When Prejudice Hits Home: Hate Crime and the Market for Mortgage Credit*

Christian Engels[†] Iftekhar Hasan[‡] Sizhe Hong[§] Dennis Philip[¶]

May 2026

Abstract

Using data covering the near-universe of U.S. mortgage applications, this article documents that racial hate crimes significantly depress local mortgage demand. This decline is specific to racial hate crimes; no comparable effects arise for general crime and nonracial offenses. Both minority and White applicants reduce applications, with most minorities responding to incidents targeting their own group, indicating targeted threat, and White borrowers responding to incidents against other groups, consistent with generalized fear. Affected areas also exhibit higher psychological distress, reduced conspicuous consumption, and lower home sales and house price growth, while rents are unaffected, consistent with effects concentrated in owner-occupied housing.

Keywords: Household Finance, Mortgage Credit, Hate Crime, Racial Animus
JEL Codes: G51, G21, D12, R21, J15, K42

*We thank John Gathergood, Pedro Gete, Michael Haliassos, Christodoulos Louca, Duc Duy (Louis) Nguyen, and Denis Sosyura for helpful suggestions and comments.

†Centre for Responsible Banking & Finance, University of St Andrews, Gateway Building, North Haugh, St Andrews KY16 9AL, UK. E-mail: ce50@st-andrews.ac.uk

‡Gabelli School of Business, Fordham University and Bank of Finland. E-mail: ihasan@fordham.edu.

§Adam Smith Business School, University of Glasgow, Gilbert Scott Building, Glasgow G12 8QQ, UK. E-mail: sizhe.hong@glasgow.ac.uk

¶Durham University Business School, Mill Hill Lane, Durham DH1 3LB, UK. E-mail: dennis.philip@durham.ac.uk

"*We have three people who are dead because they are Black,*" State Senator Tracie Davis, a Jacksonville Democrat, said at a vigil on Sunday morning. "*Shopping. In our community. Gunned down. Because they were Black.*" — New York Times, 2023

1. Introduction

Acts of violence directed at people because of their race, color or ethnicity have been rising in recent years. The 2023 FBI report records the highest level of hate-related (or bias-motivated) crimes in the three decades for which the FBI has collected systematic data. These hate crimes have the potential to cause economic harm (Cook, 2014) and displacement to lower-quality neighborhoods (Chetty et al., 2020; Christensen & Timmins, 2022). More broadly, local social crises such as opioid abuse depress real estate values through higher delinquencies, population outflow and neighborhood decline (Custódio et al., 2025). Emerging research suggests that hate crimes hinder the assimilation of immigrant communities and diminish workplace productivity (Gould & Klor, 2016; Agarwal et al., 2026). Yet we still know relatively little about how racially motivated hate crimes shape mortgage credit and housing demand, how these effects propagate across racial and ethnic groups, and whether they extend beyond targeted minorities.

In this study, we examine the economic costs of racial hate crimes through their effects on the demand for mortgage credit and housing choices. This is important because homeownership is a cornerstone of household wealth accumulation and intergenerational mobility (Ray et al., 2021), and homeowners tend to form strong community ties. However, racial hate crimes by design target specific groups and communities, often through intimidation, property destruction and symbolic acts of violence. Such incidents increase insecurity, erode trust and undermine the attractiveness of affected neighborhoods for residents and investors. We focus on whether and how racially motivated hate crimes reduce mortgage applications and originations, whether the response differs across racial and ethnic groups, and whether local housing markets contract in affected areas.

Our empirical analysis brings together several rich data sources. We use the near-universe of U.S. mortgage loan applications retrieved from the Home Mortgage Disclosure Act (HMDA) database to examine mortgage demand. Each observation in the database represents a loan application that includes geographic, lender, and loan take-up information, allowing us to study application outcomes and compare demand-side and supply-side interpretations. HMDA also records the race and ethnicity of the applicant(s), enabling us to observe variations in mortgage application patterns across different racial groups over time. We merge these data with detailed county-level information on racial hate crime incidents and victimization from the FBI's Uniform Crime Reporting (UCR) program, household-level data on psychological distress and visible consumption from the PSID and CEX, and complementary data on overall crime, county demographics, and rental and housing markets.

The typical hate crime incident (an act of vandalism, intimidation, or simple assault) may seem too localized to affect aggregate mortgage demand across an entire county. Two features of these crimes may help explain why the association is meaningful despite this. First, hate crimes are by definition targeted acts of bias whose symbolic resonance extends well beyond the immediate victim. A racially motivated assault or vandalism signals hostility toward an entire group, amplifying perceived risk among all members of the targeted community and, as we show, among non-targeted groups as well. In this respect, hate crimes can generate outsized fear relative to physical harm. Second, even low-frequency incidents can have large effects when they shift beliefs about neighborhood safety. Housing is a long-term commitment, and the decision to apply for a mortgage reflects an assessment of the future trajectory of the local community. A single salient incident can therefore alter the expected returns to homeownership in a way that a single property theft, however costly, does not, because the hate crime reveals information about the social environment rather than just the level of criminal activity.

To identify the effects empirically, we estimate a saturated lender–county–year panel model in which the key regressor is the (lagged) log of racial hate crime incidents, while controlling for county fixed effects, lender–year fixed effects, and a rich set of geographic and banking-sector covariates. This design absorbs time-invariant local heterogeneity, flexibly captures lender-specific time-varying supply conditions, and conditions on contemporaneous local economic and demographic trends. We then implement several complementary strategies to strengthen the analysis. First, we compare racial hate crimes with overall crime to test whether the negative association is specific to racially motivated incidents. Most categories of overall crime exhibit no meaningful effect on mortgage demand once racial hate crimes are introduced separately. Second, we study the George Floyd murder as a landmark racial-violence shock that triggered a surge in hate crime incidents across the country. Using a difference-in-differences design with the Borusyak et al. (2024) imputation estimator, we compare counties that experienced acute hate crime surges after May 2020 with counties that did not, and find that surge counties saw 4.8–8.5% fewer purchase mortgage applications relative to their imputed counterfactual, reinforcing the evidence that salient racial hostility episodes depress mortgage demand.

In our preferred lender–county–year specification, one additional racial hate crime incident at the sample mean corresponds to a 0.11% decline in mortgage applications per lender in a county-year; the county-year aggregate implies −0.2% to −0.6%. The effect is not confined to a single borrower group: when we disaggregate by race and ethnicity, we find that both minority (Black, Asian, Hispanic) and White applicants reduce applications in response to racial hate crime, with the nature of the response differing systematically across racial groups. By decomposing racial hate crime into incidents against a borrower's own racial group and incidents against other groups, we find patterns consistent with a targeted-threat channel through own-race hate crimes for minority borrowers (especially Hispanic applicants), and a generalized-fear channel through hate crimes directed at other racial groups for White and other borrowers, consistent with neighborhood hostility signals depressing demand across all groups.

Along the application margin, racial hate crimes raise withdrawal behavior among minority applicants and lower originations. The pattern is demand-side: denial ratios for minority applicants do not increase, and lender–year fixed effects absorb supply variation by construction. To substantiate the demand-side effects, we supplement this analysis with additional evidence from consumer surveys and census data, showing the prevalence of generalized fear in affected communities. We find that hate crimes are associated with lower psychological well-being and reduced visible consumption on clothing and jewelry. Taken together, these patterns indicate that racially motivated hate crimes undermine neighborhood trust and perceived security, consistent with households reassessing long-term housing commitments and reinforcing the mortgage demand responses we document.

Finally, to assess whether the mortgage demand effects translate into broader neighborhood consequences, we examine local housing markets using rental price data from Zillow and home sales data from CoreLogic. We find that racial hate crimes are associated with weaker local housing markets: home sales volumes and house price growth both decline significantly in more affected counties. Rental prices, however, are unaffected. This null result is informative: households deterred from home purchase who remain local would exert upward pressure on rental demand, so the absence of any rental response argues against large-scale substitution from owning to renting. The evidence is consistent with an intensive-margin effect on homeownership: hate crimes reduce the willingness to hold a long-term, illiquid, place-bound asset in affected communities, without evidence of a commensurate shift into the rental market.

We contribute to several strands of recent literature on racial bias and hate crimes, spanning their causes and consequences. As for causes, studies have identified various factors, including backlash from terror attacks or immigrant-attributed crime in a local community (Gould & Klor, 2016; Riaz et al., 2024), regional economic shocks during the recent COVID-19 pandemic (Dipoppa et al., 2023), the role of social media in spreading xenophobia (Müller & Schwarz, 2021, 2023; Grosjean et al., 2023), inflammatory political campaigns (Grosjean et al., 2023) and the impact of entertainment media in perpetuating racial stereotypes (Ang, 2023). Recent research shows that increased public concerns about immigration translate into discriminatory consumer behavior (Law & Zuo, 2022). Regarding consequences, prior research documents significant societal and economic impacts, such as immigrant communities turning to traditional values and assimilating less well (Gould & Klor, 2016), as well as decreased productivity among fund managers (Agarwal et al., 2026). Historical evidence shows that racially targeted persuasion erodes trust and steers minorities toward inferior financial products (Célérier & Tak, 2025). Whereas policy-driven rate declines improve outcomes for distressed borrowers by facilitating renegotiation (Gabriel & Lutz, 2024), we document a distinct non-price social-hostility channel: racially motivated hate crimes depress new applications across racial groups, raise withdrawals, and dampen local housing activity. Building on this literature, we show how hate crimes influence mortgage credit decisions across both minority and nonminority households. We document effects on housing and neighborhood choices, adding to our understanding of the economic consequences of hate crime.

We also contribute to the literature on housing and neighborhood choice. Previous literature has highlighted the influence of noninstitutional determinants on homeownership decisions. For instance, factors include air pollution, violent crime, neighborhood racial composition and historical anti-Jewish sentiment (Bayer et al., 2009; Bishop & Murphy, 2011; Bayer et al., 2016; D'Acunto et al., 2019). Causal evidence from the Moving to Opportunity experiment shows that neighborhood environments shape household credit outcomes (Miller & Soo, 2021). Social animosity affects housing decisions: households exposed to partisan out-group proximity adjust home-selling behavior (McCartney et al., 2024). Financing constraints help explain racial gaps in access to high-opportunity neighborhoods (Gupta et al., 2025). We complement this neighborhood effects literature by showing that racially motivated violence is associated with lower mortgage demand and weaker local housing markets, and that hate-crime-induced fear and deterioration in perceived neighborhood desirability shape neighborhood choice and attenuate mortgage take-up across groups. We also contribute to the body of work on racial bias in mortgage lending (Ambrose et al., 2021; Bhutta & Hizmo, 2021; Bartlett et al., 2022; Bhutta, 2026), which has mostly focused on supply-side discrimination by lenders against minority borrowers. In contrast, we offer novel insights from the demand side, revealing that both White and minority borrowers self-select out of mortgage opportunities in neighborhoods affected by hate crimes and racial bias. This connects the literatures on housing economics and racial discrimination in lending markets.

The paper proceeds as follows. Section 2 discusses the distinct nature of hate crimes and describes the data. Section 3 presents the baseline effect on mortgage demand, examines targeted-threat and generalized-fear channels through a race-specific decomposition and individual-level evidence on psychological distress and conspicuous consumption, and traces the effects to local housing markets. Section 4 reports the difference-in-differences design around the George Floyd murder. Section 5 presents robustness checks, including tests of supply-side discrimination and news-mediated salience. Section 6 concludes.

2. Background and data

2.1. The distinct nature of hate crime

Hate crimes, also known as bias-motivated crimes, are distinguished by the perpetrator's prejudice against specific perceived attributes such as "race, color, religion, national origin, sexual orientation, gender, gender identity, or disability" (US Dep. Justice, 2023). The majority of hate crimes are perpetrated on the basis of the victim's race (FBI, 2023). While they resemble other forms of offensive behavior in some respects, hate crimes differ in their origins in prejudice, identity, and societal attitudes (Rose & Mechanic, 2002; Lockwood & Cuevas, 2022), and in their societal implications.

To classify reported crime incidents as hate crimes, the FBI employs a two-tier decision-making process. First, the responding law enforcement officer indicates whether the offender was bias-motivated, tagging the incident as a suspected bias crime. Then a second-level judgment officer reviews the facts and makes the final determination of a hate crime occurrence. Most US states have enacted hate crime laws stipulating increased penalties or sentence enhancements for bias-motivated crimes, due to their serious consequences.

Hate crimes harm not only the victim but also the surrounding community. Victims often grapple with emotional distress, manifesting as anxiety, depression, anger, fear, and even post-traumatic stress disorder (Herek et al., 1999; McDevitt et al., 2001; Dustmann & Fasani, 2016). Social repercussions include isolation, stigma, and diminished trust in communities (Perry, 2001; Iganski, 2001). At the neighborhood level, hate crimes can erode trust, amplify social tension (Green et al., 1998), undermine social cohesion (Lyons, 2007), and diminish community solidarity (Paterson et al., 2019). Thus, they not only inflict harm on the immediate victim but can also incite retaliation, escalate communal tension, and reverberate adverse effects both within and beyond the immediate locality. These crimes have the potential to destabilize neighborhoods and disrupt societal harmony.

The distinct nature of hate crimes is recognized within the US legal framework. Both federal and state laws (in the majority of states) have instituted heightened penalties or sentence enhancements for crimes demonstrably motivated by bias (US Dep. Justice, 2023). One example is the Matthew Shepard and James Byrd Jr. Hate Crimes Prevention Act of 2009, which expanded the jurisdiction for prosecuting hate crimes and introduced protections against other forms of bias-motivated violence. Alongside these legal measures, research has examined what drives hate crime.

Theories on hate crimes recognize their complexity and multidimensional nature, spanning disciplines from psychology, sociology and economics. Integrated threat theory (Stephan et al., 2000) posits that hate crimes emerge from perceived threats from out-group members. This is complemented by social identity theory (Tajfel & Turner, 1979), which points to in-group favoritism and out-group discrimination as precursors to hate crimes. The ethnic competition theory (Scheepers et al., 2002) emphasizes the role of aggression arising from economic competition between distinct groups. Lastly, relative deprivation theory (Walker & Smith, 2002) suggests that individuals perceiving themselves as unfairly disadvantaged may be inclined to commit hate crimes against those they deem more privileged.

Empirical studies offer concrete insights into the causes and consequences of hate crime. As for causes, backlash to terror attacks, entertainment media, and social media have been identified as contributing factors. Gould and Klor (2016) show that the 9/11 terrorist attacks led to anti-Muslim backlash, resulting in hate crimes against Muslim communities. In a similar vein, Riaz et al. (2024) document that hate crimes against refugees rise sharply in the immediate aftermath of an immigrant-attributed crime event in a local community. In turn, immigrant communities assimilate less well and turn to more traditional values. With the increase in racist attacks on the Asian community during the COVID-19 pandemic, Agarwal et al. (2026) document a reduction in female fund managers' productivity for those perceived as of East Asian origin. Müller and Schwarz (2021, 2023) find that social media can propagate and amplify xenophobia, leading to spikes in hate crimes against minorities. Using data on historical screenings of entertainment media depicting racial stereotypes, Ang (2023) documents significant effects on lynchings, race riots and modern-day hate crimes.

Despite the distinct nature of hate crimes and their detrimental effects, the impact of hate crimes on mortgage credit and housing demand has received relatively little attention. We aim to fill this gap by examining how the threat of hate crimes shapes households' mortgage borrowing and local housing-market behavior across both minority and nonminority groups, contributing to the growing literature on the economic consequences of hate crime.

2.2. Hate crime data and its characteristics

We use hate crime data from the FBI Uniform Crime Reporting (UCR) program. The dataset provides incident-level information, including whether the offenders are motivated by their bias against the victim's perceived race, gender, gender identity, religion, disability, sexual orientation, or ethnicity. Law enforcement agencies report hate crime incidents to the UCR program, whether they submit through the Summary Reporting System (SRS) or National Incident-Based Reporting System (NIBRS). Therefore, UCR's Hate Crime Statistics combine incidents reported in the two sources and capture all reported hate crime occurrences.

Hate crimes are classified into racial hate crimes, sex- and gender-based hate crimes, and religious hate crimes, with racial hate crimes predominating. Our primary analysis centers on racial hate crimes, given the focus of the research. We consider a hate crime to be a racial hate crime if its bias motivation is racially or ethnically based. Accordingly, we observe the following race/ethnicity/ancestry motivations in our data: anti-Black or African American, anti-White, anti-Hispanic or Latino, anti-other race/ethnicity/ancestry, anti-Asian, anti-multiple races, anti-American Indian or Alaska Native, anti-Arab, and anti-Native Hawaiian or other Pacific Islander.[1]

To illustrate the distinct nature of hate crimes as outlined in Section 2.1, we compute the frequency and nature of hate crimes versus other crimes and provide the results in Table 1. The underlying offense types between hate crimes and other crimes differ substantially. While larceny/theft offenses dominate non-hate crimes, accounting for more than 30% of incidents and typically representing opportunistic, financially motivated behavior, the most frequent hate crimes follow a different pattern. The three most common hate crime categories are property destruction (25.16%), intimidation (22.79%), and simple assault (20.38%), which occur at similar frequencies and suggest a systematic targeting approach combining property damage with personal victimization.

This clustering of property and personal attacks in hate crimes stands in contrast with non-hate crimes, where property and personal offenses show clearer separation. Non-hate crimes display a broader distribution after larceny (30.02%), with property damage, assault, and drug offenses each comprising 12-13% of incidents, followed by burglary and fraud offenses in the 5-9% range. This pattern reinforces that non-hate crimes are often motivated by financial gain rather than targeting specific groups.

[1] We report the frequencies of these various bias motivations in Table A1 of the online Appendix.

To illustrate the evolution of hate crime incidents over time, Figure 1 plots the three main types of hate crime reported (i.e., racial, sex- and gender-based, and religious hate crimes), measured as incidents per 1,000 total crimes, from 1994 to 2020. Compared to racial hate crimes, the other two types play a smaller role. However, all hate crimes share a similar trend over time. A small peak occurs in 2001, reflecting the surge in hate crimes after the 9/11 terrorist attacks. Reported hate crimes trended downward through most of the Obama administration and then increased after the 2016 election, reaching a new historical high during the subsequent term.

For our empirical analysis, we aggregate hate crime data to the county level and use the natural log of one plus the racial hate crime count as the main explanatory variable. This transformation retains county-year observations with zero reported incidents (which map to zero in the transformed variable) while compressing the right tail of the distribution. Given that the mean number of racial hate crimes per county-year is approximately 2.27 in levels, a non-trivial fraction of county-years records no incidents, so retaining zeros is important for the representativeness of the panel. To avoid potential measurement issues due to some states not mandating reporting through their own hate crime laws, we exclude all counties in Arkansas, South Carolina, and Wyoming, which have never reported hate crimes throughout our sample period. A concern with UCR hate crime data is that reporting practices vary across jurisdictions and over time: agencies differ in whether and how they record bias motivation, and transitions between the SRS and NIBRS, the adoption of bias-crime training, and local policy responses can all shift measured incidence independently of underlying behavior. Our fixed-effects structure addresses two margins of this heterogeneity: county fixed effects absorb time-invariant differences in reporting propensity across jurisdictions, and year fixed effects capture common nationwide shifts in reporting standards. Idiosyncratic within-county changes in reporting effort are not absorbed by these fixed effects and remain a caveat on the estimates below.

2.3. Mortgage data

We retrieve information from the Home Mortgage Disclosure Act (HMDA) database on the near-universe of US mortgage applications, including lender identification, loan amount, purpose of the loan, status, location, as well as borrowers' personal information. To account for atypical loan and lender patterns, following Dagher and Kazimov (2015), we exclude loans below $25,000 and above $1 million and exclude inactive lenders that originated fewer than fifty mortgage loans in any given year. Our sample of mortgages comprises the majority of the US mortgage market (e.g., home purchases, home improvements, refinancing), covering 5,387 lenders during the sample period of 2009–2020.

For a given application, we observe the race and ethnicity of the applicant(s). Following Bhutta et al. (2017), if a (co-)applicant reports two races and one is White, that (co-)applicant is categorized under the minority race. Otherwise, (co-)applicants are categorized under the first race and ethnicity reported. Based on the applicant's and co-applicant's race and ethnicity, we categorize each application into one of six mutually exclusive and exhaustive groups (Unknown, Black, Asian, Hispanic, White, Other), following the methodology of Gerardi et al. (2021). The Unknown group captures applications for which race or ethnicity is not reported by the applicant or not recorded by the lender; we retain it as a separate category throughout rather than imputing missing race using BISG or similar methods. The White group produces the largest number of mortgage applications, with 7.12 million on average per year, followed by Hispanic applicants (909,707 applications) and Black applicants (729,471 applications). To observe the mortgage demand by each group for a county-lender combination, we then aggregate the data to the county-lender level and construct variables for each race/ethnicity group, resulting in 3,927,845 observations for our main dataset for which the summary statistics are reported in Table 2.

2.4. Additional data: surveys, geographic, and banking controls

Survey data. To explore the county-level and individual-level effects of hate crime, we utilize survey information from the Panel Study of Income Dynamics (PSID) and the Consumer Expenditure Survey (CEX).

The PSID surveys are conducted once every two years, with a nationally representative sample of households. The survey provides information on household income, wealth, education, and household demographics. We use information on respondents' psychological distress to study the potential heightened feeling of vulnerability and fear in affected neighborhoods. The detailed variable definitions can be found in Appendix A. Since the month of survey is disclosed in the data, we construct a monthly dataset for a more granular assessment of the effects. As shown in the summary statistics, nearly 15% of respondents are psychologically distressed to various degrees.

The CEX program is administered by the US Bureau of Labor Statistics to provide data on expenditures, income, and demographic characteristics of consumers in the US. To understand the role of fear in relation to hate crime and mortgage applications, we focus on the consumers' visible spending on clothing and jewelry, since the literature has shown that spending on such goods is negatively related to fear caused by crimes (Mejía & Restrepo, 2016). The exact items used to construct the variable are reported in the Appendix. The CEX has introduced state-level data for five states, namely California, Florida, New York, Texas, and New Jersey, covering population areas amounting to 36% of the US population. The data are measured at the quarterly frequency. In the summary statistics, we observe that households spend negligible amounts on clothing and jewelry at the 10th percentile or as much as $520 at the 90th percentile, during the past quarter.

Other geographic data. We gather additional geographic data from various sources. We collect the general crime data from the SRS to control for the overall crime rate in different regions. These data are at the state level.[2] In addition, we obtain (minority) population data from the Census Bureau; unemployment data from Local Area Unemployment Statistics published by the Bureau of Labor Statistics; GDP, per capita personal income from the Bureau of Economic Analysis; poverty percentage data from the Small Area Income and Poverty Estimates Program; housing price and rent data from Zillow; and home sales and loan-to-value ratio data from CoreLogic. The summary statistics of the geographic variables in Table 2 show that our sample covers counties with a wide range of sizes, levels of economic development, housing costs and crime.

Banking supply controls. To account for local credit supply conditions, we draw on bank balance sheet data from the Federal Deposit Insurance Corporation's (FDIC) Statistics on Depository Institutions (SDI) and Summary of Deposits (SOD). For each county-year, we construct deposit-weighted averages of four indicators of bank financial health: the total risk-based capital ratio, return on assets, non-current loan ratio, and loans-to-deposits ratio. The deposit weighting uses each institution's share of county-level deposits from the SOD, ensuring that institutions with larger local deposit footprints receive proportionally greater weight. These variables proxy for the lending capacity and balance sheet strength of the local banking sector, allowing us to condition on time-varying supply-side conditions that might independently affect credit availability. Including these controls helps distinguish the demand-side response to hate crime from shifts in local credit supply driven by variation in bank financial health.

[2] NIBRS also gathers general crime data that can be aggregated to the county level. However, most US law enforcement agencies did not submit data to NIBRS before 2020. Therefore, during our sample period, NIBRS covers less than one third of the population. So, we use the SRS crime rate since it is nationally representative. However, we utilize the NIBRS data to conduct additional subsample analysis.

3. Racial hate crime and mortgage demand

3.1. Baseline panel estimates

We begin by examining the relationship between racial hate crime and local mortgage demand using the full set of county-level incidents reported in the FBI's Uniform Crime Reporting (UCR) Hate Crime Statistics. The empirical model is estimated at the lender–county–year level. We choose this unit of analysis for a specific econometric reason: it permits us to include lender–year fixed effects ($\lambda_{l,t}$), which absorb all time-varying, lender-specific factors such as changes in underwriting standards, lending capacity, or branch-network strategies. Without this fixed-effects structure, one could not distinguish demand-side from supply-side responses to hate crime. Since multiple lenders operate in each county-year, observations within a county-year share the same hate-crime value; we account for this by clustering standard errors at the county level. The specification is:

$$\begin{aligned} M_{l,c,t} &= \alpha_c + \lambda_{l,t} + \gamma \log\bigl(1 + \text{Racial Hate Crime}_{c,t-1}\bigr) \\ &\quad + X'_{c,t-1}\theta + B'_{c,t-1}\mu + \varepsilon_{l,c,t}, \end{aligned} \tag{1}$$

where $M_{l,c,t}$ represents mortgage credit demand, measured as the log of applications received by lender l in county c in year t. The primary explanatory variable of interest is $\log(1 + \textit{Racial Hate Crime})_{c,t-1}$, the log of one plus the number of racial hate crime incidents in the preceding year. This transformation retains county-years with zero incidents while compressing the skewed upper tail of the distribution. Lagging by one period accounts for the potential delayed effects of exposure on mortgage demand. $X_{c,t-1}$ is a vector of geographic controls including population, GDP growth, personal income, unemployment, poverty, housing prices, the state crime rate, and the population shares of Black, Asian, Hispanic, White, Native American, Native Hawaiian or Other Pacific Islander, and other groups. $B_{c,t-1}$ is a vector of banking-supply controls including the deposit-weighted bank capital ratio, return on assets, non-current loan ratio, and loan-to-deposit ratio. Definitions of all variables are provided in the Appendix. County fixed effects (α_c) capture time-invariant local characteristics, while lender–year fixed effects ($\lambda_{l,t}$) absorb lender-wide, time-varying supply conditions. Banks can steer mortgage contract choices through non-price supply channels (Foà et al., 2019). Fintech originators adjust supply more elastically and process applications faster (Fuster et al., 2019), reinforcing the need to saturate our models with lender–year fixed effects to net out time-varying supply innovations. Supply-side frictions can meaningfully distort household mortgage choices and pricing, especially in concentrated or vertically integrated markets (Doerr & Fuster, 2025). We therefore absorb lender–year fixed effects to purge time-varying lender supply and isolate the demand response to racial hate crime. This saturated specification is central to identification, as it disentangles demand-side responses from lender-specific time-varying factors such as changes in lending policies or conditions. Standard errors are clustered at the county level. In alternative specifications, we also replace lender–year fixed effects with lender and year fixed effects separately.

Table 3 presents the baseline evidence. Column (1) reports results from the most saturated and preferred specification, which includes both county and lender–year fixed effects. The coefficient on racial hate crime is negative and highly significant. To gauge the economic salience of the result, we evaluate the effect of one additional racial hate crime incident at the sample mean (2.27 incidents): this corresponds to a 0.11% decline in mortgage applications per lender in a county-year. Aggregating across lenders, the county-year specification in Appendix Table A4 delivers point estimates of −0.2% to −0.6%[3] per additional incident at the sample mean across both applications and originations, which is the natural benchmark for county-level implications. We read $\hat{\gamma}$ as a steady-state semi-elasticity: it captures how annual mortgage activity within a county covaries with the prevailing level of local hate crime once households have had time to incorporate that level into their location and tenure de-

[3]Computed as $\hat{b} \times \log(4.27/3.27) \approx 0.267\,\hat{b}$, where $\hat{b}$ is the race-specific coefficient on Racial Hate Crime in Table A4 (Panels A and B: −0.006 to −0.024), and $\log(4.27/3.27)$ is the change in the regressor from one additional incident at the sample mean of 2.27.

cisions. Column (2) relaxes the fixed-effects structure by replacing lender–year fixed effects with lender and year fixed effects separately; Column (3) then drops lender fixed effects, retaining only year fixed effects. The coefficients remain negative, with magnitudes very similar across specifications. The stability of the results shows that the findings are robust to alternative modeling choices.[4]

These baseline estimates raise the natural question of whether the impact of hate crimes is larger in economic terms and more pronounced in significance than that of other crimes not driven by prejudice. A larger influence of hate crimes would speak to their distinct nature, as outlined in Section 2.1. At the same time, comparing the effect of hate crimes to other crimes addresses the concern that the observed decline in mortgage demand may reflect a general aversion to crime rather than the specific effect of hate-motivated incidents. To address this, Panel B uses detailed offense-level data from the National Incident-Based Reporting System (NIBRS), which provides a subsample of data with both county-level crime and hate crime incidents. We estimate regressions separately by offense type. Model I includes total crime incidents, while Model II adds racial hate crime incidents as a distinct category. Both models include county controls, county fixed effects, and lender–year fixed effects, as well as banking supply controls.

The comparison highlights a contrast. For most offense types, the coefficients on overall crime are small and statistically indistinguishable from zero, whereas the coefficients on racial hate crime are negative and statistically significant. For instance, property destruction, intimidation, burglary, and drug/narcotic offenses exhibit strong negative associations with mortgage applications when classified as racial hate crimes, but no discernible effect when measured as overall crime. In aggregate, overall crime is negatively associated with mortgage demand, but this effect is subsumed once racial hate crimes are introduced separately in Model II.

[4] We also restrict the sample of mortgages to home purchases only and find comparable results (see online Appendix Table A2). It confirms that the observed effects are not driven by changes in borrowing behavior for reasons unrelated to property acquisition, such as refinancing or equity withdrawal.

These patterns emphasize that the observed decline in mortgage applications is associated with racial hate crimes specifically, not with crime in general. Hate crimes often employ property destruction or intimidation as an end goal, rather than as a means to financial gain. This symbolic and targeted nature may help explain their sharper impact on household financial behavior relative to financially motivated crimes. Taken together, the results show the distinct salience of racial hate crimes. Whereas ordinary crime has limited or inconsistent effects on mortgage demand, hate crimes generate persistent and statistically significant reductions.

3.2. Targeted-threat and generalized-fear channels

Racial hate crimes have been characterized as "message crimes" (Perry, 2001; Perry & Alvi, 2012): under this view, unlike ordinary crime that signals financial risk to any potential victim, a racially motivated incident communicates to members of the targeted group that they are unwelcome in a location. The group-specific signal would then reach beyond the immediate victim to any co-ethnic household that learns of the incident.

Because a mortgage is a long-duration, geographically fixed commitment, a household's willingness to apply depends on the anticipated safety and stability of the location. The targeted threat channel operates through direct group exposure: hate crimes against a group's own race raise the subjective probability of future victimization for co-ethnic households, increasing the option value of waiting or relocating, which depresses mortgage applications and raises post-application withdrawals. The intensity of this effect depends on how salient and credible the threat signal is to co-ethnic households, which in turn depends on local social visibility and community networks.

The targeted-threat channel alone, however, cannot explain why majority-group households also reduce mortgage demand in response to hate crimes directed at minorities.[5] The

[5] Appendix Table A4 confirms this at the county-year level: aggregating across all lenders and replacing lender–year fixed effects with county and year fixed effects, total racial hate crime reduces applications and originations across racial groups, including White households, consistent with effects that cross racial lines.

generalized fear channel fills this gap: a hate crime is a publicly observable event that signals the presence of inter-group hostility in a neighborhood, raising uncertainty about the long-term safety and stability of that community and reducing expected returns to residential investment for all residents regardless of race. Since hate crimes against other groups carry no direct victimization risk for a given household, their deterrent effect must work through this broader signal about neighborhood desirability.

We examine both channels simultaneously in Table 4 by decomposing total racial hate crime into two regressors for each group-specific column: hate crimes against that group's own race (hate crime against own race) and hate crimes against all other racial groups (hate crime against other races, i.e., total racial hate crimes minus hate crime against own race). Formally, for each borrower racial group $r \in \{\text{Black, Asian, Hispanic, White}\}$, we estimate

$$\begin{aligned} M^r_{l,c,t} = \alpha_c + \lambda_{l,t} + \gamma_{\text{own}} \log\big(1 + \text{Hate Crime against Own Race}^r_{c,t-1}\big) \\ + \gamma_{\text{other}} \log\big(1 + \text{Hate Crime against Other Races}^r_{c,t-1}\big) \\ + X'_{c,t-1}\theta + B'_{c,t-1}\mu + \varepsilon^r_{l,c,t}, \end{aligned} \tag{2}$$

where $M^r_{l,c,t}$ is the log number of mortgage applications by race-r applicants for Panel A and the log number of withdrawals for Panel B, with the hate crime measures dated t rather than $t-1$ in Panel B. The coefficient γ_{own} captures the targeted threat channel and γ_{other} the generalized fear channel.

The application results in Panel A reveal both channels operating simultaneously, with heterogeneity across groups. Hispanic borrowers face the clearest targeted threat: own-group hate crimes reduce their applications, indicating that group-specific incidents generate direct demand deterrence. The generalized fear channel is also negative and significant for Hispanic borrowers, reflecting that the broader hostile racial climate of a county depresses their applications. White borrowers are not the primary target of anti-minority hate crimes, and the targeted channel is correspondingly absent. The generalized fear coefficient for White applicants, by contrast, is negative and precisely estimated: White households contract mortgage demand in response to hate crimes against other races, consistent with all residents responding to signals of neighborhood hostility regardless of who is directly targeted. Asian borrowers show a similar pattern: no significant targeted effect but a borderline generalized fear response. Black applicants exhibit a targeted threat effect that is statistically significant but smaller in magnitude than Hispanic borrowers', with no significant generalized fear response on the application margin.

Panel B reports loan withdrawals. The targeted-threat coefficient is positive and significant for Asian borrowers, and generalized-fear coefficients are positive and significant for Black and Asian borrowers. Hispanic and White withdrawal rates show no response to either regressor. The point estimates are jointly consistent with both channels operating, with the significance pattern varying across group–outcome cells.

Taken together, the point estimates are consistent with both channels operating. Own-race hate crime coefficients are negative and significant for Black and Hispanic applicants and positive and significant for Asian withdrawals; other-race hate crime coefficients are negative and significant for White, Hispanic, and Asian applicants. We read this as evidence that the targeted-threat and generalized-fear channels are both present in the data. The pattern is hard to reconcile with a purely supply-side interpretation operating through denial rates: if lenders were tightening credit standards, denial rates would rise rather than fall (we test this directly in Section 5.1), and the pattern would not differ systematically across the two regressors.

We complement these group-level patterns with individual-level survey evidence that fear operates at the household level. If hate crimes raise perceived threat and erode social cohesion, we expect measurable signatures in household behavior beyond mortgage choices: heightened psychological distress and retrenchment in socially visible consumption. We examine both using individual-level survey data.

The empirical exploration relies on the psychological distress measurement used in the psychology and mental health literature, where survey respondents who score above 12 on the K-6 Non-Specific Psychological Distress Scale are considered psychologically distressed (Kessler et al., 2002). We measure psychological distress using respondent-level data from the Panel Study of Income Dynamics (PSID) and link it to racial hate crime incidents reported in the 12 months before the survey interview month. Second, for understanding consumption behavior patterns, we turn to the quarterly Consumer Expenditure Survey (CEX) and examine expenditures on a category often associated with conspicuous consumption: clothing and jewelry. We expect households to reduce their consumption of visible goods in locations affected by hate crimes (Mejía & Restrepo, 2016). For this test, we evaluate racial hate crimes that have occurred in the quarter prior to the reference quarter for which expenditures are measured. The rationale is that if racial hate crimes instill fear in individuals, we should observe some effects on the behavior of conspicuous consumption.

To evaluate the individual-level effects of racial hate crimes, we estimate the following regression equation:

$$\text{Behavior}_{i,t} = \alpha_{s(i)} + \gamma \, \log\big(1 + \text{Racial Hate Crime}_{s(i),t-1}\big) + I'_{i,t-1}\theta + \varepsilon_{i,t} \tag{3}$$

where the dependent variable $Behavior_{i,t}$ represents either the measure of psychological distress or a measure of conspicuous consumption for individual i at time t. The primary explanatory variable is the past number of racial hate crime incidents, as discussed above. The vector $I_{i,t-1}$ contains individual-specific demographic and socioeconomic controls such as age, education, family size, employment status, marital status, income, and family wealth, where available in the surveys. We also control for unobserved time-invariant characteristics at the state level by including state fixed effects, denoted by $\alpha_{s(i)}$, where $s(i)$ is the state of individual i to which the hate crime regressor is matched.

The results of these analyses are presented in Table 5. In Panel A the dependent variable is a binary indicator equal to one if the respondent scores above 12 on the K-6 scale, so coefficients are percentage-point changes in the probability of being psychologically distressed for a one-unit increase in the log of one plus the racial hate crime count. The estimates are positive and statistically significant for Black, Hispanic, and White respondents, consistent with hate crimes raising the incidence of psychological distress. In Panel B the dependent variable is the log of household expenditure on clothing and jewelry, so coefficients approximate the proportional change in spending for a one-unit increase in the log of one plus the racial hate crime count. The coefficients are negative and statistically significant across all groups, consistent with households reducing visible consumption when local racial hate crimes rise.

The PSID and CEX specifications rely on state fixed effects and pool individuals across the cross-section. We read these results as descriptive, individual-level evidence consistent with the fear channel we document at the county level. Historical evidence shows that racially targeted persuasion erodes trust and steers minorities toward inferior financial products (Célérier & Tak, 2025). County-level social capital lowers the cost of debt and loosens nonprice loan terms (Hasan et al., 2017); hate crimes erode precisely this social capital, discouraging mortgage market participation.

In summary, the regression results reported in Table 5 suggest that racial hate crimes not only erode neighborhood cohesion and desirability, but also instill fear in residents, leading to increased psychological distress and altered consumption patterns. These findings imply that mortgage application declines are not merely a matter of minorities withdrawing because they expect discrimination, but may instead reflect a broader decline in local confidence. In turn, these effects likely contribute to the observed decrease in mortgage demand, consistent with a behavioral response to heightened insecurity and fear.

3.3. Aggregated downstream effects

The preceding analysis documents a contraction in mortgage demand following racial hate crimes. In this section, we examine whether this translates into broader effects on local housing markets. We study three neighborhood-level outcomes: rental prices, home sales growth, and house price growth. Together, these outcomes provide evidence on downstream consequences for neighborhood property markets consistent with weaker owner-occupied demand. As equilibrium outcomes, rents, sales, and prices cannot directly identify a tenure-choice response.

For the housing market analysis, we retrieve rental and house prices from Zillow, a leading provider of real estate and rental marketplace data. Information on home sales volumes is obtained from CoreLogic. The empirical model takes the following form:

$$\text{Outcome}_{c,t} = \alpha_c + \tau_t + \gamma \, \log\big(1 + \text{Racial Hate Crime}_{c,t-1}\big) + X'_{c,t-1}\theta + B'_{c,t-1}\mu + \varepsilon_{c,t}, \quad (4)$$

where $\text{Outcome}_{c,t}$ for county c in year t is defined below. The key explanatory variable is the log of racial hate crime for a given county in the year prior to the current period, denoted by $\mathit{Racial\,Hate\,Crime}_{c,t-1}$. The vectors $X_{c,t-1}$ and $B_{c,t-1}$ stand for geographic and banking-supply control variables respectively. As with our earlier models, α_c and τ_t represent county fixed effects and year fixed effects, respectively, and standard errors are clustered at the county level.

Results are reported in Table 6. Column (1) shows no detectable effect of racial hate crimes on rental prices. The rental specification is estimated on a much smaller sample (roughly 3,000 county-year observations) than the sales and price specifications (over 26,000), reflecting more limited Zillow rent coverage; rents are also a general-equilibrium price, so that if hate crimes deter both in-migration and out-migration, demand and supply effects partly offset in the rental market. Reading the null with these features in mind, the pattern is consistent with effects concentrated in the owner-occupied segment. If renters transitioning into ownership were the primary margin deterred, those households would remain as renters, exerting upward pressure on rental demand and prices (Gete & Reher, 2018); the absence of such upward pressure is suggestive of an intensive-margin effect on homeownership.

Turning to the owner-occupied segment, Columns (2)–(3) show that hate crimes weaken housing markets along both the extensive and intensive margins. On the extensive margin, higher racial hate crime exposure significantly reduces the growth of home sales (Column 2). On the intensive margin, house price growth also slows (Column 3). These results indicate that hate crimes reduce transaction activity and slow local house price appreciation.

Overall, racial hate crimes are associated with weaker local housing markets, reflected in lower transaction volumes and slower house price growth. The absence of a rental market response is consistent with the association being concentrated in the owner-occupied segment of the market.

4. Difference-in-differences: the George Floyd murder

We next exploit cross-county heterogeneity in the intensity of the racial hate crime backlash that followed the murder of George Floyd (GFM) as a complementary source of variation. George Floyd was killed by a police officer in Minneapolis on May 25, 2020. Widely circulated video footage of the killing led to nationwide protests under the Black Lives Matter (BLM) banner and a backlash against minorities marked by surges in racial hate crimes. That triggering events produce surges in intergroup hostility is well documented (Gould & Klor, 2016; Frey, 2020).

The two designs identify related but distinct estimands. The panel semi-elasticity in Section 3 is a steady-state object: it captures how annual mortgage activity within a county covaries with the prevailing level of local hate crime, reflecting an equilibrium in which households have had time to incorporate that level into their location and tenure decisions. The design below isolates a shorter-horizon response: the monthly change in purchase applications following an unanticipated surge that disrupts this equilibrium.

In this analysis we use monthly county-level data. The HMDA data, aggregated to the county-month level for the 589 largest counties by mortgage volume, come from Bhutta (2026) and cover January 2005 to December 2022. Panel (a) of Figure 2 shows the post-GFM hate crime surge alongside BLM protest activity, and to attenuate concerns about confounds, marks several salient events in the period. We identify counties that experienced a large surge in racial hate crimes after May 2020 and classify them as treated. We then use the resulting spatial heterogeneity in a difference-in-differences design, comparing mortgage market outcomes in treated counties with those that did not experience a surge.

Specifically, treatment is a binary indicator equal to one for counties recording three or more racial hate crime incidents in any single month within the six-month window following the GFM (May–October 2020). Restricting to this window ensures that treated counties are those whose hate crime surge was directly tied to the event and captures the backlash that followed it. The cutoff of three is motivated by the peak-month distribution of racial hate crimes in the window: shares taper sharply from 26.5% at a peak of one and 10.5% at a peak of two, then stay in a narrow 2.4–6.4% band from a peak of three upwards (Appendix Table A3). This similarity in shares from three onwards suggests a common high-intensity regime, distinct from counties with isolated one- or two-incident months. This yields 143 treated counties and 446 control counties that never reach the threshold in the six-month window. Treated counties tend to be larger and more ethnically diverse, consistent with hate crimes being more frequently recorded in urban areas. County fixed effects absorb these level differences; the identifying assumption requires only that trends in mortgage demand would have been parallel absent the surge, which we assess below.

We address two identification concerns. First, BLM protests could directly depress mortgage demand. This is unlikely: 93% of protests were peaceful (Armed Conflict Location & Event Data Project, 2020), and our Census division $\times$ month fixed effects absorb any regional protest shock common to treated and control counties within the same division. Second, protests could inflate hate crime reporting rather than actual incidence, making treatment assignment a reporting artifact. The data do not support this. Between May and June 2020, mean monthly protests per county rose by 8.9 in treated counties and 2.8 in control counties, yet mean monthly hate crimes rose by 2.8 and 0.2, respectively. This yields 0.31 additional hate crimes per additional protest in treated counties versus 0.07 in control counties, a fivefold difference. A simple reporting-only channel would produce similar ratios in both groups, so the gap is evidence against such an account.

Following the forward-engineering approach of Baker et al. (2025), we begin by defining our estimand. For each event-time e relative to the GFM, we estimate the average treatment effect on treated counties. Because treatment is concentrated in large, diverse, urban counties, the estimand is local to that population:

$$ATT(e) \;=\; \mathbb{E}\big[M_{c,\,g+e}(g) \;-\; M_{c,\,g+e}(\infty) \;\big|\; c \in \mathcal{T}\big], \tag{5}$$

where $M_{c,t}$ is the log number of purchase mortgage applications in county c in month t, g is the treatment month (May 2020 for all treated counties), $M_{c,\,g+e}(g)$ is the potential outcome under treatment at g, $M_{c,\,g+e}(\infty)$ the counterfactual absent treatment, and $\mathcal{T}$ denotes the set of treated counties. That is, $ATT(e)$ compares actual mortgage demand e months after the hate crime surge with what demand would have been had the surge not occurred. We focus on purchase applications because they directly reflect households' decisions about where to live.

We implement the imputation estimator of Borusyak et al. (2024) (henceforth BJS), which constructs an explicit counterfactual for each treated county and compares it to the observed outcome. This provides a direct estimate of $ATT(e)$ in Equation (5). Although all treated counties share the same treatment date, a conventional two-way fixed-effects (TWFE) specification would estimate county and time fixed effects using both pre- and post-treatment observations, so that post-treatment outcomes of treated counties feed back into the fixed-effect estimates used to construct the counterfactual. The BJS estimator avoids this by fitting the outcome model on untreated observations only.

The procedure has two stages. In the first stage, we estimate an outcome model using only not-yet-treated and never-treated observations:

$$M_{c,t} \;=\; \alpha_c \;+\; \eta_{d(c)\times t} \;+\; \varepsilon_{c,t}, \tag{6}$$

where α_c is a county fixed effect and $\eta_{d(c)\times t}$ is a Census division $\times$ month fixed effect. The latter absorbs region-specific time-varying conditions (e.g., differential COVID-related disruptions, local housing cycles, and regional mortgage market trends) so that identification comes from comparing treated and control counties within the same Census division and month. This makes the parallel trends assumption considerably more plausible.

In the second stage, we use the estimated parameters to impute each treated county's counterfactual outcome $\widehat{M}_{c,\,g+e}(\infty)$ and recover the treatment effect at each horizon as:

$$\hat{\tau}_e \;=\; \frac{1}{N_e}\sum_{c\,\in\,\mathcal{T}_e}\big[\,M_{c,\,g+e} \;-\; \widehat{M}_{c,\,g+e}(\infty)\,\big], \tag{7}$$

where the sum is over the N_e treated counties observed at event-time e. The post-event coefficients are obtained from this imputation procedure. The pre-event coefficients are estimated separately, by augmenting Equation (6) with event-time indicators for periods before the treatment onset. Both the imputation and the pre-trends test use only observations that are untreated at the time of observation, which includes pre-May-2020 observations from eventually treated counties. Standard errors are clustered at the state level (51 clusters).

The design requires two assumptions. First, absent the hate crime surge, purchase applications in treated and control counties within the same Census division would have followed the same path. The pre-event coefficients in Panel (c) of Figure 2, whose confidence intervals include zero and show no visible pre-trend, support this. Appendix Figure A1 extends the test to a 24-month pre-event window; none of the pre-event coefficients is statistically significant. Second, mortgage demand did not respond in anticipation of the hate crime surge. This is plausible because the surge was triggered by the GFM, whose timing was unforeseeable, and the intensity of the local backlash could not have been predicted in advance. Panel (a) also marks other salient events in this period (COVID lockdowns and the 2020 presidential election), none of which produce visible shifts in hate crime patterns for treated relative to control counties, suggesting they are not material confounds in our setting. A separate concern is that social media amplification of the event drives the mortgage response (Müller & Schwarz, 2021). We address this directly in Section 5.2, where controlling for local news coverage leaves the hate crime coefficient unchanged.

Panel (b) of Figure 2 plots mean log purchase applications in the 143 treated counties alongside the BJS-imputed counterfactual. The two series track closely in the pre-period, validating the counterfactual imputation, but diverge after May 2020, with observed applications falling below the counterfactual.

Panel (c) plots the horizon-specific coefficients $\hat{\tau}_e$ over a one-year window around the GFM. Before the event, pre-treatment coefficients are indistinguishable from zero, consistent with parallel trends. After May 2020, mortgage demand in treated counties declines relative to the counterfactual and remains depressed over the following six months. The estimates imply that treated counties recorded between 4.8% and 8.5% fewer purchase mortgage applications in the months immediately following the event, relative to the BJS imputed counterfactual. At the treated-county pre-GFM mean of 1,400 monthly purchase applications, this corresponds to between 67 and 119 fewer applications per county per month. As hate crime levels gradually return toward their pre-GFM baseline, the effect attenuates and becomes statistically insignificant by the end of the window. This pattern is consistent with households responding to the local salience of racial violence: mortgage demand is depressed while hate crimes are elevated, and recovers as conditions normalize.

We now compare the size of the hate crime shock with the annual variation that identifies Table 3. In the 12 months before the GFM, treated counties averaged 1.7 racial hate crime incidents per month. This rose to 5.4 in June 2020, declined to 2.8 by October, and returned close to pre-event levels by December. Over the seven months in the post-GFM period ($t = 0, 1, ..., 6$), treated counties accumulated 11.3 additional incidents relative to their own pre-period. Scaling the baseline semi-elasticity accordingly (11.3 extra incidents $\times$ 0.11%) implies a decline of 1.2%. The DiD estimates of 4.8–8.5% are larger by a factor of 4.0 to 7.1. This scaling is a rough calibration rather than a like-for-like benchmark, since the two designs differ in horizon (annual vs. monthly), outcome definition (all applications vs. purchase applications), and source of variation.

The gap between the two magnitudes is mostly a difference in estimands. The panel semi-elasticity captures how annual mortgage activity within a county covaries with prevailing local hate-crime levels, reflecting an equilibrium in which households have had time to incorporate those levels into their location and tenure decisions. The DiD isolates the monthly response to an unanticipated surge that disrupts this equilibrium, identified from the unexpected component of the shock in counties whose hate-crime activity rose sharply. The 4.8–8.5% DiD estimate and the 1.2% panel-implied decline therefore measure related but distinct household responses; the numerical gap follows from this difference in what each design identifies.

Having argued against a direct effect of protests and a simple reporting channel, three more factors contribute to the gap. First, concentrated hate crime surges may produce a stronger household response per incident than the same number of incidents spread across a year. Second, the DiD identifies an average treatment effect on the treated, and treated counties are larger, more diverse, and have deeper mortgage markets. If such counties are more responsive to hate crime, the ATT would exceed the average effect estimated in the cross-section. Third, the two designs do not measure the same outcome: the baseline uses all mortgage applications, whereas the DiD uses purchase applications only, which more directly reflect location choice and may therefore be more sensitive to local hate crime.

5. Additional analysis

In this section, we first consider the importance of the supply-side effects, namely whether lenders tighten standards in response to hate crime and contribute to the observed contraction. Second, we examine whether the demand-side effect is mediated by local news coverage rather than the incidents themselves.

5.1. Supply-side discrimination

A natural concern is whether a supply-side story contributes, specifically whether lenders respond to heightened racial hostility by tightening underwriting standards or differentially denying minority applicants. If lender discrimination were driving the results, we would expect denial rates to rise and loan terms to tighten following hate crimes. On the supply side, exposure to salient adverse events can independently shift lending behavior: loan officers near courthouses where foreclosure auctions occur reject more mortgage applications despite identical bank and county conditions (Huo et al., 2024). We examine whether lenders respond to hate crime shocks by tightening standards, studying conditional outcomes among applicants who do apply, focusing on denial ratios by race/ethnicity from HMDA and the terms of originated loans proxied by median loan-to-value (LTV) from CoreLogic.

Table 7 presents the results. Columns (1)–(3) report the effect of racial hate crimes on denial ratios for all applicants, minority (nonwhite) applicants, and White applicants respectively. We find weak evidence for a decline in denial rates following hate crimes, in particular for minority applicants. In fact, the effect on denial rates is negative and borderline significant for minority applicants (Column (2)), consistent with a compositional shift in the applicant pool (i.e., only relatively stronger minority borrowers apply, which lowers the ex post denial rate). Recent evidence using confidential mortgage files and automated underwriting system outcomes shows that most racial approval gaps reflect observable risk, with residual disparities remaining small (Bhutta, 2026). Consistent with this benchmark, our unchanged denial and LTV patterns indicate that post-application supply tightening is not the primary margin along which hate-crime shocks operate. The absence of an increase in minority denial ratios argues against lenders systematically tightening credit supply to minority borrowers in the wake of hate crimes.

Among White applicants, we see an increase in denial ratios, albeit modest in magnitude. Taken together with the unchanged median LTV of originated loans in Column (4), this pattern is what a demand-led contraction would predict: hate crimes depress local mortgage demand more broadly and, conditional on applying, White applicants may be disproportionately affected at the underwriting stage. The contraction in mortgage activity we document therefore operates at the extensive margin (whether households apply). Our identification works at the lender–year level and abstracts from within-lender, within-year variation across counties (the margin on which redlining or branch-closure effects would operate), and from changes in collateral requirements earlier in the application funnel; within these boundaries, the evidence is consistent with a demand-led contraction.

5.2. News-mediated salience

We next test whether the estimated hate crime effect is a media artifact, that is, whether it is mediated by local news coverage of hate crimes rather than reflecting the direct impact of actual criminal incidents. This concern arises because households may form perceptions of neighborhood safety partly through local media rather than solely through personal experience. If news coverage is the operative channel, then controlling for it should absorb the hate crime effect: the FBI crime measure would matter only insofar as it generates news, and once news coverage is held constant, the coefficient on reported hate crimes should shrink toward zero. Conversely, if the effect reflects actual crime transmitted through direct exposure, community networks, or the objective change in local safety conditions, it should survive conditioning on media coverage and remain stable regardless of whether incidents receive press attention.

We test this using article-level data from the 3DLNews2 corpus, a large-scale archive of U.S. local news articles collected from newspaper, radio, TV, and broadcast outlets via Google News and Twitter-sourced links between 1995 and 2024. We filter the corpus to articles containing hate-crime-related keywords and geocode each article to a U.S. county using the reported outlet coordinates, yielding 23,796 matched articles across 2,312 counties. We then aggregate to the county-year level to construct two lagged news variables: the log count of hate-crime articles published in a county in the prior year ($\log(1 + n_{\text{articles},t-1})$) and a binary indicator for any coverage ($\mathbb{1}\{n_{\text{articles},t-1} > 0\}$).

Table 8 reports the results. Column (1) restates the baseline. Columns (2) and (3) add the log and binary news measures alongside the hate crime variable. Two findings stand out. First, the coefficient on racial hate crime is virtually identical across all three columns: it neither shrinks nor loses significance when news coverage is included. Second, neither news measure is statistically significant: the log article count in Column (2) and the binary coverage indicator in Column (3) both have coefficients near zero. The hate crime effect on mortgage demand is therefore not explained by our county-level news coverage measures.

6. Conclusion

Higher racial hate crime exposure is associated with marked declines in mortgage applications and originations. The impact of racial hate crimes exceeds that of general crime or non-race-motivated crimes, reflecting their distinct nature. While general crime is mostly financially motivated, such as larceny (30.02% of incidents), hate crimes follow a different pattern, combining property destruction (25.16%), intimidation (22.79%), and simple assault (20.38%) in ways that suggest intentional targeting rather than opportunistic behavior.

The adverse effects are broad-based. We conduct disaggregated analyses to show that both minority and White applicants reduce mortgage activity in response to racial hate crimes. By decomposing racial hate crime into own-race and other-race incidents, we identify two complementary channels: a targeted-threat channel generating group-specific deterrence for minority borrowers, and a generalized-fear channel that reduces applications for both White and minority borrowers through signals of neighborhood hostility. Examining application outcomes, we find that adjustments occur through lower applications and higher withdrawals, while denial rates do not increase systematically for minority borrowers in hate-crime-affected areas. As such, the evidence is not consistent with a purely supply-side discrimination story. Instead, auxiliary analyses indicate that racial hate crimes are associated with heightened psychological distress and reductions in conspicuous consumption in affected counties. These findings support an interpretation in which racial hate crimes erode perceived safety, trust, and local social capital, lowering the attractiveness of long-term housing commitments. Difference-in-differences estimates around the George Floyd murder show a 4.8–8.5% drop in purchase applications in the months following the hate crime backlash, with the effect fading as hate crime levels returned to baseline.

We then show that these mechanisms manifest in broader neighborhood housing market conditions. Counties with higher racial hate crime exposure experience weaker housing markets, with marked declines in home sales volumes and house price growth. Rental prices do not respond detectably; we read the null as consistent with effects concentrated in the owner-occupied segment. The combined evidence portrays racial hate crimes as targeted shocks that depress mortgage demand and weigh on local property markets.

Our findings contribute to the literature on racial bias and hate crimes by documenting their spillovers into mortgage and housing markets. These effects extend beyond immediate victims and reach housing market activity at the neighborhood level, affecting housing choices and long-term investment decisions for both minority and majority households.

These patterns carry several implications for policy and practice. First, demand-side effects point beyond lender-side interventions alone: Community Reinvestment Act assessments and fair-lending reviews that focus on lender behavior may understate how hostile local environments suppress minority and White mortgage demand. Second, the results point to a role for targeted community outreach and credit-access programs in counties that experience acute hate crime episodes, where the mortgage demand response extends over the six-month post-event window we examine. Third, the specificity of racial (versus nonracial) hate crimes in driving the mortgage response argues for treating racial bias as a distinct policy category in housing and credit-market interventions.

References

Agarwal, Vikas, Jiang, Wei, Luo, Yuchen, & Zou, Hong. (2026). The real effect of sociopolitical racial/ethnic animus: Mutual fund manager performance during the AAPI hate [Forthcoming]. *Review of Financial Studies*.

Ambrose, Brent W, Conklin, James N, & Lopez, Luis A. (2021). Does borrower and broker race affect the cost of mortgage credit? *The Review of Financial Studies*, *34*(2), 790–826.

Ang, Desmond. (2023). The birth of a nation: Media and racial hate. *American Economic Review*, *113*(6), 1424–1460.

Armed Conflict Location & Event Data Project. (2020). *Demonstrations and political violence in America: New data for summer 2020* (tech. rep.). ACLED. https://acleddata.com/2020/09/03/demonstrations-political-violence-in-america-new-data-for-summer-2020/

Baker, Andrew, Callaway, Brantly, Cunningham, Scott, Goodman-Bacon, Andrew, & Sant'Anna, Pedro H. C. (2025). Difference-in-differences designs: A practitioner's guide [Forthcoming]. *Journal of Economic Literature*.

Bartlett, Robert, Morse, Adair, Stanton, Richard, & Wallace, Nancy. (2022). Consumer-lending discrimination in the FinTech era. *Journal of Financial Economics*, *143*(1), 30–56.

Bayer, Patrick, Keohane, Nathaniel, & Timmins, Christopher. (2009). Migration and hedonic valuation: The case of air quality. *Journal of Environmental Economics and Management*, *58*(1), 1–14.

Bayer, Patrick, McMillan, Robert, Murphy, Alvin, & Timmins, Christopher. (2016). A dynamic model of demand for houses and neighborhoods. *Econometrica*, *84*(3), 893–942.

Bhutta, Neil. (2026). *Neil Bhutta - research*. Retrieved April 10, 2026, from https://sites.google.com/site/neilbhutta/research

Bhutta, Neil, & Hizmo, Aurel. (2021). Do minorities pay more for mortgages? *The Review of Financial Studies*, *34*(2), 763–789.

Bhutta, Neil, Laufer, Steven, & Ringo, Daniel R. (2017). Residential mortgage lending in 2016: Evidence from the Home Mortgage Disclosure Act data. *Federal Reserve Bulletin*, *103*(6).

Bishop, Kelly C, & Murphy, Alvin D. (2011). Estimating the willingness to pay to avoid violent crime: A dynamic approach. *American Economic Review*, *101*(3), 625–629.

Borusyak, Kirill, Jaravel, Xavier, & Spiess, Jann. (2024). Revisiting event-study designs: Robust and efficient estimation. *The Review of Economic Studies*, *91*(6), 3253–3285.

Célérier, Claire, & Tak, Purnoor. (2025). Finance, advertising, and race. *Review of Financial Studies*, *38*(11), 3149–3204.

Chetty, Raj, Hendren, Nathaniel, Jones, Maggie R, & Porter, Sonya R. (2020). Race and economic opportunity in the United States: An intergenerational perspective. *The Quarterly Journal of Economics*, *135*(2), 711–783.

Christensen, Peter, & Timmins, Christopher. (2022). Sorting or steering: The effects of housing discrimination on neighborhood choice. *Journal of Political Economy*, *130*(8), 2110–2163.

Cook, Lisa D. (2014). Violence and economic activity: Evidence from African American patents, 1870–1940. *Journal of Economic Growth*, *19*(2), 221–257.

Custódio, Cláudia, Cvijanović, Dragana, & Wiedemann, Moritz. (2025). Opioid crisis and real estate prices [Early Access]. *Journal of Financial and Quantitative Analysis*.

D'Acunto, Francesco, Prokopczuk, Marcel, & Weber, Michael. (2019). Historical antisemitism, ethnic specialization, and financial development. *The Review of Economic Studies*, *86*(3), 1170–1206.

Dagher, Jihad, & Kazimov, Kazim. (2015). Banks' liability structure and mortgage lending during the financial crisis. *Journal of Financial Economics*, *116*(3), 565–582.

Dipoppa, Gemma, Grossman, Guy, & Zonszein, Stephanie. (2023). Locked down, lashing out: Covid-19 effects on asian hate crimes in italy. *The Journal of Politics*, *85*(2), 389–404.

Doerr, Sebastian, & Fuster, Andreas. (2025). *Affordable housing, unaffordable credit? Concentration and high-cost lending for manufactured homes* (BIS Working Paper No. 1255). Bank for International Settlements.

Dustmann, Christian, & Fasani, Francesco. (2016). The effect of local area crime on mental health. *The Economic Journal*, *126*(593), 978–1017.

FBI. (2023, July). https://www.justice.gov/hatecrimes/2023-hate-crime-statistics

Foà, Gabriele, Gambacorta, Leonardo, Guiso, Luigi, & Mistrulli, Paolo Emilio. (2019). The supply side of household finance. *Review of Financial Studies*, *32*(10), 3762–3798.

Frey, Arun. (2020). 'Cologne changed everything' – The effect of threatening events on the frequency and distribution of intergroup conflict in Germany. *European Sociological Review*, *36*(5), 684–699.

Fuster, Andreas, Plosser, Matthew, Schnabl, Philipp, & Vickery, James. (2019). The role of technology in mortgage lending. *Review of Financial Studies*, *32*(5), 1854–1899.

Gabriel, Stuart A, & Lutz, Chandler. (2024). The impact of crisis-period interest rate declines on distressed borrowers. *Review of Financial Studies*, *37*(12), 3710–3760.

Gerardi, Kristopher, Lambie-Hanson, Lauren, & Willen, Paul S. (2021, June). *Racial differences in mortgage refinancing, distress, and housing wealth accumulation during COVID-19* (Current Policy Perspectives). Federal Reserve Bank of Boston.

Gete, Pedro, & Reher, Michael. (2018). Mortgage supply and housing rents. *Review of Financial Studies*, *31*(12), 4884–4911.

Gould, Eric D, & Klor, Esteban F. (2016). The long-run effect of 9/11: Terrorism, backlash, and the assimilation of Muslim immigrants in the West. *The Economic Journal*, *126*(597), 2064–2114.

Green, Donald P, Strolovitch, Dara Z, & Wong, Janelle S. (1998). Defended neighborhoods, integration, and racially motivated crime. *American Journal of Sociology*, *104*(2), 372–403.

Grosjean, Pauline, Masera, Federico, & Yousaf, Hasin. (2023). Inflammatory political campaigns and racial bias in policing. *The Quarterly Journal of Economics*, *138*(1), 413–463.

Gupta, Arpit, Hansman, Christopher, & Mabille, Pierre. (2025). Financial constraints and the racial housing gap. *Journal of Financial Economics*, *173*, 104142.

Hasan, Iftekhar, Hoi, Chun Keung, Wu, Qiang, & Zhang, Hao. (2017). Social capital and debt contracting: Evidence from bank loans and public bonds. *Journal of Financial and Quantitative Analysis*, *52*(3), 1017–1047.

Herek, Gregory M, Gillis, J Roy, & Cogan, Jeanine C. (1999). Psychological sequelae of hate-crime victimization among lesbian, gay, and bisexual adults. *Journal of Consulting and Clinical Psychology*, *67*(6), 945.

Huo, Da, Sun, Bo, Tai, Mingzhu, & Xuan, Yuhai. (2024). Lending next to the courthouse: Exposure to adverse events and mortgage lending decisions. *Journal of Financial and Quantitative Analysis*, *59*(5), 2375–2398.

Iganski, Paul. (2001). Hate crimes hurt more. *American Behavioral Scientist*, *45*(4), 626–638.

Kessler, Ronald C, Andrews, Gavin, Colpe, Lisa J, Hiripi, Eva, Mroczek, Daniel K, Normand, S-LT, Walters, Ellen E, & Zaslavsky, Alan M. (2002). Short screening scales to monitor population prevalences and trends in non-specific psychological distress. *Psychological Medicine*, *32*(6), 959–976.

Law, Kelvin KF, & Zuo, Luo. (2022). Public concern about immigration and customer complaints against minority financial advisors. *Management Science*, *68*(11), 8464–8482.

Lockwood, Sarah, & Cuevas, Carlos A. (2022). Hate crimes and race-based trauma on Latinx populations: A critical review of the current research. *Trauma, Violence, & Abuse*, *23*(3), 854–867.

Lyons, Christopher J. (2007). Community (dis)organization and racially motivated crime. *American Journal of Sociology*, *113*(3), 815–863.

McCartney, W. Ben, Orellana-Li, John, & Zhang, Calvin. (2024). Political polarization affects households' financial decisions: Evidence from home sales. *Journal of Finance*, *79*(2), 795–841.

McDevitt, Jack, Balboni, Jennifer, Garcia, Luis, & Gu, Joann. (2001). Consequences for victims: A comparison of bias-and non-bias-motivated assaults. *American Behavioral Scientist*, *45*(4), 697–713.

Mejía, Daniel, & Restrepo, Pascual. (2016). Crime and conspicuous consumption. *Journal of Public Economics*, *135*, 1–14.

Miller, Sarah, & Soo, Cindy K. (2021). Do neighborhoods affect the credit market decisions of low-income borrowers? Evidence from the Moving to Opportunity experiment. *Review of Financial Studies*, *34*(2), 827–863.

Müller, Karsten, & Schwarz, Carlo. (2021). Fanning the flames of hate: Social media and hate crime. *Journal of the European Economic Association*, *19*(4), 2131–2167.

Müller, Karsten, & Schwarz, Carlo. (2023). From hashtag to hate crime: Twitter and antiminority sentiment. *American Economic Journal: Applied Economics*, *15*(3), 270–312.

New York Times. (2023, August). *11 fatal minutes that have Jacksonville confronting racism yet again*. Retrieved September 20, 2023, from https://www.nytimes.com/2023/08/27/us/jacksonville-shooting-victims-timeline.html

Paterson, Jenny L, Brown, Rupert, & Walters, Mark A. (2019). The short and longer term impacts of hate crimes experienced directly, indirectly, and through the media. *Personality and Social Psychology Bulletin*, *45*(7), 994–1010.

Perry, Barbara. (2001). *In the name of hate: Understanding hate crimes*. Routledge.

Perry, Barbara, & Alvi, Shahid. (2012). 'we are all vulnerable': The in terrorem effects of hate crimes. *International Review of Victimology*, *18*(1), 57–71.

Ray, Rashawn, Perry, Andre M., Harshbarger, David, Elizondo, Samantha, & Gibbons, Alexandra. (2021, September). *Homeownership, racial segregation, and policy solutions to racial wealth equity* (tech. rep.). Brookings Institution.

Riaz, Sascha, Bischof, Daniel, & Wagner, Markus. (2024). Out-group threat and xenophobic hate crimes: Evidence of local intergroup conflict dynamics between immigrants and natives. *The Journal of Politics*, *86*(4), 1146–1161.

Rose, Suzanna M, & Mechanic, Mindy B. (2002). Psychological distress, crime features, and help-seeking behaviors related to homophobic bias incidents. *American Behavioral Scientist*, *46*(1), 14–26.

Scheepers, Peer, Gijsberts, Mérove, & Coenders, Marcel. (2002). Ethnic exclusionism in European countries. Public opposition to civil rights for legal migrants as a response to perceived ethnic threat. *European Sociological Review*, *18*(1), 17–34.

Stephan, Cookie White, Stephan, Walter G, Demitrakis, Katherine M, Yamada, Ann Marie, & Clason, Dennis L. (2000). Women's attitudes toward men an integrated threat theory approach. *Psychology of Women Quarterly*, *24*(1), 63–73.

Tajfel, Henri, & Turner, John C. (1979). An integrative theory of intergroup conflict. In William G. Austin & Stephen Worchel (Eds.), *The social psychology of intergroup relations* (pp. 33–47). Brooks/Cole.

US Dep. Justice. (2023, July). https://www.justice.gov/hatecrimes/laws-and-policies

Walker, Iain, & Smith, Heather J. (Eds.). (2002). *Relative deprivation: Specification, development, and integration*. Cambridge University Press.

Figure 1
Hate crime

This figure illustrates the evolution of three primary hate crime categories per 1,000 total reported crimes over the period from 1994 to 2020. The categories displayed are racial hate crimes, sex- and gender-based hate crimes, and religious hate crimes. The data are sourced from the FBI Uniform Crime Reporting (UCR) program, combining hate-crime reports with total reported crime counts. Because agency participation and SRS/NIBRS reporting coverage change over time, the series should be interpreted as reported rates rather than underlying incidence.

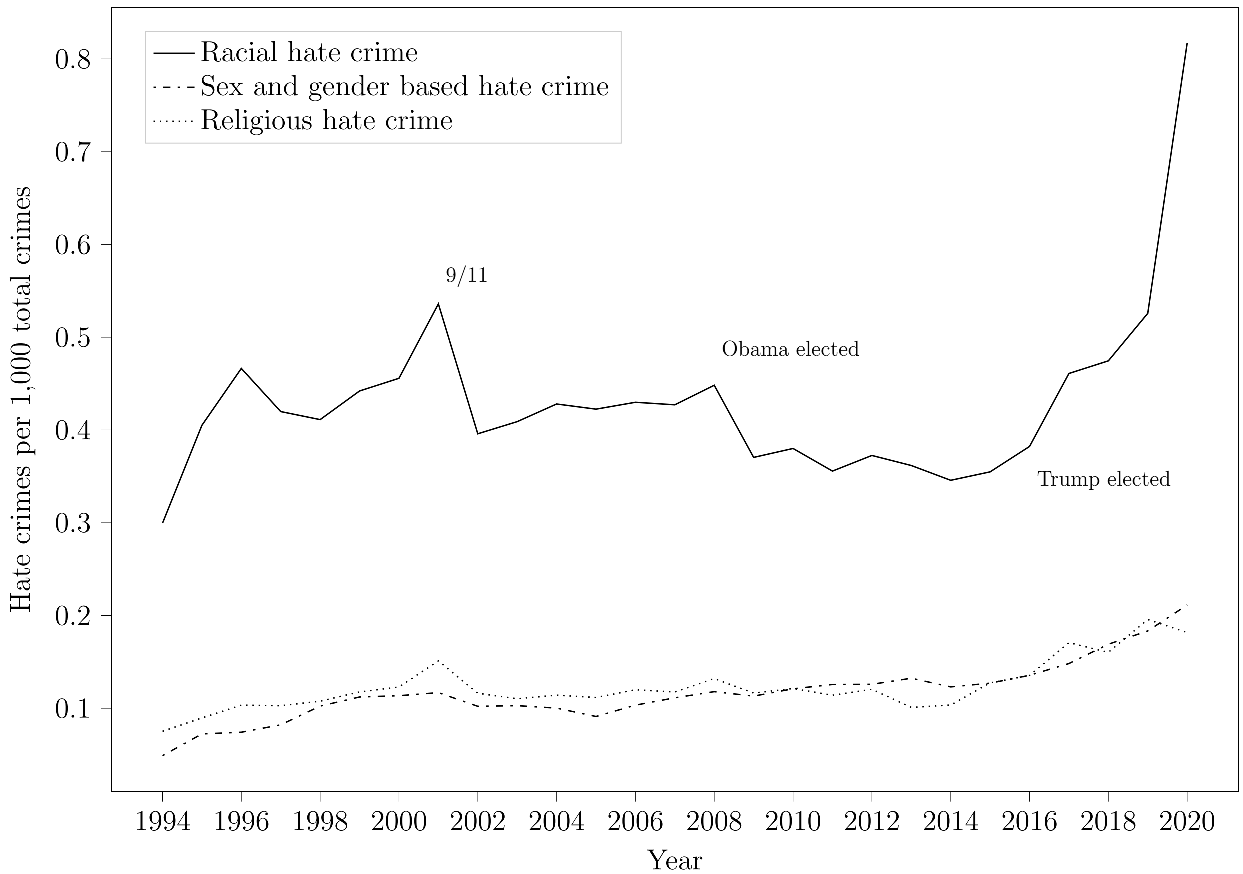

Figure 2
Difference-in-differences: Racial hate crime surges and mortgage demand

The sample covers six months either side of the George Floyd murder (May 25, 2020). Treated counties ($n = 143$) record ≥ 3 racial hate crime incidents in any single month within six months of the event; control counties ($n = 446$) never reach this threshold. Panel (a) plots mean monthly racial hate crimes (lines) alongside Black Lives Matter (BLM) protest activity (bars, right axis; control counties on the bottom, treated counties on top). Panel (b) compares mean log purchase mortgage applications in treated counties with the Borusyak, Jaravel, and Spiess (BJS)-imputed counterfactual. Panel (c) plots the imputed treatment effects (filled circles) alongside pre-event diagnostic coefficients from the parallel trends test (open circles), each with 95% confidence intervals. All specifications include county and Census division $\times$ month fixed effects. Standard errors are clustered at the state level.

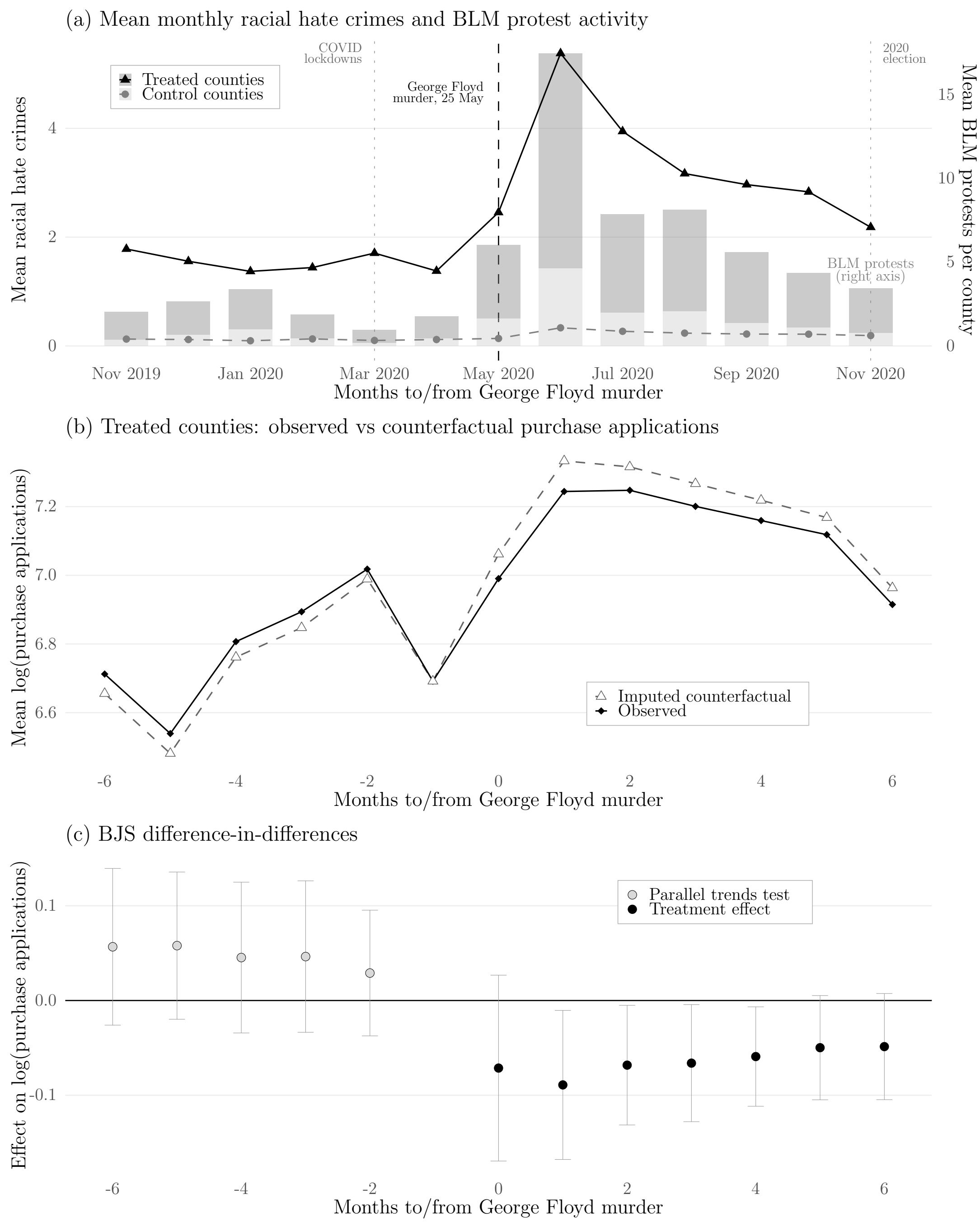

Table 1
Offense type frequencies for hate crimes and non-hate crimes

This table presents counts, percentages and cumulative percentages for offense types for hate crimes (Panel A) and non-hate crimes (Panel B), ranked in descending order by their occurrence. Data on hate crime and non-hate crime are drawn from the National Incident-Based Reporting System (NIBRS) for the period 2009–2020, covering approximately 2,800 counties that report to the program.

(A) Hate crime				(B) Non-hate crime			
Offense type	Obs.	Freq. (%)	Cuml. (%)	Offense type	Obs.	Freq. (%)	Cuml. (%)
Destruction/damage/vandalism of property	10,614	25.16	25.16	Larceny/theft offenses	22,314,989	30.02	30.02
Intimidation	9,618	22.79	47.95	Destruction/damage/vandalism of property	9,649,254	12.98	42.99
Simple assault	8,598	20.38	68.33	Simple assault	9,130,597	12.28	55.28
Aggravated assault	3,848	9.12	77.45	Drug/narcotic offenses	9,044,593	12.17	67.44
Larceny/theft offenses	3,589	8.51	85.95	Burglary/breaking & entering	6,153,985	8.28	75.72
Drug/narcotic offenses	1,543	3.66	89.61	Fraud offenses	4,283,316	5.76	81.48
Burglary/breaking & entering	1,355	3.21	92.82	Intimidation	2,803,180	3.77	85.25
Robbery	665	1.58	94.40	Motor vehicle theft	2,677,755	3.60	88.85
Fraud offenses	584	1.38	95.78	Aggravated assault	2,445,679	3.29	92.14
Weapon law violations	376	0.89	96.67	Weapon law violations	1,156,629	1.56	93.70
Sex offenses	358	0.85	97.52	Sex offenses	1,081,533	1.45	95.15
Motor vehicle theft	303	0.72	98.24	Counterfeiting/forgery	1,071,920	1.44	96.60
Arson	179	0.42	98.66	Robbery	942,253	1.27	97.86
Counterfeiting/forgery	160	0.38	99.04	Stolen property offenses	532,144	0.72	98.58
Stolen property offenses	98	0.23	99.27	Embezzlement	250,579	0.34	98.92
Kidnapping/abduction	96	0.23	99.50	Kidnapping/abduction	206,002	0.28	99.19
Pornography/obscene material	55	0.13	99.63	Arson	185,386	0.25	99.44
Homicide offenses	50	0.12	99.75	Pornography/obscene material	139,612	0.19	99.63
Embezzlement	41	0.10	99.85	Prostitution offenses	123,352	0.17	99.80
Extortion/blackmail	34	0.08	99.93	Homicide offenses	61,391	0.08	99.88
Prostitution offenses	16	0.04	99.97	Extortion/blackmail	37,009	0.05	99.93
Animal cruelty	8	0.02	99.99	Animal cruelty	31,474	0.04	99.97
Bribery	3	0.01	99.99	Gambling offenses	12,835	0.02	99.99
Gambling offenses	2	0.00	100.00	Bribery	5,002	0.01	99.99
Human trafficking	1	0.00	100.00	Human trafficking	3,743	0.01	100.00
				Federal liquor offenses	4	0.00	100.00
				Illegal entry	1	0.00	100.00

Table 2
Summary statistics

This table presents descriptive statistics for the primary variables used in the analysis, covering the sample period from 2009 to 2020. Mortgage credit variables are at the lender-county-year level, while geographic, housing market and migration variables are at the county-year level and matched to lenders in a given county. Household variables are based on household survey data and are measured at the household-survey level (monthly for PSID, quarterly for CEX). Detailed definitions of all variables are provided in Appendix A.

Variable	Observation	Mean	Std. dev.	10th	90th
Mortgage credit variables					
Mortgage applications	3,927,845	1.91	1.39	0.69	4.04
Withdrawal	3,927,845	0.60	0.94	0.00	1.95
Mortgage originations	3,927,845	1.35	1.34	0.00	3.37
Denial rate	3,728,375	0.18	0.29	0.00	0.60
Geographic variables					
Racial hate crime (log)	3,927,845	0.68	1.01	0.00	2.20
Population	3,927,845	11.46	1.47	9.67	13.51
GDP growth rate	3,927,845	0.03	0.06	−0.02	0.09
Personal income	3,927,845	43.64	13.23	31.35	58.67
Unemployment rate	3,927,845	5.88	2.62	3.20	9.50
Poverty percentage	3,927,845	14.46	5.45	7.90	21.50
Home price	3,927,845	11.92	0.55	11.24	12.63
Crime rate	3,927,845	2.97	0.68	2.09	3.85
Share of population					
Black	3,927,845	10.27	12.97	0.64	28.11
Asian	3,927,845	2.57	3.92	0.40	5.88
Hispanic	3,927,845	9.66	12.49	1.39	24.32
White	3,927,845	73.81	19.14	44.65	94.44
Native American	3,927,845	1.40	3.64	0.26	2.33
Hawaiian and Other Pacific Islanders	3,927,845	0.14	0.40	0.02	0.27
Others	3,927,845	2.16	1.35	1.11	3.41
Banking variables					
Total risk-based capital ratio	3,927,845	15.19	3.06	13.03	18.14
Return on assets	3,927,845	0.98	0.50	0.51	1.40
Noncurrent loan rate	3,927,845	1.99	1.63	0.68	4.29
Loans-to-deposits ratio	3,927,845	78.56	10.64	67.05	89.54
Housing market variables					
Rents	3,051	7.09	0.32	6.73	7.48
Growth in home sales	26,756	0.07	0.48	−0.14	0.23
Growth in home price	28,694	0.03	0.06	−0.04	0.09
Median loan-to-value ratio	24,584	0.91	0.07	0.80	0.98
Household variables					
Psychological distress	34,434	0.15	0.35	0.00	1.00
Clothing and jewelry	27,988	3.46	2.53	0.00	6.26

Table 3
Racial hate crime, offense types and mortgage demand

This table reports the effect of racial hate crime on mortgage applications. The dependent variable is the log number of applications received by a lender in a county-year. Panel A uses hate crime data from the FBI UCR Hate Crime Statistics. Panel B uses incident-based data from NIBRS: Model I includes total crime only, Model II adds racial hate crime as a distinct category; each specification is estimated in a separate regression. All specifications include county and lender–year fixed effects (our preferred specification), except Panel A Columns (2) and (3). All specifications include geographic controls (population, GDP growth, personal income, unemployment rate, poverty rate, home prices, state crime rate, and population shares by race/ethnicity), and banking-supply controls (deposit-weighted bank capital ratio, return on assets, non-current loan ratio, and loan-to-deposit ratio). Panel B excludes the state crime rate. Variable definitions are in Appendix A. Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A: Using hate crime data recorded in Hate Crime Statistics of FBI's UCR program

	Dependent variable: Log mortgage applications		
	(1)	(2)	(3)
Racial hate crime	−0.004**	−0.005***	−0.004**
	(0.002)	(0.002)	(0.002)
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Lender–year FE	Yes	No	No
Lender FE	No	Yes	No
Year FE	No	Yes	Yes
Adjusted R^2	0.403	0.365	0.108
Observations	3,927,406	3,927,840	3,927,845

Panel B: Using hate crime and crime data recorded in NIBRS

	Dependent variable: Log mortgage applications					
	Model I		Model II			
	Crime		Crime		Racial hate crime	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)
All types	−0.003*	(0.002)	−0.002	(0.002)	−0.004**	(0.002)
Offense types:						
Destruction/damage/vandalism of property	−0.001	(0.002)	−0.001	(0.002)	−0.008***	(0.003)
Intimidation	−0.001	(0.002)	−0.001	(0.002)	−0.005**	(0.003)
Simple assault	−0.002	(0.002)	−0.002	(0.002)	−0.001	(0.003)
Aggravated assault	−0.001	(0.002)	−0.001	(0.002)	0.002	(0.004)
Larceny/theft offenses	−0.003**	(0.001)	−0.003*	(0.002)	−0.003	(0.004)
Drug/narcotic offenses	−0.002	(0.002)	−0.002	(0.002)	−0.008*	(0.004)
Burglary/breaking and entering	−0.003*	(0.002)	−0.003*	(0.002)	−0.009**	(0.005)
Robbery	−0.004**	(0.002)	−0.004**	(0.002)	0.004	(0.007)
Fraud offenses	−0.001	(0.002)	−0.001	(0.002)	0.003	(0.006)
Weapon law violations	−0.002	(0.002)	−0.001	(0.002)	−0.010	(0.006)
Sex offenses	−0.003	(0.002)	−0.003	(0.002)	−0.002	(0.009)
Motor vehicle theft	−0.002	(0.002)	−0.002	(0.002)	−0.005	(0.006)
Arson	−0.001	(0.002)	−0.001	(0.002)	−0.009	(0.011)
Counterfeiting/forgery	−0.004**	(0.002)	−0.004**	(0.002)	0.015	(0.010)

Table 4

Fear channels: race-specific hate crime effects on mortgage applications and withdrawals

This table decomposes racial hate crime exposure into incidents against the borrower's own racial group and incidents against other racial groups. Each column corresponds to a borrower racial group and reports coefficients on own-race hate crimes and hate crimes against other racial groups separately. In Panel A, racial hate crime is lagged ($t-1$) as in all baseline specifications, while in Panel B racial hate crime is contemporaneous (t). All specifications include county and lender–year fixed effects, geographic controls (population, GDP growth, personal income, unemployment rate, poverty rate, home prices, state crime rate, and population shares by race/ethnicity), and banking-supply controls (deposit-weighted bank capital ratio, return on assets, non-current loan ratio, and loan-to-deposit ratio). Variable definitions are in Appendix A. Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A: Mortgage applications				
	Black	Asian	Hispanic	White
	(1)	(2)	(3)	(4)
Hate crime against own race	–0.003**	0.002	–0.007***	–0.001
	(0.001)	(0.003)	(0.002)	(0.002)
Hate crime against other races	–0.000	–0.002*	–0.004***	–0.004***
	(0.001)	(0.001)	(0.001)	(0.002)
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Lender–year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.346	0.334	0.321	0.362
Observations	3,927,406	3,927,406	3,927,406	3,927,406

Panel B: Log withdrawals				
	Black	Asian	Hispanic	White
	(1)	(2)	(3)	(4)
Hate crime against own race$_t$	0.001	0.005***	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)
Hate crime against other races$_t$	0.002**	0.002***	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Lender–year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.224	0.212	0.212	0.323
Observations	3,927,406	3,927,406	3,927,406	3,927,406

Table 5

Racial hate crime and survey-based household outcomes

This table reports the effect of racial hate crimes on survey-based outcomes. Panel A uses PSID respondent-level data: the dependent variable is an indicator equal to one if the respondent is psychologically distressed, and local racial hate crime is the log number of racial hate crimes in the 12 months before the interview. Panel B uses CEX household-level data: the dependent variable is expenditure on clothing and jewelry, and local racial hate crime is the log number in the quarter before the reference quarter. Variable definitions are in Appendix A. Standard errors are clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A: Psychological distress

	Black	Asian	Hispanic	White
	(1)	(2)	(3)	(4)
Racial hate crime	0.009*	−0.023	0.129***	0.076***
	(0.005)	(0.051)	(0.018)	(0.005)
Age	−0.000	0.002	0.000	−0.001**
	(0.000)	(0.002)	(0.001)	(0.000)
Education	0.001***	0.003***	0.002**	0.001***
	(0.000)	(0.001)	(0.001)	(0.000)
Family size	0.011***	0.010	0.010**	0.005***
	(0.002)	(0.013)	(0.004)	(0.002)
Employed	−0.034***	−0.049	−0.030**	−0.034***
	(0.008)	(0.058)	(0.014)	(0.007)
Family income	−0.667***	−0.992***	−0.128*	−0.134***
	(0.070)	(0.278)	(0.074)	(0.044)
Family wealth	0.006	−0.010	0.053*	0.000
	(0.007)	(0.015)	(0.028)	(0.002)
State FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.012	0.065	0.029	0.017
Observations	13,310	501	2,940	16,914

Panel B: Visible spending (Clothing and Jewelry)

	Black	Asian	Hispanic	White
	(1)	(2)	(3)	(4)
Racial hate crime	−0.205*	−0.350**	−0.353***	−0.399***
	(0.110)	(0.137)	(0.081)	(0.054)
Age	−0.012***	−0.027***	−0.024***	−0.023***
	(0.003)	(0.003)	(0.002)	(0.001)
Education	0.163***	0.049*	0.063***	0.264***
	(0.025)	(0.028)	(0.013)	(0.013)
Family size	0.288***	0.206***	0.245***	0.259***
	(0.036)	(0.044)	(0.021)	(0.019)
Marital status	0.026	0.366***	−0.016	0.278***
	(0.108)	(0.136)	(0.064)	(0.047)
Income	0.001	−0.021**	0.004***	0.000
	(0.028)	(0.010)	(0.001)	(0.001)
State FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.078	0.076	0.094	0.121
Observations	2,772	2,227	6,971	15,681

Table 6
Rental and housing market effects

This table reports the effect of racial hate crime on neighborhood housing market outcomes at the county-year level. The dependent variables are log rents, home sales growth, and home price growth. All specifications include county and year fixed effects, geographic controls (population, GDP growth, personal income, unemployment rate, poverty rate, state crime rate, and population shares by race/ethnicity), and banking-supply controls (deposit-weighted bank capital ratio, return on assets, non-current loan ratio, and loan-to-deposit ratio). Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	Rents	Growth in home sales	Growth in house price
	(1)	(2)	(3)
Racial hate crime	−0.044	−0.009*	−0.002**
	(0.141)	(0.005)	(0.001)
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adjusted R^2	0.963	0.038	0.497
Observations	3,036	26,727	28,678

Table 7
Mortgage denial and median loan-to-value ratio

This table reports the effect of racial hate crime on mortgage denial rates and the local median loan-to-value ratio. The dependent variable in Columns (1)–(3) is the denial rate of mortgage applications by a lender in a county-year, estimated separately for all applicants, minority (non-white) applicants, and White applicants. The dependent variable in Column (4) is the median loan-to-value ratio in a county-year. Columns (1)–(3) use lender–county–year observations with county and lender–year fixed effects; Column (4) uses county–year observations with county and year fixed effects. All specifications include geographic controls (population, GDP growth, personal income, unemployment rate, poverty rate, home prices, state crime rate, and population shares by race/ethnicity) and banking-supply controls (deposit-weighted bank capital ratio, return on assets, non-current loan ratio, and loan-to-deposit ratio). Variable definitions are in Appendix A. Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	Denial rate			
	All	Minorities	White	Median loan-to-value
	(1)	(2)	(3)	(4)
Racial hate crime	0.000	−0.001*	0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.001)
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Lender–year FE	Yes	Yes	Yes	No
Year FE	No	No	No	Yes
Adjusted R^2	0.315	0.349	0.295	0.788
Observations	3,727,927	2,246,853	3,167,960	22,541

Table 8

Racial hate crime and mortgage demand: controlling for local news coverage

This table adds measures of local hate-crime news coverage to the baseline specification. The dependent variable is the log number of mortgage applications received by a lender in a county-year. Log HC news articles is the log count of local news articles mentioning hate crimes in the prior year, drawn from the 3DLNews2 corpus. Has HC news coverage is an indicator for any hate-crime news coverage in the prior year. Column (1) reports the baseline; Columns (2) and (3) add the respective news measure. All specifications include county and lender–year fixed effects, geographic controls (population, GDP growth, personal income, unemployment rate, poverty rate, home prices, state crime rate, and population shares by race/ethnicity), and banking-supply controls (deposit-weighted bank capital ratio, return on assets, non-current loan ratio, and loan-to-deposit ratio). Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)
Racial hate crime	−0.004**	−0.004**	−0.004**
	(0.002)	(0.002)	(0.002)
Log HC news articles		0.004	
		(0.003)	
Has HC news coverage			−0.001
			(0.002)
Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Lender–year FE	Yes	Yes	Yes
Adjusted R^2	0.395	0.395	0.395
Observations	3,927,406	3,927,406	3,927,406

A. Definitions of key variables

Variable	Definition	Source
Mortgage credit variables		
Mortgage applications	The number of mortgage applications a lender in a county receives (in logs)	HMDA
Withdrawal	The withdrawn applications a lender in a county receives (in logs)	HMDA
Mortgage originations	The number of mortgage applications that are granted by a lender in a county (in logs)	HMDA
Denial rate	The denied applications divided by all not withdrawn applications received by a lender in a county	HMDA
Geographic variables		
Racial hate crime	log(1+racial hate crime incidents) in a county-year; retains zero-incident observations	FBI
Population	Population in a county (in logs)	US Census Bureau
GDP growth rate	The year on year GDP growth rate in a county	Bureau of Economic Analysis
Personal income	The average personal income of a county	Bureau of Economic Analysis
Unemployment rate	The unemployment rate in a county	US Bureau of Labor
Poverty percentage	The percentage of people in a county living in poverty	Small Area Income and Poverty Estimates Program
Crime rate	Violent crime and property crime per 100 people	FBI
Share of population	The population of a group (Black, Asian, Hispanic, White, Native American, Hawaiian and Other islanders, and Others) divided by the total population in a county (in percentages)	US Census Bureau
Banking variables		
Total risk-based capital ratio	Deposit-weighted county-year average of each bank's total risk-based capital to risk-weighted assets ratio, lagged one year	FDIC SDI / SOD
Return on assets	Deposit-weighted county-year average of each bank's return on assets, lagged one year	FDIC SDI / SOD

(Continued)

Variable	Definition	Source
Non-current loan ratio	Deposit-weighted county-year average of each bank's non-current loans to total loans ratio, lagged one year	FDIC SDI / SOD
Loans-to-deposits ratio	Deposit-weighted county-year average of each bank's total loans and leases to total deposits ratio, lagged one year	FDIC SDI / SOD
Housing market variables		
Rents	Average asking rent in a county (in logs, multiplied by 100)	Zillow
Growth in home sales	Year on year growth rate of total number of all home-sale transactions in a county	CoreLogic
Growth in home price	Year on year growth rate of home price measured by Zillow Home Value Index	Zillow
Median loan-to-value ratio	The median mortgage debt to sales price ratio in a county	CoreLogic
Household variables		
Psychological distress	Dummy variable equal to 1 if respondent's psychological distress scale is larger than 12 and 0 otherwise	PSID
Clothing and jewelry	Log of the amount spent on the two items (the exact item code can be found in appendix)	CEX

When Prejudice Hits Home:

Hate Crime and the Market for Mortgage Credit

Online Appendix

Christian Engels   Iftekhar Hasan   Sizhe Hong   Dennis Philip

List of Figures

List of Tables

Figure A1
Extended parallel trends test: 24-month pre-event window

This figure plots pre-event coefficients from the BJS imputation estimator over a 24-month window before the George Floyd murder (May 2020). Each point is the estimated difference between observed and imputed outcomes for treated counties at event-time $e < 0$, with 95% confidence intervals. The specification includes county and Census division $\times$ month fixed effects, estimated on untreated observations only. Standard errors are clustered at the state level (51 clusters).

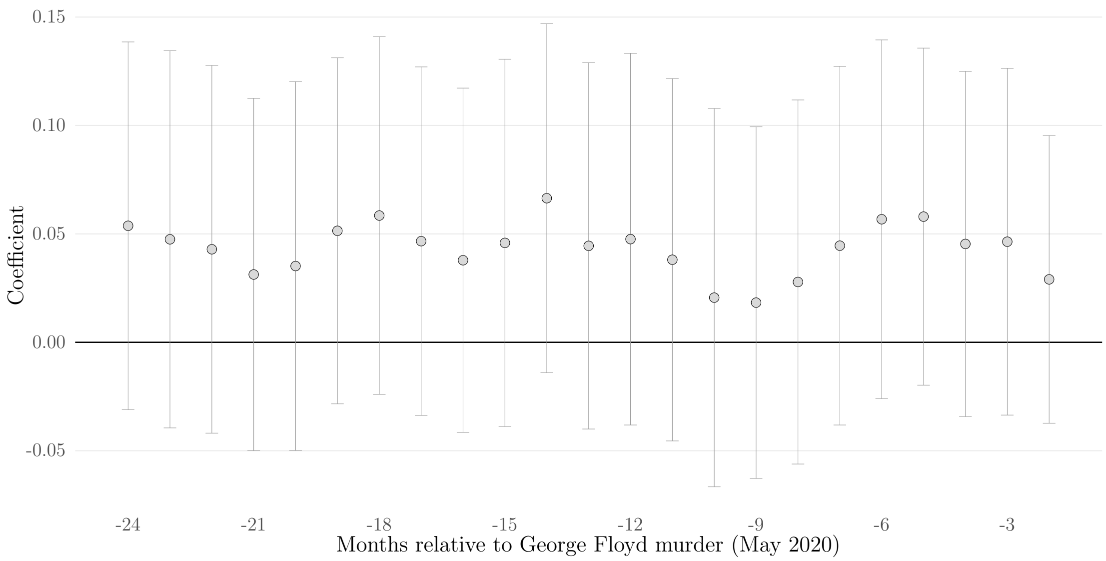

Table A1
Bias motivation frequencies for hate crimes

This table presents the count, percentage, and cumulative percentage for various types of bias-motivated hate crimes over the 2009–2020 sample period. Data are sourced from FBI's Uniform Crime Reporting (UCR) program.

Bias motivation	Obs.	Freq. (%)	Cuml. (%)
Anti-Black or African American	25,842	53.13	53.13
Anti-White	8,207	16.87	70.00
Anti-Hispanic or Latino	5,278	10.85	80.85
Anti-Other race/ethnicity/ancestry	3,627	7.46	88.30
Anti-Asian	1,898	3.90	92.21
Anti-Multiple races	1,743	3.58	95.79
Anti-American Indian or Alaska Native	1,445	2.97	98.76
Anti-Arab	515	1.06	99.82
Anti-Native Hawaiian or Other Pacific Islander	88	0.18	100

Table A2
Home purchase mortgages

This table presents regression estimates from the baseline model, restricting the analysis to home purchase mortgages. The dependent variable is the log number of mortgage applications received by a lender in a county in a year. Column (1) reports results for all minority-group (nonwhite) applications, Columns (2)–(4) for Black, Asian, and Hispanic applicants, respectively, and Column (5) for White applicants. All regressions include lender–year and county fixed effects. Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	All	Minorities	White
	(1)	(2)	(3)
Racial hate crime	−0.007***	−0.006***	−0.006***
	(0.002)	(0.002)	(0.002)
Controls	Yes	Yes	Yes
Lender–year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Adjusted R^2	0.082	0.125	0.058
Observations	3,927,845	3,927,845	3,927,845

Table A3

Distribution of peak-month racial hate crimes after the George Floyd murder

This table shows the distribution of peak-month racial hate crime counts across the 589 Bhutta counties in our monthly panel. For each county we take the maximum monthly racial hate crime count within the six months following the George Floyd murder and assign it to a bucket. Counties with no panel observations in that window (89 counties) are counted in the zero-peak bucket. The final column reports the number of treated counties implied by setting the DiD treatment threshold at the corresponding peak count.

Peak-month racial hate crimes	Counties			No. of counties making cutoff
	Count	Share (%)	Cumulative (%)	
0	228	38.7	38.7	–
1	156	26.5	65.2	361
2	62	10.5	75.7	205
3	38	6.4	82.1	143
4	34	5.8	87.9	105
5	14	2.4	90.3	71
6-9	29	4.9	95.2	57
≥ 10	28	4.8	100.0	28
Total	589	100.0		

Table A4
Racial hate crime and mortgage demand: county-year level

This table replicates the baseline specification at the county-year level. Application and origination counts are summed across all active lenders within each county-year and logged ($\log(1 + \text{count})$). The regression includes county and year fixed effects (no lender–year FE). Controls include the full set of county-level characteristics and banking supply controls (deposit-weighted bank capital ratio, return on assets, noncurrent loan ratio, and loan-to-deposit ratio). Sample restrictions and the hate crime measure are identical to the main analysis. Standard errors are clustered at the county level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Mortgage applications

	Dependent variable: Log mortgage applications			
	Black	Asian	Hispanic	White
	(1)	(2)	(3)	(4)
Racial hate crime	−0.017***	−0.009	−0.016***	−0.006*
	(0.006)	(0.007)	(0.005)	(0.004)
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.971	0.958	0.967	0.986
Observations	28,678	28,678	28,678	28,678

Panel B: Mortgage originations

	Dependent variable: Log mortgage originations			
	Black	Asian	Hispanic	White
	(1)	(2)	(3)	(4)
Racial hate crime	−0.024***	−0.011	−0.017***	−0.008*
	(0.006)	(0.007)	(0.006)	(0.004)
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.964	0.955	0.960	0.981
Observations	28,678	28,678	28,678	28,678