveys. Participants were shown a selection of names in random order and were asked to rate how strongly they associated the name with five major racial groups and whether they associated the name with high or low socioeconomic status. Respondents associated names with race and socioeconomic status. The African American names that were higher SES were perceived as having lower SES than lower SES white American names. To reduce the role that perceived differences in SES plays, we used high SES African American names and low SES white American names. For the surnames, we used the highest percent white and the highest percent African American of the top 100 most common surnames on the 2000 Census. The African American names used are Imani Williams, Nia Jackson, Andre Robinson, and Jalen Harris. The white American names we used are Amber Sullivan, Amy Wood, Jacob Myers, and Lucas Peterson.

[1] These first names are from the Koran and not specific to Somali Americans. However, in the Twin Cities context, Somali Americans are the largest, most visible Muslim group.

Each inquiry to landlords was from an email address which included the applicant's first and last names.[2] The inquiries included a greeting, a statement about seeing the rental listing, a line expressing interest in the unit, and a closing. We use a Python program designed for correspondence studies that creates email texts with these elements randomized (Lahey and Beasley 2009). None of the components are repeated within a listing.[3]

To send an inquiry, the research assistants (RAs) identified housing ads on Craigslist for studio, one-bedroom, and two-bedroom units. For each ad they found, the RAs sent the randomized emails to the landlord from fictitious applicants. In the initial study design, three emails were sent to each landlord with a time lag between each email. However, in late March 2020, Minnesota declared a state of emergency and implemented a "stay-at-home" order due to the COVID-19 pandemic (Walz 2020). Because of the stay-at-home order, landlords became suspicious when receiving three email inquiries[4]. The stay-at-home order stayed in place until late May 2020. In July 2020, Minnesota implemented a statewide mask mandate and many activities

[2] See Appendix 1 for a full list of email addresses.

[3] See Appendix 1 for more details on the email inquiries.

[4] For example, one response stated they had not gotten any inquiries for ten days and then got three in one day (the three inquiries sent by us) and was concerned it was a scam. Because the landlord was not receiving any other inquiries, the time lag was insufficient to separate the applicants from each other sufficiently to avoid suspicion. Another response asked if our applicants were related to each other. These suspicious responses only occurred after COVID began and did not occur again after we reduced the number of inquiries sent. Importantly, the landlords appeared suspicious of all three inquiries; we observed no difference by race/ethnicity in suspicious responses.

returned to more normal levels. From April 2020 to August 2020, we sent only one inquiry per ad to reduce detection risk (Balfe et al. 2022). In August 2020, we increased to sending two inquiries per ad. Inquiries were not sent to the same landlord a second time until at least six months passed; RAs used the company name, phone number, and the formatting of the ad to avoid repeated inquiries to the same landlord within the six-month period.

When applying to rental units with two or more bedrooms, the applicant mentioned that they are looking for a new home for their family while the reference to family was not included when applying to a one-bedroom unit.[5] The RAs tracked the "callbacks" - identifying which landlord sent a positive email for a fictitious applicant. When a landlord responded positively, the RA responded saying that the fictitious tenant was no longer interested in the unit. The most common positive response was an invitation to set up a time to tour the unit. A request for more information from the landlord was not considered a positive response.

In order to link each rental unit to neighborhood characteristics, we used the Census Geocoder to identify the Census tract of the rental unit. We use demographic data from the 2016-2020 American Community Survey 5-year data at the Census tract level. This data includes percent of the Census tract that is Black or African American, percent Somali American, and percent foreign born.

[5] See Appendix 1 for more details on the email inquiries.

The policy went into effect on June 1, 2020 for landlords with 15 or more rental units and went into effect later for those with fewer rental units.[6] To examine the impact of the policy, we restrict the analysis to ads that are likely to have 15 or more units.[7] We examined all ads that list a company name and over 95% of those we could identify have 15 or more rental units. Thus, we use the presence of a company name in the ad to infer that the landlord is likely to have 15 or more properties (hereafter referred to as "large companies")[8]. Some landlords are not identifiable in the ads that are posted. We do not know when the policy went into effect for these units and therefore are not able to include them in the analysis of the impact of the policy. We sent 1,808 emails in total to landlords with 15 or more units, 1,075 before the policy and 733 after the policy. About 54% (978) of the inquiries were sent to Minneapolis listings and about 46% (830) were sent to listings in St. Paul and the suburbs.

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[6] We use the date that the email inquiry was sent to classify the observation as before or after the policy. Inquiries were sent throughout the implementation of the policy.

[7] We are unable to analyze the impact of the policy on landlords with fewer than 15 units. Very few ads with the company name listed are from companies with fewer than 15 units. Ads without a company name listed are a mix of companies with more than 15 units and fewer than 15 units - we therefore don't know the relevant policy implementation date for these ads.

[8] As shown in Appendix 2, there is no evidence that there was any change in the tendency of landlords to include their name in the ad at the time of the policy implementation. Ads with a company name had similar average rent to those without a name, but were less often two or more bedrooms, less often in Minneapolis, and more likely to respond positively to the inquiry.

Descriptive statistics

Table 1 displays the descriptive statistics of the email inquiries sent to the large companies, the characteristics of the housing ads, and responses from landlords. As expected, the email inquiries are balanced with respect to race/ethnicity and gender – the program created emails where 1/3 have white American names, 1/3 Somali American names, and 1/3 African American names. Just under half the emails are from women's names and half from men's names. The order sent variable reflects the changes to the number of email inquiries sent to each landlord over the course of data collection; due to the stay-at-home order at the beginning of the pandemic, we sent three emails only for the initial portion of data collection. 56% percent of the emails were sent to ads for two or more bedrooms. The majority of emails were sent to ads for units in Minneapolis. The most common outcome is positive (40%) followed by a no response (36%). A negative response to an email inquiry was very rare (3%). The positive response rate of 40% is higher than previous correspondence studies in the labor market in the Twin Cities (Gorzig and Rho 2022).

Columns 2 and 3 of Table 1 show the results of a chi-squared test that each characteristic is distributed equally across the race/ethnicity manipulation. We fail to reject that the characteristics are distributed equally (at the 5% level) for all the email and ad characteristics. Table A3 in Appendix 2 shows the descriptive statistics for large companies before and

after the policy was implemented. Table A4 in Appendix 3 shows the same descriptive statistics for all emails and ads, not just those sent to large companies.

TABLE 1 HERE

Methods

Analysis: Did landlords change their behavior in response to the policy?

Prior to examining our main research question, we begin by investigating whether Minneapolis landlords adjusted their rental ads after the new law. It is relatively common for rental ads to use phrases like "clean background check" or "no felonies." The new policy restricts the use of criminal background checks to only recent convictions, so these phrases violate the new policy. We had two research assistants code the text in the rental ads in our data, both from before and after the policy, to indicate whether landlords included criteria that are prohibited by the new law. We examine five screening criteria banned by the new policy: requirement of income three times or higher than monthly rent, security deposit above one month rent, criminal background check that includes older convictions, specific credit score cutoff, and eviction history older than three years. The two RAs agreed on over 98% of ads coded for each of the five criteria. An ad is considered to include the criteria if one or both RAs indicated that it did.

If there is no change in the ad text, it is unlikely that the policy made an impact, and it is likely that something else is driving any resulting change in discrimination. On the other hand, a decrease in such language in the ads in Minneapolis after the policy would suggest that landlords are responding to the new law.

This analysis also serves to identify the mechanisms at work. The renter protection policy had multiple components, so examining the changes in various aspects of the ad texts gives insight into what parts of the policy had the most immediate impact on landlords' screening processes. Of course, this analysis will not capture all changes in landlord screening - we only observe the text in the ad, so any changes that occur later in the screening process will not be captured here.

Analysis: Estimating the impact of the new policy on discrimination

To examine our main research question, we evaluate whether housing discrimination changed after the implementation of the set of renter protections that restricted the use of background checks, eviction history, strict income minimums, and credit history in rental housing applications. We examine whether landlords' response rates to these applicants differ by perceived race of the potential tenants. While any individual applicant may be a better fit (email came first, landlord preferred the email text, etc.) than

another individual applicant, on average all applicants are constructed to be equivalent - any differences in average response rates across perceived race can be interpreted as discrimination. We test whether discrimination **changes** due to the implementation of the new policy. If discrimination in Minneapolis changes after the policy goes into effect, while there are no changes (or smaller changes) in St. Paul and the suburbs, the difference in the change in discrimination is the impact of the new policy. By tracking discrimination in areas unaffected by the policy, we can account for any broader changes in discrimination that are occurring separately from the new policy.

We examine the proportion of applicants who received positive contact before and after the policy went into effect. Then, we use the following regression framework to investigate whether landlords in Minneapolis changed the level of discrimination after the policy went into effect.

$$
\begin{aligned}
(1) \qquad Contact_{i,l} = \beta_0 + \beta_1 * African\ American_i + \ \beta_2 * Somali\ American_i \\
+ \beta_3 * After_i + \ \beta_4 * Somali\ American * After_l \\
+ \beta_5 * African\ American_i * After_l + \varepsilon_{i,l}
\end{aligned}
$$

The outcome variable ($Contact_{i,l}$) is an indicator variable indicating if the landlord (l) made positive contact with the fictitious applicant (i). β_1 and β_2 indicate the "baseline" level of discrimination in Minneapolis. If $\hat{\beta}_1$ and $\hat{\beta}_2$ are negative it indicates that African American and Somali American applicants are contacted less than white applicants in Minneapolis. The $After$

variable indicates if the email was sent after the new policy has been implemented. We are most interested in the coefficients β_4 and β_5; these coefficients indicate the change in discrimination after the new policy goes into effect. We cluster standard errors by ad; results are robust to other clustering choices and are available upon request.

Next, we examine if the policy's impact is different in areas with more Black and Somali American residents. We stratify Equation 1 by whether the neighborhood is above the mean for Black residents and Somali American residents. This analysis allows us to examine geographically heterogenous impacts of the policy. The impact of a policy reducing information on applicants may be different in areas where the information would have been more meaningful. For example, we could see a different impact ($\hat{\beta}_5$) for African American applicants in Census tracts with more residents who are Black, immigrant, or Somali American. For these analyses, we cluster standard errors by Census tract.

Finally, we use Equation 2 to compare the change in Minneapolis to the change in discrimination in St. Paul and contiguous suburbs to account for any broader changes in discrimination that occurred at this time. In Equation 2, the $Minneapolis$ variable indicates if the property is in Minneapolis.

$$
\begin{aligned}
(2) \qquad Contact_{i,l} = \beta_0 &+ \beta_1 * African\ American_i + \beta_2 * Somali\ American_i \\
&+ \beta_3 * Minneapolis_l + \beta_4 * After_l \\
&+ \beta_5 * Minneapolis_l * African\ American_i \\
&+ \beta_6 * Minneapolis_l * Somali\ American_i \\
&+ \beta_7 * Minneapolis_l * After_l + \beta_8 * African\ American_i \\
&* After_l + \beta_9 * Somali\ American_i * After_l \\
&+ \beta_{10} * Minneapolis_l * African\ American_i * After_l + \beta_{11} \\
&* Minneapolis_l * Somali\ American_i * After_i + \varepsilon_{i,l}
\end{aligned}
$$

We are most interested in the coefficients $\boldsymbol{\beta_{10}}$ and $\boldsymbol{\beta_{11}}$. These variables convey the difference in the change in discrimination after the new policy goes into effect between Minneapolis (where the policy would have clout) and St. Paul/suburbs (where it would not). If $\hat{\boldsymbol{\beta}}_{10}$ and $\hat{\boldsymbol{\beta}}_{11}$ are negative, it indicates that discrimination increased in Minneapolis after the policy passed more than in St. Paul or the suburban metropolitan area. If they are positive, it indicates that discrimination decreased.

It is possible that landlords begin changing their actions prior to the implementation of the policy in anticipation of it passing. Similarly, landlords in St. Paul or the suburbs may also comply with the policy if they own properties in multiple locations and use a common screening method. Both these situations would bias our results towards zero, since the change at the time of the policy, or the difference between the change in Minneapolis and the change in St. Paul and the suburbs, would be muted.

Results

Changes in ad text after policy implementation

The new policy banned five screening criteria commonly used by landlords: requirement of income three times or higher than monthly rent, security deposit above one month rent, criminal background check that includes older convictions[9], specific credit score cutoff, and eviction history older than three years. Table 2 shows the percentage of ads that included these criteria in Minneapolis before and after the policy went into effect.

As shown in Table 2, these criteria were relatively common in ads for two or more bedrooms prior to the policy and became less common after the policy went into effect. For example, 10.6% of ads for two or more bedrooms in Minneapolis included a phrase prohibiting applicants with any criminal record (including felonies more than 7 years old or misdemeanors more than 3 years old). After the policy went into effect, this dropped to 3.6% of Minneapolis ads. Among ads for two or more bedrooms in Minneapolis, all five prohibited criteria became much less common after the policy took effect ($p<.01$ for change in criminal history and credit score). Among one-bedroom units, the prohibited criteria were not often included prior to the policy and there was little change after the policy took effect, suggesting the policy was more binding for two or more bedroom units. Table A5 in Appendix 4 shows

[9] The law bars landlords from considering misdemeanors that are older than three years and felonies older than seven years. For certain felonies, landlords cannot consider convictions that are older than 10 years.

the change in ad text in St. Paul and the contiguous suburbs; there is some evidence of spillover among one-bedroom apartments, which may be more often owned by companies with properties in both Minneapolis and St. Paul. This spillover would bias the triple difference results towards zero.

Taken together, these results suggest that the new policy did impact the criteria that were included in ads for rental housing among ads for two or more bedroom units. Some of the most notable changes occurred to the proportion of ads that prohibited applicants with a criminal history and those that required a specific credit score.

TABLE 2 HERE

Impact of policy on discrimination

Figure 2 shows the positive responses to email inquiries before and after the policy in Minneapolis among large landlords. Pooling across unit size, the proportion of inquiries that received a positive response before the policy is very similar in Minneapolis and St. Paul/suburbs; the differences between the two cities prior to the policy are statistically insignificant for Somali Americans (p=.71), African Americans (p=.62), and white Americans (p=.84). Appendix 5 shows the pre-trends in Minneapolis and St. Paul by race and ethnicity; the two cities were trending closely together prior to the policy. After the policy passed, Somali Americans and African Americans experienced a large decrease in positive responses in Minneapolis both for

studios/one bedrooms and for units with two or more bedrooms. The decrease for white Americans in Minneapolis was smaller than for African Americans and Somali Americans in the studios/one bedrooms and were in fact contacted more often for units with two or more units. In St. Paul, positive responses for all three groups decreased, with white Americans experiencing the largest decrease for both studios/one bedrooms and units with two or more bedrooms.

FIGURE 2 HERE

Regression results: Impact of policy in Minneapolis

Table 3 displays the results of estimating Equation 1 for ads from Minneapolis. In Column 1, "After policy" shows the change after the policy was implemented for large companies (June 1, 2020). As shown in Column 1, landlords did not respond that differently to inquiries from Somali Americans and African Americans relative to white Americans prior to the policy. Once the policy was implemented, the proportion of white American names who received a response did not change, but this fell for both Somali American (-.121, not statistically significant) and African American (-.155, $p<.05$) inquiries. After the policy was implemented, the proportion of Somali American applicants with positive contact was -.032-.121 = -.153 relative to white Americans and the proportion of African American applicants who received positive contact was .0525-.155 = -.1025 relative to white American applicants.

Column 2 in Table 3 shows that the increase in the difference between Somali American and African American applicants relative to white American applicants is large and statistically significant among 2 or more bedroom units. There is no statically significant change for Somali American or African American applicants for the studios/one bedroom units (Column 3). Notably, the change in the ad text shown in Table 2 was also largest for the 2 or more bedroom units – discrimination increased the most among the same units whose screening methods were most affected by the new policy.

TABLE 3 HERE

Table 4 stratifies the ads with a company listed by perceived gender of the email sender. The increase in discrimination after the policy went into effect is large and statistically significant for emails perceived as coming from men, but not from women. Interestingly, African American men were more likely to be contacted than white American men or Somali American men prior to the policy.

TABLE 4 HERE

The stronger impacts for applicants with men's names is consistent with the previous research on the impact of "ban the box" policies in the labor market: the strongest impacts of these policies are typically found for Black and Latino men because of upstream biases in education, the labor market, and the criminal justice system resulting in higher incarceration rates for Black and Latino men (Raphael 2014).

Differential impacts by neighborhood

Table 5 stratifies Equation 1 by Census tract demographics. The average percent Black in our sample is 14.9%, average proportion Somali Americans is 2.3%, and average proportion foreign born is 14.6%. Columns 1-3 show the results Census tracts with lower than average proportion of Black residents, lower than average proportion of Somali Americans, and lower than average proportion of foreign-born residents. Notably, neither Somali American nor African American applicants experienced an increase in discrimination in these predominantly white Census tracts.

Columns 4-6 includes Census tracts with higher than average percentage of Black residents, Somali American residents, and foreign-born residents. The point estimate shows that African Americans were far more likely to be contacted than white American applicants prior to the policy in these Census tracts (statistically significant for Columns 4, 5, and 6) but then experienced a large and statistically significant increase in discrimination after the policy was implemented. While the increase was smaller, Somali Americans also experienced a statistically significant increase in discrimination in tracts with more Black, Somali, and foreign-born residents. Appendix 6 shows similar analysis based on the presence of racially restrictive covenants.

TABLE 5 HERE

The results in Table 5 have two important implications. First, the results shown in Table 3 will understate the impact of the policy on African American and Somali American apartment seekers. By design, we submitted our inquiries randomly with respect to neighborhood demographics. In reality, African American and Somali American apartment seekers are likely not submitting their inquiries randomly and may inquire more often for apartments in neighborhoods with a higher percentage of Black residents. Since the impact of the policy was larger for African American and Somali American applicants in these Census tracts, our overall average increase in discrimination will underestimate the increased discrimination facing the average African American and Somali American apartment seeker.

Second, these results are consistent with previous research in the role of information in housing discrimination. Ewens et al. (2014) argues that positive signals from Black applicants are more meaningful in neighborhoods with a higher population of Black residents and found that positive signals from Black applicants had a larger impact in neighborhoods with more Black residents. Notably, this finding is in contrast to that of Agan and Starr (2020) who find than ban-the-box policies in the labor market have a larger impact in neighborhoods with more white residents. We find that the impact of the policy restricting information on applicants is highest for African Americans in Census tracts with more Black residents. Somali Americans are a smaller community in Minneapolis and experience less residential segregation than African Americans (Crowell and Fossett 2020); consistent with this different

residential pattern, tract demographics play a smaller role in predicting the impact of the new policy on Somali Americans.

Using St. Paul and suburbs as control group

To account for any broader secular change in discrimination at the time the policy went into effect, Table 6 uses a triple difference to compare the changes in Minneapolis (where the policy was implemented) to St. Paul and the contiguous suburbs (where it was not).

TABLE 6 HERE

The coefficients that are of most of interest in Table 6 are the triple interactions (After policy & Minneapolis & Somali American and After policy & Minneapolis & African American). These coefficients show how the racial disparity in positive contact rates changed in Minneapolis compared to St. Paul and the suburbs. The fact that these values are negative indicates that discrimination worsened in Minneapolis relative to St. Paul and the suburbs after the policy went into effect. This pattern is largest for 2 or more bedroom rental units (Column 2) and not statistically significant for one-bedroom units (Column 3). In this analysis, St. Paul and the suburbs serve as a "counterfactual" or what would have occurred in Minneapolis in the absence of the policy, suggesting that the policy, and not other secular changes, increased the disparity in positive contact to initial inquiries from rental applicants.

Robustness check 1: Impact of protests over George Floyd's murder

Our analysis of the impact of the new policy in Minneapolis rests on St. Paul and the suburbs serving as an adequate control. One threat to identification would come from unobserved changes in the rental market around the same time as the policy that affect Minneapolis differently relative to St. Paul and the suburbs. One possible source of a change in the rental market in Minneapolis relative to St. Paul is the protests following the murder of George Floyd. The new housing policy went into effect on June 1, 2020. On May 25, 2020, George Floyd, an unarmed Black man, was killed in Minneapolis during an arrest after a police officer knelt on his neck. The next day, after videos of the incident became public, protestors took to the streets of Minneapolis. Alongside massive protests was property damage to more than 1,500 locations, mostly restaurants and stores (Penrod et al. 2020). The Third precinct of the Minneapolis Police Department and dozens of buildings were burned to the ground, many businesses were damaged extensively, and two people were killed (Jany 2020). Much of the damage was concentrated in south Minneapolis, particularly near the Third and Fifth precinct police stations.

Because the protests likely impacted the housing market in the affected neighborhoods and was close in time to the implementation of the new policy, it is possible our analysis is affected by the impact of the protests. To examine

this, Table 7 repeats the main analysis but excludes rental units in south Minneapolis where the bulk of property damage occurred (zip codes 55406 and 55407). Column 1 shows the change in discrimination in Minneapolis after the policy went into effect. Column 2 shows the triple difference – comparing the change in discrimination in Minneapolis to St. Paul.

Column 1 in Table 7 shows that excluding rental units in south Minneapolis, the increase in discrimination in Minneapolis after the policy went into effect remains similar in magnitude, although not statistically significant as found in Table 2. Column 2 shows that the change in Minneapolis is larger than St. Paul and remains similar in magnitude to that found in Table 6 and remains statistically significant. Thus, there are no substantive differences to our results when we exclude rental units in the parts of Minneapolis most affected by the protests.

TABLE 7 HERE

Robustness check 2: Impact of COVID-19

As mentioned in the data section, we altered our data collection process because of COVID-19. We began data collection in January 2020 and sent three emails to each landlord. Because the stay-at-home order in March 2020 decreased activity in the rental housing market, landlords may have become suspicious of our emails. While we sent one email per ad from April 2020 to August 2020, we sent two emails per ad from August 2020 to June

2021. Because our applicants are in competition with one another for the same rental property, discrimination could increase when we send more inquiries. The triple-difference design should account for the difference because we did not make different changes in data collection between Minneapolis and St. Paul. However, to check if the number of applications sent alters discrimination, we examine the proportion of responses with positive contact by race/ethnicity based on how many inquiries were sent to each ad.

Figure 3 shows that increasing the number of applications sent does not increase discrimination; the difference in responses to white American, Somali American, and African American inquiries is very similar between when one or two inquiries was sent and is in fact smaller when three inquiries were sent. This suggests that changing the number of inquiries sent due to COVID-19 is not driving our results.

FIGURE 3 HERE

Conclusions

In this paper, we examined the impact of a set of policies that reduced a wide range of information about rental housing applicants. We showed that this policy affected landlord behavior: after the policy went into effect, ads for 2 or more bedroom units in Minneapolis dramatically reduced the use of credit score cutoffs, text like "Clean background check" or "No felonies," and income or security deposits requirements that violated the new policy. We

additionally find increased discrimination in Minneapolis against both Somali American and African American applicants after the policy went into effect. This increase was largest for men, in Census tracts with more Black, Somali American, and foreign-born residents, and for units with two or more bedrooms. To account for possible secular changes occurring at the same time, we used a triple-difference model comparing changes in Minneapolis (where the policy went into effect) to changes in St. Paul and the contiguous suburbs (where the policy did not go into effect). We confirm our finding that discrimination in Minneapolis increased relative to St. Paul and the suburbs after the policy went into effect.

Policies that limit information on applicants, whether in employment or housing, are typically intended to decrease disparities and increase access. Our analysis sheds light on the subtle ways in which racism can lead to unintended negative impacts of well-intentioned policies. Minneapolis policymakers wanted to increase access to rental housing for people who would have previously been screened out based on criminal record, credit history and income. Indeed, we found that ads requiring specific credit scores or banning applicants with any criminal records fell dramatically after the policy went into effect – the policy achieved that aspect of its goal.

However, because this policy operates in a society with large racial disparities in income, credit score, and criminal history, restricting information on individual applicants appears to have caused landlords to rely more on stereotypes and increased discrimination against Somali American

and African American renters. The discrimination we observed occurs prior to submitting a formal rental application and largely manifests in the landlord simply not responding to inquiries from Somali American and African American applicants. This form of discrimination is virtually impossible for an individual applicant to detect or report and hard for policymakers to combat. We find that a race-neutral policy, implemented in a stratified society, has a biased result that primarily impacts African American and Somali American men.

Research on racial inequities in housing often focus on discrimination in mortgage lending because of homeownership's role in wealth building (for example, Ladd (1998), Apgar and Calder (2005), Charles and Hurst (2002), although others note that homeownership alone does not significantly reduce the wealth gap (Darity et al. 2018)). While rental housing does not build wealth, reducing discrimination in the rental housing market is important for increasing housing stability. This is particularly true in Minnesota, where the vast majority of African Americans (78%) and Somali Americans (90%) rent their homes (MNSDC 2018). Understanding how to increase access to housing and also reduce rental housing discrimination is an essential component of developing policies to increase racial equity in housing.

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Figures

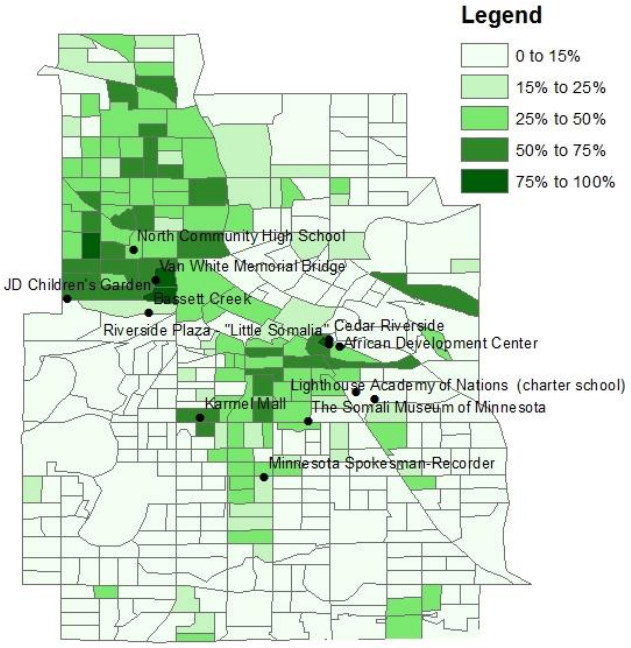

Figure 1: *A map of Minneapolis showing the proportion reporting their race as "Black or African American" (2014 pooled 5 year ACS via American FactFinder). Figure from Gorzig and Rho (2021)*

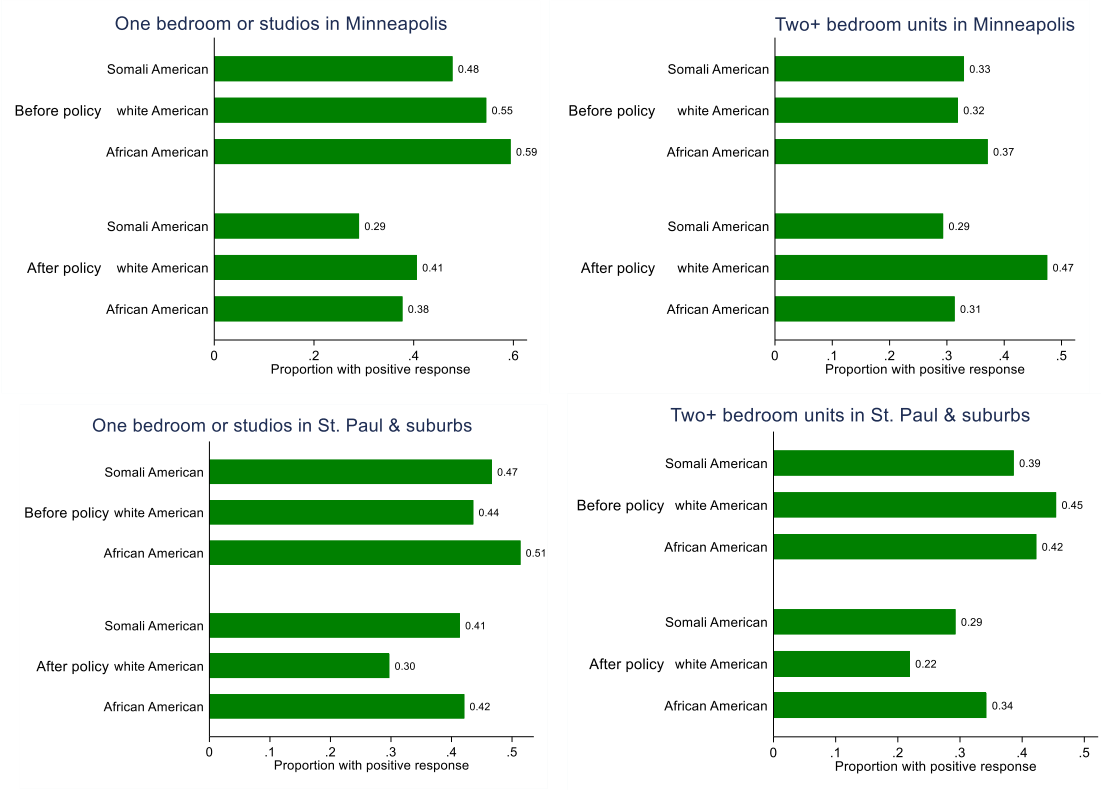

Figure 2: *Proportion of positive responses by race/ethnicity before and after the policy went into effect among ads listed by companies who have 15 or more units. Top row includes Minneapolis apartments (n= 978) and bottom row includes St. Paul (n=830). Left panel includes one bedrooms or studios and right panel includes units with two or more bedrooms.*

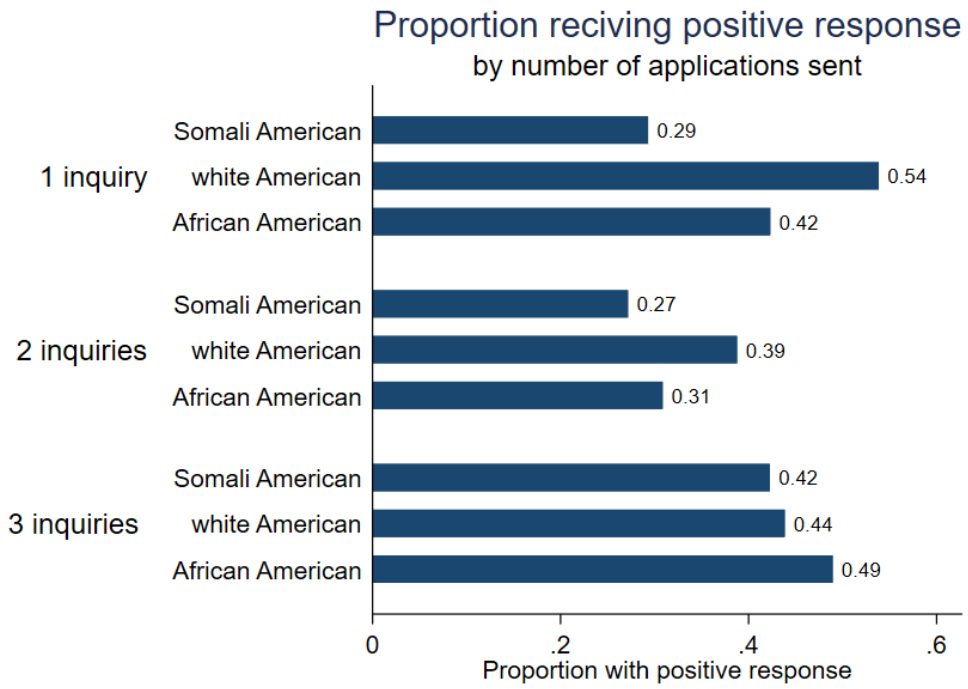

Figure 3: *Proportion of inquiries that received a positive response by the number of total applications sent to the ad and the race/ethnicity of the name used. Includes large landlords in Minneapolis. n=978*

Tables

Table 1: Descriptive Statistics among emails sent to companies likely to have 15+ units

	(1) Percentage or average	(2) Chi-squared test	(3) P-value
Email characteristics			
Somali American	33%		
White American	34%		
African American	33%		
Female	48%	.16	.92
Order sent			
Sent first	49%	3.2	.52
Sent second	33%		
Sent third	19%		
Ad characteristics			
Two or more bedrooms	56%	.56	.76
Minneapolis	54%	.29	.86
Monthly rent	$1524.44		
Prohibits those with criminal record (n=1,806)	9.6%	.57	.75
Prohibits previous evictions (n=1,806)	12.1%	.94	.63
Requires income 3x rent (n=1,806)	7.6%	.79	.67
Includes specific credit score (n=1,806)	7.4%	.23	.89
Security deposit>rent (n=1,806)	3.3%	.12	.94
Outcomes			
Positive contact	40%	7.6	0.27
Non-committal	22%		
No response	36%		
Negative	2.6%		

Column (1) shows the percentage of the emails with each characteristic. Column (2) shows the chi-squared statistic for the test that the characteristic is distributed equally across the key manipulation (African American, Somali American, and white American). Column (3) shows the p-value for the chi-squared test in Column (2). n=1,808

Table 2: Percentage of ads that violated policy in Minneapolis

Two bedroom	*Before policy*	*After policy*	*P-value*	**One bedroom/studio**	*Before policy*	*After policy*	*P-value*
Criminal history	10.6%	3.6%	.003	*Criminal history*	3.4%	2.8%	.720
Eviction history	13.1%	8.1%	.076	*Eviction history*	5.4%	6.1%	.743
Income 3x rent	11.0%	6.8%	.105	*Income 3x rent*	4.1%	6.7%	.205
Credit score	8.9%	2.7%	.004	*Credit score*	2.7%	1.7%	.469
Security deposit > monthly rent	3.5%	2.3%	.402	*Security deposit > monthly rent*	2.4%	1.7%	.610

Percentage of ads that included language in violation of the new policy among large Minneapolis landlords. The fourth and eighth columns shows the p-value from testing if the difference between before and after the policy is different from 0. The left panel (n=503) includes 2 or more bedrooms. The right panel (n=474) includes one bedrooms/studios

Table 3: Results of a Linear Probability Model: Impact of policy on receiving positive contact from landlord (Minneapolis)

Outcome variable: Positive contact	(1)	(2)	(3)
		2+	1
	All	bedroom	bed/studio
After policy (June 1 2020 - landlords with 15+ units)	0.00921	0.156*	-0.139
	(0.0608)	(0.0826)	(0.0863)
Somali American	-0.0319	0.0105	-0.0677
	(0.0498)	(0.0691)	(0.0680)
African American	0.0525	0.0520	0.0489
	(0.0480)	(0.0636)	(0.0667)
After policy & Somali American	-0.121	-0.192*	-0.0483
	(0.0749)	(0.100)	(0.111)
After policy & African American	-0.155**	-0.214**	-0.0778
	(0.0758)	(0.100)	(0.112)
Constant	0.435***	0.319***	0.545***
	(0.0411)	(0.0557)	(0.0558)
Observations	978	504	474
R-squared	0.017	0.015	0.042

Results of linear probability model regressing an indicator for positive contact from the landlord on an indicator variable for being after the policy went into effect (June 1), indicators for Somali American and African American sounding names, and their interactions. Regressions include ads from large companies in Minneapolis. Robust standard errors clustered by job ad. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Results of a Linear Probability Model: Impact of policy on receiving positive contact in Minneapolis by perceived gender of applicant

Outcome variable: Positive contact	(1)	(2)
	Women	**Men**
After policy (June 1 2020 - landlords with 15+ units)	-0.0391	0.0497
	(0.0817)	(0.0792)
Somali American	-0.00946	-0.0521
	(0.0729)	(0.0662)
African American	-0.0376	0.118*
	(0.0726)	(0.0642)
After policy & Somali American	-0.0605	-0.178*
	(0.107)	(0.103)
After policy & African American	-0.0563	-0.205*
	(0.109)	(0.108)
Constant	0.433***	0.437***
	(0.0542)	(0.0535)
Observations	475	503
R-squared	0.010	0.035

Results of linear probability model regressing an indicator for positive contact from the landlord on an indicator variable for being after the policy went into effect (June 1), indicators for Somali American and African American sounding names, and their interactions. Regressions include only ads from large companies in Minneapolis . Robust standard errors clustered by job ad. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Results of a Linear Probability Model: Impact of policy on receiving positive contact in Minneapolis by neighborhood demographics

Outcome variable: Positive contact

	(1)	(2)	(3)	(4)	(5)	(6)
	<15% Black	<15% immigrant	<2.3% Somali	>15% Black	>15% immigrant	>2.3% Somali
After policy	-0.0437	-0.0173	-0.0294	0.0890	0.0567	0.132
(June 1 2020 – landlords with 15+ units)	(0.0849)	(0.0780)	(0.0704)	(0.0833)	(0.100)	(0.124)
Somali American	-0.0641	-0.0574	-0.0282	0.0258	0.00667	-0.0222
	(0.0718)	(0.0725)	(0.0677)	(0.0803)	(0.0840)	(0.0857)
African American	0.0207	-0.00991	-0.00460	0.103*	0.154*	0.217**
	(0.0625)	(0.0542)	(0.0505)	(0.0593)	(0.0783)	(0.0898)
After policy & Somali American	-0.0839	-0.0791	-0.146	-0.201*	-0.185*	-0.101
	(0.104)	(0.103)	(0.0924)	(0.101)	(0.108)	(0.120)
After policy & African American	-0.0585	-0.0916	-0.0764	-0.289***	-0.261*	-0.357**
	(0.107)	(0.0956)	(0.0897)	(0.0983)	(0.132)	(0.145)
Constant	0.458***	0.487***	0.469***	0.403***	0.360***	0.333***
	(0.0545)	(0.0520)	(0.0477)	(0.0694)	(0.0756)	(0.101)
Observations	569	601	697	402	370	274
R-squared	0.018	0.012	0.019	0.023	0.033	0.037

Results of linear probability model regressing an indicator for positive contact from the landlord on an indicator variable for being after the policy went into effect (June 1), indicators for Somali American and African American sounding names, and their interactions. Regressions include ads with enough information to geocode to a Census tract from large companies in Minneapolis. Robust standard errors clustered by Census tract. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Results of a Triple Difference Model: Impact of policy on receiving positive contact in Minneapolis relative to St. Paul

	(1)	(2)	(3)
	All	**2+ bedrooms**	**One bedroom/studio**
After policy (June 1 2020 - landlords with 15+ units)	-0.200***	-0.235***	-0.139
	(0.0643)	(0.0826)	(0.107)
Somali American	-0.0225	-0.0682	0.0308
	(0.0519)	(0.0712)	(0.0756)
African American	0.0158	-0.0319	0.0780
	(0.0563)	(0.0774)	(0.0806)
Minneapolis	-0.0105	-0.135	0.110
	(0.0633)	(0.0860)	(0.0908)
After policy & Somali American	0.101	0.142	0.0857
	(0.0775)	(0.0966)	(0.139)
After policy & African American	0.108	0.155	0.0458
	(0.0817)	(0.104)	(0.135)
After policy & Minneapolis	0.210**	0.391***	-0.000604
	(0.0881)	(0.116)	(0.137)
Minneapolis & Somali American	-0.00945	0.0787	-0.0984
	(0.0719)	(0.0992)	(0.102)
Minneapolis & African American	0.0367	0.0839	-0.0291
	(0.0740)	(0.100)	(0.105)
After policy & Minneapolis & Somali American	-0.222**	-0.334**	-0.134
	(0.107)	(0.138)	(0.178)
After policy & Minneapolis & African American	-0.263**	-0.369**	-0.124
	(0.111)	(0.145)	(0.175)
Constant	0.446***	0.455***	0.436***
	(0.0482)	(0.0656)	(0.0716)
Observations	1,808	1,005	803
R-squared	0.020	0.021	0.032

Results of linear probability model regressing an indicator for positive contact from the landlord on an indicator variable for being after the policy went into effect (June 1), indicators for Somali American and African American sounding names, being in Minneapolis, and their interactions. Regressions include ads from large companies. Robust standard errors clustered by job ad. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Impact of policy on receiving positive contact excluding south Minneapolis

	(1)	(2)
After policy	0.00694	-0.200***
(June 1 - for large landlords)	(0.0647)	(0.0643)
Somali American	-0.0270	-0.0225
	(0.0520)	(0.0519)
African American	0.0323	0.0158
	(0.0497)	(0.0564)
After policy & Somali American	-0.107	0.101
	(0.0809)	(0.0775)
After policy & African American	-0.137*	0.108
	(0.0803)	(0.0817)
Minneapolis		-0.00557
		(0.0642)
After policy and Minneapolis		0.207**
		(0.0908)
Minneapolis and Somali American		-0.00457
		(0.0734)
Minneapolis & African American		0.0166
		(0.0751)
After policy & Minneapolis & Somali American		-0.211*
		(0.112)
After policy & Minneapolis & African American		-0.245**
		(0.115)
Constant	0.440***	0.446***
	(0.0424)	(0.0482)
Observations	885	1,715
R-squared	0.012	0.018

Results of linear probability model regressing an indicator for positive contact from the landlord on an indicator variable for being after the policy went into effect for large landlords (June 1), indicators for Somali American and African American sounding names, and their interactions. Column additionally includes an indicator for Minneapolis and interactions. Columns 1 includes only Minneapolis observations (excluding 55406 and 55407 zip codes) from companies who likely have 15+ units. Column 2 includes all observation (excluding 55406 and 55407 zip codes) from large companies. Robust standard errors clustered by job ad.
*** p<0.01, ** p<0.05, * p<0.1

Appendix 1: Experimental Design

The email addresses of our fictitious applicants are: aashawaabberi@gmail.com, abdullah.abukar16@gmail.com, amber.sullivan658@gmail.com, amy.wood4558@gmail.com, andre.robinson223@gmail.com, fathia.hassan215@gmail.com, imani.williams286@ gmail.com, jalen.harris223@gmail.com, khalid.bahdoon1620@gmail.com, lucaspeterson898@gmail.com, niajackson349@gmail.com, and myersjacob779@gmail.com.

Each email inquiry includes a greeting, a statement about finding the rental listing, a line expressing interest in the unit, and a closing. The greeting is randomly chosen from three versions, the first statement is randomly chosen from four versions, the second statement is randomly chosen from five versions, and the closing is randomly chosen from four versions. This leads to 240 possible combinations. None of the components are repeated within a listing. The components of the inquiries are given in Table A1. Figure A1 gives an example of the email inquiry and indicates the components of the randomizations.

Table A1 Email Inquiry Components

Greeting

Hello,

Hi there,

Hi,

First Statement (two or more bedrooms)

I am responding to your Craigslist ad about the available rental.

I found your rental listing on Craigslist.

I am interested in the property you are renting.

I am interested in learning more about the property you listed for rent on Craigslist.

First Statement (one bedrooms/studios)

I am responding to your Craigslist ad about the available one-bedroom.

I found your rental listing on Craigslist for a one-bedroom.

I am interested in the one-bedroom property you are renting.

I am interested in learning more about the property you listed for rent on Craigslist.

Second Statement (two or more bedrooms)

My family and I are searching for a place to live, and your property looks like a good fit.

I am looking for a home for my family and would like to learn more about this rental.

I am looking for a new home for my family and am interested in this property.

My family is looking for a new home, and your rental looks like it may be a good fit for us.

My family is searching for a new home, and we are interested in your property.

Second Statement (one bedrooms/studios)

I am searching for a one-bedroom/studio apartment and your property looks like a good fit.

I am looking for a new home and would like to learn more about this rental.

I am looking for a new home and am interested in this one-bedroom/studio.

I am looking for a new home, and your rental looks like it may be a good fit for me.

I am searching for a new home, and I am interested in your one-bedroom/studio rental.

Closing

Sincerely,

Thanks,

Thank you,

Regards,

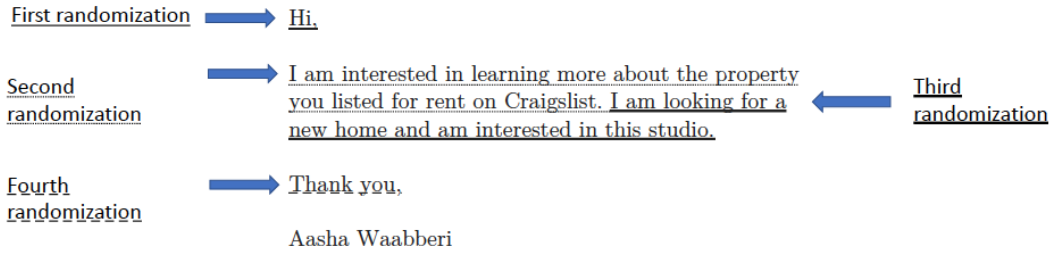

Figure A1: *Example of email text sent (1 of 240 possible combinations)*

Below are examples of positive, negative, and neutral responses from landlords.

Example of positive response received:

Hi Aasha
Great-would you like to set up a showing?
Thanks!
[redacted]

Example of negative response received:

Thank you for your interest, I believe that I have someone signing the lease soon. I wish you the best of luck in your search.

Example of neutral response:

Thank you for your inquiry Aasha. Please tell me a bit about your family—number of persons, ages of children, monthly income. Thank you. [redacted]

The number of emails sent each month varied with the amount of time the RAs had available, the number of eligible ads, and the number of emails sent per ad. While the number of total inquiries varied, Minneapolis and St. Paul/suburbs closely tracked each other over time. Figure A2 displays the total number of emails sent per month for the whole sample and Figure A3 is for those landlords who likely have 15 or more total units.

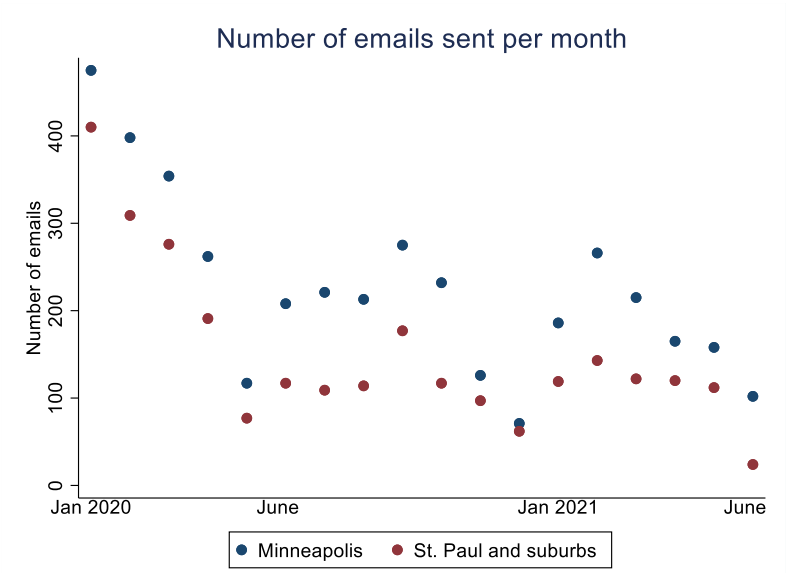

Figure A2: *The total number of emails sent each month in the full experiment n=6,740*

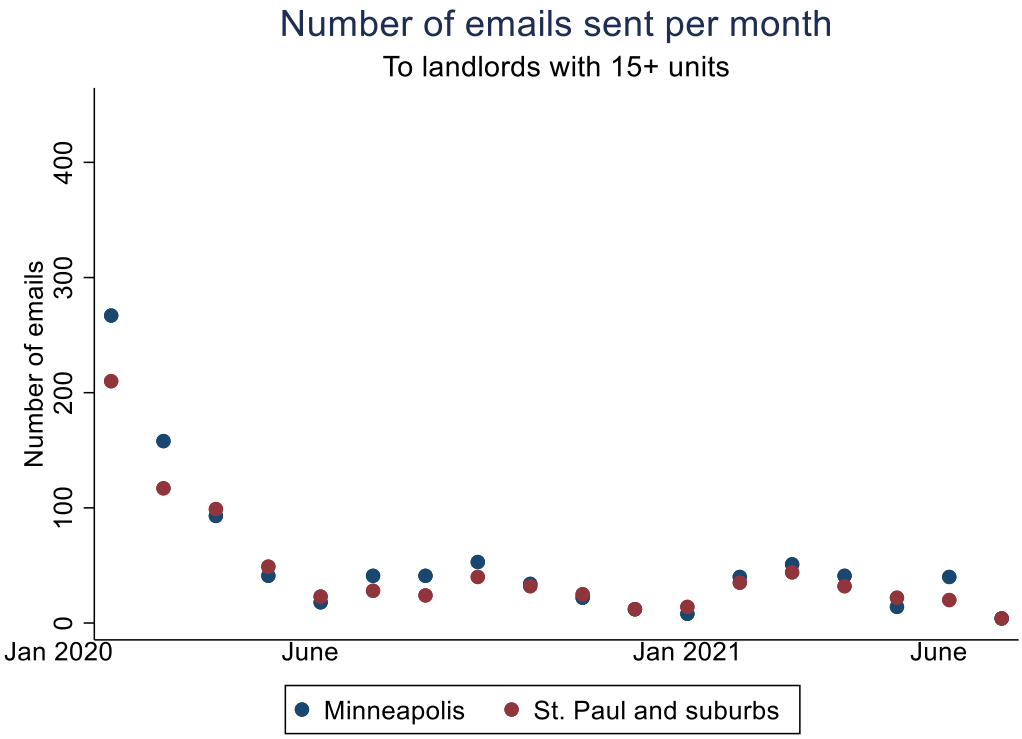

Figure A3: *The total number of emails sent each month to landlords with 15+ units n= 1,808*

Appendix 2: Listing name in ad

It is possible that landlords became cautious about listing their name in an ad after the new policy was passed. We use this variable to identify those companies with 15 or more units, so it is important to consider whether selection into our sample changed after the policy passed. That is, companies who discriminate may be less likely to be in our sample after the policy passed, biasing any estimated increase in discrimination downwards. To check for this, Figure A4 shows the proportion of emails that were sent to ads with a company name listed for Minneapolis (solid line) and St. Paul/suburbs (dashed line).

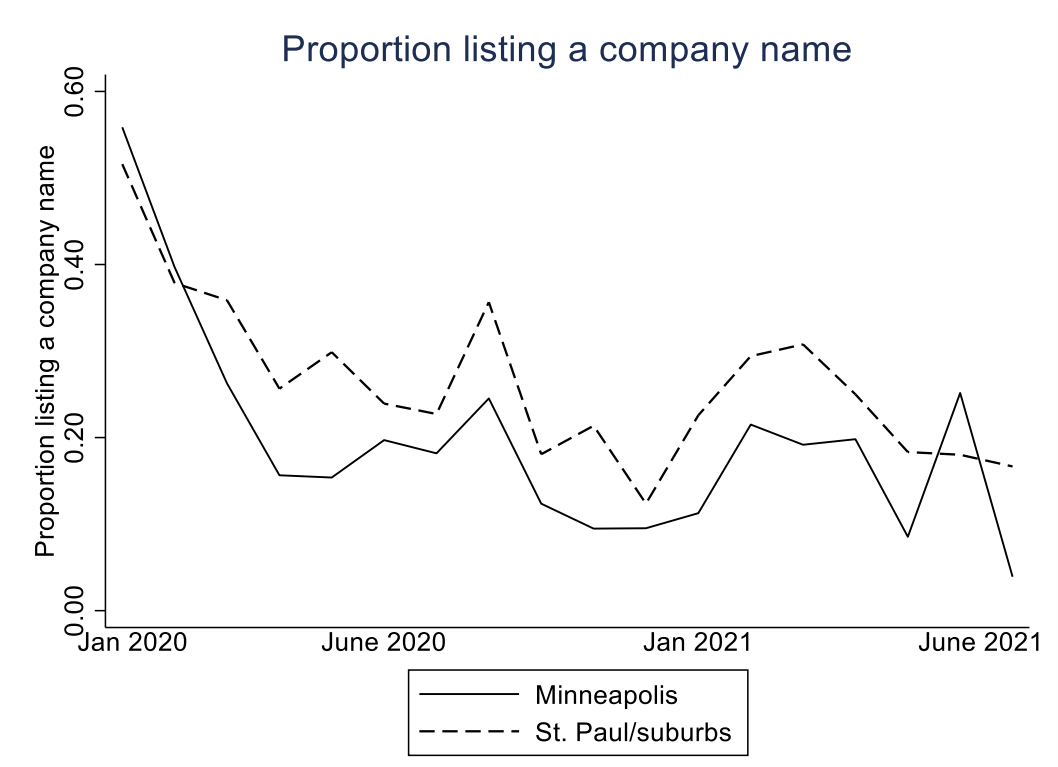

Figure A4: *Proportion of all inquiries sent to ads that have a company name listed by month and year of email inquiry. n=6,740*

During data collection, a second inquiry was not sent to the same company for at least six months, which causes large landlords to be over-represented in January 2020 when we began data collection. The proportion of inquiries sent to large landlords falls as time passes. This change is very similar between St. Paul/suburbs and Minneapolis because it is due solely to the data collection design. While a change in selection into/out of including a company name in an ad is still possible, there is little evidence of substantial change in Minneapolis landlord's willingness to include the company name in the ad at the time the policy was implemented.

Table A2 shows summary statistics for ads that listed a company name and those that did not. Ads with a company name were more likely to be one-bedroom apartments, had higher positive response rates, and were less likely to be from Minneapolis.

Table A2: Comparison of ads that listed a company name and those that did not

	No company name listed	**Company name listed**
Monthly rent	$1,528.33	$1,524.44
Minneapolis	62%	54%
Two or more bedrooms	80%	56%
Positive response	32%	40%
House	17%	5%
Apartment	33%	76%
Condo	4%	6%
Other (e.g., duplex)	45%	13%

Table A3: Descriptive Statistics among emails sent to companies likely to have 15+ units before and after the policy

	(1)	(2)
	Before policy	**After policy**
Email characteristics		
Somali American	32%	34%
White American	33%	35%
African American	35%	32%
Female	48%	48%
Order sent		
Sent first	38%	65%
Sent second	31%	35%
Sent third	31%	
Ad characteristics		
Two or more bedrooms	52%	61%
Minneapolis	54%	55%
Monthly rent	$1,538.95	$1,503.99
Outcomes		
Positive contact	44%	34%
Non-committal	20%	24%
No response	33%	39%
Negative response	2%	3%

Column (1) shows the percentage of the emails with each characteristic before the policy was implemented. Column (2) shows the percentage of the emails with each characteristic after the policy was implemented. n=733 (after), 1,075 (before)

Appendix 3: Balance test for all landlords

Columns 2 and 3 of Table A4 show the results of a chi-squared test that each characteristic is distributed equally across the race/ethnicity manipulation among all ads (not just those that are likely to owned by landlords with 15+ units). We fail to reject that the characteristics are distributed equally (at the 5% level) for all the email and ad characteristics.

Table A4: Descriptive Statistics for all emails sent

	(1) Percentage of emails or average	(2) Chi-squared test	(3) P-value
Email characteristics (n=6,740)			
Somali American	33%		
White American	34%		
African American	33%		
Female	49%	.98	.61
Order sent			
Sent first	54%	7.3	.12
Sent second	34%		
Sent third	12%		
Ad characteristics (n=6,740)			
Two or more bedrooms	74%	.16	.92
Minneapolis	60%	4.09	.131
Company name listed in ad	27%	.31	.86
Monthly rent (n=6,737)	$1527.29		
Prohibits those with criminal record	8.4%	.07	.967
Prohibits previous evictions	13.3%	.06	.969
Requires income 3x rent	6.8%	.90	.64
Includes specific credit score	6.8%	.31	.857
Security deposit>rent	4.1%	.35	.839
Outcomes (n=6,740)			
Positive contact	34%	36.5	0.000
Non-committal	18%		
No response	42%		
Negative	5%		

Column (1) shows the percentage of the emails with each characteristic. Column (2) shows the chi-squared statistic for the test that the characteristic is distributed equally across the key manipulation (African American, Somali American, and white American). Column (3) shows the p-value for the chi-squared test in Column (2). n=6,740

Appendix 4: Changes in ad text in St. Paul and suburbs

Table A5: Percentage of ads that violated policy in St. Paul ad suburbs

Two bedroom	*Before policy*	*After policy*	*p-value*	**One bedroom/studio**	*Before policy*	*After policy*	*p-value*
Criminal history	16.1%	12.8%	.29	*Criminal history*	16.0%	10.6%	.19
Eviction history	16.5%	15.9%	.85	*Eviction history*	17.8%	14.4%	.45
Income 3x rent	8.1%	7.9%	.96	*Income 3x rent*	10.7%	3.8%	.04
Credit score	13.6%	8.8%	.10	*Credit score*	13.8%	3.8%	.01
Security deposit > monthly rent	3.7%	3.5%	.93	*Security deposit > monthly rent*	6.7%	1.9%	.07

The proportion of ads in St. Paul and contiguous suburbs that included criteria prohibited by the Minneapolis policy before and after the policy went into effect (with the June 1 date). The fourth and eighth columns shows the p-value from testing if the difference between before and after the policy is different from 0. This table includes ads with a company name. n=829

Appendix 5: Monthly pre-trends prior to the policy

An important assumption of our triple difference analysis is that the two cities were trending together prior to the policy. Indeed, Minneapolis and St. Paul/suburbs had similar positive contact rates prior to the policy for all applicants combined and within each race and ethnicity. This supports the validity of our identification strategy. Among large companies, there is more noise in the monthly positive contact rates because of the smaller sample sizes for each month. However, there is no clear pattern that distinguishes the callback trends in Minneapolis and St. Paul/suburbs.

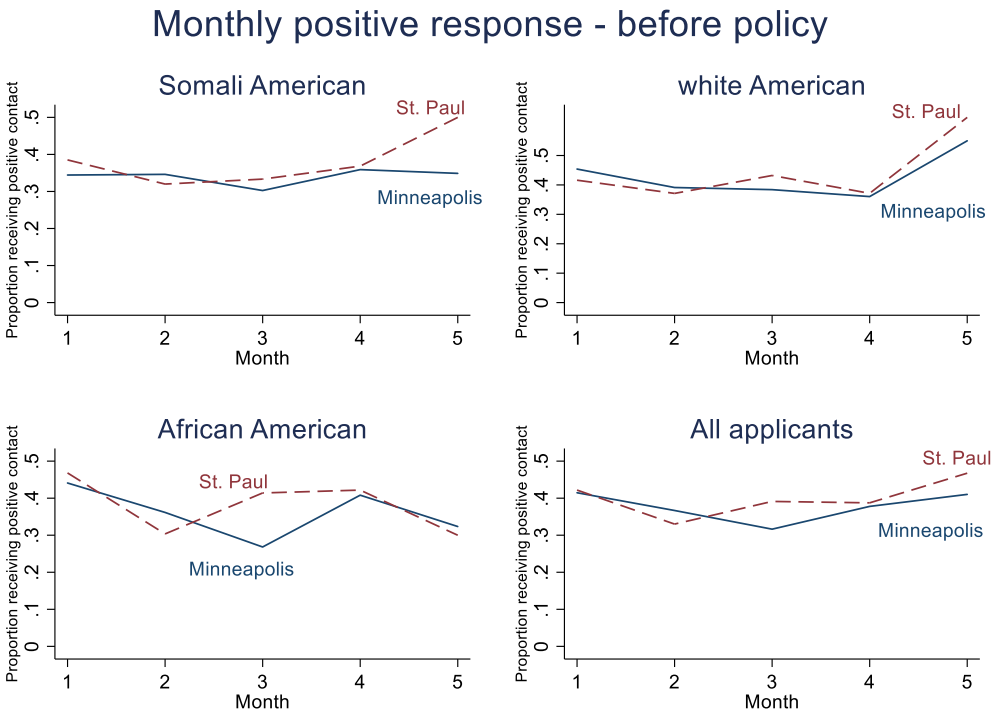

Figure A5: *The proportion who received a positive response by month before the policy was implemented. Minneapolis is shown with a solid line and St. Paul and the suburbs with a dashed line. This figure includes all applications.*

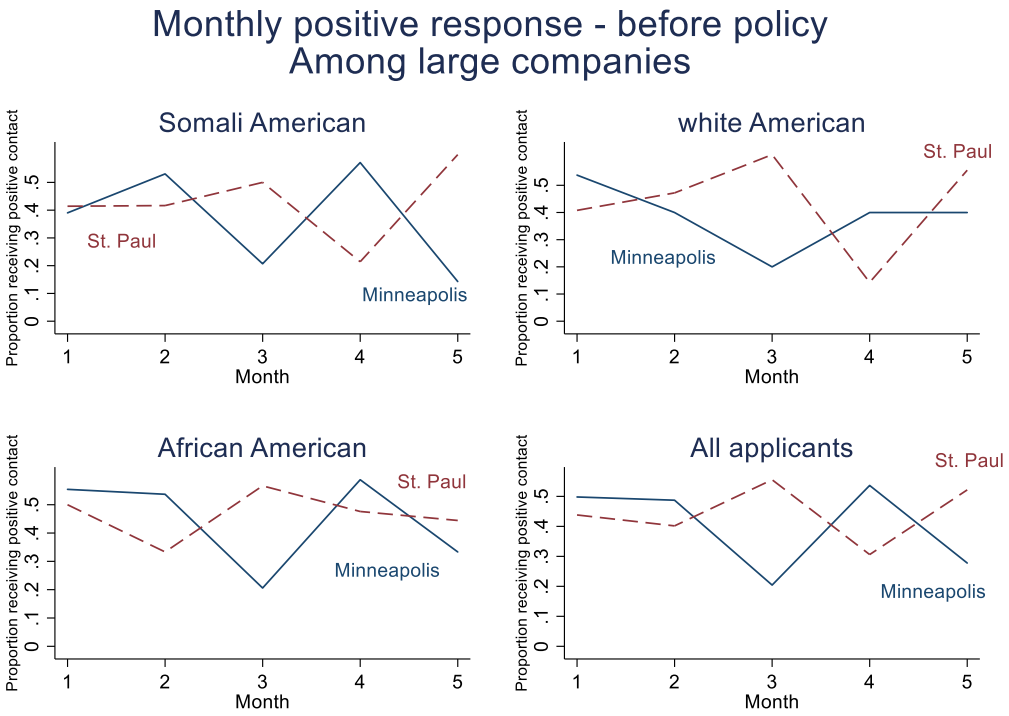

Figure A6: *The proportion who received a positive response my month before the policy was implemented. Minneapolis is shown with a solid line and St. Paul and the suburbs with a dashed line. This figure includes only applications to large companies.*

Appendix 6: Differences by other neighborhood characteristics

Table A6 shows the result of Equation 1 stratified by whether the unit is located in a Census tract with racially restrictive covenants. Comparing the coefficients in Column 1 to Column 2, shows that for Somali Americans there is no difference in the impact of the policy in areas with and without covenants. For African Americans, the increase is discrimination is concentrated in tracts *without* covenants (Column 1). This is consistent with the impact of the policy being largest for African Americans in tracts with more Black residents.

Table A6: Results of a Linear Probability Model: Impact of policy on receiving positive contact in Minneapolis by presence of historical racial covenants

Outcome variable: Positive contact

	No covenants	Covenants	All
After policy (June 1 2020 - landlords with 15+ units)	0.0251	-0.0666	-0.00338
	(0.0743)	(0.113)	(0.0625)
Somali American	-0.0330	-0.0194	-0.0305
	(0.0631)	(0.129)	(0.0568)
African American	0.0503	0.0342	0.0462
	(0.0519)	(0.0939)	(0.0454)
After policy & Somali American	-0.127	-0.116	-0.122
	(0.0897)	(0.150)	(0.0773)
After policy & African American	-0.179*	-0.0572	-0.144*
	(0.0910)	(0.128)	(0.0762)
Constant	0.450***	0.432***	0.446***
	(0.0500)	(0.0956)	(0.0440)
Observations	707	243	950
R-squared	0.016	0.025	0.018

Results of linear probability model regressing an indicator for positive contact from the landlord on an indicator variable for being after the policy went into effect (June 1), indicators for Somali American and African American sounding names, and their interactions. Regressions include ads with enough information to geocode to a Census tract from large companies in Minneapolis. Robust standard errors clustered by Census tract ad. *** p<0.01, ** p<0.05, * p<0.1