Banks Against Crime: The Impact of Home Mortgages on Neighborhood Crime[*]

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Abstract

Home mortgages are thought to enhance social capital among neighbors and encourage neighborhood revitalization. Research suggests that residential lending is associated with less crime, but most studies overlook the impact on acquisitive crime and suffer from endogeneity biases through common causes and omitted variables. This study overcomes these biases using a shift-share instrumental variables approach by leveraging the differential exposure to banks' local market share and common national mortgage shocks across 27 US cities. This research finds that when banks make more home loans, communities experience a significant decrease in burglaries, thefts, aggravated assaults, and low-level offenses and an increase in motor vehicle thefts. The effects are larger in Black, Hispanic, and poor neighborhoods and seem driven by a decrease in vacant homes without signs of gentrification. The evidence suggests that home loans are a driving factor in neighborhood revitalization and reducing prevalent crimes.

Keywords: home loans, neighborhood crime, community investments, shift-share instrument variables

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1 Introduction

Community investments rather than punishment have been proposed as an alternative strategy to reduce neighborhood crime (Krivo, 2014; Sharkey, 2018; Vélez and Lyons, 2014). Public and non-profit programs targeting high-risk individuals and areas with a clear nexus to crime-reducing components have shown promising results (Blattman et al., 2017; Branas et al., 2018; Chalfin et al., 2022; Heller, 2014; Sharkey et al., 2017). The role of private investments in reducing crime is unclear due to a misalignment between private and social benefits and liquidity constraints. Banks can help to overcome such barriers and play a role against crime by bringing external resources to the neighborhoods (Vélez and Richardson, 2012; Velez et al., 2012). They are a source of credit for myriad activities (Allen et al., 2008). One of them is providing home mortgages.

Home acquisition is a cornerstone component to accumulate wealth (Turner and Luea, 2009) and influence neighborhood dynamics. Landowners' well-being and wealth are linked to the prospects of the property. Actions that increase the value of the neighborhood benefit the household, so individuals have strong incentives to influence local regulations (Molotch, 1976). Despite the reduction in geographical mobility, homeownership relates to increases in housing tenure, local networks, and social capital investments (DiPasquale and Glaeser, 1999). Furthermore, as neighbors become aware of their common values and there is mutual trust, solidarity, and willingness to intervene for the common good, informal social control mechanisms can regulate the community's behavior, leading to crime decreases (Sampson et al., 1997; Sampson and Raudenbush, 1999).

The lack of access to credit to acquire property or improve the existing one can become a source of racial disparities, wealth inequality, and neighborhood decay (Krivo and Kaufman, 2004). Nevertheless, aggressive, high-risk lending practices are not an optimal solution (Kubrin et al., 2011). In the short-term, high-risk loans can improve property values (Pavlov and Wachter, 2011). In the long-term, they can harm communities once homeowners cannot make regular payments, and foreclosures and vacant properties rise, leading to more crime (Cui and Walsh, 2015; Ellen et al., 2013). Risky lending practices can destabilize local and global economic markets.[1] Lending to creditworthy individuals can be a path moving forward to promote safer neighborhoods.

Previous research argues that home mortgages reduce violent crime with an emphasis on homicides. The literature includes persuasive descriptive studies, but they are mostly correlational (Kirk, 2020; Saporu et al., 2011; Shrider and Ramey, 2018; Vélez and Richardson, 2012) or have not used strong identification

[1] For a broader explanation on global effects of widespread risky lending patterns, see the *Final Report of the National Commission on the Causes of the Financial and Economic Crisis in the United States*.

strategies to remove the endogeneity bias (Veléz, 2009; Velez et al., 2012). These studies do not address the concern that areas receiving home mortgages differ in observable and unobservable characteristics from neighborhoods receiving fewer loans; hence, the crime differences could be caused by other factors besides lending. Bunting (2020) uses the only credible instrumental variables research. Still, the author does not analyze the effects on the different crime categories, which is critical to assess whether loans influence property, violent, and low-level crimes, nor provide evidence of heterogeneous effects on racial or ethnic communities. Moreover, the study only focuses on Los Angeles County, California, during the Great Recession, limiting the external validity of the results.

Accordingly, this research makes five contributions. First, using a shift-share instrumental variables approach reduces the concerns of not isolating the effects of mortgages on crime and provides a stronger identification strategy than previous criminological studies. Second, this research offers insights into which crimes are the most sensitive to residential lending by examining the results on property, violent, and low-level criminal offenses rather than only focusing on violent offenses. Third, it investigates whether there are differential impacts in historically marginalized communities, a relevant margin that most studies have overlooked.[2] Also, it helps to understand which communities benefit most from community investments. Fourth, this research collects incident-level data from 27 major US cities, increasing the statistical power and reducing concerns about external validity, a common issue in single-city case studies. Finally, it covers the post-Great Recession period marked by stringent financial regulations on the mortgage housing market and closer to the current conditions.

This research relies on crimes reported to the police departments and residential lending data and a shift-share instrumental variables approach. It leverages the time and spatial variation caused by banks' idiosyncratic mortgage shocks with different market shares across communities to assess whether differential exposure to lending leads to different crime changes. To understand the instrumental variable, let's use two banks (PNC and Citibank) as examples. Both banks likely have similar knowledge of their local market, but one had a nationwide yearly decrease in home loans. At the same time, the other experienced a large increase, likely due to corporate management and conditions unrelated to local crime. The specification compares census tracts where these banks operate but have different yearly changes in nationwide lending. These different nationwide shocks are a key source of exogenous variation in local lending, allowing for the estimation of its causal effect on neighborhood crime. Instead of using only two banks, this research uses 1,118 banks to leverage variation across space and time.

[2] Only one study has examined the impacts of residential lending on property crimes and its differential effects based on race and ethnicity (Saporu et al., 2011). Still, given its cross-sectional design, one cannot assign a causal order to the mortgage-crime relationship.

The evidence suggests that mortgages reduce crime, specifically theft, burglary, aggravated assaults, and non-major crimes, although they lead to motor vehicle theft. Overall, property and major crime decreases when banks lend mortgages to individuals. The impacts are larger in Black, Hispanic, and poor communities, suggesting that historically marginalized neighborhoods benefit more from an increase in mortgage lending. A decrease in vacant homes partially drives the effects, and no discernible gentrification changes are detected, measured through sociodemographic changes. The results suggest that private investments can act as crime prevention strategies without affecting current residents. Home mortgages drive neighborhood revitalization and prevent acquisitive crimes. Still, they are insufficient to decrease serious violent crime. Other alternatives are needed to deter such criminal offenses.

The remaining article is organized as follows. Section 2 reviews the literature on lending and crime. Sections 3 and 4 explain the data and empirical strategy. Section 5 presents the results, and Section 6 concludes.

2 Background

2.1 Mortgage market in the US

Banks contribute to solving asymmetric information problems and sharing risks between lenders and borrowers by acting as intertemporal smoothing institutions (Allen et al., 2008). Some people deposit money, while others ask for loans. The economic relevance of these activities is substantial: banks' deposit money assets represent 62 percent of the US GPD, and consumer loans and mortgages 82 percent.[3]

Some features of the US market stand out from other jurisdictions. First, the US has a strong presence of government-sponsored agencies in the mortgage market. These institutions influence the secondary mortgage market, where lenders and investors sell and buy loans.[4] The public funding allows banks to benefit by selling loans to public institutions with a lower capital-to-assets ratio than private banks, making lending less expensive than their European counterparts. Second, mortgage lending involves design, selling, marketing, packaging, managing, and funding the loan and risk and delinquency management. The US mortgage lending process is spread across several institutions, offering a competitive advantage that translates into a larger lending market (Coles and Hardt, 2000). Third, most loans have a fixed rate

[3] See https://fred.stlouisfed.org/series/DDDI02USA156NWDB and https://fred.stlouisfed.org/series/HDTGPDUSQ163N

[4] For example, the Government National Mortgage Association (Ginnie Mae), created in 1968, guarantees pools of loans from mortgage banks and is backed by the US government. The Federal National Mortgage Association (Fannie Mae), also founded in 1968, and the Federal Home Loan Mortgage Corporation (Freddie Mac), created in 1970, securitize (e.g., sell a pool of loans) mortgages to provide liquidity and stability to the housing market.

and no-fee prepayment options. This feature runs the risk of shortening the mortgage duration, avoiding paying interest on the principal and increasing the uncertainty in the market. US banks hedge against this volatility by selling the loans in secondary markets and sharing the risks with other investors. The increased demand for these financial instruments provides liquidity and lowers the funding costs, even during financial distress periods (Green and Wachter, 2005).

The characteristics of the US mortgage market contribute to increasing its size, liquidity, and widespread use nationwide. While these differences would not affect the underlying driving mechanisms of lending on neighborhood dynamics, they likely influence its magnitude and ability to promote localized investments and influence communities.

2.2 Why would mortgages impact crime?

Acquiring or renovating a property requires large upfront investments that are usually out of reach unless credit is available. Banks are key institutions influencing neighborhood dynamics by facilitating these private investments. However, banks do not randomly lend money across communities; they respond to incentives and the institutional context. Banks can react positively to national laws, such as the Community Reinvestment Act, encouraging lending in low-income communities without increasing the delinquency rates (Avery and Brevoort, 2015; Bhutta, 2011; Ding and Hwang, 2020).[5] There is also evidence of banks withholding loans to credit-worthy individuals in minority prevalent areas (e.g., redlining), negatively impacting communities and residents (Aaronson et al., 2021a,b; Anders, 2023; Appel and Nickerson, 2016; Faber, 2020; Jacoby et al., 2018; Mitre-Becerril, 2024). Likewise, banks' corporate decisions to close local branches can reduce small business lending and employment growth and increase property crimes (Garmaise and Moskowitz, 2006; Nguyen, 2019).

Intra-neighborhood dynamics, such as social disorganization and collective efficacy, can also explain the role of mortgages on public safety. Local friendship networks may decrease crime by strengthening social controls and mutual trust and facilitating well-organized communities (Sampson and Groves, 1989). Residential stability creates a stronger attachment to the neighborhood (Morenoff et al., 2001; Sampson et al., 1997). Home loans encourage stability by limiting migration to other areas (DiPasquale and Glaeser, 1999), particularly during economic downturns (Modestino and Dennett, 2013).

Crime can decrease via changes in the housing tenure. Homeownership can reduce crime, particularly burglaries and robberies, by motivating behavioral changes rather than altering the sociodemographic

[5] Other studies argue that the Community Reinvestment Act does not affect banks' behavior (Bostic and Lee, 2017; Dahl et al., 2002).

composition of the community (Disney et al., 2023). Owning a home can influence the perception of crime and disorder. It is associated with a decreased fear of crime and perceptions of disorder, though longer periods of residence in the community may relate to a higher fear of crime (Lee et al., 2022; Lindblad et al., 2013). Moreover, if homeowners occupy vacant or foreclosed properties rather than replacing existing tenants and decrease the prevalence of blighted properties in the community, crime should decrease (Branas et al., 2018; Hohl et al., 2019; Kondo et al., 2015).

While mortgages could make neighborhoods safer, it does not necessarily imply that all residents would benefit. A housing boom could cause property price and rent increases and displace long-term, low-income, minority-prevalent residents, leading to new tenants with a higher socioeconomic background. Homeowners facing liquidity constraints could face problems paying a higher property tax. Renters may also find it too expensive to pay higher prices. Although homeowners' out-migration may not happen when accompanied by targeted tax relief programs (Ding and Hwang, 2020) and there is disagreement on whether gentrification induces displacement, it is still a concern in assessing neighborhood changes (Zuk et al., 2018). From a criminological perspective, studies point toward a negative relationship between crime and gentrification (MacDonald and Stokes, 2020; Papachristos et al., 2011). Accordingly, identifying who benefits from increased mortgages and neighborhood revitalization is a relevant research and policy question.

Home loans can also encourage opportunities for crime. Mortgages can go towards property improvements, signaling the availability of high-value goods, attracting potential offenders, and increasing crime. People can refinance an original loan with new conditions, such as longer terms or lower interest rates, freeing money for other activities. This situation could increase crime opportunities so that homeowners become a potential target as they expand their spending patterns.

Neighborhood revitalization may not mechanically translate to fewer crimes across the board. Some crimes, like auto theft, are pro-cyclical so that they increase as the local economic activity improves (Bushway et al., 2012; Cook and Zarkin, 1985). Finally, mortgage indebtedness could bring psychological distress, driven by fears of being unable to keep up the mortgage payments or cash constraints, particularly during difficult economic periods (Cairney and Boyle, 2004). This additional psychological burden could lead to more antisocial and criminal behaviors due to the negative stimuli and a mismatch between available means and aspirational goals (Agnew, 1992; Merton, 1938). In summary, the net effect of homeownership on crime is uncertain. Empirical evidence is needed.

2.3 Do previous studies identify a causal relationship?

Previous research finds a negative association between residential lending and violent crime. Kirk (2020) pools three-year data into a single period and controls for collective efficacy, spatial autocorrelation (e.g., the mean value of adjacent areas), and standard sociodemographics, finding that mortgage denials increase violent crime. Vélez and Richardson (2012) use a similar method, showing a decrease in homicides. Saporu et al. (2011) also pools three years of data and accounts for the dependence of observations in nested units (e.g., tracts nested in cities), finding decreases in violent and property crimes, with larger benefits in Black and Latino communities. Shrider and Ramey (2018) use longitudinal data and a random-effects model finding a significant decrease in violent crime in areas providing more mortgages.

These studies provide relevant descriptive patterns. Nevertheless, they do not address the endogeneity biases arising from common causes and omitted variables. Reverse causality is a serious concern as mortgages influence crime, and crime also affects lending. Criminal activity impacts property prices negatively (Dealy et al., 2017; Gibbons, 2004; Lens and Meltzer, 2016); one of the reasons real estate agencies use crime information to guide customers on their decision to buy a property.[6] Banks are unwilling to lend money for properties that, in case of default, would find challenging reselling. Another concern is not accounting for unobserved variables that affect crime and mortgages simultaneously (e.g., omitted variable bias). For example, neighbors willing to intervene in the collective good is a malleable, dynamic process (Hipp, 2016), so crime differences could be due to communities' idiosyncratic preferences and not due to home loans. One can argue similar to an extensive list of unobserved variables (e.g., social networks, individual preferences, risk attitudes, social capital). These unobserved confounders mean self-selection in the intervention.

Previous research does not address these endogeneity biases. Their results are likely confounding variables for the effect between mortgages and crime. Cross-sectional studies have no temporal or spatial exogenous variation (Kirk, 2020; Saporu et al., 2011; Vélez and Richardson, 2012), and they unlike can establish a causal relationship. Research using random-effects models does not solve this problem either (Shrider and Ramey, 2018) because it assumes that the unobserved heterogeneity and the primary variable of interest are uncorrelated, which is a likely unrealistic assumption. The violation of this assumption is why quasi-experimental design models are used to obtain a causal relationship.

Instrumental variable studies are a more appropriate method to overcome the endogeneity biases. Veléz

[6] In 2021, several real estate listing websites stopped providing crime data due to concerns that this practice perpetuates racial inequality. See https://magazine.realtor/daily-news/2021/12/16/realtorcom-redfin-remove-crime-data-on-listings

(2009) and Velez et al. (2012) find a negative effect of mortgages on violent crime, particularly homicides, using an instrumental variables approach. Bunting (2020) also uses an instrumental variables model, finding that mortgages reduce major crimes but do not distinguish between property and violent crime and its subcategories. However, some of these instruments are likely correlated with the omitted variable or error term, which could lead to a greater bias than an ordinary least squares estimation (Angrist and Krueger, 2001). Specifically, Veléz (2009) use the age of the housing stock in a census tract as an instrument, but this variable relates to mortgages and crime through other channels. Older properties could result from residents organizing against new developments either via a *not in my backyard* movement, homeowners associations, or pushing for a historic designation, among others, to impose residential limitations and add a property price premium. Likely, these areas also attract additional private and public investments in law enforcement, affecting crime through other mechanisms beyond mortgages. Finally, older properties could mean an area is socially disadvantaged, which could affect crime through mechanisms besides mortgages (e.g., under-policing).

Velez et al. (2012) apply a first-differences transformation (e.g., $\Delta x_{it} = x_{it} - x_{it-1}$) and instruments the endogenous variable with its past levels (e.g., Δx_{it-2} as instrument for Δx_{it-1}). This approach became common in the literature some time ago. Still, it is problematic because it assumes that the lagged values of the independent variable are uncorrelated with the differenced error term (Angrist and Krueger, 2001). This assumption is unlikely to hold if the error terms are serially correlated, a common issue in panel data, violating the assumption that the instrument is uncorrelated with the omitted variable.[7]

The only well-identified instrumental variable uses a shift-share instrument (Bunting, 2020). While the study provides credible evidence that mortgages reduce major crimes, it does not distinguish between property and violent crime and its subcategories, making it difficult to compare the results to previous studies. Furthermore, it does not examine any heterogeneous impacts in minority prevalent areas, which is a relevant margin to analyze, nor provides evidence on the potential causal mechanisms driving the impacts. This manuscript addresses those concerns.

[7] See Reed (2015) for a detailed explanation of the problems of using lagged variables as instruments. Testing a non-significance relationship of the residuals with the problematic variable is insufficient to show that the exclusion restriction holds. The residual is not the same as the error term. The residual is the difference between the observed and model's predicted values ($\hat{u}_{it} = \hat{y}_{it} - y_{it}$), while the error term is unobservable and it is part of the population model (e.g., $u_{it} = y_{it} - \beta x_{it}$). The exclusion restriction is a non-testable assumption guided by theory.

3 Data

3.1 Data sources

There is no national repository of crime incidents at the census tract level. The common crime data source is the Federal Bureau of Investigation's Uniform Crime Reporting (UCR), replaced in 2021 with the National Incident-Based Reporting System. Their smallest geographical breakdown is the agency level, which usually matches a city, town, or county. This data source is not appropriate for understanding the crime effects at the sub-city level. To overcome this data limitation, this research hand-collected time-stamped crime incident information from 27 of the most populated US cities, representing 33.3 million people or about 10 percent of the US population. These cities were chosen based on having available crime data from the last decade.[8] Some cities only publish the address of the incident rounding the locations to the nearest hundred block or blurring the address' last two digits; such cases were replaced with a five-zero number (e.g., 12XX Street Name became 1250 Street Name) and three geocoders (US Census geocoder, ArcGIS Online Geocoding Service, and the Nominatim OpenStreetMap search engine) sequentially attempted to geocode the address. The geocoding hit rate was above the minimum acceptable hit rate (Ratcliffe, 2004). To ensure accuracy in the geocoding and data aggregation processes, the crime incidents were compared to the UCR dataset. The crimes matched well in levels and trends. The incidents were categorized as major and nonmajor crimes. Major crimes include murder, robbery, and aggravated assault, comprising violent crimes and burglary, theft, and motor vehicle theft, forming property crimes. Non-major crimes are all other offenses reported to the police.

The home loans come from the Home Mortgage Disclosure Act (HMDA) data collected by the Consumer Financial Protection Bureau. It requires financial institutions to report and disclose de-identified mortgage-level data. In October 2015, there was a change in the legislation, increasing the data fields reported in the HMDA data and changing the financial institution identifier for all data collected in 2018 and onward. This study uses data up to 2017 to avoid introducing measurement errors in the analysis.[9] The mortgages consider the originated loans (e.g., excludes loans purchased by the financial institution in the secondary

[8]Some cities release data from the mid-2000s, but 2011 is the first year when most cities have complete information, so this year was chosen as the beginning of the study period. **Appendix Figure A.1** shows the geographical distribution of the cities included in this study: Atlanta, GA; Aurora, CO; Austin, TX; Buffalo, NY; Chicago, IL; Cincinnati, OH; Columbus, OH; Houston, TX; Indianapolis, IN; Kansas City, MO; Los Angeles, CA; Louisville, KY; Memphis, TN; Mesa, AZ; Milwaukee, WI; Minneapolis, MN; New Orleans, LA; New York, NY; Orlando, FL; Philadelphia, PA; Pittsburgh, PA; Raleigh, NC; San Francisco, CA; Seattle, WA; St. Louis, MO; Tucson, AZ; and Washington, DC.

[9]The 2015 Home Mortgage Disclosure Regulation C amendment explains such changes, available at `https://www.federalregister.gov/documents/2015/10/28/2015-26607/home-mortgage-disclosure-regulation-c`. Pre-2018 data is stored at `https://www.consumerfinance.gov/data-research/hmda/`, while post-2018 data is available at `https://ffiec.cfpb.gov/data-publication/`. Merging pre and post-2018 data would cause mismatches (e.g., typos in identifiers).

market) for single-family properties (e.g., excludes manufactured housing and multifamily loans). The mortgage's purpose can be home purchase, improvement, or refinancing. The first two loan types focus on new investments in the community (acquiring or renovating a property). Refinancing could lead to lower monthly mortgage payments, increasing the households' cash liquidity.

The analysis includes socioeconomic and demographic variables collected from the American Community Survey (ACS). It considers the five-year census tract-level estimates on the percentage of Black, White, and Hispanic population, age groups (below 14, 15-24, 25-39, 40-54, and over 55 years old), schooling attainment (less than high school, high school, some college, and college education), the unemployment and poverty rates, and the number of vacant and occupied properties.

The HMDA and ACS data come at the census tract level. In 2012, the Census Bureau updated its geographical boundaries, as it does every decade. This process usually means partitioning high-populated tracts. The pre-2012 data was apportioned to the new boundaries using the relationship files published by the Census Bureau.[10]

3.2 Analytical database

Table 1 presents the descriptive statistics in selected years (2011, 2014, and 2017) for the 7,810 census tracts included in the study.[11] The average census tract experienced a decrease of 13 percent in non-major crimes between 2011 and 2017. The reduction in major crimes was more muted (7 percent), as aggravated assaults and thefts showed no decrease during these years. Property crimes are almost five times more common than violent ones. Theft is the most recurrent crime reported to the police, followed by burglary, and, in third place, aggravated assault and motor vehicle thefts are equally likely. The mean census tract experienced less than one murder per year, but the large standard deviation suggests that homicides have a skewed distribution (e.g., most areas experience no such crime). Overall, the crime distribution is consistent with national crime data.

The average census tract had an increase in mortgages. The number of approved loans moved from 43 to 60 per census tract year, while the total monetary amount went from 12.1 to 19.7 million dollars in the neighborhood. Consequently, the loan amount per mortgage increased from 277 to 328 thousand dollars between 2011 and 2017. These numbers mean a yearly growth rate of 4.6, 7.2, and 2.4 percent for the number of approved mortgages, monetary value, and amount per loan. These annual growth rates relate

[10] See https://www.census.gov/geographies/reference-files/2010/geo/relationship-files.html.

[11] Seattle, WA and San Francisco, CA do not report georeferenced murders. Similarly, Atlanta, GA, Houston, TX, Indianapolis, IN, Mesa, AZ, Minneapolis, MI, and Washington DC do not report non-major crimes. The sample size decreases for these outcomes relative to the other crimes.

to a stronger mortgage and housing market that took some time to recover after the Great Recession.

The census tract sociodemographics remained stable over these seven years, suggesting no compositional changes. Census tracts have nearly four thousand residents. Most of them identify as White (51%), and a quarter of them as Black (27%) or Hispanic (24%). These characteristics are consistent with cities being more racially and ethnically diverse than the rest of the country. Individuals in their prime age (25 to 54 years old) represent nearly 45 percent of the population, while teenagers and young adults (15 to 24 years old) account for 15 percent of the tracts' residents. Almost 40 percent have a college degree or higher, while fewer than 20 percent have less than a high school diploma. These characteristics resemble the country's sociodemographics.

The unemployment rate ranged between 10.9 and 8.5 during these seven years. In 2011, it was similar to the national average but higher in 2014 and 2017 (the national unemployment rate was 7.2 and 5.7 in 2014 and 2017). The poverty rate was consistently above that in these 27 cities, about six percentage points higher than in the rest of the US (17 vs 11 percent). Finally, the mean census tract experienced a marginal rise in occupied housing units of 4.7 percent between 2011 and 2017 (a half-percent yearly change) and a decrease of about 8 percent (1.1 yearly percent growth) in vacant properties. These numbers show that the average census tract had a crime reduction and an increase in mortgages. Assessing whether this relationship is causal is the main objective of this research.

4 Empirical strategy

4.1 Econometric model

Estimating the causal effect of mortgages on crime is challenging due to unobserved confounders creating an endogeneity bias. Pooled or random effects models do not provide causal estimates. One plausible approach for estimating the relationship between mortgages and crime is a fixed-effects model, like Equation (1), regressing crime, y_{it}, on home loans, L_{it}, in tract i and year t, controlling for a vector, X_{it}, of time-variant, observed sociodemographic variables. To account for time-invariant, tract-specific unobserved variables (e.g., stable neighborhood preferences about housing and crime) and time-varying, tract-invariant confounders (e.g., national yearly economic shocks), the model also includes census tract, γ_i, and year, μ_t, fixed effects.

$$y_{it} = \gamma_i + \mu_t + \beta L_{it} + X_{it}\alpha_X + e_{it} \tag{1}$$

Despite adding controls and fixed effects, Equation (1) may not provide causal estimates because time-varying, unobserved factors, such as collective efficacy, are not accounted for in the model that influences crimes and loans simultaneously. Other models are needed to overcome the concern of endogeneity.[12] An instrumental variable approach is a prime candidate to eliminate the endogeneity bias by only using the variability in mortgages that is uncorrelated with the omitted variable bias (Angrist and Krueger, 2001). A Bartik or shift-share instrument is appropriate given the institutional context. This method leverages the presence of multiple banks in a census tract and the banks' idiosyncratic lending patterns following corporate decision-makers and national trends likely uncorrelated in time and place with local crime changes.

The shift-share instrument has two components. The shift, g_{ikt}, is the nationwide growth in mortgage loan amount by bank k between year t and $t-1$, excluding loans in city j where tract i is located. The empirical design assesses whether differential exposure to common shocks relates to differential changes, so a growth rate rather than levels is preferred (Goldsmith-Pinkham et al., 2020). As it is customary in the shift-share literature, this research uses a symmetric growth rate calculated as $(L_{it} - L_{it-1})/(0.5 * L_{it} + 0.5 * L_{it-1})$, so the values range between -2 and 2. This formula has the advantage of being symmetric, additive, bounded, and handles changes increasing from a zero baseline (Törnqvist et al., 1985).

The share, s_{ikt_0}, is the proportion of mortgage loan amount of bank k in tract i and year t_0, so it ranges from zero to one. Fixing the shares to a specific time, usually a pre-study period, is common in this research design. For this research, it was defined $t_0 = 2007$, which is one year before the Great Recession, when atypical economic changes happened in the banking industry. By fixing the shares to one period, the method relates to a difference-in-differences with a single cross-sectional variation difference used in the research design (Goldsmith-Pinkham et al., 2020). The shift-share instrument, Z_{it}, is the inner product of the nationwide bank component of the mortgage growth rate and the bank-tract shares. Formally, it is defined as Equation (2):

$$Z_{it} = \sum_{k=1}^{K} s_{ikt_0} g_{ikt} \tag{2}$$

Once the instrument is built, the model uses the standard two-stage least squares regression method. The first stage follows Equation (3):

$$L_{it} = \gamma_i + \mu_t + \beta_1 Z_{it} + X_{it}\alpha_X + e_{it} \tag{3}$$

[12] Research studying whether banks increase lending in census tracts facing a closer inspection from regulatory agencies than in comparable areas has used a regression discontinuity design leveraging the discontinuous threshold of the Community Reinvestment Act eligibility status (Avery and Brevoort, 2015; Bhutta, 2011; Bostic and Lee, 2017; Ding and Hwang, 2020). A preliminary examination of this model in these 27 cities suggested a significant change in mortgages but failed basic robustness checks (e.g., alternative thresholds). Consequently, this design is not warranted for this analysis.

where L_{it} is the logarithm of the mortgages loan amount in tract i and year t, and X_{it}, γ_i, and μ_t are sociodemographic controls and fixed effects. The reduced-form specification follows Equation (4):

$$y_{it} = \gamma_i + \mu_t + \beta_2 Z_{it} + X_{it}\alpha_X + e_{it} \tag{4}$$

where y_{it} is the logarithm of crimes in tract i and year t. Finally, the second stage or instrumental variable specification follows Equation (5):

$$y_{it} = \gamma_i + \mu_t + \beta_3 \hat{L}_{it} + X_{it}\alpha_X + e_{it} \tag{5}$$

where γ_i, μ_t, and X_{it} are defined as previously, and $\hat{L}_{it}$ is the predicted growth rate of the mortgage loan amount in tract i and year t based on the first stage in Equation (3). The standard errors are clustered at the census tract level. Some outcomes, particularly homicides, have zero incidents in any given year-tract; the inverse hyperbolic sine function was used instead of the logarithm function for all outcomes. This transformation approximates to $\log(2y)$, and it can be interpreted in the same way as a standard logarithmic dependent variable (Burbidge et al., 1988). The robustness checks use alternative functional forms.

The shift-share instrument model became common in urban, regional, and international trade economics since Bartik (1991) examined the impacts of state and local policies on job growth.[13] Instrument variables based on bank lending data have been used previously to assess the effect of credit market shocks in the real economy (Abras and de Paula Rocha, 2020; Greenstone et al., 2020). In the crime literature, the shift-share instrument method has been used to examine the public safety effects of mortgages (Bunting, 2020), migrations waves (Dehos, 2021), labor market shocks (Dell et al., 2019; Ghosh, 2018; Gould et al., 2002), gun ownership (Billings, 2020), and stop, question, and frisk strategies (Weisburd et al., 2016).

The shift-share instrument has features similar to the instrumental variable methods used in the crime literature. For instance, the shifts are built using the national lending made by a bank excluding the loans in the city of interest, which is similar to the leave-one-out average sentence approach used in the judge instrumental variable studies aiming to assess the impact of incarceration on recidivism (Aizer and Doyle Jr, 2015; Loeffler and Nagin, 2022). Likewise, historical population shares have been used as instrumental variables to predict future population concentrations but theoretically independent from

[13] Bartik (1991) was not the first one using this approach. Still, the author popularized this method and explained its logic, carrying the author's name (Broxterman and Larson, 2020; Goldsmith-Pinkham et al., 2020). A Google Scholar search of the terms *Bartik instrument* or *shift-share instrument* returns more than 1,800 results. While not all hits probably use this instrument, it signals the widespread use of the method in the literature.

current crime rates to examine the impact of immigration on neighborhood crime (MacDonald et al., 2013).

4.2 Building the instrumental variable

This research identifies 1,118 banks offering mortgages in the 27 cities included in the study between 2007 and 2017. These banks are used to build the Bartik instrument. **Appendix Figure A.2** shows that these banks jointly cover practically all US counties. The mean (median) county has 81 (96) out of the 1,118 banks. These financial institutions have widespread coverage across the US. To understand the motivation for using the shift-share design, this section examines the shift and share components of the instrumental variable.

The first component of the instrumental variables is the shifts. The shift, g_{ikt}, is the nationwide mortgage loan growth by bank k between year t and $t-1$, excluding loans from city j where census tract i is located. The nationwide growth rate comes from the loans occurring outside the cities of interest for this research. The shifts are likely exogenous to the crime incidents in the census tracts included in the analysis. The exclusion restriction would be violated if the crime incidents in the 27 cities affect lending in the rest of the country. It is unlikely that this situation is the case. Bank lending depends on the local market and is geographically close to the lender (Nguyen, 2019). Lending is also contingent on the decisions set by the central banking system (e.g., the Federal Reserve System for the US). A tight monetary policy translates into higher lending costs for banks, and they transfer such costs to consumers by setting higher interest rates on loans, decreasing their demand (Chopra, 2022).[14]

Moreover, lending depends on the banks' idiosyncratic strategies and management decisions (e.g., CEO's leadership, advertising strategies, human resources management, client management, etc.). These characteristics influence the banks' revenue and cost strategy and how much resources they will lend to consumers and expect to recover successfully. **Figure 1**, Panel A, shows the mean national mortgage growth (excluding the 27 cities) of the 1,118 banks between 2011 and 2017. There is considerable variation between banks. The average bank increased its mortgage lending amount by 4.2 percent annually. Some financial institutions experienced yearly decreases, while others experienced increases. Panel B visualizes substantial differences within banks across time. Practically, all banks experienced positive and negative yearly changes over the seven years of study (2011-2017). This pattern is consistent with the national trend in mortgages associated with changes in interest rates. This temporal variation across different banks forms the shifts of the instrumental variables.

[14] **Appendix Figure A.3** shows a negative correlation between the national mortgage loan amount and the US Treasury market yield. It also shows that the change in mortgages in the 27 cities and the rest of the country has followed the same trend during the last decade.

The second component of the instrumental variables is the shares. **Figure 2** shows the distribution of the 1,118 bank's mortgage share per census tract in 2007, the base period (s_{ikt_0}). It presents the experience of a typical bank in a census tract. The average (median) bank has 6.1 (2.5) percent of the share of the census tract mortgage loan market. Nearly 90 percent of the banks have below 15 percent of the local share. In contrast, 1.2 percent of the banks have more than 51 percent of the neighborhood market. To understand the situation of a typical census tract, **Appendix Figure A.4** aggregates the 2007 banks' share at the census tract, $s_{it_0} = f_k(s_{ikt_0})$, to estimate the mean, median, and maximum bank share per census tract. Panel A shows that banks have 13 percent of the neighborhood market share in the average census tract. This pattern is confirmed in Panel B, showing that in the median census tract, banks have a 9 percent local market share. The differences between the mean and median typical census tract suggest a slightly skewed distribution. Panel C confirms this situation by plotting the distribution of the maximum market share of a bank per census tract. The leading bank has one-third of the local market share in the typical census tract.

While one bank usually has one-third of the local mortgage market, the remaining share is scattered across other financial institutions. Specifically, the mean (median) census tract has 14 (16.5) banks offering mortgages. In most census tracts, people can choose from many financial institutions to obtain a home loan. This result should not be surprising as people look for mortgages online or visit several banks scattered around the city. Banks are unlikely to have strong market power at such a small geographical level. The large number of shares across tracts is one of the sources of variation that the shift-share instrumental variables approach leverages (cross-sectional variation).[15]

5 Results

5.1 Main results

To assess the impact of mortgage lending on crime, this section first presents the specifications used in previous studies that do not address the selection bias: random effects, fixed effects, and first differences using the lag of the independent variable as an instrument. **Table 2** Panel A shows that the random effects model suggests increases in property crimes, and similar to previous correlational studies, it also finds significant reductions in murders and aggravated assaults. Panel B shows the fixed effects model, finding a positive relationship between mortgages and property crimes. Murders and aggravated assault

[15] **Appendix Figure A.5** shows there is a positive correlation of the banks' tract shares across time: having larger shares in 2007 relates to higher shares in 2010, although there is a considerable unexplained variation.

have a negative, non-significant correlation with residential lending, while a rise in robberies drives the increase in violent crime. Finally, the first difference with the lagged of the independent variable as an instrument, Panel C, shows significant decreases in property and violent crimes. A reduction in robberies drives the decrease in violent crimes. None of the three models show significant effects on non-major crimes. These models show different results, as it is unlikely they address the endogeneity biases of reverse causality and omitted time-variant confounders.

A more appropriate model to identify the causal effect is using a shift-share instrumental variables approach. **Table 3** shows that the instrumental variable –meaning the inner product of the banks' tract share and the banks' national growth rate outside of the city where the tract is located– strongly predicts the census tract mortgages. Including covariates in the model does not change the result. The coefficients imply that a ten percent growth in the mortgage outside of the 27 cities relates to a 2.3 percent change increase in the census tract mortgages ($(e^{\beta_1} - 1)/10$ percent). The estimate is similar to Bunting (2020), suggesting that the relationship holds in other jurisdictions. The F-statistic is well above the common threshold level (Stock et al., 2002). These results suggest that the instrumental variables model is strongly associated with local mortgage changes; hence, the relevance condition holds.

Table 4 presents the reduced form and the second stage least squares (or instrumental variable) estimates. They differ in magnitude as the instrumental variable estimate equals the reduced form coefficient divided by the first stage (the scaled version of the reduced form). The second stage least squares results suggest that a 10 percent increase in the mortgage loan amount relates to a 1.1 percent reduction in major crimes, driven mainly by a 3.0 and 1.7 percent decrease in theft and burglary (changes estimated as $(e^{\beta_1} - 1)/10$ percent). Motor vehicle thefts experience a 5.1 percent increase when there is a 10 percent change in neighborhood mortgages, probably related to more crime opportunities and population movement. This result is consistent with these crimes being pro-cyclical with the economy (Cook and Zarkin, 1985; Bushway et al., 2012). Violent crime shows a negative, non-statistically significant decrease. Aggravated assault shows a significant 2.4 percent decrease for every 10 percent increase in lending. Murders and robberies have no statistically significant changes, which is a finding different from previous studies that argue that mortgages reduce homicides. Finally, there was also a significant decrease in non-serious crimes, 1.7 percent.

5.2 Robustness

This section assesses the robustness of the results to different analytical decisions taken in the research process. **Figure 3** presents the second stage least squares estimates and confidence intervals for alternative specifications. **Appendix Figure A.6** shows the robustness checks for the reduced form estimates. The first concern is that the inverse hyperbolic sine transformation of the data drives the results. The models are also estimated using the $\log(y+1)$ as an alternative functional form. This function also avoids excluding outcomes with zero crimes, likely in small populated areas or for rare outcomes. The decision to add one is arbitrary, though common in the literature. The results hold for this alternative functional form.

The main specification estimates the crime outcomes using counts rather than rates. Counts and rates may not reflect the same victimization risk, particularly when residents are used to computing the rates (Massenkoff and Chalfin, 2022). This situation is a concern in areas with many transient visitors and pedestrians (e.g., touristic places or commercial areas) but few residents. Counts are preferred to rates at small geographical levels, such as neighborhoods, given the intra-city movement of people. Still, using crime rates rather than counts leads to the same results.

A third concern is whether the results are sensitive to give more importance to densely populated areas. Two models were estimated to test this concern. One model weights the observations by the number of housing units in the census tract. It gives more importance to areas with a larger potential for receiving mortgages, as they have more residential properties. A second model weights the observations by the population living in the census tract, giving more importance to places with more potential victims. Both alternatives provide the same conclusions.

Next, one could be concerned that cities could have idiosyncratic regulations affecting the housing market. The model adds city-fixed effects to compare census tracts within cities to remove time-invariant, city-specific factors affecting neighborhoods. Moreover, cities may experience different trajectories regarding how they implement regulations. A second model adds city-specific time trends to control for such differences. The two models provide the same results as the main analysis. A similar concern could be raised at the state level. This is particularly relevant as banks are also regulated by state governments. Accordingly, a robustness check adds state-fixed effects to compare census tracts within the same state. Another model includes state-time trends to account for different trajectories of states across time. Adding time trends makes robbery statistically significant. However, only two of the ten alternative specifications for this offense are significant. The significant results could be due to a false discovery rate.

A fifth concern is that some census tracts have banks with a large mortgage market share, so their presence may be correlated with local public safety trends that could influence the results. An alternative instrumental variable was built, excluding banks with more than two-thirds of the local market share, so the inner product of the shifts and shares only considers banks without a dominant market concentration. Burglary is now imprecisely estimated, but everything else has practically the same magnitude and statistical significance.

A final robustness check assesses whether the results are sensitive to excluding banks regardless of their market share. **Appendix Figure A.7** provides a leave-one-out estimator. It builds the instrumental variable, excluding twenty banks at a time, and estimates the regression model, repeating the process 50 times. Some banks affect the precision of the estimates. Nevertheless, the sign and magnitude of the coefficients are similar between the specifications and the main results.

5.3 Heterogeneity

The main results show that increasing mortgages reduces crime, particularly theft, burglary, aggravated assaults, and low-level offenses. There are reasons to expect differential effects by racial, ethnic, and concentrated disadvantage levels. For example, credit-worthy racial and ethnic minorities, due to redlining, have been denied loans affecting long-term neighborhood and individuals' life outcomes (Aaronson et al., 2021a,b; Anders, 2023; Appel and Nickerson, 2016; Faber, 2020; Jacoby et al., 2018; Mitre-Becerril, 2024). Residing in disadvantaged neighborhoods affects whether individuals experience discrimination in market transactions (Besbris et al., 2019). Even if there is no discrimination, to the extent that minority-prevalent neighborhoods have lower baseline mortgage levels, a marginal increase could have a larger impact relative to places with widespread credit availability due to non-linear effects.[16]

Table 5 tests for heterogeneous effects by interacting the loan amount with a relevant dimension variable: the proportion of the Black, Hispanic, and below-the-poverty level population in the census tract. Then, it instruments the endogenous variable with the shift-share instrument and its interaction term.[17] The interaction term assesses whether prevalent minority neighborhoods have larger crime changes due to increased mortgages. Panel A suggests that Black neighborhoods benefit more from receiving mortgages than census tracts with no Black residents. The effects are significant for non-major, property –driven by theft reductions– and violent crimes –caused by aggravated assaults decreases. The results imply that a 10 percent increase in the mortgages in tracts with a 50 percent prevalence of Black residents experience an additional 0.9 percent reduction in property crime than tracts with no members of this race group, which experience only a 0.5 percent reduction (computed as $(e^{\beta_1 * 0.5} - 1)/10$ percent). Panel B shows that more mortgages also benefit Hispanic communities. It causes a larger decrease in property –driven by thefts and burglary– and non-major crimes. The increase in motor vehicle thefts is almost twice in Hispanic areas.

[16] A descriptive comparison finds that tracts with over 50 percent of Black (Hispanic) residents receive 17.7 (15.6) fewer million dollars per year or about 126 (53) fewer thousand dollars per loan than their non-Black (non-Hispanic) areas.

[17] The same arguments that support the use of a shift-share instrument (Z_{it}) for mortgages (L_{it}) holds for using $D_{it}Z_{it}$ for $D_{it}L_{it}$, where D_{it} is the relevant heterogeneity dimension variable.

There is a high correlation between minority communities and poverty.[18] Race can intensify inner-city unemployment, poverty, and inequality rates due to historical and current structural disadvantages. Consequently, assessing differential changes due to poverty prevalence in the community is also relevant. Panel C points toward larger marginal impacts of home loans on crime in poor places relative to affluent places. An increase of 10 percent in mortgages results in an additional property crime decrease of 2.8 percent in census tracts with a 50 percentage point difference in poverty rates. The interaction terms of poverty are larger than the differential estimates of the Black and Hispanic populations, suggesting that concentrated disadvantage is more relevant than the ethno-racial neighborhood composition to explain the differential effects of mortgages on crime. This result is consistent with evidence highlighting that economic disadvantage can be more important than race in determining social outcomes (Sampson et al., 2018; Wilson, 2003). There are no heterogeneous effects on homicides and robberies.

The marginal effect of increasing mortgages in places with widespread lending services could differ from those with limited credit access. To assess such heterogeneous effects, **Appendix Table A.1** shows the instrumental variable estimates interacting the mortgage amount with an indicator variable of being on the first, second, or third tercile of the mean local mortgage amount during the study period. The estimates are larger in the bottom tercile than the middle and top distribution groups. Particularly, there are statistically significant differences across tercile groups for property crimes, theft, motor vehicle thefts, aggravated assaults, and non-major crimes. For major crimes, the differences are imprecisely measured (p-value < 0.12), and burglary shows limited evidence of differential impacts among tercile groups. The evidence supports that the impact of mortgages on crime is larger in areas where lending is scarce. Murder and robberies do not show any statistically significant impacts in any of the three tercile groups.

5.4 Potential causal mechanisms

This study shows that mortgages reduce crime, particularly property criminal incidents. This section assesses changes in the neighborhood dynamics to examine plausible explanations for the main estimates. Individuals can buy an occupied home, replacing a renter or a previous homeowner. Alternatively, people could buy a vacant home, meaning a property where no one was living there because it was just built by a construction company, it is a secondary home (e.g., neither for permanent residency nor for rent), or it was foreclosed by a bank, among other reasons. The consequence of occupying and remediating a vacant property is likely a crime decrease as evidence suggests (Hohl et al., 2019; Kondo et al., 2015), which could

[18] Black (Hispanic) prevalence and the percent of poverty in a census tract have a significant correlation of 0.42 (0.29) in the sample used for this study.

be related to having *more eyes upon the street* (Jacobs, 1961).

Table 6 examines this potential mechanism by measuring the effect of mortgages on occupied and vacant units. There are negligible impacts on the occupied or vacant housing stock when using any mortgage type (home purchase, improvement, or refinancing) for building the instrumental variable. However, subsetting the instrument to only mortgages meant for home purchase, the point estimates suggest that a 10 percent increase in mortgages increases (decreases) the occupied (vacant) units by 0.2 percent.[19] The estimates are small, but considering that the housing stock comes from the American Community Survey and not from administrative records, attention should be paid to the sign rather than the magnitude of the estimate. The significant relationship suggests that having more natural surveillance due to fewer vacant units is one of the driving mechanisms causing crime changes.

One concern in expanding mortgage services for residential housing is gentrification. More lending could replace current residents, particularly those from minority and disadvantaged communities, with more affluent individuals. The physical place may be better off at the expense of displaced individuals. **Table 7** assesses this concern by examining whether the sociodemographic composition of the neighborhoods has changed. This situation does not seem to be the case. The proportion of White, Black, and educated individuals, common metrics for measuring gentrification, did not change. It could be possible that some areas may have experienced compositional changes, but the average neighborhood did not experience them. Accordingly, most residents seem to benefit from a safer community.

6 Discussion and conclusions

Public and non-profit community investments focusing on high-risk individuals and areas with a clear nexus with crime-reducing factors have shown promising results (Blattman et al., 2017; Branas et al., 2018; Chalfin et al., 2022; Heller, 2014; Sharkey et al., 2017). Private investments have been less studied. Home mortgages are a quintessential private investment that ties landowners' well-being to the property and community prospects. They have strong incentives to protect their investment. At the same time, local investments bring more opportunities for crime. What is the net effect of residential lending on serious criminal offenses? This research answers this question by relying on a shift-share (or Bartik) instrumental variables approach to overcome the endogeneity bias confounding the mortgage-crime relationship. The instrument leverages the differential exposure to banks' local market presence (*shares*) and common national mortgage shocks (*shifts*) to assess differential changes in neighborhood crime. The instrument is the inner product

[19] The first stage of this alternative mortgage instrumental variable is significant. Results are available upon request.

of the banks' market shares at the census tract level and the banks' national residential lending happening outside of the 27 cities included in the study. Once controlling for tract and year fixed effects, along with sociodemographics, the mortgage growth outside these cities is unlikely to correlate with factors explaining crime at the neighborhood level in time and place beyond its effect through residential lending. This instrument is a prime candidate to approximate the impact of mortgages on crime.

By analyzing crime incident microdata collected from 27 major US cities, the evidence suggests that increasing mortgages decreases property crime -driven by thefts and burglary reductions– and aggravated assaults. There is an increase in motor vehicle thefts, probably caused by a larger supply of potential opportunities and targets and their pro-cyclical relationship with the local economic activity (Bushway et al., 2012; Cook and Zarkin, 1985). Alternative model specifications and robustness checks confirm these findings. Furthermore, the crime changes are considerably larger in Black and Hispanic neighborhoods and concentrated disadvantaged areas, implying that minority prevalent and poor communities benefit more from an increase in residential lending. These heterogeneous impacts likely result from decreasing marginal returns as minority-prevalent neighborhoods (usually low-income areas) have considerably lower lending than their White counterparts. These findings are consistent with significantly larger impacts in communities where lending is scarce than in areas with widespread availability of mortgage access. One potential mechanism of the effect of home mortgages seems to be fewer vacant houses and an increase in occupied units without experiencing discernible gentrification changes, measured through sociodemographic changes.

In contrast to previous research, this study finds no reductions in serious violent offenses; at best, the decline in violent crime, driven by aggravated assaults, is suggestive (e.g., not consistently significant in all specifications). Why do mortgages reduce property and non-major crimes but not more serious felonies, as previous observational evidence has suggested? This research finds that increasing mortgages by 10 percent decreases aggravated assaults, theft, and burglaries by 2.4, 3.0, and 1.7 percent, respectively. Reducing other crime incidents may mechanically prevent some murders, but this study may not have distinguished the noise from the signal. This explanation cannot be unequivocally rejected, but given the large sample size, it is unlikely to be the main reason behind the null impacts on murder and robberies. A second explanation for null effects on serious violent offenses is that context matters. These offenses are more likely to happen due to anger outbursts, disputes going badly, retaliation, and interpersonal conflicts. It may be that private community investments are affecting only the opportunities for acquisitive crime but not the conditions for offenses happening in the heat of the moment. Future studies should also focus on people's perceptions and behavioral habits that could explain the differential impacts between property and violent offenses. A third plausible explanation is that the previous studies did not address the endogeneity bias confounding the lending and crime relationship. Cross-sectional designs, random effects models, and weak instruments may have overestimated the impacts. Weak instruments can lead to greater bias than ordinary least squares (Angrist and Krueger, 2001). This research shows that the relationship between lending and crime mostly applies to property and low-level offenses but not to violent felonies.

This study is not without limitations. First, this research uses data from 27 major US cities. Many of them face high living costs and unaffordable housing, so the effect of lending may be different in rural areas or cities with large housing inventories. Future research should explore the impact at the national level. Second, not finding an effect of mortgages on serious violent offenses does not mean it could be found in other contexts. Future research should continue assessing the impact of property and violent offenses. Finally, the study period (2011-2017) was marked by a post-financial crisis era, with relatively low mortgage interest rates and a consistent increase in home prices. This environment is no longer present, particularly since the COVID-19 pandemic. Future studies should examine the effects of lending in other market conditions.

Finally, this research contributes to the promising literature finding that localized investments can promote safer neighborhoods by revitalizing neighborhoods. While traditionally, these investments have come from public and non-profit sources, this research highlights that private investments can also deter crime. Financial institutions, inadvertently, may be contributing to reducing crime by providing mortgages to creditworthy individuals to acquire a property or improve their current home. Moreover, lending can reduce urban inequality as the social benefits of less crime are larger in minority-prevalent and lower-income neighborhoods. However, it is possible that a tight, unaffordable housing industry, like the one the US is facing right now, could exacerbate neighborhood crime. Given the wide geographical presence and large financial asset size of banks, using lending to promote neighborhood revitalization is a key and promising area of research.

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Table 1: Descriptive statistics by selected years, census tract year data

	Mean (std. dev)		
	2011	2014	2017
Non-Major crimes	215.0 (290.7)	191.7 (235.6)	186.4 (226.0)
Major crimes	141.0 (151.4)	132.7 (157.4)	131.5 (161.8)
Violent	25.5 (28.3)	24.2 (28.2)	26.3 (31.1)
Murder	0.4 (0.8)	0.3 (0.8)	0.4 (1.0)
Robbery	11.9 (14.0)	10.6 (13.1)	10.3 (13.0)
Aggravated assault	13.3 (16.4)	13.4 (17.1)	15.6 (20.6)
Property	115.5 (132.7)	108.5 (139.9)	105.1 (142.6)
Burglary	28.9 (31.5)	22.8 (24.5)	18.7 (20.4)
Theft	72.2 (104.8)	72.6 (116.7)	73.0 (121.1)
Motor vehicle theft	14.4 (15.6)	13.0 (15.9)	13.4 (16.0)
Number of loans	43.8 (66.5)	51.5 (65.2)	60.0 (72.4)
Loan amount (million dollars)	12.1 (26.3)	15.0 (24.7)	19.7 (30.7)
Population (thousands)	3.8 (2.0)	3.9 (2.1)	4.0 (2.2)
White (%)	51.1 (30.3)	51.3 (30.1)	51.0 (29.4)
Black (%)	27.7 (32.4)	27.3 (31.8)	26.9 (31.2)
Hispanic (%)	23.3 (25.4)	23.9 (25.4)	24.2 (25.2)
Age 0-14 (%)	18.5 (7.6)	18.2 (7.2)	17.9 (7.1)
Age 15-24 (%)	15.1 (8.7)	14.5 (8.8)	13.6 (8.8)
Age 25-39 (%)	24.3 (9.0)	24.6 (9.1)	25.3 (9.4)
Age 40-54 (%)	20.3 (5.6)	19.8 (5.3)	19.1 (4.8)
Age 55+ (%)	21.7 (9.3)	22.8 (9.4)	24.1 (9.4)
Less than high school (%)	19.7 (14.7)	18.6 (14.2)	17.1 (13.2)
High school (%)	25.1 (11.3)	24.4 (11.1)	24.0 (11.3)
Some college (%)	18.3 (7.2)	18.5 (7.1)	18.1 (7.1)
College+ (%)	37.0 (22.1)	38.5 (22.4)	40.8 (22.6)
Unemployment rate (%)	10.8 (7.3)	11.7 (7.7)	8.5 (6.2)
Family income (thousands)	63.6 (38.2)	65.1 (39.7)	72.0 (43.1)
Poverty rate (%)	16.8 (14.3)	18.1 (14.8)	16.4 (13.7)
Occupied housing units	1,472.5 (795.3)	1,498.9 (829.6)	1,542.3 (875.0)
Vacant housing units	192.3 (176.2)	185.1 (172.2)	177.0 (168.2)

Notes: Census tract level mean (standard deviation) in selected years from the 27 US cities included in the study, representing 7,810 tracts. Major crimes include murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Non-major crimes refer to all the other crimes reported to the police departments.

Table 2: Models used in previous studies on the effect of mortgages on crime

	Major crime	Property	Theft	Burglary	Motor vehicle theft	Violent	Murder	Robbery	Assault	Nonmajor crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Random effects										
Loan amount	0.006**	0.008***	0.007***	0.008**	0.021***	−0.004	−0.007***	−0.0003	−0.015***	0.002
	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.004)	(0.002)	(0.004)	(0.004)	(0.002)
Observations	54,174	54,174	54,174	54,174	54,174	54,174	51,934	54,174	53,876	44,161
B. Fixed effects										
Loan amount	0.006**	0.006**	0.004	0.005	0.012**	0.008**	−0.001	0.012**	−0.001	0.003
	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)	(0.005)	(0.002)
Observations	54,174	54,174	54,174	54,174	54,174	54,174	51,934	54,174	53,876	44,161
C. First differences using the lagged value as IV										
Loan amount	−0.071	−0.054	−0.006	−0.057***	0.001	−0.015	−0.001	−0.019***	0.007	0.097
	(0.050)	(0.049)	(0.042)	(0.015)	(0.017)	(0.010)	(0.001)	(0.007)	(0.007)	(0.139)
Observations	38,679	38,679	38,679	38,679	38,679	38,679	37,079	38,679	38,232	31,528

Notes: Panels A and B show the random effects and fixed effects models, using the inverse hyperbolic sine transformation in the dependent and independent variables. The estimates are interpreted as elasticities (e.g., a 10 percent change in the mortgages loan amount, relate to a $\beta_1/10$ percent change in crime incidents). Panel C shows the first differences model instrumenting the loan amount with its lagged value. The estimates are interpreted as level changes (e.g., a one million change in the mortgages loan amount relates to a β_1 change in crime incidents). All models include sociodemographic controls. Major crimes include the part I Uniform Crime Reporting categories of murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Non-major crimes refer to all the other crimes reported to the police departments. Robust standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 3: First stage estimates on census tract mortgages

	Loan amount	
	(1)	(2)
Nation loan growth	0.218***	0.215***
	(0.025)	(0.025)
Mean dep. var.	19.86	20.01
Observations	54,628	54,174
F-statistic	78.0	74.6
Year FE	X	X
Tract FE	X	X
Covariates	-	X

Notes: First stage estimates using ordinary least squares regression of the shift-share instrumental variable. The instrument is the inner product of the nation wide bank loan growth rates outside of the 27 cities and the bank-tract share on mortages. Regression follows equation (3). The independent variable uses the symmetric growth rate. The dependent variable uses the inverse hyperbolic sine transformation. The results are expressed as an increase of 10 percent in the nation loan growth, implies a $(e^{\beta_1} - 1)/10$ percent change in the census tract residential lending. The bottom row shows the mean dependent variable, expressed as the total mortgage loan in the average census tract (in millions of dollars). Robust standard errors clustered at the census tract level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 4: Main estimates: Reduced form and 2SLS estimates of mortgages on crime

	Major crime	Property	Theft	Burglary	Motor vehicle theft	Violent	Murder	Robbery	Assault	Nonmajor crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Reduced form										
Instrument	-0.024^{**}	-0.034^{***}	-0.078^{***}	-0.040^{**}	0.089^{***}	-0.023	0.013	-0.004	-0.059^{***}	-0.033^{***}
	(0.010)	(0.011)	(0.013)	(0.019)	(0.023)	(0.016)	(0.017)	(0.019)	(0.019)	(0.008)
B. Second-stage least squares										
Loan amount	-0.111^{**}	-0.157^{***}	-0.361^{***}	-0.184^{**}	0.415^{***}	-0.107	0.057	-0.018	-0.276^{***}	-0.188^{***}
	(0.048)	(0.055)	(0.076)	(0.094)	(0.115)	(0.075)	(0.078)	(0.090)	(0.097)	(0.055)
Mean crime	138.06	112.33	74.92	23.85	13.55	25.73	0.39	11.18	14.25	198.69
Observations	54,174	54,174	54,174	54,174	54,174	54,174	51,934	54,174	53,876	44,161
2SLS change	-1.1%	-1.5%	-3.0%	-1.7%	5.1%	-1.0%	0.6%	-0.2%	-2.4%	-1.7%

Notes: Panel A shows the reduced form estimates following equation (4). Panel B presents the second stage least squares (instrumental variable) estimates following equation (5). Outcomes use the inverse hyperbolic sine transformation. The results are expressed as percent changes ($e^{\beta_1} - 1$). Major crimes include the part I Uniform Crime Reporting categories of murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Non-major crimes refer to all the other crimes reported to the police departments. The bottom row shows the mean dependent variable, expressed as the average number of yearly crimes in a census tract. It is followed by the implied percent change of the second stage least squares estimates caused by a 10 percent increase in the local mortgages. Robust standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 5: Heterogeneity: 2SLS estimates of mortgages on crime interacted by sociodemographic dimension

	Major crime	Property	Theft	Burglary	Motor vehicle theft	Violent	Murder	Robbery	Assault	Nonmajor crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Black population = D										
Loan amount	−0.071**	−0.104***	−0.204***	−0.138**	0.243***	−0.016	−0.068	0.054	−0.145**	−0.065**
	(0.029)	(0.033)	(0.043)	(0.058)	(0.069)	(0.048)	(0.048)	(0.058)	(0.063)	(0.031)
Loan amount*D	−0.130	−0.177*	−0.524***	−0.161	0.572***	−0.294**	0.393**	−0.235	−0.426***	−0.443***
	(0.085)	(0.098)	(0.141)	(0.169)	(0.215)	(0.130)	(0.192)	(0.171)	(0.165)	(0.117)
Mean crime	138.06	112.33	74.92	23.85	13.55	25.73	0.39	11.18	14.25	198.69
Observations	54,174	54,174	54,174	54,174	54,174	54,174	51,934	54,174	53,876	44,161
B. Hispanic population = D										
Loan amount	−0.096**	−0.138***	−0.325***	−0.144	0.353***	−0.094	0.061	−0.028	−0.251***	−0.162***
	(0.046)	(0.052)	(0.073)	(0.089)	(0.109)	(0.072)	(0.074)	(0.085)	(0.093)	(0.056)
Loan amount*D	−0.117*	−0.154*	−0.272**	−0.318**	0.475***	−0.112	−0.025	0.068	−0.196	−0.184***
	(0.069)	(0.085)	(0.114)	(0.152)	(0.180)	(0.114)	(0.156)	(0.150)	(0.149)	(0.065)
Mean crime	138.06	112.33	74.92	23.85	13.55	25.73	0.39	11.18	14.25	198.69
Observations	54,174	54,174	54,174	54,174	54,174	54,174	51,934	54,174	53,876	44,161
C. Poverty level = D										
Loan amount	−0.055**	−0.070***	−0.144***	−0.027	0.142**	0.005	−0.017	0.065	−0.123**	−0.013
	(0.022)	(0.026)	(0.038)	(0.047)	(0.057)	(0.040)	(0.042)	(0.046)	(0.051)	(0.029)
Loan amount*D	−0.419	−0.672*	−1.751***	−1.272**	2.282**	−0.949*	0.572	−0.684	−1.294**	−1.301**
	(0.303)	(0.375)	(0.653)	(0.644)	(0.916)	(0.491)	(0.632)	(0.567)	(0.638)	(0.553)
Mean crime	138.18	112.42	74.97	23.88	13.57	25.76	0.39	11.19	14.27	198.79
Observations	54,080	54,080	54,080	54,080	54,080	54,080	51,840	54,080	53,782	44,078

Notes: Second stage least squares (instrumental variable) estimates interacting the loan amount with the relevant heterogeneity dimension. Specifically, it follows $y_{it} = \gamma_i + \mu_t + \beta_2 \hat{L}_{it} + \beta_3 \hat{L}_{it} D_{it} + X_{it}\alpha_X + e_{it}$, where D_{it} is the relevant heterogeneity dimension, which also is included in the control variables. All other parameters are as explained in the main text. Crimes and loan amount outcomes use the inverse hyperbolic sine transformation. The heterogeneity variable is the census tract proportion of the relevant group (variable goes from zero to one). Hence, the results are expressed as a ten percent increase in the mortgages loan amount in tracts with a 50 percent prevalence of the group relates to a $(e^{\beta_1 * 0.5} - 1)/10$ percent change relative to not having any members of that group. Major crimes include the part I Uniform Crime Reporting categories of murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Non-major crimes refer to all the other crimes reported to the police departments. The bottom row shows the mean dependent variable, expressed as the average number of yearly crimes in a census tract. Robust standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 6: Potential mechanisms: 2SLS estimates of mortgages on housing units

	Occupied units (1)	Vacant units (2)
A. All mortgages		
Loan amount	0.005	−0.005
	(0.008)	(0.009)
Mean dep. var.	1.52	0.19
Observations	54,137	54,137
B. Home purchase mortgages		
Loan amount	0.018*	−0.024*
	(0.010)	(0.013)
Mean dep. var.	1.52	0.19
Observations	51,057	51,057

Notes: Second stage least squares (instrumental variable) estimates of mortgages on the number of housing units in the census tract. Panel A uses all mortgages for the instrument and the endogeneous variable, which is the same approach as the main estimates. Panel B uses only mortgages with the purpose of buying a home for the instrument and the endogenous variable (it excludes mortgages for home improvement and refinancing). All outcomes use the inverse hyperbolic sine transformation. The results are expressed as percent changes ($e^{\beta_1} - 1$). The bottom row shows the mean dependent variable, expressed as the average number of yearly property units in a census tract (in thousands). Robust standard errors clustered at the census tract level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

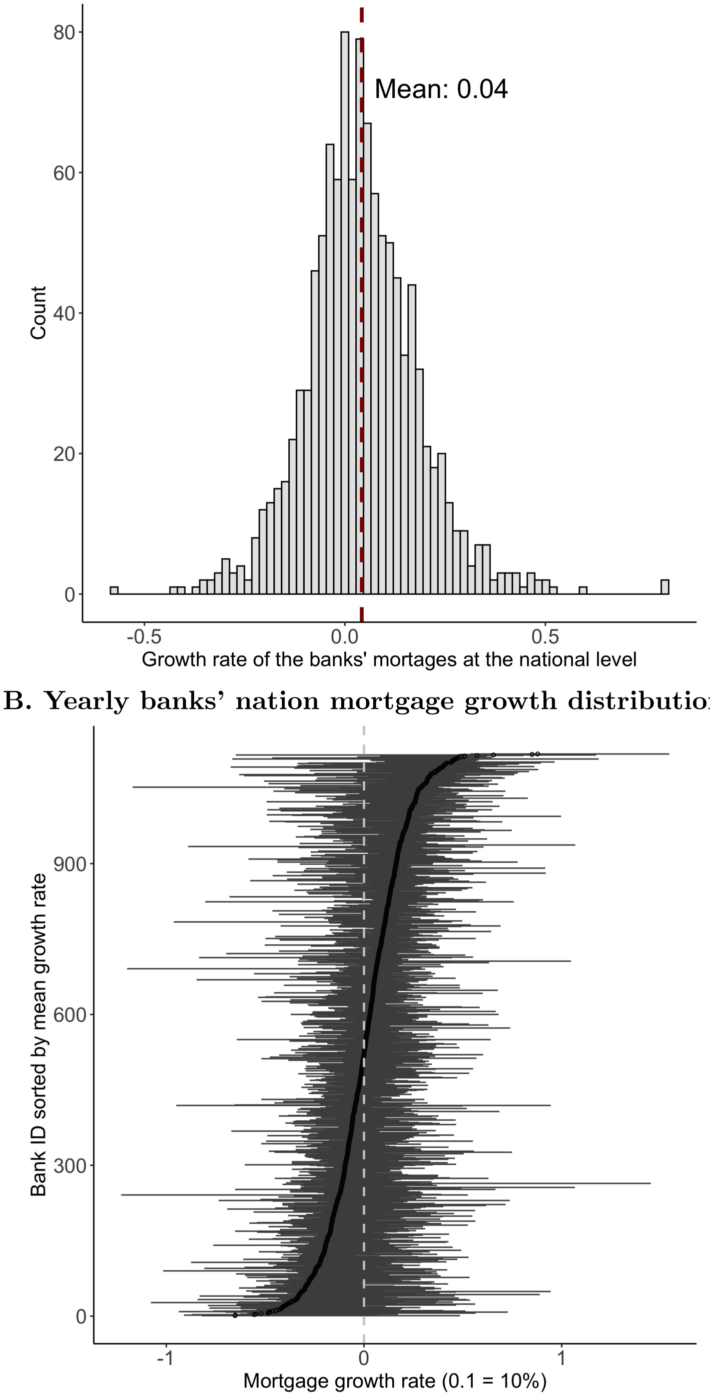
Table 7: Potential mechanisms: 2SLS estimates of mortgages on sociodemographics

	White	Black	Some college	College+
	(1)	(2)	(3)	(4)
Loan amount	0.008	−0.005	−0.004	−0.001
	(0.010)	(0.008)	(0.006)	(0.007)
Mean dep. var.	0.48	0.26	0.18	0.38
Observations	54,177	54,177	54,174	54,174

Notes: Second stage least squares (instrumental variable) estimates of mortgages on census tract sociodemographics. It shows the reduced form estimates following equation (4). Outcomes use the inverse hyperbolic sine transformation, so the results are expressed as percent changes ($e^{\beta_1} - 1$). The bottom row shows the mean dependent variable, expressed as the average proportion of the relevant sociodemographic dimension in census tract. Robust standard errors clustered at the census tract level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

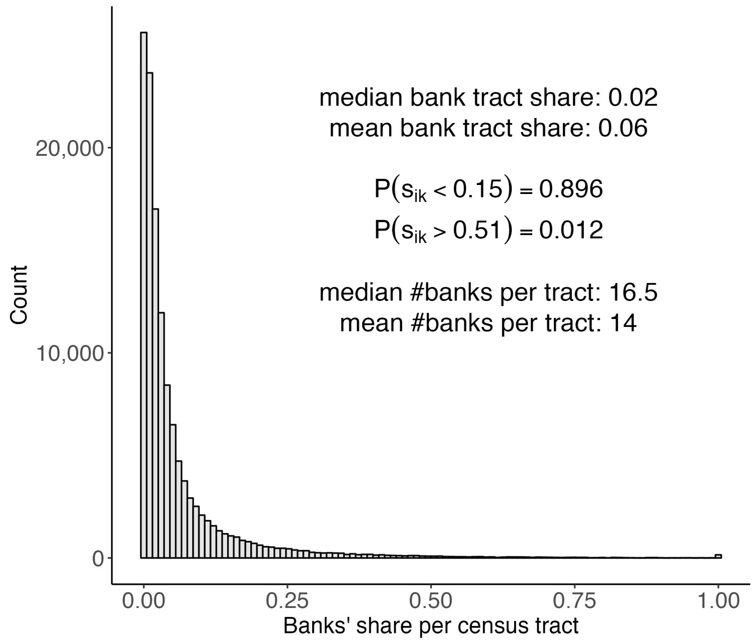
Figure 1: Banks' nation mortgage growth, 2011-2017

A. Average banks' nation mortgage growth



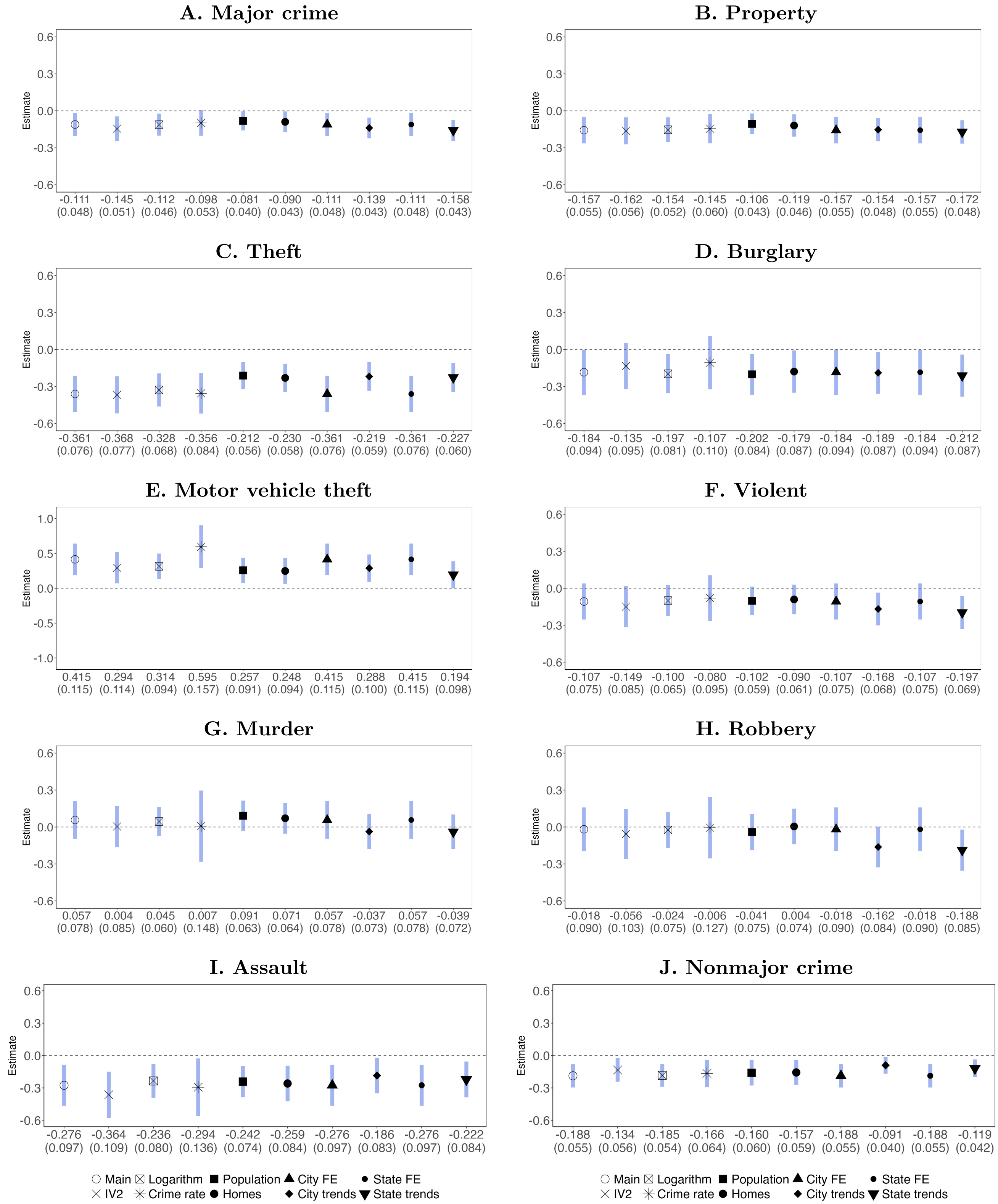
Notes: The symmetric growth measure is calculated as $(L_{it} - L_{it-1})/(0.5 * L_{it} + 0.5 * L_{it-1})$, so the values range between -2 and 2. Panel A shows the mean yearly growth rate in the nation's mortgages (excluding the 27 US cities included in this research) for each of the 1,118 banks used to build the Bartik instrument between 2011 and 2017. The vertical dashed line represents the mean bank's growth. Panel B shows the first, second (median), and third quartile of the yearly national mortgage growth (excluding the 27 cities) between 2011 and 2017 for each of the 1,118 banks. The vertical dashed line marks the zero growth rate.

Figure 2: Histogram of banks' mortgage share, 2007



Notes: The figure shows the 2007 banks' share of the mortgage loan amount per census tract. It is the *share* (s_{ikt_0}) component from the instrumental variable presented in Equation 1. There are 7,804 census tracts and 1,118 banks. The figure only includes banks with a positive presence in the census tract ($s_{ikt_0} > 0$); many banks only operate in some states. The results suggest that the typical bank has little local market concentration in the sample.

Figure 3: Alternative specifications: Second stage least squares estimates of mortgages on crime



Notes. Main: preferred estimates. IV2: IV uses only banks with less than 66% of the tract market share. Logarithm: dependent variable uses log(x+1). Population: weighted by the census tract population. Homes: weighted by the census tract residential units. Crime rate: dependent variable is crimes per 10,000 people. City FE: adds city-fixed effects. City trends: adds city fixed effects and city-time trends. State FE: add state-fixed effects. State-trends: adds state-fixed effects and state-time trends. Outcomes use the inverse hyperbolic sine transformation, except as noted. Robust standard errors clustered at the census tract level are in parentheses. Motor vehicle theft, panel E, is the only outcome using a larger vertical scale to facilitate presenting the results.

ONLINE APPENDIX

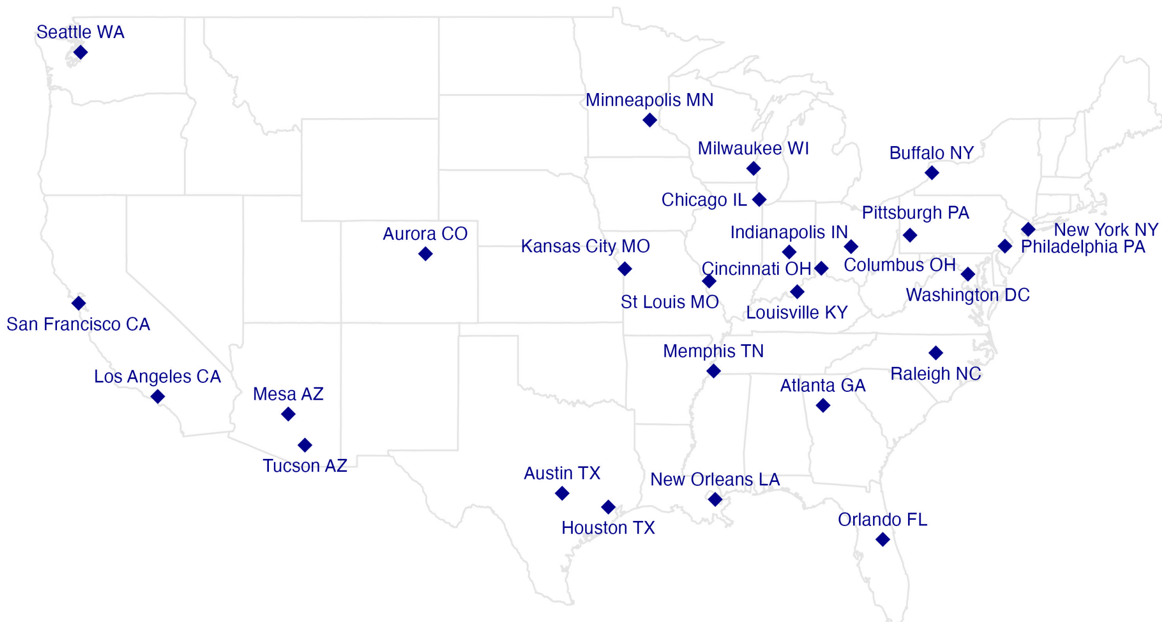
A Appendix: Tables and Figures

Table A.1: Heterogeneity: 2SLS estimates of mortgages on crime by mortgage growth tercile groups

	Major crime	Property	Theft	Burglary	Motor vehicle theft	Violent	Murder	Robbery	Assault	Nonmajor crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mortgage*1st tercile	-0.434^{*}	-0.625^{**}	-1.552^{***}	-0.691	1.753^{***}	-0.603^{*}	0.363	-0.339	-0.969^{**}	-0.835^{**}
	(0.237)	(0.289)	(0.545)	(0.441)	(0.662)	(0.362)	(0.353)	(0.405)	(0.470)	(0.353)
Mortgage*2nd tercile	-0.127^{**}	-0.184^{**}	-0.429^{***}	-0.203^{*}	0.463^{***}	-0.110	0.040	0.0002	-0.326^{**}	-0.199^{**}
	(0.062)	(0.074)	(0.121)	(0.116)	(0.153)	(0.096)	(0.078)	(0.110)	(0.128)	(0.082)
Mortgage*3rd tercile	-0.070^{**}	-0.097^{**}	-0.208^{***}	-0.122^{*}	0.249^{***}	-0.049	0.012	0.014	-0.183^{**}	-0.086^{*}
	(0.034)	(0.040)	(0.070)	(0.063)	(0.088)	(0.054)	(0.044)	(0.061)	(0.072)	(0.046)
$\beta_{tercile1} = \beta_{tercile2}$	0.10	0.06	0.01	0.16	0.02	0.09	0.29	0.29	0.08	0.03
$\beta_{tercile2} = \beta_{tercile3}$	0.12	0.04	0.00	0.24	0.01	0.31	0.57	0.84	0.06	0.01
$\beta_{tercile1} = \beta_{tercile3}$	0.08	0.04	0.01	0.14	0.01	0.08	0.28	0.32	0.06	0.02
Mean crime	138.06	112.33	74.92	23.85	13.55	25.73	0.39	11.18	14.25	198.69
Observations	54,174	54,174	54,174	54,174	54,174	54,174	51,934	54,174	53,876	44,161

Notes: Second stage least squares (instrumental variable) estimates interacting the loan amount with the tercile group of the average mortgage growth between 2011 and 2017. Specifically, it follows $y_{it} = \gamma_i + \mu_t + \sum_{j=1}^{3} \beta_j \hat{L}_{it} D_i^j + X_{it}\alpha_X + e_{it}$, where D_i^j is an indicator variable of the tercile group of the mortgage growth. All other parameters are as explained in the main text. Crimes and loan amount outcomes use the inverse hyperbolic sine transformation. The results are expressed as percent changes ($e^{\beta_j} - 1$). Major crimes include the part I Uniform Crime Reporting categories of murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Non-major crimes refer to all the other crimes reported to the police departments. The bottom rows show the pvalue of the hypothesis testing whether the coefficients across tercile groups are equal, followed by the mean dependent variable, expressed as the average number of yearly crimes in a census tract. Robust standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

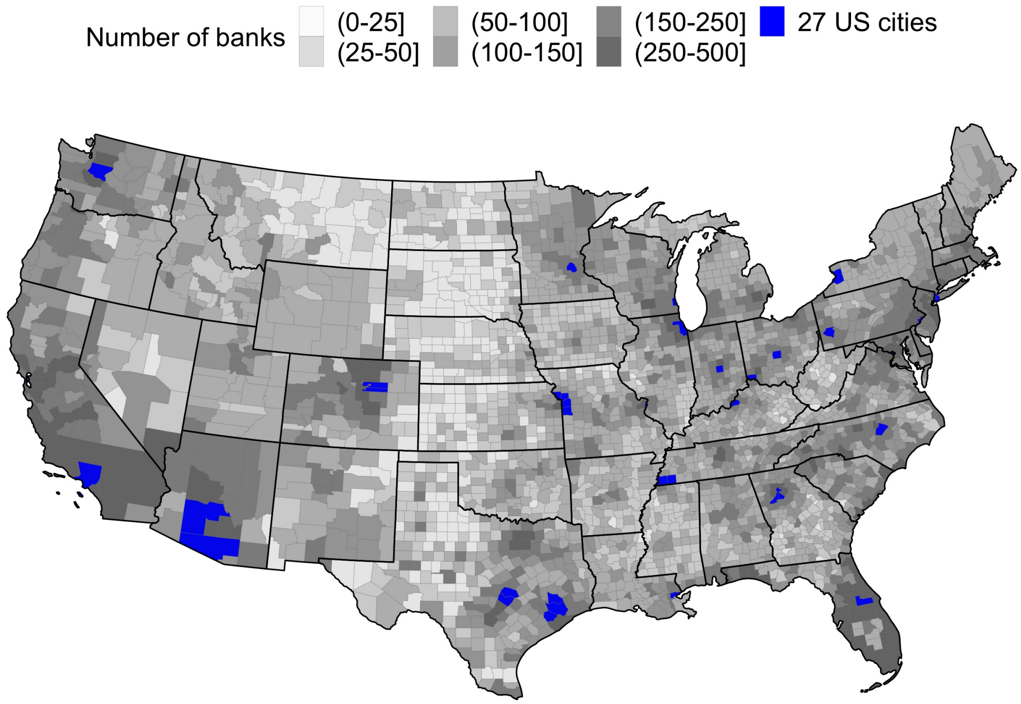
Figure A.1: Cities included in the analysis



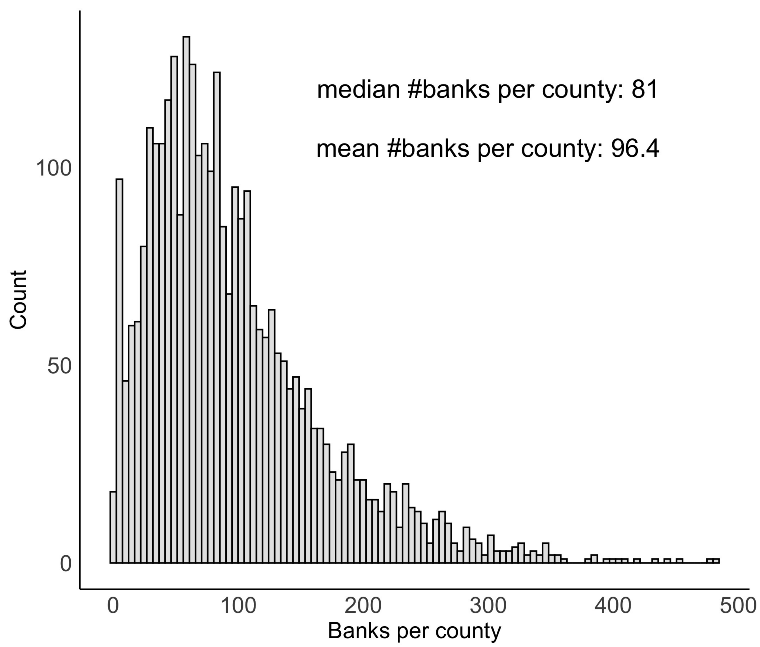
Notes: The map shows the location of the 27 US major cities included in this research. These cities had public crime data that could be aggregated to the census tract-year level.

Figure A.2: Banks' coverage across counties

A. Geographical distribution of banks' across counties



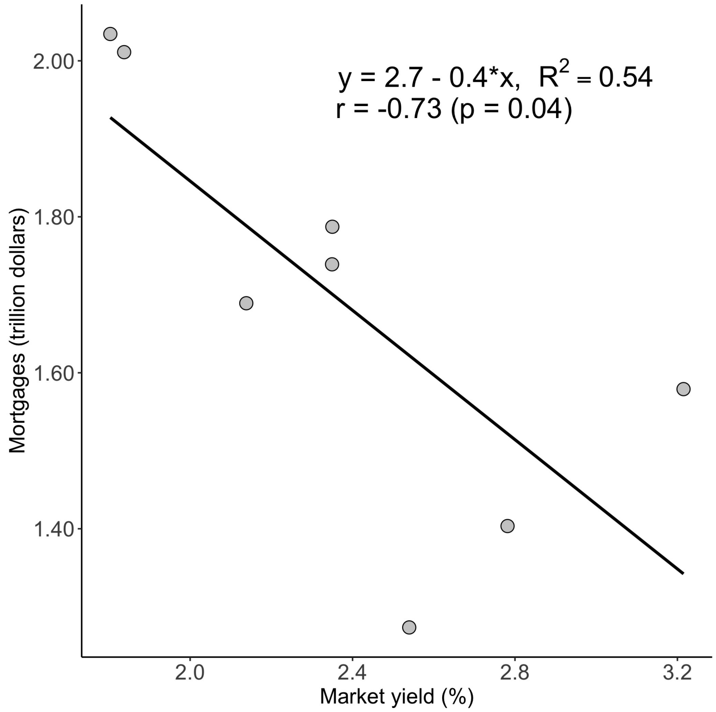
B. Distribution of banks' across counties



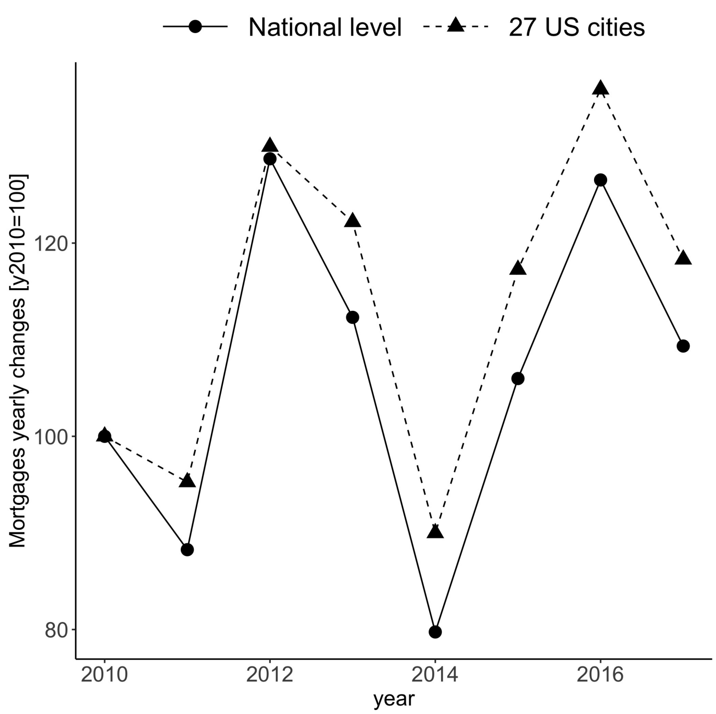
Notes: Panel A shows the number of banks per US county. It only includes the 1,118 financial institutions used in the instrumental variable. The blue-colored counties are the ones where the 27 cities included in the analysis are located. While each of the 1,118 do not cover all lower 48 states, they operate jointly across the country. Panel B presents the histogram of the banks' presence by county, showing that the mean (median) county has 96 (81) banks.

Figure A.3: Mortgages trend across time and correlation with Treasury market yield

A. Correlation of US mortgages and Treasury market yield, 2010-2017

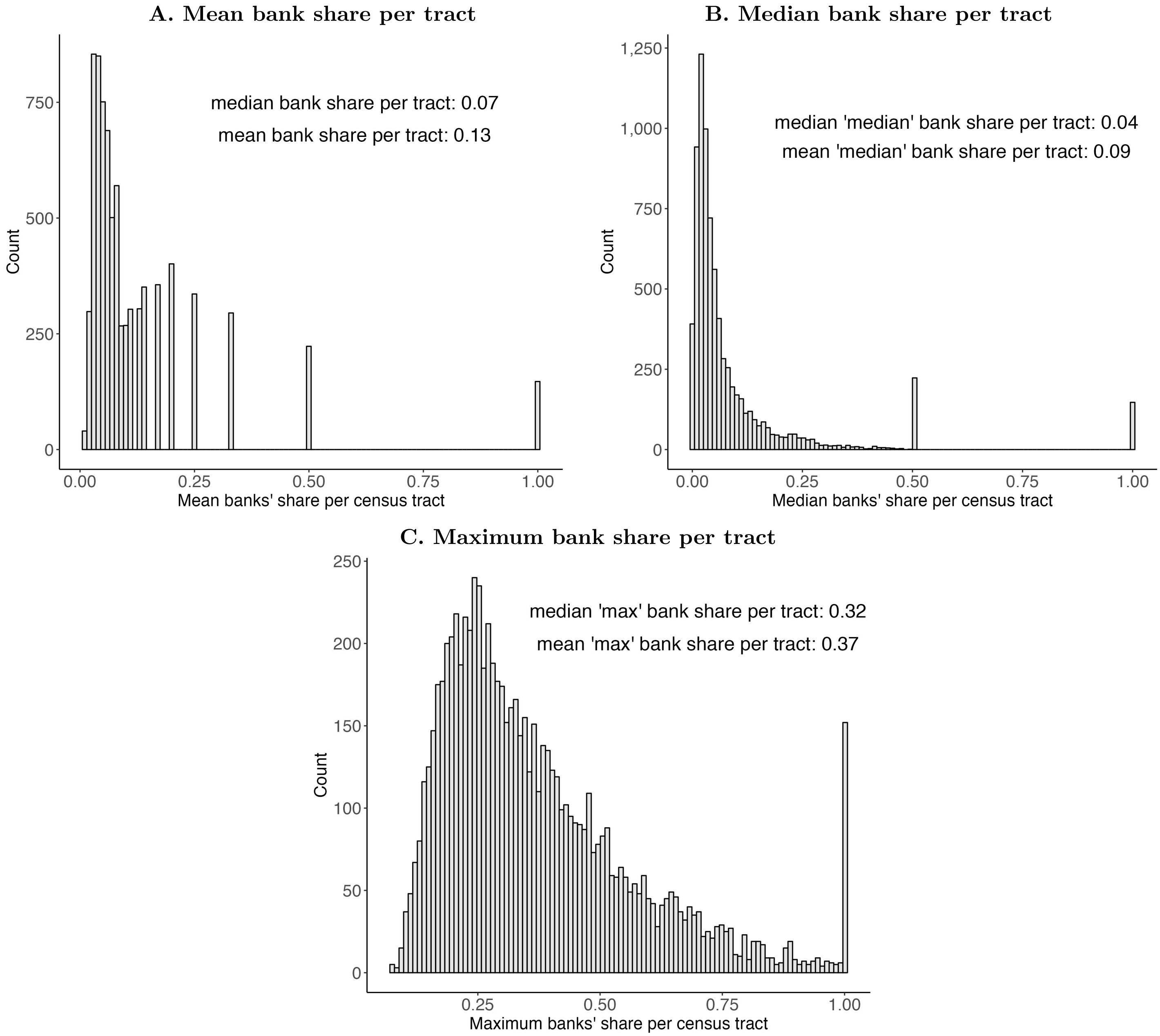


B. Mortgages changes by sample group



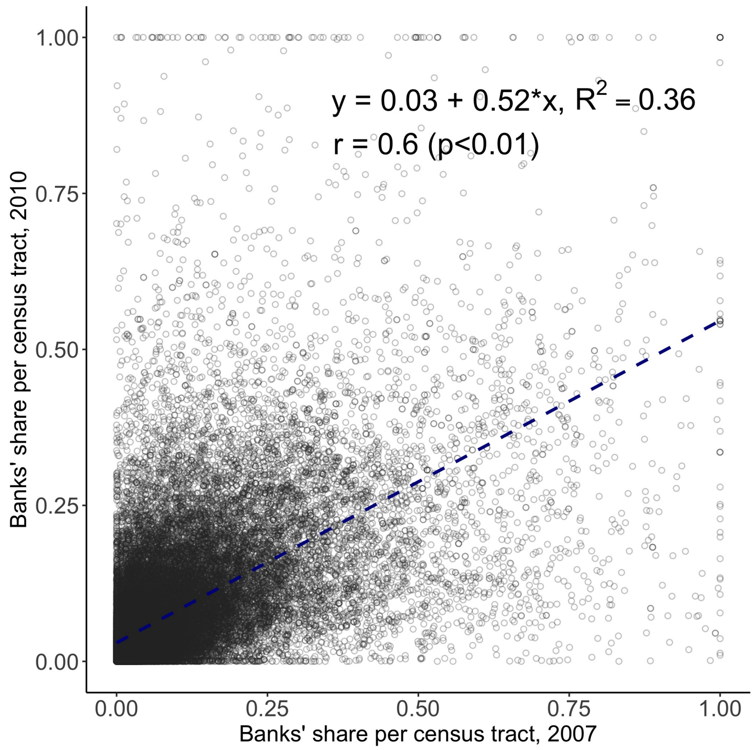
Notes: Panels A shows in the horizontal axis the mean Market Yield on US Treasury Securities at 10-Year Constant Maturity, which is the interest rate that the government pays to borrow money and influences other interest rates and lending patterns. The vertical axis shows the value of the national mortgages. The correlation also holds for each of the 27 cities used in the sample, ranging their coefficient from -0.40 to -0.92. Panel B shows the relative changes in the nation's (excluding the 27 US cities) and the 27 US cities' mortgage debt. Both follow the same pattern.

Figure A.4: Banks' shares per census tract, 2007



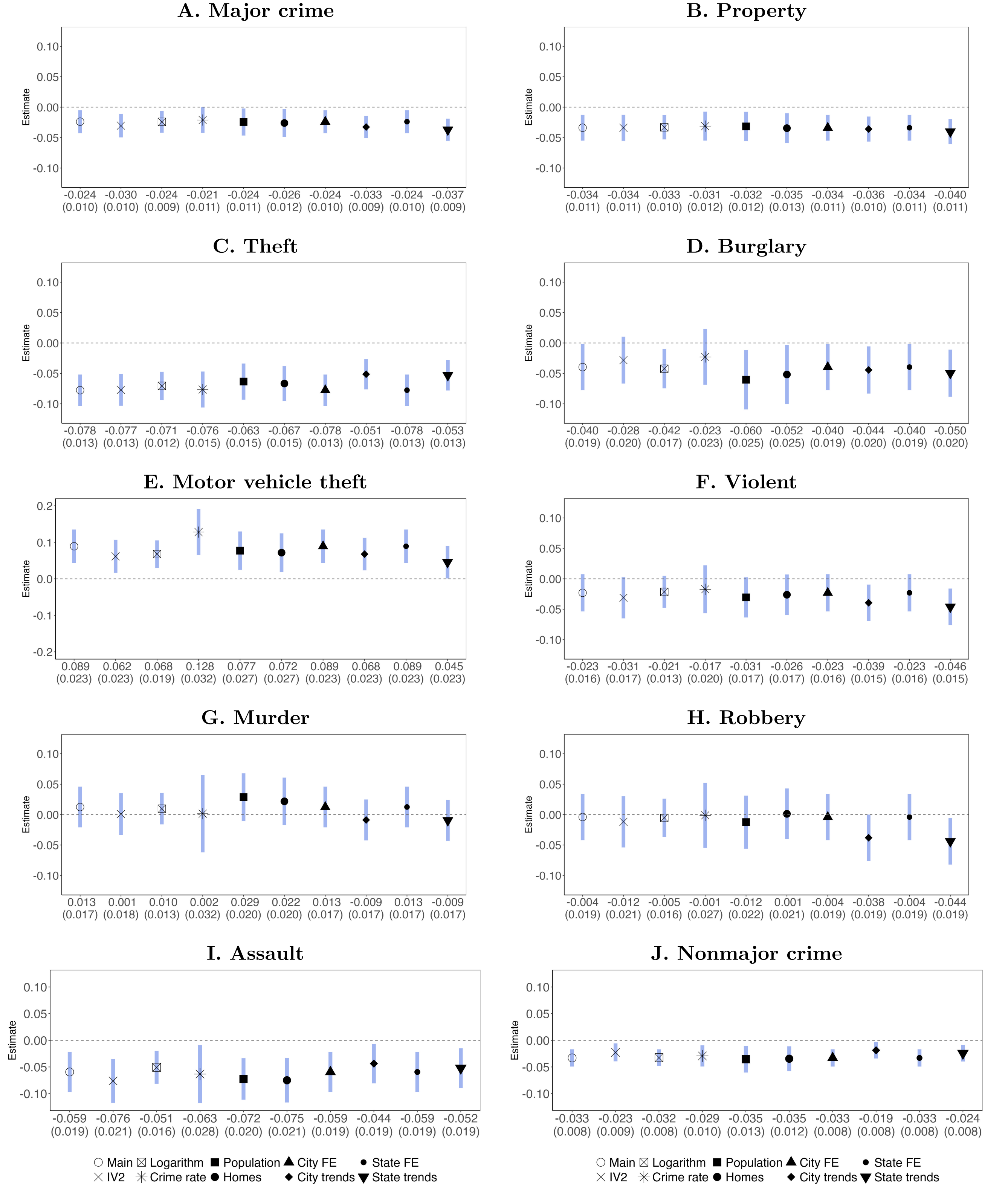
Notes: Each panel shows the mean (Panel A), median (Panel B), and maximum (Panel C) bank share per census tract. It only includes the banks used to build the instrumental variable. Each panel presents the median and mean of its distribution. Overall, the three census tract statistics and distributions suggest that while one bank usually has one-third of the local mortgage market, the remaining share is scattered across a considerable number of banks.

Figure A.5: Correlation of banks' tract shares across time, 2007 vs 2010



Notes: The figure shows the census tract banks' share of the mortgage loan amount four (2007) and one year (2007) before the study period. While having a large share in 2007 correlates with a high share in 2010, there is considerable unexplained variation in the sample. A best-fit dashed line is drawn through the data. The figure also shows its regression equation, R^2, correlation coefficient (r), and the pvalue (p) of the correlation.

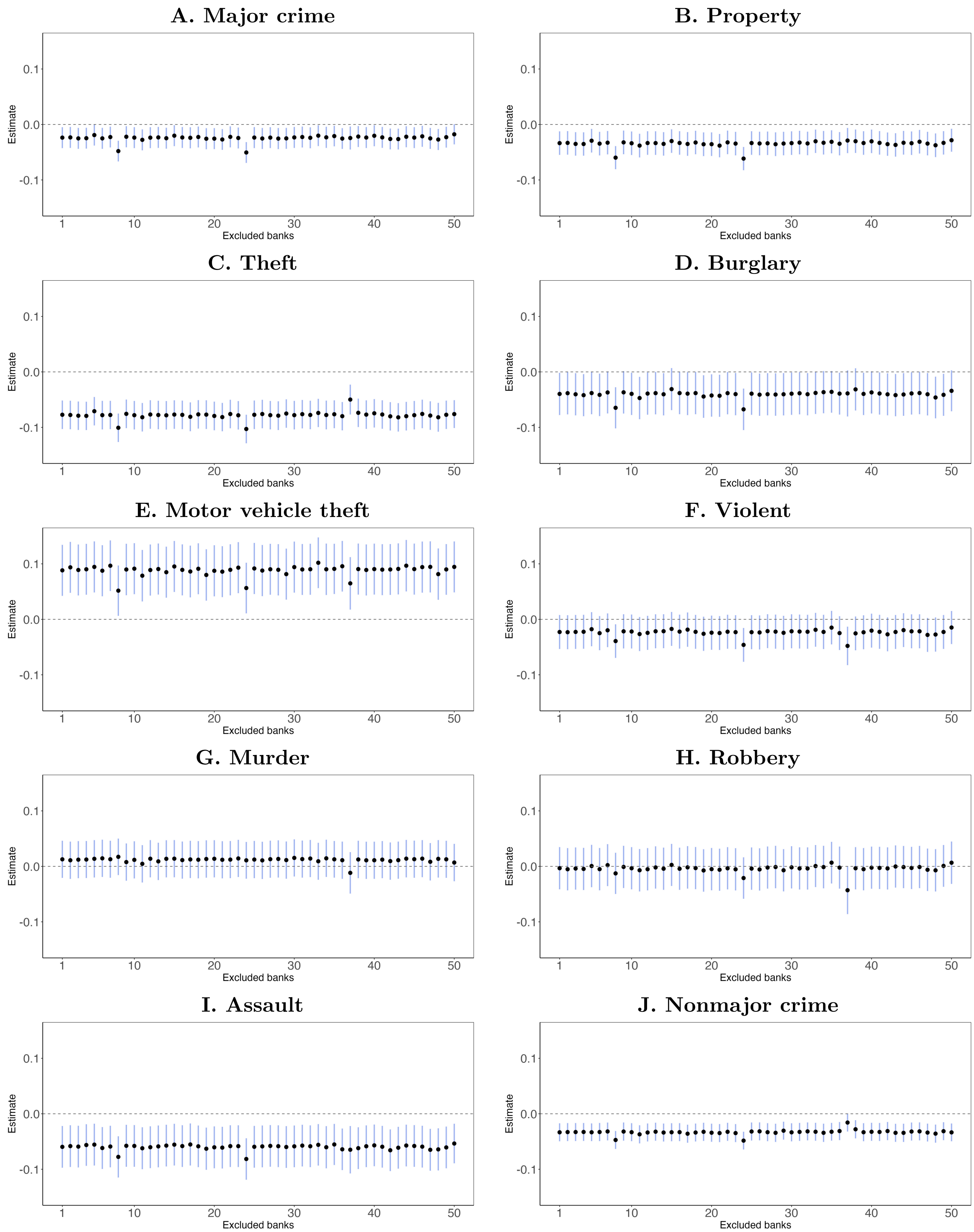
Figure A.6: Alternative specifications: Reduced form estimates of mortgages on crime



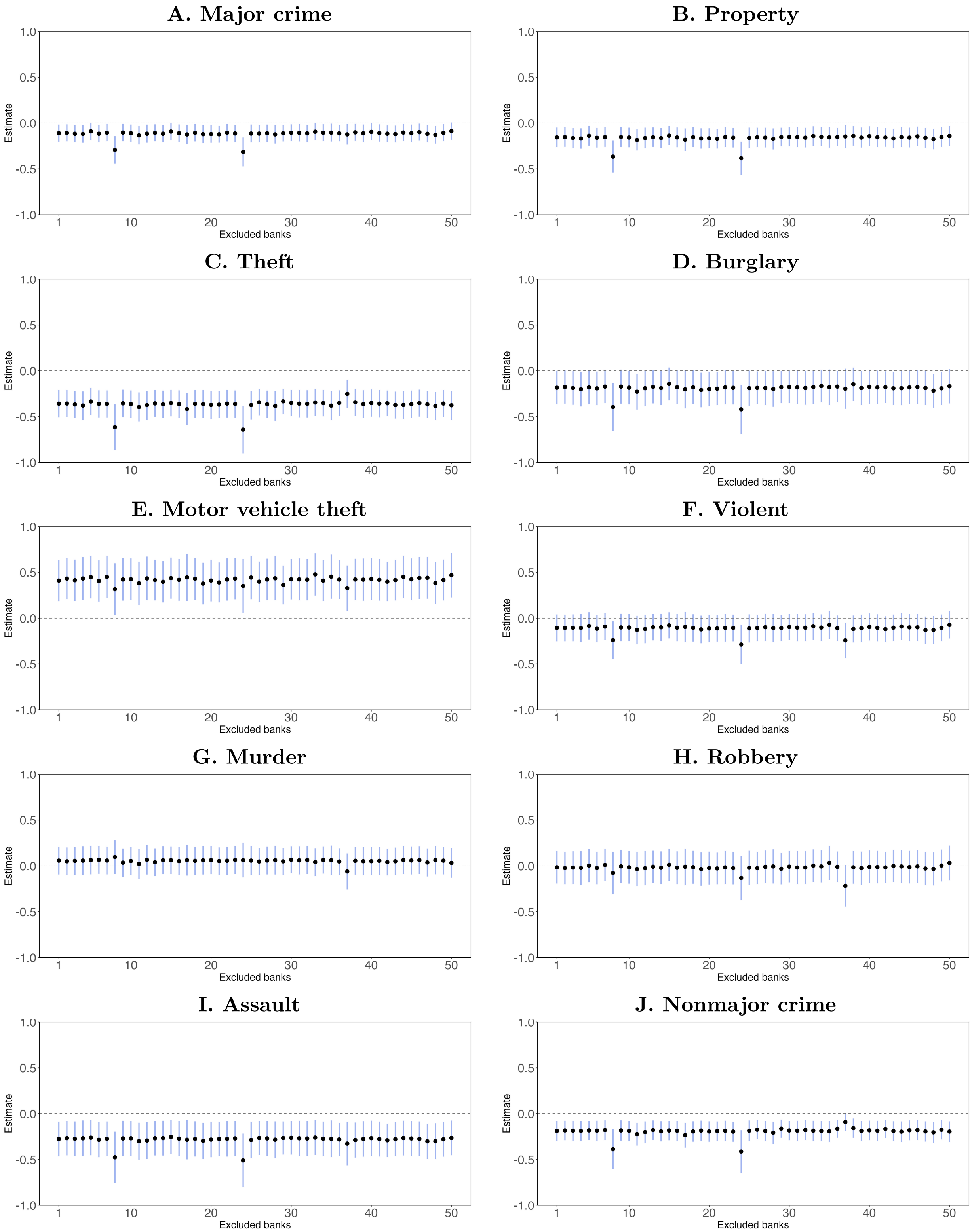
Notes. Main: preferred estimates. IV2: IV uses only banks with less than 66% of the tract market share. Logarithm: dependent variable uses log(x+1). Population: weighted by the census tract population. Homes: weighted by the census tract residential units. Crime rate: dependent variable is the crimes per 10,000 people. City FE: adds city-fixed effects. City trends: adds city fixed effects and city-time trends. State FE: add state-fixed effects. State-trends: adds state-fixed effects and state-time trends. Outcomes use the inverse hyperbolic sine transformation, except as noted. Robust standard errors clustered at the census tract level are in parentheses.

Figure A.7: Leave-one-out-estimator: Estimates of mortgages on crime

I. Reduced form estimates



II. Second stage least squares



Notes. Figures show the second stage least squares (instrumental variable) estimates of mortgages on crime. It builds the instrumental variable, excluding twenty banks at a time, and estimates the regression model, repeating the process 50 times. Outcomes use the inverse hyperbolic sine transformation, so the results are expressed as percent changes ($e^{\beta_1} - 1$). Robust standard errors clustered at the census tract level are in parentheses.