Public Housing Demolition and Crime in Chicago

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SCIENCES PO PARIS

May 2024

Abstract

Wide-spread crime in public housing projects in the US is a major issue hindering daily lives of local populations. I investigate the impact of Chicago's large-scale Public Housing demolition program on crime rates, applying a variety of Synthetic Control approaches using tract-level quarterly data. The analysis disentangles the effects of population displacement and crime network disruption, finding that the program had no significant effect on overall crime rates. The results are insensitive to small changes in design with no spatial spillovers detected. The observed decrease in number of crimes within demolition zones is attributable primarily to resident relocation, suggesting the policy did not reduce citywide crime rate.

1 Introduction

The demolition of public housing buildings (projects) is a controversial policy aimed at reducing concentrated poverty and crime. The Chicago Housing Authority's "Plan for Transformation" provides a compelling case study of this approach. While criminal networks present in the project areas undeniably inflicted harm on residents [Popkin et al., 2000, Venkatesh, 2009], demolitions intended to dismantle these networks also generated significant social costs, including displacement and reduced affordable housing availability [Almagro et al., 2023, Chicago Housing Authority, 2011]. Mixed-income replacement units were limited, leaving many displaced residents facing higher costs or limited options [Chicago Housing Authority, 2011]. Since inequality and poverty concentration are empirically linked to crime [Itskovich and Factor, 2023, Kelly, 2000, Sugiharti et al., 2023, Anser et al., 2020], these two mechanisms – dismantling criminal networks while exacerbating inequality – could exert opposing forces on crime rates.

This paper aims to answer whether areas in which the demolitions occurred experience a shift in crime rates, and discover the mechanism behind the impact of demolitions on crime locally and in general equilibrium. I provide new evidence on the crime rates in the tracts of the demolitions after the events occurred. More importantly, I examine them at a tract level assuming a portion of ex-residents leaving the tract.

I recognize two key forces that likely affect crime rates following public housing demolitions: displacement and disruption of social networks. The design of these projects, coupled with poor maintenance, income inequality, and racial segregation, fostered the rise of gangs centered around public housing. Demolitions aim to break up these networks by removing the physical spaces where the gangs operate. However, the demolitions also exacerbated urban inequality [Almagro et al., 2023], worsening the economic circumstances of former residents.

My approach relies on a reduced-form study using synthetic control and difference-in-difference methods. The analysis investigates the impact of public housing demolition on crime rates, employing a robust control group that exhibits parallel crime trends prior to demolition. While [Bruhn, 2017] highlights the potential contamination effect caused by residents relocating to control areas, this could actually bolster my analysis by suggesting broader city-wide implications in the general equilibrium. Primarily, our Synthetic Control (Synthetic Difference-in-Differences) algorithm meticulously weights the control group to ensure trend alignment – likely with areas resembling the treated zones and thus susceptible to contamination. This approach uncovers the net effect of displacement, reflecting changes in both control and treated areas simultaneously. Though the inability to isolate these effects might seem like a limitation, it opens a window into potential city-wide spillover dynamics. To validate this effect, I cross-reference several methodologies. I initiate with a traditional Two Way Fixed Effects (TWFE) model, then transition to the Generalized Synthetic Control Method (GSC), incorporating latent factors, unit and time fixed effects. However, since these assign treatment based on the initial demolition, the analysis remains limited. To rectify this, I individualize treatment for each tract-demolition, significantly expanding treated observations from 42 to 120. Under the assumption of linear effects from demolished units, I calculate the weighted average treated effect based on the number of units demolished. Lastly, I employ a more rigorous synthetic difference-in-differences (SDID) method for further confirmation. I explore spatial spillovers by excluding treated tracts and assigning treatment to adjacent areas.

This paper's analysis reveals a complex relationship between demolitions and crime rates. When considering multiple demolitions (treatments) within an area, estimated crime rate reductions range widely from 20.39 to 6.74 per 1,000 citizens. However, factoring in population displacement (under a conservative assumption of one person leaving the tract per demolished unit) narrows the range considerably: 17.27 for the Generalized Synthetic Control (SC) Method and 2.31 for Synthetic Difference-in-Differences. The notably lower SDID estimate likely stems from reduced control group contamination. Unlike other methods, SDID doesn't demand an exact pre-treatment match between treated and control groups, only a parallel trend. This lessens the likelihood of SDID control groups including areas where displaced residents relocate. If, as we hypothesize, negative social networks are also displaced, they're unlikely to continue their activities in areas already experiencing downward crime trends. Further analysis suggests no spatial spillover effects on neighboring tracts. Moreover, extending the pre-treatment period and excluding smaller demolitions yield similar estimates, demonstrating the robustness of our findings and their insensitivity to minor design adjustments.

The demolitions program and its effects has been largely explored in the literature. [Aliprantis and Hartley, 2015] study the demolitions effect in Chicago using time-series analysis, [Currie and Yelowitz, 2000] finds negative correlation between child outcomes and living in a public housing project, [Chyn, 2018] finds positive effects of projects residents relocation on children, [Jacob, 2004] neutral effects on student achievement. [Blanco, 2023] finds a strong house prices increase after the demolitions in Chicago. Almagro, Chyn and Stuart provide a structural model of the demolitions in Chicago and conclude that the program resulted in wider welfare disparities among income and racial groups [Almagro et al., 2023]. Sandler uses difference-in-differences approach to search for local effects (1/4 mile) of the demolition and shows that number of crimes decrease by 8.8 after a demolition comparing to the control group constructed based on the distance from the demolition [Sandler, 2017].

This approach, however, is likely biased by contamination [Bruhn, 2017] and no adjustment for population which left the area. [Hendey et al., 2016] find only a slight net rise of violent crime rates in the areas where voucher holders moved to, at the same time, they show a 0.77% increase in overall per capita crime for each household relocated per 1,000 households in the destination area. This is the first attempt to use the synthetic control methods to estimate the effects of the demolitions by building a comparable control group to provide evidence of public housing demolition on crime rates.

2 Context of the study - Public Housing in Chicago

The large-scale demolition of public housing projects in Chicago from 1995 to 2010 remains a topic of intense debate in academic literature. This program, motivated by a desire to revitalize the poorest neighborhoods, targeted high-rise developments where crime, poverty, and gang activity was wide-spread. However, consequences of this strategy are under extensive research.

The demolition of public housing in Chicago was part of a broader effort to address issues related to concentrated poverty, crime, and the physical deterioration of public housing developments. The Chicago Housing Authority (CHA) implemented a plan called the "Plan for Transformation," which aimed to revitalize the city's public housing stock.

While the intention was to improve living conditions and create more vibrant neighborhoods, the Plan for Transformation has been criticized for its impact on the displaced residents, the loss of affordable housing units, and the social disruption caused by the demolition of longstanding communities.

The Plan aimed to demolish over 21,000 public housing units in Chicago under heavy distress: by 1984, the percentage of working families plunged to 10% while 80% of families were single-parent in the largest Chicago housing project [Hunt, 2001]. The main reasons behind this state include poor site selection, design, tenant selection, projects mismanagement [Hunt, 2001, Popkin et al., 2000]. The Chicago Housing Authority declared that less than 25,000 out of 38,000 units of Public Housing were habitable. Facing that situation, the City of Chicago decided to demolish close to 22,000 units displacing 17,000 households between 1999 and 2010 which was equal to approximately 1.5% of the city's population [Bruhn, 2017]. In the place of the high-rising projects, low-rise mixed-income buildings were supposed to be built. However, this type of housing targets different groups and is of a much smaller scale: only 1,900 residents were relocated to mixed-income housing by 2010 [Chicago Housing Authority, 2011]. Moreover, it takes time to clean the demolished sites and build new housing instead, which provides a natural experimental environment to analyze the areas after the residents' displacement. Residents of the demolished public housing developments were typically offered the opportunity to relocate to other public housing units or were provided with housing vouchers to help them secure housing in the private market.

Even after using the housing voucher which payed up to 60% of rent [Chicago Housing Authority, 2011], the public housing fee was significantly lower, which led to higher housing cost for the displaced residents [Buron and Popkin, 2010]. This might have caused increased financial distress for the households. Simultaneously, [Almagro et al., 2023] show that the demolitions decreased welfare for low-income minority households and increased for White households. This contributes to deepening of income and racial inequality. Poverty and inequalities are one of the main drivers of crime [Itskovich and Factor, 2023, Kelly, 2000, Sugiharti et al., 2023, Anser et al., 2020], thus, leading this reform to potentially increase crime rates on the city-level, which stands in contrast with the current research [Sandler, 2017, Bruhn, 2017, Aliprantis and Hartley, 2015].

One of the reasons behind the increased crime rates in the public housing areas was the design of the neighbourhoods itself. The high-rise design, with its long, poorly lit corridors and isolated units, often separated from other neighborhoods, was seen as fostering a sense of anonymity and creating opportunities for criminal activity to flourish [Currie and Yelowitz, 2000]. Furthermore, the concentration of poverty within these developments was thought to breed despair and limit opportunities, creating a breeding ground for gang recruitment and violence [Chyn, 2018, Bruhn, 2017]. The fact that the neighborhoods were isolated from prosperous parts of the cities compounded the problem of crime and contributed to the creation of organized crime groups.

Moreover, the areas were facing high unemployment rates and concentrated poverty with less than 10 percent of residents employed and the average income of about $6,000 per year in 1995, comparing to $32,000 average [Popkin et al., 2000, BLS, 1995].

The exterior of the buildings was built up using cheap materials which have not held up well over time, covered with metal grates and with exterior amenities difficult to maintain [Popkin et al., 2000]. Popkin and Venkatesh describe the insides of the projects as poorly managed interiors, often without the basic utilities provided, walls molded, apartments without light or bathrooms. The buildings were infested with vermin, neglected and vandalized. Additionally, the whole neighborhoods were often racially segregated by official city policies [Popkin et al., 2000].A combination of factors led to increased crime, which was exacerbated by police inaction and the gang's ability to attract young people with financial incentives, a sense of structure, and appealing values [Venkatesh, 2009].

By the 1990s, neglect and mismanagement resulted in severe infrastructure problems within Chicago's public housing system. Seeking a solution, city officials opted for demolition. Federal funding arrived through the HOPE VI program (from Department of Housing and Urban Development), which empowered cities to revitalize or demolish public housing. Chicago heavily utilized this program, receiving millions in demolition grants. Residents were evicted and offered Section 8 vouchers to subsidize private-market rentals.

The demolition process was gradual, spanning from 1995 to 2010. Over 21,000 housing units were ultimately destroyed, the majority after the year 2000. This process impacted neighborhoods unevenly. Some neighborhoods saw only a handful of units demolished, while others lost hundreds, sometimes exceeding 50% of their pre-demolition housing stock [Almagro et al., 2023].

The displaced residents were offered a housing voucher, relocation to other public housing facility, scattered-site public housing or mixed-income development. Table 1 shows that most of the residents left the public housing system. Moreover, these residents most likely changed the neighborhood, most often to poor and African American ones [Buron and Popkin, 2010]. Of the household outside of the CHA system, only 581 out of 6,609 left voluntarily. This shows the vulnerability of residents and little improvement of their situation after the demolitions. It may drive us to the broader conclusion of no change in crime patterns developed in the social sites after moving out, therefore, not much of expected crime reduction resulting from the wealth improvement.

Housing Type	1999 (#)	1999 (%)	2008 (#)	2008 (%)
Households	16,552	-	9,980	-
Mixed-income developments	-	-	1,278	13%
Housing Choice Vouchers	-	-	3,978	40%
Scattered-site public housing	2,471	15%	1,571	16%
Traditional public housing developments	14,081	85%	3,153	31%

Table 1: Number of residents living CHA-subsidized housing in 1999 and 2008

The original vision for demolished public housing sites involved "mixed-income" redevelopment – a blend of public housing and market-rate units. Yet, this redevelopment faced significant delays. Even a decade after the initial demolitions, a large proportion of lots remained vacant [Almagro et al., 2023]. In 2008, only 13% of the displaced residents lived in the mixed-income developments (without indication whether these were newly built). Of the land that was redeveloped, the focus remained primarily on residential housing, with smaller portions assigned for businesses or institutions. The slow pace of the redevelopment provides an environment for examining the effects of the residents' displacement on crime.

3 Data

3.1 Description

I'm using the 2000 census population and socioeconomic characteristics on tract-level accessed from the US Census database [U.S. Census Bureau, 2000] to compare the initial status of the tracts in terms of median income, unemployment rate and family poverty rate at the beginning of the analysis.

Dataset on crimes comes from [Sandler, 2017] obtained from the Chicago Police Department through a Freedom of Information Act request. It includes all categorized crimes within the City of Chicago from January 1999 to February 2011 with detailed information on date, type of crime and its coordinates. I then aggregate the data to census tract level, month and year. The coordinates are subject to a measurement error if the police has to track the crime occurrence, however, it is geo-coded by the police in the most accurate way, and is the best source one can obtain. We're left with 2,533,049 crimes that appear in the analyzed tracts from January 1999 to February 2011, classified in 9 categories: Burglary, Theft, Car theft, Robbery, Murder, Assault, Rape, Arson, Drug crime. I reclassify the crimes to economic (1-4), violent (5-8) and drug crimes (9). This corresponds to 1,703,811 economic crimes, 249,820 violent and 579,418 drug crimes.

Finally, the dataset on public housing demolition is provided by Chicago Housing Authority (CHA) and contains the name of property, demolition start, end date, number of units demolished and coordinates (provided by [Sandler, 2017]) of the demolition from 1995 to 2010. The demolition date provided is the construction start. I subtract 60 days of that date, when the building was shut down [Sandler, 2017].

Census tract boundaries were accessed using City of Chicago data web page [data.cityofchicago.org, 2000].

3.2 Descriptive statistics

There is a total of 21,151 (17,550) units demolished in 46 (42) tracts between January 1995 (1999) and December 2010. A unit typically consists of 3 to 4 bedrooms, each intended for 2 people. Therefore, each demolition moved hundreds of people with a total of 1.5% population of the city displaced [Sandler, 2017, Bruhn, 2017]. The demolitions were concentrated with 60% of demolished units located in only 10 tracts. Full table of units demolished by tracts can be found in the Appendix (Table A1). There is substantial variation in demolition size, with projects ranging from an average of 72 units to a maximum of 1262 units.

Number of crimes and units demolished in the studied period follow opposite trends, as shown in Figure 1. This does not allow to draw any conclusions about the impact of

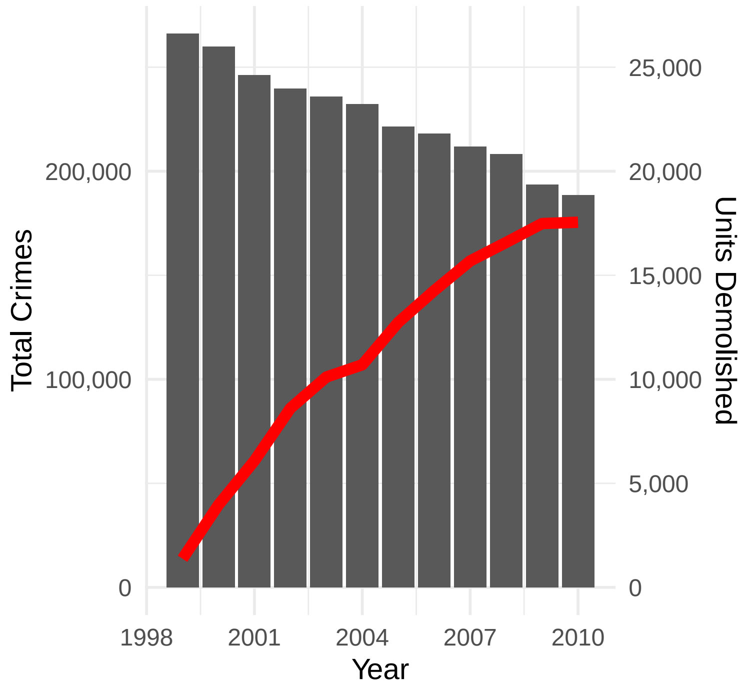

Figure 1: Units demolished and total crime in the City of Chicago.

demolished units as a declining trend in number of crimes is observable in the entire United States. Then, there's a strong heterogeneity in crime rates among tracts. Figure 1 reveals a concentration of crime in the city's eastern region, with a notable secondary cluster in areas where demolitions occurred. To account for population differences across tracts, further analysis will examine crime rates normalized by population figures from the 2000 census.

Further spatial heterogeneity is explored through economic indicators of median income, percent of families living in poverty and unemployment rate in Figure 3. A major observation is that often the tracts of public housing demolition adjoin to completely different ones with high income (especially in the north side), low percentages of families in poverty and unemployment rate. As shown in Table 2, treated tracts had less than half of the untreated income, over 2 times higher unemployment rate and almost 3 times higher percentage of families living in poverty. These facts make any distance-based treatment difficult to implement so that the control and treatment group share characteristics are comparable.

	Median Income	Families in Poverty (%)	Unemployment (%)
Treated	18,072	47.79	13.65
Untreated	38,395	17.42	6.75
Treated / Control (%)	47.07	274.49	202.22

Table 2: Economic characteristics of treated and untreated tracts in 2000.

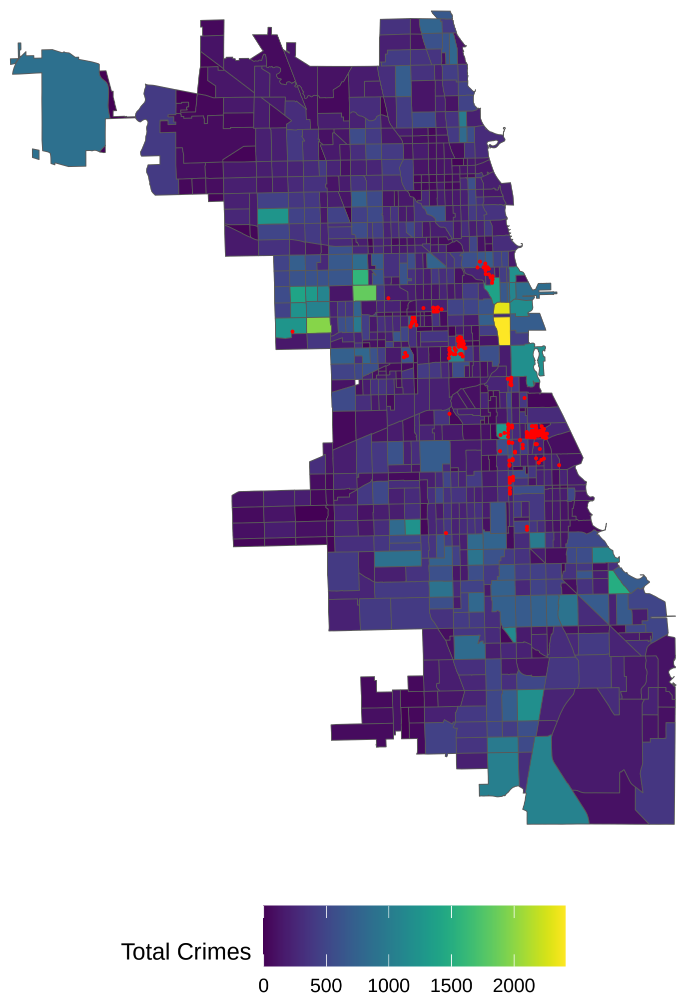

Figure 2: Total crimes by census tract in 2000. Red dots are the locations of public housing demolitions that occurred between 1999 and 2010.

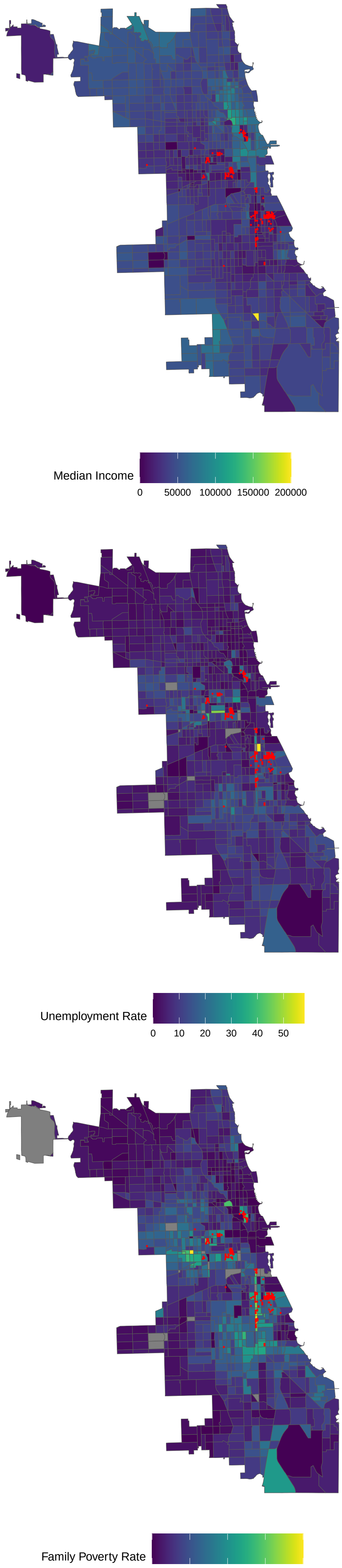

Figure 3: Economic indicators by census tract in 2000.

4 Identification

4.1 Invalidity of previous designs

Strong spatial heterogeneity of areas indicates that constructing treatment and control groups based on the distance from demolitions might not correctly identify the demolition shock. Moreover, the treatment strategy incorporated by [Sandler, 2017] indicates very local effects of 0.25 mile circle area around the demolition - without considering the residents' displacement or population adjustment. I address this by studying adjusted crime rates, where I make a conservative assumption that each unit demolished makes only 1 residents leave the tract. As we do not observe the actual displacement of the residents, this is an artificial assumption to demonstrate the effect of potential accounting for the dislocation. We do not add the resident to the other tract. This would potentially lead to an increase in crime rates in the control groups, as [Hendey et al., 2016] report a slight increase in crime rate after a public housing resident moves to a new tract. Our results are highly sensitive to this adjustment, largely decreasing the effect of demolition on crime rate.

4.2 Effect identification

The analysis is focusing on effects of public housing demolition on crime with a credible control group that follows (parallel) trend in crimes before the demolition occurred. However, [Bruhn, 2017] underlines the contamination effect resulting from residents moving out of the treatment into the control areas. This may in fact strengthen my analysis and indicate some more general city-wide effects. First, our synthetic control (synthetic difference-in-differences) algorithm will assign weights to the control group such that the trend is following (parallel) - most probably then to the areas that are similar to treated and highly exposed to the contamination. The residents who had to leave the public housing might have moved to the tracts which were a part of the control group. Thus, my estimator captures two main effects - the deviation of crime rates in the tracts of the demolitions, and in the (potentially similar) control group. I am unable to separate these forces, as I do not observe migration.

I identify this effect comparing different methods. First, I show the results of a classic Difference-in-Differences (DiD). Then, I move to Generalized Synthetic Control Method (GSC) [Xu, 2017], which includes unit and time fixed effects. Yet, all these assign treatment when the first demolition happens, making the analysis incomplete. I separate the treatment for each tract-demolition and increase number of treated observations from 42 to 120. Assuming linear effects of demolished units, I compute the average treated effect on treated weighted by number of units demolished. Finally, I use a more robust, synthetic difference-in-differences method to confirm my findings. To examine the spatial spillovers, I exclude the treated tracts and assign treatment to the neighboring areas.

4.3 Synthetic Control

Synthetic Control methods are not only popular but very easy to explain to policy-makers or business leaders. In our setting, the treated group are tracts where the demolition happened. For each tract, Synthetic Control computes weights, so that in the pre-treatment period the crimes follow the same path. The weights are stable in time in order to compute a counterfactual for the treated tract [Abadie et al., 2010]. Assuming T is time of the treatment, we predict:

$$\hat{Y}_{NT}(0) = \sum_{i=1}^{N-1} \hat{\omega}_i Y_{iT} \tag{1}$$

ω chosen so that for all $t = 1, ..., T-1 : Y_{Nt} \approx \sum_{i=1}^{N-1} \omega_i Y_{it}$ and $\omega_i \geq 0, \sum_{i=1}^{N-1} \omega_i = 1$. To choose the weights ω_i, one of the most popular methods is to regress the outcome of the treated unit on the outcome of the untreated units for each period before the treatment separately using Ordinary Least Squares (OLS). This way we minimize the difference between the treated and control group before the treatment at each point of time before the treatment occurs.

$$\hat{\omega} = \min_{\omega \geq 0, \sum_i \omega_i = 1} \sum_{t=1}^{T-1} (Y_{Nt} - \sum_{i=1}^{N-1} \omega_i Y_{it})^2$$

The weight-matching process might be cumbersome to match all the pre-treatment periods, as matching is done among units which change in time with static weights. Another difficulty that arises is the number of periods to be used for the matching. However, we might want to put more importance on the period that is closer to the treatment instead of fitting all the pre-treatment periods with a larger error. To adjust that, we can run a horizontal regression and find appropriate time weights λ_t.

$$\hat{Y}_{NT}(0) = \lambda_0 + \sum_{t=1}^{T-1} \hat{\lambda} Y_{Nt}$$

where

$$\hat{\lambda} = \min_{\lambda_t} \sum_{i=1}^{N-1} (Y_{iT} - \sum_{t=1}^{T-1} \lambda_t Y_{it})^2.$$

Another method to study similar designs is Two Way Fixed Effects regression where we assume an outcome of untreated units to be in form of

$$Y_{it}(0) = \mu + \alpha_i + \beta_t + \epsilon_{it}.$$

By minimizing

$$\min_{\mu, \alpha, \beta} \sum_{(i,t)|(i,t) \neq (N,T)} (Y_{it} - \mu - \alpha_i - \beta_t)^2$$

To predict the treated outcome

$$\hat{Y}_{NT} = \hat{\mu} + \hat{\alpha}_N + \hat{\beta}_T.$$

Where μ is a constant, α_i is a unit fixed effect, β_t is a time fixed effect and ϵ_{it} is an idiosyncratic error. Unit fixed effect is constant across time, specific for each unit, while time fixed effect is constant across units. This allows to capture individual and time trends separately, and predict the outcome of a unit based on these estimations. While Synthetic Control assumes a static relation between units over time, horizontal regression a relation between outcomes in treated period and pre-treatment periods that is the same for all units, TWFE regression an additive outcome model that captures differences between units and time periods, Synthetic Difference-in-Differences combines the all three.

4.4 Generalized Synthetic Control Method

To cross-check the SDiD results, I'm using a Generalized Synthetic Control Method presented in [Xu, 2017], which incorporates linear fixed effects model in the synthetic control method. It constructs counterfactuals for each treated unit by assigning weights to the control group based on a linear interactive fixed effects model with a unit-specific constant interacted with time-varying coefficients. It allows for the treated to be correlated with the latent factors, treatment time heterogeneity and implements a cross-validation procedure to match number of latent factors by minimizing Mean Squared Prediction Error (MSPE). The algorithm consists of 3 steps and proceeds as follows:
In step 1, an Interactive Fixed Effects (IFE) model is estimated only for the control group to obtain factors and factor loadings:

$$(\hat{L}, \hat{\lambda}_c) = \underset{\hat{L},\hat{\lambda}_c}{\text{argmin}} \sum_{i \in \mathcal{C}} (Y_i - \hat{L}\hat{\lambda}_i)^2 \tag{2}$$

Step 2 estimates factor loadings for each treated unit by minimizing the MSPE in pre-treatment periods:

$$\hat{\lambda}_i = \underset{\hat{\lambda}_i}{\text{argmin}} (Y_i^0 - \hat{L}^0\hat{\lambda}_i)^2 \tag{3}$$

where $\hat{\beta}$ and $\hat{L}^0$ are estimated in Step 1. The third step computes counterfactuals:

$$\hat{Y}_{it}(0) = \hat{\lambda}_i{}'\hat{l}_t \tag{4}$$

Data	Y, N_{tr}, N_{co}, B
Result	*Variance estimator bootstrap,* $\hat{\sigma^2}_{bs}$
1.	for $b = 1$ to B do
2.	Sample 1 out of the N_{co} control units without replacement as if it was treated when $t > T_0$;
3.	Resample N_{co} of the control group with replacement;
4.	Compute GSC estimator on the new sample, obtain residuals: $\hat{\epsilon}^p_{(m)} = Y_i - \hat{Y}_i(0)$;
5.	end, collect $\hat{\boldsymbol{\epsilon}}^{\boldsymbol{p}}$
6.	Apply the GSC method to the original data, obtaining: 1. $A\hat{T}T_t$ for all $t > T_0$, 2. estimated coefficients: $\boldsymbol{\beta}$, $\hat{F}$, $\hat{A}_{co}$, and $\hat{A}_{j,t_0}$, and 3. the fitted values and residuals of the control units: $\hat{Y}_{co} = \{1(0), 2(0), ..., \hat{Y}_{Nco}(0)\}$ and $\hat{e} = \{1, 2, ..., Nco\}$.
7.	(a) In round $k \in \{1, ..., B\}$, construct a bootstrapped sample $S(k)$ by: (i) $Y^{(k)}(0) = \overline{Y} + \varepsilon_i^{(k)}$, $i \in I_c$ (ii) $Y^{(k)}(t) = \hat{Y}_i(t) + \varepsilon_{it}^{(k)}$, $i \in I_t, t > T_0$ where each vector of $\varepsilon^{(k)}$ and $\varepsilon_t^{(k)}$ are randomly selected from sets e and e_t, respectively, and $Y^{(k)}(0) = X_i\beta + \hat{A}_{i,t_0}$. Note that the simulated treated counterfactuals do not contain the treatment effect.
8.	(b) Apply the GSC method to $S(k)$ and obtain a new ATT estimate; add $ATT_{t.t>T_0}$ to it, obtaining the bootstrapped estimate $ATT^{(k)}_{t.t>T_0}$.
9.	End of the bootstrap loop.
10.	Compute the variance of $ATT_{t,t>T_0}$ using $Var(\widehat{ATT}_{t,X,A,F}) = \frac{1}{B}\sum_{k=1}^{B}\left(\widehat{ATT}^{(k)}_{t,t>T_0} - \overline{\widehat{ATT}}_{t,t>T_0}\right)^2$

Table 3: Bootstrap Standard Error Estimation

Average Treatment Effect on Treated (ATT) estimator is thus:

$$A\hat{T}T_t = \frac{1}{N_1}\sum_{i \in T}(Y_{it}(1) - \hat{Y}_{it}(0))$$

for $t > T_0$. Under the assumptions of a true functional form, strict exogeneity of the error, weak serial correlation of the error term and regularity conditions this estimator is consistent under large N_0 or T_0.

Standard errors are obtained using a bootstrap method, presented in Table 3. While treated counterfactuals remain unobservable, we possess observed $Y_{it}(0)$ for all control units. Under the stipulations that both treated and control units adhere to an identical factor model and exhibit cross-sectional independence and homoscedasticity of error terms, we can leverage cross-validation techniques to approximate ϵ_i^p utilizing control group observations [Xu, 2017].

To elaborate, we iteratively omit a single control unit (designated as a "simulated" treated unit) and train the IFE model on the remaining control units. The discrepancy between the prediction of the left-out unit and its real value signifies a prediction error for the IFE model.

4.5 Synthetic Difference-in-Differences

Assumption 1

Data Generating Process follows

$$Y = L + \tau W + \epsilon \tag{5}$$

, where L is a low rank matrix consisting of latent factors (can include two-way fixed effects). Treatment assignment is dependent on the systematic determinants of outcomes, not idiosyncratic components:

$$\epsilon \perp\!\!\!\perp (W, L), W \not\perp\!\!\!\perp L$$

This fits our design, where more probably, tracts with higher crime rates were assigned the treatment. Additionally, the estimator does not require estimating the true factor matrix L, it only allows the true DGP to take this form [Arkhangelsky et al., 2021].

The main difference between Synthetic Control and Synthetic DiD is that it allows the control group to be shifted, so that the trends are parallel in the pre-treatment period. The estimation method for weights is therefore a penalized least squares:

$$(\hat{\omega_0}, \hat{\omega}) = \underset{\substack{\omega_0 \in \mathcal{R} \\ \omega_1 \ldots \omega_{N_0} \geq 0 \\ \sum_{i \leq N_0} \omega_i = 1}}{\operatorname{argmin}} \frac{1}{T_0} \sum_{t \leq T_0} (\bar{Y}_{N_0+1:N,t} - \omega_0 - \sum_{i \leq N_0} \omega_i Y_{it})^2 + \zeta^2 ||\omega||^2 \tag{6}$$

With time weights:

$$(\hat{\lambda_0}, \hat{\lambda}) = \underset{\substack{\lambda_0 \in \mathcal{R} \\ \lambda_1 \ldots \lambda_{T_0} \geq 0 \\ \sum_{t \leq T_0} \lambda_t = 1}}{\operatorname{argmin}} \frac{1}{N_0} \sum_{i \leq N_0} (\bar{Y}_{i,T_0+1:T} - \lambda_0 - \sum_{t \leq T_0} \lambda_t Y_{it})^2 \tag{7}$$

Ridge regression penalty with a constant term allows a non-convex combination of the control group for the treated. The tuning parameter ζ is chosen using this procedure

$$\begin{aligned} \zeta &= \left(\frac{N_1}{T_1}\right)^{1/4} \hat{\sigma} \\ \text{where} \quad \hat{\sigma}^2 &= \frac{1}{N_0 T_0} \sum_{i=1}^{N_0} \sum_{t=1}^{T_0} (\Delta_{it} - \hat{\Delta})^2 \\ \Delta_{it} &= Y_{i(t+1)} - Y_{it} \\ \hat{\Delta} &= \frac{1}{N_0 T_0} \sum_{i=1}^{N_0} \sum_{t=1}^{T_0} \Delta_{it} \end{aligned}$$

No regularization for time weights is that time units are not exchangeable so that recent time periods are weighted more than the ones further from treatment are given less of weight. Synthetic DiD is weighted regression with unit and time fixed effects, combining TWFE and

SC:

$$\hat{\tau}^{SDID} = \underset{\tau,\gamma,\alpha}{\text{argmin}} \sum_{i,t} (Y_{it} - \gamma_t - \alpha_i - \tau W_{it})^2 \times \omega_i \times \lambda_t \tag{8}$$

What is more, SDID estimator has beneficial robustness properties: under general factor model, weights are proprely assigned and SDID is consistent; under DiD model, with wrongly assigned weights, SDID is still consistent. Estimated weights converge to oracle weights, τ^* predicts counterfactual outcomes accurately and satisfies the Central Limit Theorem around its expectation under assumptions: treated units / time periods are large but small relative to the controls; rank of L is small relative to $\sqrt{min(N_0, T_0)}$; ϵ is iid and gaussian without strong long-range dependence. We are likely to satisfy all of these assumptions with 120 treated units comparing to 827 untreated tracts.

Standard errors for SDID are estimated using placebo estimator. The process of obtaining the estimator is presented in Table 4. In the context of placebo studies, a crucial prerequisite for valid inference is the assumption of homoskedasticity across units. Heteroskedasticity, meaning differing variances between treated and control units, obscures the ability to isolate the treatment effect. The core issue is that nonparametric variance estimation becomes intractable with a single treated unit. Thus, to ensure sound statistical inference, homoskedasticity is effectively indispensable. In this context, homoskedasticity is not likely to hold, therefore, the variance of SDID estimator should be read carefully. I put more focus on more precise, GSC variance estimates.

Data	Y_{co}, N_{tr}, B
Result	*Variance estimator placebo,* $\hat{\sigma^2}_{pl}$
1.	for $b = 1$ to B do
2.	Sample N_{tr} out of the N_{co} control units without replacement to 'receive the placebo';
3.	Construct a placebo treatment matrix $W_{co}^{(b)}$ for the controls;
4.	Compute the SDID estimator (6) based on $(Y_{co}, W_{co}^{(b)})$;
5.	end
6.	$\hat{\sigma^2}_{pl} = \frac{1}{B} \sum_{b=1}^{B} (\hat{\sigma^2}_{SDID,b} - \overline{\hat{\sigma^2}_{SDID}})$

Table 4: Placebo Variance Estimation

5 Results

The Generalized Synthetic Control method I use has to fulfill the assumptions listed in Section 4.4. If the model is correctly specified so that factors can adjust for all the trends, time and unit fixed effects, the errors should stay homoscedastic and validate the analysis. However, even if the assumption of the functional form is violated, it is accounted for in

Synthetic Difference-in-Differences. The double robustness of this estimator in a large sample states consistency of both, specified and misspecified factor model (see Section 4.5).

5.1 Treatment assignment

My first try assigns the treatment to the tracts where number of units demolished is larger than 0, with the treatment continuing after. To smooth the trend in crimes, I aggregate the data by quarter. For a better accuracy and given a strong seasonality, I apply a minimum period of pre-treatment of 6 quarters. This leaves me with 35 treated and 827 control areas in the primary analysis. Treatment timing is displayed in the Figure 4.

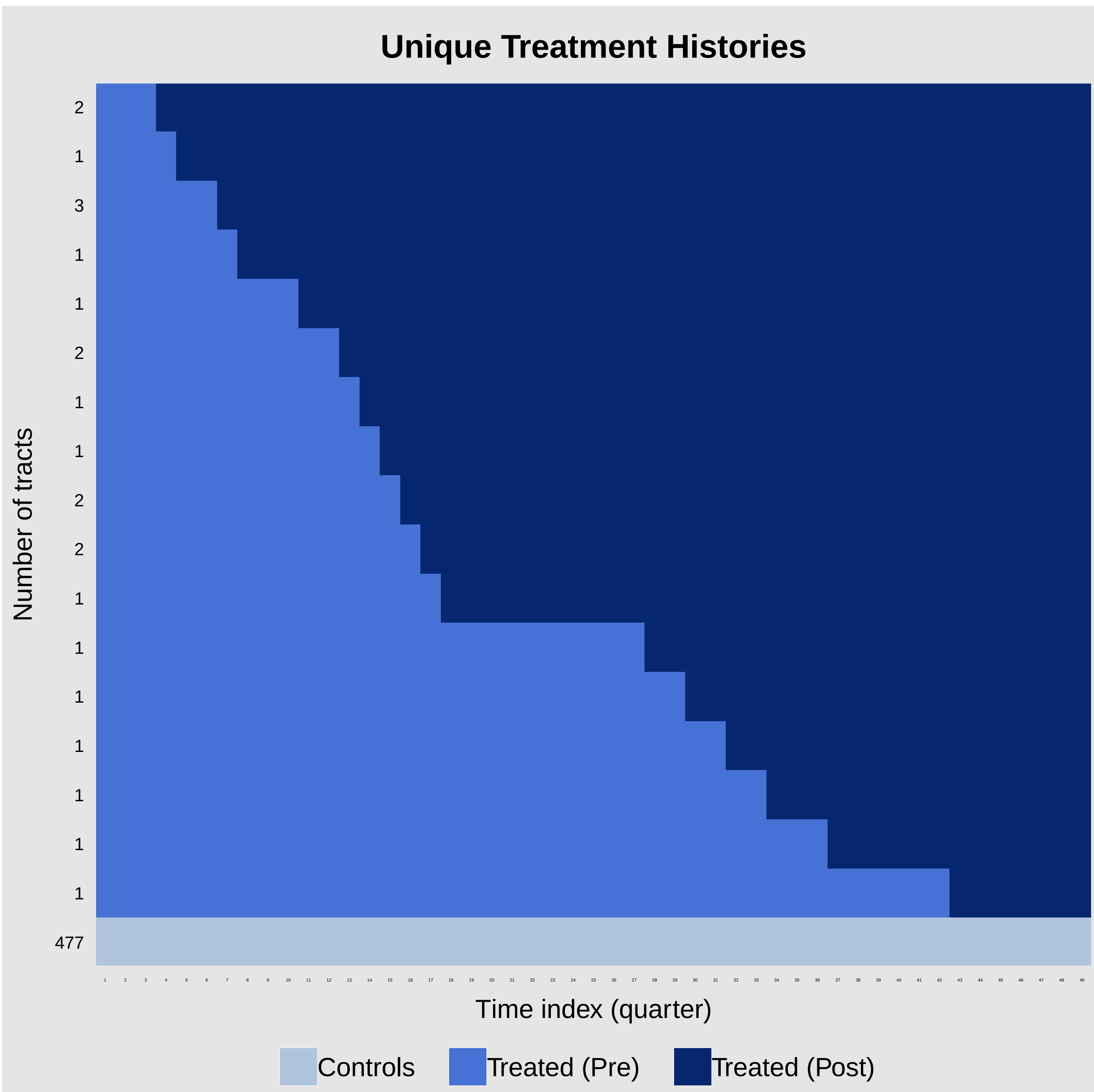

Figure 4: Treatment timing of tracts. The control group is trimmed.

Next, I winsorize the data on crimes and crime rate. Some of the tract have population of 1 or 2 persons, which cause crime rates to explode. This is can be caused by the non-residential area type, census under-counting and open spaces. My winsorizing takes a high level of 5% given a large variation of population. Figure 6 shows the distribution of winsorized crime rates within the city in 2000. Large heterogeneity allows us to build a comparable control group composed of untreated tracts. I focus on short-term effects of 6 and 10 quarters after the treatment occurs, as increasing the pre-treatment period leads to significant loss in the number of units demolished.

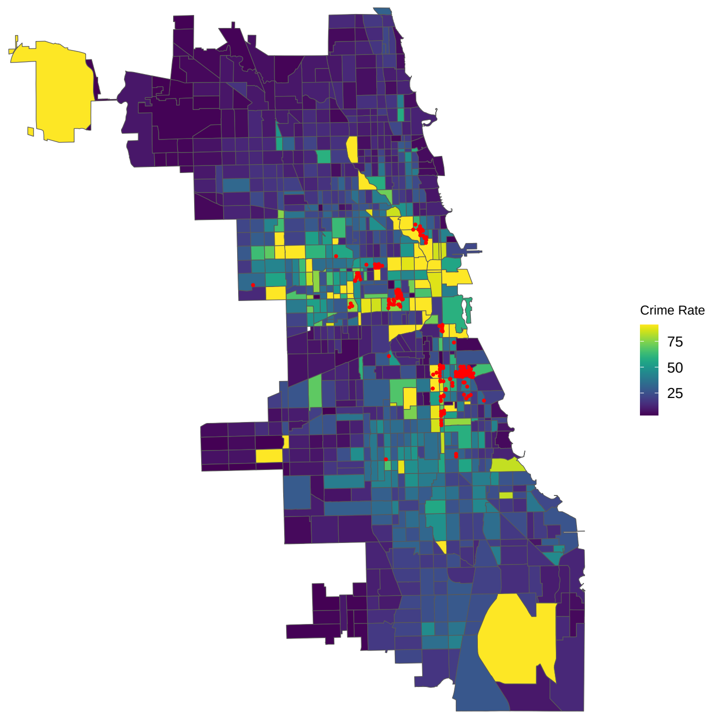

Figure 5: Winsorized quarterly crime rate per 1,000 citizens by census tract in 2000.

5.2 Findings

5.2.1 Two Way Fixed Effects

Simple TWFE shows a large decline in crime rate per 1,000 people, of 6.73 in the 6th quarter of the treatment and 12.31 of the 10th. However, the algorithm has difficulties following the trend in the pre-treatment period with significantly different value from 0 in the 6th quarter before the treatment starts and with a strong anticipation effect right before the treatment. This might be due to the eviction timing which follows earlier schedule than the demolition closure which is use as a treatment date.

5.2.2 General Synthetic Control

GSC tries to imitate the treatment group by assigning weights to the control group. Crime rate still follows the pattern of TWFE, however, the magnitude decreases by $\frac{1}{4}$ to 9.33 reduction in crime rate in the 10th quarter of the treatment. Although it becomes more accurate than TWFE, the algorithm still has difficulties in following the pre-treatment trend, with strong anticipation pattern visible. However, this crime rate assumes no change in population when units are demolished. I propose a conservative population adjustment of 1

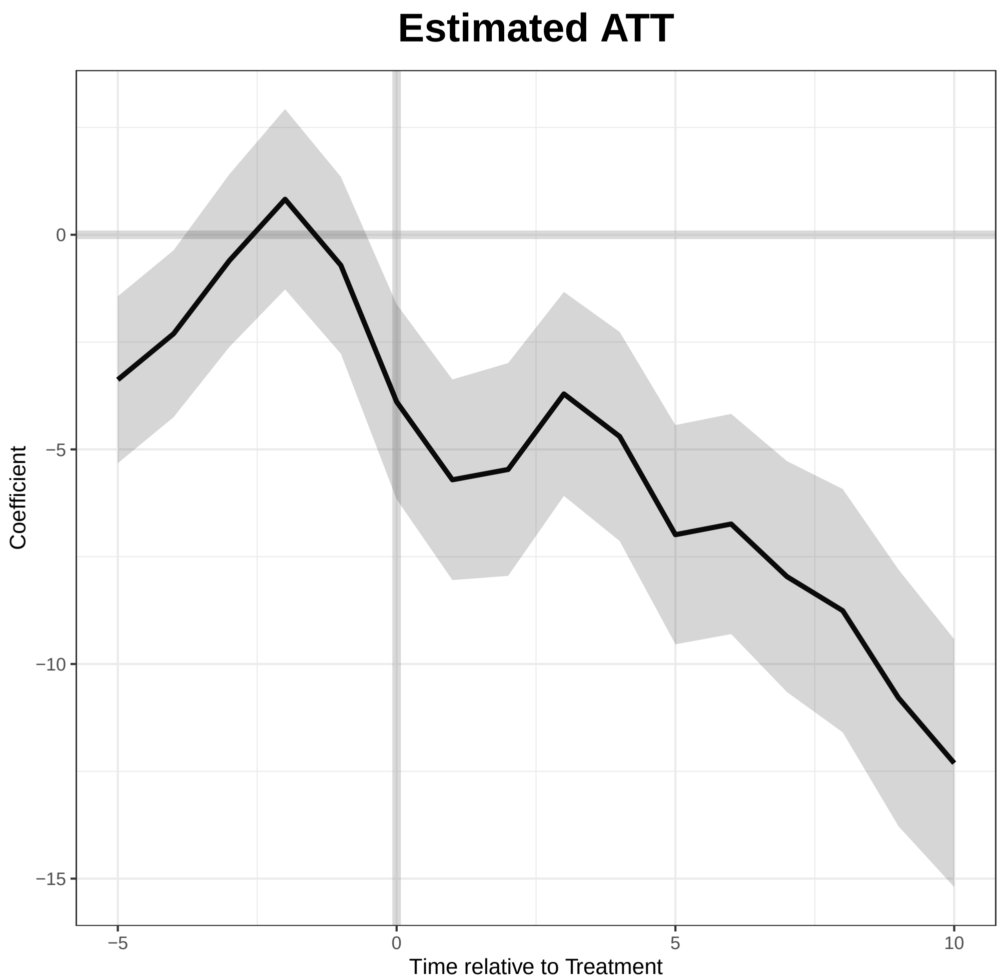

Figure 6: TWFE regression of first demolition on crime rate by tract (per ’000).

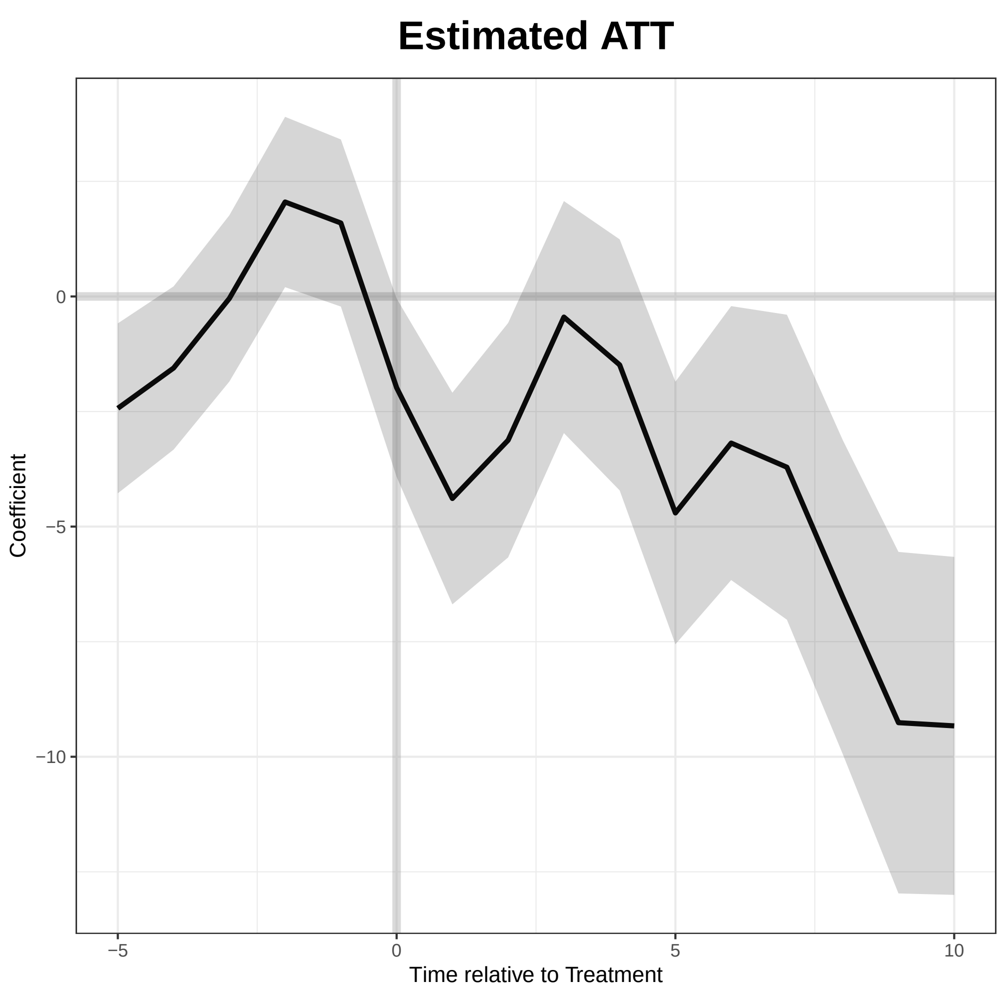

Figure 7: GSC regression of first demolition on crime rate by tract (per ’000).

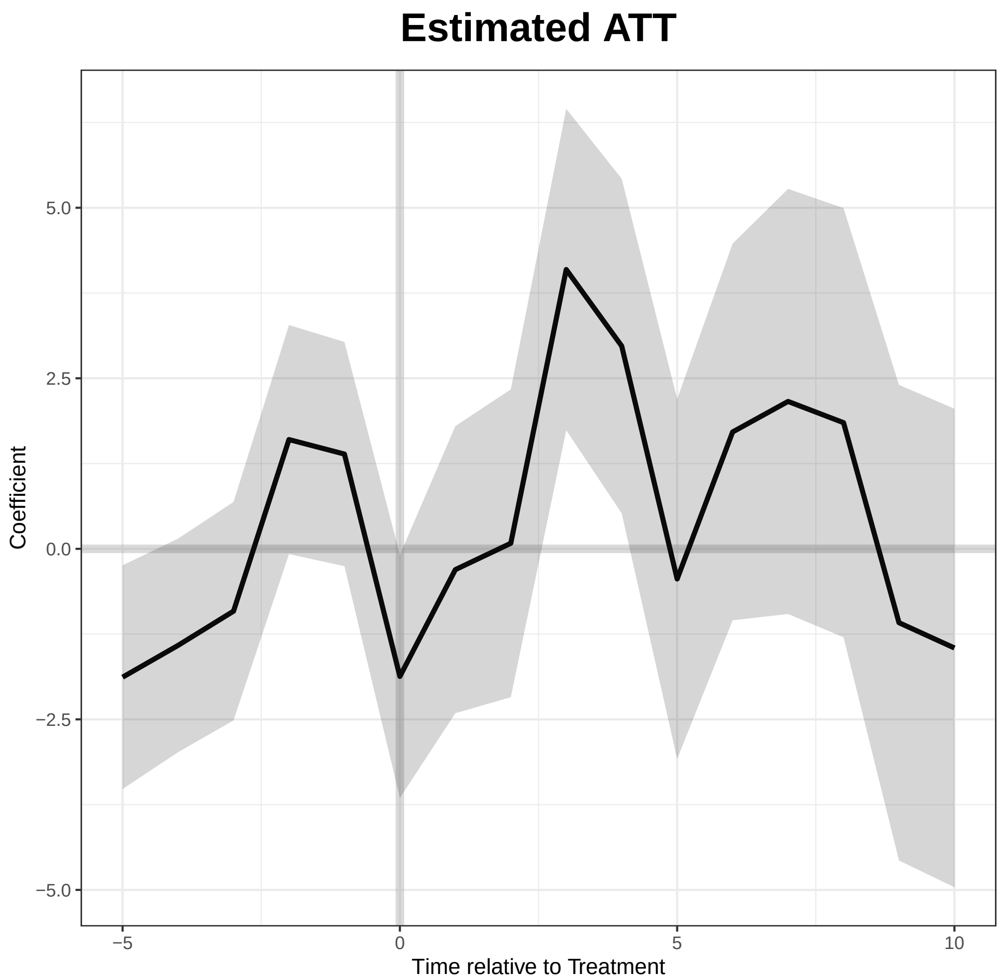

Figure 8: GSC regression of first demolition on adjusted crime rate by tract (per ’000).

person moving out of the tract when a unit is demolished. A unit is typically composed of 3-4 bedrooms with each bedroom intended for 2 people [Bruhn, 2017]. As residents may relocate within the same tract and although the occupation rate of the projects was high [CHICAGO HOUSING AUTHORITY, 2001], I assume only 1 person to move out of the tract. Even with the cautious assumption, as the denominator of crime rate goes down, the effects of demolition on crime rate seems indifferent from 0. This indicates that the demolition caused a decrease in a number of crimes in a tract simply by displacing people.

This analysis can be treated as an approximation of a General Equilibrium. Accounting for the relocation of people from the tract changes the results dramatically. Note that I do not put increase the population in the control tracts (to which the resident could have moved) which would further decrease the crime rate in the control group. This could even indicate an increase in crime rate in the tracts where demolitions occurred. The analysis needs further investigation with relocation data. However, already the conservative assumption of 1 person leaving the tract per 1 unit demolished, and not adding them to the control tracts seriously violates the results obtained in the current literature.

5.2.3 Synthetic Difference-in-Differences

Synthetic difference in difference estimates the treatment 10 quarters in but assigns weights to both, units and time. As the treatment timing varies, I separate the tracts to run the analysis and aggregate the results later. On average, the demolitions reduce the crime rate in the tract by 4.87 crime per 1,000 citizens, however, the mean error is 4.65, which means that this estimate does not pass a majority of statistical tests making it almost indistinguishable from 0. This is most likely a result of better pre-treatment matching. A sample graph for tract 80400 is shown in Figure 9. The red area shows the time weights (λ) while the arrow shows deviation from the trend. The weights make regression more local, as it matches similar periods and units with a higher weight. It also assigns higher weights to the periods which are closer to the treatment.

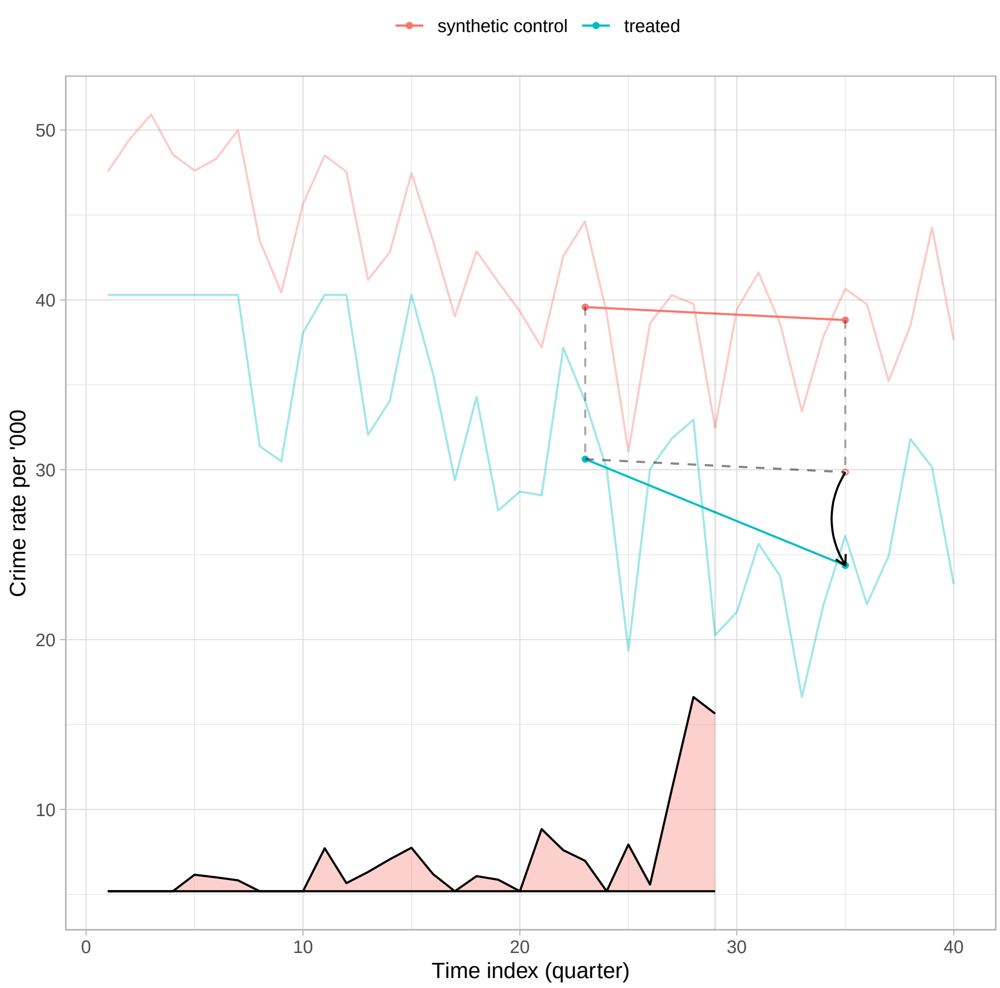

Figure 9: SDID regression of first demolition on crime rate for tract 80400 (per '000).

When adjusting for the population displacement from the tract, our estimate inverse to a positive value of 0.56 increase in crime rate in the 10th quarter of treatment, however, with an error of 4.16 making the estimate statistically indifferent from 0. This shows the importance of a population decrease channel in the total crime reduction. Nevertheless, we are going to adjust for multiple treatments in the next section.

5.3 Heterogeneous treatment

Given the fact that the tract is treated multiple times by different demolitions, I adjust this by separating the tracts to distinguish treated units, and weight the ATT by the numbers of units demolished, assuming constant marginal effects. In fact, in order to keep the further demolitions in the tract, I simply omit the first treatment if it happened before the 6th quarter. As I assume linear effects of the demolitions, it shouldn't influence my findings. However, if one assumes decreasing marginal effects, this design should be adjusted. omitt-ting demolitions which took place before the 6th quarter reduces the number of units from 17,550 to 16,054.

If a tract has gone through multiple demolitions in different time periods, it is broken out into different IDs, which indicate a unique demolition. The post-treatment period reaches at max the next demolition. That way, we do not estimate the post-treatment period more than once. Finally, I weight the estimates by number of units that appeared in the demolition. This way, my estimate is a weighted average of heterogeneous and staggered treatments.

5.3.1 General Synthetic Control

Bringing additional information on additional (multiple) demolitions minimizes the error of the analysis, with an average weighted estimate of 14.66 crime rate reduction in the 6th quarter and 20.39 10th quarter of the treatment. The standard errors are 2.44 and 3.28 correspondingly.

When crime rate is adjusted for the potentially displaced population, these estimates change to 15.86 and 17.27 respectively with standard errors of 2.36 and 3.97. We observe that weighting changes the impact of the population adjustment by assigning higher weights to tracts with a decrease than those in which the crime rate increased, which explains the inflated estimate in the 6th quarter.

5.3.2 Synthetic Difference-in-Differences

Unweighted estimate increases comparing to the previous case of setting the treatment after the first demolition, and is now a reduction of 5.24 crime per 1,000 citizens 10 quarters into the treatment with again, an error of 1.09. Estimate weighted by number of units demolished increases to 6.74 reduction in crime with an error of 5.18. Adjusting for the population displaced leaves us with a much smaller weighted estimate of 2.31 reduction in crime rate with an error of 1, which Again, the SDID analysis shows a less substantial decrease, however with a much larger error. The weighted estimate with staggered and heterogeneous treatment indicates almost insubstantial decrease in crimes after the demolition.

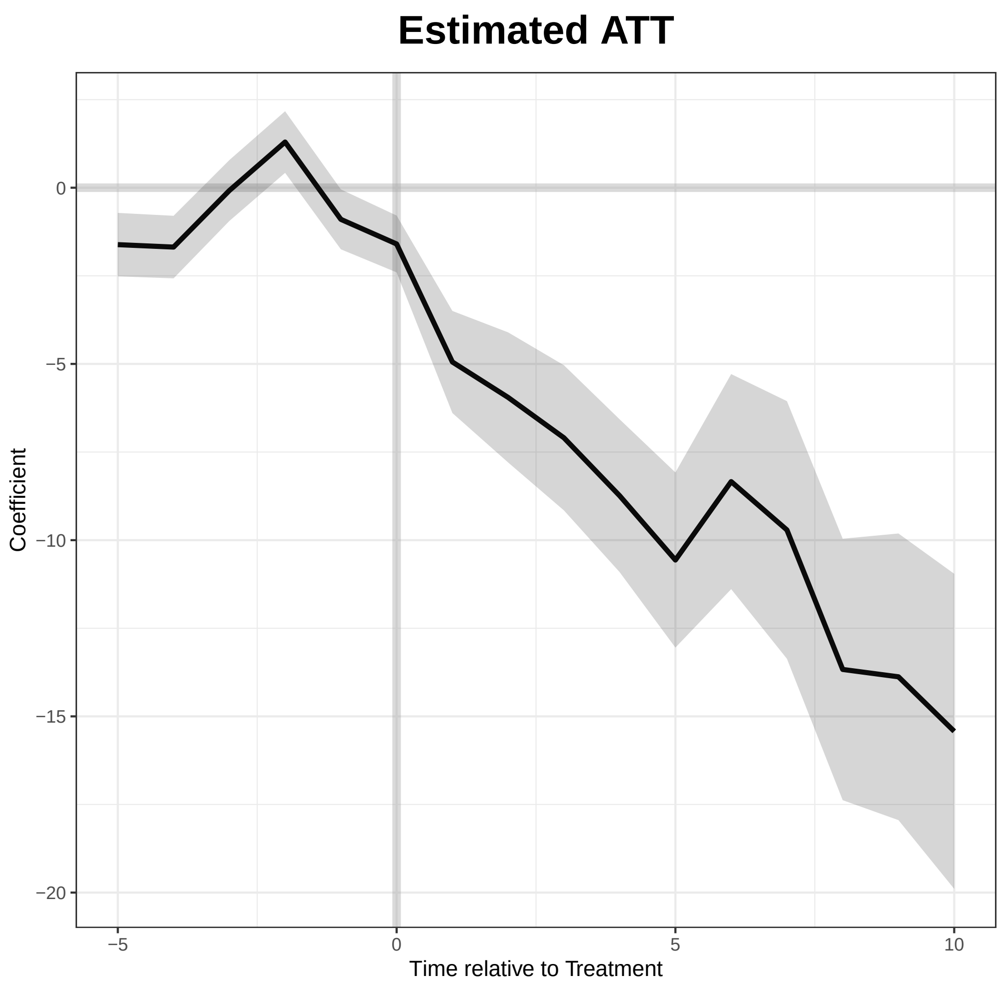

Figure 10: GSC (unweighted) regression of heterogeneous demolitions on crime rate by tract (per ’000).

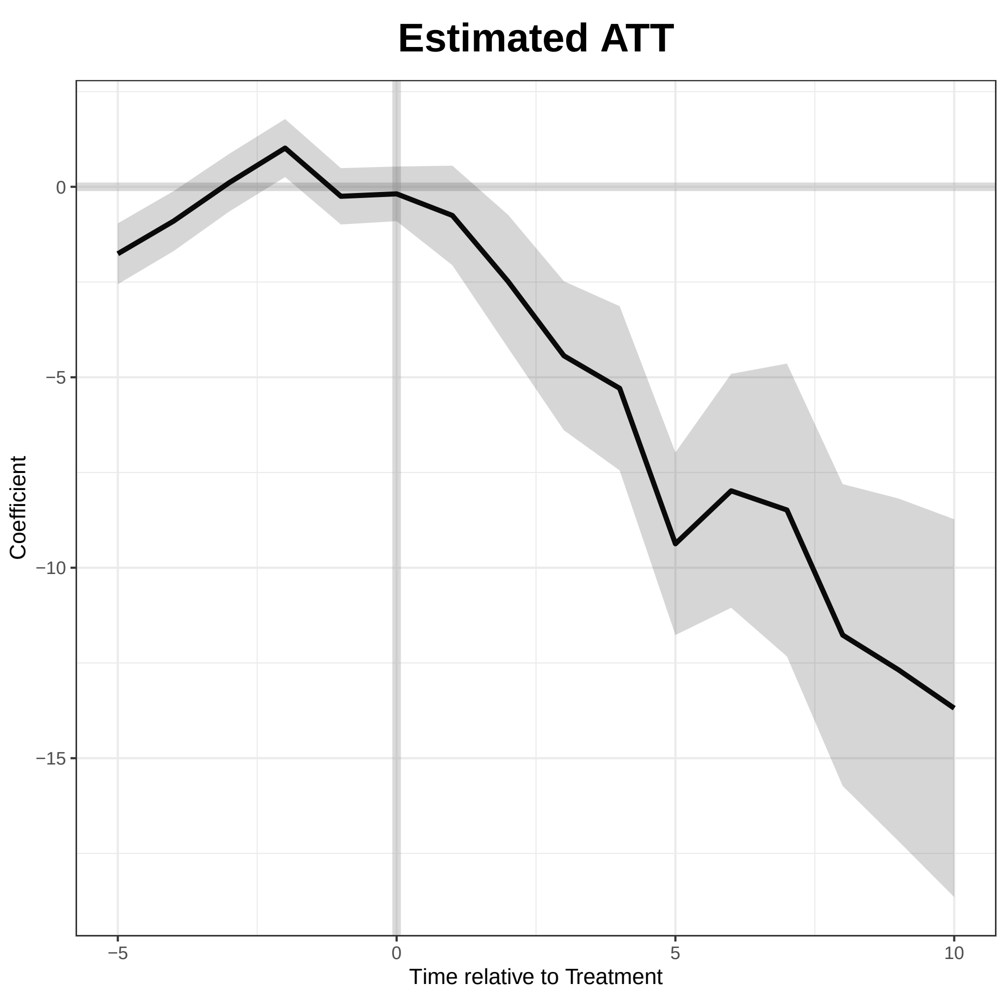

Figure 11: GSC (unweighted) regression of heterogeneous demolitions on adjusted crime rate by tract (per ’000).

5.4 Heterogeneity of crimes

Different types of crime contribute to the crime reduction in a similar way. The summary of the results when splitting total into economic, violent and drug crimes is described in the table 5. While economic crimes reduction are a significant part of all crimes in the 6th quarter of the treatment, it is drug crime reduction that causes the crime rate drop to decline in the longer term.

	Economic	Violent	Drug
Estimate (6Q)	-2.672	-1.643	-2.110
(Standard error)	(1.863)	(-0.258)	(0.823)
Estimate (10Q)	-0.45	-1.183	-7.344
(Standard error)	(3.694)	(0.297)	(1.082)

Table 5: Estimates of multiple demolition on crime rate by tract and type of crime. Coefficients do not add up as of separate winsorizing.

5.5 Spillover effects

I search for spatial spillovers by assigning treatment to all neighboring tracts, and excluding the originally treated tracts from the analysis. I use the heterogeneous treatment analysis and GSC to calculate the spillovers, as it provided me with precise estimates, shows dynamic effects, and a direct implication of the demolitions. We will examine whether these findings are robust when changing the treatment group, or when conducting a sensitivity analysis.

As the areas around the treated tracts can differ a lot from the treated units, I do not expect a large spillover effect. In fact, [Sandler, 2017] has shown that the results of the demolition are very local and disappear within a radius of 3 miles. As the area of a tract is ... on average, I do not expect spatial spillovers affecting other tracts. Indeed, the GSC which indicated a large drop in crime rates in the treated tracts, shows no significant change of crime rates in the neighboring tracts. Large errors visible in Figure 12 confirm strong variety in the neighboring areas. Weighting the estimates by units gives me an estimate of 0.51 crime rate reduction with an error of 2.17, which makes the estimate statistically indifferent from 0.

5.6 Threats to identification

The obvious threat to identification is 'contamination' of our control units. In fact, as described before, we are unable to identify the pure effect of the demolition on crime rates inside the tract. Instead, we estimate a difference between control groups (potentially con-

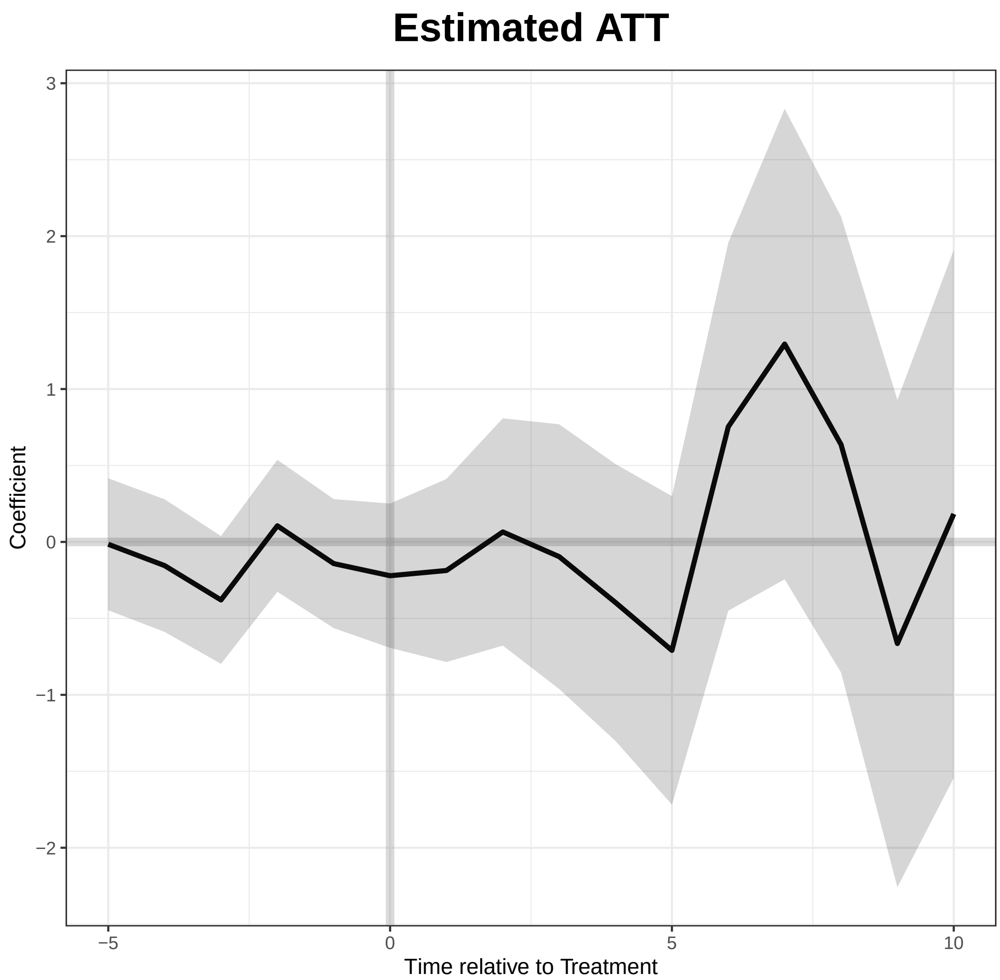

Figure 12: GSC (unweighted) regression of heterogeneous demolitions in neighboring tracts on crime rate by tract (per '000).

taminated) and treatment group that is created after the demolition. Yet, the control group should be similar to the treatment group before the treatment starts, so the analysis of effects of public housing demolition in the city of Chicago is valid.

A serious threat to identification for GSC is a factor model misspecification, yet, our primary results are confirmed by SDID analysis, which allows for this misapplication. However, later analysis raises doubts about any effect except the population one that caused reduction in absolute crime.

5.7 Sensitivity analysis

5.7.1 Increase in the pre-treatment period

First, this might help to adjust the pre-treatment path more precisely, and improve the accuracy of our analysis. However, applying a minimum pre-treatment period of 8 quarters reduces the number of units demolished from 16,054 to 14,792. Indeed, the anticipation effect is weaker, but the estimates do not change significantly, visible on Figure 13, with weighted estimates of 14.03 (SE 2.55) and 16.99 (SE 3.39) reduction in crime rates in 6th and 10th quarter of the treatment respectively.

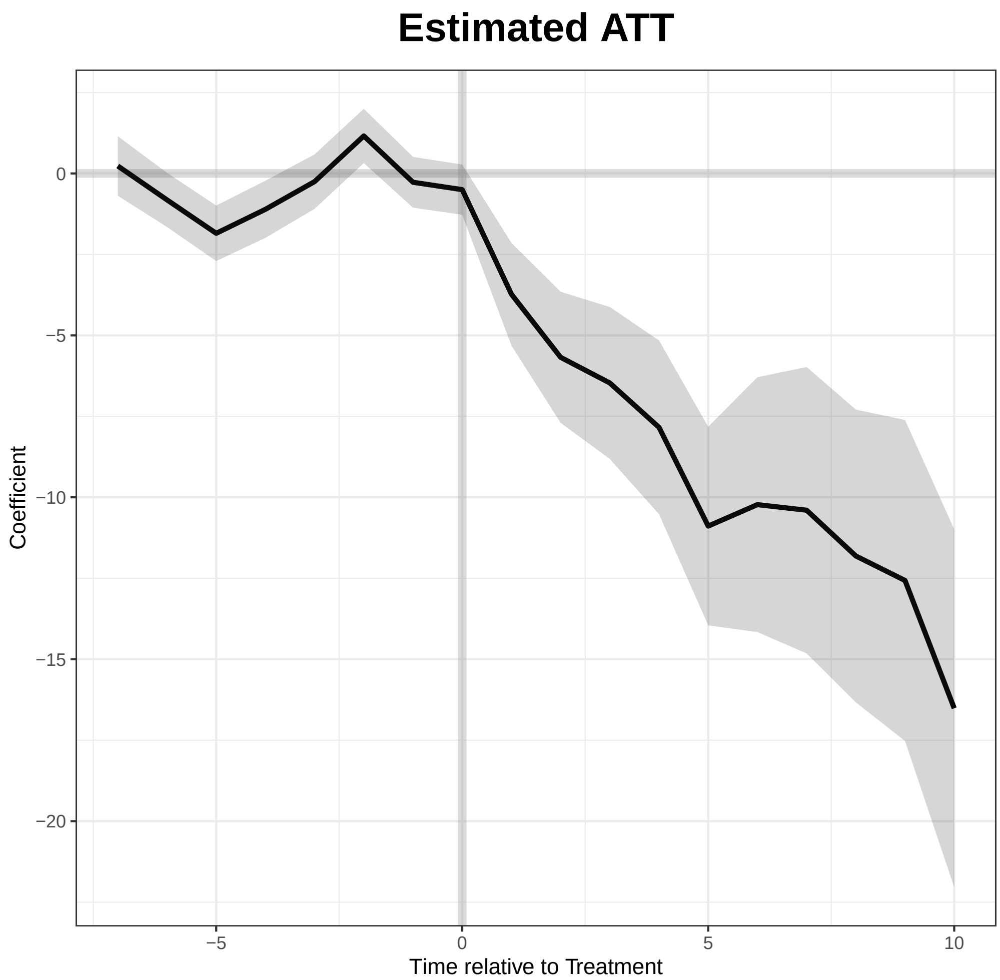

Figure 13: GSC (unweighted) regression of heterogeneous demolitions on crime rate by tract (per '000) with minimum 8 quarters of pre-treatment period.

5.7.2 Omittting small demolitions

Applying a threshold of effective treatment when the demolition exceeds 75 units reduces the number of units that affect treatment from 16,054 to 15,412. This will decrease the precision of the analysis, however, it serves as a robustness check to verify whether in fact the crime reductions are not driven by tracts with few of them. Additionally, we can expect a small shift in the pre-treatment fit as we reduce the number of treated units.

The precision of the estimates weakens, although the point estimates follow a very similar pattern with slightly lower effects of 12.2 decrease in crime rate in the 6th quarter and 15.60 in the 10th quarter of the treatment. This is self-explanatory, as we remove the additional impact of small demolitions on the crime rates. omitting the additional effect decreases the estimates and increases the error by reducing number of observations.

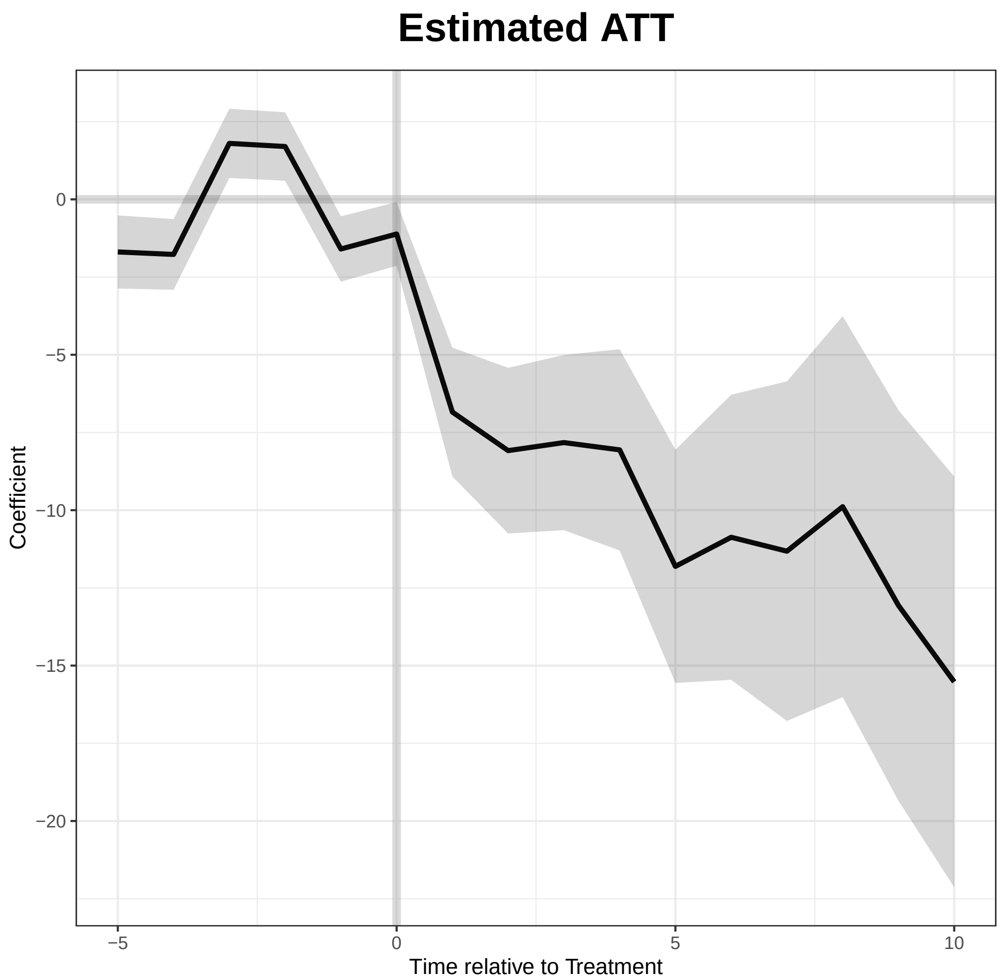

Figure 14: GSC (unweighted) regression of heterogeneous demolitions on crime rate by tract (per ’000) with minimum of 75 units demolished.

6 Conclusion

The Chicago Plan for Transformation was supposed to reduce crimes in the areas of demolition. Two main channels that stand behind the crime change in the demolition areas are the residents displacement and breaking criminal networks. Design of the projects, lack of maintenance work, income and racial segregation contributed to gang development around the public housing. The demolitions should advance a disruption of the social networks by depriving the gangsters of their headquarters. At the same time, the demolitions deepened inequalities within the city [Almagro et al., 2023], and worsen the material status of the ex-residents. These 2 factors, poverty and inequality, might have resulted in an adverse than premeditated effect - increasing city-crimes.

To examine the topic, I use 2 methods which adjust for a non-random treatment selection posing a threat for identification when conducting a difference-in-differences analysis. Potentially, the areas with higher crime rates could have been selected the first ones to be treated. Both, Generalized Synthetic Control and Synthetic Difference-in-Differences allow for the correlation between the treatment, outcome and tract characteristics. Additionally, both of the techniques provide us with consistent estimators. Large discrepancy in crime rates around the city allows us to create a credible control group and produce valid estimates.

The analysis conducted in this paper shows ambiguous effects of demolitions on crime rates. Accounting for multiple treatments of the areas produces estimates ranging from 20.39 to 6.74 of crime rate reduction per 1,000 citizens. However, adjusting for population displacement of 1 person per 1 unit demolished refines the range to 17.27 for Generalized Synthetic Control Method and 2.31 for Synthetic Difference-in-Differences. Much lower estimate for SDID can be a result of lack of control group contamination, as the pre-treatment path does not have to exactly follow the treated group, but can be parallel. Therefore, the control group in SDID is less probable to consist of the areas to which the residents move. If we assume the negative social networks to displace as well, it is not probable that they move to continue their activity in the areas shifted downwards in terms of crimes. Further analysis indicates no spatial spillovers on neighboring tracts. Increase in pre-treatment period and omitting smaller demolitions do not change the estimates significantly, assuring that the estimates are not sensitive to slight design changes.

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7 Appendix

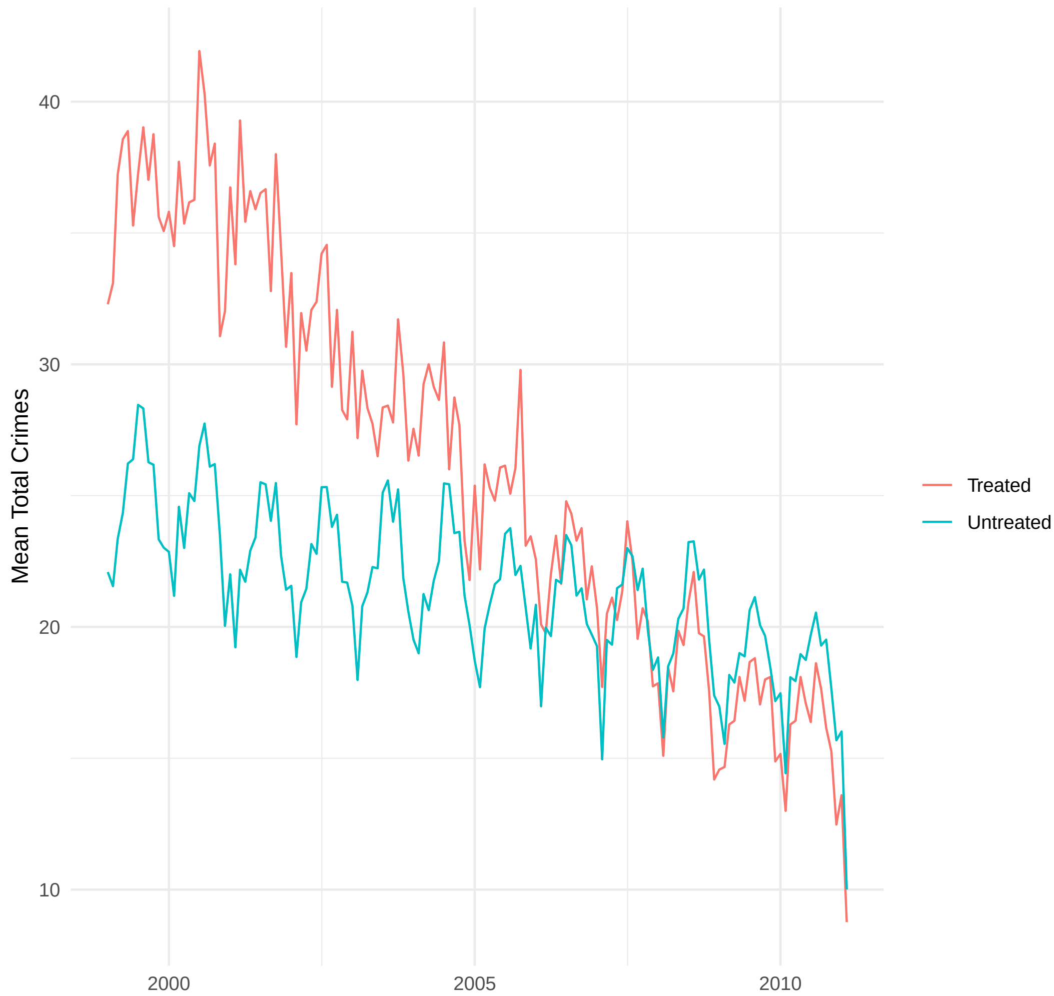

Figure 1: Monthly crime evolution among treated and untreated tracts.

	Single treatment		
	TWFE	GSC	SDID
Crime rate (10Q)	-12.310	-9.329	-4.874
(Standard error)	(1.473)	(1.873)	(4.645)
Adjusted crime rate (10Q)	-4.028	-1.453	0.555
(Standard error)	(1.307)	(1.789)	(4.159)
Treated demolitions	35	35	35
Untreated tracts	827	827	827
All tracts	862	862	862
Units demolished	11,705	11,705	11,705
Min. T_0	6	6	6

Table 1: Summary table of first demolition treatment estimates on crime rate.

	Multiple treatments				
	GSC	SDID	GSC (Spillover)	GSC	GSC (75+)
Estimate (10Q)	-20.388	-6.743	-0.007	-16.985	-15.597
(Standard error)	(3.289)	(1.093)	(2.467)	(3.393)	(8.317)
Adjusted crime rate (10Q)	-17.27	-2.308	-	-17.715	-17.826
(Standard error)	(3.971)	(1.000)	-	(2.990)	(21.027)
Treated demolitions	120	120	514	113	69
Untreated tracts	827	827	697	827	827
All tracts	869	869	827	867	868
Units demolished	16,054	16,054	-	14,792	14,125
Min. T_0	6	6	6	8	6

Table 2: Summary table of multiple demolition treatments estimates on crime rate.

	Multiple treatments	
	GSC	SDID
Estimate (10Q)	-20.388	-6.743
(Standard error)	(3.289)	(1.093)
Adjusted crime rate (10Q)	-17.27	-2.308
(Standard error)	(3.971)	(1.000)
Treated demolitions	120	120
Untreated tracts	827	827
All tracts	869	869
Units demolished	16,054	16,054
Min. T_0	6	6

Table 3: Summary table of multiple demolition treatments estimates on crime rate.

Census Tract	Number of Units
17031351500	1644
17031351100	1448
17031283900	1389
17031381400	1262
17031280800	1118
17031080800	991
17031283200	753
17031280400	686
17031330300	666
17031360300	665
17031080500	556
17031283800	479
17031380600	474
17031380500	474
17031400200	473
17031381700	467
17031360200	464
17031291500	323
17031381600	316
17031381800	316
17031080400	280
17031280500	279
17031351200	264
17031381000	206
17031350200	203
17031380300	183
17031400800	171
17031390300	155
17031280900	148
17031081900	136
17031080600	134
17031081800	130
17031340600	78
17031380400	72
17031380100	48
17031231700	36
17031360100	32
17031590200	21
17031252100	3
17031390200	3
17031670700	2
17031370100	2

Table 4: Distribution of demolished units by Census Tract

8 Acknowledgements

Writing this up was not an easy task, although it was just a cherry on the top of the cake after such an intellectual journey. This would not have been possible without all the precious people by my side.

I want to thank all the Sciences Po professors who provided me with an opportunity and inspiration to dig more into social issues, studying economics as such. Thank you Roberto, Florian, Aurélie. You're probably not aware how much it means to learn from you. It is priceless.

Certainly, I would like to give shout out to my mom and my brother who never doubted me. This assurance made it possible to calmly deal with stress, deadlines and all the challenges these 2 years imposed on me. Thank you for your wisdom and emotional support. I love you both.

This university would not be the same without you, Marghe. Not enough words can be written to my one and only. The good spirit you have inspires me every day to become a better person. You have no idea how much I value your support. I love you.

To my friends who were my safe space during all the tough moments and made the happy ones even happier. Thank you Andre, Hui Yann, Ignacio, Rhea. For all the time in the library, movie nights, great food but first and foremost the conversations and moments we shared together. This will stay in me forever.

Finally, I want to thank the French government for financing this life-changing opportunity for a small-town boy like me.