# Housing Improvement and Crime

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**Abstract**

We evaluate a policy implemented in Chicago geared towards improving the housing stock in distressed neighborhoods. The program first led to an increase in municipal building permits for renovations and loan applications for home purchases establishing a link to actual housing improvement. Next, treated neighborhoods saw significant reductions in burglaries and thefts. However, they also led to an increase in less serious offenses such as simple assaults and trespassing, likely due to changes in residential demographics. Adjacent neighborhoods also experience a reduction in burglaries and robberies but no significant changes in other offenses. Our findings provide evidence of relevant neighborhood gains as a result of low-cost, place-based, housing interventions which prioritize renovation of existing private stock of houses.

**Keywords:** Building Permits, Difference in Differences, Micro Market Recovery Program, Neighborhood

**JEL Codes:** H40, I38, R20

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# 1 Introduction

We study whether improving the housing stock in a neighborhood leads to welfare improvements via a reduction in crime prevalence. A vast literature in sociology and criminology has argued, since at least the mid-twentieth century, how neighborhood characteristics and features can have a profound impact on crime prevalence (Shaw and McKay, 1942). Economic deprivation and residential instability have been strongly linked to crime rates (Sampson et al., 1997). Broader neighborhood environments can also go beyond contemporaneous outcomes and have been shown to shape the human capital of younger residents among other relevant outcomes. (Chetty et al., 2016; Blau et al., 2019).

Our focus is a place-based policy intervention, the Micro Market Recovery Program (MMRP), implemented by the city of Chicago in 2011 in response to the foreclosure crisis. The program sought to rebuild and restore distressed communities via a number of policy channels. It encouraged public-private and non-profit partnerships to reinvest in renovating vacant buildings and also provided financial counseling, forgivable home repair loans, and credit for a down payment to private individuals facing negative housing conditions (City of Chicago, 2019). Overall the objective was to improve the general upkeep of designated areas by revitalizing their existing private housing stock.

We evaluate the net effect of this multi-faceted program on different types of crimes, paying attention to the concern that treated neighborhoods were already under housing stress and hence may not be comparable to untreated areas. To this end, we employ a number of distinct and mutually reinforcing empirical strategies in our analysis. First, besides standard panel and time-fixed effects, our main identification strategy also allows for linear unit-specific trends. Under this setting, we present evidence that MMRP and non-MMRP neighborhoods had similar pre-treatment crime dynamics, across multiple offense categories, making the analysis amenable to a difference-in-differences (DID) estimation. Throughout our analysis, we employ the approach developed in De Chaisemartin and d'Haultfoeuille (2020) to account for any potential negative weighting concerns in our DID design. Second, we also present a sensitivity analysis that allows for any parametric violations of pre-trend as recently proposed by Rambachan and Roth (2023). Third, our main results hold even when we employ a matched control group formed only by non-treated areas that were similar in demographics and housing characteristics to the treated group in the pre-treatment period.

In order to confirm the program's takeup, the first part of our analysis shows an increase in building and construction activity as well as loan applications for housing purchases in treated neighborhoods. The surge in construction activity is restricted to the treated areas and is not experienced by neighboring areas not eligible for the program. Additionally, we document no discernible changes in business activity in areas where the MMRP was implemented. The latter two findings help rule out potential confounders stemming from general economic and/or construction booms in treated areas.

Next, we establish that housing interventions provided by the program indeed lead to changes in criminal activity in treated areas. We estimate substantial reductions in property crimes, in particular, 11% in burglaries and 7% in thefts. We provide evidence that this is not a result of a simple displacement of crime to nearby areas and in fact show that adjacent areas also benefited from these reductions in crime. In particular, we observe a statistically significant decrease in burglaries in neighborhoods up to two kilometers away, but the effect sizes diminish as the distance from the focal blocks increases. We also document an appreciable drop of 5.4% and 8.5% in robberies in the treated blocks and their immediate neighbors, respectively. However, only the latter is statistically significant. Overall, we document a sizable reduction in serious property offenses like burglary and robberies in and around the treated areas.

On the other hand, treated areas also experience an increase of 5% in simple assaults and 5.5% in trespassing violations. The latter are particularly interesting as they provide a link to a potential increase in vigilance that has often been linked to the influx of more affluent residents, who have tastes for lower crime prevalence and hence are more likely to report even minor infractions. Indeed, we present empirical evidence that potentially links the rise in minor offenses to the changing demographic characteristics of treated areas.

Some of the earliest work in this literature was either case-study-based or exploited cross-sectional variation, making it difficult to establish causal links.[1] A more recent strand of the literature uses quasi-experimental variation in neighborhood built-environment to study this question. This is helpful, since it is difficult to directly observe the entire stock of neighborhood amenities and

[1] For an excellent review of this literature please refer to MacDonald (2015).

flows into or from it, urging researchers to largely focused on certain key elements. For instance, Ellen, Lacoe, and Sharygin (2013) and Cui and Walsh (2015) establish that foreclosed properties lead to increases in violent and public disorder crime. Various cities in the United States have also adopted policies to demolish abandoned buildings in part motivated by the assertion that they prove to be magnets for criminal activity. Aliprantis and Hartley (2015); Sandler (2017); Larson et al. (2019) find that demolition of high-rise public or obsolete housing led to a decrease in local crime, especially violent offenses. On the other hand, Diamond and McQuade (2019) show that publicly funded housing programs that attracted relatively higher-income households contributed to an overall reduction in neighborhood crime rates albeit only in poorer neighborhoods.[2] Interestingly, less attention has been devoted to studying interventions seeking to repair the existing private stock of housing. One exception is the study by South et al. (2021), which documents a 22% reduction in crimes in areas where houses of low-income owners received structural repairs.[3]

Conceptually, a major potential mechanism argued in the above literature is the so-called broken window theory (Wilson and Kelling, 1982) which argues that dilapidated and blighted neighborhoods can signal low surveillance to potential offenders, providing pathways to an increase in criminal incidence. Under this theory, by improving the private housing stock, the MMRP could signal higher surveillance and social order, having a deterrence impact on crime. Under the Becker (1968) model of criminal behavior, this increased surveillance can increase the probability of apprehension reducing the expected gains from a potential criminal enterprise in these neighborhoods. Higher surveillance may also lead to overzealous reporting of minor offenses like trespassing on private property, a finding that we document in our analysis as well. However, finding concrete empirical support for the existence of such effects has been elusive (Caetano and Maheshri, 2018).

Another channel through which housing improvement can impact crime rates is a change in the composition of residents. Neighborhood flux, even when it leads to socioeconomic improvements, tends to have a destabilizing influence that results in increases in crime in the short term (Kirk and Laub, 2010). Researchers have studied the effect of gentrification itself on neighborhood

[2] An earlier study by Freedman and Owens (2011) evaluating the same federal program, the Low-Income Housing Tax Credit (LIHTC) reached similar conclusions for violent offenses at the county level but found no associated reductions in property crime.

[3] However, the overall evidence is mixed: Spader, Schuetz, and Cortes (2016) find no measurable effect of such rehabilitation activities on the crime rate in three different cities in the U.S, while Ukert et al. (2023) found an increase in crime in West Philadelphia neighborhoods where vacant properties were renovated.

crime rates.[4] Papachristos et al. (2011) link the diffusion of coffee shops in a neighborhood, their proxy for gentrification, with reductions in homicides but an increase in street robberies. Autor, Palmer, and Pathak (2017) directly measure gentrification using the cessation of rent control, and estimate a 16% reduction in overall crime, particularly in property crime. Of course, the above two channels can also feed into each other as the influx of more affluent residents may also lead to other improvements in the upkeep of the neighborhood triggering broken-window theory-based explanations. Empirically isolating these channels is a complicated identification problem but we try to provide some suggestive evidence, particularly in support of gentrification as a plausible mechanism, as part of our analysis.[5]

The rest of the paper is organized as follows. Section 2 provides details about the program, lists our data sources, and presents descriptive statistics. Section 3 explains the methodology. Section 4 discusses and interprets the results, while Section 5 studies potential mechanisms. Finally, Section 6 provides some concluding discussion.

## 2 Background and Data

### 2.1 Micro Market Recovery Program

The MMRP is an initiative launched by the city of Chicago's Mayor Rahm Emmanuel in 2011 as a response to the foreclosure crisis. The program's main goal was to fortify distressed communities by reducing the cost of home ownership and financing renovations on targeted neighborhood blocks. The program provides housing support to local families through resources from private, public, and non-profit partnerships, and comprehensively utilizes those resources to address the specific needs of targeted communities (City of Chicago, 2019).

The main idea is to work with an area's existing housing stock by stimulating the reoccupation of vacant properties and helping residents by connecting homeowners with financial assistance for home repairs, foreclosure counseling, and legal or technical assistance.

[4]Gentrification is defined as the process whereby the character of a poor urban area is changed by wealthier residents moving in, improving housing, and attracting new businesses, typically displacing current inhabitants in the process (Oxford Languages and Google accessed on March 2023).

[5]Another related channel through which the program could impact crime is by providing access to the credit market to even existing residents. Previous studies have documented that psychological stress induced by liquidity constraints may be an important driver of criminal behavior, for instance, in the wake of job loss (Britto et al., 2022).

The program also provided $15,000 in down payment assistance to eligible individuals interested in buying a house in designated areas. Qualified homeowners can also benefit from counseling from an accredited agency. Individuals interested in purchasing or rehabilitating a home in an MMRP area may also be eligible to receive subsidies through the Neighborhood Stabilization Program (NSP) or the tax increment financing promoted by the city of Chicago.[6] The program also offered forgivable loans up to $35,000 to help current owner-occupants with home repairs. Requests for grant funds were allocated based on the completion date of all application requirements and by nature of the repair with priority to health and safety repairs, energy efficiency improvements, and repairs to the exterior of the home.

The program was launched in 2011 targeting "micro markets" in nine areas, and was expanded to four more areas in 2013. While the exact procedure used for designating a neighborhood eligible was not made public, the Mayor in his original announcement as well as subsequent communications outlined the factors considered in the selection of MMRP areas. Five related features of a neighborhood determined their eligibility into the program: i) areas undergoing high rates of foreclosures even after the dissipation of the so-called foreclosure crisis which roughly lasted from 2007 to 2010; ii) neighborhoods where although a large number of properties are vacant there is still residual market interest such that revitalizing efforts can be successful; iii) in the absence of the program there was limited private activity in the housing markets to buttress supply; iv) institutional capacity at the neighborhood exists at an adequate level to encourage community involvement; and v) detailed data on property ownership is available which can help intricately target individual housing units (based on the original press release announcing the program from the Office of the Mayor (2011) and Annual Action Plan by City of Chicago (2016)).

In addition to the above, other extremely localized, neighborhood-level factors were also considered in designating an area eligible for support under MMRP. For instance, in the case of the Humboldt Park area, the MMRP's involvement was part of a broader strategy to revitalize the neighborhood by leveraging its proximity to key amenities like Green Line train stations and the

[6] That is the MMRP program does not preclude participation in other housing programs implemented by the city. The NSP provides emergency assistance to state and local governments to acquire and redevelop foreclosed properties that might otherwise become sources of abandonment and blight within their communities. This program provides grants to every state, certain local communities, and other organizations to purchase foreclosed or abandoned homes and to rehabilitate, resell, or redevelop these homes to stabilize neighborhoods and stem the decline of the values of neighboring homes. The program is authorized under Title III of the Housing and Economic Recovery Act of 2008.

Garfield Park Conservatory. These attributes made certain blocks within the neighborhood particularly attractive for the program's investment and intervention efforts DePaul Institute for Housing Studies (2014).

The thirteen designated target areas are mapped in Figure 1. On average, an MMRP area has 80 census blocks. In each targeted area, a community partner organization assists individuals and households interested in the program. Besides the requirement that the property targeted for purchase or repair must be located within one of the designated areas, to be eligible citizen must have moderate household income, earn up to 80% of the area median income, and not owe the city any delinquent debt (like parking tickets or water debt). The property must be owner-occupied to qualify, i.e., applicants cannot use funds from the program on a rental property.

Around $12.8 million was budgeted for funding the program until 2018. The resources came from the city's budget, grants from various nonprofit agencies, and the Illinois attorney general's office settlement with major banks accused of questionable lending practices related to the foreclosure crisis.[7] In 2015, for instance, the Department of Planning & Development allocated about 25% of MMRP funds to be used for home purchase and 75% for home improvement grants (City of Chicago, 2015). Since its inception, MMRP has assisted over 4,483 units, including the reoccupation of 1,300 vacant and abandoned housing, the reduction of home ownership cost for 427 families, and counseling over 1,300 people on housing-related issues.[8]

[7] https://www.usnews.com/news/best-states/articles/2018-07-09/chicago-targets-zombie-housing-for-renewal-block-b accessed on February 2019.

[8] https://www.lisc.org/chicago/our-work/comprehensive-community-development/housing/mmrp/ accessed on February 2019.

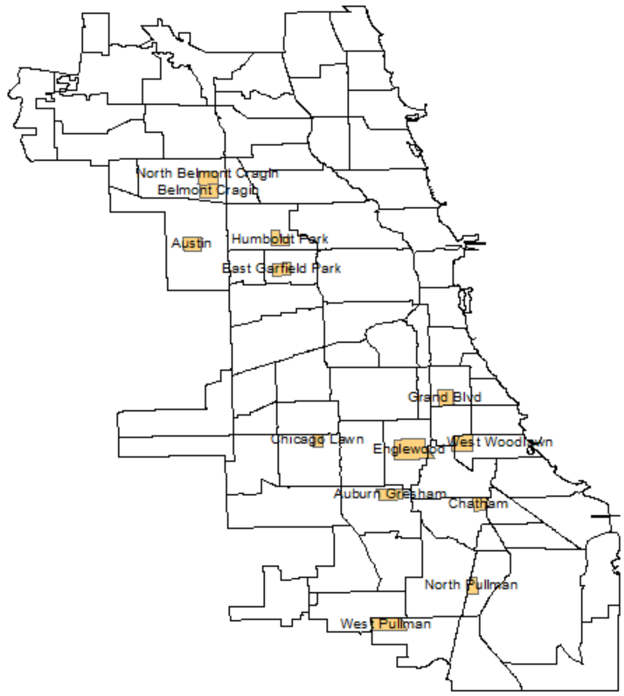



Figure 1: MMRP zones in orange and community borders.

## 2.2 Data

We assemble data from a number of sources to form a yearly longitudinal dataset of census blocks for the City of Chicago from 2006 to 2018.[9] Most of the data come from the city of Chicago open data portal, which releases the MMRP zones map, detailed information on criminal incidents reported to the police, and data on building permits and business licenses issued by the city administration.[10]

Information on crime is extracted from the Chicago Police Department's CLEAR (Citizen Law Enforcement Analysis and Reporting) system. This dataset reflects reported incidents of crime that occurred in the City of Chicago and were processed by law enforcement agencies, i.e., the reporting officer completed a case report.[11] In other words, they include all reported offenses irrespective of whether they were cleared by the police department or an arrest was made during the investigation. It is important to note that the Chicago Police Department dynamically updates

[9] We use the 2010 U.S. Census boundaries for a census block and consolidate our data across years to conform correctly to this mapping.

[10] https://data.cityofchicago.org/ accessed on October 2019.

[11] For instance, if the police responds to a 911 call and concludes that an offense was not committed then such an incident would not be included in the CLEAR reporting system.

the original reported offenses and amends or corrects any potential errors that may be part of the preliminary report. Since we are accessing historical data from 2006 to 2018 where the latest year of data was accessed at least 3 years after the original report, the likelihood of potential measurement error is minimized. Nevertheless, we reiterate that the crime figures used in this study are based on reported offenses only and do not measure police response/effort in resolving these incidents. The data include coordinates corresponding to the most proximate block address to where the offense occurred. Using the nearest distance, each incident is then linked to a census block. The data also report the crime category according to the classification from the FBI Uniform Crime Reporting (UCR) program.

Since one portion of the intervention focuses on home repair, information about building permits issued by the city of Chicago can help explore the actual take-up of the program. A building permit is required for new building construction, demolition, renovations, the installation of heating and cooling systems, or any plumbing or electrical work. The Department of Buildings provides information of all permits issued by the city since 2006. The building address associated with a permit is geolocated, which allows us to link a permit to the relevant census block. Similarly, we also collect data on business licenses issued by the Department of Business Affairs and Consumer Protection. We use this information to measure changes in local economic development as a potential confounder. The data contain the location of the business and the date of application completion, allowing us to link an issued license to the census block and year.

Three additional data sources merit discussion. These data only provide information for larger geographic aggregations and are not available at the census block level. We employ data on loan applications provided through Home Mortgage Disclosure Act (HMDA) and compiled by the Consumer Financial Protection Bureau. We use these to study the impact of the program on home purchases.[12] We consider applications for owner-occupied homes which was an eligibility requirement for the program. The data are available at the year and census tract level from 2007 to 2017.

We also access data on foreclosures and mortgages activity per 100 residential parcels from the Institute of Housing Studies, Depaul University[13]. The variables are available at the community

[12]Data downloaded from https://www.consumerfinance.gov/data-research/hmda/historic-data/ on October 2019.

[13]Data downloaded from https://www.housingstudies.org/data-portal/ on October 2019.

level and their values in 2008 are used as explanatory variables to calculate the probability of being in the program for the matched DID analysis.

Finally, data on the 5-year estimates from the American Community Survey (ACS) were collected for information on the demographic and housing characteristics of a neighborhood. Specifically, we gather variables such as average per capita income, residential race shares, and occupancy rates at the census block group level for 2010 and 2016. Pre-treatment characteristics are used to calculate the probability of being in the program for the matched DID analysis. Additionally, these variables are utilized to investigate the program's potential effects on neighborhood composition and housing features.

### 2.3 Descriptive Statistics

To enhance our understanding of how the program is assigned within communities dealing with housing challenges and high foreclosure rates, and to later assess the MMRP's potential displacement effects, we categorize non treated areas into four mutually exclusive groups based on their proximity to the treated blocks. *MMRP neighbors* are blocks adjacent to treated areas whose centroid is within 250 meters of an MMRP area. *MMRP community* consists of all blocks whose centroid lies outside 250 meters of a MMRP area but within the boundary of a community that hosts a MMRP area. Finally, the third and fourth groups consider all blocks whose centroid lies within one kilometer of a MMRP community boundary and within one to two kilometers of a MMRP community boundary. Besides helping us to characterize the treated areas, these groups will allow us to assess the impact of the program on nearby areas that are not directly receiving program benefits, focusing on the extent to which these areas may experience displacement or other effects due to the program.

Table 1 presents the summary statistics for crime rates, housing metrics, and demographic details for all non-treated blocks, blocks within the MMRP area, and the four distinct classifications of nearby non-treated blocks as defined above. [14] The reported measures are for 2010, the year prior to the first year of the intervention.

MMRP-treated blocks compared to those without the intervention have higher counts of crime

[14] A "treated block" or MMRP block is identified as a census block whose centroid falls within a designated MMRP area. By this definition, a total of 1,047 blocks were considered as having received the treatment.

across all categories, a lower percentage of occupied properties (80% vs 87%), and a lower rate of homeownership (36% vs 53%). This is expected since the treatment was targeted at already distressed communities. This fact also manifests itself in foreclosure fillings in 2008 with MMRP areas having twice as high a rate as non-MMRP blocks. (7.17 vs 3.81).

We also observe that compared to either all non treated blocks or even nearby blocks, MMRP blocks are quite distinct in some key demographic characteristics. For instance, MMRP blocks are located in block groups with a lower percentage of residents with college degrees, more households with annual income below $25,000, and a substantially higher share of minorities. From these comparisons, we learn that indeed the program targeted particular types of neighborhoods, where housing and socioeconomic conditions were less favorable.

However, since we implement a DID design, these pre-existing differences will only impact our estimation if our outcomes of interest also coevolve differentially, i.e., a failure of the DID identification assumption - the so-called parallel trends. In Section 4, we present a number of exercises establishing that our analysis does not suffer from the above concern.

Table 1: Summary statistics in 2010 by treatment status.

| | (1) Not MMRP | (2) MMRP | (3) MMRP Neighbors | (4) MMRP community | (5) 1km MMRP community | (6) 1-2km MMRP community |
|---|---|---|---|---|---|---|
| Burglary | 0.59 | 1.09 | 0.89 | 0.83 | 0.76 | 0.58 |
| Theft | 1.74 | 1.91 | 1.96 | 1.57 | 1.56 | 1.48 |
| Motor vehicle theft | 0.43 | 0.68 | 0.72 | 0.57 | 0.51 | 0.45 |
| Robbery | 0.32 | 0.70 | 0.75 | 0.49 | 0.39 | 0.28 |
| Assault/Battery agg | 1.10 | 2.74 | 2.49 | 1.68 | 1.52 | 1.01 |
| Criminal sexual assault | 0.05 | 0.07 | 0.08 | 0.06 | 0.05 | 0.04 |
| Assault/Battery simple | 0.83 | 1.63 | 1.50 | 1.06 | 0.98 | 0.76 |
| Criminal damage | 0.91 | 1.45 | 1.33 | 1.07 | 1.08 | 0.89 |
| Trespass | 0.21 | 0.38 | 0.39 | 0.22 | 0.23 | 0.15 |
| Building permits | 0.79 | 0.62 | 0.69 | 0.58 | 0.59 | 0.63 |
| Business licenses | 0.23 | 0.15 | 0.17 | 0.13 | 0.15 | 0.18 |
| All loans application | 175.80 | 62.00 | 67.37 | 104.43 | 93.19 | 144.64 |
| Loans home purchase | 49.56 | 25.82 | 27.51 | 39.35 | 32.14 | 42.15 |
| Loans home improvement | 6.56 | 5.83 | 5.99 | 7.68 | 6.50 | 7.70 |
| Loans home refinancing | 119.68 | 30.35 | 33.87 | 57.39 | 54.55 | 94.80 |
| Community mortgages | 12.03 | 11.06 | 11.20 | 11.11 | 11.52 | 12.30 |
| Community foreclosure filings | 3.81 | 7.17 | 6.81 | 6.47 | 4.98 | 3.68 |
| Median year moved in | 2000 | 2001 | 2000 | 1999 | 1999 | 2000 |
| % white residents | 0.40 | 0.06 | 0.09 | 0.11 | 0.20 | 0.38 |
| % black residents | 0.39 | 0.81 | 0.80 | 0.75 | 0.64 | 0.42 |
| % residents less high school | 0.21 | 0.28 | 0.27 | 0.25 | 0.24 | 0.21 |
| % residents high school | 0.45 | 0.54 | 0.54 | 0.55 | 0.52 | 0.46 |
| % residents college | 0.33 | 0.18 | 0.19 | 0.20 | 0.24 | 0.33 |
| % household income $<$ 25K | 0.29 | 0.47 | 0.43 | 0.38 | 0.36 | 0.28 |
| % household income 25-50K | 0.24 | 0.25 | 0.26 | 0.26 | 0.27 | 0.24 |
| % household income 50-100K | 0.29 | 0.22 | 0.23 | 0.26 | 0.26 | 0.31 |
| % household income 100-150K | 0.11 | 0.05 | 0.05 | 0.07 | 0.08 | 0.10 |
| % household income $\geq$ 150K | 0.07 | 0.02 | 0.02 | 0.03 | 0.03 | 0.07 |
| % buildings occupied | 0.87 | 0.80 | 0.81 | 0.84 | 0.84 | 0.87 |
| % tenure is owner | 0.53 | 0.36 | 0.39 | 0.49 | 0.50 | 0.55 |
| Observations | 42788 | 1047 | 1424 | 6963 | 8377 | 7004 |

# 3 Methodology

## 3.1 Baseline Empirical Strategy

We start with the following straightforward equation, where variation in treatment status across time and space identifies, $\beta$, the parameter of interest.

$$Y_{bt} = \beta \mathrm{MMRP}_{bt} + \eta_b + \lambda_t + \varepsilon_{bt} \tag{1}$$

In this equation, $Y$ represents the log of the number of crimes for a given offense type in census block $b$ in year $t$.[15] We use other dependent variables to study several aspects of the program. For instance, the number of construction permits issued to a given area is used to measure the program's take-up on housing repair. We also look into demographics and housing characteristics to study the potential impact of the program on neighborhood composition as a potential mechanism for changes in crime prevalence.

Our treatment indicator is denoted by $\text{MMRP}_{bt}$ and takes the value one if block $b$ receives the treatment in year $t$. $\eta_b$ and $\lambda_t$ represent block and year fixed effects respectively. The block fixed effects take into account pre-existing differences in the level of the dependent variable for treated and control units. While year fixed effects absorb overall time trends in the outcome variable. Finally, $\varepsilon_{bt}$ represents the error term. We cluster standard errors at the census tract level to account for the correlation in errors across blocks but within census tracts. These also align more closely with the designated treated areas of the MMRP and correspond with the recommendations by Abadie et al. (2023) for clustering at the geographic level at which treatment occurs.

The main identifying assumption in a DID design is the existence of parallel trends in the evolution of outcomes of treated and control units in the absence of treatment. That is, units in the control group form an appropriate counterfactual for what would have happened over time for the treated units in the absence of the program. We know based on descriptive statistics discussed in Section 2 that treated and non-treated blocks were distinct in terms of crime level, housing, and demographic characteristics, and thus may also differ with respect to crime trends in the pre-treatment period. Figure A.2 in the appendix shows raw time trends for different crime categories by MMRP status. While the pre-trends seem similar for a number of offenses, certain crimes like burglary do show non-parallel coevolution. Therefore, our baseline estimation strategy augments Equation 1 to include block-specific linear trends as illustrated in Equation 2 below. This is a popular approach in DID designs when the researchers are worried about pre-existing trends, see for instance Dobkin et al., 2018 and Goodman-Bacon, 2018.

$$Y_{bt} = \beta \text{MMRP}_{bt} + \eta_b + \lambda_t + \phi_b t + \varepsilon_{bt} \tag{2}$$

[15] We implement a $log(x+1)$ transformation to our crime variables to deal with zero observed crimes. We also provide estimates using the number of crimes on the level. These results can be found in the appendix and are qualitatively similar to those using the log approach.

$\phi_b t$ accounts for existing linear trends at the block level and thus addresses the potential failure of the parallel trend assumption. This approach is valid as far as a linear approximation is appropriate for capturing pre-existing trends for non-treated and treated areas. Therefore, in our analysis, we also implement the recently developed approach by Rambachan and Roth (2023) that bounds the treatment effect under potential violations of the parallel trends assumption away from linearity.

Throughout our analysis, we employ the approach developed in De Chaisemartin and d'Haultfoeuille (2020) to estimate both the average treatment effects (ATEs) as well as the dynamic treatment effects over time (i.e., event-study estimates). Recent findings in the econometric literature have cautioned empirical researchers about the existence of negative weighting when using two-way fixed effects to estimate Equations 1 and 2. The problem is exacerbated in staggered treatment designs, i.e. when there is variation in treatment timing (Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021). This is the case with our setup as well albeit muted: in nine areas (783 blocks) the program started in 2011, and in four other areas (264 blocks) the program started two years later in 2013.

### 3.2 An Alternative Empirical Strategy: Matched DID

We assess the appropriacy of the estimates obtained under our baseline strategy by implementing a distinct alternative strategy. Using ideas from the matching on observables literature, we identify non-treated areas that are likely to provide adequate counterfactuals to our treated areas.[16] First, we use propensity score matching to find untreated blocks which have similar pre-treatment housing and demographic characteristics compared to the treated MMRP blocks. We exclude neighboring blocks and blocks in the same community to MMRP-treated areas as potential propensity score matching, since those could be indirectly affected by the program as studied in section 4.3. We then estimate Equation 1 using only the MMRP-treated blocks and their closest "matched neighbors" from the first step (also employing De Chaisemartin and d'Haultfoeuille, 2020 for the estimation).

For the first step above, we estimate a Probit model where the outcome variable takes the value one if the block is ever treated and is zero otherwise. [17] In light of the program's motivation and the information obtained from the summary statistics in Table 1 we use the following explanatory

[16]We thank two anonymous referees for pointing us in this direction.

[17]However, since we do not observe demographic and housing characteristics at the block level, the covariates used in the matching exercise are defined at the block group level. In essence, this implies that the routine will pick up control blocks from within the same nearest matched block group.

variables measured in the pre-treatment period to predict the probability of being in the program. Community level number of mortgages and foreclosure filings per 100 residential parcels in 2008 are used to capture features of the local house market and foreclosure activity. We also use the percentage of buildings occupied, the share of residents who own their property, and the median tenure of residents in years, measured at the block group level. Finally, variables about the racial composition, schooling, and income level of residents are used to capture the socioeconomic demographics of treated areas. After the propensity score is calculated, for each treated block we select their closest match in terms of the propensity score. These non-treated blocks that had a similar and close probability of being treated as MMRP blocks are then selected to form the control group. Details on the propensity score matching estimation are available in Table A.2 in the appendix.

# 4 Results

## 4.1 Event-Study Style DID Estimates

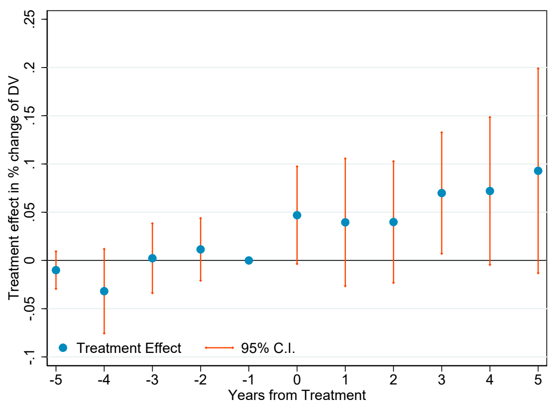
We start our analysis by presenting evidence that the MMRP indeed had an effect in changing the residential landscape of treated neighborhoods. We do not observe the exact residential building and/or property which used funding from the program. However, we have access to administrative data on all building permits issued by the municipal authorities during the sample period. Since the program requires repair works to be done by licensed and insured contractors who are required to obtain appropriate building permits[18], we estimate dynamic treatment effects of the program using building permits as the dependent variable.

Figure 2(a) provides event study estimates for permits for new constructions and renovations issued at the block level in treated areas compared to control areas based on Equation 2 and estimated via the approach developed in De Chaisemartin and d'Haultfoeuille (2020). The time period just before the initial treatment year is omitted for identification. We find no evidence that treated areas were evolving differently in the issuance of permits before the treatment. We see an increase, albeit noisy, in permits immediately after the program starts and it continues thereafter. The combined average treatment effect for treated blocks post-treatment is a 6% increase in construction and renovation permits. The latter estimate is significant at the 10% level. We

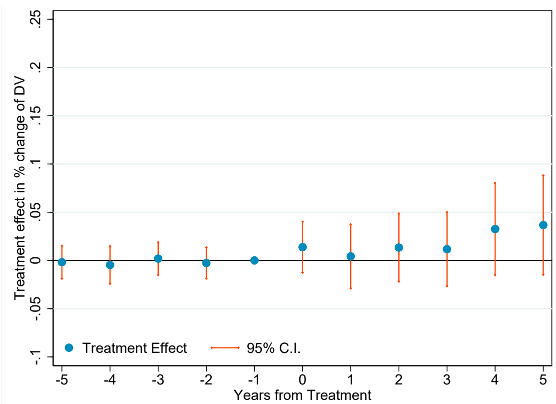
[18]https://www.picbchicago.org/micro-market-recovery-program-mmrp/ accessed on December 2019.

take the increase in repairs and renovations as evidence that the program was effective in helping residents to repair their properties, and in shoring up dilapidated properties making them fit to go back on the residential market. In appendix A.1, we also show an increase in home loan applications in census tracts where the treated blocks were located.

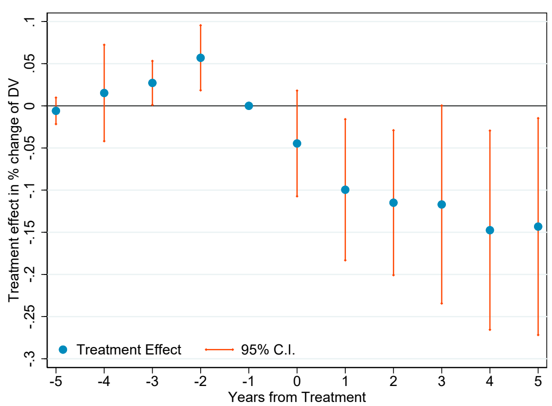
To rule out that the rise in construction activity is not driven by a general property development/economic boom happening in treated areas, Figure 2(b) examines the impact of the program on the issuance of business licenses. These include the opening or renewal of licenses for the operation of restaurants, cafes, and other commercial establishments. We estimate a close to zero and statistically insignificant effect on business licenses throughout the sample period. Therefore, it is unlikely that any potential MMRP effects on crime are coming from confounding trends in neighborhood economic growth. At the same time, the lack of meaningful impact on business licenses rules out change in economic activity as a mechanism of potential effects on crime (see, e.g., Papachristos et al., 2011).



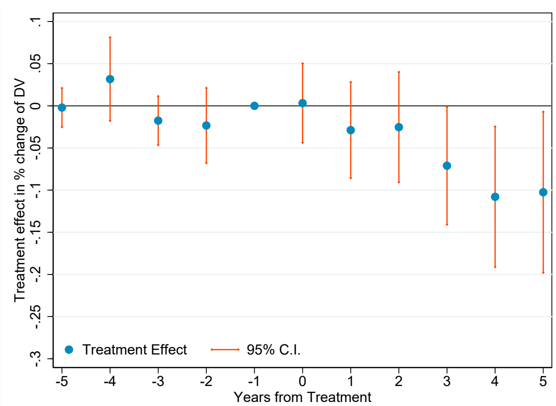

(a) Permits for New Construction and Renovations



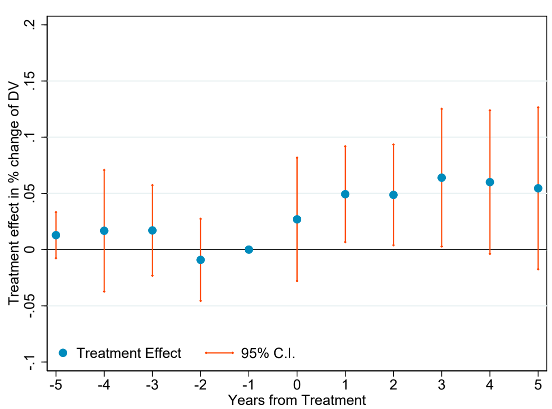

(b) Business Licenses



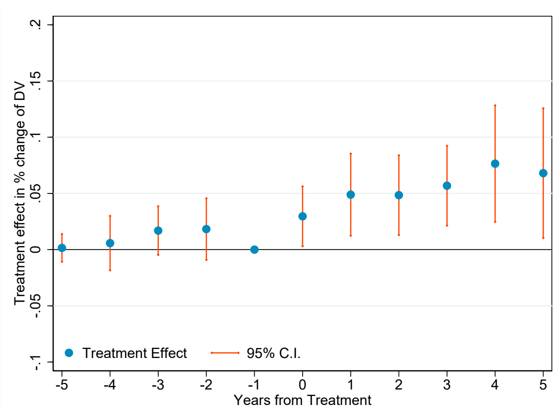

(c) Burglary




(d) Theft




(e) Simple Assault




(f) Trespass

Note: The plots show study event estimates of dynamic MMRP effects on outcomes in treated blocks using the method in De Chaisemartin and d'Haultfoeuille (2020). The sample size is 570,830 and estimations include block-fixed effects, year-fixed effects, and block-specific linear trends. We also control for dummy variables for nearby blocks beyond the MMRP areas used to study spatial spillover.

Figure 2: Event Study Style DID Estimates.

We now showcase event-study estimates for those crime categories where we uncover discernible treatment effects. Table 2 presents the full set of results for all offense categories under both our baseline specification given in Equation 2 as well as the matched DID approach.

The second row of Figure 2 presents event study estimates using two common property crimes as outcomes: burglaries and thefts. We document a substantial reduction in the incidence of burglaries post-treatment with the effect being persistent through the sample period. The average effect post-treatment amounts to an 11% drop in burglaries with the highest in a given year being 15%, five years after the start of program.

However, for burglary, we do witness some statistical significance in the pre-treatment time period albeit in the opposite direction, i.e. burglary seems to be trending upwards in treated areas. If in the counterfactual this trend had continued, sans treatment, then this could impact the interpretation of our findings. We assess the importance of this concern by implementing the sensitivity analysis proposed by Rambachan and Roth (2023). Their approach allows for violations of the parallel trend assumption and provides bounds on the estimated treatment effect in the presence of such violations post-treatment. Since our baseline setup employs a unit-specific linear trend for estimation and we are concerned about secular long-run trends in criminal offenses, we employ the "smoothness" approach outlined in Rambachan and Roth (2023). This allows for the differential pre-treatment trends to evolve smoothly over time, but bounds by how much the slope may change over contiguous time periods.[19] Appendix Figure 4(a) presents the results from this exercise. The estimated bounds on the treatment effect do not include zero even for the highest deviation from linearity in the pre-trends. This result allays the pre-trend concerns and helps us establish the validity of the above finding of a reduction in burglaries within treated areas after the implementation of program.

Moving on to larceny or theft, the most common type of property crime, we uncover no discernible pre-trends as shown in Figure 2(d). The treatment effect however only materializes in later years, after the neighborhoods have presumably gone through extensive renovation and rehabilitation. Thus the average reduction is estimated at only 5.5% if we aggregate all post-treatment effects. However, the reduction in thefts is as high as 10% five years after the program's implementation.

For minor offenses such as simple assault and trespassing on private property, the event-study estimates present a different picture than the results for property crimes. We estimate an increase

[19] We closely follow the implementation of the second empirical example given in Rambachan and Roth (2023) and consider values of $M$ between zero (linear violation) and 0.04.

in the incidence of these offenses following the rollout of treatment. These results could be due to population changes and gentrification sparked by the program. There are three main ways gentrification could result in an increase in less serious crimes. First, gentrified areas often see an increase in police presence Beck (2020), which could mechanically increase notifications of crimes to law enforcement. Second, new and current residents could become more vigilant and have a lower tolerance for petty crime, resulting in a greater number of incidents being reported. This can particularly explain the rise in trespassing offenses. Finally, residential turnover often severs existing social ties that are the foundation of social control and makes the formation of new ones challenging (Boggess et al., 2022), which could result in a rising of conflicts from social interactions in the short run. We explore these mechanisms more in later sections.

In summary, these results show that the program was effective in improving the private housing stock in treated areas, leading to a decrease in property crimes but resulting in an increase in less serious offenses such as simple assaults and trespassing.

## 4.2 Average Treatment Effects: Baseline and Matched DID Results

As discussed earlier, our treated and control regions differ in various neighborhood compositions and crime measures. In addition, the treatment itself was targeted at distressed neighborhoods, which may also imply baseline differences in treated and control areas. However, this becomes a concern only if these pre-existing differences also contribute to the dynamic evolution of our outcomes of interest across treated and control areas. Our earlier analysis found limited evidence of this, nevertheless, we adopt an alternative strategy to tackle this problem differently.

Table 2 presents the ATE for treated blocks on different categories of crime using two different methodologies: our baseline strategy of using all non-treated blocks as control but allowing for linear trends at the block level, and an alternative strategy that applies propensity score matching to select a subset of non treated blocks similar to MMRP blocks in terms of housing and demographic characteristics.[20] Comparisons across these approaches allow us to verify the robustness of our main findings.[21]

[20]Event-study estimates for motor vehicle theft, robbery, aggravated assault, sexual assault, and criminal damage are presented in the appendix.

[21]Table A.2 in the appendix provides details on the propensity score matching estimation and tests of difference of means and pre-tends.

Table 2: MMRP's average treatment effects in treated blocks according to different identification strategies.

| | Baseline | Alternative |
|---|---|---|
| Panel A: Property crimes | | |
| Burglary | -0.111** | -0.079*** |
| | (0.049) | (0.030) |
| Theft | -0.055* | -0.049* |
| | (0.031) | (0.029) |
| MV Theft | 0.012 | 0.016 |
| | (0.026) | (0.033) |
| Panel B: Violent Crimes | | |
| Robbery | -0.054 | 0.018 |
| | (0.036) | (0.036) |
| Agg. Assault | -0.003 | -0.017 |
| | (0.027) | (0.037) |
| Sex. Assault | 0.010 | 0.010 |
| | (0.014) | (0.014) |
| Panel C: Less serious offenses | | |
| Sim. Assault | 0.051** | 0.070 |
| | (0.025) | (0.056) |
| Crim. Damage | -0.040 | -0.050 |
| | (0.026) | (0.039) |
| Trespass | 0.055*** | 0.049 |
| | (0.019) | (0.034) |

Note: The table presents the estimated effect of the program for MMRP blocks on a given dependent variable (rows). First column shows the estimates based on Equation 2, using all non treated blocks as control group. The sample size is 570,830 and estimations also include dummies for treatment on nearby blocks beyond the MMRP areas used to study spatial spillover. The second column refers to estimation of Equation 1 using a subsample of treated blocks and their closest neighbors according to propensity score matching. In this approach the sample size is 28,756. All estimations follow the method in De Chaisemartin and d'Haultfoeuille (2020), which accounts for variation in treatment time. Standard errors clustered at the census tract level in the first column I and at the block group level in the second column are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Overall the estimates are consistent across methodologies. For instance, under the baseline strategy, the MMRP decreases burglaries by 11%. The alternative approach returns a comparable estimate of 8% reduction in burglaries, statistically significant at the 1% level. Similarly, estimates for theft are also consistent across methodologies. Theft incidents are estimated to fall by 5.5% under the baseline strategy and by 5% under the alternative strategy.

In Table 2, we also report offenses for other crime categories in our data that were not covered in Figure 2. For motor vehicle theft we obtain similar point estimates under both approaches, but their magnitude is small and they are statistically insignificant. Results for serious violent crimes are inconclusive. We observe some discrepancies in the point estimates for robbery and aggravated assault but both approaches yield noisy and statistically insignificant results. The lack of significant estimates for violent crimes is not surprising as this category of offenses is less responsive to economic incentives as described in crime models. That is especially true for violent crimes involving interactions between people and within families (Draca and Machin, 2015). Finally, in Panel C we report estimates for less serious offenses. These results are fairly similar across methods for all crime categories, but they are only statistically significant under the baseline strategy.

In general, these comparisons support the validity of our main empirical strategy and confirm the finding that the MMRP persistently reduced burglaries and thefts, while increasing cases of simple assault and trespassing. We are not able to precisely point out the effects on serious violent crimes, however, if anything the effects are modest or towards reductions in robberies and aggravated assaults.

### 4.3 Spatial Spillovers in Crime

A neighborhood-level change capable of impacting crime rates, like housing renovations or occupancy of vacant properties, is likely to affect areas beyond just the immediate vicinity. Revitalization of a given neighborhood can often spark economic prosperity in nearby areas as well. In the context of the MMRP, housing incentives in treated areas could heat the housing market, push private investment in surrounding streets, and generate cascading spatial effects on housing improvement. On the other hand, meaningful crime reductions in treated areas may come at a cost of crime redistribution to other neighborhoods. If crime is simply displaced to nearby locations, then any social welfare benefits of MMRP would be muted. Therefore, a full evaluation of a place-based policy in the context of dense urban areas should also take into consideration potential positive or negative externalities to nearby areas.

In order to estimate displacement effects on crime we use Equation 2 to study additional variables of interest: treatment indicators for the areas beyond the MMRP blocks. We consider four spatially close groups of non-treated blocks. "MMRP neighbors" are blocks adjacent to treated areas whose centroid is within 250 meters of an MMRP area. "MMRP community" consists of all blocks whose centroid lies outside 250 meters of an MMRP area but within the boundary of the community in which an MMRP area is located.[22] Finally, the third and fourth groups consider all blocks whose centroid lies within one kilometer of an MMRP community boundary and within one to two kilometers of an MMRP community boundary. On average, these treatment zones expand our baseline treatment zone to a radius of about 4000 meters. For estimation we separately implement the estimator in De Chaisemartin and d'Haultfoeuille (2020) for each of the above treatment indicators, while controlling for indicators for all other treatment zones.

Table 3 reports average treatment effect estimates of the program's spillovers on criminal incidents beyond the treated areas. The spatial groups we study are formed by mutually exclusive groups, and in general, the distance from an MMRP site is increasing with the table's columns. Panel A of Table 3 shows the program's effect on property crimes. For burglary, we document substantial reductions both in the immediate neighborhoods as well as beyond - up to one kilometer away from MMRP communities. However, theft incidents decrease only in MMRP blocks, and we do not detect spillovers to nearby areas.

In Panel B, we turn to violent crimes. While robbery is generally classified as a violent offense, there is a financial motive involved as well, such that it can be classified as an acquisitive crime. Robberies fall by 5.4% in MMRP blocks and 8.5% in neighbor blocks. However, only for neighbor blocks the effect is statistically significant. If we combine these two areas into a consolidated treatment zone, we uncover statistically significant reductions in the prevalence of robbery as well. For other more serious violent offenses, we generally observe imprecise point estimates but overall there seems to be limited impact on crimes of this nature.

Finally, in Panel C we see that for less serious crimes, estimates are sizeable and statistically significant only for treated areas. The fact that we do not observe meaningful changes in minor

[22] The city of Chicago defines 77 spatial aggregations of city blocks that comprise organic communities.

offenses in nearby areas reinforces the argument that changes in the composition of residents could explain the growth of minor offenses in treated areas.

Table 3: MMRP's average treatment effects on categories of crime in treated and nearby areas.

| | (1) MMRP | (2) MMRP Neighbors | (3) MMRP community | (4) 1km MMRP community | (5) 1-2km MMRP community |
|---|---|---|---|---|---|
| Panel A: Property crimes | | | | | |
| Burglary | -0.111** | -0.049* | -0.095*** | -0.077*** | -0.017 |
| | (0.049) | (0.026) | (0.021) | (0.017) | (0.017) |
| Theft | -0.055* | 0.004 | -0.016 | -0.023 | -0.002 |
| | (0.031) | (0.039) | (0.020) | (0.017) | (0.016) |
| MV Theft | 0.012 | -0.036 | -0.018 | 0.001 | -0.005 |
| | (0.026) | (0.030) | (0.014) | (0.012) | (0.019) |
| Panel B: Violent Crimes | | | | | |
| Robbery | -0.054 | -0.085*** | -0.020 | 0.002 | -0.002 |
| | (0.036) | (0.033) | (0.014) | (0.012) | (0.008) |
| Agg. Assault | -0.003 | -0.037 | -0.012 | -0.025** | 0.001 |
| | (0.027) | (0.028) | (0.019) | (0.011) | (0.011) |
| Sex. Assault | 0.010 | -0.003 | -0.002 | 0.004 | -0.008* |
| | (0.014) | (0.010) | (0.005) | (0.004) | (0.004) |
| Panel C: Less serious offenses | | | | | |
| Sim. Assault | 0.051** | 0.012 | -0.005 | 0.005 | -0.010 |
| | (0.025) | (0.027) | (0.014) | (0.012) | (0.012) |
| Crim. Damage | -0.040 | 0.019 | 0.001 | -0.023 | -0.008 |
| | (0.026) | (0.030) | (0.019) | (0.019) | (0.021) |
| Trespass | 0.055*** | 0.013 | 0.003 | -0.002 | 0.009 |
| | (0.019) | (0.024) | (0.011) | (0.008) | (0.007) |
| Panel D: Permits and business licenses | | | | | |
| Const. + Renovations | 0.060* | 0.019 | -0.016 | 0.016 | -0.003 |
| | (0.032) | (0.027) | (0.015) | (0.014) | (0.013) |
| Business licences | 0.019 | 0.001 | 0.008 | -0.001 | 0.005 |
| | (0.019) | (0.014) | (0.007) | (0.007) | (0.008) |

Note: Each cell presents the estimated effect of the program for a treated group (columns) on a given dependent variable (rows). Estimations for each outcome variable and treatment area are done separately, however, treatment dummies for the other treated areas are included as controls. Treatment groups are mutually exclusive. The sample size is 570,830 and the estimations include block fixed effects, year fixed effects, and blocks linear trends as presented in Equation 2. The table shows ATEs estimated using the method in De Chaisemartin and d'Haultfoeuille (2020). Standard errors clustered at the census tract level are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

While considering the spatial impact of the program, we revisit the building permit analysis presented in Section , which shows that the MMRP indeed led to an increase in construction and renovation permits in treated blocks. We extend this analysis to the above-defined spillover zones. This allows an alternative way to study whether localized economic shocks, which are likely to be bigger than a census block, are a potential source of confounding elements. In the last panel of Table 3, we see that while treated blocks witness an increase of around 6% in the number of construction and renovation building permits issued, adjacent blocks to MMRP areas, where properties did not qualify for the program, do not experience any changes in construction activities. We capture a similar pattern of findings for the issuance of business licenses as well. These last two checks reestablish that the above-estimated reductions in crime prevalence are indeed likely to be a direct result of MMRP-induced housing improvement in treated neighborhoods only.

There is a persistent concern in the economics of crime literature regarding the actual efficacy of crime-reducing policies since they may lead to just a redistribution of the criminal offense landscape. Therefore, the evidence in this section is important as it establishes reductions in crime in treated regions are not a result of mere displacement of crime to nearby areas. In fact, we establish that the reduction in burglaries and robberies extends beyond the focal treated area, while the increase in minor offenses is only concentrated in the treated blocks.

# 5 Potential Mechanisms

So far we have established that a policy targeted at improving the housing stock in distressed neighborhoods leads to important welfare consequences, in the form of property crime reductions. We now explore some of the potential mechanisms that may help explain the causal link between neighborhood improvement and crime.

## 5.1 MMRP and Neighborhood Composition

The housing incentives offered by the MMRP could directly affect the composition of residents in treated areas. A distinct strand in the economics of crime has explored the impact of gentrification on neighborhood crime prevalence (e.g., O'Sullivan, 2005; Porreca, 2023). In our context, it is difficult to study changes in neighborhood composition over time at as finely grained aggregation as census block. The only data source that provides a workable solution is the ACS, from which we collect data on 5-year estimates at the block group level. We collect these data for 2010, one year before the institution of MMRP as a policy, and for 2016.[23] Since we do not have an annual series by block pre-treatment to check for parallel trends, we implement the matched DID approach to construct a comparable group to our treated blocks as described in our alternative strategy.

[23]The collection at these two points in time avoids overlapping pre-treatment and post-treatment years since ACS 5-year estimates also uses data from the four previous years.

Table 4 presents the findings from this exercise. First, we do uncover an increase in the affluence of residents in treated blocks: a 3.8 percentage points reduction in the share of residents in the lowest income bracket (< $ 25,000) and a 3.2 percentage points increase in the share of residents within the third highest income bracket ($50,000-100,000). Given that the MMRP areas were already distressed neighborhoods, it would be unlikely to see an increase in the share of the highest income bracket. Moreover, the program's legibility required applicants to have low or moderate household income, earning up to 80% of the median income for the Chicago Metropolitan Area. Therefore it is not surprising to see meaningful changes within these income ranges. Second, while MMRP areas do not experience a change in the share of white residents, the program is associated with 2.2 percentage points decrease in the share of black residents. In terms of educational attainment, MMRP blocks observe a decline in the share of residents with college, which is counterbalanced by an increase in the share of residents with high school degrees.

Third, we use these data to also corroborate our earlier findings regarding the effect of the program on housing measures. We document meaningful and statistically significant increases in occupancy and house ownership rates by three and four percentage points, respectively. There is also a significant decrease in the median year residents moved in, which coupled with homeownership, could signal retention of current residents and improvement in housing stability. These results are in line with the goals of the program and empirically confirm that the MMRP was effective in stimulating housing occupancy, homeownership, and residential permanence.

Given that we observe data for only two years around the program, we interpret the above estimates with caution. In summary, neighborhood compositional changes in terms of income and race in the treated areas largely align with changes associated with gentrification. Since we observe gains in house occupancy and lower residential turnover, changes in the composition of residents in MMRP areas may be due to an influx of new residents rather than the replacement of current residents. Overall these estimates provide empirical support for gentrification-related mechanisms leading to the drop in serious property offenses documented earlier.[24] While population growth and changes in the profile of residents could explain the positive effect on simple assaults and trespassing violations through so-called eyes-on-the-street channels.

[24] In particular, higher occupancy of properties, higher home ownership, and lower residential turnover are likely the factors behind the program's de-escalating effects on property crimes as previously documented as well (Cui and Walsh, 2015; Ellen et al., 2013; Disney et al., 2020; Ihlanfeldt et al., 2018).

Table 4: MMRP's average treatment effects on demographic and housing outcomes in treated blocks.

| Dependent Variable | Coeff. | SE | Mean DV |
|---|---|---|---|
| % household income less than 25K | -0.038** | 0.015 | 0.47 |
| % household income 25K to 50K | 0.009 | 0.020 | 0.25 |
| % household income 50K to 100K | 0.032*** | 0.006 | 0.22 |
| % income 100K to 150K | 0.003 | 0.004 | 0.05 |
| % household income 150K or more | -0.006 | 0.007 | 0.02 |
| % white residents | 0.004 | 0.004 | 0.06 |
| % black residents | -0.022*** | 0.004 | 0.81 |
| % residents with less than high school | 0.002 | 0.010 | 0.28 |
| % residents with high school | 0.018* | 0.009 | 0.54 |
| % residents with college | -0.019*** | 0.006 | 0.18 |
| % buildings occupied | 0.028*** | 0.010 | 0.80 |
| % tenure is owner | 0.042** | 0.018 | 0.36 |
| Median year residents moved in | -1.047*** | 0.291 | 2001 |

Note: Each cell presents the estimated effect of the program on treated blocks on a given dependent variable (rows). The sample size is 4,424, which includes MMRP blocks and their closest neighbors according to propensity score matching. The outcome variables are obtained from the ACS 5-year estimates in 2010 and 2016. The estimations include block fixed effects and year fixed effects as presented in Equation 1. Standard errors clustered at the block level are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

## 5.2 Treatment Effect Heterogeneity by Neighborhood Characteristics

In this section, we explore heterogeneity in the estimated effects of the program by some salient neighborhood characteristics. This is a common approach taken in the empirical literature to shed some potential light on the mechanisms and channels through which the baseline effects are operating. At the very least, one can hone down to the characteristics of neighborhoods that are driving our main findings. We also explore if the effect varies by the intensity of take-up. We split our estimation sample into meaningful parts and re-estimate our baseline specification for all offense categories using the approach by De Chaisemartin and d'Haultfoeuille (2020). Throughout this exercise, we only report the average treatment effect across the entire treatment period.

First, using the municipal permit data, we split our estimation sample into above and below-median permit activity. Figure 3(a) shows that the point estimate for burglaries is indeed twice as large in areas with above median number of permits than in those with less activity. However, the decline in theft and the increase in minor assaults are concentrated in areas where takeup in construction is low. Therefore, we do not document monotonic effects across crime categories by the intensity of building activity in the post-treatment period.[25] Ukert et al. (2023) also uncover a non-monotonic relationship between crime and housing renovation intensity. Various factors

[25] We do not find many differences across other offenses and large standard errors do not allow us to reject the hypothesis that effects are identical across the two groups.

can rationalize such effects including the specific built environment of specific neighborhoods, the type of renovations undertaken, the elasticity of relevant offenses to the exact changes in housing amenities in the neighborhood. Given current data resources, it is difficult to empirically study these channels and this is a promising avenue for future research.

Next using pre-treatment data from the 2010 ACS, we match census blocks to their block group and then split the sample on salient neighborhood characteristics.[26] In Figure 3(b) we use information in the ACS that tracks the number of properties occupied in a block group. An area is characterized as having a low occupancy rate if, in the pre-treatment period, less than 80% of its properties are occupied. The motivation here is that places with low occupancy rates have the most potential for substantial and appreciable improvement in the house stock quality. Due to the sample split our standard errors become larger, however, we do observe that the point estimates for burglary, theft, robbery, trespassing, and criminal damage are all higher in low-occupancy areas. This provides evidence that the policy was indeed successful in reducing crimes in more distressed neighborhoods as measured by a higher share of vacant housing - a feature that the policy specifically targeted.

In Figure 3(c) we separate neighborhoods by whether they can be characterized as low-income or not.[27] While the baseline reduction in theft seems to be mostly driven by low-income neighborhoods, the decline in burglary is almost entirely concentrated in affluent areas. We also document a 10% reduction in aggravated assaults in poorer neighborhoods, a finding being washed in our aggregated estimates. Overall this implies that welfare gains due to housing improvement linked crime reductions are distributed across both low- and high-income neighborhoods and are a function of the type of offense one considers.

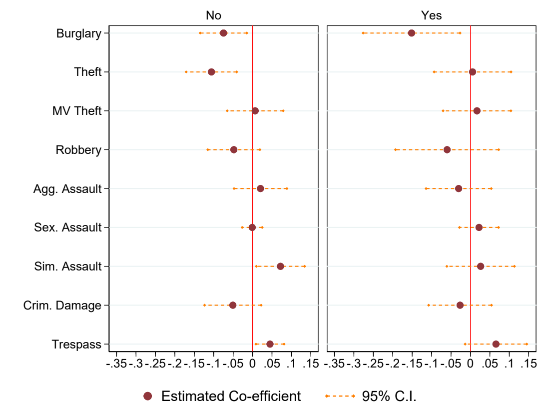
On the other hand, in 3(d) we repeat the above exercise for whether a neighborhood has a high share of minorities or not. Here we observe similar point estimates for almost all offenses across the two subsamples, albeit they are very noisy. However, the size of the point estimate for burglary is twice as high in those areas which had a lower minority share pre-treatment. Of notice we again use pre-treatment data for this exercise to avoid splitting the sample on an endogenous variable

[26] Cutoff points are selected based on observed characteristics of the main sample while trying to maintain comparable sample size of treated blocks in the two groups.

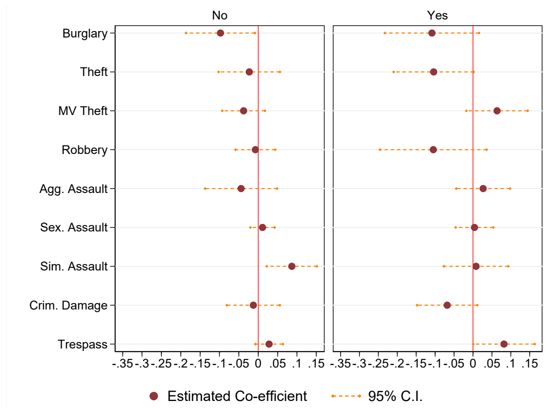
[27] A block group is defined as low-income if it has at least 50% of households with annual income below $50,000. Based on this classification 46% of the treated blocks are located in low-income neighborhoods.

like income or minority share, given the findings on gentrification in the previous subsection.

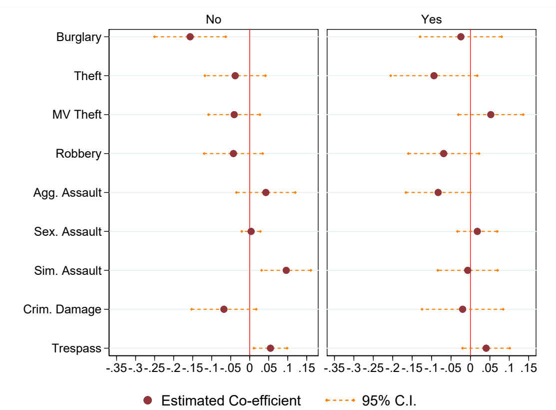
The takeaway from this section is threefold. First, although the program delivers crime reductions exactly in areas that had the lowest rate of residential occupancy pre-treatment, the reductions are not monotonically increasing in the housing renovation intensity. Second, the returns are distributed across both rich and poorer neighborhoods implying that housing improvements can lead to broader welfare gains. Third, since the program was relatively small in terms of coverage, our heterogeneity analysis is statistically less precise underscoring the need for more research in the future to determine the exact modalities of the diffusion of effects across varying neighborhood built environments.



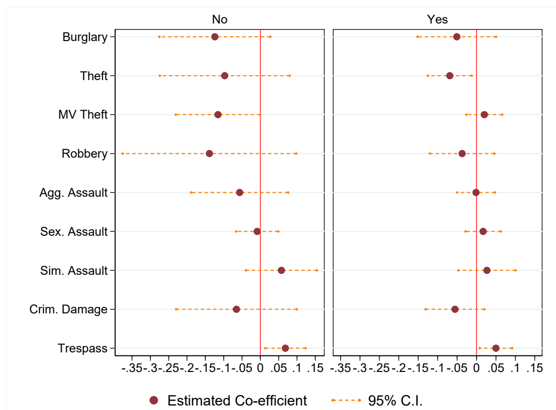

(a) Above Median # of Permits




(b) Low Occupancy Rate




(c) Low Income Neighborhood




(d) High Minority Share

Figure 3: Heterogeneity Analysis by Neighborhood Characteristic.

# 6 Discussion and Conclusion

This study evaluates whether neighborhood improvement by restoring and reoccupying existing private housing stock affects crime. Specifically, we study a program implemented by the city of Chicago in 2011 designed to promote housing reinvestment by reducing the cost of homeownership, attracting new owners to vacant buildings, and providing forgivable loans for home repair. To measure how the program affected local criminal activity we use rich geocoded incident-level crime data and block-level variation in exposure to the intervention. We also investigate possible spatial spillover effects into areas farther away from treated blocks.

Using municipal construction permit data we document improvements in private housing stock in targeted blocks. We also provide evidence for an increase in loan applications for home purchases following the program's rollout. Two findings prove that the intervention was indeed adopted and utilized by eligible residents.

We then estimate sizeable effects on local crime – with appreciable reductions in burglaries, thefts, and to some extent robberies. Importantly, we also show that the reductions in burglaries go well beyond the treated areas and the impact is discernible in neighborhoods close to two kilometers away from treated blocks. The reduction in robberies is also more salient when we consider treatment effect spillovers into nearby areas. However, we also find that the program has a small but positive impact on light and minor offenses such as simple assault and trespassing although these are concentrated only in treated areas.

Some key results make us confident that the intervention's effects on crime described above are causal. First, we only document a large increase in building permits in the treated areas and null effects in any nearby neighborhoods. Second, we do not observe sizeable or significant changes in business activity in treated areas, which indicates no confounding effects from trends in economic activity. Finally, the change in criminal activity is mostly concentrated in treated blocks and spillover effects are decreasing with distance from these blocks.

We also extended our analysis to explore the mechanisms behind the MMRP's effects on crime. We observe an increase in higher-income residents and a reduction in minority share. However, we also observe a decrease in college graduates which is offset by an increase in residents with high school degrees. Although it is empirically difficult to pinpoint which mechanism is driving the changes for which offense categories, overall it may be the case that gentrification drives the increase in minor offenses, particularly trespassing violations, as more affluent residents have a higher taste for low crime in their neighborhoods and hence are more likely to report even minor infractions to the police. This higher vigilance by residents along with the signaling-based theories outlined under the broken-window hypothesis may explain the precipitous fall in burglaries and robberies in and around the treated areas.

We take two approaches to contextualize our findings better. First, we contrast our results with comparable studies in the economics of crime literature that have evaluated other housing interventions. Second, we conduct a small back-of-the-envelope calculation to assess welfare gains in monetary terms.

As our main empirical specification is at the block level, we compare our estimates to studies conducted at a similar geographic aggregation.[28] For instance, Autor et al. (2017) conclude that a one-standard-deviation increase in gentrification in Boston, as measured by rent decontrol intensity, led to a 19% decrease in aggregate property crime and a 3.8% fall in violent crimes. Our comparable outcomes for what they include in their property category are burglary (-11.1%) and theft (-5.5%), and robbery (-5.4%) for their violent category. Rent de-control in Boston had profound impacts on gentrification with 40-80% increase in rents of deregulated units, and thus was an intense form of treatment; finding effect sizes in a similar ballpark after the implementation of a modest policy like the MMRP highlights the important role that neighborhood revitalization policies can play in crime reduction. Similarly, Sandler (2017) documents around a 2.5% reduction in burglary and theft at the block level after the demolition of public housing in Chicago. We find much larger effects for these offenses in the wake of housing improvement, again implying that revitalization of neighborhoods may have bigger welfare gains than demolishing decrepit/vacant housing altogether.

While it is difficult to place appropriate monetary values on the costs of victimization, both direct and indirect, a simple back-of-the-envelope calculation can provide some policy context to our findings. Chalfin (2015) provides an excellent review of the vast literature in criminology providing cost estimates across offense types. We perform this calculation for only those crimes where we find statistically significant estimates. We use the ATEs from Table 3 and multiply these by the

[28] One complication in this approach is that the extant literature uses various forms of aggregating individual crimes into composite categories, which makes comparisons harder.

average number of reported offenses in the respective blocks pre-treatment as displayed in Table 1.[29] Next, we multiply these by the total number of blocks in each treatment zone and then by the cost estimates from Chalfin (2015).[30] The impact of the MMRP amounts to net savings of around \$2.8 million per year. The program used around \$13 million of the municipal budget till 2018, while the above cost savings in the form of reduction in crime prevalence amount to around \$22 million ($2.8 \times 8$ years). Therefore, only the welfare gains precipitated from this decline in crime resulted in a net benefit of \$9 million.

The findings of this study underscore important positive externalities of neighborhood upkeep policies. We conclude that place-based intervention, like the MMRP, should be considered a viable tool to help low-income neighborhoods suffering from disinvestment and housing stock depreciation and that this can have profound implications on urban crimes. The program is especially interesting because it is relatively low cost, respects the pre-existing housing stock of a given area, and focuses on the revitalization of incumbent residents.

[29] We consider all estimates that are statistically significant, whether positive or negative. Due to the lack of estimated costs for trespassing, we leave it out of the cost-benefit analysis. We also leave out the estimates on aggravated assault and sexual assault in blocks farther out of the MMRP community borders.

[30] Specifically, these are around \$5,500 per burglary, \$2,100 per theft, \$41,000 per robbery, and \$89,000 per assault in 2012 dollars. Chalfin (2015) calculates these as the median value based on the extant literature in criminology.

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# Appendix A Additional and Supplementary Results

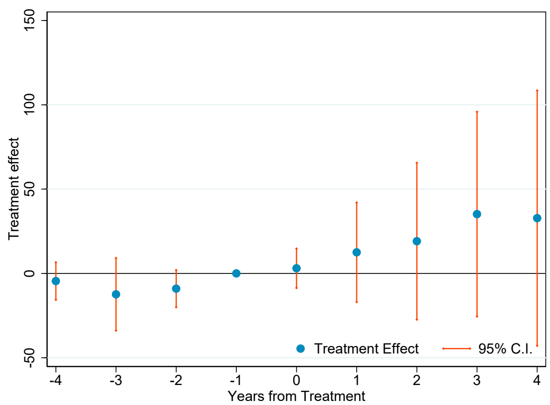
## A.1 MMRP and Loan Applications

A feature of the MMRP policy initiative also revolved around providing small funds to prospective home buyers in lieu of down payment on new house purchases. It is difficult to construct a data set for home purchase/loan activity at the census block level. However, in this section, we are able to furnish evidence from a data set at the census tract level, which provides us with some measure of housing purchases.

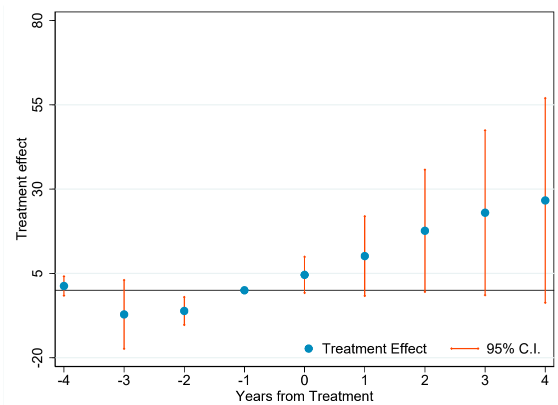
In particular, we employ data on loan applications from 2007 to 2017 available under the Home Mortgage Disclosure Act (HMDA). We consider four outcomes of interest: all loan applications, applications for home purchases, home improvements, and house refinancing. Since the census tracts are much larger units and are likely to include neighborhoods with vastly varying housing profiles over time we implement the matched DID approach for this analysis. This also provides us with a way to aggregate our census block-level data up to the census tract level. First, we compute the propensity score for being in the program at the census block level. Next, we aggregate this propensity score to the census tract level and use as control only those non-treated tracts whose average propensity score is in the support of the treated areas' propensity score. This avoids the inclusion of non-treated areas with residential and housing profiles very distinct from MMRP neighborhoods. We also allow for linear trends by census tract in our estimations for this subanalysis.

The event-study estimates from the above exercise are presented in Figure A.1. We observe a significant and gradual increase over the years in loan applications for house purchases. The ATE is around 15.6, which translates to a 60% (15.6/25.8) increase in the average mortgage application for house purchases in treated areas. The estimates on loans for house improvement and refinancing are close to zero and statistically insignificant. This makes sense since the MMRP program provided small loans for renovations to private households through its own funds, while only providing funds for down payment as part of the program. Hence, those home buyers who were at the margin of the purchase decision and were induced by the latter incentive to go through would have had to apply for a full house loan application, which is manifested in the HDMA data.

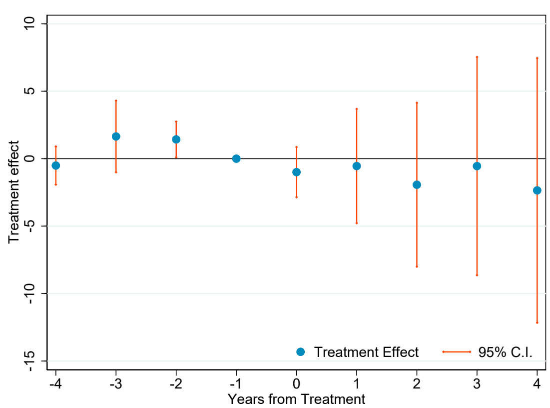
Finally, it is worth mentioning that we do not find any statistically significant pre-trends for any of the loan outcomes, assuring us that in our estimation sample treated and untreated census tracts were evolving similarly pre-treatment. Second, these estimates also employ the methods developed in De Chaisemartin and d'Haultfoeuille (2020) and are robust to any negative-weighting concerns. Based on these results, we are confident that the MMRP contributed to an increase in home purchases in treated areas.



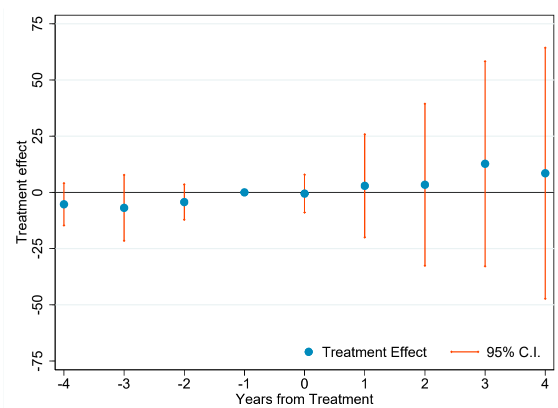

(a) All loan applications




(b) Applications for house purchase




(c) Applications for house improvement

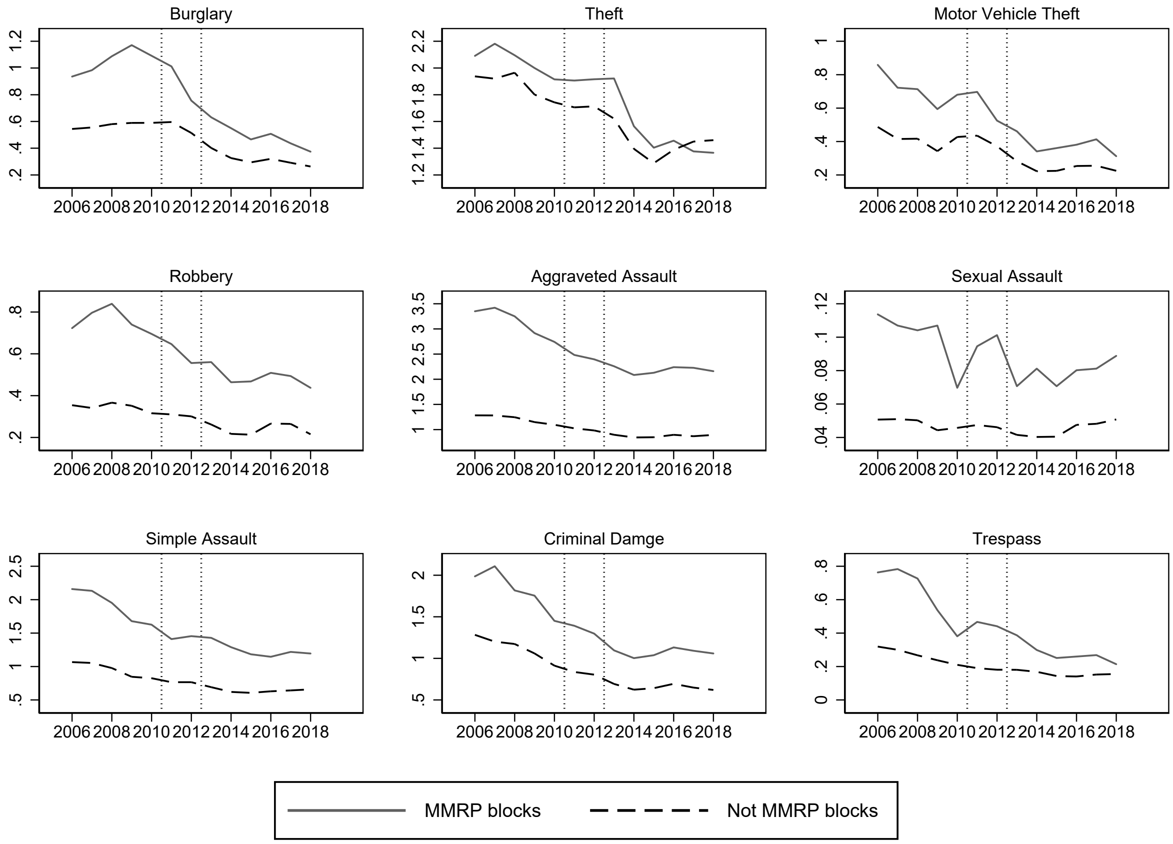



(d) Applications for house refinancing

Note: The plots show study event estimates of dynamic MMRP effects on outcomes in treated tracts using the method in De Chaisemartin and d'Haultfoeuille (2020). The sample size is 3,211. The estimations include tract-fixed effects, year-fixed effects, and tract linear trends.

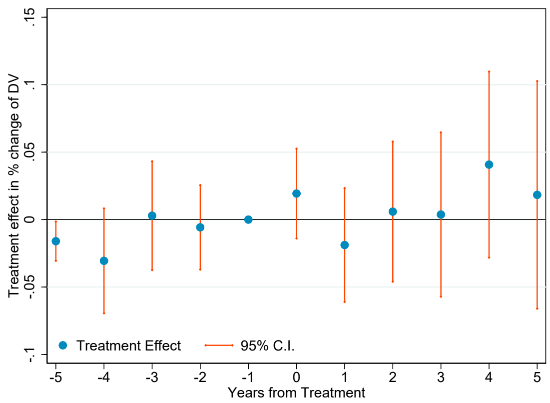
Figure A.1: Event Study Style DID Estimates for Loan Applications.

## A.2 Additional Plots and Tables

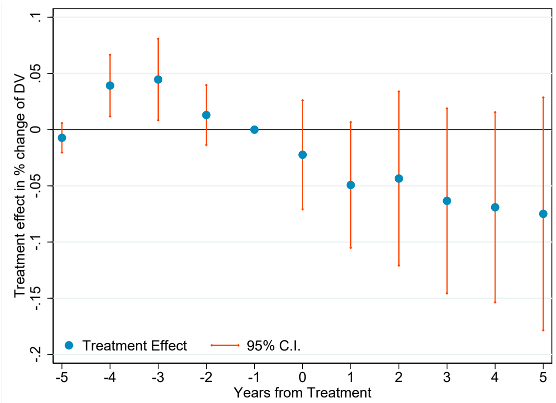



Note: The graphs show the average number of criminal incidents by block over the years.

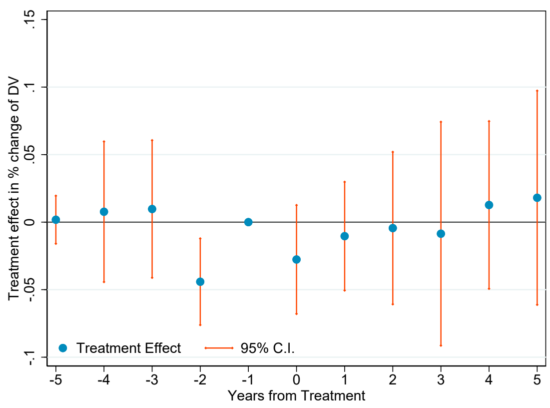
Figure A.2: Crime trends by MMRP status



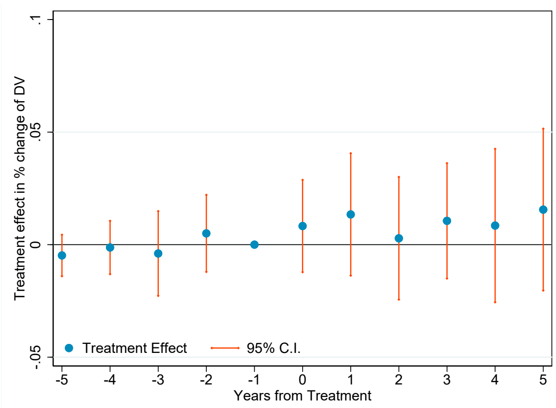

(a) Motor Vehicle Theft



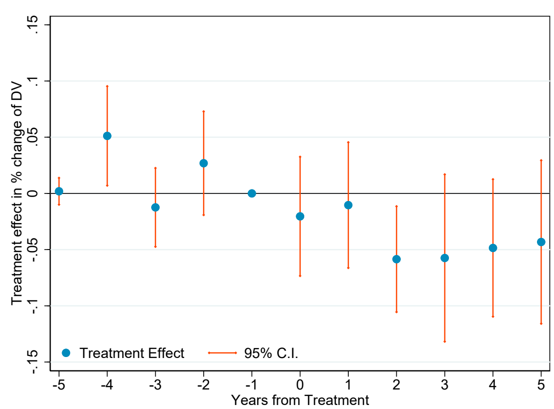

(b) Robbery




(c) Aggravated Assault



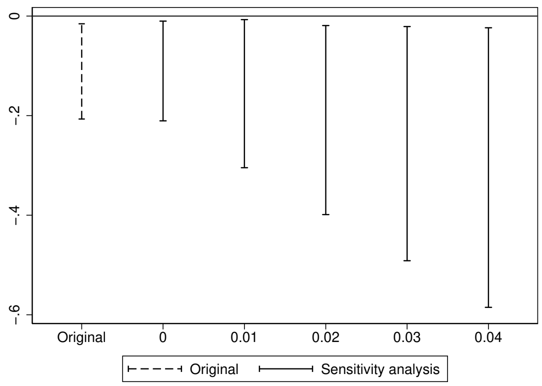

(d) Sexual Assault



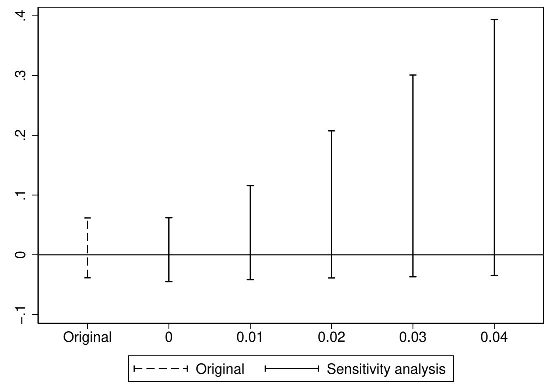

(e) Criminal Damage

Note: The plots show study event estimates of dynamic MMRP effects on outcomes in treated blocks using the method in De Chaisemartin and d'Haultfoeuille (2020). The sample size is 570,830 and estimations include block fixed effects, year fixed effects and blocks linear trends. We also control for dummy variables for nearby blocks beyond the MMRP areas used to study spatial spillover.

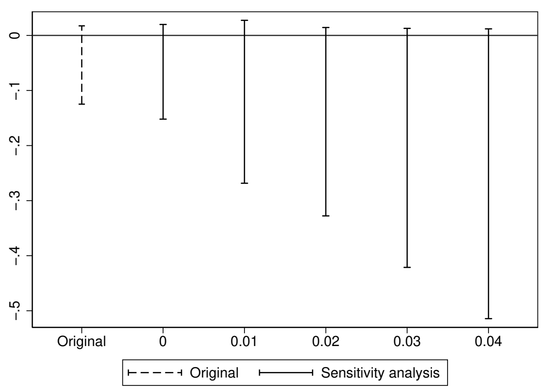
Figure A.3: Event Study Style DID Estimates for Remaining Offenses.



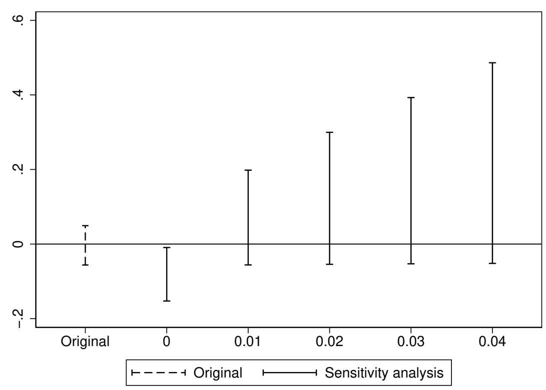

(a) Burglary



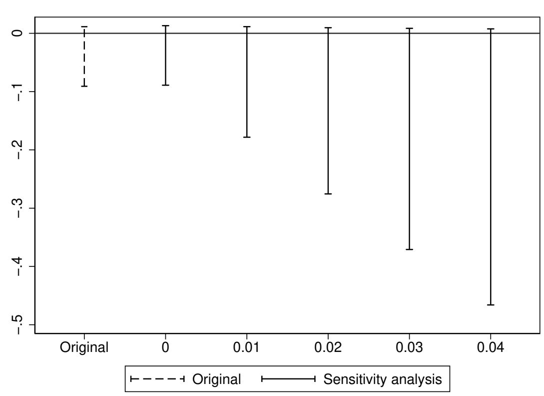

(b) Motor Vehicle Theft




(c) Robbery




(d) Aggravated Assault




(e) Criminal Damage

Note: The plots show robust confidence sets for the average treatment effect based on about how non-linear the difference in trends can be. The confidence sets are shown for $\Delta = \Delta^{SD}(M)$ for different values of $M$. $M = 0$ corresponds with allowing only for linear violations of parallel trends and larger values of $M$ allow for larger deviations from linearity. For each category of crime, we restrict the sign of the bias in the post-period to be the same as the direction of difference in trends in the pre-treatment period. We only present this analysis for categories of crime where treated and control groups present statistically significant differences in at least one year before the program was implemented.

Figure A.4: Sensitivity Analysis based on Rambachan and Roth (2023)

Table A.1: MMRP's average treatment effects on categories of crime in treated and nearby areas. Dependent variables on the level.

| | (1) MMRP | (2) MMRP Neighbors | (3) MMRP community | (4) 1km MMRP community | (5) 1 to 2km MMRP community |
|---|---|---|---|---|---|
| Panel A: Property crimes | | | | | |
| Burglary | -0.421** | -0.105* | -0.237*** | -0.207*** | -0.053 |
| | [-71.4%] | [-9.6%] | [-26.6%] | [-24.9%] | [-7%] |
| | (0.169) | (0.061) | (0.064) | (0.048) | (0.033) |
| Theft | -0.022 | 0.141 | 0.025 | -0.138 | -0.031 |
| | [-1.3%] | [7.4%] | [1.3%] | [-8.8%] | [-2%] |
| | (0.202) | (0.162) | (0.045) | (0.085) | (0.064) |
| MV Theft | 0.049 | -0.051 | -0.048 | 0.020 | -0.013 |
| | [11.4%] | [-7.5%] | [-6.7%] | [3.5%] | [-2.5%] |
| | (0.057) | (0.069) | (0.041) | (0.026) | (0.055) |
| Panel B: Violent Crimes | | | | | |
| Robbery | -0.099 | -0.168 | -0.039 | -0.001 | 0.022* |
| | [-30.9%] | [-24%] | [-5.2%] | [-0.2%] | [5.6%] |
| | (0.068) | (0.135) | (0.033) | (0.024) | (0.013) |
| Agg. Assault | 0.210*** | -0.016 | -0.044 | -0.050 | 0.003 |
| | [19.1%] | [-0.6%] | [-1.8%] | [-3%] | [0.2%] |
| | (0.068) | (0.184) | (0.057) | (0.050) | (0.034) |
| Sex. Assault | 0.029 | -0.003 | -0.005 | 0.005 | -0.013*** |
| | [58%] | [-4.3%] | [-6.3%] | [8.3%] | [-26%] |
| | (0.020) | (0.022) | (0.007) | (0.009) | (0.004) |
| Panel C: Less serious offenses | | | | | |
| Sim. Assault | 0.206** | 0.145 | -0.052 | 0.056 | -0.056*** |
| | [24.8%] | [8.9%] | [-3.5%] | [5.3%] | [-5.7%] |
| | (0.098) | (0.096) | (0.046) | (0.049) | (0.016) |
| Crim. Damage | 0.117 | 0.126 | 0.012 | -0.030 | -0.033 |
| | [12.9%] | [8.7%] | [0.9%] | [-2.8%] | [-3.1%] |
| | (0.114) | (0.131) | (0.023) | (0.038) | (0.075) |
| Trespass | 0.363** | 0.023 | -0.076*** | -0.049 | -0.002 |
| | [172.9%] | [6.1%] | [-19.5%] | [-22.3%] | [-0.9%] |
| | (0.160) | (0.123) | (0.017) | (0.030) | (0.026) |

Note: Each cell presents the estimated effect of the program for a treated group (columns) on a given dependent variable (rows). Estimations for each pair dependent variable and treatment area are done separately, however treatment dummies for the other treated areas are included as control. Treatment groups are mutually exclusive. The sample size is 570,830 and the estimations include block fixed effects, year fixed effects and blocks linear trends as presented in Equation 2. The table shows ATEs estimated using the method in De Chaisemartin and d'Haultfoeuille (2020). Standard errors clustered at the census tract level are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table A.2: Propensity score matching, mean and pre-trend comparison of MMRP blocks and selected control group.

| | Probit | | PS Neighbors | MMRP | p-value | | p-value diff. |
|---|---|---|---|---|---|---|---|
| Variable | Coef. | SE | Mean | Mean | diff of means | Variable | pre-trends |
| Comm. mortgages in 2008 | 0.02 | 0.00 | 10.75 | 11.06 | 0.17 | Burglary | 0.05 |
| Comm. foreclosure in 2008 | 0.33 | 0.00 | 7.11 | 7.17 | 0.90 | Theft | 0.39 |
| % buildings occupied | 1.23 | 0.05 | 0.82 | 0.80 | 0.48 | MV Theft | 0.13 |
| % tenure is owner | -0.29 | 0.04 | 0.41 | 0.36 | 0.48 | Robbery | 0.60 |
| Median year residents moved in | 0.03 | 0.00 | 2001 | 2001 | 0.79 | Agg. Assault | 0.28 |
| % white residents | -3.95 | 0.08 | 0.04 | 0.06 | 0.52 | Sex. Assault | 0.15 |
| % black residents | -1.20 | 0.04 | 0.86 | 0.81 | 0.60 | Sim. Assault | 0.89 |
| % residents with less than high school | -0.08 | 0.06 | 0.27 | 0.28 | 0.60 | Crim. Damage | 0.72 |
| % residents with high school | -0.24 | 0.06 | 0.55 | 0.54 | 0.59 | Trespass | 0.17 |
| % residents with college | (omitted) | | 0.18 | 0.18 | 0.85 | | |
| % household income less than 25K | -2.18 | 0.14 | 0.45 | 0.47 | 0.67 | | |
| % household income 25K to 50K | -2.51 | 0.14 | 0.27 | 0.25 | 0.42 | | |
| % household income 50K to 100K | -2.50 | 0.14 | 0.22 | 0.22 | 0.78 | | |
| % income 100K to 150K | -2.17 | 0.17 | 0.04 | 0.05 | 0.52 | | |
| % household income 150K or more | (omitted) | | 0.02 | 0.02 | 0.96 | | |
| Observations | 35432 | | 1165 | 1047 | | | |

Note: With the exception of community mortgages and foreclosures which are measured in 2008, the other explanatory variables are from the 2010 ACS 5-year estimates.