The Promise of Integration: Impact of HOPE VI Public Housing Demolitions on Neighborhood Crime in Chicago

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Submitted to the Department of Economics of Amherst College in partial fulfillment of the requirements for the degree of Bachelor of Arts with honors.

Abstract

This thesis examines the impact of a large income-mixing redevelopment program, specifically the HOPE VI public housing demolitions, on neighborhood public safety externalities in Chicago. To identify the causal effect of demolitions on crime reported by local law enforcement, I leverage the variation in the timing of program applications. Using a two-way fixed-effects specification, I find a significant reduction in crime following demolitions in the post-millennium years between 2001 and 2015. This result is generally robust to the inclusion of neighboring demolitions as spatial lags and addressing selection bias through matching. An event study further shows that the decrease in crime is dependent on time elapsed since the first tract-level housing demolitions and largely driven by implementations in entities with relatively higher socio-economic status. Overall, this study provides evidence that the demolitions of public housing units under the HOPE VI program induce a positive change in reported crime within neighborhoods.

JEL Classification Codes: R12, R23, R28, J15.

Keywords: Structural Inequality, Public Housing, Crime and Policing.

Acknowledegements

I would like to express my deepest gratitude to Professor Reyes for her guidance and wisdom throughout my thesis journey. Your constant support and meticulous feedback have been invaluable, and words cannot describe how fortunate I feel to have had you in my corner. Thank you for being a mentor, advisor, and friend.

To Professor Barbezat, your passion for doing good in this world is contagious. I am very grateful for your thoughtful suggestions on my writing and organization, as well as the kindness and positivity you bring to every interaction.

I also want to thank professor Ishii for challenging me to deepen and substantiate my understanding of public housing related topics in the early stages of this thesis. Your criticism over the years is understood and appreciated. Thank you to Professor Hyman and Professor Sims for their additional input and abundant patience.

Lastly, I want to thank Alex for being my rock, and my family for making me who I am today. 只要有你,一切安好。

Contents

1 Introduction

The multigenerational and detrimental effects of urban inequality, racial discrimination, and concentrated poverty have prompted mounting policy responses at both the federal and local levels in the United States (Shell, 2020; Sampson and Wilson, 2008; Peterson and Krivo, 1993). Place-based strategies, which aim to directly address the deprivation of resources and economic opportunities experienced by residents in historically subjugated neighborhoods, especially those residing in high-rise public housing projects, are one set of such initiatives.

This thesis contributes to the literature on community impacts of place-based public housing policies by examining one of the most ambitious efforts at mixed-income redevelopment: The Housing Opportunities for People Everywhere (HOPE VI) program. A key feature that sets HOPE VI apart from other similar programs is the complete demolition of dilapidated housing projects in order to make room for reconstructed low-density housing developments at the original project sites. Using Chicago as a case study, I examine the impact of housing demolitions on measures of public safety and social order as reported by police. The list of project sites of demolitions are obtained from the Chicago Housing Authority; incident-level crime data from the Chicago Police Department's open data portal is aggregated at the census tract level and used as a proxy for illegal activities in a given area.

To estimate the causal effects of HOPE VI demolitions, I utilize the variation in the timing of program application and employ a staggered difference-in-differences framework with entity and time fixed effects. In addition to temporal effects, the physical changes to the built environment resulting from large public housing projects, along with the movement of residents in and out of neighborhoods due to demolitions and reconstructions, suggest the possibility of spatial spillover effects of HOPE VI to nearby census tracts. To account for this, I examine the potential for tract-to-tract spillovers by including a cross-regressive term of the aggregate demolition status of the ten closest neighboring tracts as a spatially lagged explanatory variable.

My main set of specifications conveys that the changes in reported crime induced by public housing demolitions are both statistically and ecologically significant. The two-way fixed-effects specification reveals that HOPE VI reduces the tract-level crime rate at 12.12 occurrences per thousand people on a monthly basis, indicating that the policy intervention is responsible for 29% of the crime reduction in the treated tracts between 2001 and 2015. When a tract-specific linear time trend is allowed, the average treatment effect of HOPE VI is -10.11 occurrences per thousand people, a magnitude that is nearly three-quarters as large as the average reported crime rates in the counterfactual, which includes census tracts contained in neighborhood areas with public housing developments. Adding the neighboring demolition status as an explanatory variable slightly diminishes the impact of a tract's own demolition on its reported crime. However, while the spillover effects are negative and of considerable magnitude, they are not statistically significant.

The estimation methods used in this study rely upon two important assumptions: parallel trends and homogeneous treatment effects. Specifically, the parallel trends assumption assumes that the difference in crime rates between the treatment and control groups would be constant in the absence of demolitions. However, imposing a tract-specific linear time trend allows for a differential slope for each census tract, requiring pair-wise differences to be linear over time instead. Also, the evolution of the treatment effects should be homogeneous across treatment groups and across time periods.

Based on preliminary analysis of crime trajectories, it appears that the pre-demolition evolutions for the treatment and control groups are indeed comparable. However, the heterogeneity observed in the baseline census tract-level outcomes and covariates raises concerns about potential selection bias. It is possible that factors that determine demolition assignment may also impact reported crime outcomes. To address this issue, propensity score matching is utilized to select a subset of treatment and control tracts that closely resemble each other based on a set of baseline tract demographics, including racial composition and income distributions. Although smaller in magnitude, qualitatively similar results arise from this matched dataset. On average, HOPE VI leads to a reduction of 6.48 points in per thousand population crime rates in the TWFE model and 4.40 points when a linear time trend is allowed.

To examine the dynamic effects of demolitions over time and further validate the parallel pre-trends, an event study is conducted. The results of this part of my analysis confirm that the coefficients on the lead variables are jointly indistinguishable from zero, indicating exogeneity in demolition assignment. Furthermore, the magnitude of crime reduction increases with the length of exposure to demolitions. Treatment effects are also found to be greater in census tracts with higher socio-economic advantages. This is in line with the conjecture that historical disinvestments in underprivileged areas impede the effectiveness of housing policies. Finally, to further address potential bias introduced by heterogeneity across cohorts and time, an alternative group-time average Callaway-Sant'anna (2021) estimator is utilized, and baseline covariates are incorporated through a two-step Doubly Robust procedure.

Overall, the findings indicate that the demolitions of public housing projects under the HOPE VI program had a significant and positive impact on reducing reported neighborhood crime rates in selected Chicago neighborhoods between 2001 and 2015. However, it is important to note that discerning the effectiveness of the HOPE VI program in promoting neighborhood well-being is limited by the contentious nature of historical crime statistics.

These are likely influenced by racial bias and damaging narratives that shift the blame for systemic issues to public housing tenants. As a result, the reporting of crime is also biased by community norms regarding the police. It is worth noting that this thesis does not examine individual outcomes following relocation, nor does it delve into the underlying mechanisms that contribute to the reported changes. Therefore, this thesis urges future studies to comprehensively discuss whether and how housing policies have altered the social fabric of neighborhoods, provided pathways for upward social mobility, or perpetuated existing inequalities.

2 Background

2.1 20th Century Urban Crisis

In the wake of the American victory in WWII, as investments in the war effort bolstered industrial production and reinvigorated the workforce, the prospect of well-paying manufacturing work in the factories in the North, along with dire economic conditions and oppressive social conditions in the Jim Crow South, accelerated the patterns of Black exodus in a "Second Great Migration" from the South and in-migration to cities of the Northeast, Midwest, and West (Gregory, 2009). As city centers grew ethnoculturally diverse in subsequent decades, the urban landscape changed in several ways.

Residential neighborhoods were increasingly stubbornly segregated along racial and ethnic lines. Boustan (2010) describes some potential channels by which the white population responded to the in-migration of Black and Brown tenants and other people of color. As one might think at first, since the housing supply is not perfectly elastic, higher housing demand begets higher housing prices, encouraging some white households to move into surrounding suburbs. However, the bid-up of housing prices does not account for the observed scale of white flights: every Black arrival is associated with 2.7 white Departures (Boustan, 2010). Although it is possible that individual white families may have moved for reasons that are not directly related to race, the larger trend of white flight and the wholesale abandonment of urban centers cannot be dismissed as something innocuous. This process is highly selective, based on race and income, and fundamentally motivated by racial intolerance. The refusal of white residents to move into vacant urban properties following a normal turnover and the creation of all-white destination neighborhoods further justify the racial motivation behind the exodus of these former city dwellers (Frey, 1979).

This distaste for non-white racial and ethnic identities also manifested itself in local, state, and federal-level discriminatory policies that mandated residential segregation — a phenomenon that is by no means some natural outcome of market forces or community preferences, but rather a feature of the deliberate persecution and subjugation aimed at marginalized people. Zoning has regulated land use by prohibiting people of color from sharing residential space with white homeowners through density restrictions and perpetuated environmental racism by purposely locating manufacturing activities in minority neighborhoods (Shertzer, Twinam, and Walsh, 2014). Redlining and federal loan programs were also designed to systematically deny mortgages, insurance loans, and other financial services on the basis of race and ethnicity, resulting in unequal access to credit. This differential access can have significant impacts on individuals' long-term socio-economic success, affecting family structures, incarceration rates, and income distribution, as noted by Aaronson (2021). Further, the arrival of interstate highways, supported by federal transportation investments, created physical barriers between predominantly white middle-class and affluent neighborhoods and encroaching Black communities (Karas, 2015). All of these policies are deeply rooted in a history of mutually reinforcing racialized exploitation and capital accumulation, which has perpetuated inequality and injustice for generations (Urban Institute, 2009).

The rise of residential suburbanization mentioned above is accompanied by a shift in the location of industrial and manufacturing establishments from urban centers to the suburbs, and eventually to foreign countries. The linkage between suburbanization of housing and employment is supported by a positive correlation between spatially decentralized populations and employment (Glaeser, 2001). As central city employers have increased their demand for skills and education, the urban labor market has become increasingly polarized into low-wage and high-wage sectors, disproportionately harming urban minority blue-collar workers in most deindustrializing cities since their educational prospects are heavily constrained by racial segregation in the public school system (Wilson, 1987).

The idea that suburbanization of jobs and involuntary housing market segregation act together to create a surplus of workers relative to the number of available jobs in inner-city neighborhoods was formalized by John Kain in 1965 as the spatial mismatch hypothesis (Kain, 1965; 1968).[1] However, the extent to which severely limited residential choices, combined with the dispersal of jobs from central cities, explains the low rates of employment and earnings for inner-city populations is still a subject of ongoing research. Ellwood and Leonard (1986) reject the spatial mismatch hypothesis, finding that spatially determined access to jobs has a negligible impact on Black disadvantages in labor employment. Instead, they argued that the key explanatory variable is not space, but race. This is derived from evidence that racial composition of a census tract, more than any individual or spatial characteristics, such as skills or commuting, accounts for most of the variation in the employment-population

1. In his seminal paper in 1968, Kain discusses this relationship between housing market segregation and black employment drawing evidence from Detroit and Chicago, specifically.

ratio. Others have attributed low Black and Brown employment to a combination of spatial unavailability of suitable jobs along with racial mismatch, the idea that widespread aversion to young, inner-city Black workers has dictated the lack of employment opportunities into which Black workers are hired. (Cohn and Fossett, 1996; Aponte, 1996; Hellerstein et al., 2008).

All of this led to endemic patterns of socioeconomic precarity and highly segregated neighborhoods in urban centers, pervasive across major American metropolises. For instance, in 1960, Chicago had a population of 3.5 million, with approximately a quarter of the population being Black or African-American (Chicago Reader, 2013). Of this population, around 70% resided in community areas that were over 94% Black, while 29.7% lived in poverty (compared to 7.4% of white residents), and 7.6% were unemployed (compared to 2.3% of white residents). Unfortunately, these statistics have not seen significant improvement over time. As of 2012, 63% of Chicago's Black population still lived in community areas that were 95% Black, 34.1% lived in poverty, and 19.5% were unemployed (Chicago Reader, 2013). Systemic and structural racism, deeply embedded in systems, laws, policies, and beliefs, are the front-seat driver of the staggering and detrimental urban crises that underprivileged inner-city residents have continued to face.

2.2 The Failure of Public Housing Projects

Public housing in the United States was initially established with the goal of providing temporary, decent, and affordable housing to individuals and families facing economic hardship in the aftermath of the Great Depression (Chaskin and Joseph, 2015). Typically administered by federal, state, and local agencies, public housing is usually priced well below the market rate.

The Chicago Housing Authority (CHA) is the third largest public housing authority in the United States, serving over 20,000 low-income households across the city today (CHA, 2023). The majority of its public housing units were high-rise buildings located on the West and South Sides, commonly referred to as "the projects." One of the country's most concentrated public housing developments is the Robert Taylor Homes, which opened in 1962 and consisted of 28 identical, 16-story buildings with 4,400 apartments. At its peak, the development was home to around 27,000 people, most of whom were Black or African Americans (Hunt, 2001). Multiple Bronzeville gangs often fought for control of the project, making it a place plagued by unsettling violence. The elevators were often broken, and the stairwells were dark as light fixtures frequently malfunctioned or vandalizeed. Many apartments suffered from rodent infestations and inadequate heating, making them almost uninhabitable (Hunt, 2001).

Several factors have contributed to the failure of high-rise public housing projects like the Taylor Homes. Residential segregation is a primary factor that makes these developments destined to fail. White residents often resist the presence of public housing projects in their neighborhoods, fearing that they will reduce property values and reduce the area's amenity values. These sentiments are often backed by racist politicians and city planners who ensure that existing racially-segregated living patterns are preserved. As a result, most housing projects are built in neighborhoods where political opposition is low, and the population is predominantly Black and underprivileged (University of Pennsylvania Law Review, 1974). Furthermore, the Brooke Amendment of 1969 established a cap on public housing rents, requiring that tenants not be charged more than 30 percent of their adjusted monthly income. While well-intentioned, this legislation has led to unintended consequences, as it has driven out working families and accelerated the decay of public housing. Since the rental income is minimal, the amendment provides little incentive for landlords to invest in maintenance and security, worsening the poor living conditions. The rent cap acts as a disincentive for the poor to seek work, as any increase in income could lead to a corresponding increase in rent, leaving them with little to no additional disposable income. Lastly, the lack of adequate funding resulted in poor building quality, ineffective management, and improper maintenance. As living conditions deteriorated, those who could afford better options moved elsewhere, leaving behind a tenant population that consisted of the most impoverished and the most vulnerable (Urban Institute, 2013).

2.3 Deconcentrating Poverty through Housing Policies

The Robert Taylor Homes, along with many other historical high-rises in Chicago, were constructed as part of a massive federal urban renewal plan to eradicate slum neighborhoods. However, instead of providing disadvantaged families with adequate housing and new opportunities, these developments perpetuated patterns of concentrated poverty and racial segregation.[2] To address the precarious conditions of public housing, current housing and community development strategies focus on curtailing poverty concentration in inner-city neighborhoods and improving the living conditions and access to opportunities for their residents. These efforts typically employ two approaches: the “people-based” approach and the “place-based” approach (Chaskin and Joseph, 2015).

The people-based approach to housing policy often takes the form of *dispersal*, which aims to integrate low-income residents into more economically productive neighborhoods to achieve positive long-term individual outcomes. Through vouchers, residents are enabled to move out of inner-city areas with inadequate housing, public services, employment op-

2. Richard Rothstein of the Economic Policy Institute describes in his book, *The Color of Law*, that federal housing program established under the New Deal, was as good as a ”state-sponsored system of segregation.”

portunities, and education systems and into areas characterized by middle-class residents, abundant resources, and beneficial social networks. Unlike the concentration of subsidized housing in large complexes managed by local housing authorities, the dispersal approach encourages public housing tenants to rent housing in the private market for higher mobility. One notable application of the people-based approach is the "Moving to Opportunity" (MTO) program, a major housing mobility experiment implemented at scale in cities such as Baltimore, Boston, Chicago, Los Angeles, and New York City starting in 1994 (Chetty, 2016; Goetz, 2010). In these cities, a sample of public housing tenants was randomly assigned to one of three groups: an experimental group that received conditional vouchers that could only be used to move to a low-poverty census tract, a section 8 group that received unrestricted vouchers, and a control group. The key findings of the program are detailed in Chetty et al. (2016): children whose families moved to a low-poverty census tract upon receiving experimental vouchers fared better than those who resided in public housing or project-based Section 8 housing located in a high-poverty neighborhood. The effectiveness of housing voucher subsidies is most prominent when an individual is exposed to low-poverty housing at an early age. Chetty's results indicate that effective affordable housing initiatives may share the following characteristics: they target families with young children and provide an additional push to relocate to affordable, high-opportunity areas.

The second approach to poverty deconcentration involves a place-based *redevelopment* strategy aimed at improving the physical, social, structural, and economic conditions of the places where public housing tenants reside. This approach is primarily implemented through the eradication and renovation of distressed public housing into mixed-income developments that integrate a range of incomes. The demolition stage of this process requires the complete relocation of original residents to make way for the construction of revitalized housing.

Within the new developments, some units are set aside for public housing residents, some are priced affordably with the use of subsidies, and some are available at market rates (Chaskin and Joseph, 2015). An example of such an initiative that follows this approach is the HOPE VI program (1992), whose community-level externalities will be the main subject of this thesis.

2.4 The HOPE VI Program

In 1992, Congress authorized $300 million to create the Housing Opportunities for People Everywhere (HOPE VI) program, which intended to rebuild the most physically distressed public housing in the country by moving the foci of public housing policies away from project-based assistance and toward the promotion of mixed-income housing complexes and the use of housing subsidies to deconcentrate pockets of intense poverty.

HOPE VI entailed not only the relocation of residents in distressed housing to neighborhoods with (supposedly) improved conditions but also involved the complete demolition of the original housing structure to facilitate newly established low-density mixed-income replacement projects (Goetz, 2010). When an existing building is demolished, residents are typically offered the choice of a portable voucher to relocate within the private housing market or settle into another public affordable housing community; alternatively, they are offered to return to the revitalized reconstruction site upon the completion of several screening tests (Cunningham, 2004; Popkin, 2002).

Since the inception of the HOPE VI program, there have been a total of 262 revitalization grants awarded between fiscal years 1993 and 2010, totaling approximately $6.2 billion nationally. In Chicago, from 1993 through 2006, the HOPE VI program awarded the Chicago Housing Authority a total of $258M in revitalization grants and $83.4M in demolition, making the CHA the largest grantee out of all local housing agencies in the United States. The demolition grants involved a total of 22,681 residential public housing units out of the existing stock (HUD, 2007). Most of these housing establishments are aging and dilapidated high-rise buildings on Chicago's West and South sides (see Figure 1). Under HOPE VI, most units were to be replaced by low-rise, scattered-site, and mixed-income residential developments.

Some of the stated objectives of HOPE VI are to reduce poverty concentration, encourage self-sufficiency among residents, and provide supportive services and communities for low-income tenants (HUD, 2007). The rationale behind the program is derived from William Julius Wilson's concepts of social isolation and concentration effects, as outlined in his book *the Truly Disadvantaged*. Wilson contends that the lack of sustained social contact between low-income residents and individuals and institutions of mainstream society, as well as being immersed in a social atmosphere comprised mostly of individuals in the same socio-economic status, are the primary causes of poor social, psychological, and material well-being in communities that experience concentrated poverty and are subjected to segregation (Wilson, 1987). Considering these points, it seems that the most direct policy presciptions would be to address high social isolation and poverty concentration by increasing exposure to middle or working-class families and by promoting spatial deconcentration and dispersion.

However, HOPE VI's implementation likely does not address existing structural segregation and high concentration of poverty, as demolishing housing alone cannot solve the systemic issues deeply rooted in social and economic systems of urban landscapes. Further, according to Kost (2012), the mixed-income format of HOPE VI is likely conflicted with its promise of integration. The income-based admission policy for refurbished housing often leads to the exclusion of the most underprivileged tenants, while the incorporation of market-rate units reduces the already limited affordable housing stock, resulting in the reaffirmation of racial and economic segregation. By 2003, the HOPE VI program had displaced around 49,000 residents through its demolition and revitalization grants (United States General Accounting Office, 2003), and less than 12% of those displaced from old housing eventually move into the replacement housing (Goetz, 2014).

Scholars have also questioned the program's true intentions. The benefits of HOPE VI appear to be felt primarily by private investors, developers, and managers who have been recruited to finance and reconstruct HOPE VI projects, as well as by residents of surrounding areas whose property values have appreciated due to the systematic dispersal of public housing tenants away from demolition sites (Keene, 2011). According to Almagro, Chyn, and Stuart (2023), HOPE VI demolitions have resulted in socio-economically disparate impacts, generating substantial welfare gains for non-poor white households but welfare loss for low-income minority households. This appears to be driven by two mechanisms: changes in the racial compositions of targeted neighborhoods toward a larger share of white households and appreciation of rental prices that disproportionately benefits homeowners and harms renters.

HOPE VI has several key differences when compared to MTO. While MTO encourages voluntary mobility among low-income tenants seeking better opportunities, HOPE VI mandates the relocation of impoverished individuals to neighborhoods that may not necessarily provide improved conditions. Furthermore, MTO was put to an end two years after its initiation, while HOPE VI thrived for two decades due to the political influence of those who benefited from supplying these developments. The resistance of white middle-income communities toward the in-migration of public housing tenants with experimental vouchers likely contributed to the termination of MTO, while the support that HOPE VI received primarily came from non-poor constituents (Goetz, 2014; Ellickson, 2010).

2.5 Individual and Neighborhood Outcomes

One open question raised by HOPE VI's income-mixing is as follows: what has happened to low-income public housing residents excluded from new mixed-income developments in favor of higher income individuals?

Previous studies have examined these outcomes on an individual level. Jacob (2004) finds that demolishing high-rise public housing and providing households the option of using housing vouchers to relocate to different neighborhoods do not necessarily produce better or worse educational outcomes in children who belong to households affected by the demolitions. This observation is explained by poor relocation outcomes; children who were driven out by the demolition usually fare not much better than their disadvantaged, untreated peers: they reside in areas with high poverty and enroll in schools of suboptimal quality. Chyn (2018) examines the long-run outcomes of moving out of disadvantaged neighborhoods by creating a sample of displaced and non-displaced households and exploits public housing demolition as an exogenous factor as it only applies to a limited number of buildings in the same location. His results show that relocating children of any age from public housing into communities with improved social and economic conditions significantly benefits them. Other evaluations of relocation in Chicago and Seattle have reported improvements in the well-being of former public housing tenants. These residents generally have expressed satisfaction with their new neighborhood conditions, including better services and amenities (Popkin and Cunningham, 2002; Garshick, Kleit, and Carlson, 2003). However, it is worth acknowledging that these residents' original housing conditions prior to relocation were often poor. Therefore, while these positive results are encouraging, they should not be viewed as exceptional or surprising.

Performing a longitudinal study similar to those mentioned above would require quantifying each relocatee's wellbeing before and after the demolitions of their residency, as well as the economic and social conditions of their new neighborhoods. This makes conducting a tracking study logistically challenging. And as such, my ability to discern individual outcomes is constrained by the lack of availability of such granular data. I instead, turn to exploring changes in neighborhood conditions following HOPE VI. Specifically, I analyze the demolition patterns in Chicago to address the following question: How does the clearance of large high-poverty public housing under HOPE VI influence patterns of crime in and around that housing development?

There are several upsides to using tract-level crime rates as outcomes. First, unlike census data on neighborhood characteristics such as racial or ethnic compositions, educational attainment, or rates of employment which are only available every ten years, crime statistics are available on a daily basis. This provides more variation and greater degrees of freedom in the analysis, allowing for the capture of dynamic changes in crime patterns over time and the investigation of the short-term and long-term effects of demolitions. Additionally, crime is a critical public policy concern and one that is highly relevant to the success of HOPE VI and other place-based initiatives. Using crime as the outcome measure allows for a direct assessment of the effectiveness of these programs in improving neighborhood safety and security. However, it is important to note that the mechanisms and interpretation of the linkage between public housing demolitions and crime statistics as recorded by law enforcement is heavily contested, which I will briefly discuss in the next section.

2.6 Community Cohesion, Crime, and Policing

The expected relationship between high concentration of poverty and crime has historically been motivated by the linkage between the structural and physical conditions of concentrated disadvantage and neighborhood instability. Shaw and McKay's (1942) research suggests that the physical deterioration of residential housing structures often contributes to stronger social disorganization, exacerbating delinquency in the surrounding geographical space. Sampson and Raudenbush (1999) finds that the degree of collective efficacy, or the social control of public space, is negatively associated with the level of violent crime. Assuming that these theoretical assumptions hold true, it is reasonable to expect that the HOPE VI program, with its stated goal of reducing economic deprivation in residential areas by altering the structural circumstances of public housing, could potentially strengthen the collective power of public housing residents to maintain adequate social controls in urban residential enclaves and, in turn, mitigate crime.

However, attributing community order and safety solely to the collective efficacy of its residents is highly problematic, and historically, this notion has been used to justify law enforcement's discriminatory practices such as over-policing and mass incarceration. Wilson and Kelling's (1982) "broken windows" theory is a prime example, which argues that visible signs of disorder and neglect in a neighborhood, such as those seen in public housing, can encourage criminal activities to fester. This theory later became the basis for "zero-tolerance" policing, where minor offenses like loitering, graffiti, or public intoxication are heavily penalized to prevent major offenses (The New Yorker, 2018). Such practices are often carried out through heightened surveillance, increased frequency of "stops and frisks," and more arrests, particularly in neighborhoods with predominantly Black and Brown residents, and fueled by implicit and explicit bias that perceives members of minority groups as criminals regardless of their actions (Holmes and Smith, 2008). The epidemic of racist policing perpetuates a history of police brutality and corruption, further eroding trust between law enforcement and public housing residents, whose safety and livelihood are threatened on a daily basis.[3]

Overall, the use of historical crime data from local law enforcement agencies to evaluate the impact of HOPE VI demolitions is limited and should be approached with caution, due to the heavy biases in policing and reporting, as well as the subjective decisions of individuals to report crimes. Therefore, it should by no means be interpreted as a definitive or complete measure of public safety. However, despite these limitations, historical crime data has been commonly used to inform the understanding of place-based housing policies and their influence on crime levels.

A study of five project sites in Milwaukee, Wisconsin, and Washington, D.C., indicates a linkage between housing demolition and crime reduction in surrounding areas (Cahill, 2011). Further, this positive effect lasts for at least one or two years beyond the intervention period and is diffused as the distance from the target site increases (Cahill, 2011). Turbov and Piper (2005) also find a general decline in reported criminality in three project sites located in Atlanta, GA; Louisville, KY; and Pittsburgh, PA, following public housing demolition. In the early 2010s, demolitions in Buffalo, N.Y., induced a drop in reported crime at buffers surrounding project sites (Wheeler, 2018). These findings suggest that HOPE VI demolitions may influence patterns of crimes both spatially and temporally. In this thesis, I examine the effects of HOPE VI demolitions by leveraging the variation in the timing of census tract-level applications and accounts for spillover effects from geographical entities to their neighbors. I also analyze the effects by length of exposure and by baseline characteristics of the neighborhoods.

3. In Chicago, police misconduct is a widespread issue that contributes to a significant number of documented use-of-force complaints and arrests in minority communities, particularly in public housing projects (Jain, 2022). This problem was exemplified by the largest exoneration in Chicago's history, which occurred in 2013 when 226 convictions were vacated. Former CPD sergeant Ronald Watts was personally responsible for all of these wrongful convictions.

3 Data

To identify the implementation of HOPE VI public housing demolition over time, I use a list of project sites provided by the Chicago Housing Authority (CHA) which includes completion dates, addresses, and the number/type of units demolished in the city of Chicago between 1995 and 2010. In order to determine the precise location of demolition sites, I combine this dataset with a shapefile layer of census tract boundaries (standardized to the 2010 Census) extracted from the Chicago Data Portal using ArcGIS.

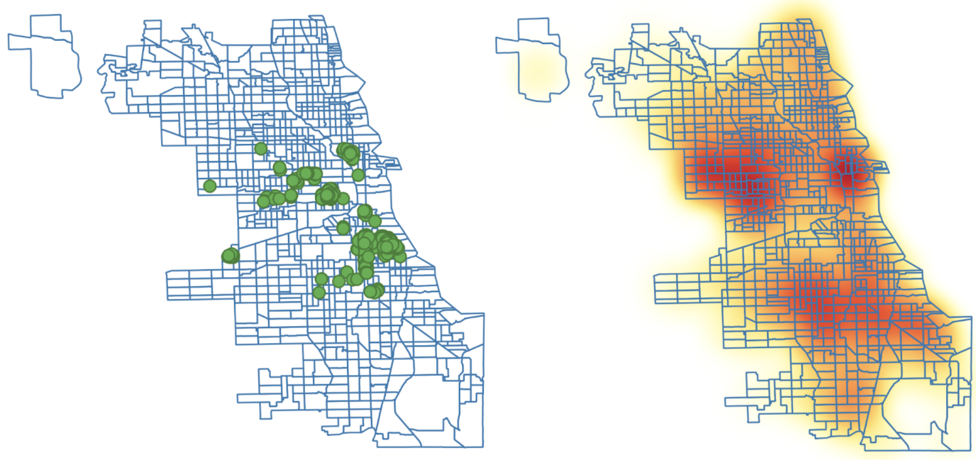
The longitudinal data of reported incident-level crime from 2001 to 2015 is obtained from the Chicago Police Department's CLEAR (Citizen Law Enforcement Analysis and Reporting) system. The dataset includes the longitude and latitude coordinates, the redacted address, the community area, and date-time information of each occurrence of reported crime, as well as the Illinois Uniform Crime Reporting Code (IUCR), which is directly linked to the primary type and description of the incidence. I merge this layer spatially with the shapefile and cross-reference the entries to census tract-level demolitions. Figure 1 displays the location of the 33 public housing projects demolished between 1995 and 2010 and the heatmap of reported crime between 2001 and 2022.

To estimate the treatment effect in the decade of 2000s, I restrict my analysis to observations contained in post-millennial years. Additionally, because of the differential in characteristics between tracts containing high-rise public housing and those without, I exclude neighborhood areas with no public housing structures within their boundaries from my sample. This reduces the number of tracts in my sample from 801 to 740. The list of neighborhood areas with established public housing is obtained from the City of Chicago Data Portal. Baseline covariates are collected from two sources: the Federal Financial Institutions

Examination Council's Online Census Data System (FFIEC) and the IPUMS National Historical Geographic Information System (NHGIS). Population data by-year from the NHGIS time-series are already standardized to 2010 census tract boundaries pre-extraction. Demographic, income, and housing characteristic data for the baseline year of 2000 come from FFIEC reports and are manually standardized to 2010 census tract boundaries in ArcGIS.

Summary statistics for tract baseline characteristics are provided in Table 1. Notable differences in tract characteristics between the treated and never-treated groups exist. Treated tracts have a significantly lower (29.6 percentage points of mean difference) median family income than never-treated tracts, and they also have a significantly higher proportion of the population below the poverty line (on average, 50.42% compared to 22.82% for untreated census tracts). While 37% of residential units in untreated tracts are occupied by owners, only 15.54% of housing units in demolished tracts are occupied by owners. This observation is in line with my initial conjecture that HOPE VI occurs in urban areas where majority of tenants are not property owners themselves. Furthermore, Black population in control tracts is, on average, 39 percentage points lower than in treated tracts, while Hispanic identifying population has a higher presence in these comparison groups. Overall, census tracts that undergo housing demolitions have a high concentration of people of color, as the percentage of minority population is 91.45%. Contrasting demographic compositions and economic disparities as seen in Table 1 should not be ignored as they likely have an impact on the crime rates observed during the decade of HOPE VI applications. I will address this baseline heterogeneity in a later section.

Figure 1: Location of demolished housing 1995-2010; Crime Heatmap 2001-2022



Source: Chicago Police Department Citizen Law Enforcement Analysis and Reporting; Chicago Housing Authority

Table 1: Baseline Data

	(1) Treatment	(2) Control	(3) Difference
Percentage of Population below Poverty Line	50.42	22.82	-27.60*** (3.17)
Median Family Income as Percentage of State Median	43.51	73.11	29.60*** (6.80)
Percentage Black	77.63	38.63	-39.00*** (7.37)
Percentage Hispanic	10.93	19.64	8.71* (4.43)
Percentage Asian	2.77	3.66	0.89 (1.33)
Percentage White	8.55	37.75	29.20*** (5.88)
Percentage Minority	91.45	61.63	-29.82*** (5.94)
Percentage of Housing Units Occupied by Owner	15.54	37.00	21.46*** (3.92)
Percentage of Housing Units Occupied by Family	48.98	66.29	17.31*** (5.09)
Observations	36	704	740

Notes: Table 1 presents the estimates of mean values for observed covariates for the panel sample in the baseline period (2000) accompanied by mean difference estimates. Sample is restricted to census tracts contained in neighborhood areas with public housing establishments. Standard errors are in parentheses.
* p<0.1, ** p<0.05, *** p<0.01.

4 Empirical Strategy

The aim of this paper's empirical work is to examine the impact of HOPE VI housing demolitions on specific groups of census tracts, as well as potential spillover effects on neighboring tracts. To accomplish this, a generalized difference-in-differences analysis is performed using a two-way fixed-effects approach. Additionally, the study investigates whether there is a diffusion process at play by including a spatial cross-regressive term of demolitions in neighboring areas to assess how demolitions in nearby entities affect the census tract in question. Lastly, an event study framework is adopted to chart the dynamic effects of the intervention over time.

4.1 Difference in Differences

4.1.1 Two-Way Fixed Effects

My initial methodology leverages the variation in the timing of demolition across census tracts through a two-way fixed-effects regression model, represented by the following equation:

$$Y_{iym} = \alpha_i + \lambda_y + \mu_m + \beta^{\mathrm{TWFE}}(Demo_{iym}) + \epsilon_{iym} \tag{1}$$

where $Demo_{iym}$ is the dummy variable indicating whether a demolition has happened in census tract i by year y and month m. α_i, λ_y, and μ_m are unit and time fixed effects which lessen the timing and treatment restrictions of the two-period, two-cohort DiD specification. Specifically, α_i absorbs any unobservable time-invariant characteristics that are specific to each tract, while λ_y and μ_m control for any common year-to-year transitory shock felt by all tracts and month-to-month seasonality in the outcome variable, respectively. To account for any potential correlation within census tracts, the standard errors are clustered at tract level. The coefficient of interest is β^{TWFE}.

Equation (1) assumes that the demographic characteristics are unchanging within a census tract over time. It allows for some degree of flexibility which is advantageous compared to an OLS regression with multiple controls, as the fixed-effects absorb relevant covariates that either vary by tract or by time and not both. Nevertheless, factors which influence crime rate may vary within an entity over time, confounding the estimates of the census tract effects. Further, if these changing factors are correlated with the adoption of demolitions across tracts and do not vary uniformly city-wide, then the estimate of coefficient on $Demo_{iym}$ will be biased. Imposing a unit-specific time trend mitigates these concerns:

$$Y_{iym} = \alpha_i + \lambda_y + \mu_m + \beta^{\text{TWFE}}(Demo_{iym}) + \delta_i \cdot t + \epsilon_{iym} \tag{2}$$

In equation (2), unit-specific linear time trend $\delta_i \cdot t$ is generated by multiplying tract-specific coefficient with time trend t. By allowing slopes of tract-level monthly crime rates to vary across entities, I control for heterogeneity in outcome behaviors and identify the impact of HOPE VI demolitions as deviations from the expected linear trajectories in the crime rates.

Typically, in a traditional two-group two-period DiD model, the coefficient on the interaction between treatment and post is interpreted as the treatment effect, assuming the parallel trends assumption is met. However, the interpretation of the TWFE estimator β^{TWFE} with varying treatment timing in equation (1) and (2) is complicated by several factors. According to Goodman-Bacon (2019), this estimator is in fact a weighted average of all possible individual 2×2 DiD estimators in the data. As the implementation of HOPE VI demolitions varies from tract to tract, my estimation approach relies upon the following identifying assumptions:

(i) Parallel trends: in the absence of structural demolitions of HOPE VI, both treatment and control groups would have experienced the same outcome evolution in crime. Notice that the inclusion of tract-specific linear time trend weakens this condition as it allows for differences in the outcome variable between the treatment and counterfactual groups to vary by a linear factor in the absence of housing interventions. (ii) Treatment assignment is exogenous: conditional on the observable covariates, any differences in crime rate between the treated and never-treated groups are solely due to the implementations of demolitions and not due to other factors that are correlated with treatment assignment. (iii) Homogeneous treatment effects: the effect of intervention is constant across all cohorts (i.e., all census tracts within neighborhoods containing established public housing) and across time periods. (Goodman-Bacon, 2021; L.Sun and Abraham, 2021; De Chaisemartin and D'Haultfœuille, 2020) I will examine assumptions (i)-(iii) in the result section.

4.1.2 DiD with Spatially Lagged Indicator

Additionally, there are reasons to suspect the presence of spatial spillover effects of HOPE VI demolitions on crime. The small geographical scale of census tracts makes it conceivable that the out-migration patterns of relocated residents from their original demolished residence may affect crime rates in neighboring tracts. Kingsley and Pettit (2003) provide evidence to support this claim, finding that the median distance of relocation from HOPE VI sites to new residences in the City of Chicago was 4.2 miles, with an average of 5.3 miles. The diffusion of treatment effects from demolished census tracts to nearby areas can be examined via a spatially lagged explanatory variable ($W\mathbf{X}$), which is used to capture the weighted sum of values for neighborhood i by using its local neighbors as weights. (Anselin, 2002; Florax and Folmer, 1992) Specifically:

$$[W\mathbf{X}]_i = \sum_{j \neq i} w_{ij} \mathbf{X}_j \tag{3}$$

Since my panel data takes on a structure with cross-sectional entities and time-series periods, the spatial lag in (3) can be incorporated into estimation equation (2) by the following form:

$$Y_{iym} = \alpha_i + \lambda_y + \mu_m + \beta_1^{\text{TWFE}}(Demo_{iym}) + \beta_2^{Spatial} \sum_{j \neq i} w_{ij}(Demo_{jym}) + \delta_i \cdot t + \epsilon_{iym} \tag{4}$$

The term $\sum_{j=1}^{N} w_{ij}(Demo_{jym})$ represents the interaction effect of the spatial relationship between tract i and tract j, multiplied by the demolition status for neighboring jurisdiction j at year y and month m. To construct the binary spatial weights matrix, I consider a census tract j to be a neighbor of census tract i if it falls into the ten nearest entities of census tract i. The centroids of each tract are obtained from the shapefile provided by the Chicago Data Portal, and each pair is matched via distance mapping. In equation (4), the coefficients of interest include β_1^{TWFE} and $\beta_2^{Spatial}$, which is the average spillover effects of tract-level demolition applied to a closest neighbor.

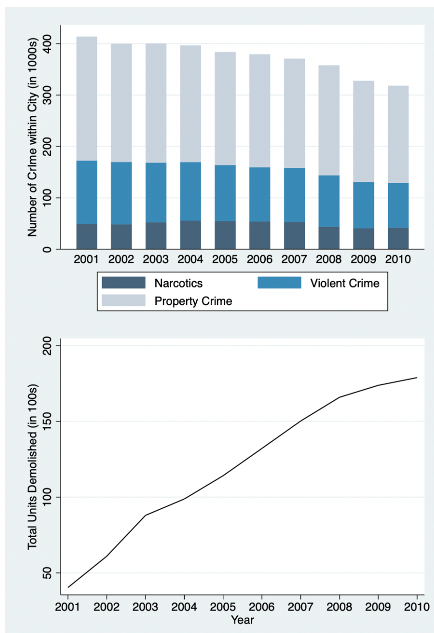
4.2 Event-Study Design

While the TWFE formulation in equation (1) and (2) utilizes the timing variation of the treatment, it fails to account for the lead and lag effects of public housing demolitions. Specifically, it assumes uniformity of the treatment effect at year y and month m, irrespective of how many periods prior that the demolition occurs. Given that there is likely a delay between the time of implementation and when an effect is realized, I map out the full dynamic response of the crime rates to the housing demolitions using an event study design:

$$Y_{i,y} = \alpha_i + \lambda_y + \sum_{k=-5}^{-2} \theta_k D_{i,y}^k + \sum_{k=0}^{10} \beta_k D_{i,y}^k + \epsilon_{i,y} \tag{5}$$

Where $Y_{i,y}$ is the outcome variable in the census tract i in year y. α_i and λ_y are census tract and year fixed effects, respectively. $D^k_{i,y} := \mathbf{1}\{y - E_i = k\}$ is the relative period indicator for being k years away from event E_i ($k = -1$ or the period immediately before the treatment is excluded to avoid collinearity). This means that, for running variable $k = 1$, the expression $D^k_{i,y} := \mathbf{1}\{y - E_i = k\}$ will be equal to 1 in year 2001 for an event E_i that occurs in 2000 for tract i, and will be equal to zero for all other time periods. Coefficients on lead periods θ_k's examine pre-demolition outcome behaviors. Coefficients β_k's on lags are interpreted as the average treatment effect at different lengths of exposure to the first tract-level demolition.[4]

Figure 2: City-wide Crime and Total Units Demolished 2001-2010



Notes: Figure 2 shows city-wide crime rate per 1000 people per year by type of crime as categorized by Illinois Uniform Crime Reporting Codes and the total number of units demolished in 100s, 2001-2010. *Source:* Chicago Police Department Citizen Law Enforcement Analysis and Reporting; Chicago Housing Authority.

4. That is, assuming parallel trends and homogeneous treatment effects across groups. Otherwise, this parameter describes all the weighted average of all possible pairwise DiD estimators in the data (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021).

5 Results

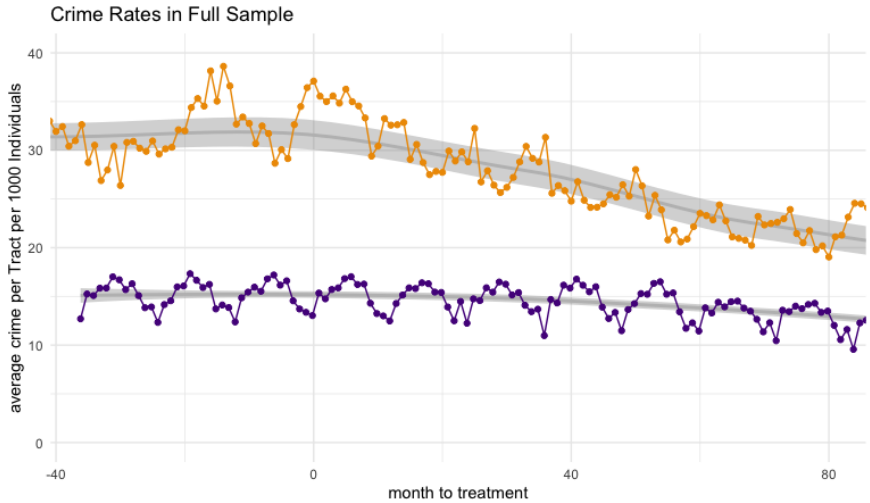
5.1 Preliminary Results and Parallel Trends

As shown in Figure 2, the overall reported crime exhibits a downward trend in Chicago during the 2001 to 2010 time period; this happens simultaneously as the cumulative number of demolition units in the city grows. However, the correlation between public housing demolitions and crime reduction does not necessarily indicate a causal relationship. Factors such as changes in political leadership, racial and ethnic makeup, or incarceration rates may contribute to the changes in crime in Chicago during the period in which public housing was being demolished and could potentially contribute to a spurious correlation between public housing demolition and crime. My TWFE estimation equation, compared to an OLS regression, is more advantageous since it partially mimics the effect of random assignment to treatment and control groups and establishes a treatment effect as the difference between the observed outcomes in the two groups. The inclusion of a linear time trend captures tract-level unobserved characteristics that influence crime rates, with the slopes of the trends allowed to vary across tracts.

However, pre-existing differences over time between the treated and never-treated groups are still a concern for the validity of my estimation as any changes in trend differentials not absorbed by tract-specific slopes over the pre-treated periods could indicate a threat to identification, undermining the confidence with which we can attribute the change in crime solely to the housing demolitions. To address this issue, I assess the parallel pre-trend assumption, which posits that in the absence of treatment, the outcome variable would have evolved similarly for all cohorts.

Figure 3 assesses the parallel trends assumption, presenting the average monthly per-

Figure 3: Per Thousand Crime Rate by Month to First Treatment



Notes: This figure depicts the monthly crime rate per thousand people per tract, covering a period of approximately 40 months prior to the first demolition (or prior to the average timing of first demolition in the treated for my comparison group) to 80 months after. The treatment group of my sample is limited to observations where demolitions occurred between 2000 and 2010, and the crime rate data covers the period of 2001 to 2015.

Table 2: TWFE Regression Results of Demolitions on Overall Crime

	(1) Binary Indicator Only	(2) Two-way fixed effects	(3) With Linear Time trends
Panel A. No Spatial Lags			
Demolition	15.87**	-12.12***	-10.11*
	(5.319)	(3.529)	(5.618)
Observations	132,467	132,467	132,467
Adjusted R-squared	0.033	0.644	0.745
Panel B. with Spatially Lagged Explanatory Variable			
Demolition	14.18**	-11.21***	-9.581
	(5.668)	(3.657)	(6.193)
10*nn_status*	1.395**	-8.405	-7.100
	(0.608)	(6.245)	(6.900)
Observations	132,467	132,467	132,467
Adjusted R-squared	0.051	0.650	0.710
Tract FE		YES	YES
Year and Month FE		YES	YES
Unit-specific Linear Time-trend			YES

Signif. codes: *** p<0.01, ** p<0.05, * p<0.1

Notes: Table 2 reports the results of the TWFE regression analysis using monthly per thousand people crime rate at the census tract level from 2001 to 2015 as the outcome variable. Standard errors are in parentheses. Panel A includes no spatial lags, as specified in equations (1) and (2), and Panel B includes the spatially lagged neighboring demolition status variable to account for spatial spillover effects. The treatment groups are tracts within public-housing neighborhood areas that were treated under HOPE VI demolitions. It is assumed that demolition remains in effect after it is initiated for a census tract.

Table 3: TWFE Regression Results of Demolitions on Crime by Type of Offenses

	Violent Offenses		Property Crime		Narcotics	
	(1) TWFE	(2) Time Trends	(3) TWFE	(4) Time Trends	(5) TWFE	(6) Time Trends
Panel A. No Spatially Lags						
Demolition	-3.208***	-2.866***	-2.725***	-1.313**	-5.620**	-6.253
	(0.707)	(0.838)	(0.729)	(0.432)	(2.393)	(4.978)
Observations	127,196	127,196	132,113	132,113	97,252	97,252
Adjusted R-squared	0.671	0.733	0.691	0.694	0.414	0.555
Panel B. with Spatially Lagged Explanatory Variable						
Demolition	-2.712***	-2.053***	-2.654***	-0.830**	-5.178**	-6.098
	(0.736)	(0.793)	(0.825)	(0.416)	(2.481)	(4.849)
10*nn_status*	-1.864*	-1.367	-1.435	-0.532	-3.480	-2.818
	(0.736)	(0.793)	(1.381)	(1.121)	(2.723)	(2.498)
Observations	127,196	127,196	132,113	132,113	97,252	97,252
Adjusted R-squared	0.672	0.733	0.697	0.738	0.418	0.550
Tract FE	YES	YES	YES	YES	YES	YES
Year and Month FE	YES	YES	YES	YES	YES	YES
Unit-specific Linear Time-trend		YES		YES		YES

Signif. codes: *** p<0.01, ** p<0.05, * p<0.1

Notes: Table 3 presents the effects of housing demolitions on reported crime rates per thousand people, broken down by type of offenses using Illinois Uniform Crime Reporting (IUCR) codes, which are four-digit codes used by law enforcement agencies to classify incidents. Panel A and Panel B are described the same way as in Table 2. Standard errors are clustered by census tracts and presented in parentheses.

thousand people crime rate in treated and control groups from approximately three years prior to the adoption of first demolition up to five years after. The plot shows the average crime rates for the treatment group plotted around zero, representing the time of first demolition in individual tracts. In contrast, the untreated group's crime rates are plotted around the average demolition time in the treated tracts, which occurs in 2003 for the full sample. Upon initial examination, the temporal patterns of the outcome variable show a parallel and consistent trend between the treatment and control groups prior to the intervention. The per-capita crime rate experiences a decline in the subsequent periods after the first wave of housing demolition at the census tract level, with consistent higher crime rates observed during the summer months. To assess the validity of these observations and plausibly establish causality, an event study design is employed to test for any deviations from parallel trends.

5.2 Main Results

5.2.1 Two-way Fixed Effects

Table 2 presents my main findings from the two-way fixed effects regressions. The results show that HOPE VI housing demolitions have a significant and negative impact on tract-level crime rates per thousand people, based on my specifications. The coefficient on the *Demolition* indicator variable varies depending on whether a tract-specific time trend is included. In Panel A, column (1), the analysis is conducted without including any tract or year and month fixed-effects. The estimated coefficient on *Demolition* is statistically significant at 15.87 ($p < 0.01$), but subject to strong bias. This is because tracts that are touched by demolitions typically have higher reported crime rates to begin with, and the binary indicator of demolition status, which only captures whether a tract has undergone a housing demolition or not, does not adequately control for potential unobserved time-invariant or time-varying confounding factors that affect both the decision of building clearance and the crime rate in a census tract.

In comparison, model (2) and (3) more appropriately address this selection bias by including tract and time fixed effects, thereby plausibly estimating an unbiased causal treatment effect of housing demolitions on reported crime. In column (2), adding tract, year and month fixed-effects yields a TWFE estimate of -12.12 ($p < 0.01$) crimes per thousand people and increases the adjusted R-squared value to 0.644. In 2001, the average crime rate in treated tracts was 62.51 incidents per thousand tract population per month. By the end of my panel in 2015, this value decreased to approximately 20.89. Notably, the HOPE VI housing demolitions alone account for 29% of this 41.62 point decrease, indicating a substantial contribution of the demolitions on reported crime rate trajectories. In model (3), the incorporation of tract-specific linear time trends further improves the explanatory power of the regression to 0.745. A coefficient of -10.11 ($p < 0.1$) suggests that after allowing for outcome trajectory over time to take on tract-specific slopes, the application of first tract-level demolition decreases the overall crime rate by 10.11 per thousand individuals on a monthly basis. This is again a considerable reduction. Sizing both estimates in column (2) and (3) relative to the average tract-level monthly crime rate of 31.26 incidents per thousand individuals in the treated tracts and 14.00 in the never-treated tracts, we see that the decrease in crime rate attributable to demolitions explains a drop that is almost as large as three-quarters of the average reported crime rates in the counterfactual. This relationship is illustrated in Figure 3.

The effects of HOPE VI demolitions on different types of offenses are presented in Table 3. Index crimes are categorized into several types based on the Illinois Uniform Crime Reporting (IUCR) Codes, with the primary types including violent offenses (predominantly consisting of battery and assault), property crime (burglary, robbery, arson, theft, etc.), and narcotics (or all drug-related offenses). The effect of demolitions on violent crime is -3.208 ($p < 0.01$) in the fixed effects only model, and -2.866 ($p < 0.01$) when a linear time trend is added. These values are sizable when compared to the average per-thousand people violent crime rates of 8.18 per month in treated tracts and 3.81 in never-treated. The reduction effects with regard to property crime are lower than those of violent offenses, with a 1.31 ($p > 0.01$) point decrease in per thousand crime rates attributable to HOPE VI demolitions, controlling for tract-specific linear time trends. The highest magnitude of treatment effect is observed in the dependent variable of drug-related criminal activities. Controlling for tract, year, and month effects, HOPE VI housing demolitions decrease monthly drug-related crime rate by 5.620 ($p < 0.05$) incidents per thousand tract population. Across Table 3, it is observed that when allowing for tract-specific slopes over time, the housing demolitions of HOPE VI is responsible for 27.11% of the 10.57 point decrease in violent offenses, 12.05% of the 10.89 point decrease in property crime, and 34.41% of the 18.17 point decrease in drug-related crime (although only suggestive) between 2001 and 2015, in census tracts to which demolitions have applied.

5.2.2 DiD with Spatially Lagged Indicator

As discussed in section 4.1.2, there may be potential spatial spillover effects from a census tract to its neighboring entities. To account for this, I include in my specifications an additional term of $10nn\_status$, which represents the aggregated demolition status in a tract's ten closest neighbors.

Panel B of Table 2 reveals that the inclusion of a spatially lagged explanatory variable ($10nn\_status$) slightly attenuates the impact of a census tract's own demolitions on its overall crime, implying that diffusion from nearby tracts is potentially occurring. Although the average treatment effect of demolitions in a nearest neighboring tract is lower than the effect of a tract's own demolitions and statistically insignificant, it is still negative and substantial in magnitude. In column (2), after incorporating entity and time fixed effects, the coefficient on *Demolition* indicates a statistically significant reduction of 11.21 overall reported crime rates per thousand people ($p < 0.01$). Furthermore, comparable findings are obtained when a tract-specific time trend is included, with somewhat lower average effects of demolishing within the tract and a considerable but non-inferential spillover effect from the ten nearest neighbors. Notably, at a p-value of 0.123, the impact of a tract's own demolitions is no longer significant in (3). The standard errors reported in panel B are also slightly higher than those in panel A. Although insignificant, we can express the magnitudes of the spatial coefficients in Panel B of Table 2 in terms of the coefficients on *Demolition*: In Models (2) and (3), the average effect size of demolitions in a closest neighbor is approximately 75% of the effect size of a tract's own demolition. Table 3 shows that the average impacts of a neighboring demolition on tract-level violent, property, and drug-related crime are negative and lower than the effects of a tract's own demolitions. However, these effects are not statistically distinguishable from chance, except for column (1) where neighboring demolitions have a marginally significant effect of -1.864 ($p < 0.1$). Nonetheless, these findings suggest that the influence of HOPE VI may extend beyond the treated tracts, which is consistent with previous research discussed in section 2.6.

The results in Tables 2 and 3 demonstrate that incorporating a unique linear trend-line for each tract can help mitigate the bias that may arise from omitted variables that are correlated with crime rate and vary over time. As economic and social conditions, along with law enforcement structures, vary within each tract and shape crime and policing patterns, imposing an intercept for each census tract and time period alone may misrepresent the underlying outcome trajectories and fail to capture the true variation in crime rates induced by HOPE VI demolitions. Therefore, allowing each tract to have its own linear trend-line provides a more accurate representation of the impact of demolitions on crime rates. Tract-to-tract spillovers are consistently suggested but not inferential as supported by my data.

5.3 Selection Bias and Propensity Score Matching

Recall that my previous sample is restricted to census tracts located within community areas containing established public housing projects. This is because using all other areas of Chicago as counterfactual for HOPE VI neighborhoods is likely inappropriate. The original site selection process for high-density high-poverty public housing establishments centered heavily around the objectives of keeping costs low and holding residential segregation firmly in place: almost all high-rise public housing in Chicago was built in neighborhoods closest to the downtown central business district or along a contiguous stretch of land extending directly south of downtown where the average income is relatively low, and the population predominantly consists of Black and Brown residents. Due to this incomparability between neighborhoods with and without affordable housing, I have excluded neighborhood areas with no public housing structures within their boundaries from my sample.

As shown in Table 1, significant differences still exist between the treatment and comparison tracts, even within this selected subset of public housing neighborhoods. Given these observations, we may be concerned about the validity of my identification if untreated census tracts in my sample still differ from treated tracts in ways that affect both their likelihood of receiving HOPE VI and their likelihood of experiencing changes in crime over time. To address baseline heterogeneity in economic and social characteristics, I use propensity score matching to select a set of control observations that closely resemble each treated observation (Rubin and Rosenbaum, 1984). It is worth noting that the preliminary assessment of the parallel trends assumption seems to hold, so propensity score matching is not desperately needed here, but it is nevertheless conducted as a robustness check.

I use the K-Nearest Neighbor matching algorithm with a ratio of 5:1 and a caliper of 0.2. This means that each treated unit is matched to five corresponding controls whose propensity score (i.e., the predicted probability of treatment assignment given the observed covariates), estimated using a logistic regression, is within a distance of 0.2 standard deviations away. The variables chosen for matching are percentage of tract population that are Black and Hispanic, tract median estimated family income in 2000 as a percentage of state median, percentage of tract population under poverty line, percentage of units occupied by owners, and percentage of 1-to-4 family units.[5] These variables are selected because they are statistically significant in determining both participation and attainment.

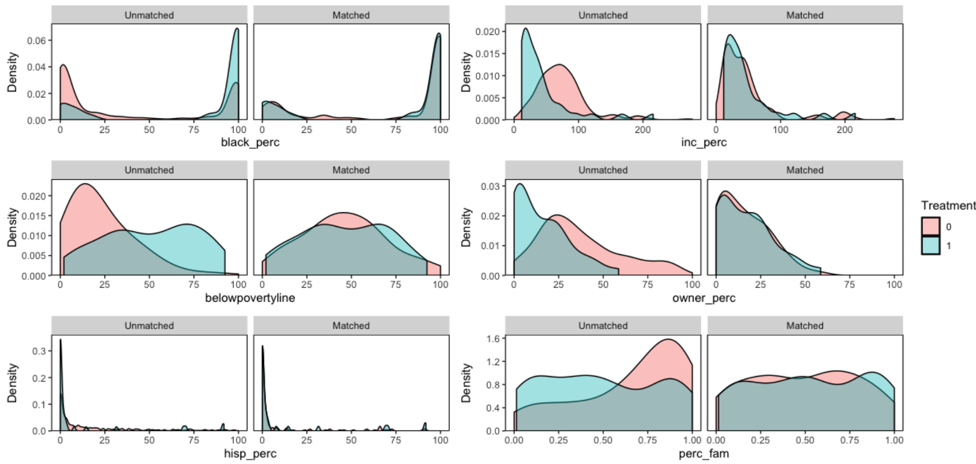
Out of 36 census tracts treated in 2000-2010, 33 are matched to a total of 130 control units. It is important to note that the validity of PSM relies on the conditional independence assumption, which states that there should be no systematic differences between the treatment and control groups in terms of the potential outcomes, conditional on the observed covariates. However, this condition is not verifiable since only the outcome that occurs under the assigned treatment or control condition is observable to the researcher.

To assess the quality of matching, I evaluate the balance of covariates and examine the propensity score distribution. The density plot of each baseline observable included in the matching process is displayed in Figure 4, with the x-axis representing the covariate values and the y-axis showing the density of the sample at that covariate value. Matching produced strong baseline interchangeability between the treated and control groups, as indicated by the substantial common support and the likeness in overall shapes and skewness of the covariate distributions. The standardized mean differences of all predicting baseline covariates fall within the 0.1 threshold, as shown in Figure 5. Additionally, Figure 6 displays comparable propensity score range and distribution among the matched units. These results provide greater confidence in the quality of my matching outcomes.

Using the matched dataset, I re-estimated the TWFE regression specified in equations (1) and (2), while also weighting observations by their respective propensity scores to account for potential biases caused by the matching process. The PS-adjusted TWFE estimators consistently suggest a negative effect of demolitions across both TWFE models, as shown

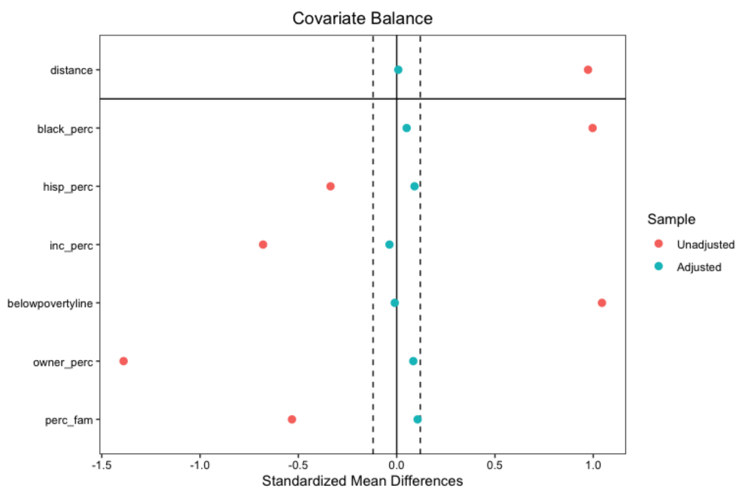
5. housing units that have less than 5 residences and are occupied by the property owner. This includes condominiums, townhouses, and single-family homes and excludes apartment buildings

Figure 4: Distributional Balance for Baseline Covariates



Notes: Figure 4 displays the distributional balance before and after matching for several baseline covariates (2000). They are: percentage of population Black, tract-level median family income as percentage of state median, percentage of tract population living below poverty line, percentage of housing units occupied by owners, percentage population non-white Hispanic, percentage of housing units with less than 5 residences and are occupied by the property owner.

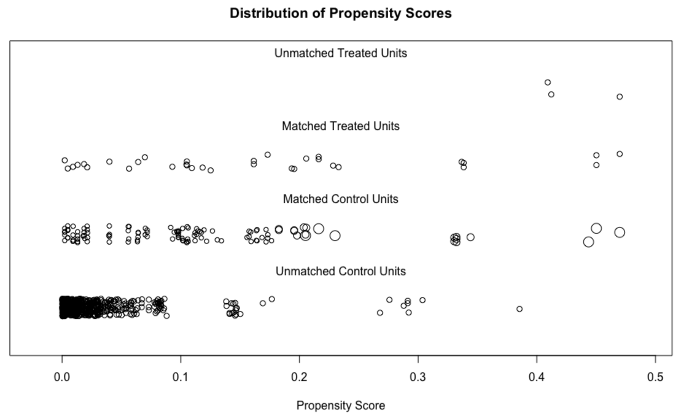
Figure 5: Standardized Mean Difference



Notes: Figure 5 is a "Love" plot graphically displaying covariate balance before and after adjusting.

in columns (2) and (3) of Table 4. However, it is worth noting that the magnitudes of the coefficients on *Demolition* are smaller in comparison to those in Table 2, at a value of -6.480 ($p < 0.01$) and -4.395 ($p < 0.1$). This reduction in size implies that using an alternative control group that is more similar to the treatment group, instead of using all census tracts in public housing neighborhood areas that do not experience demolitions as counterfactual,

Figure 6: Distribution of Propensity Score



Notes: Figure 6 visualizes the distribution of propensity scores before and after matching.

Table 4: TWFE Regression Results after Matching

	(1) Indicator Only	(2) Two-Way Fixed-Effects	(3) Linear Trends
Demolition	4.647	-6.480***	-4.395*
	(3.448)	(1.856)	(2.274)
Adjusted R-squared	0.010	0.608	0.674
Observations	26,994	26,994	26,994
Tract FE		YES	YES
Year and Month FE		YES	YES
Linear Time-trend			YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Table 4 presents my results from the two-way fixed-effects specifications in equation (1) and (2) using the propensity matching-restricted data set without spatial lags. The dependent variable is monthly per thousand people crime rate at census tract level.

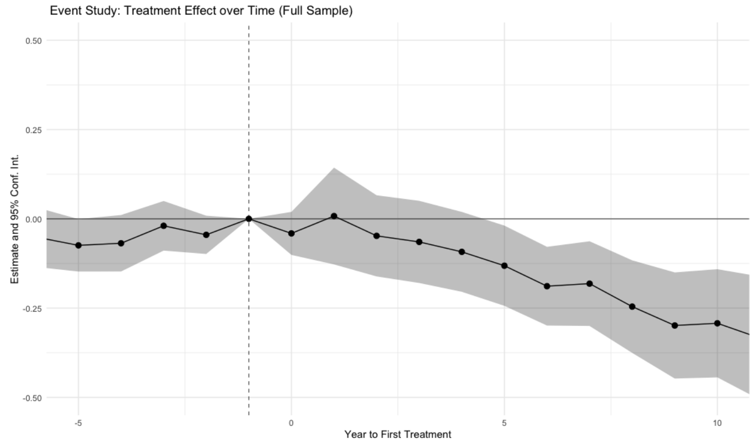
slightly alters the results detailed in section 5.2. However, the crime-reduction effects are still present.

5.4 Event Study Results

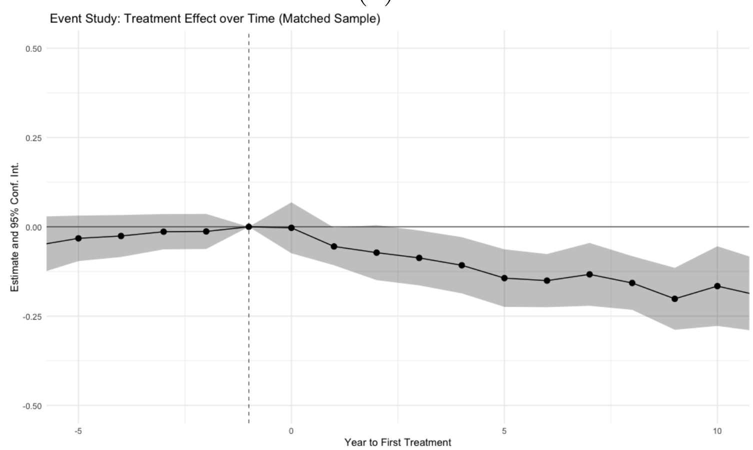
To better understand the dynamic effects of public housing demolitions on neighborhood crime rates, I conduct an event study analysis that examined how the impact of demolitions changed over time and whether there was evidence of a pre-trend.

To rule out anticipatory behavior prior to treatment or the causal effects of being treated

Figure 7: Event Study with Full Sample and Matched Sample



(a).



(b).

Notes: Figure 7 plots the event study estimates from the two-way fixed effects model, with the outcome variable being per-capita crime rates per year per census tract. The specification does not include any baseline controls and does not incorporate weights. Subfigure (a) illustrates the dynamic treatment effects using the full sample, while subfigure (b) uses the propensity-matching restricted panel.

in the future on current outcomes leading up to the demolition, the pre-treatment coefficients on the interaction term $D_{i,t}^k$ from equation (5) should be jointly indistinguishable from zero. As shown in Figure 7, all of the error bars on the β_k estimates for $k < 0$ contain the zero line, indicating little to no effect during the pre-treatment period.

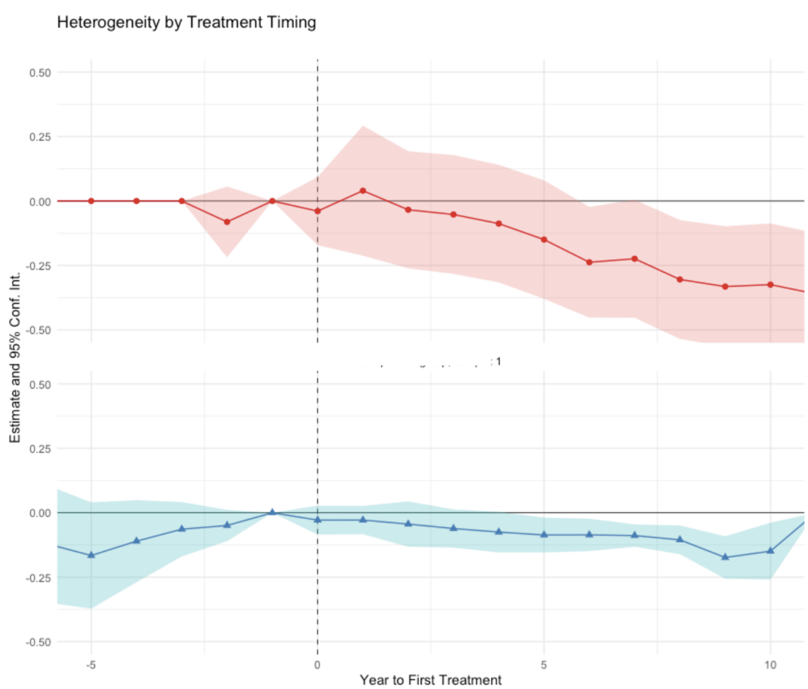
The dynamic effect of the treatment over time appears similar for both the full sample and the propensity-score matched panel during the post-demolition periods. As depicted in Figure 7, the crime-reduction effects of HOPE VI slightly increase in magnitude for the treated tracts as time goes by, and consistently remain negative for a decade after the first demolition. However, the immediate effect in the subsequent years that follow is statistically insignificant. Furthermore, Figure 7 indicates that tract-specific characteristics indeed determine some extent of the variation in the dependent variable and hence the magnitude of the effect of HOPE VI demolitions on crime rates over the long haul. This is inferred from stronger dynamic treatment effects in the full sample than in the matching-restricted sample. In the previous section, Figure 6 shows that the distribution of propensity scores in unmatched but treated tracts is higher than most of entities successfully matched to comparison groups. This suggests that the conditional probability of receiving a demolition in unmatched tracts, to some extent, explains how much the intervention is to affect crime rates dynamically.

5.5 Group-Time Average Effects

In this section, I state the danger of relying on traditional two-way fixed-effects regression models with leads and lags to conduct causal inference about treatment effect dynamics and apply the framework developed by Callaway and Sant'anna (2021) to engage with such limitations.

So far, I have outlined several key features of my research design. Firstly, it is important to note that not all census tracts are subject to demolitions, and those that remain untouched serve as the control group. Once a census tract is subject to housing demolitions, it remains treated for the remainder of the panel. Secondly, while it is reasonable to assume that the parallel trend assumption is met, it is also possible that this is conditional on a set of observable census tract characteristics. As shown in Figure 7, controlling for baseline covariates through propensity score matching results in coefficients on the interaction between time elapsed from first demolition and the binary demolition indicator jointly insignificant in pre-treatment periods. Finally, the dynamics of the treatment effect of HOPE VI housing demolition are likely heterogeneous across different treatment cohorts. This inference is based on conducting event study analysis for subgroups defined by whether the census tract is affected by HOPE VI demolitions before or after the mean intervention date. Based on Figure 8, it is apparent that the census tracts that underwent demolitions prior to June 2003 (i.e., early demolitions) exhibit coefficients on lagged variables that are notably different from those with treatment applied at a later date. Therefore, assuming a homogeneous treatment effect across cohorts is likely implausible.

Figure 8: Heterogeneous Treatment Effect by Timing of First Demolition



Notes: Figure 8 plots the dynamic treatment effects by timing of first demolition, with the top plot depicting early-treatment group and bottom plot depicting late-treatment group, separated by mean first-demolition date. Note that entities subjected to early demolition seem to be driving the overall results.

Under these assumptions, it is demonstrated that the two-way fixed effects estimator is likely to suffer from severe bias. (Callaway and Sant'anna, 2021; Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon 2021) The solution propsed by Callaway and Sant'anna involves aggregating *group-time average treatment effects* into summary measures of the causal effects. In other words, their framework allows for a unique estimate per cohort of units treated at the same point in time. Following the guidelines of their paper, I evaluate the average treatment effect for census tracts who are members of a particular group g at a particular time period t:

$$ATT(g,t) = \mathbb{E}[Y_t(g)] - Y_t(0)|G_g = 1] \tag{6}$$

I define groups by the time period when a tract first experience building clearance. For instance, $ATT(g = 2002, t = 2005)$ describes the average effect of participating in the treatment in year 2005 for a group that becomes treated in 2002. For simplicity, I also assume that for any $t < g$,

$$\mathbb{E}\left[Y_t(g) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) \mid X, G_g = 1\right] \tag{7}$$

which is equivalent to stating that for any period t prior to the first treatment, the expected outcome for group g is the same as it is for control units. In other words, the anticipation of structural demolition does not influence the crime rate in the census tracts treated at g, conditional on a set of time-invariant covariates X. The process of accounting for baseline observables is similar to the matching method in previous sections. However, Callaway and Sant'anna adopt a generalized propensity score, which is defined by

$$p_g(X) = P\left(G_g = 1 \mid X, G_g + C = 1\right) \tag{8}$$

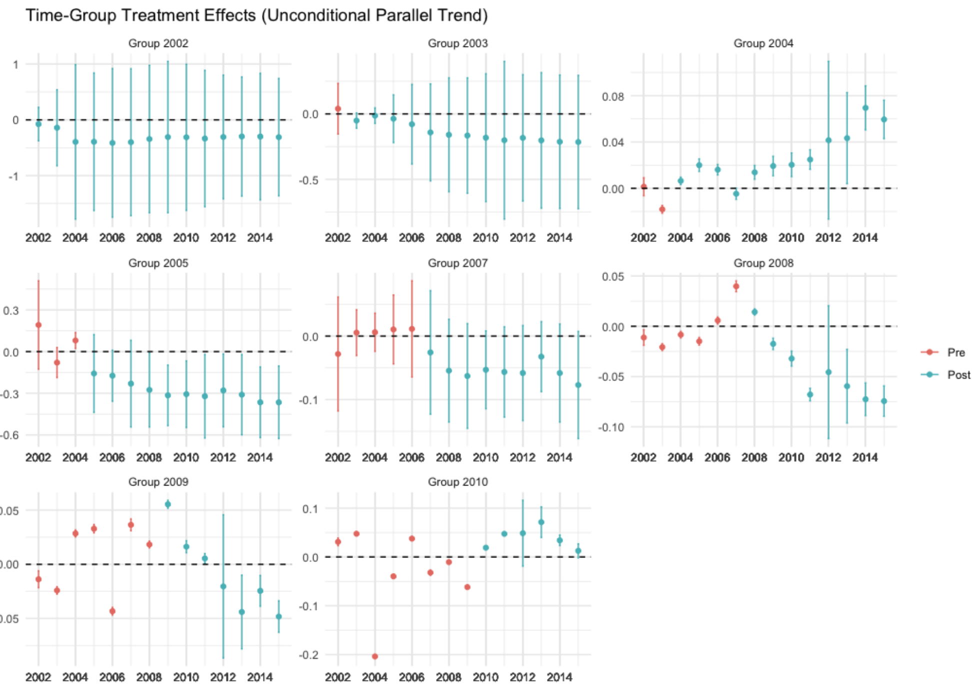
denoting the probability of a census tracts contained in group g, or being treated at time g, conditional on X. Represented by C is a binary variable indicating if a census tract is ever-treated in the panel. Hence, any tract in the sample either belongs in group $G_g = 1$ or is never-treated ($G_g + C = 1$). If the matching procedure is implemented correctly, conditional parallel trends hold:

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, C = 1\right] \tag{9}$$

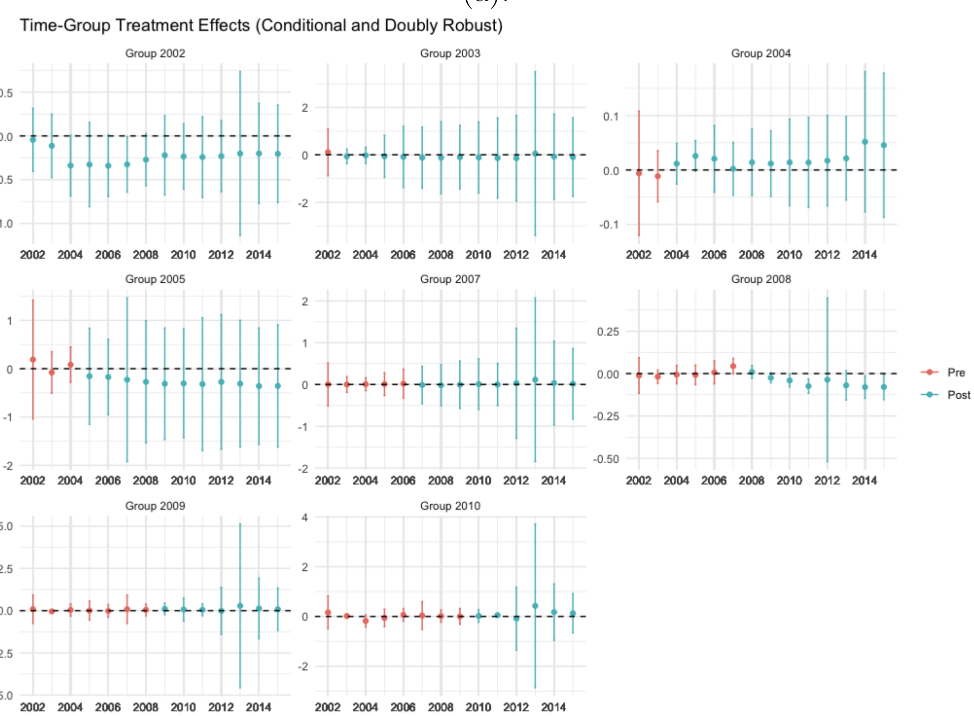
I proceed to compute the group-time average treatment effect in R using the Callaway and Sant'anna's *DiD* package. My results in this part of my analyses address the following set of questions: i). How does treatment effect differ based on the year in which the demolition occur? ii). What is the aggregated effect of HOPE VI demolition across all tracts? iii). How does treatment effect vary with the length of exposure?

Table 3 presents the following findings. Panel A reports the results assuming unconditional parallel trends and clustering at the census tract level. The single overall ATE parameter represents the weighted average (by group size) of all available group-time average treatment effects. This parameter is statistically significant at -0.151 with a standard error of 0.049. The group-specific effects summarize average treatment effects by the timing of the first tract-level demolitions. Each group-time average is aggregated to obtain the group (g= year of demolition) level parameters. The magnitudes of these effects vary across year groups. Further, the effect of HOPE VI demolitions on per capita crime rates appears to be negative and increasing in magnitude the longer census tracts are exposed to the structural clearance. In particular, from period e=0 to e=9, the partially aggregated event study estimators increase in magnitude, from -0.054 to -0.250. This is comparable to those depicted in Figure 7. Starting in period three, the effect of demolitions on crime becomes statistically significant at a 0.129 decrease in per-capita crime by year. The effect size at ten years after initial tract-level program application is almost twice as large in size as (or 93.8% higher than) in period three. However, Figure 9 suggests that these results

Figure 9: Callaway and Sant'ana Time-Group Treatment Effects



(a).



(b).

Notes: Figure 9 presents group-time average treatment effects of demolitions on per-capita crime rates per year at the tract level under unconditional (panel a) and conditional parallel trends (panel b). Red lines present the point estimates and 95% confidence band for pre-treatment periods, while blue lines depict the post-demolition coefficients, allowing for clustering at the census tract-level. The groups are separated by the timing (in year) of the first demolition in tract.

Table 5: Aggregated Treatment Effect Estimates

Panel A: Unconditional Parallel Trends						
	Partially aggregated					Single parameter
Overall ATE						−0.151*
						(0.049)
Group specific effects	[2002]	[2003]	[2004]	[2005]		
	−0.310	−0.142	0.028*	−0.282*		−0.136*
	(0.215)	(0.134)	(0.004)	(0.075)		(-0.046)
	[2007]	[2008]	[2009]	[2010]		
	−0.053	−0.044*	−0.009	0.039*		
	(0.026)	(0.004)	(0.005)	(0.005)		
Event study	e=0	e=1	e=2	e=3	e=4	
	−0.054	−0.062	−0.109	−0.129*	−0.156*	−0.174*
	(0.027)	(0.028)	(0.049)	(0.051)	(0.054)	(0.059)
	e=5	e=6	e=7	e=8	e=9	
	-0.158*	-0.161*	-0.166*	-0.189*	-0.250*	
	(0.055)	(0.057)	(0.059)	(0.067)	(0.082)	
Panel B: Conditional Parallel Trends						
	Partially aggregated					Single parameter
Overall ATE						−0.103
						(0.055)
Group specific effects	[2002]	[2003]	[2004]	[2005]		
	−0.236	−0.084	0.021∗	−0.279*		−0.086
	(0.172)	(0.105)	(0.006)	(0.084)		(0.051)
	[2007]	[2008]	[2009]	[2010]		
	0.018	−0.049*	0.100	0.119		
	(0.066)	(0.007)	(0.089)	(0.064)		
Event study	e=0	e=1	e=2	e=3	e=4	
	−0.048	-0.050	-0.100	-0.091	-0.104	−0.115
	(0.030)	(0.026)	(0.053)	(0.056)	(0.064)	(0.062)
	e=5	e=6	e=7	e=8	e=9	
	-0.106	-0.091	-0.111	-0.136	-0.218*	
	(0.062)	(0.076)	(0.064)	(0.071)	(0.087)	

Signif. codes: * 95% confidence band does not cover 0

Control Group: Never Treated, Anticipation Periods: 0, Estimation Method: Doubly Robust

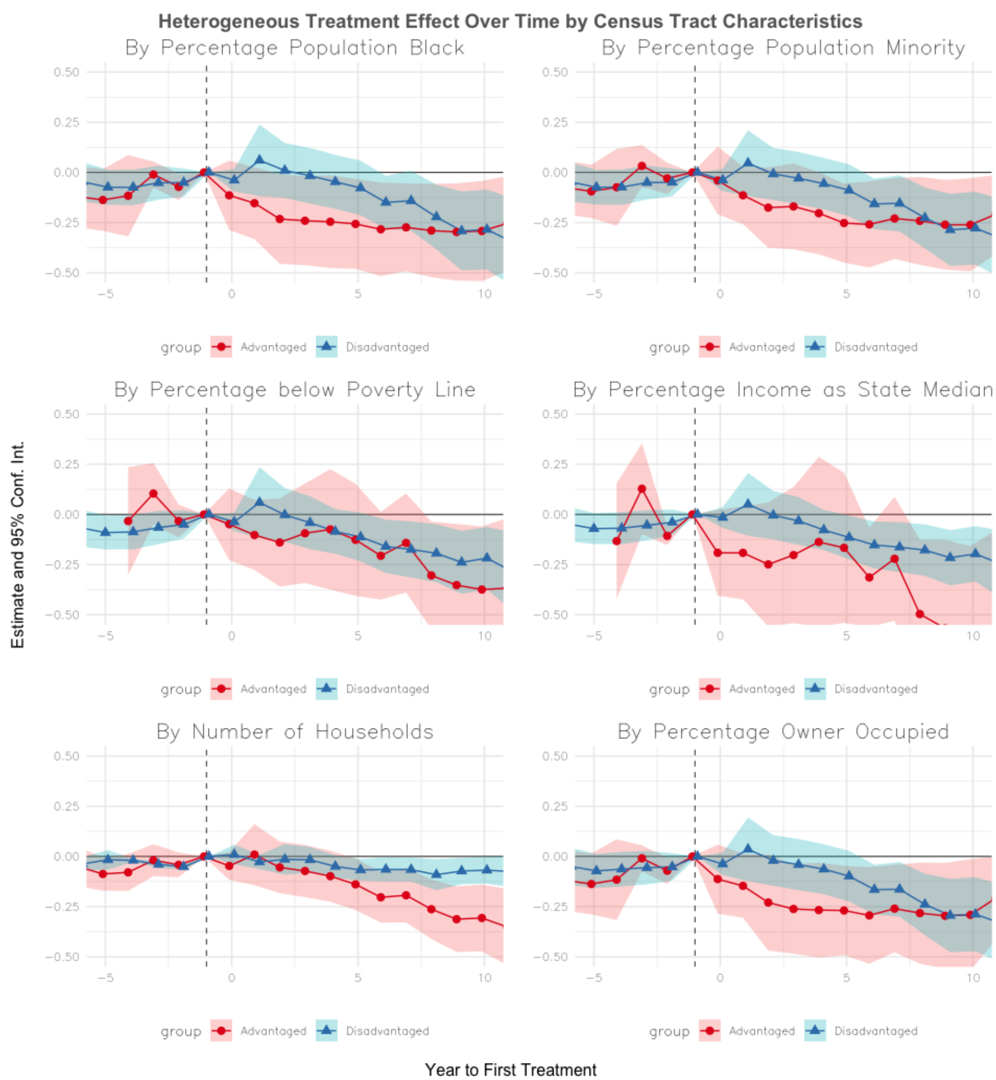
Notes: Table 3 reports aggregated and partially aggregated treatment effects under conditional and unconditional parallel trends assumptions. Row "overall ATE" reports a simply weighted average (by group size) of all group-time averages. "Group specific effects" are group averages where groups are informed by timing of first tract-level demolitions. "Event study" details treatment effect by length of exposure.

are to be taken with a grain of salt, as CS estimators rely on parallel trends assumption for each of group g seen in equation (9), which clearly do not seem to hold, especially for tracts treated at a later time.

Panel B presents results based on the assumption of conditional parallel trends, which assumes that census tracts with the same characteristics would follow the same trend in crime rate in the absence of treatment. Hence the objective of accounting for baseline observables is similar to the propensity score matching method proposed in Section 5.2. However, the Doubly Robust (DR) estimation is used here to improve my causal inference by combining two different approaches: Outcome Regression (OR) and Inverse Probability Weighting (IPW), as recommended by Sant'Anna and Zhao (2020). The OR method models the evolution of the outcomes over time, while the IPW method uses the propensity score to compute a weighted average of differences in outcomes over time to estimate the ATT. DR combines the strengths of both methods and remains consistent even if one of the two models is misspecified. In other words, as long as the outcome evolution of the counterfactual is correctly predicted *or* the conditional probability of a unit being in the treated group given a list of covariates is correctly modeled, the DR estimator should be consistent for the average treatment effect on the treated.

The covariates used in the analysis of census tracts include the percentage of Black and Hispanic populations, percentage of owner-occupied housing units, number of households in a census tract, and percentage of population living below the poverty line. The outcomes of the doubly robust method sufficiently passes the "pre-test", as seen in (b) of Figure 9. Panel B of Table 3 reports a statistically insignificant simple weighted average of -0.103, which is smaller than that obtained under the assumption of unconditional parallel trends. This suggests that some of the observed covariates are correlated with the treatment variable and are also related to the outcome variable. By controlling for these covariates using the doubly robust approach, the effect of demolitions on crime rates is thereby diminished. Similarly, most of partially aggregated group specific effects also become smaller and statistically insignificant. Event study finds that the amplification effect over time likely still exists but the coefficients do not have enough statistical power to reject the null. Overall, the crime-reduction effect of housing demolition is ambiguous as CS estimators find no conclusive findings.

Figure 10: Heterogeneity in Dynamic Treatment Effect over Time



Notes: Figure 10 illustrates the heterogeneity of dynamic treatment effects across various neighborhood characteristics. Each subfigure presents the event study estimates by group based on whether the covariate value is above or below the panel average, with circles indicating results for tracts with higher socioeconomic advantages, and triangles for tracts with relatively lower advantages.

5.6 Heterogeneity and Potential Mechanisms

This section investigates the heterogeneity of the results across different neighborhood characteristics and sheds light on the underlying mechanism behind the observed patterns of reduction in crime following housing demolitions.

Figure 10 illustrates the dynamic treatment effect over time for various characteristics, including the percentage of Black population, those below the poverty line or belonging to minority groups, the number of households per tract, the median family income as a percentage of state median, and the percentage of housing units occupied by their owners. Notably, public housing demolitions in tracts with a lower than average number of households, a low share of people of color, and high levels of owner occupancy appear to be more effective in reducing crime. These effects are stronger for at least five years after the initial tract-level demolition, compared to tracts with more than 1487.6 households, a higher than average share of Black and non-white minority population (40.4% and 63.9%), less than 72.1% of family income as a percentage of state income, or less than 35.9% of property owner occupancy.

To highlight the differential impact of public housing demolitions on crime rates, I interpret the dynamic effect size in tracts with higher socioeconomic advantages with respect to underprivileged tracts. I find that census tracts with a lower percentage of Black population experience a significantly greater reduction in crime in the first eight years following initial demolitions. At period five, the effect of public housing demolitions on crime rates is more than ten times as large in tracts with a lower percentage of Black population, reducing crime rates by 0.26 incidents per capita per year. In contrast, in tracts with high shares of Black population, the effect is much smaller, at -0.02. Similarly, tracts with a higher share of housing units occupied by property owners experience a larger decrease in crime in period five, of -0.27, a magnitude twice as large as it is for tracts with lower property ownership (-0.13). Notably, the coefficients for percentage of population below the poverty line are comparable between the two groups at period five (-0.13 and -0.14).

One possible explanation for this pattern is that public housing demolitions may have been carried out more effectively or with greater planning and coordination in areas with relatively higher socio-economic status. Alternatively, it could be that characteristics of these locations make them more conducive to the benefits of public housing transformations.

It is worth noting that in general more underprivileged tracts appear to catch up over time in terms of crime reduction, but the benefits of public housing revitalization may take longer to materialize in these areas due to a variety of factors. One potential factor is the amount of time it takes for new businesses and other positive developments to take root in historically disinvested neighborhoods, where there may be a lack of infrastructure and amenities needed to attract new investment. Persistent structural inequities, manifested in decades of discriminatory practices, primarily targeted Black and Brown tenants and laid the foundation for the segregation of other people of color, are potentially attributable to enduring patterns of concentrated poverty and neighborhood disorders that can be difficult to overcome even with targeted interventions like public housing revitalization.

Additionally, despite the reported neighborhood-level crime-reductions, this thesis does not provide any evidence to suggest that mixed-income housing polices, such as HOPE VI, do not further exacerbate the existing structural disadvantages faced by underprivileged areas. This is because residential movements resulting from the replacement of dilapidated projects with mixed-income developments are left unaccounted for. Such movements may involve changes in residents' socioeconomic status, crime distribution within and between neighborhoods, displacement and marginalization of low-income families, and reduction of affordable housing stock. Instead, this thesis provides aggregated or partially aggregated estimates of the effects of HOPE VI structural demolitions on entity-level crime statistics. These estimates are subject to scrutiny due to various problems identified with the Chicago Police Department, as described in section 2.6. These issues raise serious concerns about the reliability and validity of CPD crime data as a measure of public safety.

To better understand the mechanisms underlying the disparate effects of HOPE VI housing demolitions on local criminal offenses, further analyses are warranted. These analyses could examine potential mediating factors that transmit changes in response to the housing policy, such as the injections of new businesses, the promotion of community cohesion and resources, or improvements in educational infrastructure. By decomposing how the observed relationship between public housing demolitions and crime reduction is substantiated, these analyses would provide insights into how the benefits of public housing transformations can be optimized with respect to the socio-economic contexts of the targeted neighborhoods.

6 Conclusion

It has been the focus of this thesis to present an initial step toward the analysis and interpretation of patterns of neighborhood crime following place-based housing policies. To achieve this, I use census tract-level reported crime rates as the outcome variable and leverage the staggered nature of demolition application, employing a difference-in-differences framework with entity and time-fixed effects. I also consider spatial spillover effects by incorporating a spatially lagged indicator of neighboring demolitions. A combination of propensity score and DiD methods, as well as the bias-adjusted group time average treatment effects model proposed by Callaway and Sant'anna (2021), enable me to isolate the effects of HOPE VI from other changes that would likely occur in the absence of the program. Based on the two-way fixed-effects specifications, my findings indicate that, on average, the HOPE VI public housing demolition program significantly reduces reported crime at the census tract level. Interestingly, the crime-reduction effects are no longer statistically significant as shown by the group-time average treatment effects of CS estimators under the assumption of parallel trends conditional on a set of baseline census tract covariates. Event studies suggest that the crime-reduction effects increase with the length of exposure to the first tract-level building clearance. The time it takes for crime-reduction effects to materialize and the size of such effects are also dependent on neighborhood characteristics, with greater effects observed in census tracts with higher income, lower share of Black and minority population, and higher property ownership.

These results are in line with the conjecture that physically dismantling high-rise public housing projects and replacing them with low-rise mixed-income refurbished units could potentially alter the incidence of criminal offenses, or rather, police perceptions and enforcement practices in targeted neighborhoods. The precise mechanism that facilitates the estimated effects is left unanswered. One potential explanation is a peer effect in crime,[6] suggesting that dispersing law-abiding public housing tenants away from disruptive individuals could potentially reduce their exposure to crimes and thus lessen the reinforcement of such offenses. Another possibility is that the ways in which public housing redevelopments admit tenants are discriminatory by race, which leads to a change in the racial compositions of targeted neighborhoods and possibly more relaxed policing, as patterns of police enforcement are heavily informed by the predominant demographics of a community.

Given these uncertainties, it is crucial to avoid making conclusions beyond the scope of my findings. Specifically, my thesis offers no inquiry into whether the physical structure of high-rise public housing projects is directly responsible for the fostering of criminal activities, nor that the demolitions of these units are by any means effective in generating welfare-enhancing outcomes for those relocated. On the contrary, it should be recognized that perceptions and stereotypes linking public housing tenants to criminal behaviors are the front-seat drivers

6. see Bayer et al., 2009

in the stigmatization of and police violence in and against areas that are predominantly occupied by low-income or Black and Brown populations. Hence, it is fundamentally flawed to perpetuate these harmful narratives as to justify the crime-reduction of HOPE VI under the assumption that it disperses "delinquent" individuals. Similarly, narrowly focusing on collective efficacy as a solution to crime overlooks the role that external institutions and systems play in shaping these outcomes.

In conclusion, while the findings of this thesis provide valuable insights into the impact of income-mixing housing demolition and redevelopment programs on reported measures of neighborhood safety, further analysis is needed to fully understand their mechanisms and broader societal implications, especially in the context of countering the legacies of racial segregation and concentrated disadvantages in the urban residential landscape.

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