Essays in Urban and Housing Economics

A Thesis Submitted to Trinity College Dublin, the University of Dublin in Application for the Degree of Doctor of Philosophy

By

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Dublin, 2024

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I declare that this thesis has not been submitted as an exercise for a degree at this or any other university and it is entirely my own work. Chapter 2 of this thesis is co-authored with Sun Kyoung Lee and Ronan Lyons. Chapter 3 of this thesis is co-authored with Ronan Lyons.

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Maximilian Günnewig-Mönert March 20th, 2024

Summary

This dissertation consists of three essays at the intersection of urban economics and the economic effects of affordable housing policy. While they do not use common data or methods, they all tackle fundamental issues concerning affordable housing policy's direct and indirect effects on housing markets and urban and regional dynamics.

Chapter 1 examines the long-run effects of the building design of public housing on neighborhood composition and rental prices in New York City from 1930 to 2010. It documents sizeable effects on racial composition by using a newly assembled dataset on the US census tract level and leveraging the staggered rollout of public housing. White population declined in tracts with public housing projects with significant spillover effects to adjacent tracts, while black population increased only in public housing tracts. The effects on white and black population are driven by a specific project type called the "Tower in the park" – slim brick high-rises and vast green spaces in between. Falling rent prices around "Towers" indicate negative demand effects. A cross-sectional analysis finds that "Towers in the Park" are more associated with higher incarceration rates than non-towers. However, incarceration rates cannot entirely explain spillover effects on white population. Finally, calibrating a structural neighborhood choice model evaluates the welfare consequences of building design. The model demonstrates that removing public housing can increase amenity values and improve welfare. It shows that welfare gains can be explained by "Tower in the park" removal. This suggests that building design can mitigate the negative externalities of public housing.

Chapter 2 contributes to an intensively discussed topic, rent control policies, by examining the effects of a not-yet-studied policy design: the 1920 New York City (NYC) rent control laws. These laws gave elected civil court judges the power to decide in landlord-tenant cases whether a rent increase was "reasonable" or not, giving them discretionary authority to set rents according to their notions of "reasonableness." This discretionary approach gave rise to the phenomenon of "tenant" and "landlord" judges who openly advocated for the interests of their respective sides. The chapter improves on the existing literature on rent control regarding data and identification. It uses the binding nature of court borders between land and tenant-judge and landlord-judge controlled districts and implements a spatial regression-discontinuity-design. This strategy reveals a 10% increase in market rents within landlord-judge-controlled districts, contrasting with no effect on transaction prices. The chapter rationalizes these results, positing a mechanism whereby rent control undermines landlord profitability in the face of escalating legal expenses, thereby shaping market dynamics.

Chapter 3 explores the responsiveness of housing supply to prices and costs, using the case of Ireland over the last half-century. It uses data for the country and Dublin at a quarterly frequency from the 1970s and a county-level panel from the 1990s. It uses an error-correction framework, supported by an instrumental variables approach, given endogeneity concerns. It shows that responsiveness to prices rose after the 1980s, then fell in the 2000s before rising again. The chapter also documents significant heterogeneity in elasticities at the county level, with supply in Dublin among the least responsive to prices and costs. These findings suggest new avenues for research on the determinants of supply elasticities.

Acknowledgements

I owe a great deal of gratitude to many individuals who have supported me throughout this thesis. Although I cannot express my thanks to everyone in detail, I would like to extend my sincere appreciation to the following key contributors.

I am deeply grateful to Ronan Lyons and Jason Barr for their invaluable guidance as advisors. Ronan's passion for urban and housing economics research inspired me to pursue my work in this field. His constructive feedback was incredibly insightful in the early stages of my projects. Jason has also been a wonderful mentor, offering valuable big-picture comments that helped sharpen and focus my work. I am truly thankful for his unwavering support throughout the job market.

I also want to acknowledge the faculty of the Trinity Department of Economics for their valuable feedback and mentorship during the program. I extend my thanks to Marvin Suesse, Martina Kirchberger, Nicola Fontana, and Vitaliia Yaremko for their engaging, motivating, and helpful discussions. This work has greatly benefited from the input of seminar participants at Trinity College Dublin and the CEPH work-in-progress workshop. I am especially grateful to Christina Lysaght for her indispensable assistance with various administrative matters. I consider myself fortunate to have had the opportunity to collaborate with excellent partners along the way. Working with Sun Kyoung Lee (coauthor in Chapter 2) has been a joy, and I particularly appreciated our extensive research discussions.

This journey would have been much more challenging without the support of my classmates, many of whom have become friends. I am thankful to my colleagues and dear friends at TRISS for making my Ph.D. experience so enjoyable. Sharing this journey with Vincent Thorne, Eugenia Frezza, Alexis de Saint Lager, Francesco Barilari, Beren Demirölmez, Iris Wohnsiedler, Simone Arrigoni, Matteo Pograxha, Enrico Cavalotti, Bhavya Shrivastava, and Paul Blanchard has not only improved my research but also made Dublin much more fun. Thank you for creating such an amazing community. I am looking forward to staying in touch with all of you in the future and witnessing the success of your promising careers.

I want to emphasize the support of Vincent Thorne and Eugenia Frezza during the final years of my Ph.D. Their assistance made the job market process much smoother.

Lastly, I want to express my gratitude to my family, friends, and partner. I am thankful to my parents, Cornelia and Wolfgang, and my grandmother, Gretel, for their patience and care. Despite the distance, my friends in Bielefeld have been a constant part of my journey. I want to thank Zina Cohen for her support during COVID and the years in Berlin. I will always treasure the memories of the time we spent together. I extend my thanks to Simon Luck for the wonderful moments we shared, laughing and discussing politics.

Finally, I want to thank my partner, Prachi. You have been my biggest cheerleader along the way. I feel like a big part of this PhD is also yours. I can't wait to see what is in this life for us.

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Introduction

Housing constitutes a necessity, yet urban areas suffer from a shortage of affordable accommodation, burdening specific households with rental costs that outstrip their income. Consequently, governments are asked to intervene by implementing policies to improve the situation. The main aim of housing policy is to make habitable and sustainable housing options accessible. Toward this end, housing policy encompasses an array of interventions, ranging from restrictive measures such as rent control, eviction safeguards, and housing allocation schemes to stimulative initiatives like social housing support, housing subsidies, and tax incentives for homeowners. Nevertheless, akin to any governmental intervention, housing policies engender both intended and unintended repercussions. While the intended objective is to ensure housing affordability, thereby moderating tenants' rental burden, these policies can contribute to observable disparities in property valuations, income distributions, and neighborhood demographics. They influence the aesthetic appeal, cleanliness, demographic makeup, and overall amenities of neighborhoods, consequently impacting property values and the quality of housing services. Understanding these effects can help policymakers and the general public implement more effective strategies for fostering affordable housing while mitigating unintended ramifications.

The present thesis explores these topics through three chapters. The first two chapters investigate the role of public housing and rent control and their effects on market rents, population, and welfare. In the last chapter, I study the economic geography of housing supply elasticities and their link to planning and affordability.

In the first chapter, I estimate the long-term effects of public housing on market rents, racial sorting, and welfare. I provide new evidence on sorting black and white populations as a response to public housing construction in New York City from 1930 to 2010. In the 1930s and 1940s, public housing was of exceptional quality to accommodate upwardly mobile working-class residents. Moreover, these early public housing projects were also marked by clear racial segregation policies, with a predominant allocation to white residents. Against this backdrop, tracts with public housing projects followed different demographic trajectories than trends in the rest of New York City. Specifically, in tracts designated for public housing, there was a significantly higher white population in 1930 and a more pronounced decline in the white demographic in the subsequent decades compared to the rest of the city.

I leverage the staggered rollout of public housing in New York City as a useful quasi-experimental setting. I overcame data limitations by assembling a novel and comprehensive panel dataset at the census tract level for New York City by combining newly digitized historical records with data from the US Census. Rental and real estate prices are sourced from the New York Times real estate section. I collect information about New York City Housing Authority (NYCHA) projects – including population figures, property attributes, completion dates, and racial demographics – from historical documents and NYCHA development data books. Finally, I use cross-sectional data on tract-level incarceration rates. Harmonizing census tracts to 2010 boundaries yields a balanced panel dataset, encompassing 2,164 census tracts for each of the nine census years spanning 1930 to 2010.

I show that white population is declining in public housing tracts with significant spillover effects to surrounding areas. Black population increased, but only in public housing tracts. These effects are not driven by public housing per se but rather by a specific building type called the "Tower in the Park" – slim brick high-rises of different layouts with wide green spaces in between. Urban activist Jane Jacobs has famously argued that these buildings attract crime and destroy the fabric of neighborhoods. In a cross-sectional analysis, I find that "Towers in the Park" are more associated with crime than non-Towers. However, crime cannot entirely explain spillover effects on white population. Finally, I evaluate the welfare consequences of the externalities of public housing using a static neighborhood choice model. The model demonstrates that removing or altering public housing areas as a ratio of the total tract area can increase amenity values and improve welfare, especially for white households. These gains add up to $200 for White households and $400 for Black households. These counterfactuals inform the impact of public housing on its neighborhood and show that redesigning buildings might help to mitigate negative externalities.

This chapter contributes to the literature on the neighborhood effects of public housing in two distinctive ways. Firstly, it takes a long-term perspective by studying the construction-impact of public housing from 1930 to 2010. Previous studies investigated the demolition impact of public housing over shorter time horizons. Using a novel panel data set at the census tract level, this chapter can deliver a robust estimate of changes in the racial composition of neighborhoods and market rents. Secondly, I explore the role of heterogeneous building design, a departure from previous literature that assumes homogeneity. By estimating a structural neighborhood choice model, I demonstrate that low-scale projects integrated into the urban fabric can have minimal consequences and remodeling "Towers" can be welfare-improving.

In a second paper, we study the effect of rent control on housing market outcomes. While previous literature studied the consequences of the two most frequent designs, first-generation (rent ceilings) and second-generation rent control (rent growth control), we investigate the effects of a not-yet-studied policy design: the 1920 New York City (NYC) rent controls. This policy design resembled modern Cause Evictions elements and the legal authority to ceil prices. In particular, the 1920 laws gave elected civil court judges the power to decide in landlord-tenant cases whether a rent increase was "reasonable" or not, giving them discretionary authority to set rents according to their notions of "reasonableness." This discretionary approach gave rise to the phenomenon of "tenant" and "landlord" judges who openly advocated for the interests of their respective sides.

We exploit this feature of NYC rent control by using the binding nature of municipal court district boundaries and implementing a regression discontinuity design to measure the effects of rent control on market rents and transaction prices. In particular, we use the distance to the court borders between tenant and landlord judge districts. To measure the judge's leniency, we use variation in a judge's party affiliation and argue that Democrat judges ruled in favor of tenants. In contrast, Republican judges ruled in favor of landlords. We complement this approach with an event study design, which allows us to exploit the continuous nature of the district with both Democrat and Republican judges.

To study the 1920 NYC rent control laws, we collect property-level rental and transaction price information from two sources. Firstly, we use the New York Times real estate section to collect market rents. Secondly, we collect prices from the Real Estate Record and Builders' Guide, a weekly publication of real estate transactions. The final samples consist of 12,186 rental and 8,945 transaction-based observations. To study the policy mechanism, we collect information on all municipal district court judges, including their political affiliations and election cycles, from the NYC Official City Directory. Finally, we reconstruct the 1920s MCD boundaries for NYC from historical maps to build our treatment.

We find that in Republican-controlled districts, rents at the boundary jumped by about 10% after the policy had been introduced, while before the introduction of the policy, rent prices were smooth at the boundary. These results are confirmed by magnitude and significance using an event study approach. Mixed districts can expect 6% - 8% higher rent prices than Democrat-only districts. Since we do not observe the individual judge decisions, we rationalize these results through a simple mechanism. If a landlord cannot be sure she is facing a tenant judge, she will always pay the controlled rent or refrain from increasing rent since asking for a higher rent can lead to costly lawsuits. The 1920 rent control laws would have allowed tenants to withhold rent and wait until their landlord brought the case forward before a judge to obtain an eviction warrant. This could generate non-recoverable income losses for the landlord. We do not find evidence that rent control affected commercial and residential transaction prices. This can be because landlords expected controls to last only temporarily; in the long run, expected earnings would not have been affected.

The study contributes to the literature in two regards. Firstly, it provides the first study investigating a not-yet-studied rent control design. Second, it improves on the existing literature in terms of identification. It uses a regression discontinuity design by exploiting the political affiliation of civil court judges, which allows the measurement of city differences in rent control intensity. Finally, we construct a novel and comprehensive data set for New York City from 1918 to 1926. We combine market rents and transaction prices, merge into neighborhoods, and judge characteristics. Second, the dietary

In the last chapter, we examine the determinants of new supply in Ireland, a country with a volatile housing system over recent decades. We focus particularly on supply's responsiveness to prices and costs, using complementary approaches and data from the 1970s. We also examine the link between prices, costs, and supply, measured in both stocks and flows, following two approaches in the literature.

We find strong evidence of a long-run relationship between supply flow measures, housing prices, and construction costs. We then use error-correction models to estimate that relationship and supplement that with an instrumental variable specification that uses a series of demand-shifters to validate the ECM results. Our baseline is a quarterly series running from 1975 to 2022, where the outcome of interest is the number of units for which planning permission is granted, conceptually the measure of supply most closely linked to price changes. We find similar results using other measures of supply (commencement of, capital formation in, and completion of new dwellings), Dublin-only data, annual data from 1970, and a panel of Ireland's 26 counties.

We do this using data for the country and Dublin at a quarterly frequency from the 1970s and a county-level panel from the 1990s, and we use four main specifications in line with best practices for error correction models. We find strong evidence across all our specifications that housing prices, construction cost, and new housing supply measures are cointegrated; we do not find any similar evidence of a relationship between housing stock and prices/costs. Under our baseline, the estimated elasticity of new housing supply to prices nationally is +0.9, while the elasticity to costs, net of tax reliefs, is larger in magnitude (-1.9). We present evidence that responsiveness to prices rose after the 1980s, then fell in the 2000s before rising again. We also document significant heterogeneity in elasticities at the county level, with supply in Dublin among the least responsive to both prices and costs. These findings suggest new avenues for research on the determinants of supply elasticities.

This chapter contributes to the literature on housing supply elasticities (HSE) in the following way. It provides estimates of HSE in Ireland at both national and regional levels, allowing variation over a fifty-year period for the first time. Studies of other counties suggest lower HSE after 1995; while that is true in the 2000s, HSE was at its highest estimated level in the late 2010s. There is no systematic correlation between estimated price elasticities at the county level and either cost elasticities or estimated ease of approval in the planning process. Nonetheless, those counties at either end – including Dublin, the country's most urban county, and Leitrim, arguably its most rural – are instructive. These findings do not contradict the idea that land-use restrictions or other policy barriers limiting supply have grown in importance over time and are more relevant in Dublin than elsewhere; instead, a more detailed analysis of elasticities across space (and over time) is required.

The thesis closes with a summary of its key findings in the Conclusion section.

Chapter 1

Public Housing Design, Racial Sorting and Welfare. Evidence from New York City Public Housing 1930-2010

1.1 Introduction

Public housing programs aim to provide affordable housing to low-income households. However, as place-based programs, public housing projects can significantly impact their surroundings and play a crucial role in shaping neighborhoods. These housing externalities can alter the appeal of neighborhoods, and individuals with varying preferences may choose different locations based on these changes. Consequently, public housing can influence the local composition of residents.

One argument concerning how these projects affect neighborhoods centers on building design, specifically the "Tower in the Park" concept – slender high-rises surrounded by extensive green spaces, which became emblematic for public housing in the United States (Plunz, 2016). Influential figures like Jane Jacobs and Oscar Newman notably criticized this design, contending that it inadvertently led to crime-ridden and lifeless environments due to the un-policeable indoor and outdoor spaces within these projects (Jacobs, 1992; Newman, 1997).

In this paper, I study how the architectural layout of public housing projects in New York City from 1930 to 2010 generated externalities that impacted the racial composition of neighborhoods, rental rates, and welfare. I establish a causal relationship between public housing construction, racial sorting, and rents.[1] The

[1]The study of public housing demolition dates back to the early stages of initiatives like the Moving to Opportunity projects. This body of literature centered around Chicago indicates

challenge in establishing causation lies in the circular relationship between public housing and the characteristics of the areas where it has been constructed. To address this, I leverage the staggered implementation of public housing projects across the city, employing a stacked difference-in-difference design following the methodology outlined by Blanco **and** Neri (2023). This framework utilizes the distance to public housing projects as a measure of treatment intensity, allowing for the estimation of disparate effects on rents and demographic outcomes. Specifically, treatment is defined at the census tract level. Outcomes in tracts near public housing projects are compared to slightly more distant tracts.

I assemble a novel panel dataset at the census tract level for New York City, combining newly digitized historical records with data from the US Census. I collect and digitize rental prices from the New York Times real estate section and information about New York City Housing Authority (NYCHA) projects from historical documents and the NYCHA development data books. Additionally, cross-sectional data on tract-level incarceration rates serve as a proxy for crime. Harmonizing census tracts to 2010 boundaries results in a balanced panel dataset covering 2,164 census tracts for each of the nine census years from 1930 to 2010.

Neighborhoods experience significant socioeconomic changes due to public housing. White population declined by 23% in treated tracts over the medium run (0-30 years) and by 78% in the long run (40-60 years). Moreover, I find significant spillover effects, leading to an 18% medium-run decline of white population in adjacent areas and a 29% decline in the long run. In contrast, black population increases by up to 73% (0-30 years) and 54% (40-60 years) in treated areas, with no significant spillovers. Turning to property level renral prices, I find no statistically different effects on rent prices. However, I do not observe rent reductions at any distance.

Furthermore, I provide new evidence that these effects are driven by specific project types, namely "Towers in the Park". In line with the predictions of Jacobs (1992) and Newman (1997), I find that white population declines significantly in tracts with a "Tower" [2] (-79%) and adjacent tracts (-36%), while the effects for

moderately positive impacts from public housing demolition, particularly for residents and minority populations (Jacob, 2004; Chetty **andothers**, 2016). More recent research has extended its scope to examine the consequences of demolitions on a broader range of outcomes, including rental rates and construction trends. It is important to acknowledge that studies involving alternative forms of affordable housing provision, such as the Low-Income Housing Tax Credit Scheme, housing vouchers, or mixed-income redevelopment, may not be directly comparable to the traditional government-operated public housing model.

[2] Jacobs (1992) never provided a clear definition of what a "Tower in the Park" is. In Section 1.5.1, I use the two distinguishing criteria: it must be of sufficient height with a sufficiently low ground coverage. I establish a threshold for the height of 10 stories using the New York Department of Buildings requirements. To determine a threshold for ground coverage, I use the average of 26% across public housing projects. Moreover, I show that only considering quality and importance -

non-tower buildings are considerably smaller. Rent prices within "Tower" tracts also experience substantial declines, indicating negative demand effects, whereas price effects for non-tower buildings are minimal. Rents fall by 30% (0-30 years) and by 21% (40-60 years) in "Tower" tracts and by 16% in the long run around "Tower" buildings. Moreover, I do not find significant effects of changes in black public housing residents as drivers of white population losses in treated and adjacent tracts, ruling out potential tipping effects.

To delve deeper into the mechanisms, I conduct a cross-sectional analysis using 2010 incarceration rate data. This analysis reveals that "Tower in the Park" projects have higher crime rates than non-tower projects, although there is no evidence of spillover effects. However, I find supporting evidence that a one percent increase in incarceration rates in "Tower" projects leads to a fall of .21% of white population within treated tracts, suggesting that stigma associated with "Tower"-style projects may render nearby neighborhoods unattractive (Tach **and** Emory, 2017).

To understand welfare implications, I incorporate these findings into a static model of neighborhood choice following Bayer **andothers** (2007) **and** Almagro **andothers** (2023) which allows households to sort into neighborhoods based on preferences for public housing type. This assessment informs current policy debates centered around two questions: Should we continue public housing, and if so, what kind of public housing should be developed? The objective here is to study counterfactualscenario where I modify the characteristics of public housing. This exercise aims to provide policymakers with insights into the effects of different public housing designs.To carry out this analysis, I need to empirically estimate preference parameters, which cannot be recovered from the difference-in-difference design. In the model, I recover the preference parameters by instrumenting all endogenous variables with tract characteristics 1.5 to 2.5 miles away from a given tract. I use these parameters and the structure of the model to estimate the change in welfare from two counterfactual scenarios. Welfare is then expressed as a rent equivalent, which is required to make households in the counterfactual scenario indifferent to the actual scenario. The rent equivalent is given in dollars per month.

In the first scenario, I eliminate all public housing projects, assuming all units in the city become private. Over time, welfare gains decline and stabilize after 1970, settling at approximately $200 for White households and $400 for Black households. The model also sheds light on how welfare is generated without public housing. Welfare gains are most pronounced in treated tracts

proxied by area share - yields results that explain spillovers.

and lowest in the second neighborhood ring. In contrast, rent prices are lower in the counterfactual scenario, particularly in the second ring. Consequently, the demand for distant locations decreases when public housing is removed, leading to welfare gains in remote areas due to reduced rental costs. Residents in public housing tracts benefit primarily because they have a stronger preference for not living in close proximity to public housing projects.

In the second scenario, I explore the removal of "Tower in the Park"style public housing. I calculate rent equivalents by considering household preferences for both "Tower" and non-tower projects. Removing "Towers" results in welfare improvements of $72 for Whites and $161 for Blacks. Conversely, removing non-tower buildings has considerably smaller effects, with welfare gains of $30 for White households and $69 for Black households.

Note that while the removal of both types of public housing leads to welfare improvements, the largest gains are associated with the elimination of "Towers." These findings indicate that revamping "Tower in the Park" style public housing can enhance the overall quality of life in neighborhoods. For instance, redeveloping these projects and their surrounding areas by adding more private or mixed-income units or integrating them into the existing urban fabric through architectural modifications could be viable solutions. However, it's crucial to weigh the feasibility of such initiatives against the benefits they bring to the community.

This paper contributes to three broad literatures. Firstly, it aligns with the literature investigating the external impacts of affordable or subsidized housing construction. Two key findings from this literature are worth noting. In the context of public housing demolitions in Chicago, previous studies have identified significant positive effects. Within a quarter-mile radius of demolition sites, all types of serious crimes decreased by 8.8%, with this effect diminishing as distance from the demolished projects increased (Sandler, 2017). Additionally, house prices and rents increased by up to 20% over the ten years following the demolition. Furthermore, in the long run, residents were less likely to be low-income and black (Blanco, 2022). Secondly, within the context of affordable housing construction, there are considerable amenity effects. Low-Income Housing Tax Credit (LIHTC) developments or the transition to mixed-income housing, can attract higher-income homebuyers in low-income areas Diamond **and** McQuade (2019) **and** Blanco **and** Neri (2023). In New York City, subsidized housing has generated significant price appreciation in the immediate vicinity (Schwartz **andothers**, 2006). Federal public housing constructed between 1977 and 2000 has not typically led to reductions in property values (Ellen **andothers**, 2007).

My paper contributes to this literature in two distinctive ways. Firstly, it takes a long-term perspective by studying the construction-impact of public housing from 1930 to 2010. The results indicate that the effects for "Tower in the Park" style projects are symmetric to results obtained from the demolition in public housing. Secondly, I explore the role of heterogeneous building design, a departure from previous literature that assumes homogeneity. I demonstrate that low-scale projects integrated into the urban fabric can have minimal environmental consequences. Given that "Towers" were primarily constructed between 1940 and 1970, while low-scale projects came afterward, this finding aligns with the conclusions of Ellen **andothers** (2007).

Another related area of research explores how historical factors shape cities and towns. Two papers closely related to mine are those by Dalmazzo **andothers** (2021) and Bromhead **and** Lyons (2022), which investigate the effects of historical housing policies on population dynamics and their consequences. In a broader context, geographic features, transportation infrastructure, or disruptive events like wars and catastrophes can have enduring effects on agglomeration and population (Bleakley **and** Lin, 2012; Ager **andothers**, 2020; Heblich **andothers**, 2020; Dericks **and** Koster, 2021). My contribution to this literature is by utilizing public housing as a population-shifting mechanism to identify neighbohood effects. It directed demand away from certain areas and altered the composition of those areas. Additionally, I contribute by assessing the causal effects of public housing in the context of America's largest city.

Finally, my paper adds to the literature that employs structural models to investigate the effects of urban policies. Previous studies have examined the causes of geographic racial segregation, including theoretical models of segregation, measurements of segregation indexes over time, and estimations of tipping points and White flight (Schelling, 1971; Cutler **andothers**, 1999; Logan **and** Parman, 2017; Card **andothers**, 2008; Lee, 2022; Boustan, 2010). In this sense, public housing in NYC can be seen as causally accelerating existing pattern of segregation on a more granular spatial scale. I contribute to this literature by building upon the frameworks of Bayer **andothers** (2007) and Almagro **andothers** (2023). My paper complements these previous approaches by examining the location choices of ethnic groups due to heterogeneous preferences over buildig design. Importantly, I can rule out effects due to changes in resident composition. To recover choice parameters, I utilize plausibly exogenous changes in tract exposure to public housing and residents in public housing.

The paper proceeds as follows; Section 1.2 provides details on the historical context and describes the data. Section 1.3 introduces the empirical analysis. In

Section 1.4, I estimate the long run effects of public housing. In Section 1.5, I estimate the effect of "Tower in the Park" style structures. Section 1.6 introduces the theoretical model and the estimation procedure of the model's parameters. Section 1.7, details the counterfactual mechanism and presents welfare estimates for black and white population, and Section 1.8 concludes.

1.2 Context and Data

In this section, I describe the historical context, highlighting the most important events that characterize the development of public housing and racial dynamics of racial segregation in New York City. Then, I provide a quick overview of the primary data used in the analyses and their sources.

1.2.1 Background

Public housing construction in the United States began as a response to the Great Depression, initiated by the Public Works Administration (PWA) in 1933. The PWA's primary objective was to create jobs in the construction industry, and its secondary objective was to eliminate slums. The Housing Act of 1937 emphasized both objectives and led to the creation of the U.S. Housing Authority, which funded local housing projects to combat "unsafe and unsanitary housing conditions" (Allen **and** Van Riper, 2020; Radford, 2008; Fogelson, 2003).

New York City played a pioneering role in public housing starting in 1936, with over a quarter of all U.S. public housing units located there by 1940. The New York City Housing Authority (NYCHA) managed and developed these projects, aiming to replace and refurbish slums with well-maintained housing complexes to increase neighborhood quality. Newly constructed projects were mainly low-rise buildings that blended in with the existing 19th and early 20th-century building environment (Williams (2014), Bloom (2008) **and** Marcuse (1986) and Figure 1.1). This paper only considers government-run affordable housing construction among the various types of provision due to its scale and physical attributes.[3] The early projects were subject to the racial prejudices of their times, and screening measures ensured tenants were married couples with two children and an employed head of household. Projects were primarily segregated by race to keep them appealing to white residents(Allen **and** Van Riper, 2020;

[3] In addition to public housing, New York City experimented with publicly subsidized affordable housing called "The Mitchell-Lama" program enacted by state law in 1955. It should encourage developers to build affordable middle-class housing and to stem middle-class flight out of New York City. In exchange, developers were granted low-interest loans or real estate tax benefits. From 1950-1970, New York's public housing construction surpassed the Mitchell-Lama program's (Woodfill, 1971).

Bloom, 2008; Marcuse, 1986; Vale **and** Freemark, 2012). This strategy is evident in the locations selected for the new tenant. **??** illustrates the distribution of projects developed across different decades and the quartiles of the neighborhood characteristics in which they were constructed. Projects from the 1930s and 1940s were predominantly built in majority-white neighborhoods with below-median density, aiming to attract white residents.

After World War II, the United States faced a housing shortage, further worsened by the return of servicemen. To address this issue, the 1949 Housing Act was enacted to increase the construction of public housing, while the clearance of slums remained a significant objective under Title I.[4] Between 1950 and 1970, New York saw a surge in public housing, with 72,499 units constructed in the 1950s and 42,721 units in the 1960s, surpassing pre-war construction (Plunz, 2016). This amounted to 25% of all units built in New York City in the 1960s. During this period, the design of public housing projects shifted to slim high-rises surrounded by open areas, known as the "Tower in the Park" style. This style is characterized by decreased ground coverage from 27% to 15%, while the average project height increased from 5 to 16 stories. The construction costs per room remained low at $17,317 (in 2010 dollars) in both decades, well below the $26,783 of pre-war projects (see Figure 1.1). Projects built in the 1950s were still predominantly located in majority-white neighborhoods, exhibiting considerable variation in development and population density. In contrast, approximately 60% of projects constructed in the 1960s were situated in areas with above-median density. These locations were more frequently black neighborhoods, a trend that became increasingly pronounced after 1970 (see **??**).

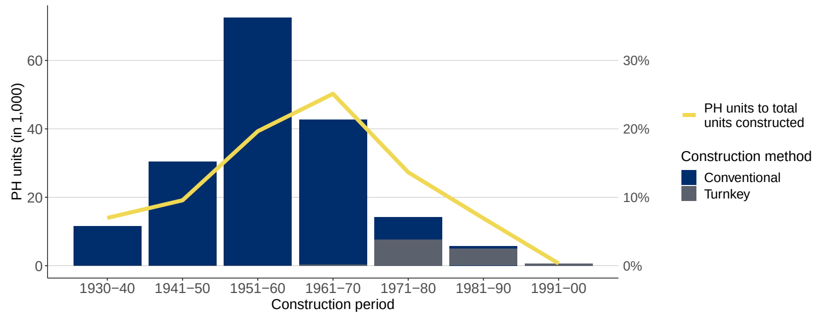
During this time, there was a significant shift in the demographics of public housing residents. As New York City's population shifted towards urban immigrants, more than half of all newly developed projects were allocated to Blacks and Hispanics from 1950 to 1970. By December 1971, it became apparent that Whites were leaving public housing projects (Friedman, 1966). Projects played a crucial role in influencing changes in the spatial distribution of New York's population. In 1950, a tract with a public housing project had, on average, 4,577 Whites, 1,453 more than the average tract in the rest of the city. In 2000, there were, on average, 717 Whites, 636 less than the average tract in the rest of New York (for more details, see Figure 1.C.1 and Figure 1.C.3).

Demographic shifts and the "Tower in the Park" design garnered criticism and reduced public support. Famously, Jane Jacobs and Oscar Newman blamed

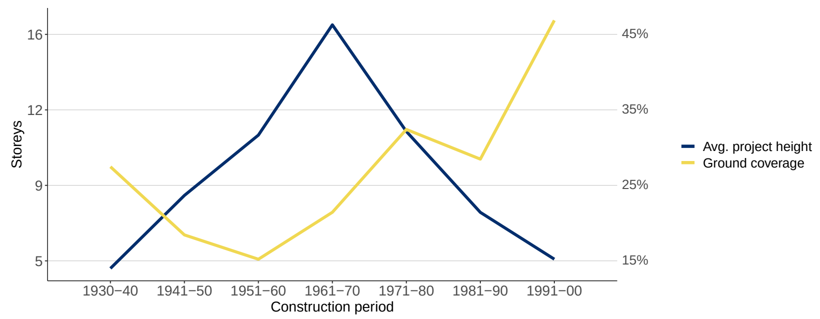
[4] It is worth emphasizing that Robert Moses, who chaired the Mayor's Slum Clearance Committee, wielded considerable authority in New York's urban renewal initiatives. Nevertheless, the extent of his involvement in the housing program is less thoroughly researched (Caro, 1975)

Figure 1.1: Public Housing in New York City

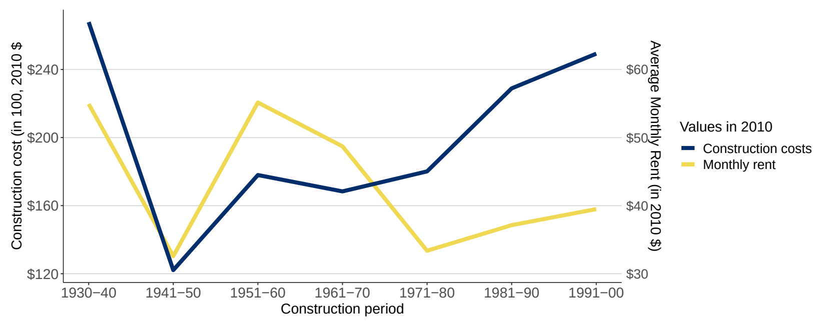
(a) Construction by Decade



(b) Height and Ground Coverage

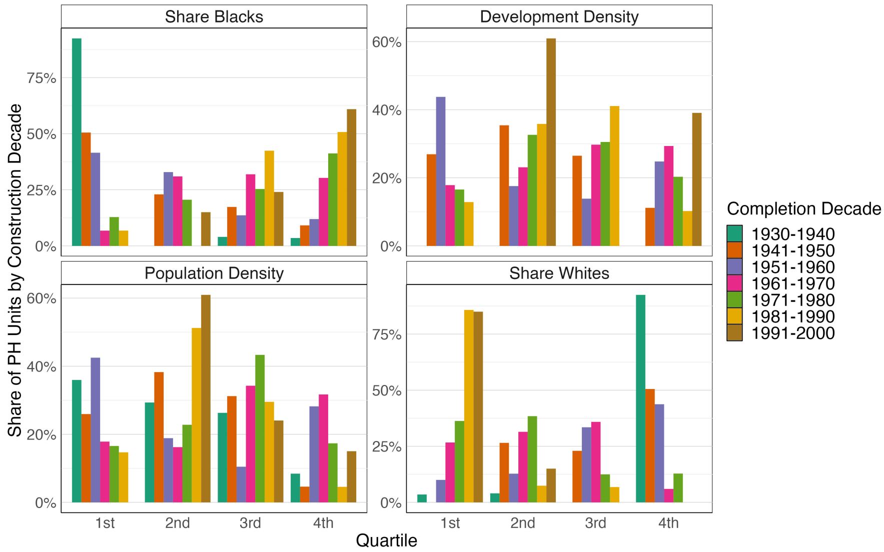


(c) Rent and Construction Costs



Note. Figure 1.1 reports trends of public housing by construction decade. Projects have been grouped in constriction periods by their completion date. Panel (a) shows the total number of units within a decade. There are two acquisition methods. Under the *Conventional Method*, the authority acquires the land and contracts for General Construction, Heating and Ventilation, Elevators, Electrical, and Plumbing work. Under the *Turnkey Method*, the developer buys the land, constructs the Development, and sells it to the Authority under the terms of a pre-agreed contract. The yellow line shows the total number of public units as a share of total units constructed in New York City within the decade. Panel (b) shows the average height and ground coverage ratio - this is the total ground floor area of the building footprints of a development, divided by a development's total area. The average was taken across all public housing projects constructed within a decade. Panel (c) reports ask real rent at opening date in a public housing project average by construction cohort and the average cohort real construction costs; both variables have been defalated using 2010 CPI. *Source.* NYCHA development data book. Details on the construction of data the data set can be found in Section 1.2.2.

Figure 1.2: Evolution of public housing by construction period



Note: Figure 1.2 shows the share of public housing units by construction cohort by quartile of baseline tract characteristics. Tract characteristics were taken the decade before a public housing project arrived. Next, total public housing was grouped by quartile as a share of the total number of units constructed within the dace. Each decade refers to the projects constructed nine years before. Details on Data construction
Source. NYCHA development data book and US federal census. Details on the construction of the data set can be found in Section 1.2.2.

the "Tower in the Park" as a utopian idea that generates crime-driven and unlively places by having large, un-policeable indoor and outdoor spaces, lacking potential care of residents and shop-owners (Jacobs, 1992; Newman, 1997). Rising opposition resulted in a policy shift at the local level in favor of low-density public housing (Clapp, 1976). Moreover, while the majority of projects until 1960 was build in mainly white neighbohoods the 1970s were seeing a shift towards mainly black neighbohoods.

In the 1970s, federal support for public housing declined, with the Housing and Community Development Act of 1974 reducing funding for new public housing construction. Instead, market- and income-based affordable housing provision was favored (Vale **and** Freemark, 2012). This period was marked by rising challenges associated with public housing. Residents reported rising crime rates and noticeable deterioration of housing stock throughout the 1970s (Bloom, 2008).

During the 1980s, there was a shift in focus towards community-based organizations and market-oriented subsidies, such as the Low-Income Housing Tax Credit (LIHTC). This led to less attention on public housing programs, causing mismanagement and rapid deterioration of existing units. In New York City, public housing construction sharply declined, and the few projects built were often low-rise single houses (Wyly **and** DeFilippis, 2010). In 1993, the HOPE VI Program was implemented after a national commission identified severely distressed public housing units. The program aimed to demolish, rehabilitate, or rebuild these units. Nationwide, from 1993 to 2010, about 97,000 units were demolished, with residents moving to other public housing or receiving housing vouchers. Although HOPE VI was utilized to a limited extent in New York, it had a significant impact, with the first NYCHA development to undergo demolition under the program being Prospect Plaza in Brooklyn in 2005 (Goetz, 2012; Fernandez, 2010).

1.2.2 Data

I assemble a spatially disaggregated data set on public housing in New York City from 1930 to 2010. The primary data source for New York City is the United States population census, which I augment with data on public housing projects and the construction environment in 2002 and 2010.

Demographic information The basis for the analysis are historical data on New York City from the United States federal census from 1930 to 2010 on census tract level. The outcome variables of interest from the census are demographic tract characteristics such as total, white and black population[5] Using information on public housing residents in a tract in each year from the NYCHA development data book allows me to distinguish between private residential population. A challenge when building a geographical panel level data set are boundary changes over time. Census tract boundaries experience substantial changes throughout most of the 20th century, especially for Brooklyn, Kings County and Queens County. Therefore, I adjust the earlier tracts to 2010 census tract boundaries using overlapping area weights to obtain a balanced panel. A potential drawback of this procedure is that it assumes tract-level observations are uniformly spatially distributed. I check the robustness of this approach by comparing population statistics on Borough level to the reweighed series on Borough level. For most of the Borough deviation from the Borough average in small and mainly a problem for the year 1940. For this year the census reports population counts for health districts instead of census tracts in NYC. Details of these procedures and sources

[5]Only population, median contract rent, ownership, and black and white population are consistently available from 1930 to 2010. Other variables such as median home values, dwelling counts and unemployment are available from 1940 onwards. There are two reasons that prevent me from including median contract rent as an outcome variable.

are available in Appendix 1.C.3.

Housing market outcomes I use the housing unit counts in the federal census to measure housing stock. As discussed above, housing counts have been reweighed by overlapping area weights and in Appendix 1.C.3. Information on the number of public housing units obtained from the NYCHA development data book allows me to distinguish between private construction within a tract and public construction. To obtain private market rental information, I digitize rent prices and ask price levels from the New York Times real estate section for each decennial census year from 1930 to 2010 to investigate how public housing affected rents. Only properties for which exact address or cross-street information was available have been used to ensure the correct geolocation, and the Google Maps API has been used to geocode the rental data. Moreover, listings were required to have at least information on dwelling size. Using property-level rent data has come at an advantage and a cost. First, it avoids the drawbacks of the census dataset. The census data are generally top-coded and only allow respondents to select given price ranges, though this varies across years. Moreover, the reported median contract rent on the tract level likely captures the rent paid in a public housing unit rather than market rents. The cost of using newspaper data, notably the New York Times, is twofold. First, given the nature of the New York Times as an upper-middle-class newspaper, properties in there may not be a representation across all market segments and are biased towards the upper end of the market. However, any newspaper does not cover the bottom end of the market. Second, a considerable drawback of this data is that it is biased towards Manhattan, and only specific areas like Midtown or the Upper West and East Side are continuously covered. Appendix 1.B Figure 1.B.3 shows the spatial extent of the data and the location of tracts with public housing units. Another drawback of the rent is that it reflects the upper end of the market instead. Therefore, the results would only be representative of a subset of the real estate market. I show the full description of the collection procedure, summary statistics, and an example image of the source in Appendix 1.C.2, Table 1.C.5 and Figure 1.C.4.

Public housing characteristics I amend the census data with information on public housing projects from 1936 to today, which allows testing for potential channels through which public housing could affect its neighborhoods. I obtain this information from the NYCHA Development Data Book, available annually from 1948 to today. It provides information on funding sources, population, size, rent per room, type of development, construction and development costs of each project, and the construction date. Information for the year 1940 in the NYCHA Development Data Book is inferred from archival sources from the Wagner and LaGuardia Archives. Moreover, I augment this data with information on racial composition, such as the number of white, black, Hispanic, and Chinese residents of the projects obtained from the Wagner and LaGuardia Archives for all projects constructed until 1971.

I spatially match public housing projects with 2010 census tracts to obtain the area share of a tract designated for public housing. However, this results in some projects being situated in two tracts because of their size. To adjust for this, I reweigh demographics, apartments, and ground coverage by the area of the given project as a share of the total project area. Finally, I obtain information on maintenance requirements for NYCHA developments in US dollars for 2011. An example of race statistics is shown in Figure 1.C.4.

Crime To identify potential disamenity effects generated by public housing, I obtain 2020 tract-level State incarceration rates. These data have been sourced from the New York State prison population numbers, and incarceration rates at the census tract levels are from the Prison Policy Initiative (PPI). They reflect the number of individuals incarcerated in a New York State prison during the 2010 census count. Only individuals with a valid New York State address were allocated to their residential census tract prior to imprisonment. Consequently, neighborhood incarceration rates are derived from the census tract of residence before incarceration, independent of the prison's location. Figure Figure 1.B.3 in Appendix 1.B illustrates the geographic dispersion of incarceration rates.

Sample The final sample consists of a panel of 2,164 census tracts based on 2010 tract boundaries per year from 1930 to 2010. The final set has 225 public housing tracts and about 1,500 rental observations per year. All prices and costs had been deflated by the CPI deflator and normalized to the 2010 CPI level. Summary statistics for the main outcome varoables are provided in Table 1.C.1, detailed rental statistics are shown in Table 1.C.5 and public housing statistics can be found in Appendix 1.C.1. The following section describes the empirical strategy to estimate the causal effects of public housing and further transformations of the data.

1.3 Empirical strategy

To assess the long-term impact of public housing on population and rents, I employ a difference-in-differences (DiD) approach. This method utilizes the differences in the timing of public housing construction across the city. I assign treatment at the tract level based on whether a tract had a public housing project at least once in a census year. The completion date is used as the relevant event triggering the effect, as commonly used in the literature (Asquith **andothers**, 2023; Pennington, 2021).[6]

The main challenge in the empirical analysis is selecting a suitable comparison group that accurately reflects what would have occurred in the absence of public housing. Ideally, one would conduct an experiment randomly assigning public housing projects to census tracts. However, such an experiment is not feasible. Instead, I must address the concern that the allocation of public housing across the city can be correlated with pre-construction tract and household characteristics. For example, construction sites were chosen based on the price of land and the population density, which makes such tracts more likely to be selected for construction than those without. Another challenge derives from the allocation procedure of NYCHA. Anecdotal evidence suggests that NYCHA selected its tenants from nearby areas rather than considering their location choices (Goodman, 2019). Thus, given that I am interested in the ethnic composition of the population as the outcome variable, I have to rule out mechanical changes in population composition at the neighborhood level – larger than tracts – stemming from the ethnic composition of the projects themselves.

To address these challenges, I utilize a stacked difference-in-differences design following Blanco **and** Neri (2023) that uses the variation in proximity to public housing projects to define the comparison group. I create rings of census tracts around each treated tract to define proximity and construct two rings of tracts around each treated tract. The outer ring serves as the comparison group to treated tracts and tracts in the inner ring. Treated tracts have been excluded from any other first or second ring, ensuring that the control group of each treated tract solely consists of never-treated tracts. Doing so for each project requires to append these tract-project rings such that tracts my occur several times in the dataset. Figure 1.3 in Panel 1.3a illustrates the spatial layout of fixed tract rings and overlapping tracts.

[6] Since I use decennial census years, the first time a project is observed after completion is at the end of the corresponding decade. For example, projects completed from 1961 to 1970 will be observed as treated in 1970. Thus, treatment effects in a given census year are a weighted average of all projects within a given treatment year cohort or period.

The analysis is conducted at two levels: census tract-level outcomes and property-level rental data.

Because tracts have fixed boundaries, proximity is defined by being adjacent to a public housing project. I construct two rings of tracts around each treated tract. The outer ring serves as the comparison group to treated tracts and tracts in the inner ring. Doing for each project requires the to append these such that rings my occur several times in the dataset. Treated tracts have been excluded from any other first or second ring, such that the control group of each treated tract solely consists of never-treated tracts. Finally, I create a dummy for each ring and interact it with pre- and post-treatment year dummies for the corresponding project. Figure 1.3 in Panel 1.3a illustrates the spatial layout of fixed tract rings and overlapping tracts.

The analysis is conducted at two levels: census tract-level outcomes and property-level rental data.

Census Tract-Level Analysis First, I am using data from the decennial census, including total population and the number of black and white people. The key assumption is that, in the absence of public housing, population statistics would change similarly in both the treated tract and the tracts in the control group. Any differences in outcomes should only be due to the impact of public housing.

The validity of this strategy requires balanced demographics and rents across control and treatment groups prior to treatment. I test this by checking if significant differences exist in treatment probability based on outcome variables, as reported in Appendix 1.C. For example, a one percent increase in the black population significantly affects the likelihood of being treated, so I control for this baseline characteristic.[7] For census outcomes I estimate the following event study equation at the census tract/property m, project p, and year t level:

$$y_{m,p,t} = \sum_{r \in R} \sum_{\tau=-60}^{60} \beta_{\tau,r} \left(t - Y_p, r = r(m,p)\right) + \delta' \mathbf{X_{m,p,t}} + \rho_{p,t} + \zeta_{p,r(m,p),c} + u_{m,p,t} \tag{1.1}$$

The parameter of interest, denoted as $\beta_{\tau,r}$, captures the effect of the arrival of public housing on demographics over time in each treated tract, relative to tracts in the outermost rings. I interact each time dummy with an indicator for the ring $r(m,p)$ in which a tract or a housing unit m around project p is located. Y_p denotes the year when a project p was completed and the set of rings is defined as $R = \{Treated, 1st\ ring\}$.

[7] These results are confirmed using a non-stacked panel in Table 1.C.3.

Project-specific controls are included to capture variations in the evolution of outcome variables across rings for each project. Project-census year fixed effects ($\rho_{p,t}$) account for time patterns across all rings surrounding each project p, while project-ring-neighborhood (NTA) fixed effects ($\zeta_{p,r(m,p),c}$) control for baseline differences of tracts across each ring, allowing for differences among tracts located in the same neighborhood but on opposite sides of a ring. This pattern is similar for property level outcomes only that I control for project-ring-tract fixed effects ($\zeta_{p,r(m,p),c(m)}$).

Property-Level Analysis For rental property data, I match geocoded properties with the corresponding tracts. The spatial layout of matched rental listings for two public housing projects is shown in Figure 1.3 Panel 1.3b. A rental observation is considered to be treated if it is located within a treated tract. I compare properties within a treated tract and within the first tract ring to those properties in the third ring. Properties may appear in multiple rings, as tract rings may overlap. If treated properties occurred in a control ring, they were dropped.

The identifying assumption is that, without public housing, rents change similarly in both rings, and any difference in rents should solely reflect the impact of public housing. I estimate the following event study equation at the property i, census tract m, project p, and year t level:

$$y_{i,m,p,t} = \sum_{r\in R} \sum_{\tau=-60}^{60} \beta_{\tau,r} \left(t - Y_p, r = r(m,p)\right) + \delta' \mathbf{X_{i,m,p,t}} + \rho_{p,t} + \zeta_{p,r(m,p),c} + u_{i,m,p,t} \tag{1.2}$$

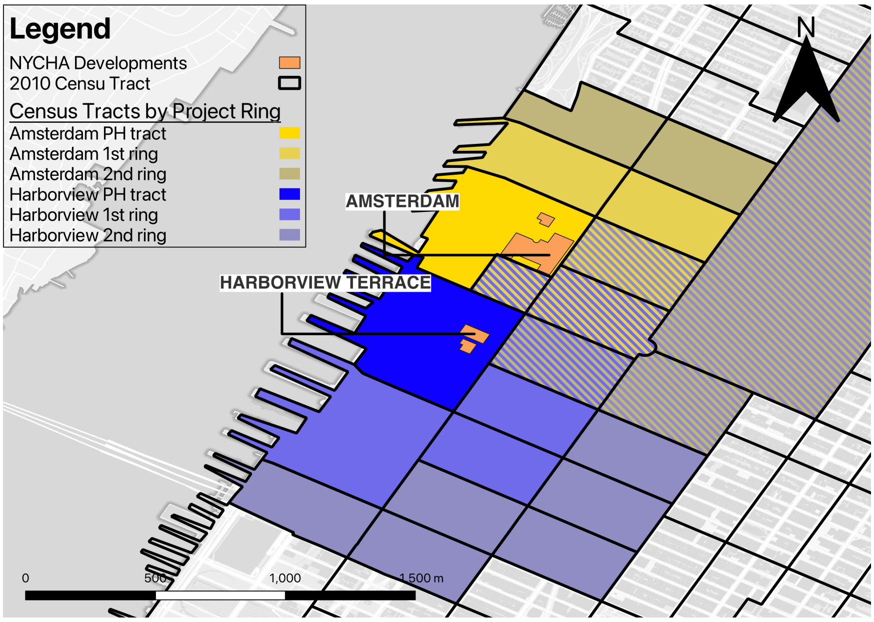
$\beta_{\tau,r}$, captures the effect of public housing on rents for properties within a treated tract, relative to properties in the outermost rings. I interact each time dummy with an indicator for the ring $r(m,p)$ in which a tract or a housing unit m around project p is located. Y_p denotes the year when a project p was completed and the set of rings is defined as $R = \{Treated, 1st\ ring\}$.[8] The vector $\mathbf{X_{i,m,p,t}}$ includes property characteristics such as the number of rooms and whether the dwelling was furnished and had AC, water, or heat included in the rental price.

In both the census tract-level and property-level analysis, I incorporate controls with project fixed effects. By allowing controls ($\mathbf{X_{m,p,t}}$) to vary by project, $\beta_{\tau,r}$ becomes a weighted average of project-specific treatment effects. This is equivalent to running equations Equation 1.1 and Equation 1.2 separately for each project and

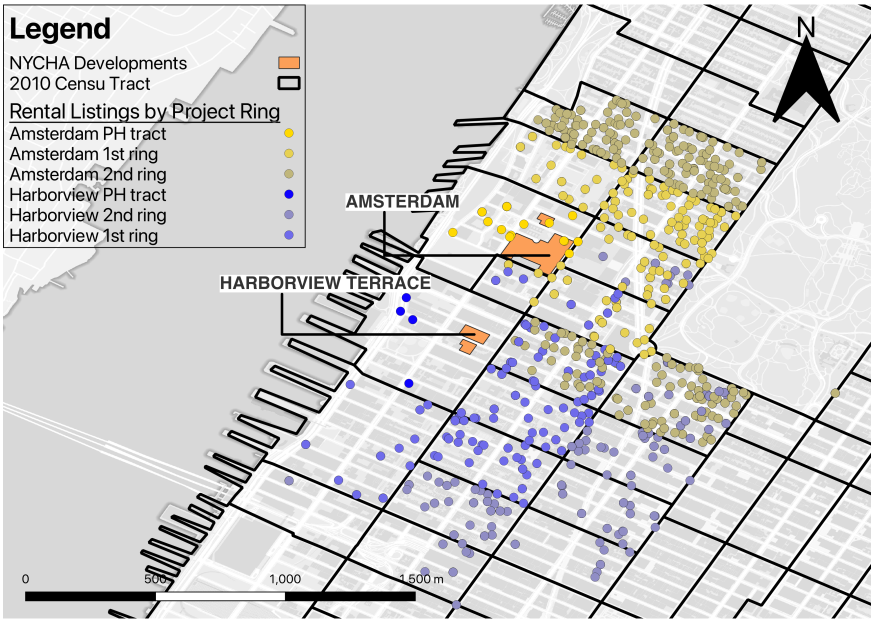
[8]Instead of using census tracts, I also use flexible distance rings around projects to utilize the granularity of the property level rental data. I use 250m, 300m, 350m and 400m radii. The sets of rings for alternative radii are $\{0-250m, 250-500m\}$, $\{0-300m, 300-600m\}$, $\{0-350m, 350-700m\}$ and $\{0-400m, 400-800m\}$. Properties in the third ring are the omitted category.

Figure 1.3: Treatment construction

(a) Census Tracts by Project Ring



(b) Rental Listings by Project Ring



Note. Panels (a) and (b) provide an illustrative example of overlapping neighborhood/distance rings for two public housing projects: Harborview Terrace and Amsterdam Houses. In Panel (a), the concept of neighborhood rings is depicted, with blue and yellow hatched census tracts representing the areas that belong to the respective public housing tracts and are located within their respective rings. It is important to note that these tracts may appear multiple times in the dataset. If a public housing tracts was lying within a neighborhood ring to another public housing tracts, it was excluded from the respective ring such that no treated tract appears in the control group.
Panel (b) shows rental listings matched to the respective census tract ring. Blue listings belong to Harborview Terrace and Yellow listings to Amsterdam houses. Different shades of blue and yellow indicate the census tract ring a property is located in. Similar to Panel (a), properties that lie within both rings will be classified as belonging to the respective project-ring, potentially appearing multiple times in the dataset.

then combining the coefficients using regression weights.[9] Specifically, project years are weighted by the frequency of tracts in each ring. Standard errors are clustered at the neighborhood (NTA) level for census tracts and at the project level for the project regression.[10]

However, this estimation strategy has a significant limitation that needs to be addressed. Specifically, it does not take into account general equilibrium effects, where projects could impact rents and population across the city. Projects can make certain neighborhoods more or less attractive, affecting the demand for different ethnic and income groups. Additionally, projects can increase the supply of low-income housing in the city. The overall effect on the city level should be minimal, with the most significant impact being concentrated near the projects. There is a concern that individuals may move to nearby areas, which would violate the Stable Treatment Unit Value Assumption (STUVA). In the Appendix 1.C Figure 1.C.1, I show the deviation of the primary outcome variables by treatment and control group from the long-run trend of the average tract in the rest of New York City. The treatment group deviates substantially from the rest of New York City over time, while the control group closely follows the overall city trend. If individuals sorted themselves into the control areas, we would expect those areas to differ from the average trend in the rest of the city. If significant city-wide effects exist, my estimates could be underestimated, but the relative comparisons across rings would remain unaffected. Additionally, rent prices are forward-looking, so the effects on prices should start when information about construction first arrives. These anticipation effects are absorbed, as treatment effects are averages of all projects completed at any time within a census decade, and estimates are a composite of anticipation and completion effects.

1.4 Reduced form estimates

I present two sets of findings. First, I show the long-run effects of public housing construction on rent prices and the existing housing stock. Results reveal only very light effects on the housing market. Second, I report population and racial

[9]A stacked difference-in-differences design is a reliable approach for accounting for heterogeneous treatment effects, something traditional difference-in-differences estimators may not be able to handle effectively (Callaway **and** Sant'Anna, 2021; Sun **and** Abraham, 2021; Borusyak **andothers**, 2021).

[10]In Appendix 1.D.4, I report event study results using four alternative estimators that correct for the shortcomings of standard two-way fixed-effects (TWFE) models. In particular I am using the de Chaisemartin and D'Haultfoeuille estimator (De Chaisemartin **and** D'Haultfœuille, 2020); Callaway and Sant'Anna estimator (Callaway **and** Sant'Anna, 2021); Sun and Abraham estimator (Sun **and** Abraham, 2021); and Borusyak imputation estimator (Borusyak **andothers**, 2021). I am estimating a dynamic TWFE specification in a panel setup at the census tract level.

composition results over the long run from 1930 to 2010. Reduced form estimates show a substantial decline in white population in treated and adjacent tracts.[11]

Effects on Prices: Rents and Construction Public housing construction has a zero effect on private market rent prices in treated tracts while having minor positive effects in its immediate surroundings. Panel (a) and (b) in Figure 1.4 display the effects on total units and units net of public housing units in a given census tract. Housing units have been normalized by the land available for construction in each tract. As outlined in Section 1.2.1, public housing replaces the existing stock while adding only slightly to it. There is a positive effect of construction on total housing units in a census tract, adding up to 15% of the stock of total units as compared to the control ring. However, private residential units are significantly reduced. The results suggest that public housing construction leads to a decline of private housing units of 50% on average, stabilizing immediately after construction. Pooled results in Figure 1.D.1 show a long-run differential decline in private units from -45% to -67%. Moreover, I report small but significant declines of units of -7% in tracts within the first ring.

Figure 1.4 Panel (c) plots event study results for property level rental data. Housing units within a treated tract and and those in the first ring experience no effect relative gains to the omitted group (units in the outermost ring). However, properties in treated tracts exhibit positive rent effects 50 to 60 years after public housing construction. I report pooled results in Appendix 1.D.1, Figure 1.D.0g. Medium-run estimates for rents in treated tracts are insignificant and range from -1.3% (0-30 years after construction) to 0% (40-60 years after construction). Thus pooled effects cancel out the rent hikes in the last decades after public housing construction.[12]

An important dimension of heterogeneity is the construction period. Not only was the housing program substantially altered after 1970, also the type and extend of buildings changed. I investigate these changes by estimating Equation 1.1 for buidlings constructed before and after 1970. An adavantge of the tacked research design is that all projects belomging to either and their rings can be easily dropped. Buildings constructed after 1970 have no significant effect on rent prices within the first and second ring 30 years after construction (see Appendix 1.D.2).

However, this result is subject to significant limitations. Firstly, the rental listings reflect the upper end of the market and only capture a particular market

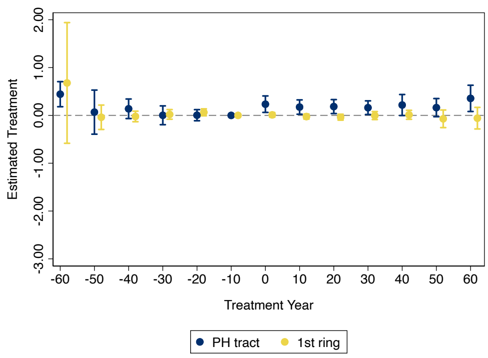
[11]Because I find substantial effect sizes, I convert all point estimates from log points to percent using $exp(\hat{\beta}) - 1$.

[12]These results are confirmed using alterantive distance rings (see Figure 1.D.1)

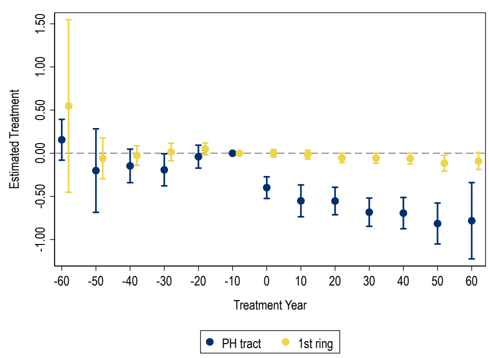
segment. Second, because of the geogrpahic concentration of the data, the results are biased towards the effect of public housing construction in Manhattan rather than in New York City.

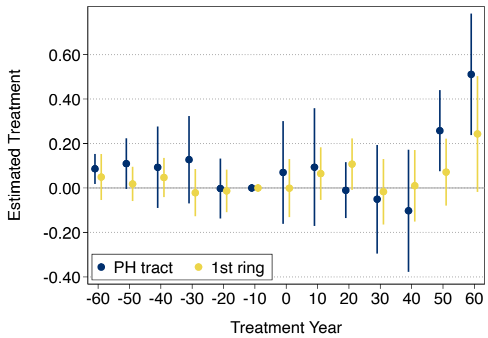
Figure 1.4: Effect on rents and housing unit

(a) Housing units



(b) Priv. housing units





(c) log(Rent)

Note: Figure 1.4 plots report coefficients $\hat{\beta}_{\tau,r}$ in Equation 1.1 and Equation 1.2; standard errors are clustered at the project level; the vertical lines show the estimated 95% confidence intervals. Panel 1.5c uses property level rent data controlling for property characteristics such as number of rooms, if heating, water and furniture were included in the rent; Panel 1.4a and 1.4b use housing counts from the US census; all estimates are weighted by the freqencey of observations within a rings; the omitted category are tracts within a second ring.

These results align with research on public and affordable housing investments in New York City. First, as shown by Ellen **andothers** (2007) federal public housing constructed between 1977 and 2000, federally subsidized developments have not typically led to reductions in property values and have led to increases in some cases. Furthermore, Schwartz **andothers** (2006) showed that investment in subsidized housing between 1987 and 2000 increased with project size and decreased with distance from the project sites. In this paper, I show that later-constructed public housing projects do not cause prices to fall in the immediate neighborhood. Moreover, I report for the first time the long-run consequences of federally subsidized developments built during the program's height in the 1950s and 1960s, which are large-scale compared to most buildings completed after the 1970s. Those were low scale with three to four stories and had moderate densities. Moreover, the result reflects more recent research on public housing demolitions in that there are adverse long-run effects on rent prices (and potentially property prices) (Blanco, 2022; Hunt, 2009).

Population and racial composition Public housing construction significantly impacts demographics by shifting population over space. Project construction changed the racial composition of neighborhoods through resident selection. Figure 1.5 displays results from estimating Equation 1.1 on total tract population, tract population minus public housing population, and white and black population. Pooled estimates are reported in Appendix 1.D.1 Figure 1.D.1. Since I take the natural logarithm of the outcome variables, $\beta_{\tau,r}$ can be interpreted as the percentage difference in the outcomes between the respective ring and control, holding all other factors constant.

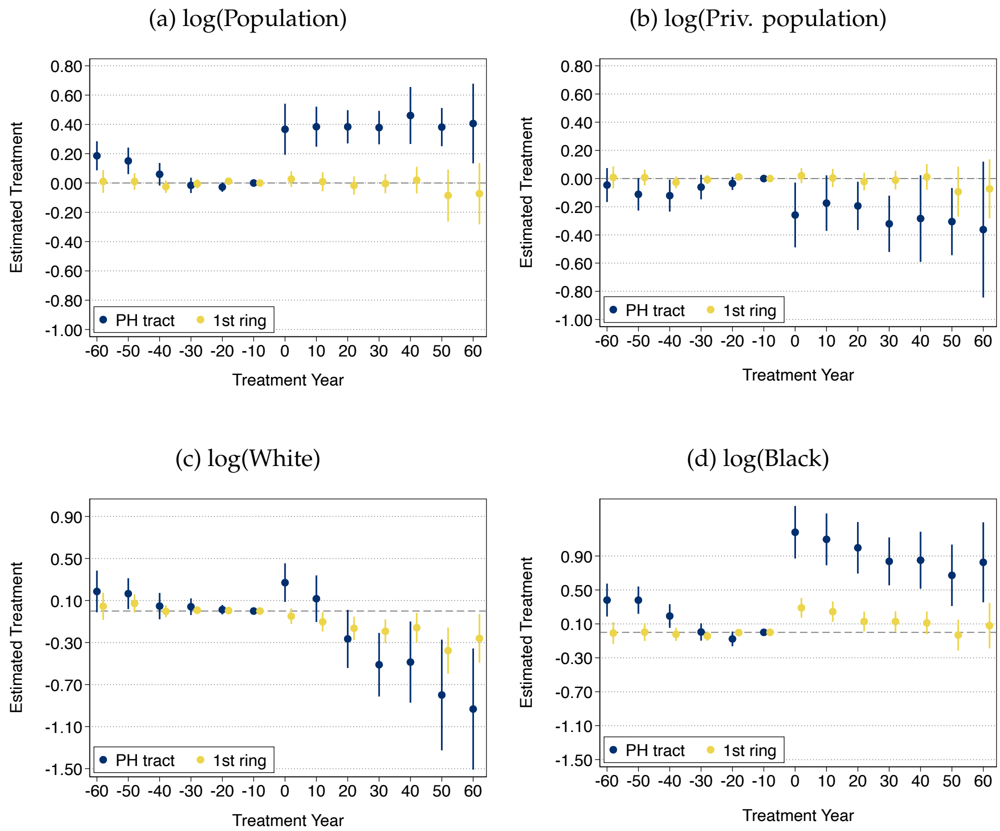
Panel (a) shows that tracts with public housing projects experience an increase of up to 27% in total population relative to the omitted group (tracts in the outermost ring), a figure that goes down to a fall in total population by 9% within the first ring. This effect is highly significant. The population living in private developments significantly decreases by 31% in treated areas (Panel (b)) while the effect for the first ring is similar (–9%). While those effects set in immediately and are stable for the whole observation period in the treated tract, the short-run effect within adjacent areas reports a population decline from –7% (0-30 years after construction) to a long-run decline of 15% (0-30 years after construction) (See Appendix 1.D.1 Figure 1.D.1).

Panel (c) and (d) show the effect of public housing construction on the log of the total white and black populations. The arrival of public housing increases white population slightly for up to two decades after construction before entering a longer decline. Results from estimating Equation 1.19 reveals that within the first 30 years after construction, the white population declined by about -17% percent, further falling to –72% for 40 to 60 years after construction. Moreover, there is a significant long-run decline within nearby tracts, totaling about -34% in the long run. Conversely, the overall black population increased by about 80%. In contrast to the white population, there is no significant negative effect on the total black population in the broader area.

Results closely reflect the pattern of allocation as outlined under Section 1.2.1. The short positive effect for the white population reflects the overall allocation pattern for newly constructed projects and the corresponding outflow of whites from the projects. Similarly, black population intake in projects increases population immediately after construction in the long run. Therefore, construction is associated with a change in the racial composition of tracts in which public housing is constructed relative to the outmost ring.

The magnitude and behavior of the estimated effects suggest that they are very local and have no general equilibrium consequences. Second, potentially, all population movements for the black population are absorbed by the project itself. Tract population is instead exchanged, and spillover materializes only for whites, indicating differential population losses in nearby areas are due to declines in population.

Figure 1.5: Effect on demographics



Note: Figure 1.5 plots report coefficients $\hat{\beta}_{\tau,r}$ in Equation 1.1 for each treated tracts and rings around a project; standard errors are clustered at the project level; the vertical lines show the estimated 95% confidence intervals; the omitted category consists of tracts within a second ring. Panel 1.5a to 1.5d use weighted unit counts from the US census; estimates have been weighted by frequency by ring; the sample includes 2162 time-consistent census tracts in New York City.

An essential dimension of effect heterogeneity is the period in which projects have been constructed. As shown in Section 1.2.1, the type of building changed from "Tower in the Park" style buildings - slim high-rises with low ground coverage - to small projects on scattered sites. This shift occurred mainly in the 1970s and was accompanied by legislation fostering vouchers for private-sector apartments. I thus estimate Equation 1.1 separately for projects constructed before and after 1970. This allows me to use data from the La Guardia and Wagner archives on the racial distribution of projects available until 1970. I, therefore, can distinguish between white and black residents in a census tract living in public housing and in private developments. This information, though, is only available for projects consecrated before 1972. I assume that black and white population in those projects remains constant afterwards. Results are shown in Appendix 1.D.2 Figure 1.D.3. Population estimates for the construction period after 1970 in Panel 1.D.3b have no significant effects and are small as compared to the pre-1970 period (Panel 1.D.3a). Similarly, the effect on the overall white and black population as reported in Panel 1.D.3e to 1.D.3h exhibit a similar pattern. While the white population declines gradually, the effect on the black population stabilizes around 30 years after construction. In summary, Figure 1.D.3 reveals that the long-run effects in Figure 1.5 are entirely driven by public housing projects constructed before 1970 and can therefore be attributed to the "Tower in Park" style of housing.

1.5 Mechanism

In this section, I examine how public housing impacts the decline of the white population in treated and adjacent areas. I focus on two potential factors. The first factor relates to the effect of public housing on replacing existing housing and changing the urban layout of those areas. Urban theorist Jane Jacobs criticized the construction style known as "Tower in the Park' - tall buildings with various layouts on a shared plot and large green spaces in between - for creating unsafe and uninviting places. This is because these buildings provide large, unmonitored indoor and outdoor spaces that lack the care of residents and shop owners (Jacobs, 1992). Architect Oscar Newman associated "Tower in the Park" with crime in his theory of defensible space, suggesting that the design contributes significantly to differences in crime rates (Newman, 1997). Therefore, project design could create disadvantages or disrupt intact neighborhoods. The second factor is that public housing projects influence the composition of residents. This influence can occur because individuals have direct preferences regarding the racial makeup of project residents or because public housing residents indirectly impact local public services, such as schools or crime. In both cases, based on preferences for public housing residents and the building's layout, white population might tend to avoid living near public housing projects. I use a comprehensive set of public housing characteristics to understand these effects better.

1.5.1 Building Design

A challenge to test for building design following the argument in Jacob lies in the lack of a clear definition of a "Tower in the Park". However, following Jacobs, it must have two main criteria: it must be of sufficient height with a sufficiently low ground coverage. To determine a height threshold, I use the requirements of the New York Department of Buildings, which states that a building with more than 75 feet is considered a high-rise building.[13] Given a legally minimum required ceiling height of 7′6 feet, a "Tower" would have at least 10 stories.[14] This is slightly below the average public housing building height of 11 levels. To determine a threshold for ground coverage, I use the average of 24% across public housing projects. However, it might be that some tower buildings are too small, meaning that they only consist of a single high-rise building, which would be easily integrated into the city environment and not make any significant alterations to the broader area. Importantly, Jacobs is arguably thinking of an ensemble of buildings that removed cross streets and is not integrated into the city's fabric (Jacobs, 1992). Moreover, certain projects are rather small compared to the overall tract area. Therefore, I amend the above definition of a "Tower in the Park" by making sure that these buildings are sufficiently large relative to their tract, using the area used for public housing construction as a share of the total tract area. I use the average area share of 26% as threshold to derive an adjusted classification of a "Tower in the Park", which gives 22 Tower and 203 non-tower tracts.[15] I estimate the following equation:

$$\begin{aligned} y_{m,p,t} = &\left(\theta_{0r} Post^{0-30}_{p,t} + \theta_{1r} Post^{40-60}_{p,t}\right) \times (Tower + No\ tower) \\ &+ \delta' \mathbf{X_{m,p,t}} + \rho_{p,t} + \zeta_{p,r(m,p),c} + u_{m,p,t} \end{aligned} \tag{1.3}$$

This estimation is similar to Equation 1.19, where $Tower$ and $No\ tower$ are dummies for tracts having towers in the park-like projects and not. I show pooled estimates in Figure 1.6.[16] Point estimates for white population in treated tracts with a "Tower"-style project are more pronounced (-34% (0-30 years) and -79% (40-60 years)), with stronger spillover effects (-22% (0-30 years) and -36% (40-60 years)). The effects of non-tower buildings are only slightly smaller, with the

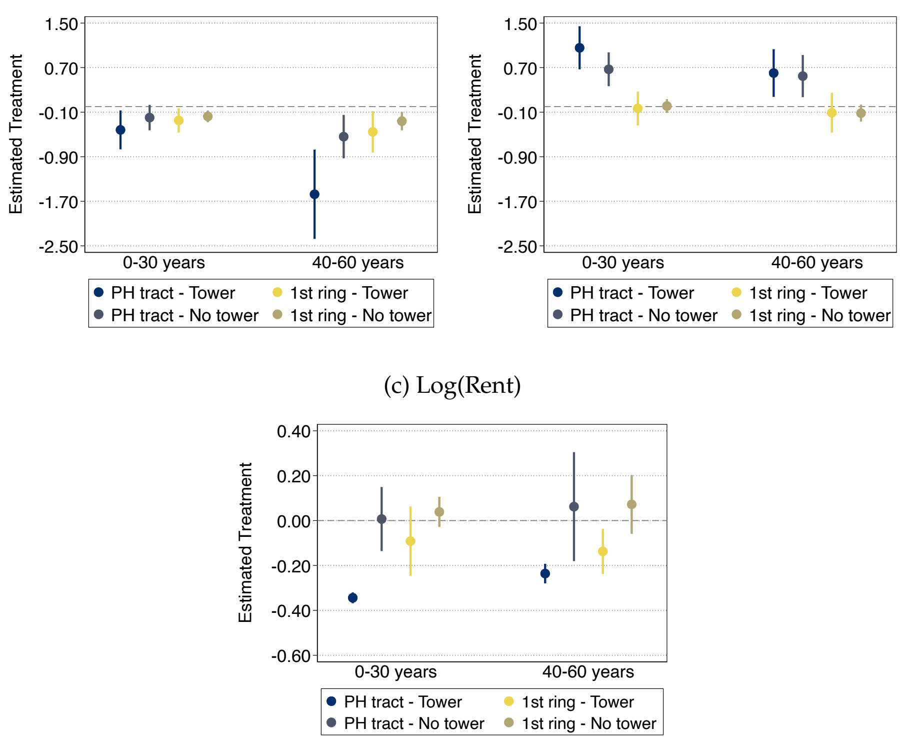
[13] Key Project Terms: Educational and Institutional

[14] Design Professional Requirements: Creation and Alteration of Habitable Apartments In Basements or Cellars of 1 and 2-Family Buildings

[15] In Appendix 1.D.1 Figure 1.D.3 I relax the assumption on area share and just rely on heigt and ground coverage as criteria.

[16] I report event study results in Appendix 1.D.3.

Figure 1.6: Effect of "Tower in Park"



(a) Log(White)

(b) Log(Black)

(c) Log(Rent)

Note. Figure 1.6 reports point estimates for coefficients θ_{0r} and θ_{1r} in Equation 1.3; all coefficent have been interacted with adjusted Tower dummies; standard errors are clustered at the project level; the vertical lines show the estimated 95% confidence intervals. Panel (a) to (b) report differences for treated tracts and tracts in the first ring compared to a second neighbour ring; Panel (c) compares properties within a treated tract and in the second tract ring around projects to those within a third ring.

long-run decline of the white population of 42% and 23% in treated and adjacent tracts (Subfigure 1.6a). The effects for black population are similar in pattern and magnitude for "Towers".

Rent prices fall within public housing tracts with "Towers" by around 30% (0-30 years)) and by 21% (40-60 years). Though not significant, point estimates in the second ring around "Towers" are negative (-13%, 0-30 years) and fall by 16% in the long run (40-60 years). Estimates for non-tower tracts are close to zero in short run (0-30 years). Only long run estimates ar slightly positive though insignificant (Subfigure 1.6c). This indicates negative demand effects to living near "Tower" buildings.

1.5.2 Public Housing Residents

While I previously focused on the design aspects, such as the "Tower in the Park" model, this chapter shifts the focus to the composition of public housing residents. This section hypothesizes that the observed changes in white population in treated and adjacent areas are driven by changes in the demographic composition of public housing projects. Specifically, substantial changes in a project's resident composition over time could explain the movement of the White population, particularly due to the intake of Black residents.

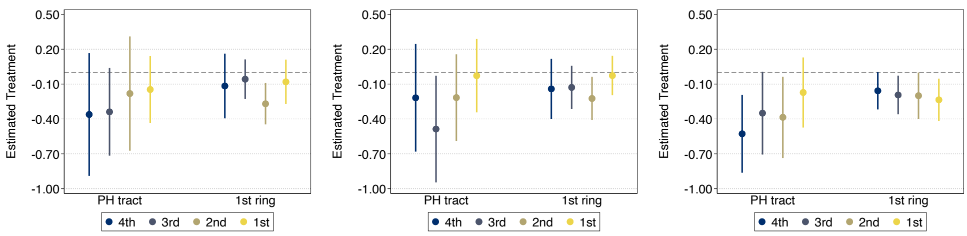
To test this tipping mechanism, I use the number of black public housing residents as of December 1971 and the change in black residents from the initial opening date to December 1971. Additionally, I use the total public housing population to test for preferences toward residents in general. In the empirical model, I interact ring dummies with quartiles of the respective public housing characteristic:

$$
\begin{aligned}
y_{m,p,t} = &\sum_{q\in Q} \left(\gamma_{0q} PH\ tract_{p,t} + \gamma_{1q} 1st\ ring_{p,t}\right) \times \mathbb{1}(q = q(m,p)) \times age_p \\
&+ \delta' \mathbf{X_{m,p,t}} + \rho_{p,t} + \zeta_{p,r(m,p),c} + u_{m,p,t}
\end{aligned}
\tag{1.4}
$$

where $\mathbb{1}(q = q(m,p))$ is an indicator if a project's p characteristic in tract m lies in the respective quartile $Q = \{1,2,3,4\}$ and $\beta_{0,q}$ and $\beta_{1,q}$ can be interpreted as the average effect on units in ring $r \in \{0,1\}$ in quartile q.

Figure 1.7: Effect on log(white)

(a) Change in black residents (b) Black residents Dec. 1971 (c) Public housing resident

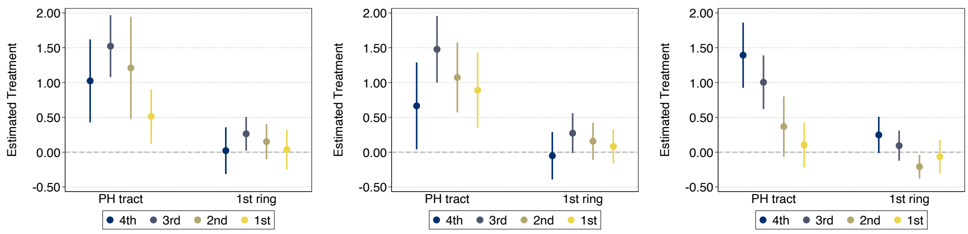


Note. Figure 1.7 reports point estimates for coefficients γ_{0q} and γ_{1q} in Equation 1.4; both coefficients have interacted with quartiles indicators of the distributions of the change public housing residents from initial occupancy to Dec. 1971, the black resident population as of Dec. 1971 and the average total public housing residents within a treated tract; the vertical lines show the estimated 95% confidence intervals. Panel 1.7a to 1.7c report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; outcome variables are obtained from the US census.

Results are shown in Figure 1.7 to 1.9. Importantly, there are no significant effects of the black population and the change in black residents on the white population neither in treated nor in adjacent tracts (Figure 1.7a and 1.7b). However, point estimates are large, ranging from 14% to 30% in the first and fourth quartile. Point estimates in adjacent tracts are small, with about 8% (q1) and 11% (q4). Effect sizes for the distribution of black residents are similar, with the strongest effect in the third quartile in treated tracts (-39%). In contrast, the effects of total public housing residents on the white population ranged from -16% in the first to -41% in the fourth quartile with a public housing tract. In adjacent tracts, white population declines around all quarters of the public housing resident distribution on average by 18% across all quarters (Subfigure 1.7c).

Figure 1.8: Effect on black population

(a) Change in black residents (b) Black residents Dec. 1971 (c) Public housing resident



Note. Figure 1.8 reports point estimates for coefficients γ_{0q} and γ_{1q} in Equation 1.4; both coefficients have interacted with quartiles indicators of the distributions of the change public housing residents from initial occupancy to Dec. 1971, the black resident population as of Dec. 1971 and the average total public housing residents within a treated tract; the vertical lines show the estimated 95% confidence intervals. Panel (a) to (c) report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; outcome variables are obtained from the US census.

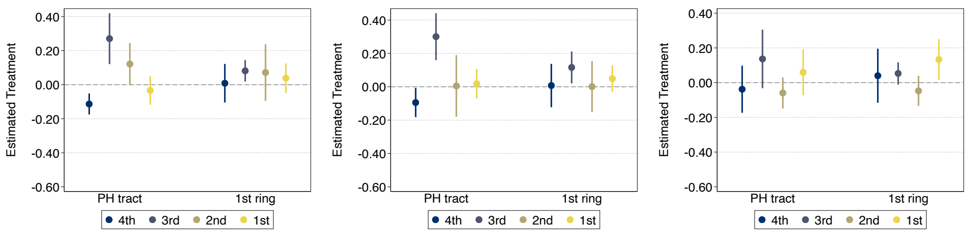
The effect of the change in public housing residents reveals that black residents increase from 67% (q1) to 179% (q4), though the effect is strongest in the third quartile (359%) (Subfigure 1.8a). The effects of total public housing residents on black population show that most of the increase in black reported in Figure 1.5 stems from black public housing residents, that is, treated tracts witness an increase of black population ranging from 11% in the first to 303% in the fourth quartile of the total public housing resident distribution. On average, there are no significant spillover effects besides a 19% decline in black residents around tracts in the second quartile of the public housing resident distribution (Subfigure 1.8c).

Effects on rent prices show no consistent pattern related to Black and total public housing residents (Figure 1.9). There are significant spikes in the first ring around a project in the third and fourth quartile of the distribution of changes in Black public housing residents of 31% and -11%, respectively (Subfigure 1.9b). However, there are no significant spillover effects. Moreover, there is only a positive effect of total public housing residents in the lowest quartile in the second ring of 14% (Subfigure 1.9c).

These results suggest that the changes in the White population are not

Figure 1.9: Effect on rent

(a) Change in black residents (b) Black residents Dec. 1971 (c) Public housing resident



Note. Figure 1.9 reports point estimates for coefficients γ_{0q} and γ_{1q} in Equation 1.4; both coefficients have interacted with quartiles indicators of the distributions of the change public housing residents from initial occupancy to Dec. 1971, the black resident population as of Dec. 1971 and the average total public housing residents across all project; the vertical lines show the estimated 95% confidence intervals. Panel (a) to (c) uses property level rent data comparing rents in a treated tract and a first tract ring to properties in a second tract ring; rental ask prices have been obtained from the New York Times.

primarily driven by the presence of Black residents in public housing. The lack of significant effects in adjacent tracts and the non-monotonic relationships regarding the Black population indicate that tipping effects play a minor role. This is further evidenced by smaller effects compared to the "Tower in the Park" effects shown in Figure 1.6. However, the negative effects on the White population seem to be more closely associated with the overall public housing population.

1.5.3 Discussion

As housing affordability becomes a growing issue, this result has implications for building affordable housing. As shown in Blanco **and** Neri (2023), public housing as mixed-income housing built by private developers can mitigate adverse effects from existing patterns of poverty, such as crime, by improving school quality and nutrition of children through the same. Low-income housing can, when built by private developers, create amenable space as compared to those places before (Diamond **and** McQuade, 2019). This paper, however, is concerned with governmentally constructed and operated buildings. While previous studies take those units as generally flawed, I argue that buildings that are integrated into the urban fabric of a city can provide low-income housing while not affecting the neighborhood in an unintended way. Nevertheless, the question is what design implies. The building type driving the effects are large-scale projects with low ground coverage, often turned on themselves away from the street. Thus, as argued in Newman (1997), those buildings shave less “defensible space” – space that residents can control – and might attract more crime. In order to test this hypothesis, I perform a cross-sectional analysis using incarceration rates IR_m in tract m as an outcome. First, I run a version of Equation 1.19, which adjusts the panel set-up to a cross-sectional setup:

$$\begin{aligned} IR_{m,p} = &\left(\eta_{0r} PH\ tract + \eta_{1r} 1st\ ring\right) \times (Tower + No\ tower) \\ &+ \delta' \mathbf{X_m} + \xi_{p,n} + u_{m,p} \end{aligned} \tag{1.5}$$

Here, $\xi_{p,n}$ are project-by-neighbohood fixed effects. I compare tracts with a public housing project and adjacent tracts to a second ring further away. The vector $\mathbf{X_m}$ contains the log of the black and white population, total owners, number of college-educated, population density, and median contract rent. I use baseline controls to make sure that variables are not affected by the treatment.

Table 1.1: Public housing and crime

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables:	log(IR)		log(White)		log(Black)	
PH tract x Tower	1.22***	1.24***				
	(0.146)	(0.159)				
PH tract x No tower	1.04***	0.976***				
	(0.090)	(0.083)				
Ring 1 x Tower	-0.059	-0.067				
	(0.128)	(0.135)				
Ring 1 x No tower	0.075	0.050				
	(0.046)	(0.043)				
PH tract x Tower x IR_p			-0.276***	-0.208***	0.257***	0.307***
			(0.075)	(0.077)	(0.041)	(0.046)
PH tract x No tower x IR_p			-0.104***	-0.057	0.270***	0.258***
			(0.038)	(0.040)	(0.028)	(0.028)
Ring 1 x Tower x IR_p			-0.122*	-0.075	0.024	0.044
			(0.065)	(0.064)	(0.031)	(0.030)
Ring 1 x No tower x IR_p			-0.013	-0.007	0.041***	0.040***
			(0.017)	(0.018)	(0.016)	(0.015)
Controls	✗	✓	✗	✓	✗	✓
Project-NTA FE	✓	✓	✓	✓	✓	✓
Fit statistics						
Observations	2,785	2,695	2,762	2,672	2,811	2,722
R^2	0.71207	0.71543	0.86192	0.86290	0.81181	0.81102
Within R^2	0.03821	0.04408	0.00876	0.01970	0.02061	0.02624

Note. Table 1.1 Reports point estimates for coefficients η_{0r} and η_{1r} in Equation 1.5; all coefficients have interacted with Tower dummies; standard errors are clustered at the project level; the dependent variable in columns (1) and (2) is the log of the incarceration rate; in columns (3)-(4) and (5)-(6) the dependent variable is the log of white and black population respectively; tower dummies in columns (3)-(6) have been interacted with the log of the incarceration rate of the treated public housing tract; as tract level control variables I use the log of white and black population, median contract rent, population density, total owners and number of college-educated at baseline. Standard errors have.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Results from running Equation 1.5 are shown in Table 1.1. Columns (1) and (2) confirm that "Tower in the Park" tracts have 239% to 246% higher incarceration rates than tracts in the second tract ring. Nevertheless, non-tower tracts still have 165% higher incarceration rates after adding controls. In both cases, there is no evidence of spillover effects. This can be considered mild evidence for the hypothesis that "Towers in the Park" are more crime-ridden. However, since incarceration rates give the location of criminals, it does not indicate where these crimes have been committed. Next, I want to test if higher crime rates in "Tower" tracts can account for push factors for the white population. Therefore, I match the crime rate of treated tracts to the first ring around them and interact "Tower" dummies with project incarceration rates. Columns (3) to (6) show results from this exercise. A one percent increase in the incarceration rate around "Tower" decreased the white population by .27% and by .12% in adjacent tracts. Using controls in (2) renders these effects insignificant besides the effect of "Towers" with .21%.

This set of results suggests that the "Tower in the Park" building facilitates crime, which hints towards a negative demand effect around "Towers". It can be considered as mild evidence for the arguments by Jacobs (1992) and Newman (1997) that "Towers" are more likely to be crime-ridden. However, non-tower tracts exhibit larger crime rates than the control group and are not statistically different from Towers. Moreover, there is mixed evidence that higher crime rates in "Tower in the Park" buildings explain spillovers on the white population. However, while not spilling over into the wider neighborhood, localized incarceration rates could stigmatize the wider area, making it less attractive for the white population. While these effects are indicative that crime can be seen as a potential channel, a problem could be that incarceration rates do not reflect actual incidents. Finally, while public housing is considered to be exogenous in this cross-sectional regression, it is unclear if criminals sort themselves into public units or if public housing is incentivizing residents to commit crimes, for example, through stigma or the lack of opportunity to find employment.

Besides crime as a driver of sorting, another channel could be the removal of amenable space. "Towers in the Park" often required the remodeling of entire city blocks, which entailed decreasing space for restaurants, offices, or, more general, mixed-use developments that have the potential of providing service in close proximity. Finally, the distinct urban form makes the project visible from a distance. Thus, projects might shape surrounding areas, neighborhood reputations, property values, and residential decisions simply through the stigma associated with public housing (Tach **and** Emory, 2017). The above results are only able to disentangle those arguments partially. Changing the structures of towers, i.e., by decreasing height and increasing ground coverage, would not likely mitigate crime. Moreover, incarceration rates in public housing tracts do not significantly affect the white population in nearby tracts, which suggests that the stigma of large and, therefore, visible projects or the lack of amenable space potentially can also account for drivers of sorting. Finally, since public housing became more tailored towards lower-income households, it could entail a decline in school quality and other local public goods.

Another concern related to the quasi-experimental setting. There are many moving parts in the city and beyond. For instance, one would need to disentangle the role of suburban pull factors, the reason different areas grow differently, whether there is something inherently different about growth in one area versus another, what accounts for intergenerational transfers, etc. Each of these channels would require a different natural experiment. This is in addition to channels such as replacing amenable space, school quality, or crime, which are important but beyond the scope of this paper.

1.6 Residential sorting model

This section develops a model of equilibrium sorting by combining a discrete choice model of residential demand according to Bayer **andothers** (2007) with a model of housing supply to further study the welfare implications of public housing and "Towers in the Park" in particular. It follows the estimation procedure proposed by Almagro **andothers** (2023). I amend the model by accounting for spillover effects wich directly follow the empirical Equation 1.19. This helps in interpreting the results of the model in relation to the empirical strategy.

1.6.1 A Model of neighborhood Demand and Housing Supply

The following set-up consists of utility maximizing Indirect utility for a households n of ethnic group $g \in \{b, w\}$ which chooses her tract location m at time t is given by:

$$V^g_{nmt} = \varphi^g_{nmt} + \epsilon_{mt} \tag{1.6}$$

where φ^g_{nmt} is the component of indirect utility for census tract m that is common to all households of group g - called mean indirect utility hereafter - and ϵ_{mt} is an idiosyncratic shock which are drawn from an Extreme Value Type I distribution. The common component of indirect utility is:

$$\begin{aligned}\varphi^g_{nmt} &= \beta^g_{1t} s^w_{mtP} + \beta^g_{2t} s^b_{mtP} + \beta^g_{3t} \mathbb{1}(r=1)_{mtPH} + \beta^g_{4t} \mathbb{1}(r=2)_{mtPH} \\ &+ \beta^g_{5t} \mathbb{1}(r=3)_{mtPH} + \beta^g_{6t} log(r_{mt}) + \beta^g_{7t} log(hu_{mt}) + \beta^g_{8t} log(w_{mt}) + \xi_{mt}\end{aligned} \tag{1.7}$$

The effect of public housing is modeled by β^g_{3mt} switches to one if a tract has public housing unit or lies in the first or second ring around public housing. Thus, the dummies will capture any effect in adjacent areas caused by public housing and allows for spillover effects of public housing on utility. I use the following census variables to capture tract characteristics, where s^w_{mtP} and s^b_{mtP} are the shares of households that are white or black. Rent, r_{mt}, is measured as the medium contract rent within a tract m in year t, hu_{mt} measure housing units in tract m and w_{mt} is the median household income in the same tract. Finally, ξ_{mt} is a vector of exogenous un–observable neighborhood characteristics.

The vector $\boldsymbol{\beta^g_t} = (\beta^g_{1t}, \beta^g_{2t}, \beta^g_{3t}, \beta^g_{4t}, \beta^g_{5t}, \beta^g_{6t}, \beta^g_{7t}, \beta^g_{8t})$ contains preference parameters and may differ arbitrarily across groups as well as neighborhood unobserved quality, ξ_{mt}. I use vectors (e.g., $\mathbf{r}$, $\mathbf{s^w}$, and $\mathbf{s^b}$) to represent aggregates across the set of M – many neighborhoods. I assume that home prices are equal to the present discounted value of rents, and therefore homeowners face the same optimisation problem as renters. Given the distributional assumption on ϵ_{mt}, the probability that a household of group g chooses to live in tract m is:

$$\pi^g_{mt} = \frac{exp(\varphi^g_{mt})}{\sum^M exp(\varphi^g_{mt})} \tag{1.8}$$

The demand for living in neighborhood m equals the total number of households, across all groups, that want to live in m, assuming each household occupies one housing unit. Taking total population of group g, N^g_t , in the City of New York City exogenous yields the following housing demand equation:

$$D_{mt} = \sum_g \pi^g_{mt} N^g_t \tag{1.9}$$

As in Almagro **andothers** (2023) the model is closed by assuming an isoelastic supply function such that the number of housing units in tract m is given by:

$$S_{mt} = \delta_{mt} r^{\phi}_{mt} \tag{1.10}$$

where δ_{mt} is a supply shifter and ϕ is the supply elasticity.

Assuming exogenous city population and public housing population, the model describes an equilibrium when prices and tract demographics characteristics fulfil the following market clearing conditions:

$$D_{mt}(\mathbf{r}^*, \mathbf{s}^{\mathbf{w}^*}, \mathbf{s}^{\mathbf{b}^*}; \beta) = S_{mt}(r^*_{mt}) \qquad \forall\, m \tag{1.11}$$

$$\frac{D^b_{mt}(\mathbf{r}^*, \mathbf{s}^{\mathbf{w}*}, \mathbf{s}^{\mathbf{b}^*}, \beta)}{D_{mt}(\mathbf{r}^*, \mathbf{s}^{\mathbf{w}^*}, \mathbf{s}^{\mathbf{b}^*}; \beta)} = s^{b^*}_{mt} \qquad \forall\, m \tag{1.12}$$

$$\frac{D^w_{mt}(\mathbf{r}^*, \mathbf{s}^{\mathbf{w}*}, \mathbf{s}^{\mathbf{b}^*}, \beta)}{D_{mt}(\mathbf{r}^*, \mathbf{s}^{\mathbf{w}^*}, \mathbf{s}^{\mathbf{b}^*}; \beta)} = s^{w^*}_{mt} \qquad \forall\, m \tag{1.13}$$

A fixed point of the system of Equations 1.11 to 1.13 can be found using a non-linear optimisation procedure. I use Newton's method to solve for the equilibrium vectors ($\mathbf{r}$, $\mathbf{s}^{\mathbf{w}}$, and $\mathbf{s}^{\mathbf{b}}$) given the preference parameters $\boldsymbol{\beta}^g_t$, supply elasticity ϕ and supply shifter δ setting the tolerance criteria to e^{-10}. This is described in greater detail in Appendix 1.E.

1.6.2 Quantification of the model

To study the consequences of public housing demolitions using our model, a necessary step is to obtain estimates of the household preference parameters β, supply elasticity ϕ and supply shifter δ. The models' outside option can be normalised to zero $\varphi^g_{0t} = 0$ as is standard in the literature and the preference parameters can be identified by the following equation which is implied by Equation 1.8:

$$\log\left(\frac{\pi^g_{mt}}{\pi^g_{0t}}\right) = \beta^g_{1t} s^w_{mtP} + \beta^g_{2t} s^b_{mtP} + \beta^g_{3t} \mathbb{1}(r = 1)_{mtPH} + \beta^g_{4t} \mathbb{1}(r = 2)_{mtPH}$$
$$+ \beta^g_{5t} \mathrm{r}_{mtPH} + \beta^g_{6t} log(hu_{mt}) + \beta^g_{7t} log(w_{mt}) + \mu_m + \theta_t + u_{mt} \tag{1.14}$$

In order to estimate preference parameters, I distinguish between population living in public housing and private dwellings. This is important in order to disentangle the effect of public housing population and private sector residents. Thus, s^w_{mtP} and s^b_{mtP} are white and black population shares. The coefficients s^{b3}_{mtPH} to s^{b5}_{mtPH} represent dummies which switch to one given the distance relationship of the tract to the nearest public housing project. The third ring is the omitted group. Finally I collect neighborhood effects ξ_{mt} into the fixed effects μ_m and use θ_t census tract and year fixed effects.

Since I consider the choice to settle in a given distance in relationship to public housing as defined by neighbour rings around public housing tracts, the outside option is to live in anywhere else in New York City. Equation 1.14 can then be estimated using maximum likelihood. The dummy $\mathbb{1}(r = r(m, p))$ is equal to one if a tract lied in the respective ring and s^b_{mtPH} is the number of public housing residents in the closest project of the number of residents of in tract m. To model a social interaction effect I interact the ring dummies with the share of the closest of public housing population population. I define the choice probabilities using the share of households of group g that reside in each tract as:

$$\hat{\pi}^{g}_{mt} = \frac{\#\, residents\ of\ group\ g\ in\ tract\ m}{\#\, residents\ of\ group\ g\ in\ New\ York\ City} \tag{1.15}$$

For estimation, I addresses two main threats for credible identification of the preference parameters of the model of residential choice. First, I include a series of fixed effect terms by estimating the model using year and tract fixed effects. Second, since rent prices are potnetially correlated with neighbohood unobservables, I instrument all variables besides public housing dummies following the arguments in Berry (1994) and Bayer **andothers** (2007). The argument is that prices, housing stock and demographics for any particular tract will be affected not only by its own attributes but also by the availability of tracts that are close substitutes for it. That is, two tracts with identical characetritics may have very different prices, depending on how they are situated relative to other locations within New York City. I construct a set of instruments based on tract characeteristics 1.5 to 2.5 miles around each tract used in the analysis to account for this pattern of substitution and isolate exogenous variation in public housing. I use the average development inensity, or the number of housing units devided by the total tract area, $avg.\ develop.\ intensity_{1.5-2.5}$, the average share of white and black population, $avg.\ share\ white_{1.5-2.5}$ and $avg.\ share\ black_{1.5-2.5}$, the average public park area share, $avg.\ share\ park_{1.5-2.5}$ and the average population density $avg.\ pop.\ dens._{1.5-2.5}$.

Thus, I obtain $\boldsymbol{\beta}^g$ by an IV regression of the vector of mean indirect utility where rent prices, housing units, income and demographics are instrumented with tract chracteristics further away. As argued under Section 1.4, conditional on controls and fixed effects the location of public housing is random, therefore the effect of public housing as modelled can be interpreted as causal. Both fixed effects vary arbitrarily by ethnic group. Instead of using a stacked design as described in Section 1.3 I use a panel version. If a tract is a neighbour to two project it becomes activated as neighbour of the nearest first treated public housing project. Figure 1.B.2 shows the spatial layout of the panel data. In order to estimate the preference parameters I am interest in a world with public housing. Thus, for each census year I estimate Equation 1.15 only for the time after treatment for treated tracts and their neighbours to build a meaningful counterfactual.

One main caveat for the interpretation of the estimated coefficients in Table 1.2 is that as reduced–form parameters they reflect the combined impact of additional preferences that are not explicitly modeled. For example, white households might prefer to live in neighborhoods with a higher White population share because of racial animus, preferences for public goods that are associated with demographic composition, or preferences for particular types of consumption amenities. However, I treat the intake of public housing residents as exogenous conditional on having a public housing or not. Table 1.2 displays results from estimating Equation 1.15 for white and black population.

Table 1.2: Instrumental Variable Estimates of Neighborhood Preference Parameters

Model:	(White)	(Black)	(White)	(Black)
log(med. rent)	-1.920**	-1.494***	-2.021**	-1.534***
	(0.8164)	(0.5443)	(0.7734)	(0.5740)
log(Own)	0.1703	-0.1100	0.1607	-0.0725
	(0.5088)	(0.4209)	(0.4922)	(0.4292)
log(HU)	0.2393	0.8574*	0.3069	0.9058*
	(0.8049)	(0.5116)	(0.7283)	(0.4704)
Share white	0.0233	-0.0389	0.0293	-0.0356
	(0.0328)	(0.0290)	(0.0317)	(0.0283)
Share black	-0.0287*	0.0121	-0.0224	0.0163
	(0.0150)	(0.0174)	(0.0138)	(0.0157)
log(Income)	1.419	1.026*	1.255	0.9178*
	(0.9040)	(0.5412)	(0.7762)	(0.4797)
PH tract	-0.4211	-0.8713***		
	(0.3267)	(0.2677)		
Ring 1	-0.2876	-0.2411		
	(0.1910)	(0.1479)		
Ring 1 x Tower			-0.5345	-1.063**
			(0.4018)	(0.4733)
Ring 1 x no Tower			-0.4275	-0.8796***
			(0.3771)	(0.3191)
Ring 2 x Tower			-0.3361	-0.3694
			(0.4911)	(0.3320)
Ring 2 x no Tower			-0.2811	-0.2382
			(0.2002)	(0.1588)
Year FE	✓	✓	✓	✓
Tract FE	✓	✓	✓	✓
Fit statistics				
R^2	0.46317	0.74255	0.48515	0.75024
Observations	6,508	6,460	6,666	6,618
F-test	27.338	30.359	24.501	27.622
1st Stage F	39.0	43.2	42.9	48.1

Note. Table 1.2 presents regression results of preference parameters for a static logit location choice model using Equation 1.14; I use population counts across census tracts for a set of tracts from 1950 to 2020. I estimate preference parameters separately by race/ethnicity. Log median rent, Black and White population share, and log median income are instrumented following Bayer **andothers** (2007), where I take public housing construction as exogenous variables. Columns 1 and 2 report results using simple treatment dummies switching in after a public housing project opened. Columns 3 and 4 interact ring dummies with dummies for having a "Tower in the Park" as defined in Section1.5.1. The instrumental variables in this specification are based on weighted averages of tract characteristics that are within a 1.5-2.5 miles ring for each census tract. I am using $avg.\ develop.\ intensity_{1.5-2.5}$, $avg.\ share\ white_{1.5-2.5}$, $avg.\ share\ black_{1.5-2.5}$, $avg.\ share\ park_{1.5-2.5}$, $avg.\ pop.\ dens._{1.5-2.5}$. Standard errors are clustered at the neighborhood level.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Conditional on a neighborhood's fixed effects and costs of living, white households prefer to live in neighborhoods where with a higher concentration of other white households. black households when making their location decision. White population living in private dwellings in a tract does not play a role for either ethnic groups when making their location decision with estimates close to zero while only marginally reject the concentration of black population. In contrast, black residents not only prefer the presence of other black residents but also have stronger preferences not being close to white population. For the purpose of the following analysis, the public housing estimates behave in the expected way. The difference in probability to live in a public housing tract as compared to the third ring is larger for blacks than for white population. Similarly for the second ring white population is around 6% more likely to settle in there as black population is about 12% more likely to live close by. This could hint the fact that black residents have a stronger preference for public housing residency as compared to white population. These results closely reflect the result from estimating Equation 1.19 which shows negative spillover effects for white population.

Finally both white population and black population prefer places with more apartments. A one percent increase in rents decreases the probability to live in a tract by 8% for black population and insignificantly for white population by 1.8%. All households have a higher probability to live in the tracts comprised by this analysis as compared to other places in New York. This results does not result from the fact that ethnicities overall does not prefer lower rents but rather from the fact that individuals might be willing to pay more living far away from public housing. These results are confirmed using hedonic rent estimates of ask prices. While preference parameters for race of whites and blacks stay stable, hedonic rents are positively associated with the probability of living outside the tract of observation for black population.

Next, I calibrate the supply shifter using the housing market clearing condition (Equation 1.11) yields:

$$\delta_{mt} = \frac{D_{mt}(\mathbf{r}, \mathbf{s}^{\mathbf{w}}, \mathbf{s}^{\mathbf{b}}; \beta)}{r_{mt}^{\phi}} \tag{1.16}$$

Using the full set of $\boldsymbol{\beta}_t^g$ I calibrate the supply shifter for each tract for each year t. I set the medium term housing supply elasticity $\phi = .65$ following estimates by Saiz (2008) for the years 1970 to 2000. There are no estimates of housing supply elasticties for earlier years for New York City to the knowledge of the author. While later estimates by Baum Snow suggest a lower elasticity in the year 2010 this drop seems drastic and might be due to the use of different methods and levels of aggregation.

1.7 Welfare Impacts of Public Housing Construction

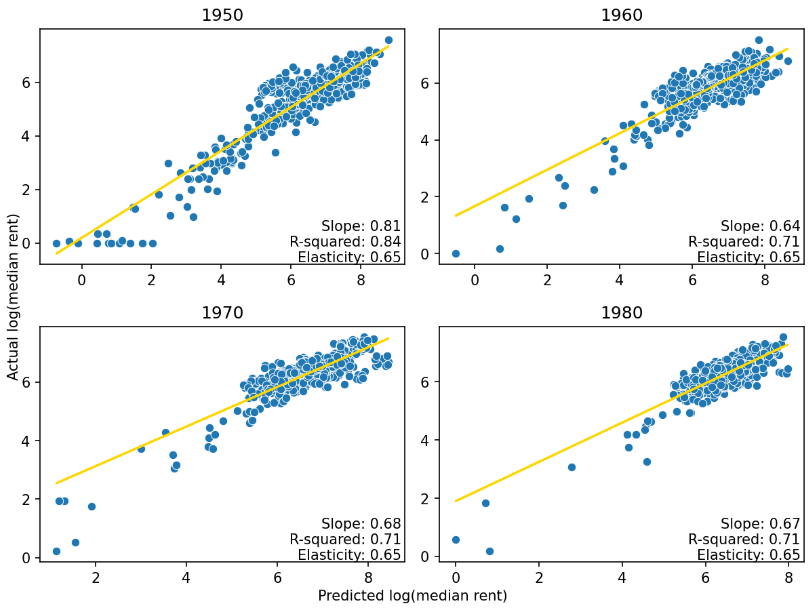
1.7.1 Model fit

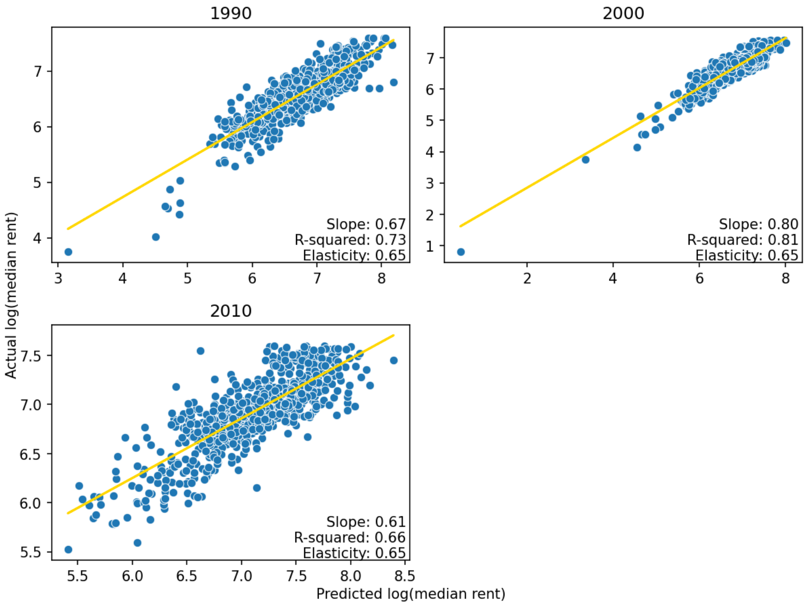
The final aim of model is to assess the welfare consequences of public housing over the long run. I solve the model by finding a fixed point to the system of $3xM$ Equations above (see Equations 1.11 to 1.13). Details on the equilibrium solver I report in Appendix 1.E. Before describing the welfare estimates and the counterfactual, I perform a validation exercise using the equilibrium rental prices. I ignore the racial share estimates because they depend on both the demand and supply components of the model.

Figure 1.10 plots actual log rents in census tracts against predicted equilibrium rents log rents that are implied by the associated model equilibrium. In this exercise I include the models unobservables. The fit of predicted rent and actual rent varies quite considerable by year. While the model has a reasonable fit for the first three decades it performs poorly for the years 1980 to 2010.

A main result from this exercise is that differences between the actual and simulated data can arise because of the elasticity. Higher elasticities lead better fits though, the model is bounded from above and below for specific elasticities, yielding now solution for values above and below.

Figure 1.10: In-Sample Fit of Structural Model Using Rent Dat





Note. Figure 1.10 plots actual log rents in census tracts against log rents that are implied by the model estimates where unobservable components of neighborhood quality are included. The number of housing units supplied is set to equal the number of housing units implied by the demand system.

1.7.2 Welfare

Using the estimated preference parameters, a specified set of neighborhood characteristics $(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b})$ and the assumption on the distribution of the idiosyncratic shock ϵ_{mt}, the consumer surplus for group g in closed-form solution associated with a set of alternatives is given in the standard log–sum–exp form:[17]

$$CS_t^g = \ln\left(\sum_m \exp(v_{mt}^g(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}))\right) \tag{1.17}$$

Where v_{mt}^g is indirect utility as defined in Equation 1.6. To asses the welfare consequences of public housing I perform two counterfactual exercises. First, I remove public housing entirely from a tract. I do this by setting the dummies in the utility function to zero, thereby setting the preferences parameters over public housing to zero. This counterfactual informs about how a world without public housing would have looked liked, in which agents never exposed to these buildings. The second counterfactual deals with a change in spatial extend of

[17]By definition, the consumer surplus is the utility, in money terms, that a household receives in the choice situation. Household n chooses the alternative that provides the greatest utility. Therefore, $CS_n = max_j(U_{nj} = (V_{nj} + \epsilon_{nj}, \forall j)$. If each ϵ_{nj} is iid and Type 1 Extreme Value distributed then, using the the distributional properties of ϵ_{nj}, the expectation $\mathbb{E}(CS_n)$ becomes: $ln(\sum_j exp(V_{nj}))$.

the projects. Letting "Tower" style buildings become private units can to lead to sizeable equilibrium effects as households might resort across the entire city.

While the usual welfare analysis would require an expenditure function, I rely on the notion of a rent equivalent to compute renter welfare changes from a counterfactual world ($\mathbf{r}^1, \mathbf{s^w}^1, \mathbf{s^b}^1$) relative to a baseline scenario ($\mathbf{r}^0, \mathbf{s^w}^0, \mathbf{s^b}^0$) in monetary terms (Almagro **andothers**, 2023). Thus, the group-specific rent equivalent, RE^g, is the increase in rent that is necessary to leave the household indifferent with respect to the baseline values as follows:

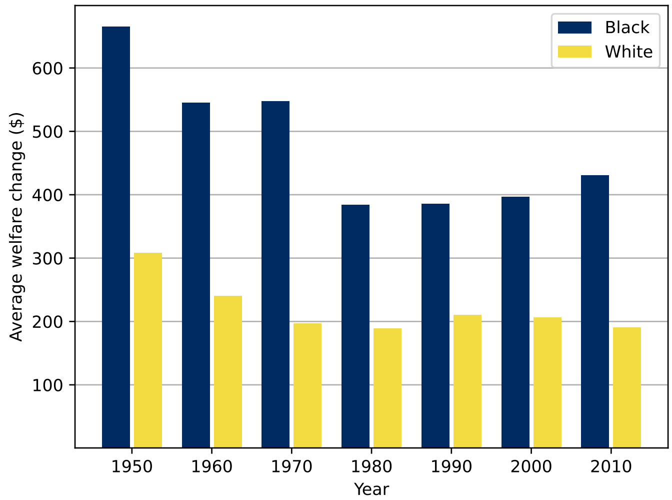
$$\begin{aligned}\Delta CS_t^g = \ln&\left(\sum_m \exp(v_{mt}^g(\mathbf{r}^1 + RE^g, \mathbf{s^w}^1, \mathbf{s^b}^1))\right)\\ &- \ln\left(\sum_m \exp(v_{mt}^g(\mathbf{r}^0, \mathbf{s^w}^0, \mathbf{s^b}^0))\right)\end{aligned} \tag{1.18}$$

Figure 1.10 shows the rent equivalent (RE) averaged for black and white households by year from the first counterfactual exercise removing all public housing from the city. These results show that welfare is positive on average for both for white and balck population in all census years. The positive effect for blacks is mostly twice as high as for white population throughout all years. In 1950 the RE for Whites is \$307 and \$664 for Blacks. This value averages for whites around \$200 in years after 1960. In contrast, Black's RE is averageing around \$400 in ther years 1980 to 2010.

These results complement welfare estimates from Almagro **andothers** (2023) showing that in a world without public housing construction welfare for non public housing residents would have been higher. However, welfare gains are differently distributed. In Almagro **andothers** (2023), whites gain more than Blacks. In particular, non-poor white population gains \$230 which cloesely reflect average welfare of white of \$200, non-poor black population gains \$39. Average welfafe is about 50% smaller. Furthermore, my results are in line with the empirical literature on public housing showing that in particular in the US context public housing is an important driver of neighborhood through a disamenity channel.

These positive gains can be explained by two mechanisms. First, blacks have a stronger aversion of living within public housing tracts than white population (see Table 1.2). Therfore, removing projects removes this disutilty. Second, both white and black population have an aversion of high rents. The disutility from higher rents is larger for whites than blacks. Average rent prices are lower across census tracts by removing all public housing projects from the city's stock. That

Figure 1.10: Summary of Welfare Consequences of Public Housing Demolitions



Note. Figure 1.10 reports the average rent equivalent due to public housing construction for black (blue) and white (yellow) population for each census year. I compare welfare under the actual state - with public housing - to a counterfactual scenario in wich all public housing projects have been removed. Welfare is expressed as the change in rents that would make households indifferent between the counterfactual and actual states of the world as expressed by Equation 1.18. A positive value implies that demolitions lead to higher welfare; unobservable components of neighborhood quality are included.

is the counterfactual rent distribution is shifted towerds left with a lower mean (see Appendix 1.E, Figure 1.E.1).

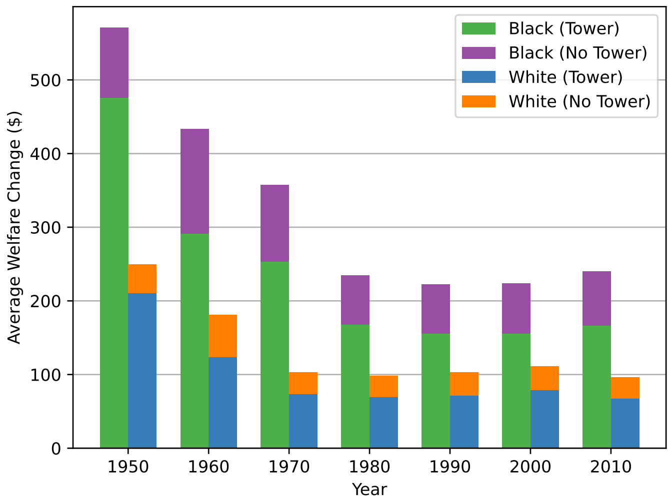
The largest changes in rents are observed in the third ring around public housing projects. Rent differentials in a public housing tract and the first ring are considerable lower and fall to $60 and $85 respectively in the period 1980 to 2000 from a peak of $433 and $432 in 1950 (see Subfigure 1.E.2a). Which contribute to lowering welfare gains.

Rent equivalents follow the trends in rent differentials in the second and first ring. However, rent equivalents for both whites and blacks within public housing tracts are increasing not decreasing (see Figure 1.E.2). This reveals the importance of two counterbalancing mechanisms. First, lower rent prices in the counterfactual szenario drive welfare gains in areas further away from public housing, where preference are over public housing are considerable lower. In contrast, rents in public housing tracts are similar in the counterfactual szenario while welfare is increasing for both white and black population. In particular, in the periods from 1980 to 2010 REs increase from $288 to $494 for whites (Subfigure 1.E.2c) and from $484 to $780 for blacks (Subfigure 1.E.2b), therby outperforming any gains from lower rents in previous preiods. This reflects strong aversion to live very close to public housing projects and confirms falling rents in public housing tracts (see Section 1.5.1, Figure 1.6).

In the second counterfactural scenario I explore welfare changes due to the removal of "Tower in the Park" style public housing and non-tower buildings. As argued in Section 1.5 these building type is an important component driving spillover effects of public housing. I estimate the model by removing either "Towers" or non-towers while keeping the respective structures in place. The model is recalibrated using estimates from Table 1.2 columns (3) and (4). Figure 1.11 gives rent equivalents (RE) from the removal of the other building type. First, welfare gains from removing "Towers" outperforms removing welfare from non towers. Moreover, though preference paramters have been re-estimate, total welafre gains are nearly identical to the removal of all public housing projects, which allows a direct comparison to the total welfare gain. For example, for black population around 72% of total welfare can be attributed to the removal of "Towers" and 69% for Whites. Welfare gains are also strictly larger for black population than for Whites which is reflected in the differential valuation of living close to public housing projects. Blacks devalue living close to "Towers" more than whites at all distance (see Table 1.2). Removing them would increase welfare by up tp $238 in 1950 for Blacks on average while this value stabilizes at around $161 after the 1970s. For Whites REs are stable after 1960 around $72. These gains are large compared to removing all non-tower projects. Non-tower REs for Whites range from $29 to $57 and for Blacks range from $67 to $142.

This results informs about the welfare effects of racial sorting induced by public hosuing. One way to think about different mechanisms through which public housing works is to return to the original estimates from reduced form estimation (Figure 1.5). White population falls continuously 10 years after construction indicating that the initial externality shock accumulates over time. The fact that welfare gains stabilize after a certain time indicates further that there are adjustment costs to price public housing efficiently. The result further highlights potential welfare returns to re-modeling public housing. The provision of affordable housing can entail welfare gains to its residents which are not factored into the results in Figure 1.10 and Figure 1.11 suggesting these results to be an upper bound. As suggested by Almagro **andothers** (2023) to distinguish between welfare for race-by-income groups, there are considerable welfare losses from removing public housing for poor white and black households while overall welfare gains are driven by gains for rather upper income households. In order to avoide potential losses due to the removal of affordable housing, re-modeling of public housing could provide an alternative to demolition. Since welfare gains

Figure 1.11: Summary of Welfare Consequences of Public Housing Types Demolitions



Note. Figure 1.11 reports the average change in each groups welfare due to public housing construction. I compare welfare under the actual state - with public housing - to two counterfactual scenarios; in the first scenario, only "Tower in the park" style buildings have been removed (Tower); in the second scenario, only non-tower buildings have been removed (No Tower). Welfare is expressed as the change in rents that would make households indifferent between the counterfactual and actual states of the world as expressed by Equation 1.18. A positive value implies that demolitions lead to higher welfare; unobservable components of neighborhood quality are included. Welfare estimates have been weighted using shares of "Tower in the Park" projects and non-tower projects as of total public housing projects in census year t.

are driven by the demlition of "Towers in the Park" redeveloping projects and project areas by filling in open space with additional private units or integrating existing projects into a retail or mixed use enviornemnt could be explored as potential options.

1.8 Conclusion

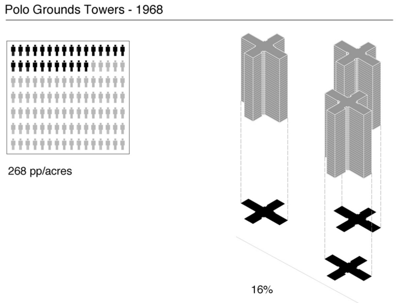
In this paper, I ask how public housing construction shaped neighborhoods in New York City from 1930 to 2010. Specifically, I study how a particular building type called "Tower in the Park" affect the location decision of white and black population. Over an 80-year period, I estimate that public housing construction increased the concentration of the black population in treated tracts while at the same time decreasing the concentration of the white population in the immediate and wider vicinity of the new projects. These effects are driven by "Tower in the Park"- style projects, while non-tower projects have significantly lower effects. The spatial pattern of net price effects is consistent with negative demand effects of white population where public housing units are large concerning the previous stock. Overall, I identify the "Tower in the Park" style projects as those that increase segregation between white and black populations. I use a structural approach to quantify how these changes shaped welfare and study distributional considerations across racial and income groups. Demolition of public housing in the model generated large welfare improvements for white and black households. The effects of demolitions arise from lower average rents and less segregated neighborhoods. This could indicate that subsequent resorting generates large gains for white households, bidding less for certain areas, thereby improving rents for all households. My findings highlight that scale matters. Welfare gains increase for white households with reductions in the area share of public housing. However, the rent equivalent does not change with reductions in the area share for black population. This highlights the disparate impacts on welfare and hints at the limitations of policies that aim to revitalize neighborhoods and benefit lower-income households. Nevertheless, a key policy implication of our results is that redevelopment can potentially play a key role in shaping the welfare impacts of urban renewal programs such as public housing demolition. While governmentally run public housing has often been blamed as inefficient, this paper argues that public housing, which is integrated into the urban fabric, can provide low-income housing while not affecting the neighborhood in an unintended way. Thus, this paper corroborates existing findings by Blanco **and** Neri, 2023. While concerned with private regenerations of public housing, my findings highlight that mixed-race developments and higher-quality buildings mitigate the negative effects of public housing and can favor the wider area. Moreover, those regenerations are tailored to the private market in their design and generate an urban layout that suits the city structure. In particular, when conversion is costly, efforts to fill large empty green spaces between towers could be made by an approach to make places more convenient to live in for different income groups, which might have similar effects as in Blanco **and** Neri, 2023.

Appendix 1.A Additional Material

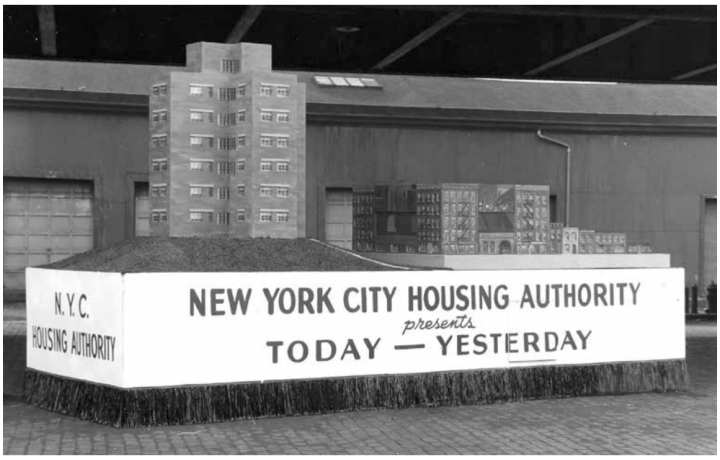
Figure 1.A.1: Tower in the Park

(a) Polo Ground Towers

(b) Polo Ground Towers - Floor-plan



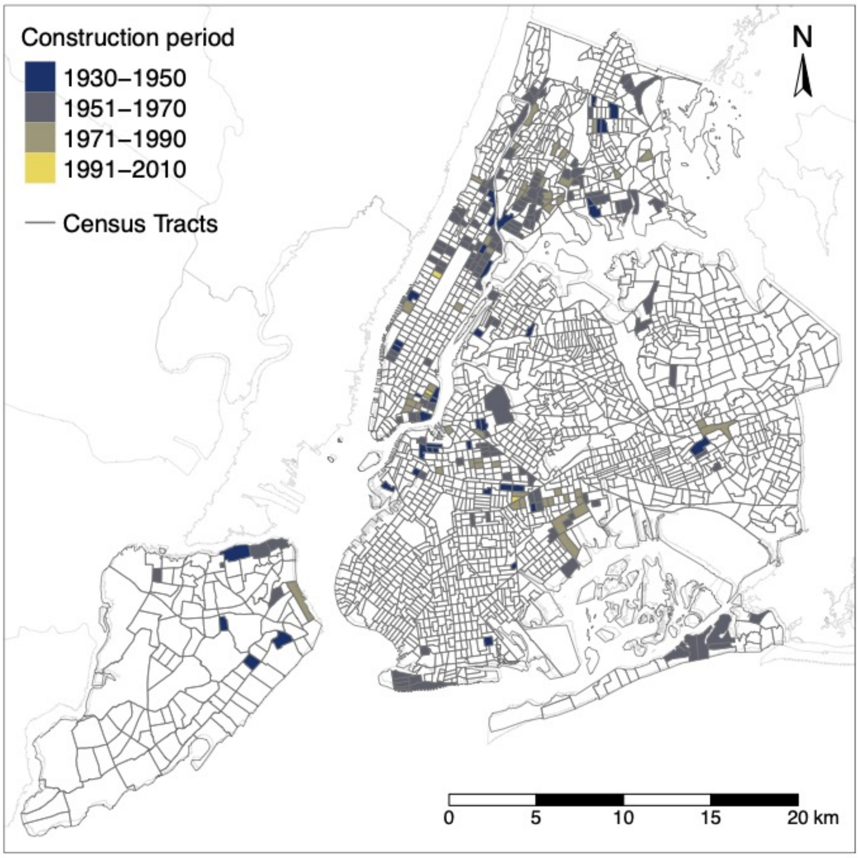
(c) "Today - Yesterday", 1948



Source. Panel 1.A.1a: La Guardia and Wagner Archives, NYCHA Collection, LAGCC, CUNY; Panel 1.A.1b: https://skyscraper.org/housing-density/history/; Panel 1.A.1c: Bloom **andothers** (2016).

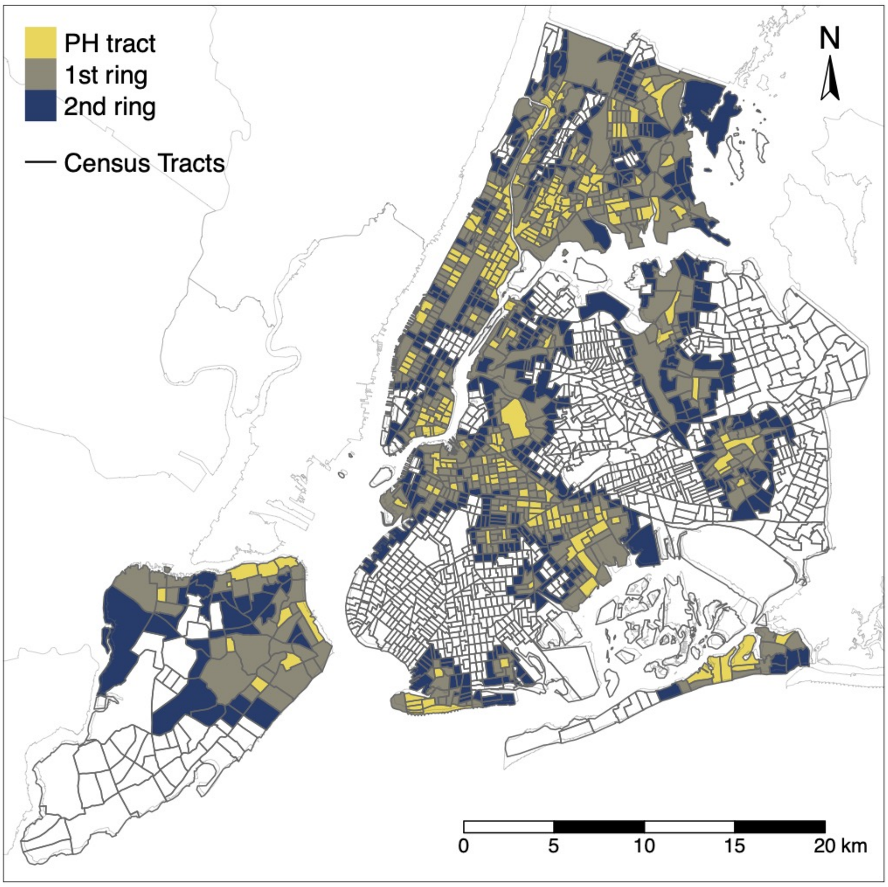
Appendix 1.B Additional maps

Figure 1.B.1: Evolution of public housing by construction period



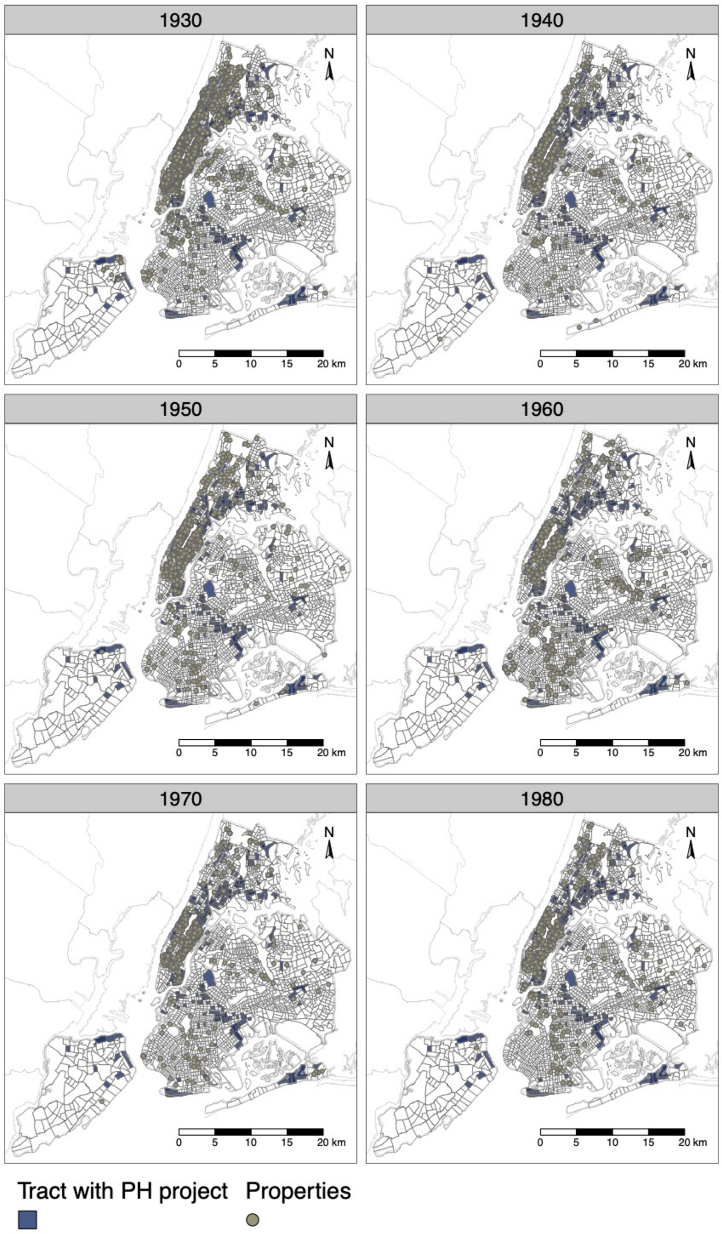
Note: Figure 1.B.1 displays 2010 census tracts. Tracts highlighted in color contained at least one public housing project. Some tracts have more than one project. Public housing tracts have been grouped in construction periods based on the completion date of the first project.
Source. La Guardia and Wagner Archives, NYCHA development data book. Details on the construction of the data set can be found in Section 1.2.2.

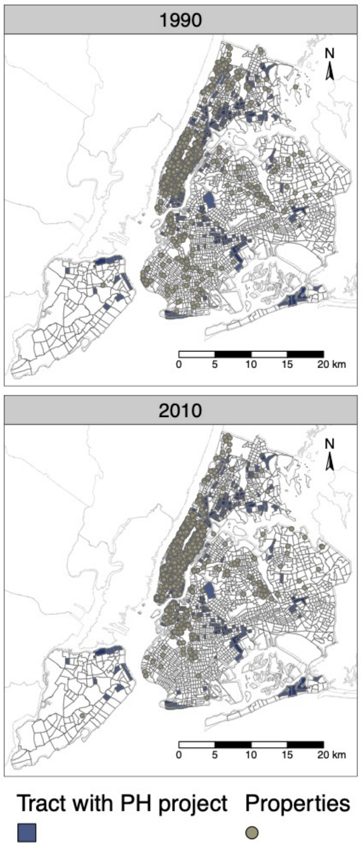
Figure 1.B.2: Tracts by distance relationship to public housing



Note. Tracts by distance relationship as used in the analysis in panel setup.

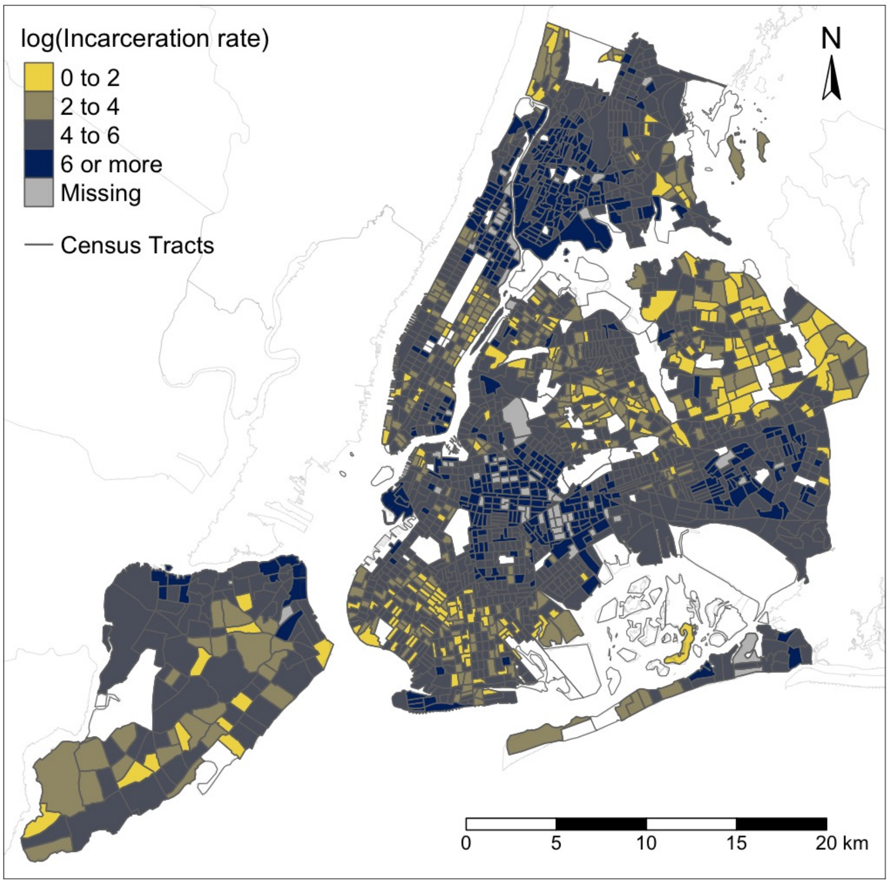
Figure 1.B.3: Spatial extent of Rental Data





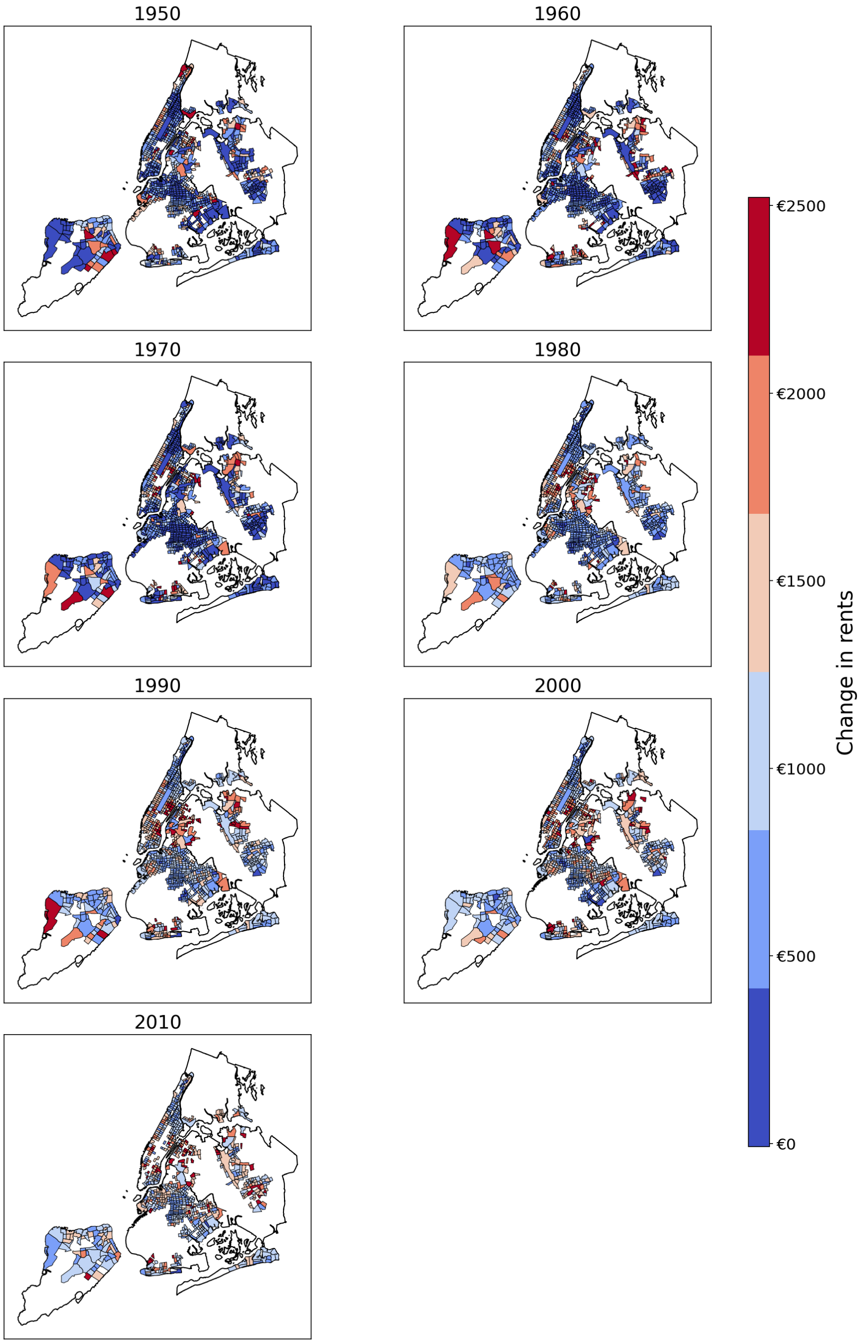
Note: Geocoded rental data fro each given census year are shown as red dots. All census tracts which have had a public housing unit ever during the observation period are colored in green. *Source.* New York Times Real estate sections. Details on the construction of the data set can be found in Section 1.2.2.

Figure 1.B.3: Incarceration rate



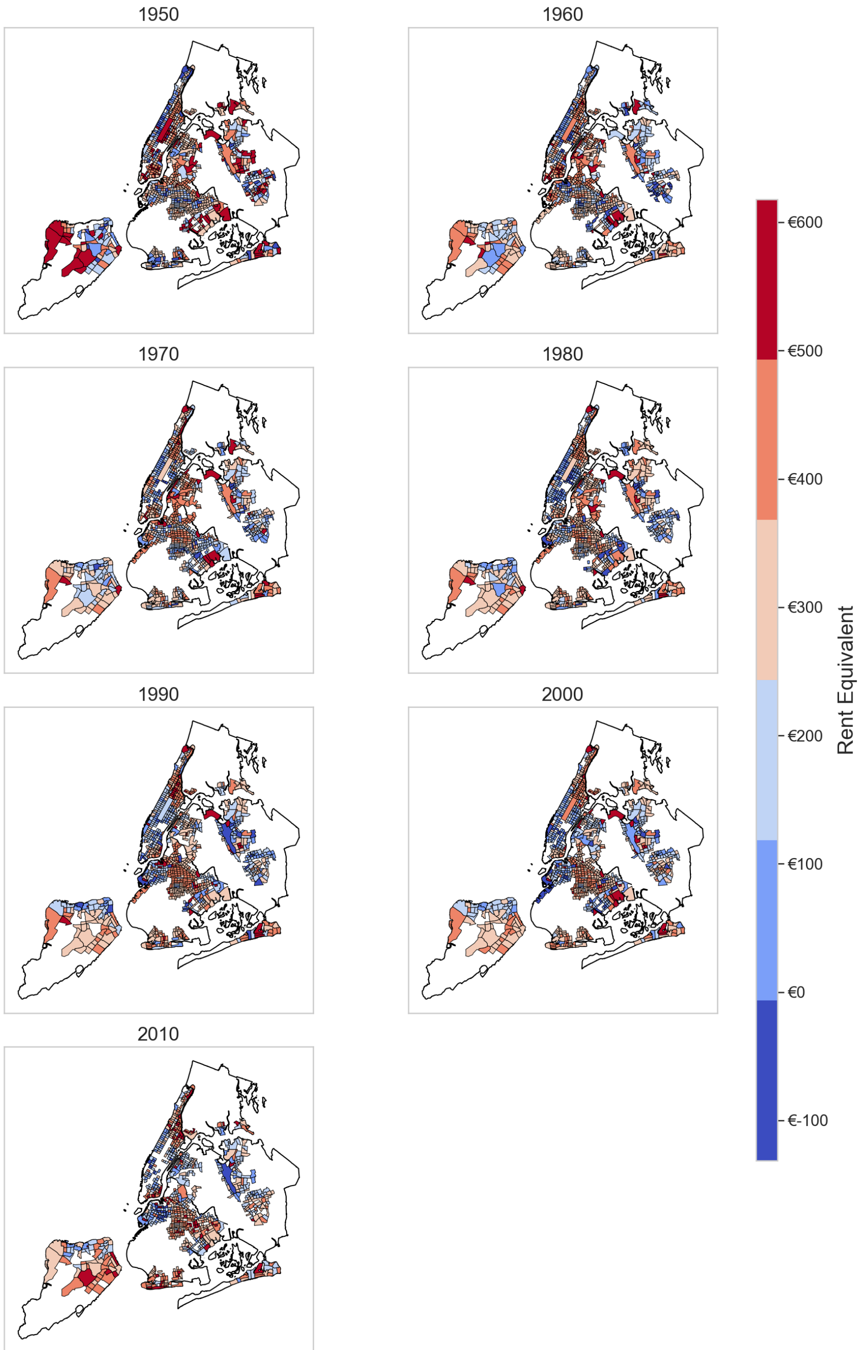
Note. Figure 1.B.3 shows the log of the incaration rate by tract

Figure 1.B.4: Rent Differentials from Simulated Public Housing Removal



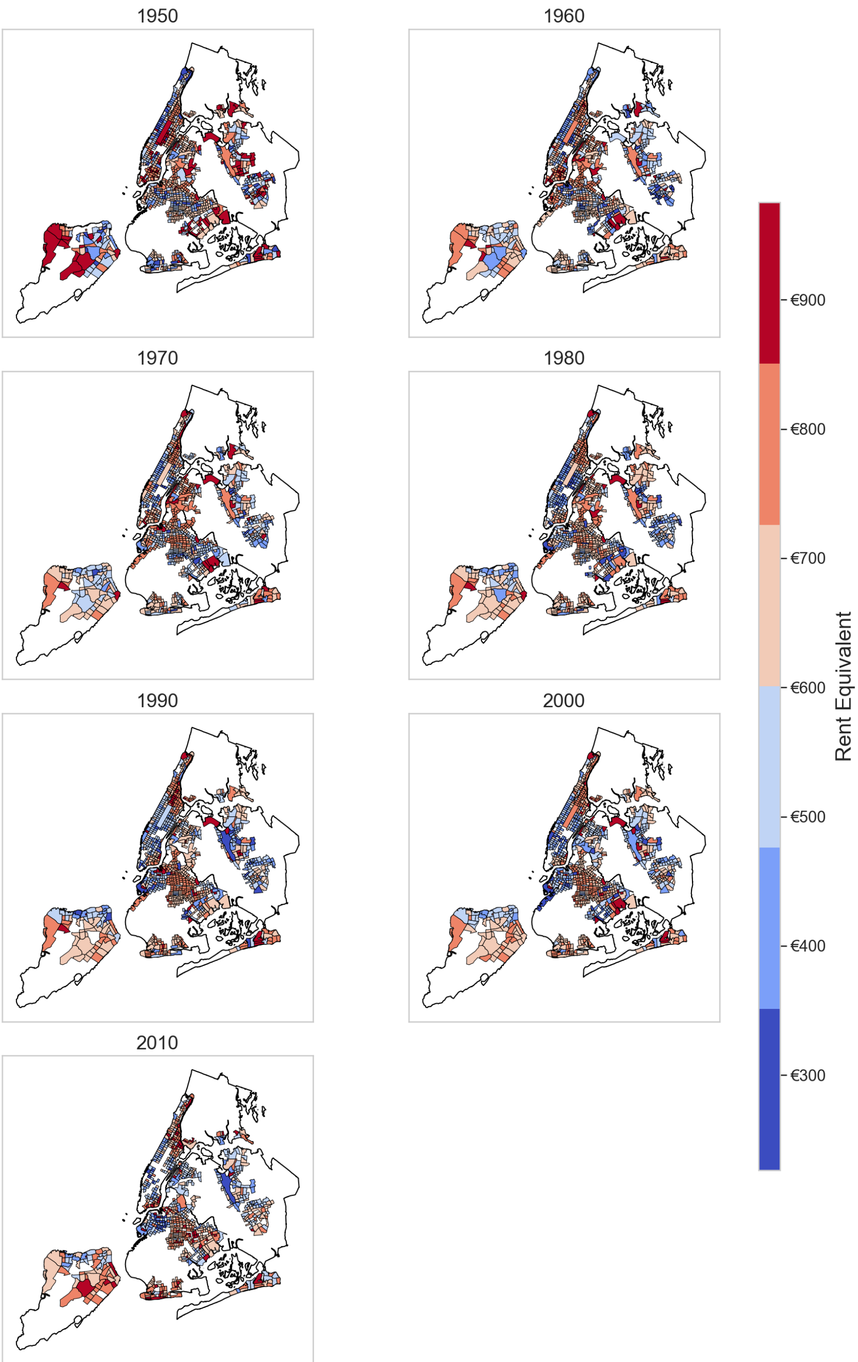
Note. Figure 1.B.4 shows rent differentials by tract; I take the difference between the actual predicted rent and the counterfactua rent; the counterfactural szenario corresponds to removing all public housing projects and letting the housing stock become private. Rent differentials are plotted by census year and within 2010 census tract boundaries; positive rent differential imply lower counterfactual rents; unobservable components of neighborhood quality are included.

Figure 1.B.5: Rent Equivalent for Whites



Note. Figure 1.B.5 shows change equivalents (RE) for *white* populations - that is, the rent differential that would make households indifferent between the counterfactual and actual states of the world as expressed by Equation 1.18; REs are plotted by census year and within 2010 census tract boundaries. A positive value implies that demolitions lead to higher welfare; unobservable components of neighborhood quality are included.

Figure 1.B.6: Rent Equivalent for Blacks



Note. Figure 1.B.6 shows change equivalents (RE) for *black* populations - that is, the rent differential that would make households indifferent between the counterfactual and actual states of the world as expressed by Equation 1.18; REs are plotted by census year and within 2010 census tract boundaries. A positive value implies that demolitions lead to higher welfare; unobservable components of neighborhood quality are included.

Appendix 1.C Data

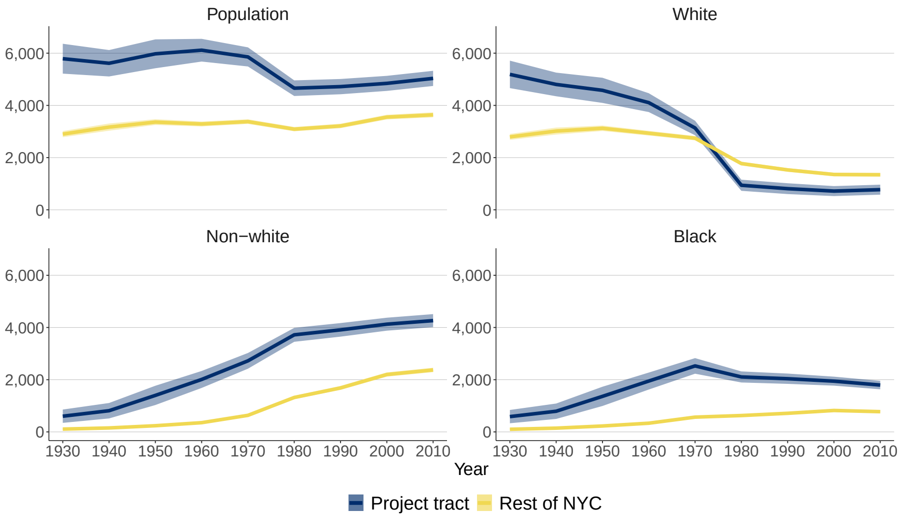
Table 1.C.1: Summary statistics

Year	Monthly rent ($)	Rooms	Median rent ($)	Population	White	Black
1930	1915***	3.7***	243***	3201***	3042***	151***
	(20.037)	(0.027)	(15.166)	(64.617)	(60.954)	(19.173)
1940	1089***	4.7***	90***	3423***	3203***	211***
	(40.3)	(0.64)	(1.953)	(70.377)	(66.362)	(22.546)
1950	1074***	4***	348***	3633***	3275***	345***
	(21.395)	(0.102)	(4.406)	(60.725)	(55.055)	(29.795)
1960	1024***	4.2***	426***	3582***	3057***	501***
	(19.684)	(0.049)	(4.394)	(52.689)	(46.034)	(28.919)
1970	1682***	3.8***	567***	3637***	2785***	770***
	(30.17)	(0.058)	(5.353)	(47.945)	(40.68)	(30.679)
1980	1431***	2.9***	578***	3254***	1685***	781***
	(33.387)	(0.064)	(4.935)	(42.41)	(35.432)	(25.813)
1990	1474***	2.3***	816***	3370***	1453***	850***
	(30.481)	(0.57)	(7.166)	(43.656)	(34.093)	(26.551)
2000	1877***	2.7***	874***	3686***	1287***	938***
	(52.082)	(0.051)	(8.221)	(44.62)	(33.213)	(27.273)
2010	1725***	2.9***	1133***	3784***	1283***	883***
	(106.261)	(0.061)	(7.402)	(47.167)	(33.91)	(26.979)

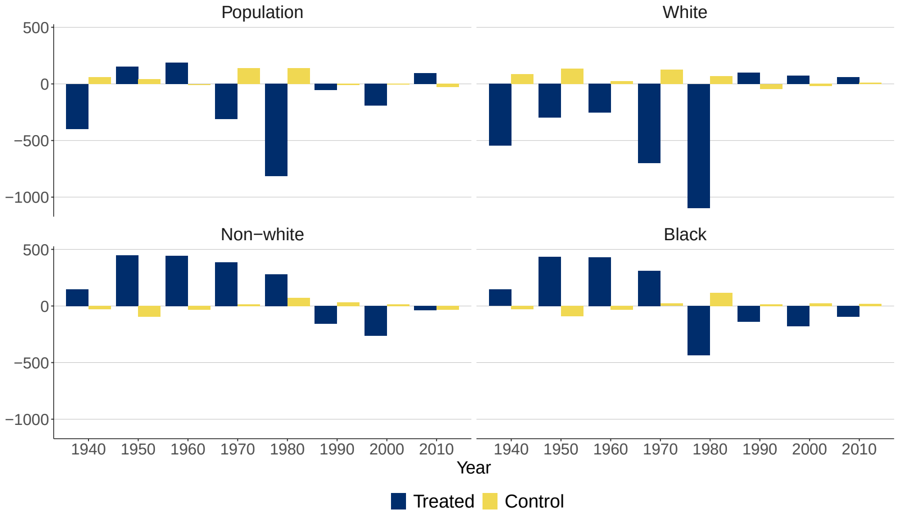
Note. Table 1.C.1 displays averages for the main outcome variables; standard errors in parentheses; significance levels have been obtained from a two-sided t-test. Monthly rent and the number of rooms are taken from the newspaper ads, and median contract rent, population, white and black population had been taken from the United States federal census. Census variables have been harmonized on 2010 census tract boundaries and averages correspond to the average census tract (see Appendix 1.C.3 for more details). “Monthly rent” and “Median rent” had been deflated by the CPI deflator and normalized to the 2010 CPI level.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.
Source. New York Times; US Decennial Census.

Figure 1.C.1: Demografic trends

(a) Demografic trends

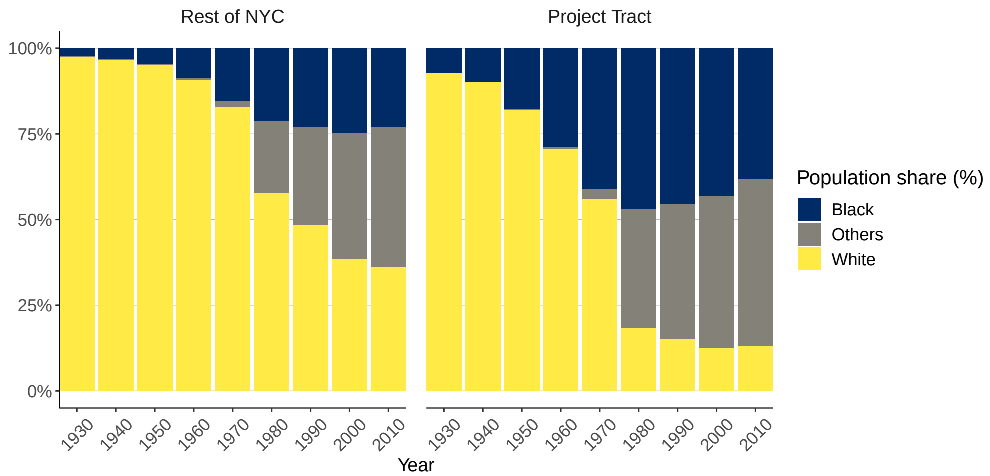


(b) Deviation from average city tract



Note. Figure 1.C.1 reports trends of the main outcome variables. Panel (a) shows yearly averages for demographic variables: total population, white, black and neiter black nor white population; I compute averages for all treated tracts, or, in other words, those which ever had a public housing unit within its boundaries (Project Tract) and all remaining tracts in New York City (Rest of NYC). Panel (b) reports the deviation of the average treated and control tract as defined in Section 1.3 from the average tract in the rest of new york city.
Source. US Decennial Census, NYCHA development data book. Details on construction of the data set can be found in subsection 1.2.2.

Figure 1.C.2: Racial composition of PH Tracts and within the Rest of New York



Note: Figure 1.C.2 reports the racial composition of census tracts with a public housing project (Project Tract) and all other tracts in New York City (Rest of NYC); I compute the average of the respective race as a share of the total tract population. Averages for all tracts which ever had a public housing unit within its boundaries (Project Tract) and all remaining tracts in New York City (Rest of NYC).
Source. US Decennial Census; NYCHA development data book. Details on construction of the data set can be found in subsection 1.2.2.

Table 1.C.2: Balance Tests: Stacked dataset

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables:			$\mathbb{1}(r = PHtract)$			
log(Pop)	0.1332***			0.0877		
	(0.0319)			(0.0753)		
log(White)		0.0899			0.0203	
		(0.0560)			(0.0674)	
log(Black)			0.3360***			0.3151***
			(0.0479)			(0.0649)
Projct-Year FE	✓	✓	✓	✓	✓	✓
Project-Borough-CD FE	✗	✗	✗	✓	✓	✓
Fit statistics						
Observations	13,556	9,673	9,673	5,798	5,798	5,798
Pseudo R^2	0.03780	0.05531	0.07949	0.06473	0.06387	0.08163
BIC	26,551.8	15,235.6	15,080.3	14,801.6	14,806.1	14,714.4

Note. Table 1.C.2 shows estimates from a logistic regression using a dummy variable equal to one if a census tract is treated and to zero if the treat is within the second ring as dependent variable. I use the stacked sample, implying all fixed effects had to be interacted with project fixed effects; the sample only contains control and treatment before treament. The 1st ring around treated tracts has been excluded. Standard errors are clustred at the project level (level at which the data have been stacked).
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Table 1.C.3: Balance Tests: Panel dataset

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables:	$\mathbb{1}(r = PHtract))$					
log(Pop)	0.3634***			0.0660		
	(0.1062)			(0.0784)		
log(White)		0.2139***			0.0132	
		(0.0669)			(0.0738)	
log(Black)			0.4506***			0.4544***
			(0.0530)			(0.0808)
Year FE	✗	✗	✗	✓	✓	✓
NTA FE	✗	✗	✗	✓	✓	✓
Fit statistics						
Observations	3,199	3,199	2,809	1,478	1,478	1,478
Pseudo R^2	0.04723	0.02292	0.16209	0.19754	0.19667	0.24121
BIC	3,867.4	3,965.7	2,572.4	2,061.1	2,062.9	1,974.8

Note. Table 1.C.3 shows estimates from a logistic regression using a binaty variable equal to one if a census tract is treated and to zero if the treat is within the second ring as dependent variable. I use the panel (unstacked) sample; the sample only contains control and treatment before treament. The 1st ring around treated tracts has been excluded. Standard errors are clustred at the neighbohood level.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

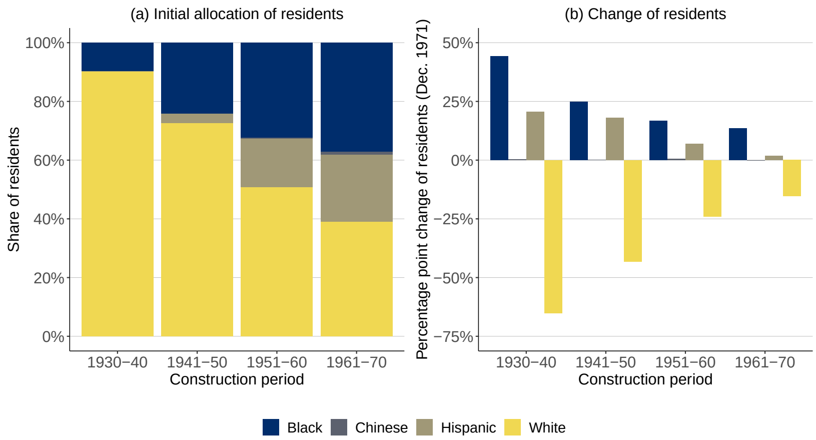
1.C.1 Public housing statistics

Table 1.C.4: Public housing characteristics by construction decade

	1930-1940	1941-1950	1951-1960	1961-1970	1971-1980	1981-1990	1991-2000
	Total counts						
Projects	10.00	28.00	60.00	77.00	60.00	26.00	9.00
Units	10955	25432	63006	37662	13115	5335	587
	Median characteristics						
Units	1171	967	1040	407	208	184	48
Height (stry)	4.50	8.00	11.17	17.33	9.75	6.25	4.50
Ground coverage	26%	19%	15%	17%	34%	28%	43%
Area share	25%	23%	30%	9%	04%	05%	9%
Construction cost	$12,854.93	$14,712.01	$13,963.55	$15,345.32	$17,287.91	$25,965.57	$26,962.55

Note. Table 1.C.4 displays public housing project information within the decade of their constructio; projects were grouped into construction period cohorts based on their opening date. The first two rows report total counts by construction decade. Row three to six shows median public housing characteristics by construction decade. Area share refers to the tract area occupied by public housing projects. Ground coverage refers to the build-up share of public housing land. Average height is given in storeys by project. Construction cost per room are deflated by the CPI and given in $2010.
Source. NYCHA Development Data Book. Details on construction of the data set can be found in Section 1.2.2.

Figure 1.C.3: Racial composition by construction decade



Note. Figure 1.C.3 displays the ethnic composition of NYCHA projects based on their construction decade. Projects have been grouped in constriction periods by their completion date. Panel (a) presents the resident shares by ethnicity at the time of initial occupancy. Panel (b) illustrates change for each ethnic group from the project's start date to December 1971 in percentage points.
Source. La Guardia and Wagner Archives, NYCHA development data book. Details on the construction of the data set can be found in subsection 1.2.2.

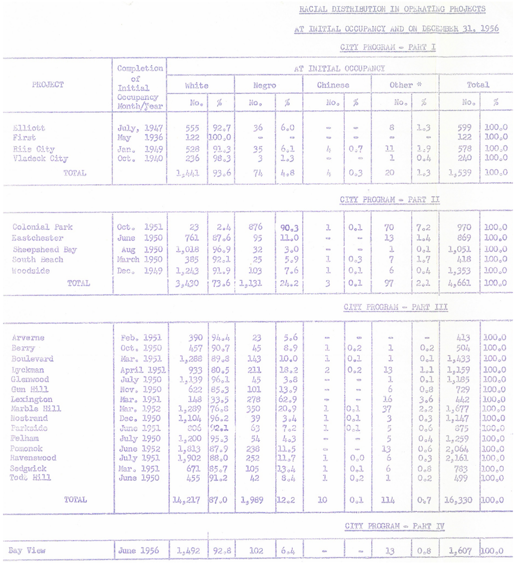
1.C.2 Rent data collection

Rent data were collected from the real estate section of the New York Times (NYT). This was undertaken in context for the HIstorical Prices in HOusing Project (HiPHoP) project at Trinity College Dublin. Figure 1.C.4 Panel (b) gives an example of a typical listing page in the NYT. For each census year, the standard approach was to choose 12 sets of listings, one per month collected on the last Sundays. Sundays were chosen as the day with by far the largest set of real estate listings. This was true for the vast majority of years; where another day of the week had the largest set of listings, this was used instead. Within each set of listings, targets were set for valid rental ads: 1500 rental listings.

The final listings which were used depended on the fact of having the correct address. For this to have either cross street or street number was required to be available, to ensure the correct location. In a next step the Google Geocode API was used to geocode the addresses. If an address matched main and cross street or with the exact street number the rental listing was included. If not it was kicked out. This procedure yields the final sample of rental listings shown in Table 1.C.5. The years 1930 and 1940 has more observations than the following years since existing data from HiPHoP had been added.

Figure 1.C.4: Example of data used

(a) Racial distribution in projects



RACIAL DISTRIBUTION IN OPERATING PROJECTS

AT INITIAL OCCUPANCY AND ON DECEMBER 31, 1956

CITY PROGRAM – PART I

PROJECT	Completion of Initial Occupancy Month/Year	AT INITIAL OCCUPANCY: White No.	White %	Negro No.	Negro %	Chinese No.	Chinese %	Other * No.	Other * %	Total No.	Total %
Elliott	July, 1947	555	92.7	36	6.0	–	–	8	1.3	599	100.0
First	May 1936	122	100.0	–	–	–	–	–	–	122	100.0
Riis City	Jan. 1949	528	91.3	35	6.1	4	0.7	11	1.9	578	100.0
Vladeck City	Oct. 1940	236	98.3	3	1.3	–	–	1	0.4	240	100.0
TOTAL		1,441	93.6	74	4.8	4	0.3	20	1.3	1,539	100.0
CITY PROGRAM – PART II											
Colonial Park	Oct. 1951	23	2.4	876	90.3	1	0.1	70	7.2	970	100.0
Eastchester	June 1950	761	87.6	95	11.0	–	–	13	1.4	869	100.0
Sheepshead Bay	Aug 1950	1,018	96.9	32	3.0	–	–	1	0.1	1,051	100.0
South Beach	March 1950	385	92.1	25	5.9	1	0.3	7	1.7	418	100.0
Woodside	Dec. 1949	1,243	91.9	103	7.6	1	0.1	6	0.4	1,353	100.0
TOTAL		3,430	73.6	1,131	24.2	3	0.1	97	2.1	4,661	100.0
CITY PROGRAM – PART III											
Arverne	Feb. 1951	390	94.4	23	5.6	–	–	–	–	413	100.0
Berry	Oct. 1950	457	90.7	45	8.9	1	0.2	1	0.2	504	100.0
Boulevard	Mar. 1951	1,288	89.8	143	10.0	1	0.1	1	0.1	1,433	100.0
Dyckman	April 1951	933	80.5	211	18.2	2	0.2	13	1.1	1,159	100.0
Glenwood	July 1950	1,139	96.1	45	3.8	–	–	1	0.1	1,185	100.0
Gun Hill	Nov. 1950	622	85.3	101	13.9	–	–	6	0.8	729	100.0
Lexington	Mar. 1951	148	33.5	278	62.9	–	–	16	3.6	442	100.0
Marble Hill	Mar. 1952	1,289	76.8	350	20.9	1	0.1	37	2.2	1,677	100.0
Nostrand	Dec. 1950	1,104	96.2	39	3.4	1	0.1	3	0.3	1,147	100.0
Parkside	June 1951	806	92.1	63	7.2	1	0.1	5	0.6	875	100.0
Pelham	July 1950	1,200	95.3	54	4.3	–	–	5	0.4	1,259	100.0
Pomonok	June 1952	1,813	87.9	238	11.5	–	–	13	0.6	2,064	100.0
Ravenswood	July 1951	1,902	88.0	252	11.7	1	0.0	6	0.3	2,161	100.0
Sedgwick	Mar. 1951	671	85.7	105	13.4	1	0.1	6	0.8	783	100.0
Todt Hill	June 1950	455	91.2	42	8.4	1	0.2	1	0.2	499	100.0
TOTAL		14,217	87.0	1,989	12.2	10	0.1	114	0.7	16,330	100.0
CITY PROGRAM – PART IV											
Bay View	June 1956	1,492	92.8	102	6.4	–	–	13	0.8	1,607	100.0

(b) NYT real estate section

8 W APARTMENTS Sunday advertisements must be ordered before 2 P. M. Saturday. THE

Apartments Furnished—Manhattan

Apartments of Three, Four, Five Rooms

Continued From Preceding Page

70'S (Park-Madison)—Sublet most unusual studio apartment, 4 rooms, 2 baths, beautifully furnished in the modern manner; dining gallery overlooks spacious duplex living room; cool, quiet. RHinelander 4-6437.

70'S, EAST—Three rooms, southern exposure, cross-ventilation; to October; reasonable. Sholl. Albert B. Ashforth, Inc., 12 E.44 MU.2-1100

70'S, Lower (off Central Park West)—4½ rooms; quiet; cool; beautifully furnished; 3 exposures; piano; May-October, longer; reasonable. ENdicott 2-2045.

70'S (lower, off Central Park West)—Sublet 4½ rooms till October, 1941; 3 exposures, spacious; $90. ENdicott 2-2045.

70'S, WEST—Unusual, beautiful, large 3 rooms; reasonable right party. TRafalgar 7-6848.

70'S, WEST—4 (2 Bedrooms), Elegantly Furnished! Outstanding Bargain; $80. TRafalgar 4-3156.

71ST (240 West End) (14B)—Three large, cool rooms, kitchen, exquisite, completely furnished; accommodating 2 or 4 persons.

71ST, 325 WEST—Living room, bedroom, complete kitchen, bath; elevator; telephone; reasonable.

72D, EAST—Sublet charmingly furnished 3½ (16th floor); outside dinette-kitchen; unobstructed; cross-ventilation; October-longer. RHinelander 4-8785.

72D, 405 EAST—3 large rooms; May 15 occupancy. RE. 7-4772, afternoons or evenings; also unfurnished.

72D, EAST—4½ rooms, 2 baths, June-Oct., $100. BUtterfield 8-6882, Monday.

72D, 124 WEST—Luxurious 3 rooms, 3 exposures, surrounding terrace, $125. Supt.

73D, 28 EAST—Summer bargain. Attractively furnished, 3 large room, bath, kitchen, dinette, extra toilet; cross draft; new elevator building. Waterman, ELdorado 5-6900.

73D, 210 EAST (9C)—Furnished 4 rooms, 2 baths; 9th floor; East Village; sublet Summer. Kahn. BUtterfield 8-3886.

73D, 105 WEST—Modern elevator, 3 rooms, large kitchen, unobstructed view; lease, $65. SU. 7-5554, SA.

73D, 40 WEST—Airy 3-room apartment, attractively furnished, bath, kitchenette, frigidaire.

75TH ST., 157 EAST—Cooperative building; living room, bedroom, dressing room and bath; fireplace; well furnished; $100; excellent meals served if desired; selective tenantry. Apply Mrs. Brown, premises. RHinelander 4-0550.

Apartments Furnished—Manhattan

Apartments of Three, Four, Five Rooms

187TH, 825 WEST (overlooking Hudson River—Sublet 4 rooms, furnished, with antiques, unusual view, reasonable rental. See Supt.

189TH, 701 WEST (Broadway) (5E)—Sublet, 3½ modern rooms, cozy, elevator; $55.

BEEKMAN PLACE
Attractive Summer Rental
4½ rooms, master bedroom, paneled nursery or library, connecting baths, large living room, dining foyer, kitchen, cross-ventilation; East River view; immediate occupancy until October, by appointment. Telephone PLaza 8-0349. REASONABLE RENTAL.

BEEKMAN PLACE, 32—Sublease now until October, attractive 3 rooms, cool, smart, river view. Sacrifice. Call PLaza 5-3155.

CABRINI BLVD., 160 (Castle Village) (Apt. 36)—Artist's beautifully decorated 4½-room apartment facing Hudson; sublet furnished-unfurnished; immediate occupancy to October. WAdsworth 7-2816.

CENTRAL PARK WEST (1 West 85th)—Five rooms, 7 windows overlooking park; sacrifice; piano, radio; complete. TRafalgar 7-5321.

CENTRAL PARK WEST—Attractive 4 light rooms, 2 baths, cross ventilation; June-October; references. TRafalgar 7-4378.

CENTRAL PARK WEST, 25—5 rooms to sublet till October; attractive furnishings. Apply renting office, Century Apartments.

CLAREMONT AVE., 191 (Columbia University)—Delightful 4 rooms, elevator, piano; $58. Hankins.

EAST END AVE.—Sublet to October; 3 rooms, bath, kitchen, cross-ventilation; top floor, walk-up; worth it for gorgeous view across Carl Schurz Park and miles of East River; also roof privileges; $50. REgent 7-4152.

GRAMERCY SECTION, modern building, 3½ rooms, beautifully furnished, roof garden, maid service; 15th floor; sacrifice. GRamercy 3-4667, or office, STuyvesant 9-3884.

GRAMERCY PARK—Unusually attractive apartment; beautifully furnished 5 rooms, magnificent view; 3 exposures; 12th floor; $150. GRamercy 5-4029.

GRAMERCY PARK, 8—3½ rooms, new; 3 exposures; all housekeeping appliances, radio; 4 months lease. Supt.

GRAMERCY PARK NORTH, 60—3-4, modern, twin beds, dinette. GRamercy 3-7399.

GREENWICH VILLAGE—Comfortable, attractive sublet, elevator building, 3 rooms, 2 exposures; $65.

Apartments Furnished—Manhattan

Apartments of Six Rooms and Over

RIVERSIDE DRIVE in 80's: 7 outside rooms, 3 baths, 10th floor, marvelous views, grand piano, 4 radios, television; rent for Summer or longer; $185 per month. TRafalgar 4-0208.

RIVERSIDE DRIVE—Sacrifice 6 rooms, new Steinway grand; river view. Algra Realty Corp., 70 Pine St.

FURNISHED beautiful Park Ave. 6-room apartment, real home, 3 baths, 35-ft. living room, den, dining, kitchen, 2 master bedrooms, maid's room; best transportation everywhere; for 3 to 5 months to responsible party; $300. W 486 Times.

EXCEPTIONALLY well-furnished apartment, 6 rooms, all 4 exposures; $150 monthly until October or longer. SUsquehanna 7-1745.

MUSICIAN sublet furnished 6 rooms, grand piano; beautiful river view. MOnument 2-3726.

Penthouse—Terrace Apartments

Furnished

1ST TO 5TH AVES.—EAST-WEST. Mrs. Ida Catlin, 1049 Lexington Ave. PENTHOUSE SPECIALIST. Unfurnished-furnished and apartments; many attractive offerings; excellent values; mail orders; state requirements; immediate or October. REgent 4-1400.

5TH AVE.—24TH FLOOR—DUPLEX 40' Salon, 4 Masters, 5 Baths, Landscaped Terraces. Also Unfurnished. BUtterfield 8-9430.

35TH, 137 EAST—Sublet approximately May 25, Sept. 25; unusually distinctive 3½ rooms, modern penthouse; large terrace; reasonable to suitable tenant. CAledonia 5-5360.

40TH (between Madison and 5th)—Deluxe duplex penthouse apartment; living room, dining room, kitchen and pantry; maid's room and bath; 2 master bedrooms, 3 baths, terrace; handsome antique furnishings; $600 month; occupancy April 30. Call for appointment. MUrray Hill 3-5245.

50'S, EAST—Unusual penthouse; studio living room, 2 master bedrooms, 3 baths, dining room, bar, kitchen, pantry, maid's room, fireplaces; terrace surrounds apartment; furnished by decorator; also unfurnished. PLaza 3-5058.

50'S, EAST—River view; 5-6 room duplex, with roof terrace, large living room, separated by plate glass partition from dining room; grand piano; furnished-unfurnished. Weekdays, Chace. PLaza 3-1700.

50'S (Sutton Place Vicinity)—River View, Garden Living Room; three rooms, bath; exquisite furnishings. $135. Immediate. PLaza 3-5572.

Note. Figure 1.C.4 shows examples of archival data sources. Panel (a) shows page three of the Dec. 31st 1956 NYCHA report "RACIAL DISTALBUTION IN OPERATING PROJECTS" indicating the racial distribution at initial occupancy and Dec. 31st 1956 for each project. Panel (b) shows page 8W of the New York Times real estate section; columns indicate the borough location as well broad rental characteristics such as furnished or not.
Source. LaGuardia and Wagner Archives, NYCHA collection, Box. Nr. 0071B6; NYT 28.04.1940

Table 1.C.5: Summary rent statistics

Year	Obs	Avg. rent	Avg. rent pr	Avg rooms
1930	8027	2034.31	773.87	4
		(1832.05)	(732.06)	(3)
1940	1890	1234.11	415.8	4
		(2154.22)	(978.84)	(3)
1950	1361	1213.67	511.5	3
		(815.71)	(357.64)	(4)
1960	1507	1183.12	365.95	4
		(860.66)	(223.77)	(2)
1970	1404	1856.95	589.76	3
		(1313.91)	(446.65)	(2)
1980	1285	1331.02	459.89	2
		(1576.09)	(556.38)	(3)
1990	1456	1415.23	606.08	2
		(1523.01)	(654.5)	(3)
2000	972	1661.08	481.33	3
		(2665.49)	(812.81)	(3)
2010	800	1548.88	328.54	3
		(4689.02)	(708.17)	(3)

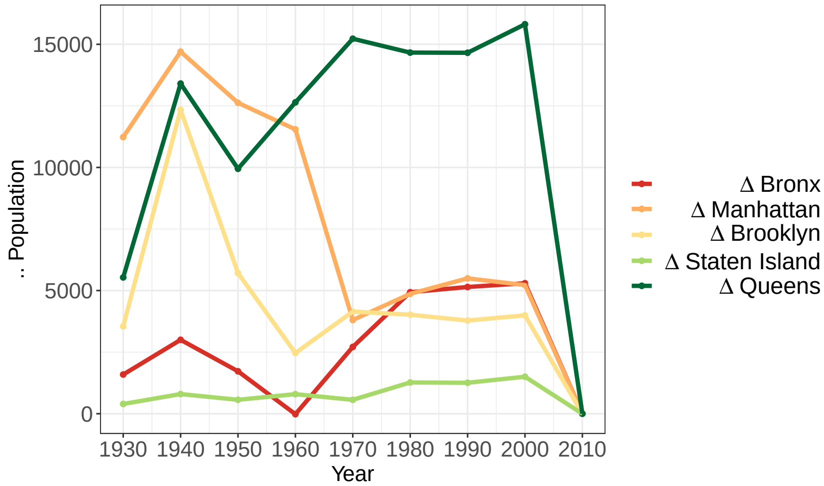
Note. Table 1.C.5 shows all rental listings used in the corresponding analysis by year. Column "Avg. rent" refers to the average monthly rent, column "Avg. rent pr" is the average rent per room per year and "Avg. room" is the mean of rooms across properties; standard deviations are given in parentheses.
Source. New York Times.

1.C.3 Tract Harmonisation

A major difficulty in making use of census tract-level data for this longitudinal analysis is that the tract boundaries change considerably across time, making it challenging to have a time consistent panel dataset. I tackle this problem by taking reweighing observation based on overlapping areas weights (AW) using 2010 census tract boundaries as target areas. Let S be the set of all overlapping land areas in target tract t then weighted estimates for target tract t are defined as $\hat{y}_t = \sum^S \frac{A_s}{A_t} * y_s$. However, this procedure is susceptible to error because it requires to assume a uniform spatial distribution of geographic information. For example, allocating half of tract Z's residents in 2000 to tract A and the other half to tract B in 2010. In that case, both affluent and poor residents of tract Z would be evenly split between the two 2010 tracts. However, poverty is likely not spatially evenly distributed.

Figure 1.C.5 compares the reweighed series for New York City Boroughs with the original series, both aggregated in borough level. Using AW weights created some deviation of the original population series especially until 1960.

Figure 1.C.5: Deviation due to boundary harmonization



This deviation is highest for Queens throughout the observation period while being lowest for Staten Island. Nevertheless, the degree of error depends on how tract boundaries change: consolidations, splits, and complex changes. The error would be expected to be larger for the latter two changes as discussed in Logan, Zhang **andothers** (2021).

Appendix 1.D Additional results

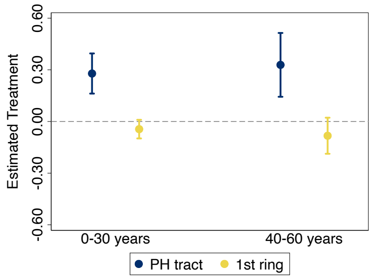
1.D.1 Pooled estimates

I estimate a version of Equation 1.1 and Equation 1.2 that aggregates post-treatment event year dummies into a medium and long run interval: $Post0-30$ (0 to 30 years) and $Post40-60$ (40 to 60 years). This division allows me to obtain more informative DiD estimates. Pooled effects will net out potential spikes or confidence and are based on the fact that effects for demographics and rent often materialize in the urban context. The following estimation equation aims to capture such differential effects over time:

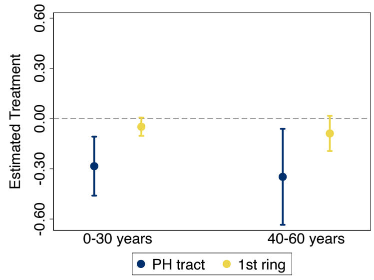
$$\begin{aligned} y_{m,p,t} = &\sum_{r\in R}\left(\theta_{0r}Post_{p,t}^{0-30} + \theta_{1r}Post_{p,t}^{40-60}\right) \times \not{1}(r = r(m,p)) \\ &+ \delta'\mathbf{X_{m,p,t}} + \rho_{p,t} + \zeta_{p,r(m,p),c} + u_{m,p,t} \end{aligned} \tag{1.19}$$

Figure 1.D.1: Pooled results - baseline

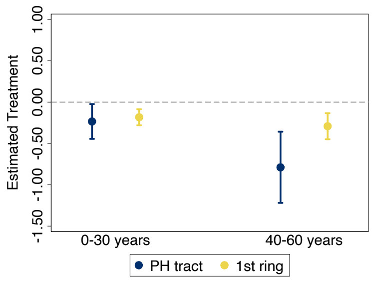
(a) Log(Population)



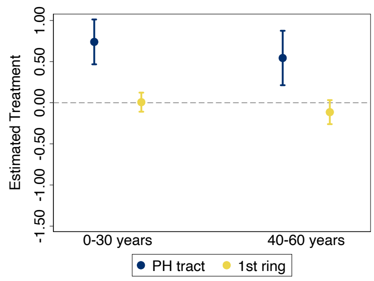
(b) log(Priv. population)



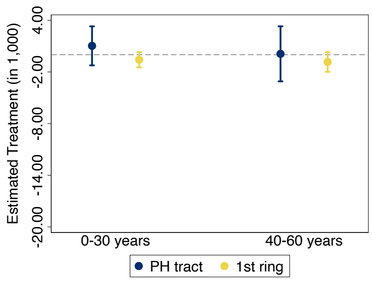
(c) Log(White)



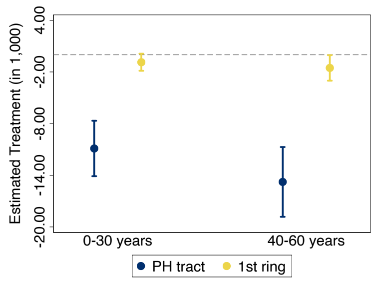
(d) log(Black)



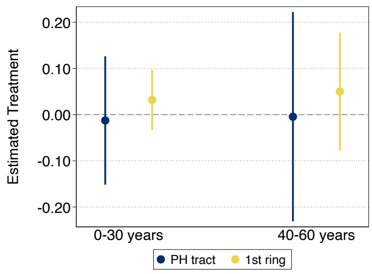
(e) Housing units)



(f) Priv. housing units

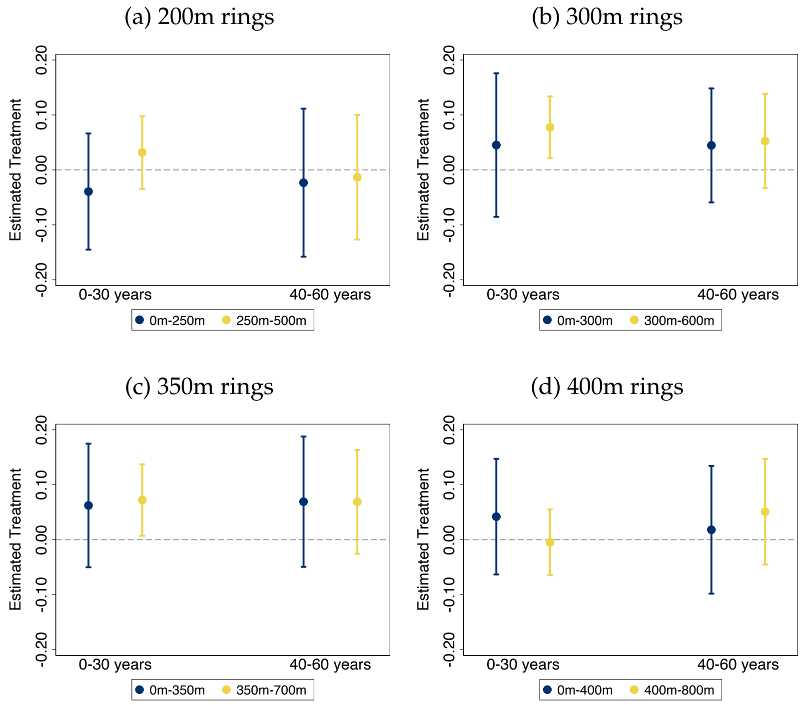


(g) Log(Rent)



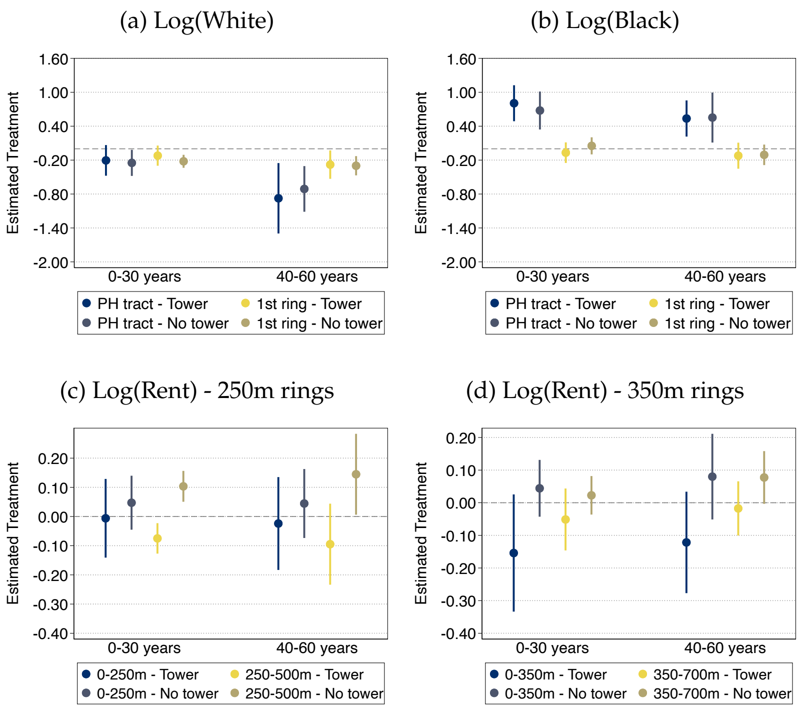
Note. Figure 1.D.1 reports point estimates for coefficients θ_{0r} and θ_{1r} in Equation 1.19; both coefficients have been interacted with ring dummies; standard errors are clustered at the project level; the vertical lines show the estimated 95% confidence intervals. Panel (a) to (f) report differences for treated tracts and tracts in the first ring compared to a second neighbour ring; outcome variables are obtained from the US census. Panel (f) shows point estimates using property level rent data. The omitted group are tracts and properties within th 2nd ring.

Figure 1.D.1: Pooled results - rents



Note. Figure 1.D.1 reports point estimates coefficients θ_{0r} and θ_{1r} in Equation Equation 1.19; both coefficients have been interacted with ring dummies; standard errors are clustered at the project level; the vertical lines show the estimated 95% confidence intervals. Panel (a) to (d) uses property level rent data with alternative distances rings of 250m, 300m, 350m and 400m. The omitted group is within a third distance that is 500m-750m, 600m-900m, 700m-1050m and 800m-1200m respectively.

Figure 1.D.2: Effect of relaxed "Tower in Park"

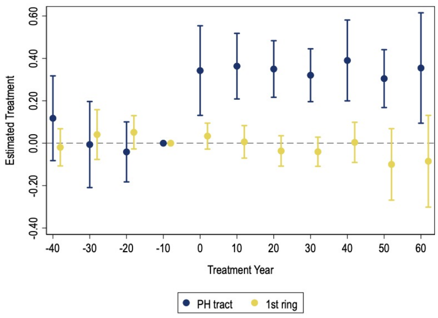


Note. Figure 1.D.2 reports point estimates for coefficients θ_{0r} and θ_{1r} in Equation 1.3; all coefficent have been interacted with Tower dummies; standard errors are clustered at the project level; the vertical lines show the estimated 95% confidence intervals. For this exercise two criteria described in Section 1.5.1 have been realxed: area share and consruction costs. Therfore a "Tower"is defined as a buildng with more than 10 stories and below 24% building coverage; doing so results in 89 tracts with "Tower"-style projects and 136 non-tower tracts. I estimate the following equation. Panel (a) to (b) report differences for treated tracts and tracts in the first ring compared to a second neighbour ring; panel (c) and (d) compare properties within a first (0m-250m; 0m-350m) and second distance ring (250m-500m; 350m-700m) around project tp those within a third ring (500m-750m; 700m-1050m).

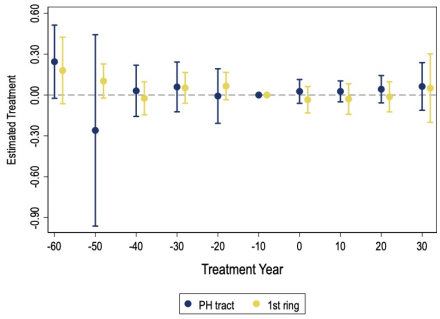
1.D.2 Event study results - Construction Periods

Figure 1.D.3: Construction period heterogeneity

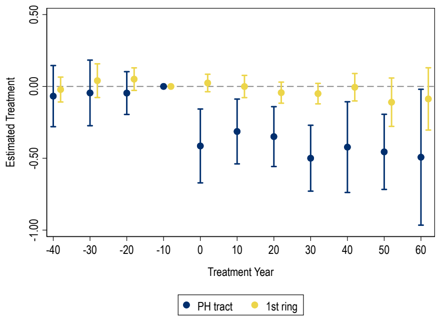
(a) Pre 1970: log(Population))



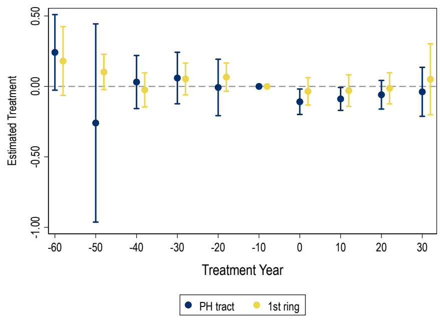
(b) Post 1970: log(Population))



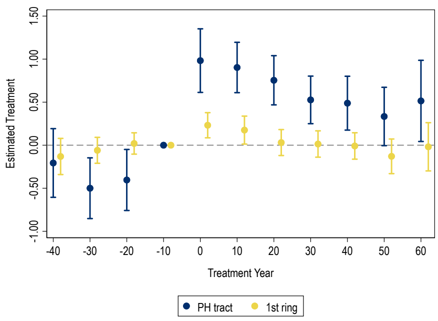
(c) Pre 1970: log(Priv. Population))



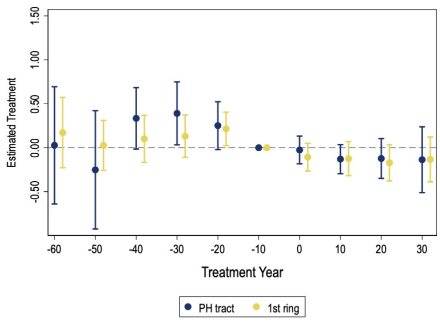
(d) Post 1970: log(Priv. Population))



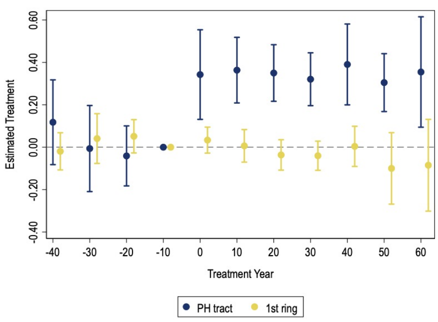
(e) Pre 1970: log(Black))



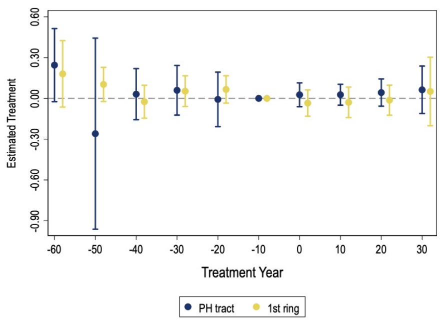
(f) Post 1970: log(Black))

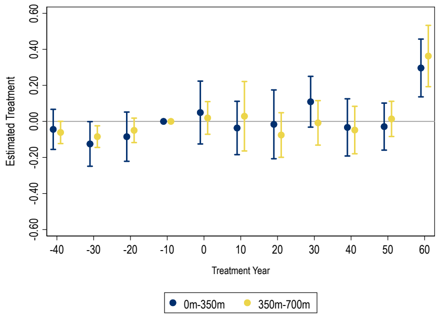


(g) Pre 1970: log(White))

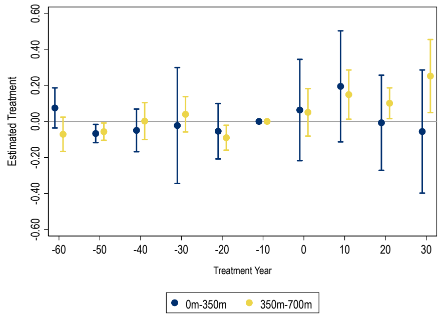


(h) Post 1970: log(White))





(i) Pre 1970: log(Rent))



(j) Post 1970: log(Rent))

Note. Figure 1.D.3 plots report coefficients $\hat{\beta}_{\tau,r}$ in Equation **??**; the sample is split in all ring panals with projects constructed before 1970 and afterwards; the vertical lines show the estimated 95% confidence intervals; Panel (a) to (h) use weighted unit counts from the US census; the omitted category is tracts within a second ring. Panel (i) and (j) use property level rent data comparing rents in a first ring (0m-350m) and a second ring (350m-700m) to properties 700m-1050m away; rental ask prices have been obtained from the New York Times.

1.D.3 Event study results - Building Design

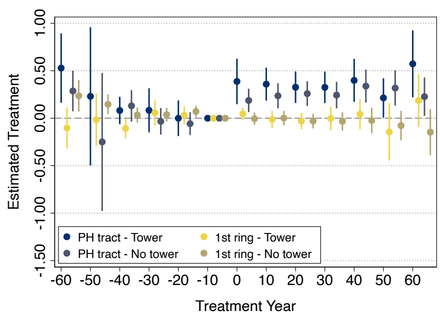
Using the definitions provided in Section 1.5.1, I defien a “Tower in the Park” as follows: a a project with a height larher than 9.87 and ground coverage below 23%. An “Adjusted Tower” is defined as public housing project with a height larher than 9.87, ground coverage below 23%, an area share above 20% and construction costs below \$17868. This takes construction quality and importance realtive to the area int account. If a project is not satisfying any of these crietria it is considered a “No Tower”. I estimate the following equation:

$$\begin{aligned} y_{m,p,t} = & \sum_{r \in R} \sum_{\tau=-60}^{60} (Tower + No\ tower) \times \beta_{\tau,r} \left(t - Y_p, r = r(m,p)\right) \\ & + \delta' \mathbf{X_{m,p,t}} + \rho_{p,t} + \zeta_{p,r(m,p),c} + u_{m,p,t} \end{aligned} \tag{1.20}$$

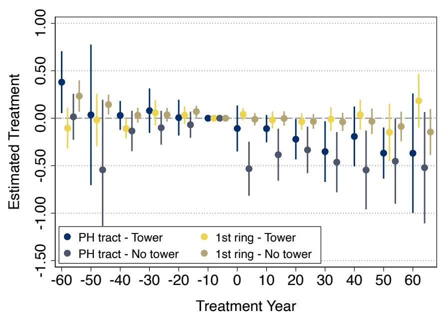
Thus, this estimation is similar to Equation 1.1, where $Tower$ and $No\ tower$ are dummies for tracts having a “Tower in the park” like projects and not. Results of estimating Equation 1.20 for “Tower”-style prjects are shown in Figure 1.D.3 and for “Adjusted Tower”-projects are shown in Figure 1.D.3.

Figure 1.D.3: Event study results “Towers”

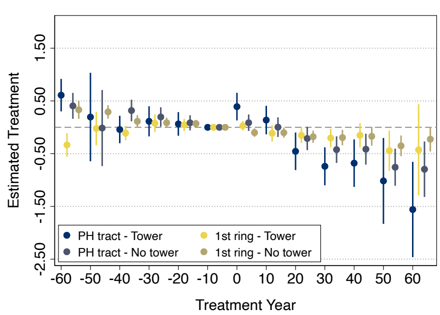
(a) log(Population)



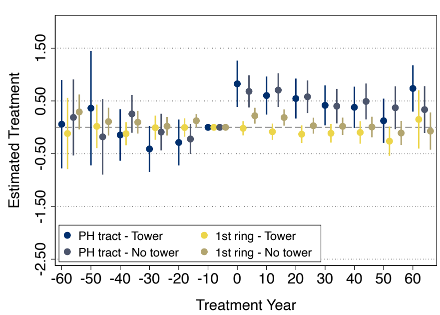
(b) log(Priv. Population)

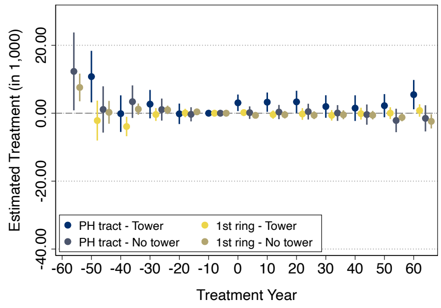


(c) log(White)

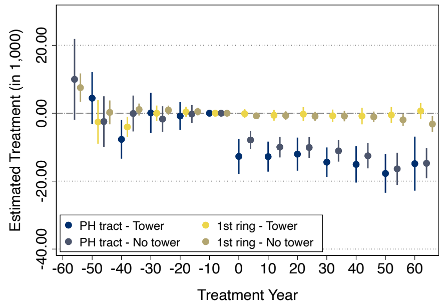


(d) log(Black)

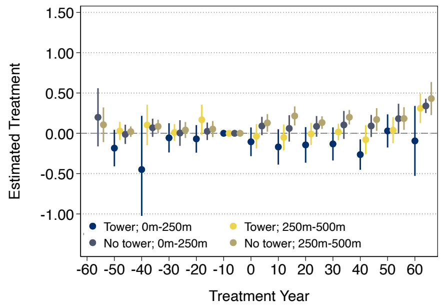




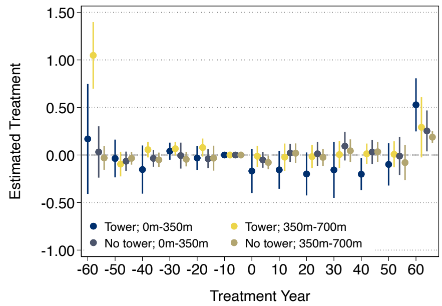
(e) Housing units



(f) Priv. housing units



(g) log(Rent) - 250m rings

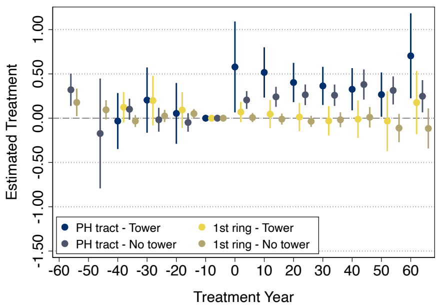


(h) log(Rent) - 350m rings

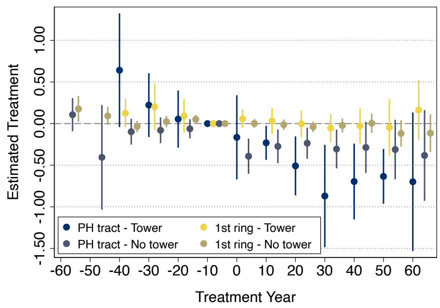
Note. Figure 1.D.3 report coefficients $\hat{\beta}_{\tau,r}$ in Equation 1.20; all coefficients have been interacted with "Tower" and non-tower dummies as per deintion in Section 1.5.1; the vertical lines show the estimated 95% confidence intervals; Panel (a) to (f) use weighted unit counts from the US census; the omitted category is tracts within a second ring. Panel (g) and (h) use property level rent data comparing rents in a first ring (0m-350m) and a second ring (350m-700m) to properties 700m-1050m away; rental ask prices have been obtained from the New York Times.

Figure 1.D.3: Event study results "Adjusted Towers"

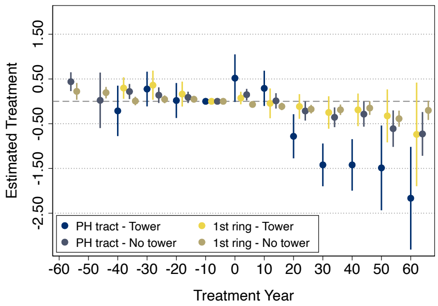
(a) log(Population)



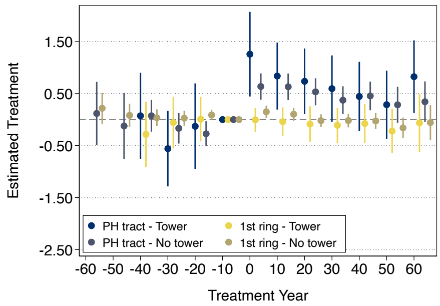
(b) log(Priv. Population)



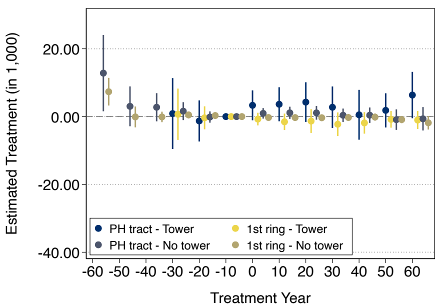
(c) log(White)



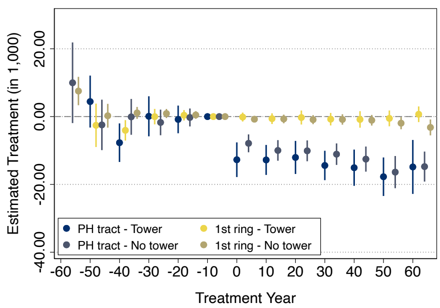
(d) log(Black)



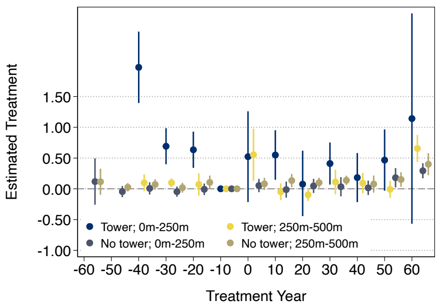
(e) Housing units



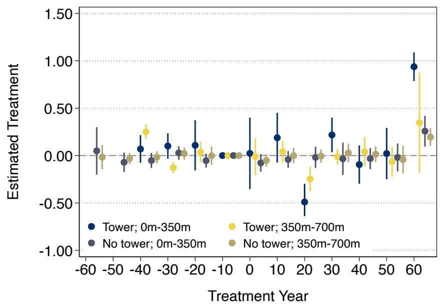
(f) Priv. housing units



(g) log(Rent) - 250m rings



(h) log(Rent) - 350m rings



Note. Figure 1.D.3 report coefficients $\hat{\beta}_{\tau,r}$ in Equation 1.20; all coefficients have been interacted with "Adjusted Tower" and non-tower dummies as per definition in Section 1.5.1; the vertical lines show the estimated 95% confidence intervals; Panel (a) to (f) use weighted unit counts from the US census; the omitted category is tracts within a second ring. Panel (g) and (h) use property level rent data comparing rents in a first ring (0m-350m) and a second ring (350m-700m) to properties 700m-1050m away; rental ask prices have been obtained from the New York Times.

1.D.4 Event study results - Panel setup

In this Section, I report event study results in this section using alternative estimators that correct for the shortcomings of standard two-way fixed-effects (TWFE) models. Specifically, the literature focused on the "forbidden" comparison between later-treated and earlier-treated units, which the TWFE estimator might not handle correctly. As shown in Goodman-Bacon (2021), the TWFE estimator might choose weights that lead to the estimator having the wrong sign. The estimators proposed in the literature differ in terms of who they use as the comparison group (e.g., not-yet-treated versus never-treated) and the pre-treatment periods used in the comparisons (e.g., the entire pre-treatment period versus the final untreated period).[18]

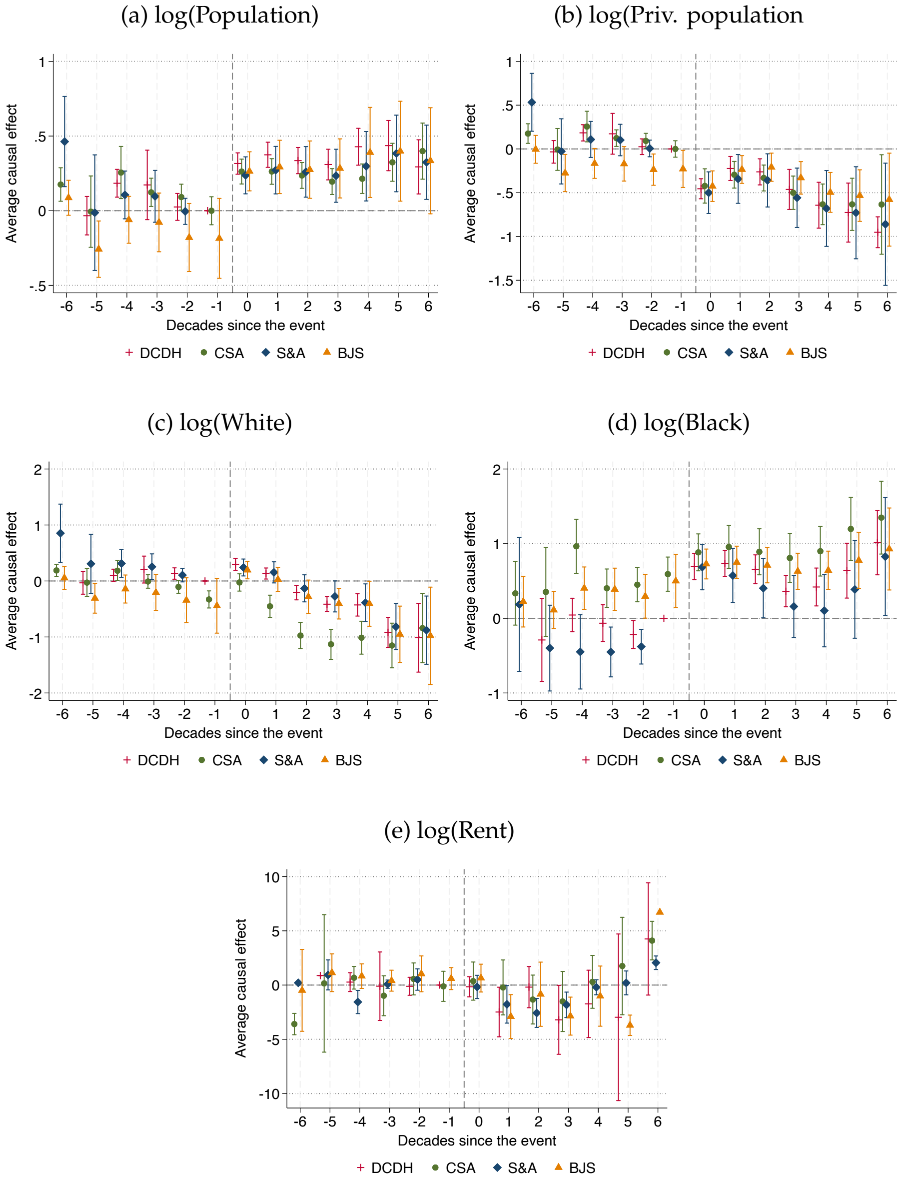
To test the coherence of the approach using a stacked design, as proposed in Section 1.3, I use the panel setup. In this setup, a tract is treated when it has had a public housing project within its boundaries at any point in time. To serve as the appropriate control group, I compare treated tracts to tracts in the second ring, surrounding the inner ring. This is motivated by two reasons. First, the second ring serves as a coherent control group from the stacked to the panel setup. Second, since it is reasonable to assume public housing generates spillovers, dropping the first tract ring around public housing will suffice not to violate STUVA. Figure 1.B.2 shows the spatial layout of treatment and control. I estimate the following dynamic specification:

$$y_{i,m,t} = \sum_{\tau=-60}^{70} \beta_\tau \left(t - Y_p\right) + \rho_t + \zeta_i + \Xi_{m,t} + u_{i,m,t} \tag{1.21}$$

The parameter of interest, denoted as β_τ, captures the effect of the arrival of public housing in census year t relative to the year of construction Y_p compared to the outermost rings. I control for census year ρ_t and tract ζ_i fixed effects. Finally, I allow tracts within a neighborhood to trend differently each year by including non-parametric neighborhood trends $\Xi_{m,t}$. Results from estimating Equation 1.21 are shown in Figure 1.D.4.

[18] I refer to Roth **andothers** (2023) for an excellent overview of recent advancements in the DiD literature and practical guidance on how these estimators differ.

Figure 1.D.4: Effect of public housing



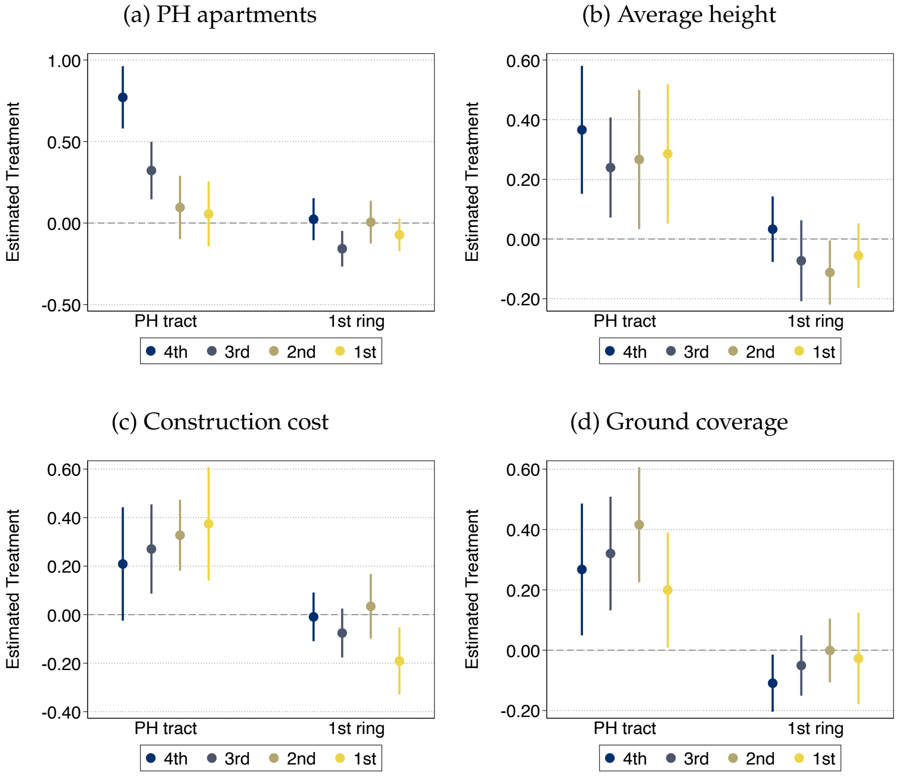
Note. Figure 1.D.4 displays coefficients $\hat{\beta}_\tau$ from estimating Equation 1.21. Panel (a) reports results using the total population, (b) the net population, (c) white population and (d) white population as outcome variable; Panel (e) uses property level rent data. For furher details on the outcome variables see Section 1.2.2. The abbreviations refer to the following estimators: DCDH, de Chaisemartin and D'Haultfoeuille estimator (De Chaisemartin **and** D'Haultfœuille, 2020); CSA, Callaway and Sant'Anna estimator (Callaway **and** Sant'Anna, 2021); S&A, Sun and Abraham estimator (Sun **and** Abraham, 2021); BJS, Borusyak imputation estimator (Borusyak **andothers**, 2021). Note that the CSA estimator does not allow for non-parametric neighborhood time trends. Therefore, I control for the outcome variable at baseline. The bar denotes 95% confidence intervals; standard errors are clustered at the neighborhood (NTA) level.

1.D.5 Public Housing Characteristics

In this section I test for the effects of different poublic housing characteristics on the set of outcome variables. In particular I am testing for foour buildings characetristics which are used in Section 1.5.1 to define a "Tower in the Park". I test the effect of project size and layout, by using the average number public apartments relative to the existing housing stock[19], the average height of all project buildings within a tract and the total area used for construction relative to the total tract area. To test for differences in buildings quality I use construction costs per room as a measure of construction quality. I estimate Equation 1.4 in Section 1.5.2 by interacting the ring dummies with quartiles of the respective public housing characteristics. Results for are given Shown by Figure 1.D.5 to Figure 1.D.7 .

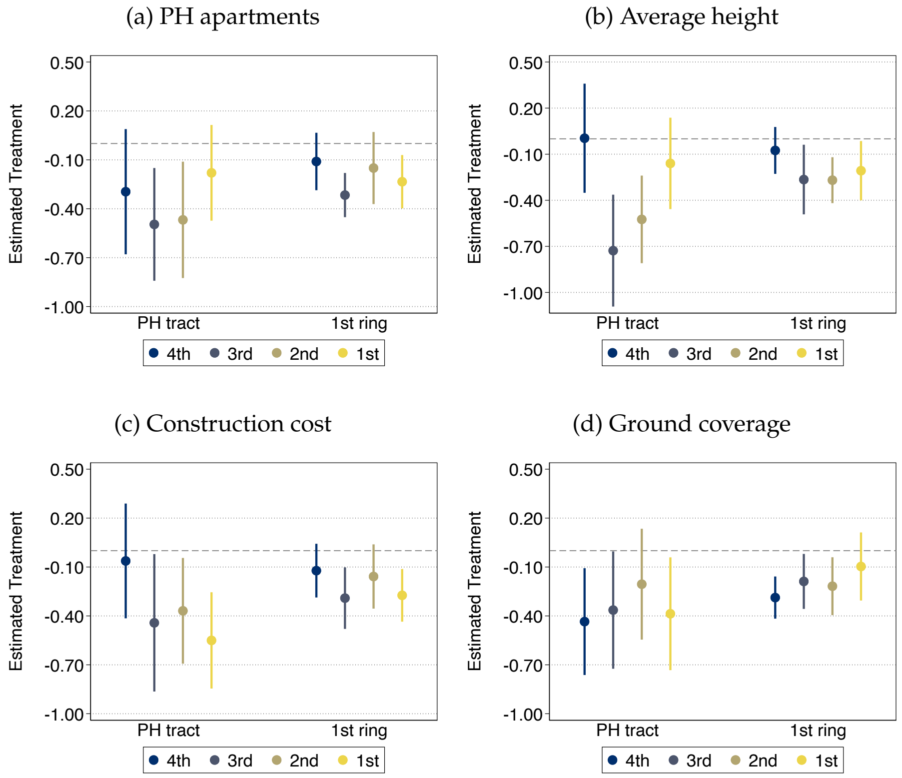
[19]Using the the stock in the respective year would mean measuring public housing units against itself. Therefore, I use the housing in the respective census year before public housing has been built and take the average over the decade in order to account for potential changes within the 10 years between census years.

Figure 1.D.5: Effect on log(pop)



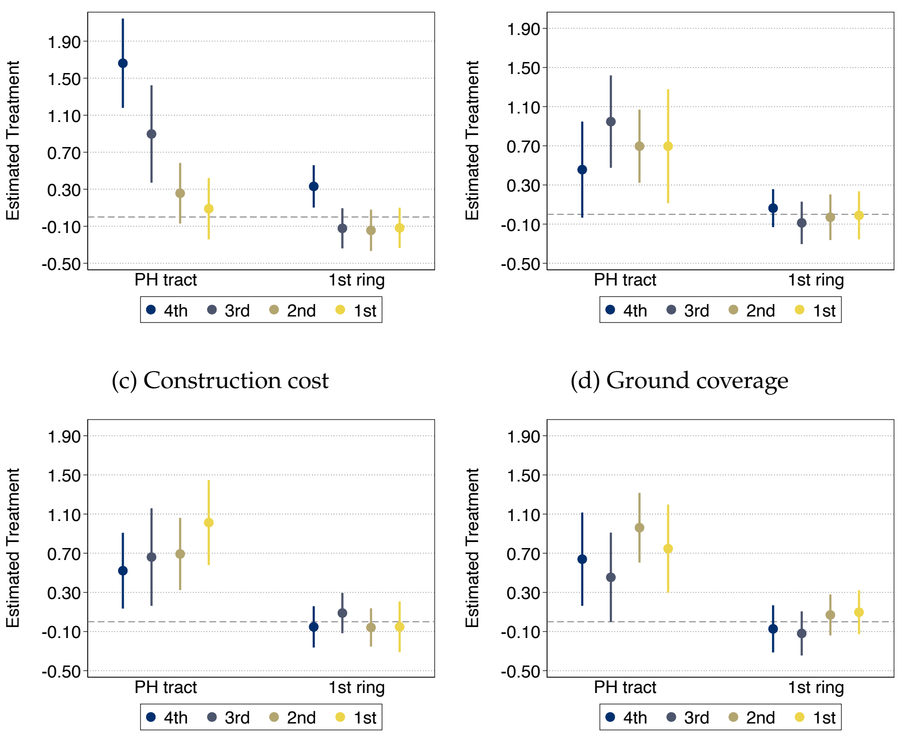
Note: Figure 1.D.5 reports point estimates for coefficients γ_{0q} and γ_{1q} in Equation 1.4; coefficients report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; all both coefficients have been interacted with quartiles indicators of distributions of the average number public apartments relative to the existing housing stock (Panel (a)), the average height of all project buildings within a tract (Panel (b)), construction costs per room (Panel (c)) and ground coverage (Panel (d)); the vertical lines show the estimated 95% confidence intervals; report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; outcome variables are obtained from the US census.

Figure 1.D.6: Effect on log(white)



Note. Figure 1.D.6 reports point estimates for coefficients γ_{0q} and γ_{1q} in Equation 1.4; coefficients report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; all both coefficients have been interacted with quartiles indicators of distributions of the average number public apartments relative to the existing housing stock (Panel (a)), the average height of all project buildings within a tract (Panel (b)), construction costs per room (Panel (c)) and ground coverage (Panel (d)); the vertical lines show the estimated 95% confidence intervals; report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; outcome variables are obtained from the US census.

Figure 1.D.7: Effect on log(black)



(a) PH apartments (b) Average height

(c) Construction cost (d) Ground coverage

Note. Figure 1.D.7 reports point estimates for coefficients γ_{0q} and γ_{1q} in Equation 1.4; coefficients report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; all both coefficients have been interacted with quartiles indicators of distributions of the average number public apartments relative to the existing housing stock (Panel (a)), the average height of all project buildings within a tract (Panel (b)), construction costs per room (Panel (c)) and ground coverage (Panel (d)); the vertical lines show the estimated 95% confidence intervals; report differences for treated tracts and tracts in the first ring compared to a second neighbor ring; outcome variables are obtained from the US census.

Appendix 1.E Model estimation

In this section I detail the solution method for the model. The model solved if there exists a solution for the equilibrium given by Equations 1.11 to 1.13 derived in Section 1.6. Expanding the equlibrium's vector form gives:

$$\begin{bmatrix} D_{1t}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}; \beta) - S_{1t}(r_{1t}) \\ \vdots \\ D_{Mt}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}; \beta) - S_{Mt}(r_{Mt}) \end{bmatrix} = 0$$

$$\begin{bmatrix} \frac{D^b_{1t}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}, \beta)}{D_{1t}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}; \beta)} - s^b_{1t} \\ \vdots \\ \frac{D^b_{Mt}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}, \beta)}{D_{Mt}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}; \beta)} - s^b_{Mt} \end{bmatrix} = 0$$

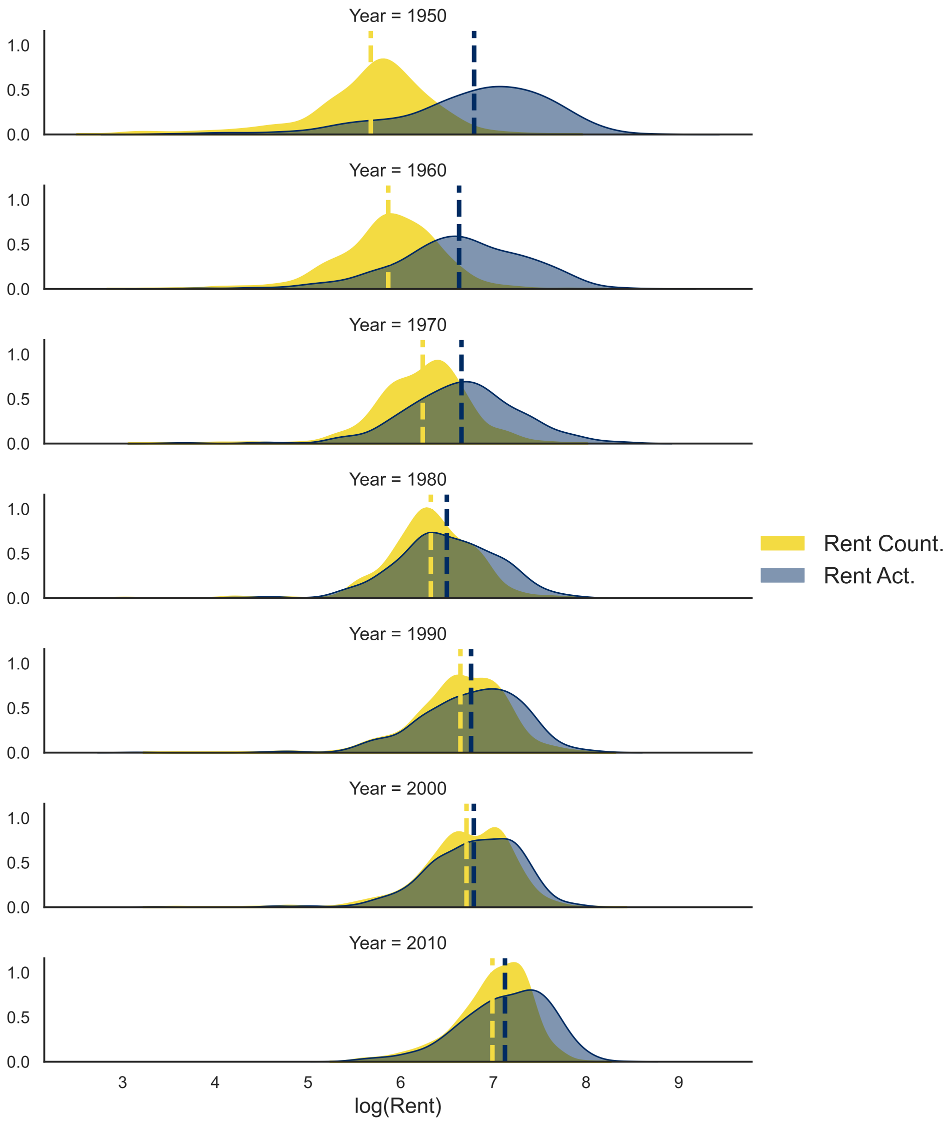
$$\begin{bmatrix} \frac{D^w_{1t}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}, \beta)}{D_{1t}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}; \beta)} - s^w_{1t} \\ \vdots \\ \frac{D^w_{Mt}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}, \beta)}{D_{Mt}(\mathbf{r}, \mathbf{s^w}, \mathbf{s^b}; \beta)} - s^w_{Mt} \end{bmatrix} = 0$$

A solution to this system of 3xM system of equations will set it simultaneously to zero. The correspodning vector consists of three vectors; one vector of rents, one for shares of blacks and one for shares of whites, given values for ϕ, δ_{mt}, $\boldsymbol{\beta}^g$. To find this fixed point, I use the Newton's Method solution algorithm. Newton's Method iterates over the above system of equations for an initial guess $x_0 = (\mathbf{r}^0, \mathbf{s^w}^0, \mathbf{s^b}^0)$ and tries to find a critical vector such that $f'(x^*) = 0$.

$$x_{n+1} = x_n - \frac{f(x_n)}{f'(x_n)} \tag{1.22}$$

Where $f'$is the Jacobien of the equilibrium system. Since I am only interest in finding the root of the 3xM system of equation, I do not use f'' which would check if the solution is a local maximum or minimum. I set the tolerance criteria to $\|x_{n+1} - x_n\| < e^{-10}$. I use the JAX automatic differentiation package in Python. Note that, since the model is static, there M neighbohoods in each time period t and estimate the model for each time period seperatly. I keep ϕ and $\boldsymbol{\beta}^g$ constant and only recalibrate δ_{mt} for each t using Equation 1.16.

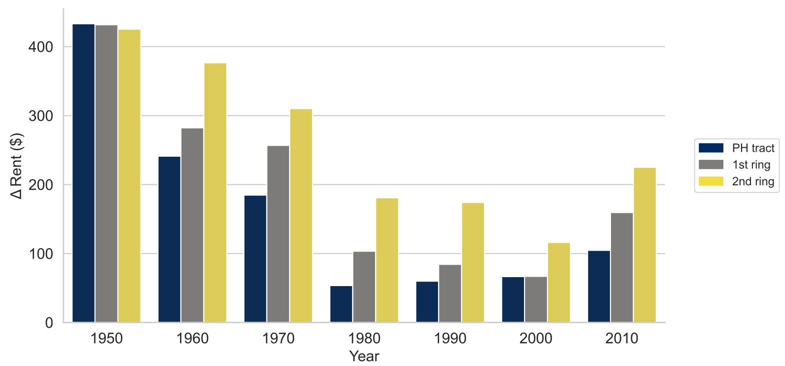
Figure 1.E.1: Equilibrium Rent Distributions



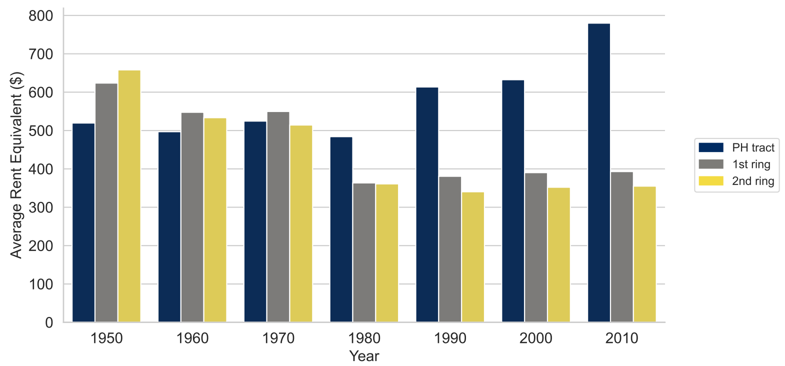
Note. Figure 1.E.1 reports distributions of the estimated equilibrium rent. Estimates under the actual scenario - no public housing demolitions - are shown in blue and those under the counterfactual scenario - removing all public housing - are shown in yellow; the dotted lines give the average of each distribution. The model had been estimated for each census year.

Figure 1.E.2: Equilibrium Differentials by Distance Relationship

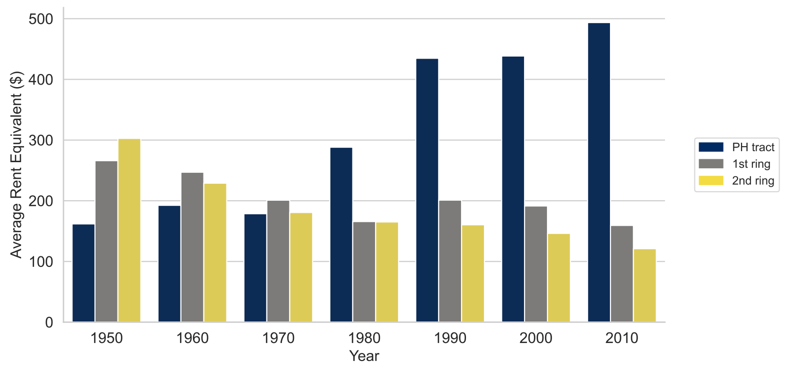
(a) Rent Differential



(b) Rent Equivalent Blacks



(c) Rent Equivalent Whitesl



Note. Figure 1.E.2 reports differentials from the removal of all public housing projects in New York City and letting the stock become private. Panel (a) reports the average difference between predicted actual and countercultural rents by distance rings. Panel (b) and Panel (c) display the average Rent Equivalents as calculated by Equation 1.18 by distance ring.

Chapter 2

Under Control? The effects of New York City Rent Control on 1920s Housing Market

Joint with Sun Kyoung Lee and Ronan Lyons

Contribution

My contribution to this study includes collecting, cleaning and managing additional raw data and preparing the final data set, performing the analyses, preparing the tables and figures for the manuscript, and co-writing the manuscript.

2.1 Introduction

In recent years, the issue of housing affordability has become more pressing due to the increases in rental rates and housing prices in many urban centers. This has led to calls for governmental intervention to address the hardships experienced by households. Among the many policy instruments available, rent control is the most prominent regulatory measure and has enjoyed long-standing popular support. However, like any governmental intervention, rent control has both intended and unintended consequences that are complex. Rent controls can result in welfare losses due to misallocating investments, residents, and untargeted design. Several studies have pointed out this issue (Diamond, McQuade **and** Qian, 2019; Autor **andothers**, 2014; Sims, 2007; Glaeser **and** Luttmer, 2003).

Previous studies have examined the effects of rent control policies on the housing market, specifically the two most common designs: first-generation rent ceilings and second-generation rent growth controls. However, in this paper, we study a new policy design, the 1920 New York City (NYC) rent control laws, which have not yet been studied. This policy design combined modern Just Cause Evictions elements with the legal authority to control prices. The 1920 laws gave elected civil court judges the power to determine whether a rent increase was "reasonable" or not, providing them with discretionary authority to set rents based on their ideas of "reasonableness." This resulted in the emergence of "tenant" and "landlord" judges who openly advocated for the interests of their respective sides. (Rajasekaran **andothers**, 2019; Fogelson, 2013).

We exploit this feature of NYC rent control by using the binding nature of municipal court district (MCD) boundaries and implementing a Regression Discontinuity Design to measure the effects of rent control on market rents and transaction prices. In particular, we use the distance to the court borders between tenant and landlord judge districts. To measure the judge's leniency, we use variation in a judge's party affiliation and argue that Democrat judges ruled in favor of tenants. In contrast, Republican judges ruled in favor of landlords. We complement this approach with an event study design, allowing us to exploit districts' continuous nature with both Democrat and Republican judges.

To study the 1920 NYC rent control laws, we assemble a novel database of housing market outcomes for NYC from 1918 to 1926. We collect property-level rental and transaction price information from two sources. Firstly, we use the New York Times real estate section, henceforth market rents. Secondly, we collect prices from the Real Estate Record and Builders' Guide, a weekly publication of real estate transactions. The final samples consist of 12,186 rental and 8,945 transaction-based observations. Next, to study the policy mechanism, we collect information on all municipal district court judges, including their political affiliations and election cycles, from the NYC Official City Directory. Finally, we collect a sample of newspaper articles that cover landlord-tenant cases, which enables us to infer the decision behavior of about 42 judges. This enables us to show the relationship between political affiliation and judge decisions.

We find that in Republican-controlled districts, rents at the boundary jumped by about 10% after the policy was introduced, while before the introduction of the policy, rent prices were smooth at the boundary. These results are confirmed by magnitude and significance using an event study approach. Mixed districts can expect 6% - 8% higher rent prices than Democrat-only districts. Since we do not observe the individual judge decisions, we rationalize these results through a simple mechanism. If a landlord cannot be sure she is facing a tenant judge, she will always pay the controlled rent or refrain from increasing rent since asking for a higher rent can lead to costly lawsuits. The 1920 rent control laws would have allowed tenants to withhold rent and wait until their landlord brought the case forward before a judge to obtain an eviction warrant. This could generate non-recoverable income losses for the landlord. We do not find evidence that rent control affected commercial and residential transaction prices. This can be because landlords expected controls to last only temporarily; in the long run, expected earnings would not have been affected.

Our paper is related to the vast literature investigating the effect of rent control on rent prices.[1] The most prominent effect of rent control is related to the policy's effectiveness on prices (of controlled properties). A broad consensus of papers finds rent control effective, decreasing rental prices in controlled properties that can generate a rental discount (Olsen, 1972; Linneman, 1987).

However, several distortions or landlord responses are challenging the policy's effectiveness in providing affordable housing. We highlight the three most important ones for this paper. First, lower rents in control properties can lead to distortions in the supply of rental housing. A branch of the literature shows that rent control can lead to a contraction in the supply of rental units (Sagner **and** Voigtländer, 2023; Kholodilin **and** Kohl, 2023; Sims, 2007). This can be attributed to lower expected profits for developers (Basu **and** Emerson, 2000), conversions of rentals into owner-occupied properties (Diamond, McQuade **and** Qian, 2019; Smith **and** Tomlinson, 1981), or lower vacancies due to longer tenancies in controlled properties (Krol **and** Svorny, 2005; Wilhelmsson **andothers**, 2011; Arnott **and** Igarashi, 2000). However, this view is not unchallenged in the literature. Most notably, Jofre-Monseny **andothers**, 2023 finds rent control effective while having no negative supply effect in Massachusetts. Second, rent control can impact housing quality by reducing the incentive to invest in maintenance. This can reduce the value of controlled properties (Moon **and** Stotsky, 1993; Gyourko **and** Linneman, 1990; Sims, 2007). Third, these direct effects of rent control lead to various external or indirect effects. Rent control segments the real market, and demand for rental properties spills over into the uncontrolled sector, leading to higher rents in the uncontrolled sector (Mense **andothers**, 2019; Dolls **andothers**, nodate). Reduced supply, in combination with landlord behavior, can change the composition of neighborhoods and lead to gentrification or increased homeownership and property values (Diamond, McQuade **and** Qian, 2019; Fetter, 2016). Moreover, lower quality can spill over to adjacent areas, rendering the neighborhood less attractive and reducing the values of uncontrolled properties (Autor **andothers**, 2014) or even attract crime (Autor **andothers**, 2017).

This literature usually finds that the higher the intensity of rent control, the stronger its effects (Fetter, 2016; Early, 2000; Breidenbach **andothers**, 2019). This can be attributed to variations in the design of rent control. For example,

[1] We restrict our literature to the most recent contributions and those most relevant to the US context. For an excellent overview of the literature, see Kholodilin, 2024

differences in price ceilings and price increase limitations or negligence in their execution can render controls differentially effective.

We contribute to this literature by investigating a new policy design that works through judges' discretion over rents. We propose a mechanism affecting landlords' profit expectations due to costly law proceedings. We further show that there are no effects on transaction prices. However, we find suggestive evidence that rent control shifted the construction profitability, leading to higher buildings in less controlled districts.

Second, we build on the literature on judges. This literature mainly asks what affects a judge's harshness. Both Gordon, 2007 and Lim, Snyder **and** Strömberg, 2015 find that elected judges impose longer sentences than appointed ones. Moreover, partisan judicial elections tend to mirror political election results. Lim, Snyder **and** Strömberg, 2015 find that voters in partisan elections vote based on their party loyalty simply as a short-cut or tie-breaking rule, which, in turn, is revealed by the non-significant effects of media coverage decision harshness. Lim, Silveira **andothers**, 2016 find no systematic evidence that sentencing decisions are strongly influenced by party affiliation in partisan elections. Most of this literature is dealing with criminal charges. In a different context, Lim **and** Yurukoglu, 2018 shows that party affiliation, precisely the proportion of Republicans on the public utility commission, is strongly related to critical decisions such as the adjudication of return on equity to electric utilities. Finally, Mueller-Smith, 2015 shows that judges may vary in their relative treatment of different types, allowing a given assignment to increase or decrease the probability of incarceration depending on a given defendant's traits.

We contribute to this literature by showing that elected officials' political affiliation affects their decision-making according to the party's ideology.

The paper is organized as follows. Section 2.2 describes the historical and institutional context. Section 2.3 discusses the data sources and provides evidence on judges' decision-making behavior. Section 2.4 introduces the mechanism and discusses the empirical analysis. In Section 2.5, we estimate the effect of rent controls using a regression discontinuity design. We complement this strategy with an event study approach in Section 2.6. Section 2.7 concludes.

2.2 Historical and Institutional Context

World War I had a significant impact on New York City's housing situation. The war led to a shift in resource allocation away from construction, which ended the pre-war housing boom. With rising population, this caused vacancy rates to plummet from 5.6% in March 1916 to 0.2% in February 1921 (Grebler, 1952). Consequently, housing prices rose by 5.1% between 1914 and 1918, with a further 15% increase between June 1919 and June 1920 (BLS, 1941), though anecdotal evidence suggests price increases were more pronounced. For instance, the monthly rent for a small four-room apartment increased by 125% in four months, from $18.50 in June to $42.00 by September. Another apartment on Park Avenue and 92nd Street saw its annual rent jump from $2,400 to $5,750 by October 1st (Fogelson, 2013; New York (State)., 1921). This situation led to tensions between tenant trade unions and landlords, resulting in rent strikes and harassment.

In response to rising rents, the state government implemented rent control laws in 1920. Rent control was introduced in April 1920 and later amended in September 1920. The laws stipulated that (Fogelson, 2013):

- rent increases of more than 25 percent per year were unjust, unreasonable, and oppressive,
- the rent laws applied to all buildings built before April 1920 (September 1920) while new construction was exempted and
- the municipal district court judges were empowered to judge over the fact if a rent increase was 'reasonable' and an eviction warrant was applicable. By this, the judges could grant stays of up to twelve months and undo un-reasonable rent.
- A landlord who failed to furnish essential services could be charged with a misdemeanor, which was punishable by a fine of $1,000, a year in prison, or both.

The design of this policy, with municipal court judges[2] making decisions that could act as rent ceilings depended significantly on the judges' attitudes. Contemporary accounts noted that municipal district judges wielded more power than ever before, as they could rule on the reasonableness of rent increases. The judges could determine the reasonableness of a rent increase, subject to judicial interpretation, and rule out increases. It was within the judge's power to approve a rent hike of more than 25 percent or disapprove one of less than 25 percent (Fogelson, 2013).

[2]In 1920, there were 24 municipal court districts (MCDs), and the number of MCDs increased to 25 in 1924, 26 in 1930, 27 in 1931, and 28 from 1932 onwards. Each judicial district has at least one judge, which can vary up to six. On average, there are 2.6 judges per court district. The total number of judges by district increased over time as well. There were 45 judges in 1918, 53 in 1930, and 64 in 1934.

Municipal court judges served ten-year terms and were eligible for election if they resided in the district and had served as an Attorney of State for at least five years. Judges could be removed for cause by a two-thirds vote of the State Senate upon the Governor's recommendation.[3] Judges were significant public figures whose appearances, opinions, and decisions were frequently covered by newspapers. Elected in partisan elections, judges were incentivized to make public proclamations, particularly regarding rent laws, to mobilize voter support. Some judges, such as Peter A. Sheil, publicly embraced the arrival of the rent laws by proclaiming that the "days of the greedy landlord are gone" by now.[4] Others went further by making predictions about their future decisions. For example, Jacob Strahl, judge at the 4th District Court in Brooklyn, was regarded as "the tenants' friend." In late April 1920, Strahl announced that he would not issue eviction warrants on May 1st [expiration for unspecified leases under common law], and shortly after that, he said he would not dispossess anyone for failing to pay a rent increase. Similarly, William E. Morris announced, "I'll say right now I'm a pro-tenant and I don't care who knows it." [5] On the other hand, Peter A. Sheil, judge at the 1st District Court in the Bronx, favored landlords. Of the more than two hundred tenants who appeared before him in late April for non-payment of rent, only a few had their raises reduced and then only by one or two dollars. Most were ordered to pay the total increase. This behavior led to the opinion that there were "tenant judges" and "landlord judges." (Fogelson, 2013).

The Emergency rent laws were subject to heavy criticism through their existence from several parties, including real estate interest groups such as the Greater New York Taxpayer Association (GNYT). Nevertheless, the laws were further extended mainly based on advice from the Stein Commission, a body implemented in August 1923 by Governor Smith. However, at the end of 1926, construction was booming again. Until October 1st, 1927, there was a net increase of more than 94,000 apartments, with more than 74,000 in new-law tenements and the rest in one- and two-family houses. In addition, vacancy rates increased to 2.2 percent in March 1925 (Grebler, 1952), and criticism, especially from Isidor Berger, president of the GNYT, mounted. Based on a second report by the Stein Commission in 1925, which mainly stated that conditions were improving (Fogelson, 2013), a phase-out began in 1926 in the form of luxury decontrol, exempting units renting for more than $20 per room per month. After 1928,

[3]The Mayor could fill vacancies for the remainder of the year, with full-term elections held during the next general election. According to the Green Book, judges' salaries were $9,000 in Manhattan, Bronx, and Brooklyn, and $8,000 in Queens and Richmond.

[4]Bronx Judges Override 10P.C. Ruling on Rents. (1921, October 6). New York Tribune.

[5]Landlords' Greed Stirs Wrath of Justice Morris. (1920, August 11). The Sun and New York Herald, 16.

apartments renting for $10 or more per room per month were excluded. The laws expired in April 1929 (Collins, 2013).

2.3 Data and Difference among Judges

This section describes how we construct the dataset on New York City's housing market outcomes from 1918 to 1926. Constructing such a novel database requires various data digitization. In this section, we describe the data construction procedure and snippets of sample data. We report annual summary statistics for the datasets in Appendix 2.B Table 2.B.2. We further provide suggestive evidence using newspaper articles on landlord-tenant cases that judges decided based on party ideology.

2.3.1 Data

Housing Market-related Outcomes First, we use newly disaggregated data collected at Trinity College Dublin to obtain residential rent price data and property characteristics. The data were randomly drawn from the New York Times real estate section. Advertisements were included *only if* all of the following criteria could be verified: the broader location, the number of rooms, the price, and the type of the object. The cut-off date for sampling was the last Sunday for the second month in a quarter. The data provide information about the building's characteristics, such as the address, the rent, the number of bedrooms, and utilities included in the rent.

Using the Google Maps API, we create the geo-coordinates of the addresses (from the above digitized data). However, as house numbers of these addresses have changed, using the current algorithm to geolocate these addresses may yield a different location than the precise location information. Moreover, there have been some street name changes in the city. Instead of using house number information, we use street intersections to create geo coordinates to address house number changes. To address street name changes, we correct the street addresses using Bromley fire insurance maps and the PLUTO 2002 shapefiles. Figure 2.A.6 shows a detail of manually corrected observations and the underlying lots, addresses, and house numbers.

Second, we use the archival books called *the Real Estate Record and Builders' Guide* (henceforth, *Guide*). It is a weekly publication of real estate transactions, land, mortgage, building permit listings, and commentary on the real estate market. From this source, we collect conveyances and recorded leases (the latter is a work in progress). We use digitized copies of the original books (Figure 2.1) and convert these images into machine-readable property records. Through this process, we have 8,945 conveyance records from 1918 to 1926. Due to the abovementioned issues, we only keep observations with street information, yielding 23’002 observations. We geocode these data using solely cross-road information.

Figure 2.A.2 and Figure 2.A.3 show the spatial distribution of our rent and price data. We obtain the hedonic rent by neighborhood tabulation area (NTA). We observe Manhattan and the Bronx consistently in both data sets, while other city areas are only measured incompletely. However, we seem to be able to capture the main characteristics of NYC’s rental market. For example, in all years, the Lower East Side is one of the poorer neighborhoods, while the Upper West and Upper East Side represent more affluent residents. The pattern for transaction prices is similar.[6]

Figure 2.B.3 shows the rent indices for NYC plotted against indices by the Bureau of Labour Statistics and NY Fed. Both indices on Panel A and B follow broadly the same pattern. We can say that the rent index matches the overall trend very well. However, our index spikes at the beginning of the period and flattens out stronger over the period. Further investigating the bias of our rent data by plotting the rent distribution against the distribution in the 1930 US Federal Census reveals that our distribution statistically dominates the census distribution. Thus, our data stem from the upper end of the market and are geographically centered in Manhattan.

Judge Information Third, we gather information about judges from the NYC Official City directory, known as *the Green Book*. This directory provides each judge’s municipal court district (MCD), party affiliation, and re-election date. All judges in our study are affiliated with a political party. The majority are Democrats (93 judges), followed by Republicans (30 judges), one Liberal Party affiliate, and one Socialist Party member. Over our study period, the share of Republican judges by MCD has fluctuated. Figure 2.A.4 illustrates the spatial distribution of Republican judges in each MCD for selected years. This distribution remains relatively stable during the rent control years until 1928. However, in the early 1930s, the share of Republican judges steadily declined, nearly disappearing by 1935.

To support our assertion that Democrat judges favored tenants while Republicans sided with landlords, we collected 72 newspaper articles about judges. These

[6]We further investigate the spatial representativeness of the data by using rental data. To assess whether this bias stems from the fact that we only observe part of the city’s neighborhoods, we calculate frequency weights as the number of observations within a neighborhood divided by the total number of rental observations in Figure 2.B.4. This confirms that higher average rents in our sample largely stem from spatial bias.

Figure 2.1: Examples of data sources

(a) New York Times


2 Rooms & Bath $100 A Month
Telephone and Maid Service Included.
Open Fireplaces.
Also 3 rooms and bath,
Living room 18 ft. x 28 ft.,
19 & 21 West 31st St.
Strictly High-Class Fireproof Apartment

55 West 86th St.
JUST COMPLETED
High class housekeeping, kitchenette or bachelor apartments. Exceptionally large, light rooms with unusually spacious closets.

Living room 13x23, all outside, extra large rooms, elevator; new and thoroughly modern.
SENIOR & ALLEN, Inc.
505 Fifth Ave.

4 ROOMS $65.00
Large and light, beautifully decorated, all improvements; lease responsible party. Apply Supt.
569 WEST 125TH ST.

SEVEN ROOMS
AND TWO BATHS
1109-1111 Madison Ave.
CORNER 83D ST.
Elegant high-class apartment. All light rooms. Possession. Rent $3,500 per annum.
JOHN A. SCHOEN, 115 Bible House. Tel. Stuy. 7695.

Hendrik Hudson Annex
110th Street & Broadway
Northwest Corner.
7 Rooms, $3,100.
8 Rooms (Corner) $3,600.
The Rockfall
545 West 111th St.
Northeast Corner Broadway.
6 and 7 Rooms, $2,500.
8 Rooms (Corner) $3,200.
Apply on premises, or
NASSOIT & LANNING,
B'way & 89th St. Tel. 8380 Riverside

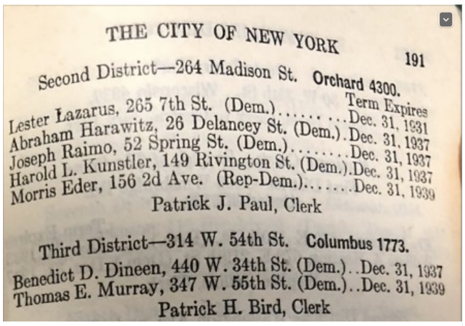
56 ST.—342 WEST
ONE BLOCK FROM BROADWAY.
High-Class Elevator Apartment House.
3 ROOMS AND BATH.
APPLY SUPT. ON PREMISES.

690 RIVERSIDE DRIVE,
Cor. 146th Street, elevator apartment, 4 large rooms; immediate possession.
Rent $2100. Apply on Premises.

Unfurnished—East Side.

Only 2 Apartments Left
Belgrave Block
Madison Ave., 49th to 50th St.
2 - 3 Rooms—$900 to $1,500
Cruikshank Company
141 Broadway Rector 4100
Worthington Whitehouse, Inc.,
446 Madison Av. Plaza 4600.

(b) Green Book

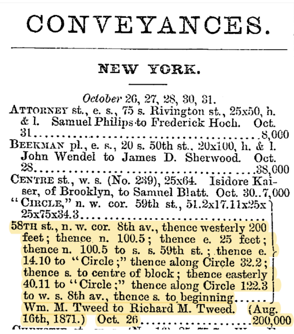

THE CITY OF NEW YORK 191

Second District—264 Madison St. Orchard 4300.

Term Expires
Lester Lazarus, 265 7th St. (Dem.)........Dec. 31, 1931
Abraham Harawitz, 26 Delancey St. (Dem.).Dec. 31, 1931
Joseph Raimo, 52 Spring St. (Dem.)........Dec. 31, 1937
Harold L. Kunstler, 149 Rivington St. (Dem.).Dec. 31, 1937
Morris Eder, 156 2d Ave. (Rep-Dem.).......Dec. 31, 1939
Patrick J. Paul, Clerk

Third District—314 W. 54th St. Columbus 1773.
Benedict D. Dineen, 440 W. 34th St. (Dem.)..Dec. 31, 1937
Thomas E. Murray, 347 W. 55th St. (Dem.)..Dec. 31, 1939
Patrick H. Bird, Clerk

(c) Real Estate Record and Builders Guide


CONVEYANCES.

NEW YORK.

October 26, 27, 28, 30, 31.

ATTORNEY st., e. s., 75 s. Rivington st., 25x50, h. & l. Samuel Philips to Frederick Hoch. Oct. 31........8,000

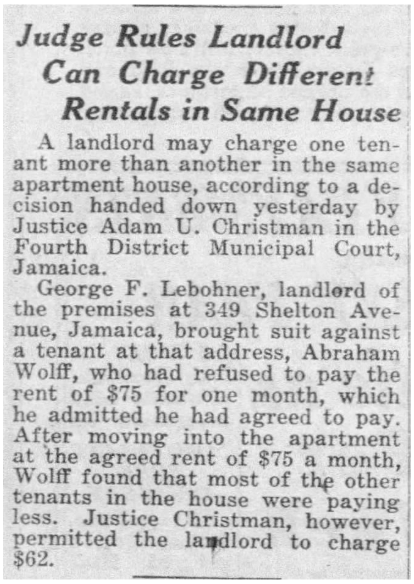
BEEKMAN pl., e. s., 20 s. 50th st., 20x100, h. & l. John Wendel to James D. Sherwood. Oct. 28........38,000

CENTRE st., w. s. (No. 239), 25x64. Isidore Kaiser, of Brooklyn, to Samuel Blatt. Oct. 30..7,000

"CIRCLE," n. w. cor. 59th st., 51.2x17.11x25x 25x75x34.3........

58TH st., n. w. cor. 8th av., thence westerly 200 feet; thence n. 100.5; thence e. 25 feet; thence n. 100.5 to s. s. 59th st.; thence e. 14.10 to "Circle;" thence along Circle 32.2; thence s. to centre of block; thence easterly 40.11 to "Circle;" thence along Circle 122.3 to w. s. 8th av., thence s. to beginning...... Wm. M. Tweed to Richard M. Tweed. (Aug. 16th, 1871.) Oct. 26........200,000

(d) Daily News


Judge Rules Landlord Can Charge Different Rentals in Same House

A landlord may charge one tenant more than another in the same apartment house, according to a decision handed down yesterday by Justice Adam U. Christman in the Fourth District Municipal Court, Jamaica.

George F. Lebohner, landlord of the premises at 349 Shelton Avenue, Jamaica, brought suit against a tenant at that address, Abraham Wolff, who had refused to pay the rent of $75 for one month, which he admitted he had agreed to pay. After moving into the apartment at the agreed rent of $75 a month, Wolff found that most of the other tenants in the house were paying less. Justice Christman, however, permitted the landlord to charge $62.

Note. Figure 2.1 shows example of the main data sources used in the paper. Panel 2.1a shows a snapshot of the real estate section of the New York Times; Panel 2.1b displays the Green Book; Panel 2.1c shows the Real Estate Record and Builders' Guide; and Panel 2.1d shows an example of a landlord tenant case from the Daily News.

Source. New York Times; Citywide Administrative Services (1918); Real Estate Record and Builders' Guide; Green Book; Daily News.

articles, spanning from 1918 to 1926, cover landlord-tenant cases. Our sample includes 42 judges (23 Democrats and 19 Republicans). Articles were sourced from newspapers.com using search terms such as the judge's full name (e.g., "William E. Morris") or variations like "Judge Morris" and "Justice Morris." We focus on two types of articles: those describing landlord-tenant cases concerning rent issues and those involving eviction demands. However, this data set has limitations.

Firstly, we observed only 26 of the 53 judges from 1920 to 1924 in rent cases and 23 of the 58 judges from 1920 to 1926 in eviction cases. The frequency of appearances varied significantly, with some judges appearing once and others up to eight times. Consequently, the representativeness of judges' decisions is uneven. Additionally, there is potential bias due to newspaper reporting, which may favor more prominent cases or judges who seek public attention. Therefore, while indicative, these findings should be interpreted with caution. The complete list of newspapers used and the classification of judges can be found in Appendix 2.B Table 2.B.1.

2.3.2 How different were judges?

How have judges differed in their verdicts on rent cases? Our primary argument is that a judge's party affiliation correlates with their sentencing behavior. Historically, the Republican Party was aligned with big business interests (Link, 1959) and typically opposed legislation aimed at redistributing wealth or assisting the laboring classes (Nelson, 2001). This suggests that Republican judges would be inclined to rule in favor of landlords. Conversely, the Democratic Party, split between a progressive urban electorate and a conservative rural southern base (Link, 1959), suggests that Democratic judges would be more likely to rule in favor of tenants.

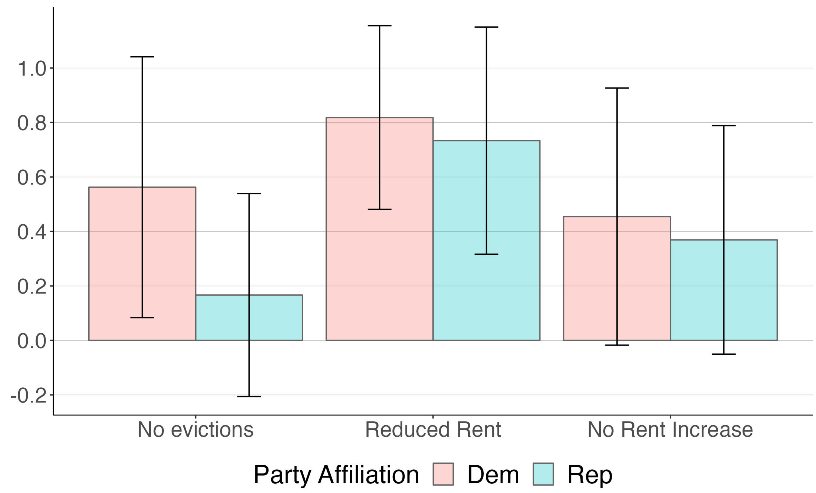
Judges may also have been incentivized to take sides in their rulings for various reasons. As public figures, judges' appearances and opinions were often covered by newspapers at trade unions, dinners, and festivals. Given that judges were elected in partisan elections, they could mobilize voters by taking a stand on rent laws. However, judges might depart from strict party lines, especially in New York City, where Democrats were historically linked to the corrupt Tammany Hall, and Republicans, such as Fiorello La Guardia, promoted social welfare policies (Williams, 2014).

One challenge in exploring this argument is the lack of historical rent case records, making it difficult to test for judicial bias. To address this, we collected information on municipal court judges and landlord-tenant cases reported in local newspapers. These articles provided insights into judges' stances on rent laws. We classified the judges' decisions using three criteria, assigning a dummy variable equal to one if:

- The judge reduced the rent demanded by the landlord.
- The judge allowed any rent increase or none.
- The judge refused the landlord's eviction demand.

We then averaged these decisions for each judge and subsequently by party affiliation. The results are summarized in Figure Figure 2.2. For eviction cases, Republican judges granted a stay in 17% of cases, compared to 56% in Democratic districts. Regarding rental reductions, Republican judges reduced the rent demanded by landlords in 73% of cases, while Democratic judges did so in 81% of cases covered in the newspapers. Finally, Republican judges did not allow any rent increase in 40% of cases, compared to 46% for Democratic judges.

Figure 2.2: Judge decisions



*Note.*Figure 2.2 gives the average decisions made by judges from the Republican and Democratic parties. We first calculated the average decision for each judge based on three criteria: tenant evicted, rent reduced, and no increase in rent. Subsequently, we computed the average of these judge decisions within each party faction (Democrat or Republican). The vertical lines represent one standard deviation. Further details on the construction of the data set can be found in Section 2.3.1.

These findings indicate that Democratic judges tended to rule in favor of tenants. However, due to potential representativeness issues in our data, these results should be considered indicative at best. They do, nonetheless, support the general positions of the Republican and Democratic parties.

Empirically, this consideration is motivated by the literature on judges. First, the empirical literature on judges shows that the appointment system can influence judges' decision-making behavior. Both Gordon, 2007 and Lim, Snyder **and** Strömberg, 2015 find that elected judges impose longer sentences than appointed ones. Second, partisan judicial elections tend to mirror political election results. Lim **and** Snyder, 2015 finds evidence that electoral behavior is highly biased in partisan judicial elections. In partisan elections, the correlation between the Democratic vote share in political and judicial elections is above 0.9, while in nonpartisan elections, the correlation is well below 0.5.

2.4 Empirical Strategy

2.4.1 Conceptual framework

We propose a straightforward framework to analyze how rent control may have impacted the housing market in New York City. Let us assume that landlords aim to maximize their profit by setting a rent amount, denoted as r. In the absence of rent control, this rent would be determined through the market equilibrium, which we denote as r^*. For the purpose of this argument, let us assume that under the Rent Control laws of the 1920s, landlords would always seek the highest possible rent, given the likelihood that the controls would be enforced. Since there were multiple judges per municipal court district (MCD), a landlord could encounter a landlord judge with probability p and a tenant judge with probability $1 - p$. The controlled rent is lower than the market rent, meaning that $\bar{r} < r^*$. If a landlord demanded a higher rent than the controlled rent, the tenant could refuse to pay, and the landlord would file a case to evict the tenant. However, if the landlord lost the case, they would incur costs represented by c, which includes hold-up and solicitor costs.[7] Therefore, the payoffs for the landlord in choosing r can be expressed as follows:

$$\mathbb{E}(r) = \begin{cases} pr^* + (1-p)(\bar{r} - c) & \text{if } \bar{r} < r \\ \bar{r} & \text{if } \bar{r} = r \end{cases}$$

Consider three cases. Let us assume that the probability of facing a landlord judge is $p = 1$. In this case, the expected payoff of setting the rent to the market rent would be greater than the expected payoff of setting it to the average rent, $\mathbb{E}(r^*) > \mathbb{E}(\bar{r})$. Now, let us assume that the probability of facing a landlord judge is $p = 0$. In this case, the expected payoff of setting the rent to the average rent minus the cost would be less than the expected payoff of setting it to the average rent, $\mathbb{E}(\bar{r} - c) < \mathbb{E}(\bar{r})$. If the probability of facing a landlord judge is 0.5, the

[7] We do not restrict this cost to be just the cost of a solicitor. It could also include the forgone rents and deterioration and damage to the property in case of rent strikes.

landlord will only increase the rent if the market rent minus the cost exceeds the average rent, $r^* - c > \bar{r}$.

If the landlord is certain they will face a tenant judge, they will set the controlled rent expecting lower income. However, if they are certain they will face a landlord judge, they will set the market rent. If the probability of facing a landlord judge is between 0 and 1, the landlord's choice will depend on the actual payoffs and the cost of the lawsuit.

That the rent control mechanism was used frequently and provided a credible threat can be inferred from the number of Summary Proceedings Instituted in the City of New York compiled by the Stein Commission. For the whole city area, there were 118,240 summary proceedings in 1920, increasing to 125,856 in 1921, which had to be handled by about 50 judges (New York (State)., 1921).

2.4.2 Regression discontinuity

A key contribution of this paper is identifying the causal impact of rent control. The main challenge with comparing outcomes within municipal court districts (MCD) by whether a landlord or tenant judge was elected is that the assignment of judge type is not random; for example, the district electorate most likely to elect a landlord judge may also be those where the share of landlords is high, or the housing stock is constraint due to symbiosis of owners. Such unobserved factors could lead to high rents and an elected landlord judge; therefore, estimates from standard regression analysis may be biased.[8]

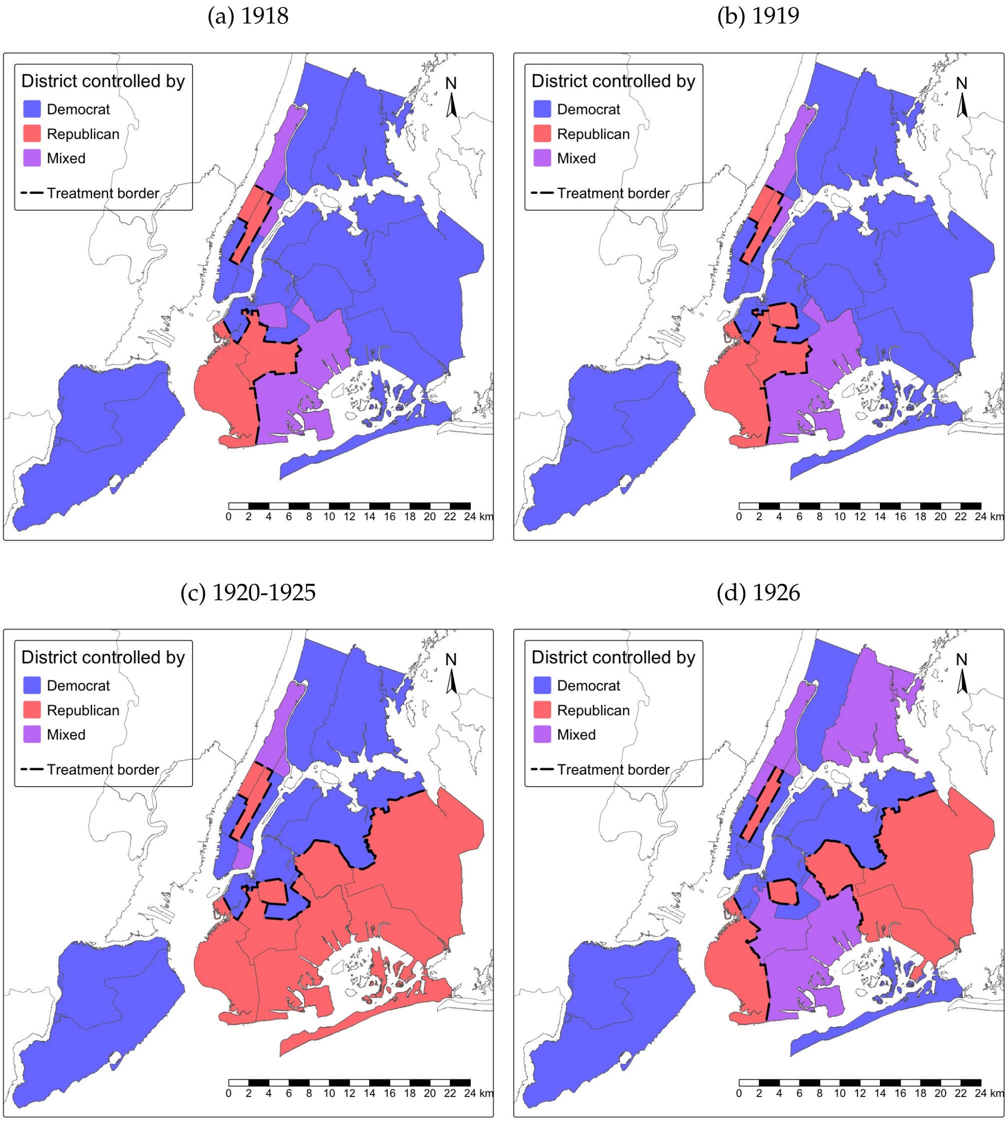
Our primary strategy exploits the binding nature of court boundaries. Because courts handle cases within the same district, verdicts by landlord judges will cause rents to be higher just up to the border of a tenant judge. To measure the judge's leniency, we exploit variation in the political alignment of judges. As argued in Section 2.3.2, we assume Democrat judges will judge in favor of tenants and Republican judges in favor of landlords.

We follow the reasoning in Section 2.4.1 and consider only districts where only Republican or Democrat judges are elected. For each year, we combine all Republican and Democrat-only court districts to exploit the distance to the nearest joint MCD boundary as displayed by Figure 2.3. Thus, for the primary analysis, we excluded districts with both Democrat and Republican judges. In Appendix 2.C.2, relax our empirical strategy by including those MCDs in the analysis, which

[8]We provide evidence that, on average, all Republican and all Democrat MCDs are similar on various neighborhood characteristics such as the shares of blacks, whites, owners, and second-generation immigrants as well as total population and income. Mixed districts differ on average significantly only in terms of the number of owners and total population. We plot these differences in Appendix 2.B Figure 2.B.1

had Republican and Democrat judges. For this exercise, we consider an MCD as treated if the share of republican judges was larger than 50%.

Figure 2.3: Treatment boundary



Note. Figure 2.3 shows the municipal court districts (MCD) in New York City. Each district had been colored according to the political affiliation of the elected MCD judges. All districts with only Republican judges are colored in red; all districts with only Democrat judges are colored in blue; districts with judges from both parties are colored purple. The dotted line gives our treatment boundary; in our baseline treatment, we consider the distance to Republican and Democrat-only MCDs; since elections alter the spatial distribution of judges, we plot the variation in treated and control MCDs in Panel (a) to (d); note that there nore changes from 1920 to 1925 in Panel (c)

We implement a Regression Discontinuity Design (RDD) where the forcing variable is the distance to a municipal court boundary. The forcing variable is positive within Republican districts and negative for Democrat districts; therefore, the cutoff is $c = 0$.

We estimate the following equation at the property level:

$$y_{i,m,t} = \theta \cdot 1(distance_i > 0)_{i,t} + f^a(distance_i) + \\ f^b(distance_i) \cdot 1(distance_i > 0)_{i,t} + \mathbf{X}_{i,t,m} + \gamma_t + \theta_m + u_{i,t} \quad (2.1)$$

$y_{i,m,t}$ is the variable of interest for property i in in neighborhood m in year t. $distance_i$ measures the distance from property i to the nearest MCD border. $distance_i$ is negative if the MCD is controlled by a Democrat judge and positive otherwise, excluding mixed districts. The two unknown functions f^a and f^b are assumed to be smooth in distance. Under the identification assumption that $u_{i,t}$ does not change discontinuously at distance 0, β_i provides an unbiased estimate of the effect on rents. Finally, **X** contains property level and geographic controls.[9]

We use a local non-parametric approach, with triangular kernel density function in the optimal bandwidth proposed by Imbens **and** Kalyanaraman (2012) as our baseline. We cluster standard errors at the neighborhood level to account for the correlation between nearby properties. We also present robust bias-corrected confidence intervals, correcting for the fact that confidence intervals are sensitive to bandwidth choice.

The identification assumption is that there is no change in the density of the running variable at the cutoff. First, we examine whether the density of the forcing variable, the distance to the MCD boundary, is continuous at the discontinuity. Figure Figure 2.4 and Figure 2.5 show histograms of the forcing variable for the entire range of rents and residential and commercial transactions in bins of 12.5 meters. Neither figure reveals any apparent sorting around the discontinuity, and the estimate from the McCrary test is small and statistically insignificant.

2.4.3 Event studies

In this section we study how the relationship between rent control and rents and transaction prices and how it may vary depending on the intensity of rent control. In line with the conceptual framework presented in Section 2.4.1, we empirically test whether the likelyhood of facing a landlord judge incentives landlords not to

[9] The controls vary by sample of rental ads and conveyances. For each rental ad, we observe the total number of rooms, whether the property was furnished, whether water and electricity were included and whether the property was a flat or a house. Since parks are being developed over the observation period, we include geographic controls such as distance to the coastal line and the nearest park each year. For each transaction, we observe the total square footage, the main construction materials, land use, whether the property was a loft, if it is located on the top floor or basement, and if it was a flat or a house. Here, we also include the distance to the coastal line and the nearest park.

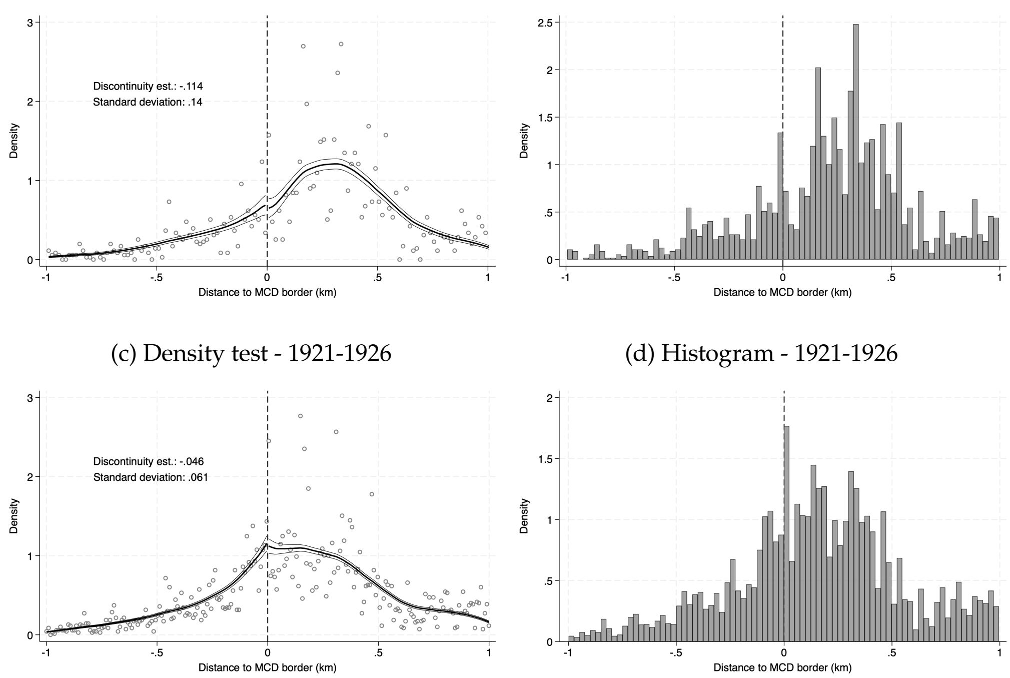
Figure 2.4: Continuity at cut off - rental dataset

(a) Density test - 1918-1920

(b) Histogram - 1918-1920

(c) Density test - 1921-1926

(d) Histogram - 1921-1926



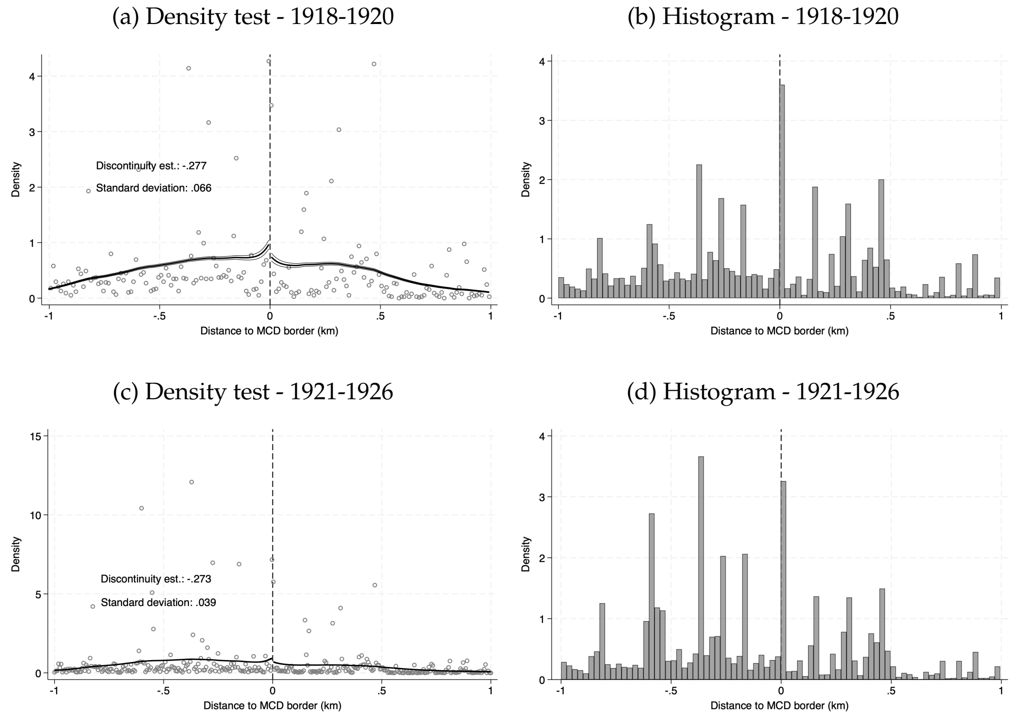
Note. Figure 2.4 presents results from testing if the continuity assumption at the threshold holds. We report tests for the period before and during rent control—panel (b) and (d) the distribution of the running variable. Bins are 12.5 meters in a 1km bandwidth around the cutoff at 0. Panels (a) and (c) show McCrary tests to see whether there is a discontinuity in the density of properties at the MCD boundary.

increase rents. Pertaining the institutional setting we propose two continuous treatments: (1) the share of Republican judges in a MCD and (2) the number of republican judges in year t in MCD u. The former would translate into the probability of encountering a landlord judge and the second in the marginal effect of an additional republican judge on rents. We further use the binary treatments from the RDD in order to check for consistency of results. Equation (2) gives our event study specification specification:

$$y_{i,m,t} = \sum_{\tau} \beta_\tau \cdot post_{1920} \cdot T_{t,u}(\tau = t - 1920) + \mathbf{X}_{i,t,m} + \gamma_t + \theta_m + u_{i,m,t} \qquad (2.2)$$

$y_{i,t,m}$ is the outcome for observation i in year t in court district m. The variable $T_{t,u}$ denotes treatment, for which we use the measures mentioned above. We compare the effects of our continous traetments to to the year of rent control implemention 1920. Property level controls are included in $\mathbf{X}_{i,t,m}$ and γ_t and θ_m are time and neighbourhoods fixed effects. The latter control for differences in unobserved differences across neighbourhood. We cluster standard errors at

Figure 2.5: Continuity at cut off - conveyances



Note. Figure 2.5 presents results from testing if the continuity assumption at the threshold holds. We report tests for the period before and during rent control—panel (b) and (d) the distribution of the running variable. Bins are 12.5 meters in a 1km bandwidth around the cutoff at 0. Panel (a) and (c) show McCrary tests of whether there is a discontinuity in the density of properties at the MCD boundary.

the neighborhood level. The idenfication assumption is that in absence of rent control, the intensity would not matter for rent prices, or, in other words, prices in MCDs with at least one Republican judge would have moved in parallel trends to Democrat only disticts.

2.5 Effect of rent control on rents and transaction prices

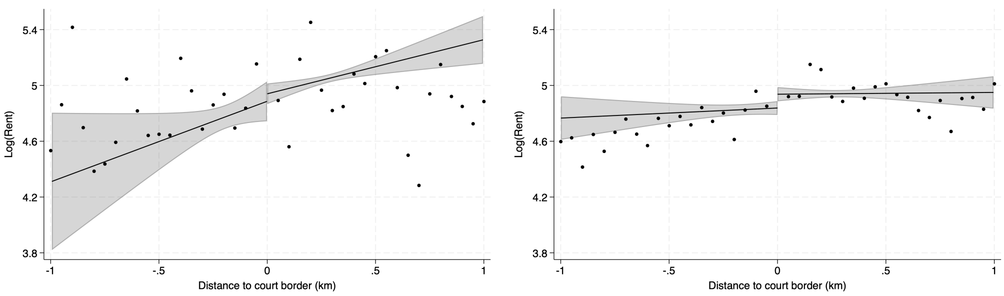
In this section, we first concentrate on market rents and present the results of estimating Equation 2.1. Next, we test wether rent control effected residential and commercial transaction prices.

2.5.1 Effect on ask rents

We begin by showing RD graphs of locally linear regressions in Figure 2.6. The Panel 2.6a shows a smooth relationship of rental prices at the cutoff before the intrduction of rent conrtol. In the rent control period from 1921 to 1926 rent prices jump discontinously at the border (Panel 2.6b).

Figure 2.6: Effect at cut off on real prices

(a) pre 1920 rep50 (b) 1920-1926 rep50



*Note.*Figure 2.5 shows the binned scatterplot relationship between rental prices and the RDD running variable (distance to nearest MCD border) using 12.5 meter bins; Panel (a) shows the relationship before the introdocution of rent control; Panel (b) shows the relationship during rent control; Democrat districts have negative distances and lie to the left of the zero line, while Republican districts have positive distances and lie to the right of the zero line. All regressions follow Equation 2.1; we used a bandwidth of 1km; the shaded area show 95% confidence intervals; standard errors have been clustered at the neighbohood level.

Even though these figures indicate a positive RD treatment effect of being in a Rpublican controlled court district, they still leave room for more refined analysis. For this purpose, Table 2.1 presents regression results from estimating Equation 2.1 for a subsample before the introduction of rent control. The optimal bandwidth, $\hat{b}$, calculated using the Imbens **and** Kalyanaraman (2012) algorithm. To check if effects vary by bandwidth choice we report estimates for double and half the optimal bandwidth of the specifcation including full controls. Results using a linear specifcation indicate no signicant jumps at the cutoff. Estimates range from 7.6% to -10%. Using a quadratic specifcation shows similar results. While there is variation in the cutoff estimates our preffered specification using the full set controls with the optimal bandwidth $\hat{b}$ reports a small negative but insignifance difference of 2.5% lower rents in Republican MCDs.

Next we estimate Equation 2.1 for a subsample during the rent control period from 1921 to 1926. We show these results in Table 2.2. All estimates using a linear fit signicant are significant and positive and similar in magnitude ranging from 12% without to 9.4% with controls across bandwidth choice. The quadratic fit identifies slightly larger jump of 11.2$ in our preffered specification, using full set of controls and the optimal bandwidth. It is notworty to highlight that in Table 2.1 rent prices are estimates to be 19% lower using half the optimal bandwidth while in Table 2.2 the same specifation exhibits a 8.7% jump. While both results are insiginfcant they additionally support the hypothesis that rent control increased

Table 2.1: Effect at cut-off on ask rents - 1918-1920

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.044	0.076	-0.103	0.058	-0.006	-0.025	-0.188	0.022
	0.109	0.089	0.143	0.067	0.18	0.145	0.221	0.101
Controls		✓	✓	✓		✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.556	0.542	0.271	1.085	0.779	0.682	0.341	1.364
Obs.	2728.000	2586.000	2586.000	2586.000	2728.000	2586.000	2586.000	2586.000
R2	0.189	0.431	0.495	0.408	0.190	0.422	0.480	0.407
ci_l_rb	-0.229	-0.138	-0.533	-0.162	-0.416	-0.354	-0.496	-0.284
ci_r_rb	0.272	0.245	0.258	0.266	0.410	0.283	0.466	0.293

Note. Table 2.2 reports regression results for ask rents; the data had been subsetted for the pre rent control period 1918-1920; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

rent prices in Republican controlled districts relative to Democart controlled MCDs. Moreover, none of these results is rejected using robust bias corrected confidence intervals.

Since our sample of rental observation is biased towards Manhattan we report estimates for replicating the above analysis for Manhattan only. We report estimates from this exerise in Appendix 2.C.1. Table 2.C.1 and Table 2.C.2 display results from estimating Equation 2.1 for Manhattan only. Before the introduction of rent control we find variation in cutoff estimates ranging from -23% to 6.7% across linear specifications and similarly for the quadratic fit. However of these coefficients is significant. For the rent control preiod we find positive estimates ranging from 4% to 10% increaes in the linear specification. However, our only significant estimate of 19.1% increase is confirmed by the quadratic for $\hat{b}/2$ (Table 2.C.2).

Next we test if the effect varies incuding the mixed districts. We consider a MCD as Republican controlled if the share of Republican judges is larger 50%. We estimate Equation 2.1 using the same set-up as above. We report results in Appendix 2.C.2 in Table 2.C.7 and Table 2.C.8. Similarly to the results discussed above, we do not find evidence for significant differences at the border before the introduction of rent control. However, during rent control there is mixed evidence for jumps in prices. We find consistent evidence for price increases

Table 2.2: Effect at cut-off on ask rents - 1921-1926

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.120**	0.097**	0.097*	0.094***	0.137**	0.112*	0.087	0.107**
	0.038	0.034	0.040	0.025	0.051	0.047	0.056	0.035
Controls		✓	✓	✓		✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.817	0.889	0.445	1.779	1.138	1.087	0.543	2.174
Obs.	8481	8169	8169	8169	8481	8169	8169	8169
R2	0.107	0.278	0.280	0.271	0.108	0.277	0.281	0.271
ci_l_rb	0.039	0.020	-0.052	0.027	0.035	0.020	-0.090	0.014
ci_r_rb	0.206	0.164	0.208	0.177	0.257	0.220	0.260	0.200

Note. Table 2.2 reports regression results for ask rents; the data had been subsetted for the rent control period 1921-1926; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

using $\hat{b}*2$ using both linear and quadratic fit. Nevertheless, smaller bandwidth choices render the effect insignificant and even close the zero line using $\hat{b}/2$.

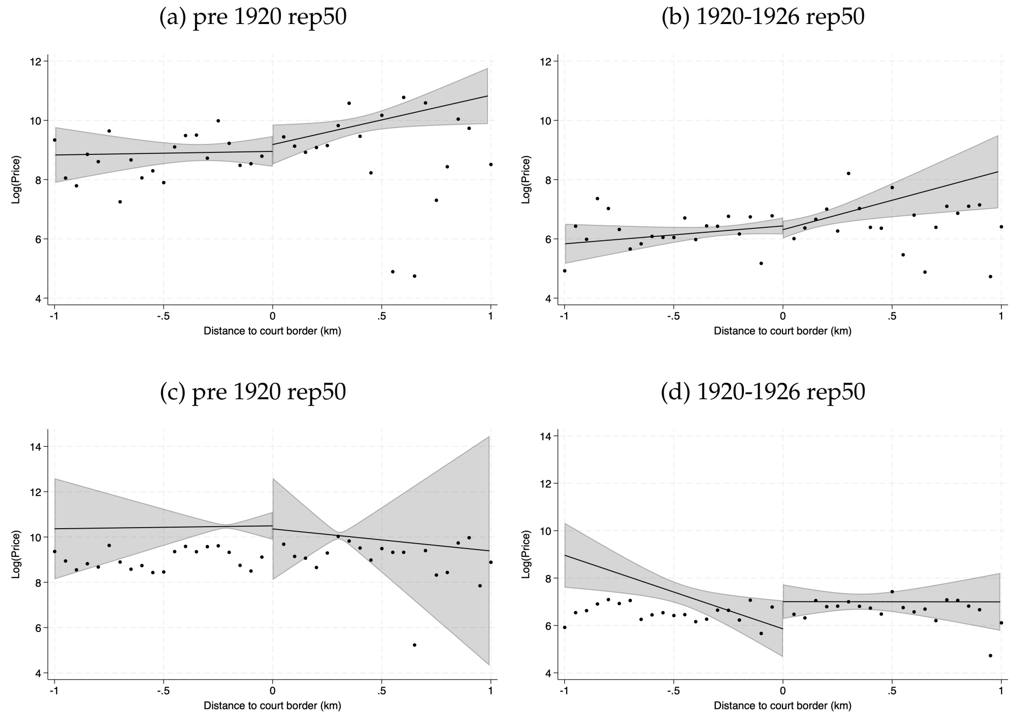
Finally, we test for sensitivity of outcomes to different RD parameter choices. Appendix 2.C.3 shows that treatment effects are highly stable in magnitude across bandwidths choices before and during rent control (Figure 2.C.1). For each bandwidth choice rent prices after the introduction of rent control are higher by the same factor. Panel 2.C.1c and 2.C.1d in particular show that estimates become significant a bandwidth larger than 300 maters.

2.5.2 Effect on residential & commercial transaction prices

In this section we test if rent control affected transaction prices. We persue the same strategy as we did for rental price. Graphic results are reported in Figure 2.7. By visual inspection, there is no evidence for a jump of residential transaction prices before and during rent control. Moreover, there is no clear evidence for a jump in commercial prices at the boundary. Though one should note the low number of observations in the sample which boils down to 206 before the introduction of rent control and 662 during rent control which reduces our power.

We investigate these effects further for resitdential transaction prices in

Figure 2.7: Effect at cut off on real prices



*Note.*Figure 2.5 shows the binned scatterplot relationship between transaction prices and the RDD running variable (distance to nearest MCD border) using 12.5 meter bins; Panel (a) and (b) show the relationship using residential transaction prices before and during rent control; Panel (c) and (d) show the relationship using commercial transaction prices before and during rent control; Democrat districts have negative distances and lie to the left of the zero line, while Republican districts have positive distances and lie to the right of the zero line. All regressions follow Equation 2.1; we used a bandwidth of 1km; the shaded area show 95% confidence intervals; standard errors have been clustered at the neighbohood level.

Table 2.3 and Table 2.4. While we do not find any significant effects, we observe a reversal of signs. Before rent control using bandwidth $\hat{b}$ prices are 9% higher in Republican districts at the border. Results for the rent control period indicate a reversal of signs. Across all bandwidth specification prices are lower at the boundary and often negative. These results are confirmed by estimates for Manhattan only (Table 2.C.3 and Table 2.C.4), alterntive treatment boundary using the distance to majority Republican MCDs (Table 2.C.9 and Table 2.C.10). In particular using alternative bandwidths over the interval from 100 meters to 1km (Figure 2.C.2) shows that for all bandwith choices above 300 meters RD estimates during the rent control preiod are close to the zero line and stable.

Compraing estimates for commercial trasnaction prices reveals a similar picture. There are large positive differences at the border in Republican MCD during rent control as reported in Table 2.6. We observe a significance difference in log points from 1 to 2.149 across linear specifications. These effects are large

Table 2.3: Effect at cut-off on residential prices - 1918-1920

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.272	0.091	0.121	0.199	0.231	0.038	0.221	0.219
	0.401	0.338	0.400	0.238	0.419	0.335	0.481	0.252
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.860	0.794	0.397	1.588	1.731	1.421	0.876	3.503
Obs.	1177	1078	1078	1078	1177	1314	1078	1078
R2	0.122	0.162	0.210	0.168	0.127	0.168	0.166	0.166
ci_l_rb	-0.616	-0.661	-0.709	-0.668	-0.673	-0.672	-0.901	-0.596
ci_r_rb	1.082	0.778	1.272	0.813	1.156	0.778	1.361	0.724

Note. Table 2.3 reports regression results for residential transaction prices; the data had been subsetted for the pre rent control period 1918-1920; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 give RD estimates using a linear specification; in column (1)-(2), the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

and we beleive them to be unrlieable. Two aspects needs to be taken into account. First, haveing established signicant and large differences at the border before rent control suggest existing fundamental differences in commercial properties on either of the border (see Table 2.5). Second, we only observe few commercial transactions to either side of the border. Using alternative bandwidths reveals that choices of below 300 meters before and 200 meters during rent control yield empty results due to the lack of observations (see Figure 2.C.3). Additionally, the we observe a large variance in transaction prices (Table 2.5). Thus, while it is plausible to argue that rent control was not affecting commercial transaction prices given the policy's design and the results above, power issues do not allow a final conclusion.

Table 2.4: Effect at cut-off on residential prices - 1921-1926

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.233	-0.131	-0.220	0.065	-0.308	-0.190	-0.204	0.030
	0.244	0.245	0.294	0.186	0.263	0.276	0.339	0.200
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.596	0.696	0.348	1.393	1.468	1.494	0.747	2.988
Obs.	4286	3770	3770	3770	4286	3770	3770	3770
R2	0.214	0.231	0.235	0.214	0.205	0.214	0.230	0.203
ci_l_rb	-0.845	-0.737	-1.071	-0.739	-0.930	-0.818	-1.139	-0.714
ci_r_rb	0.222	0.331	0.204	0.361	0.213	0.365	0.329	0.337

Note. Table 2.4 reports regression results for residential transaction prices; the data had been subsetted for the rent control period 1921-1926; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 give RD estimates using a linear specification; in column (1)-(2), the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.5: Effect at cut-off on commercial prices - 1918-1920

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.479	0.899	0.827***	0.494	-1.675	-0.654	-1.753***	0.726
	0.643	1.065	0.210	0.866	0.936	1.089	0.283	1.402
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.509	0.677	0.338	1.354	0.720	0.743	0.389	1.554
Obs.	206.000	169.000	169.000	169.000	206.000	199.000	169.000	169.000
R2	0.460	0.463	0.737	0.291	0.357	0.473	0.718	0.286
ci_l_rb	-2.094	-1.613	0.023	-2.139	-3.954	-3.028	-2.213	-2.768
ci_r_rb	0.467	3.136	1.202	3.278	-0.958	0.142	-1.416	3.261

Note. Table 2.5 reports regression results for commercial transaction prices; the data had been subsetted for the pre rent control period 1918-1920; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 give RD estimates using a linear specification; in column (1)-(2), the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.6: Effect at cut-off on commercial prices - 1921-1926

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	1.032***	1.423***	2.149***	1.084***	1.291***	2.023***	1.656**	1.522***
	0.288	0.348	0.400	0.266	0.358	0.535	0.582	0.346
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.696	0.869	0.435	1.738	1.328	1.158	0.579	2.317
Obs.	662.000	513.000	513.000	513.000	662.000	513.000	513.000	513.000
R2	0.332	0.401	0.475	0.356	0.286	0.384	0.433	0.339
ci_l_rb	0.658	0.705	0.878	0.507	0.643	0.986	0.065	0.746
ci_r_rb	1.791	2.274	2.750	2.220	2.097	3.311	2.481	2.661

Note. Table 2.6 reports regression results for commercial transaction prices; the data had been subsetted for the rent control period 1921-1926; the running variable is the distance from census block centroid to the treatment boundary as shown in Figure 2.3. Columns 1–4 give RD estimates using a linear specification; in column (1)-(2), the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

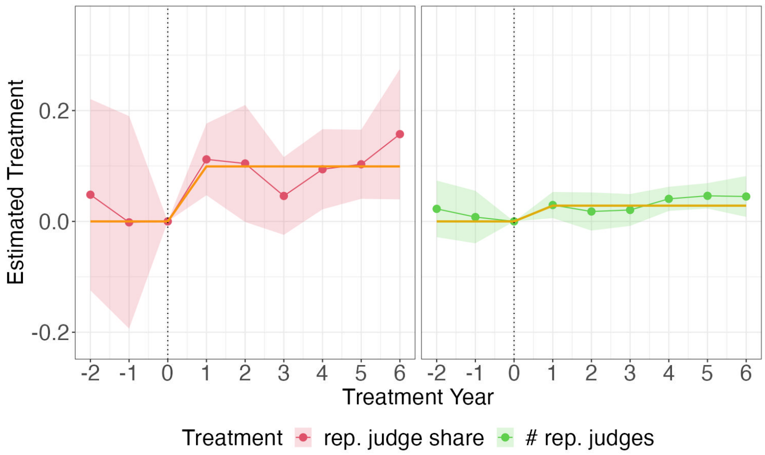
2.6 Event study results

In this section we report effect using two measures for treatment intensity. Results from estimating Equation 2.2 for our rent data are shown in Figure 2.8. We find again a convincing effect of rent control on rental prices. The difference in rental prices between Municipal Court Districts (MCD) that are controlled by 0% and 100% by a Republican averages at 10%, which close the the resulst we report in Table 2.2. An additional republicam judge increases rental prices by about 3%. Given on average 2 Republican judges by MCD this would average to 6% higher in a typical mixed district. Panel (b) and (c) report results for residential and commercial transaction prices. Both prices behave similarly compared to the RD estimates as reported in Section 2.5.2. We find a difference in transaction prices between MCDs that are controlled by 0% and 100% by a Republican of -24% and 28% for residential and commercial properties respectively. As argued in Section 2.5.2, there is effect large temproal variation in prices. Residential prices exihibt substantial pre control variation, violating the parallel trends assumption. Similarly the effects for commercial prices exhibit large confidence intervals. Both result from large variation in prices across the sample. Moreover, while the RD estimates were yielding signifcant effects on transaction prices, we can confirm that these effects are not systematic and not driven by the 1920 rent control laws.

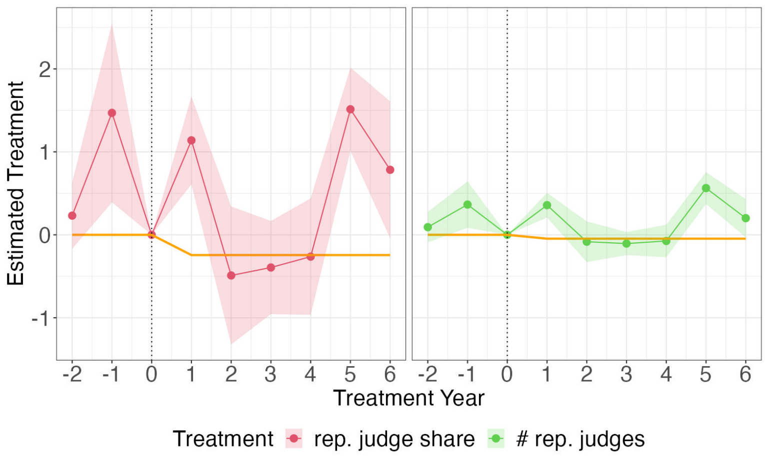
These resulst are confirmed by using the binary treatments from the the RD design in Appendix 2.C.4. There is no evidence for pretends in rent prices using the either the Republican only vs. Democrat only treatment or majority Republican districts (Figure 2.C.4). Point estinate average at 10.7% and 8.8% for either treament. Results for transaction prices are reported in Panel 2.C.4b and Panel 2.C.4c. There are no signifcant and systematic effects for transaction prices.

Figure 2.8: Effect of continous treatments

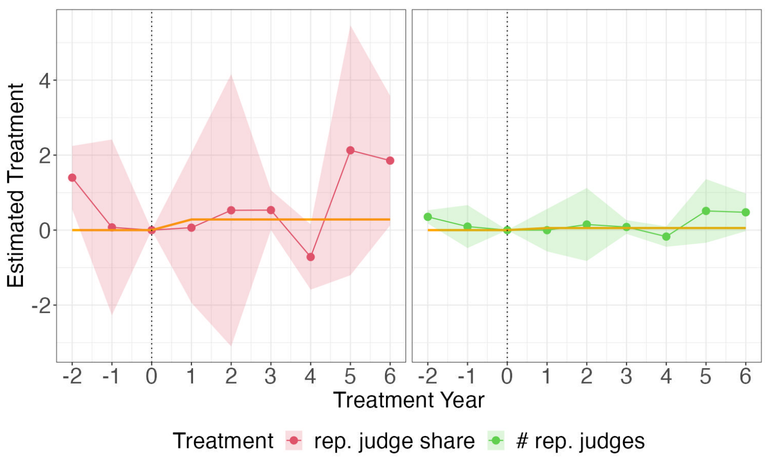
(a) Effect on rent prices



(b) Effect on residential prices



(c) Effect on commercial prices



Note. Figure 2.8 reports point estimates for β_τ in Equation 2.2 using the full et of property level controls, year and neighborhood foxed effects; year dummies have interacted with (1) the share of Republican judges in MCD u or (2) the number of Republican judges in MCD u; standard errors are clustered at the neighborhood (NTA) level; the shaded area show the estimated 95% confidence bands; the orange line plots the aggregated average from simple interaction between treatment $T_{t,u}$ and an indicator variable $\mathbb{1}(t > 1920)$. Panel 2.8a reports differences for ask rents tracts, Panel 2.8b differences in residential transaction prices and Panel 2.8c differen ces in commercial transaction prices.

2.7 Conclusion

While rent control has been one of the most studied policies in economics, only recent studies have empirically investigated its causal mechanisms. This paper investigates the effects of the first rent control laws in the United States, passed in 1920 in New York City. Compared to previous policy decisions, the 1920s laws empowered judges to decide on a case-by-case basis over rent increases.

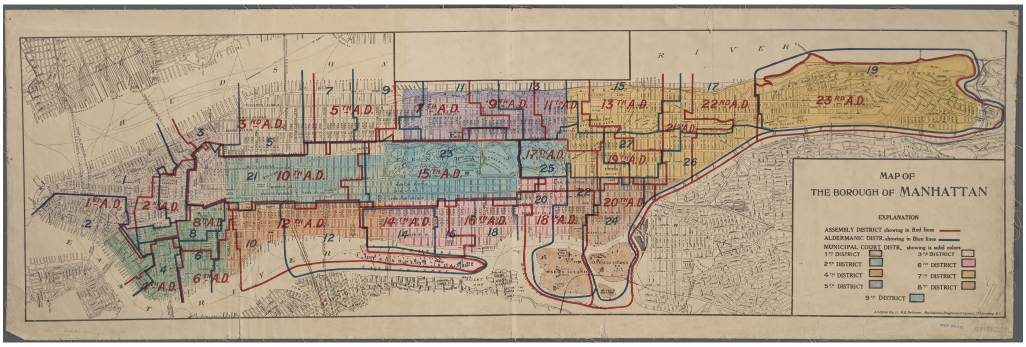
Overall, we find evidence across a variety of tests that the 1920 rent control laws were affecting market rents through judge rulings, at least indirectly. We establish that Republican judges were more lenient towards landlords than Democrat judges. While we cannot establish a direct link between court rulings and rents, we exploit the binding nature of court boundaries. Using a RD design, we find a jump in rents at the border between Republican and Democrat judges of 10%. These results are confirmed using an event study design. We propose a mechanism according to which landlords anticipate the costs of lawsuits since they know the partisanship of a judge. Therefore, landlords align with the policy if there is a probability of having a tenant judge. However, we cannot confirm a similar effect on transaction prices. Neither commercial nor residential transaction prices respond to judges. This result is surprising, at least for residential prices, since rents reflect the landlord's income from residential property.

While the effect on rents confirms the proposed rent control mechanism, the lack of response of residential transaction prices could be due to the short-term and provisional characteristics of rent control. The control had to be renewed every two years by the legislature in Albany, and landlords could expect rent controls to be abolished on a rolling basis. Moreover, given that judges could be elected even within the system, variation could lead to an adjustment of landlords' price expectations regarding prices.

Future research might investigate these channels in greater detail. Since we did not find supporting evidence that rent control shifted transaction prices, the link remains underexplored and might be overcome with better data. Furthermore, future research could explore the quantity response of the 1920s rent control. For example, does rent control shift the market strong enough for developers to invest more in the other building types exempted from control? This could be the case if, even if exempted from control, developers expect new buildings to get control shortly. Moreover, while rising rents were not possible in controlled districts, landlords could demolish their properties and increase capital intensity by constructing taller buildings or reducing apartment sizes to increase incomes.

Appendix 2.A Supplementary Maps

Figure 2.A.1: Historical Municipal District Courts - Manhattan



Note. Figure 2.A.1 shows the Borough of Manhattan, the Assembly, Aldermanic, and Municipal Court Districts in 1918.
Source. Lionel Pincus and Princess Firyal Map Division, The New York Public Library (1918). Map of the Borough of Manhattan, showing the Assembly, Aldermanic, and Municipal Court Districts.

Figure 2.A.2: Spatial distribution of rental properties

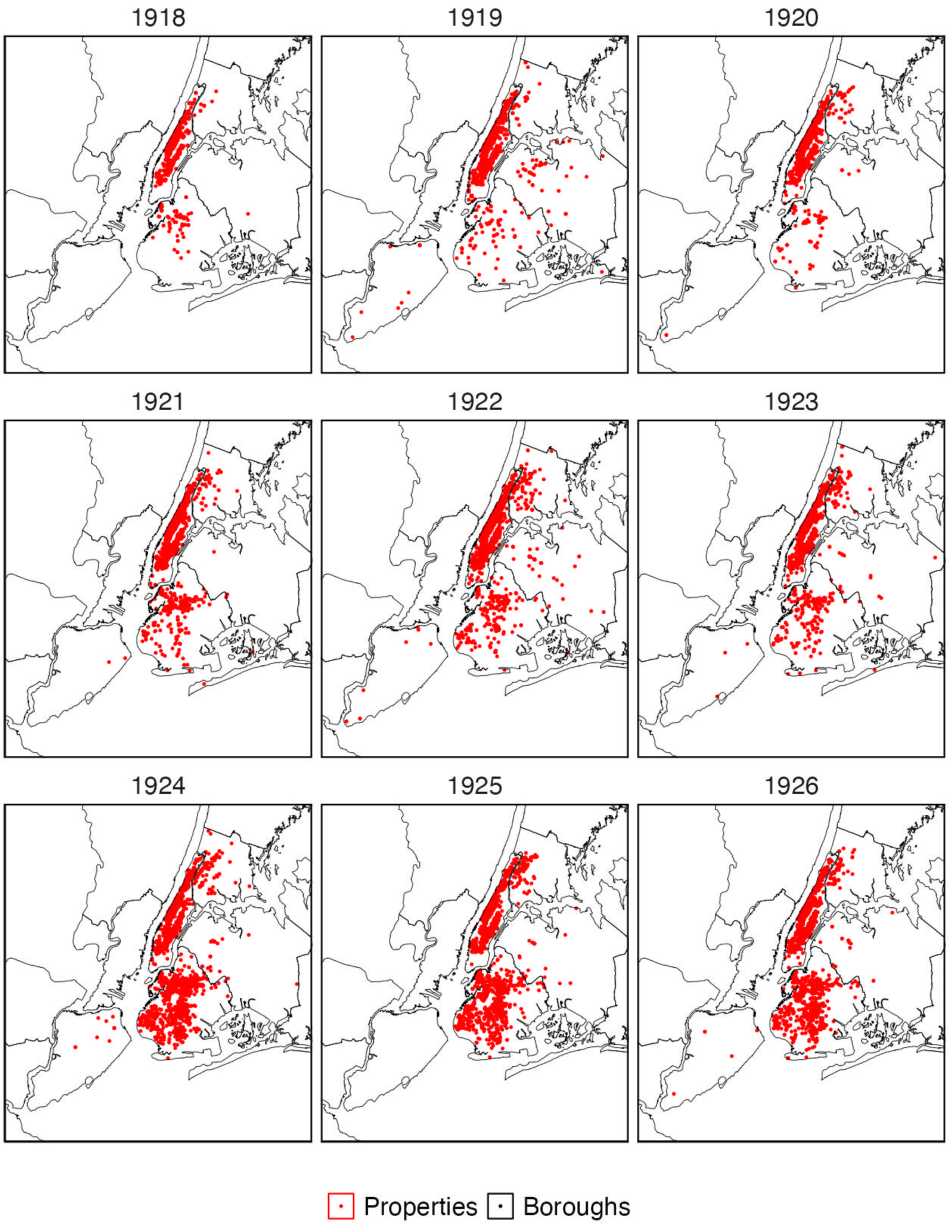


Figure 2.A.3: Spatial distribution of conveyances

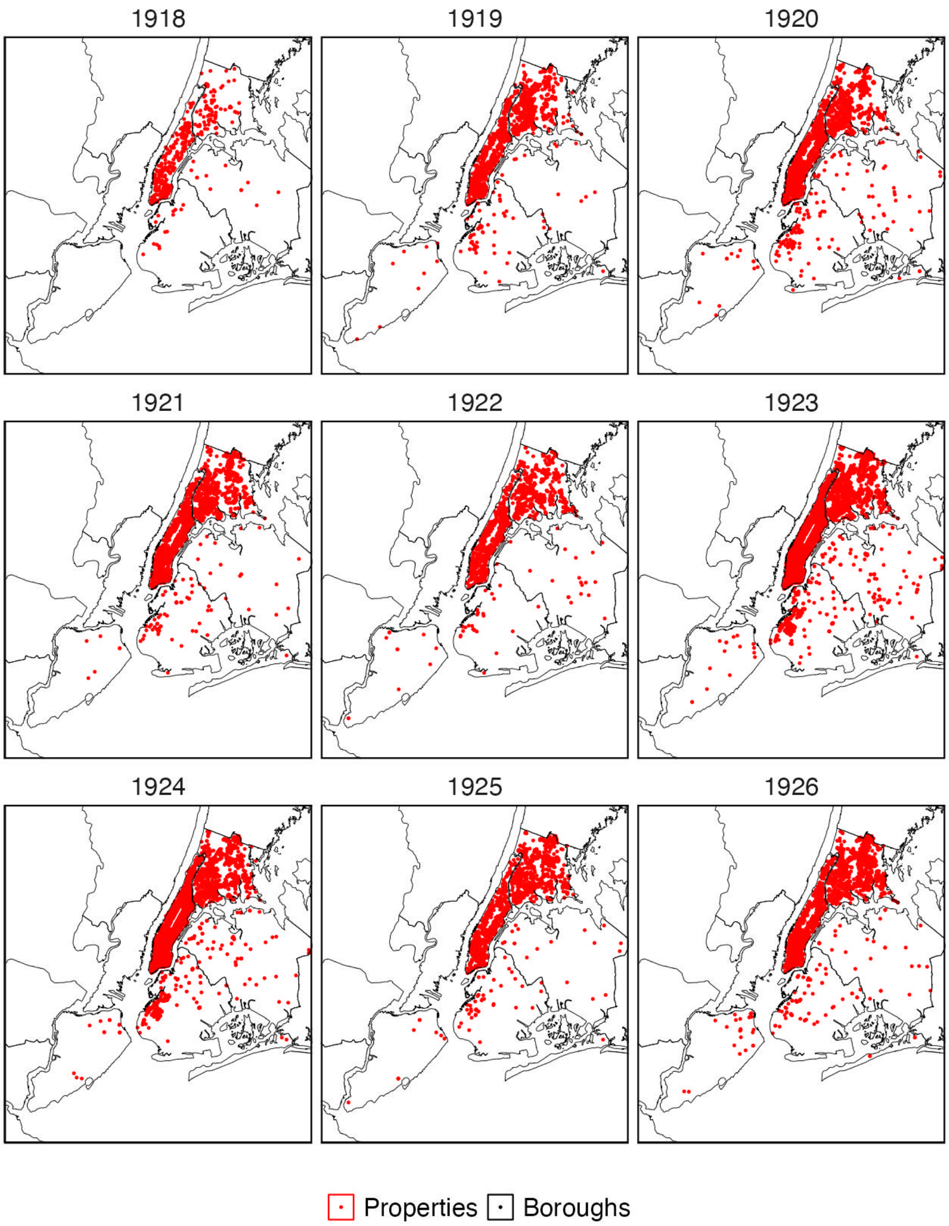
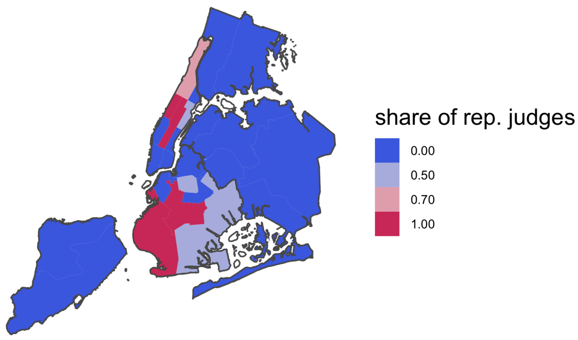
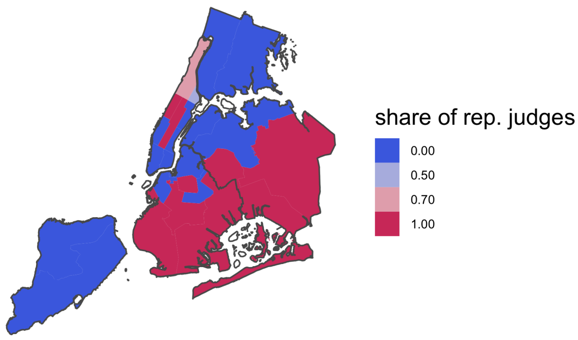


Figure 2.A.4: Share of Republican judge

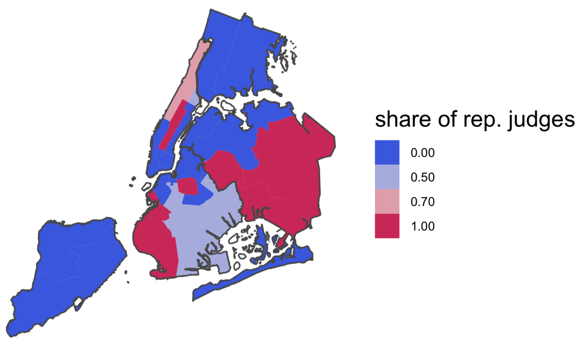
(a) 1918



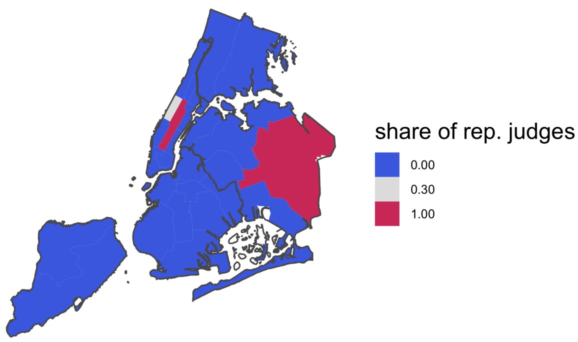
(b) 1922



(c) 1927



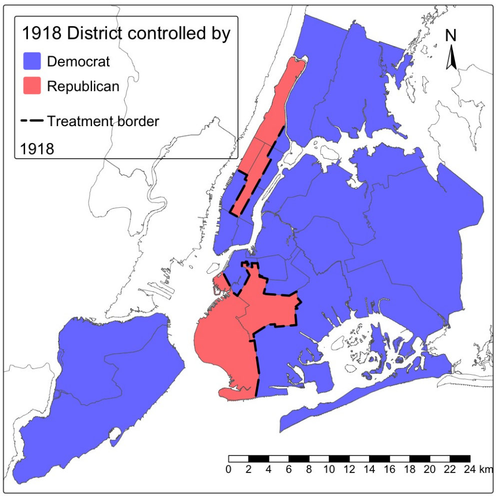
(d) 1931



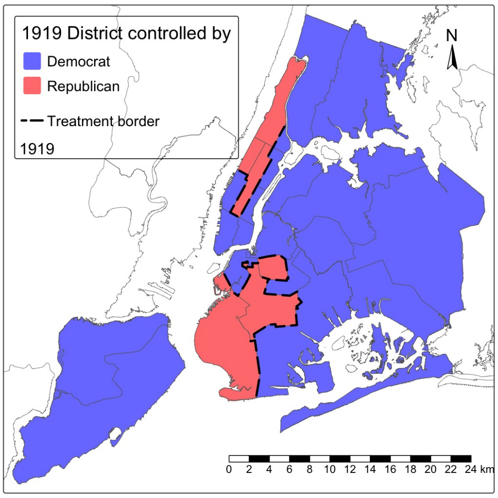
Note. Figure 2.A.4 shows the municipal court districts (MCD) in New York City. Each district had been colored according to the share of Republican judges elected at each point in time; we plot the variation in judge shares in MCDs in Panel (a) to (d); note that there nore changes from 1920 to 1925 in Panel.

Figure 2.A.5: Alternative treatment boundary

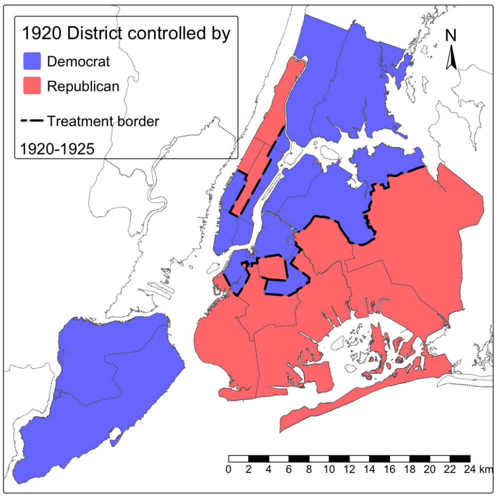
(a) 1918



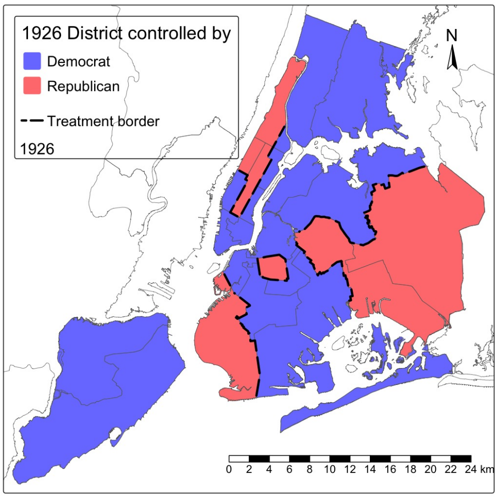
(b) 1919



(c) 1920-1925



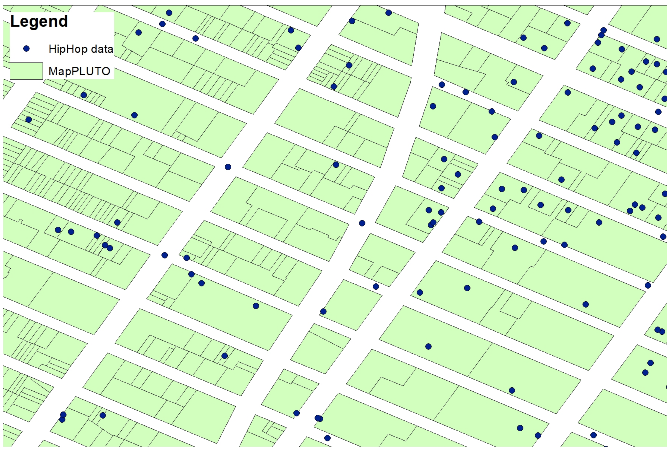
(d) 1926



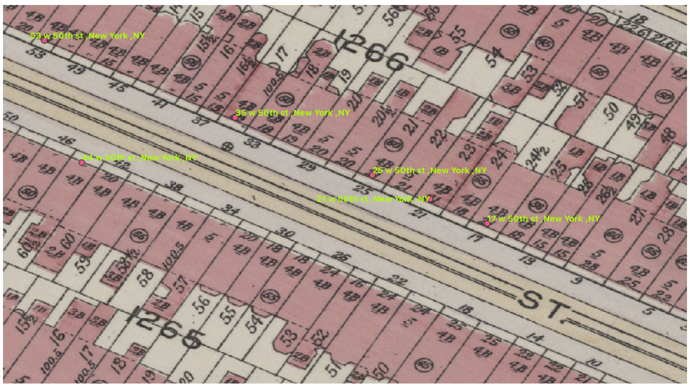
Note. Figure 2.A.5 shows the municipal court districts (MCD) in New York City. Each district had been colored according to the political affiliation of the elected MCD judges. A district is considered as Republican controlled if the share of Republican judges within the MCD is larger than 50%; therfore there are no mixed colored districts. The dotted line gives our treatment boundary; in our baseline treatment, we consider the distance to majority Republican and majority Democrat MCDs; since elections alter the spatial distribution of judges, we plot the variation in treated and control MCDs in Panel (a) to (d); note that there nore changes from 1920 to 1925 in Panel (c).

Figure 2.A.6: Example of manual geocoding

(a) PLoto 2002 lot files

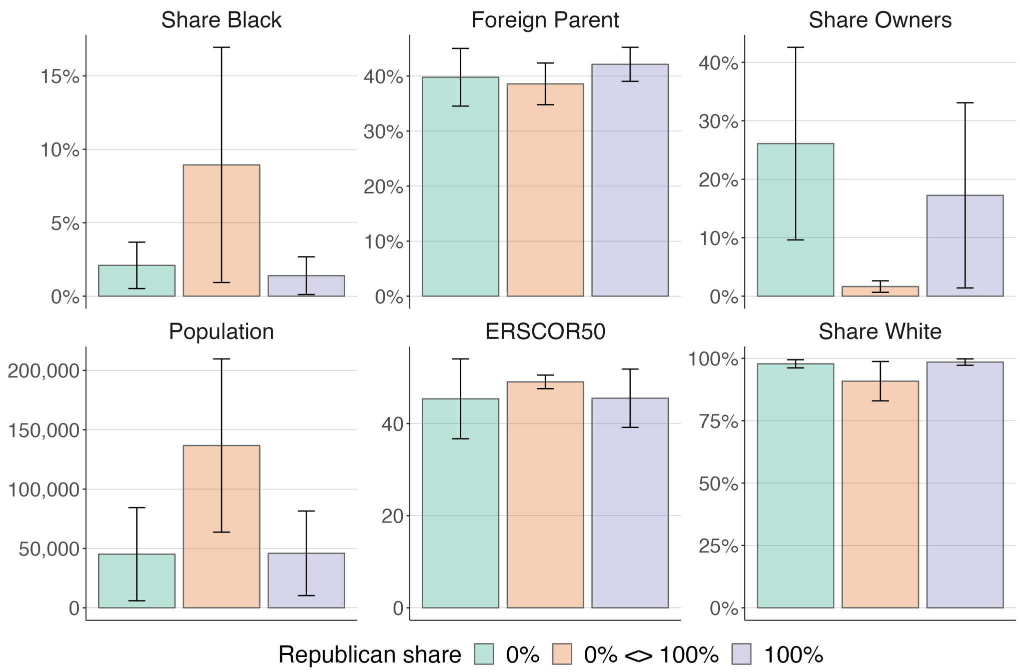


(b) Bromley fire insurance maps



Appendix 2.B Descriptive statistics

Figure 2.B.1: Differences across MCDs



Note. The figure shows census aggregates for MCDs by share of Republican judges. Individual-level data from the 1920 decennial census were aggregated on the enumeration district level. Next, we aggregate NTA aggregates using overlapping area weights. An NTA was counted to a MCD if more than 50% of its area was within the MCD; MCDs were collapsed in three groups; no republican judges, Republican-only and mixed. The bars show the average for the shares of second-generation immigrants, blacks, whites, and owners, income, and population by the share of republican judges. The vertical lines represent one standard deviation.
Source. Author's own calculations; US federal census.

Table 2.B.1: Judge Coding

Name	Newspaper	Year	Month	Day	Reduction of rent	No increase	Tenant not evicted
0. Grant Esterbrook	New York Tribune	1920	Jul	24	0	0	
Aaron J. Levy	Daily News	1922	Jun	21			1
Abram Ellenbogen	The Evening World	1920	Jan	14			0
Abram Ellenbogen	New York Times	1920	April	21			0
Adam Christmann, Jr.	Daily News	1921	Nov	12	1	0	
Benjamin Hoffman	New York Times	1920	Apr	13	1	1	0
Benjamin Hoffman	The Sun	1920	Apr	13	1	1	0
Charles B. Law	The Evening World	1921	Sat	8	1	1	
Charles J. Carroll	Daily News	1926	Sep	29			0
Edgar F. Hazelton	The Brooklyn Daily Eagle	1920	Oct	29	1	1	
Edgar F. Hazelton	The Brooklyn Daily Eagle	1920	Oct	29	0	0	
Edgar F. Hazelton	The Brooklyn Daily Eagle	1921	Aug	24			1
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar J. Lauer	New York Herald	1921	May	13	0	0	0
Edgar M. Doughty	The Brooklyn Daily Eagle	1921	Jun	22	1	1	
Edgar M. Doughty	Standard Union	1922	Apr	16			1
Edgar M. Doughty	Standard Union	1923	Aug	20	1	0	
Frank J. Coleman, Jr.	New York Herald	1921	Jan	18	1	1	
George L. Genung	The Evening World	1921	Feb	4	1	1	
George L. Genung	New York Times	1921	Oct	22	0	0	
Harrison C. Glore	Standard Union	1921	May	13			0
Harry Robitzek	New York Herald	1922	Jan	26			0
Harry Robitzek	The Evening World	1922	Mar	14	1	0	
Harry Robitzek	Daily News	1920	Apr	9	0	0	
Harry Robitzek	New York Times	1920	Apr	29	0	0	0
Harry Robitzek	New York Times	1923	Jan	24	1	0	
Jacob Marks	Evening World	1921	Apr	28			
Jacob Marks	New York Times	1922	Apr	16			1
Jacob Panken	New York Tribune	1920	May	7			1
Jacob Panken	New York Herald	1922	Nov	24			1
Jacob S. Strahl	New York Times	1920	Jan	1			1
Jacob S. Strahl	New York Times	1920	Jan	1			1
Jacob S. Strahl	The Evening World	1920	Sep	20	1	1	
Jacob S. Strahl	New York Herald	1922	May	9			1
James A. Dunne	Standard Union	1922	Jan	4			1
James A. Dunne	New York Herald	1921	May	3			1
James A. Dunne	Standard Union	1921	Dec	18	0	0	
James A. Dunne	The Evening World	1922	Jan	14	1	0	
John G. McTigue	Daily News	1921	Sep	16	1	1	
John Hetherington	Brooklyn Times	1922	Jan	25			0
John Hetherington	New York Times	1922	Jul	2			1
John M. Cragen	Brooklyn Times	1921	Dec	11			0
John M. Cragen	Brooklyn Times	1922	Jan	25			1
John R. Davies	New York Tribune	1921	Nov	25	1	1	
John R. Davies	New York Times	1920	Apr	21	1	0	
John R. Farrar	The Brooklyn Daily Eagle	1922	Jun	22	1	1	
John R. Farrar	The Brooklyn Daily Eagle	1922	Jun	22	1	1	
Leopold Prince	New York Times	1920	Apr	29	1	0	
Leopold Prince	New York Times	1924	Jan	27	1	1	
Michael J. Scanlan	Evening World	1920	Sep	9	1	0	
Michael J. Scanlan	Daily News	1920	Sep	3	1	0	
Michael J. Scanlan	New York Tribune	1920	May	7	1	0	
Samson Friedlander	New York Herald	1921	Oct	27	1	0	
Samson Friedlander	New York Tribune	1920	May	7			0
Thos. E. Murray	New York Tribune	1920	May	8			0
Timothy A. Leary	New York Times	1922	Jun	20			1
William Blau	New York Tribune	1920	Aug	1	1	0	
William Blau	New York Tribune	1920	Aug	1			0
William C. Wilson	New York Times	1920	April	21	1	0	
William E. Morris	New York Tribune	1920	May	8	1	0	
William E. Morris	New York Herald	1922	Apr	13			1
William E. Morris	Democrat and Chronicle	1920	Aug	10	1	1	1
William F. Moore	The Evening World	1921	Sep	6	1	1	
William J. A. Caffrey	Daily News	1921	Dec	11			1
William J. Bogenshutz	Standard Union	1923	Nov	5	0	0	0
William J. Bogenshutz	Standard Union	1922	May	14	0	0	
William Young	New York Times	1921	Apr	10	0		0

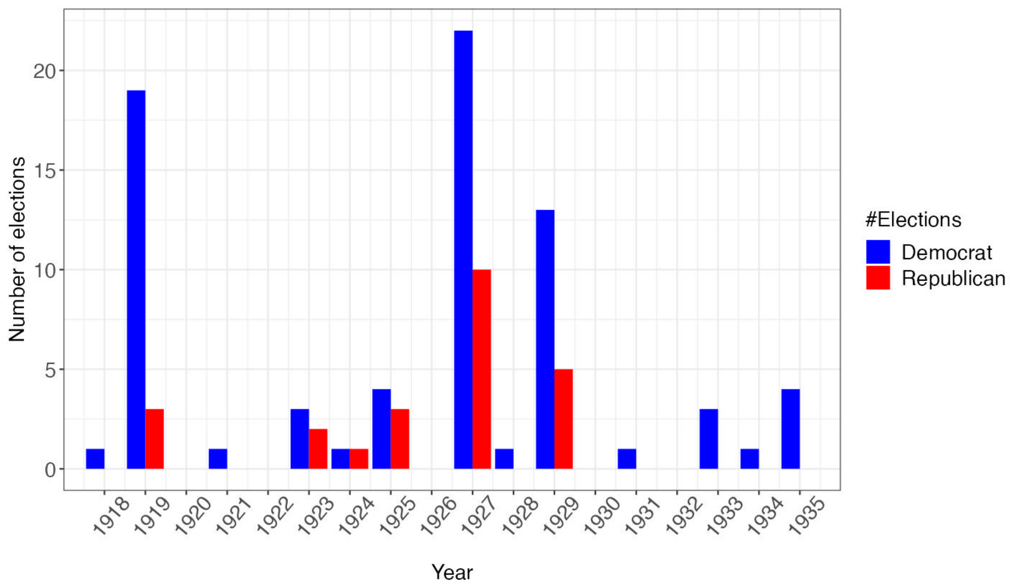
Note. Table 2.B.1 displays the teh full list of articles used to classify judge descisions in Chpater 2.3.1. It reports the name the Newspaper as well as the classification fo a judge' descisions. Eviction equals to one if a tenant was evicted and zero otherwise, rent decrease equals to one if a judg decided to decrease the amount demanded by a landlord and no increase equals one if a judge was not granting any increase demanded by the landlord.

Table 2.B.2: Descriptive statistics

	1918	1919	1920	1921	1922	1923	1924	1925	1926
	Panel A: Rents								
Monthly rent	$130	$179	$296	$197	$166	$158	$142	$149	$159
	(108.64)	(119.989)	(475.107)	(138.714)	(85.932)	(92.998)	(89.933)	(100.003)	(150.552)
Rooms	5	4	3	4	4	4	4	4	4
	(2.746)	(2.145)	(2.132)	(2.356)	(2.334)	(2.262)	(2.052)	(1.933)	(2.116)
N	809	847	922	1623	1256	1494	1582	1964	1689
	Panel B: Residential transactions								
Price	$21414	$19887	$20949	$15777	$10727	$6578	$8249	$29206	$13126
	(41731.103)	(111685.514)	(44138.273)	(33231.838)	(19765.609)	(20830.28)	(28282.25)	(73762.78)	(28884.448)
sqft	2398	2645	2248	2488	3381	3255	3437	2689	3315
	(1541.186)	(2067.903)	(1647.158)	(6355.562)	(13521.422)	(21090.19)	(25394.846)	(2714.239)	(11581.862)
N	144	559	813	617	376	2671	1899	336	533
	Panel C: Commercial transactions								
Price	$119280	$78721	$101572	$61646	$42563	$33619	$21148	$107912	$110787
	(410843.403)	(165992.411)	(190404.928)	(150753.604)	(71252.575)	(104504.112)	(66470.361)	(204695.079)	(412839.862)
sqft	2530	2477	2528	2076	2555	3062	4058	2188	2214
	(3575.654)	(2508.669)	(2895.888)	(1908.584)	(2515.375)	(3935.621)	(17841.586)	(3032.147)	(2619.047)
N	23	58	148	71	37	316	221	44	82
	Panel D: Judges								
Avg. Judge	2.33	2.35	2.48	2.49	2.49	2.49	2.46	2.46	2.46
	(1.022)	(0.994)	(1.243)	(1.214)	(1.214)	(1.214)	(1.22)	(1.22)	(1.22)
N judges	45	46	46	47	47	47	48	48	48
Avg. Rep. judge	0.93	1.11	1.04	1.02	1.02	1.02	1	0.94	0.85
	(1.338)	(1.524)	(1.349)	(1.343)	(1.343)	(1.343)	(1.337)	(1.262)	(1.22)
N Rep. judges	15	17	20	20	20	20	20	19	17

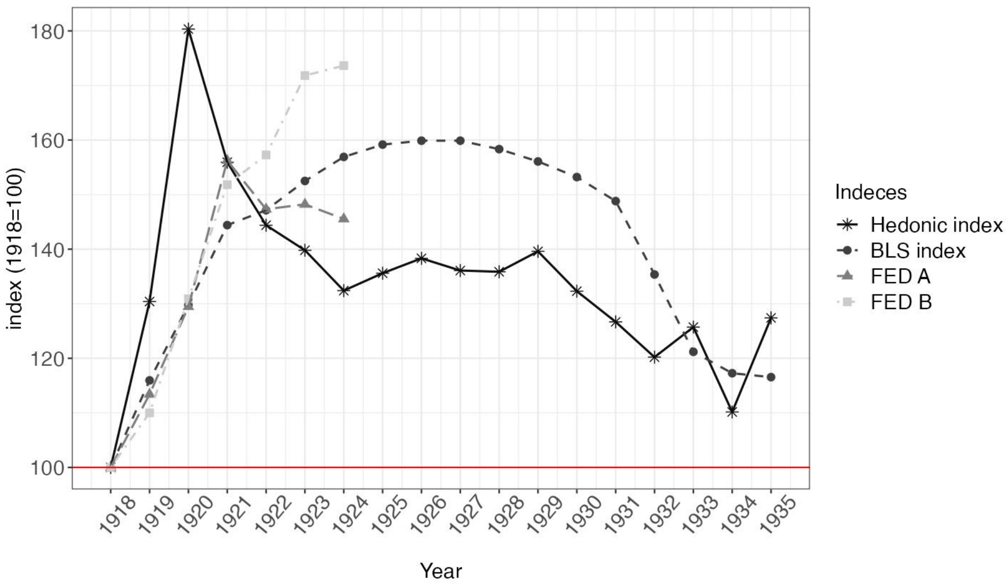
Note. Table 2.B.2 reports means and standard deviations in parentheses. Panel A describes the main outcomes in the rent dataset. Panel B-C describes the transaction price of residential and commercial properties. Panel D displays the average number of (republican) judges by municipal court district. Totals are indicated by N. All prices had been deflated using the cpi deflator and are given in 1918 Dollars.
Source. (State) (1925). The City of New York.

Figure 2.B.2: Judge elections



Note. Figure 2.B.2 shows the absolute number of elections by year. Elections have been grouped by political affiliation of the winning judge, which also includes winning incumbent judges. Therfore, the figure includes elections which are changing as well as preserving the a seat in a court.
Source. Citywide Administrative Services (1918).

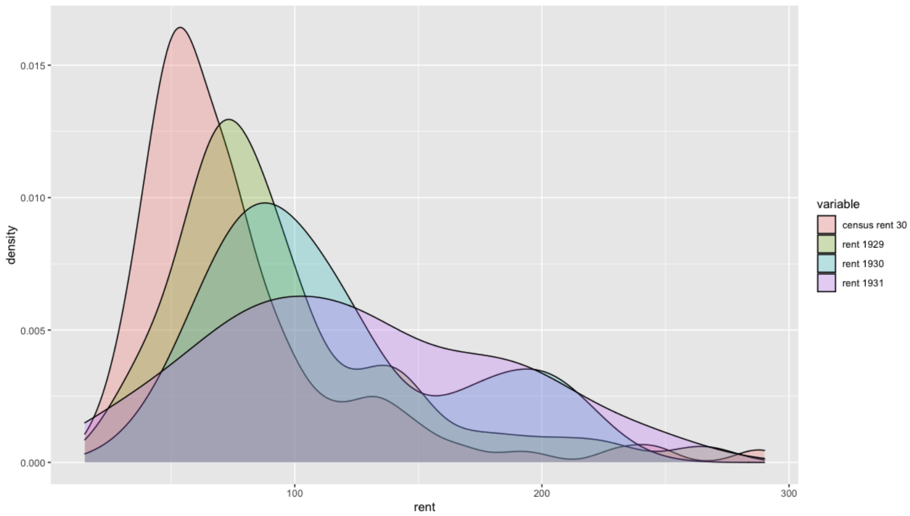
Figure 2.B.3: Rent indeces



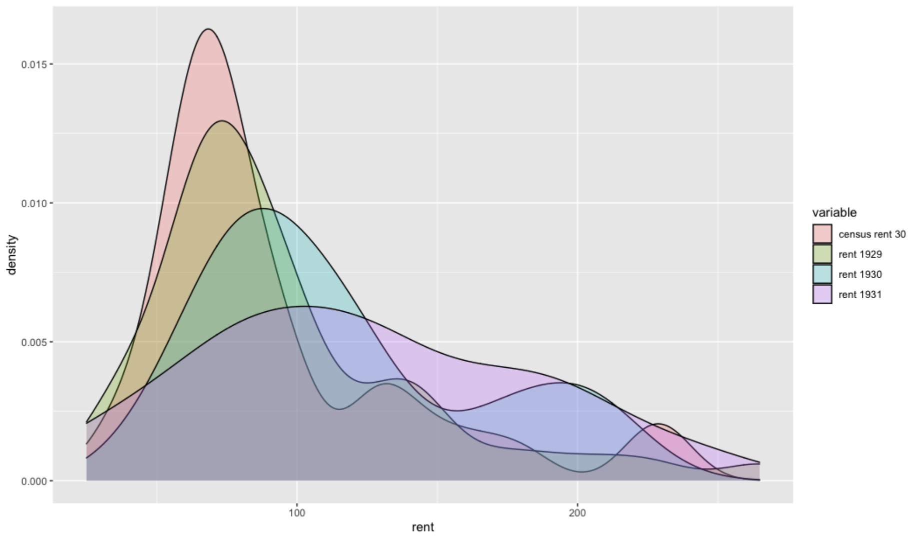
Note. Figure 2.B.3 shows rent indexes for New York City using 1918 as the base year. The black solid line shows a hedonic index using market rents (Hedonic index). The index values have been obtained from the fixed effects of regressing the logarithm of rent on property-level controls and time-fixed effects. The black dashed line shows values from a sitting tenants index by the Bureau of Labor Statistics (BLS index). Finally, the light gray dashed and dashed-dotted lines are indices from the Federal Reserve. FED A gives rental prices for properties at the upper end of the market. FED B shows index values for properties at the lower end of the market. Both indeces were taken from Table 4 in (State) (1925).
Source. Author's own calculations; BLS (1941); (State) (1925).

Figure 2.B.4: Rent distributions

(a) Census and sample distribution



(b) Reweighted census distribution



Note. Figure 2.B.4 shows the distribution of the contract rent from the 1930 census and from our sample of market rents for the years 1929 to 1931. Panel 2.B.4a plots the rent distribution in the 1930s census vs the sample distributions from 1929 to 1931. Panel 2.B.4b plots the reweighted distribution in the 1930s census vs the sample distributions from 1929 to 1931. We calculate frequency weights as the number of observations within a neighborhood divided by the total number of rental observations. We calculate the difference in neighborhood weights between the census and our rent sample by subtracting the weights from our sample from the census. We then add one to each weight. Thus, we give the average rent in the census a higher weight when it is observed with a higher frequency than in our sample and for neighborhoods observed at a lower frequency, we reduce the weight of the distribution.
Source. Author's own calculations; US federal census.

Appendix 2.C Additional Results

2.C.1 RDD estimates for Manhattan

Table 2.C.1: Effect at cut-off on rental prices - 1918-1920 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.238	-0.067	-0.048	0.067	-0.304	-0.164	0.205	-0.022
	0.186	0.137	0.096	0.088	0.293	0.222	0.163	0.150
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.196	0.296	0.148	0.591	0.356	0.339	0.169	0.678
Obs.	2489.000	2355.000	2355.000	2355.000	2489.000	2355.000	2355.000	2355.000
R2	0.473	0.472	0.559	0.405	0.436	0.465	0.554	0.399
ci_l_rb	-0.764	-0.437	-0.051	-0.354	-0.882	-0.678	0.136	-0.617
ci_r_rb	0.145	0.232	0.582	0.286	0.369	0.288	1.034	0.253

Note. Table 2.C.1 reports regression results for ask rents; the data had been subsetted for the pre rent control period 1918-1920 and only for properties located in Manhattan; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.2: Effect at cut-off on rental prices - 1918-1920 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.100	0.039	0.076	0.058	0.022	0.061	0.191*	0.039
	0.061	0.052	0.071	0.044	0.109	0.090	0.092	0.054
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.354	0.340	0.170	0.681	0.327	0.314	0.157	0.627
Obs.	5438.000	5156.000	5156.000	5156.000	5438.000	5156.000	5156.000	5156.000
R2	0.284	0.301	0.282	0.299	0.288	0.308	0.283	0.298
ci_l_rb	-0.050	-0.087	-0.011	-0.061	-0.218	-0.122	-0.048	-0.130
ci_r_rb	0.234	0.141	0.354	0.139	0.265	0.268	0.411	0.217

Note. Table 2.C.2 reports regression results for ask rents; the data had been subsetted for the rent control period 1921-1926 and only for properties located in Manhattan; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.3: Effect at cut-off on residential prices - 1918-1920 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.195	0.033	0.043	-0.081	-0.128	0.045	0.155	-0.200
	0.444	0.428	0.405	0.364	0.513	0.464	0.547	0.392
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.360	0.334	0.167	0.668	0.669	0.771	0.372	1.489
Obs.	530.000	494.000	494.000	494.000	530.000	633.000	494.000	494.000
R2	0.157	0.184	0.339	0.138	0.121	0.141	0.186	0.121
ci_l_rb	-1.129	-0.868	-0.825	-1.086	-1.186	-0.943	-1.042	-1.073
ci_r_rb	0.789	1.016	0.830	1.094	0.982	1.081	1.265	0.973

Note. Table 2.C.3 reports regression results for residential trasnaction prices; the data had been subsetted for the pre rent control period 1918-1920 and only for properties located in Manhattan; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.4: Effect at cut-off on residential prices - 1921-1926 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.676*	-0.466	-0.779**	-0.045	-0.522	-0.315	-0.725*	-0.159
	0.267	0.266	0.255	0.283	0.352	0.348	0.296	0.329
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.354	0.340	0.170	0.681	0.327	0.314	0.157	0.627
Obs.	5438.000	5156.000	5156.000	5156.000	5438.000	5156.000	5156.000	5156.000
R2	0.284	0.301	0.282	0.299	0.288	0.308	0.283	0.298
ci_l_rb	-0.050	-0.087	-0.011	-0.061	-0.218	-0.122	-0.048	-0.130
ci_r_rb	0.234	0.141	0.354	0.139	0.265	0.268	0.411	0.217

Note. Table 2.C.4 reports regression results for residential transaction prices; the data had been subsetted for the rent control period 1921-1926 and only for properties located in Manhattan; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.5: Effect at cut-off on commercial prices - 1918-1920 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.497	0.711	-121.092***	1.251	-2.194**	-0.924	-5.677***	-1.079
	0.414	0.587	0.000	1.157	0.773	0.919	0.084	1.098
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.411	0.483	0.242	0.967	0.567	0.628	0.299	1.195
Obs.	172.000	145.000	145.000	145.000	172.000	167.000	145.000	145.000
R2	0.570	0.677	1.000	0.402	0.443	0.624	0.756	0.437
ci_l_rb	-2.239	-0.803	-121.092	-2.093	-3.584	-2.563	-6.009	-3.708
ci_r_rb	0.164	0.765	-121.092	2.036	-2.365	-0.274	-5.513	1.053

Note. Table 2.C.5 reports regression results for commercial transaction prices; the data had been subsetted for the pre rent control period 1918-1920 and only for properties located in Manhattan; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.6: Effect at cut-off on commercial prices - 1921-1926 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.552*	0.407	5.316***	2.145***	1.296***	1.407**	-0.187	2.022**
	0.227	0.235	0.083	0.411	0.225	0.508	0.274	0.662
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.200	0.202	0.101	0.404	0.416	0.459	0.229	0.918
Obs.	570.000	444.000	444.000	444.000	570.000	444.000	444.000	444.000
R2	0.475	0.905	0.964	0.507	0.357	0.489	0.909	0.412
ci_l_rb	-0.045	-0.148	4.421	0.807	0.671	0.179	-0.689	0.129
ci_r_rb	0.974	0.823	5.499	2.709	1.688	2.246	0.441	2.897

Note. Table 2.C.6 reports regression results for commercial transaction prices; the data had been subsetted for the rent control period 1921-1926 and only for properties located in Manhattan; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.3. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

2.C.2 RDD estimates for alternative treatment boundary

Table 2.C.7: Effect at cut-off on rental prices - 1918-1920 - alternative boundary

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.063	0.021	-0.032	0.053	0.064	-0.025	-0.178	-0.028
	0.097	0.058	0.084	0.040	0.146	0.099	0.147	0.072
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.556	0.762	0.381	1.525	0.879	0.731	0.366	1.463
Obs.	3544.000	3383.000	3383.000	3383.000	3544.000	3383.000	3383.000	3383.000
R2	0.221	0.445	0.487	0.425	0.226	0.449	0.495	0.427
ci_l_rb	-0.169	-0.129	-0.448	-0.134	-0.257	-0.256	-0.384	-0.235
ci_r_rb	0.272	0.140	0.128	0.136	0.405	0.188	0.180	0.158

Note. Table 2.C.7 reports regression results for ask rents; the data had been subsetted for the pre rent control period 1918-1920; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.A.5. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.8: Effect at cut-off on rental prices - 1921-1926 - alternative boundary

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.086*	0.048	0.002	0.068*	0.097*	0.048	0.001	0.075*
	0.038	0.038	0.056	0.029	0.049	0.048	0.063	0.035
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.797	0.543	0.272	1.087	1.289	0.925	0.462	1.850
Obs.	11847.000	11469.000	11469.000	11469.000	11847.000	11469.000	11469.000	11469.000
R2	0.122	0.285	0.301	0.280	0.122	0.285	0.285	0.262
ci_l_rb	0.011	-0.036	-0.175	-0.030	-0.012	-0.059	-0.213	-0.037
ci_r_rb	0.176	0.133	0.206	0.155	0.203	0.143	0.199	0.164

Note. Table 2.C.8 reports regression results for ask rents; the data had been subsetted for the rent control period 1921-1926; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.A.5. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.9: Effect at cut-off on residential prices - 1918-1920 - alternative boundary

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.291	0.180	0.320	0.144	0.231	0.133	0.405	0.108
	0.367	0.343	0.433	0.214	0.411	0.349	0.555	0.248
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.718	0.751	0.376	1.502	1.307	1.368	0.674	2.694
Obs.	1516.000	1396.000	1396.000	1396.000	1516.000	1742.000	1396.000	1396.000
R2	0.142	0.156	0.209	0.165	0.168	0.170	0.165	0.208
ci_l_rb	-0.457	-0.521	-0.744	-0.589	-0.704	-0.642	-0.917	-0.582
ci_r_rb	1.069	0.903	1.332	0.895	1.137	0.982	1.502	0.835

Note. Table 2.C.9 reports regression results for residential transaction prices; the data had been subsetted for the pre rent control period 1918-1920; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.A.5. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.10: Effect at cut-off on residential prices - 1921-1926 - alternative boundary

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.039	0.052	-0.009	0.174	-0.024	-0.061	-0.009	0.034
	0.214	0.196	0.254	0.131	0.231	0.250	0.323	0.170
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.715	0.931	0.466	1.862	1.684	1.428	0.714	2.856
Obs.	6432.000	5791.000	5791.000	5791.000	6432.000	5791.000	5791.000	5791.000
R2	0.219	0.236	0.227	0.219	0.214	0.230	0.235	0.202
ci_l_rb	-0.500	-0.481	-0.834	-0.489	-0.624	-0.627	-0.874	-0.571
ci_r_rb	0.459	0.434	0.521	0.373	0.428	0.473	0.601	0.395

Note. Table 2.C.10 reports regression results for residential trasnaction prices rents; the data had been subsetted for the rent control period 1921-1926; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.A.5. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.11: Effect at cut-off on commercial prices - 1918-1920 - alternative boundary

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	-0.347	1.015	0.935***	0.470	-1.367	-0.445	1.221***	0.754
	0.660	0.792	0.185	0.727	0.779	0.559	0.253	1.161
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.555	0.599	0.299	1.197	0.698	0.597	0.356	1.425
Obs.	224.000	184.000	184.000	184.000	224.000	219.000	184.000	184.000
R2	0.406	0.521	0.718	0.327	0.355	0.485	0.720	0.310
ci_l_rb	-1.921	-0.870	0.581	-1.733	-3.385	-2.009	0.801	-2.227
ci_r_rb	0.854	2.509	1.337	2.999	-0.694	-0.088	1.616	2.751

Note. Table 2.C.11 reports regression results for commercial transaction prices; the data had been subsetted for the pre rent control period 1918-1920; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.A.5. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; All specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

Table 2.C.12: Effect at cut-off on commercial prices - 1921-1926 - alternative boundary

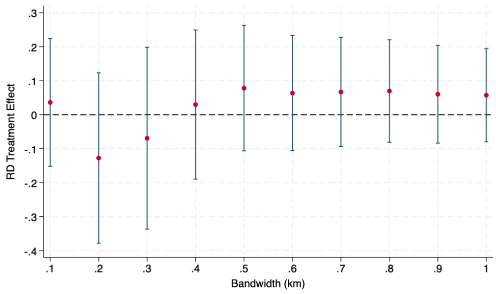
	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.310	0.461	1.335***	0.264	0.390	0.712	0.828	0.275
	0.281	0.471	0.325	0.357	0.350	0.607	0.494	0.502
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.788	0.779	0.390	1.558	1.179	1.060	0.530	2.120
Obs.	771.000	596.000	596.000	596.000	771.000	596.000	596.000	596.000
R2	0.333	0.397	0.459	0.352	0.302	0.382	0.420	0.334
ci_l_rb	-0.238	-0.578	0.292	-0.778	-0.210	-0.433	-0.484	-0.812
ci_r_rb	0.946	1.691	1.910	1.531	1.200	2.146	1.724	1.698

Note. Table 2.C.12 reports regression results for commercial transaction prices; the data had been subsetted for the rent control period 1921-1926; the running variable is the distance from a property to the treatment boundary as shown in Figure 2.A.5. Columns 1–4 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens **and** Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, total square feet, and indicators for main construction materials, for land use, if the property was a loft, if it is located at the top floor or basement, and if it was a flat or a house; all specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

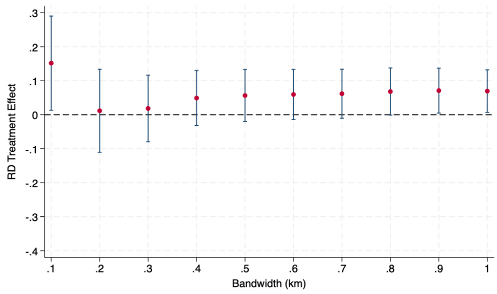
2.C.3 RDD estimates for Alternative bandwidth choices

Figure 2.C.1: Alternative bandwidth - Effect at cut off on rental price

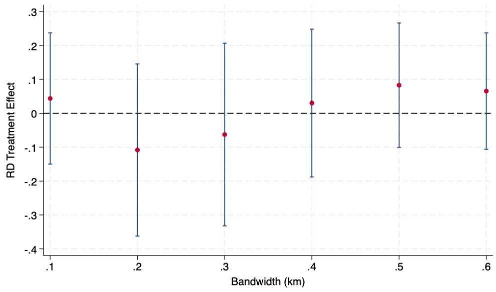
(a) Republican only MCD 1918-1920



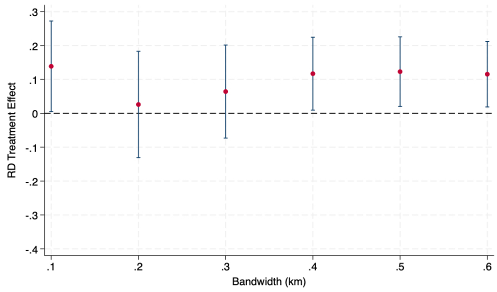
(b) Republican only MCD - 1921-1926



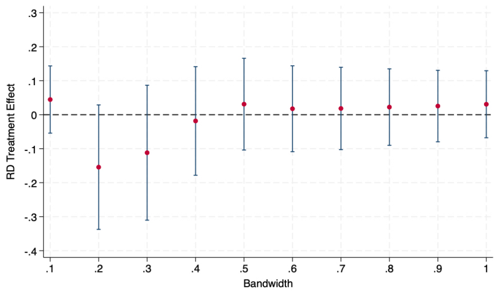
(c) Republican only MCD - Manhattan - 1918-1920



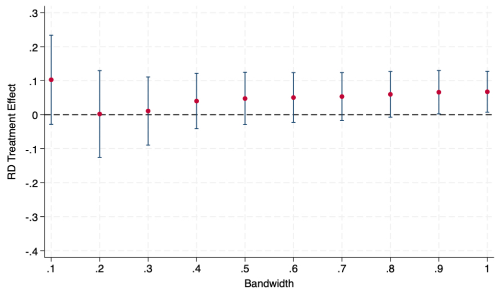
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



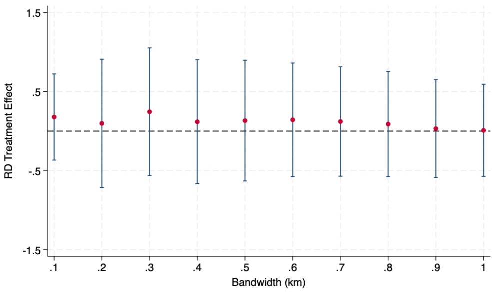
(f) Majority Republican MCD - 1921-1926



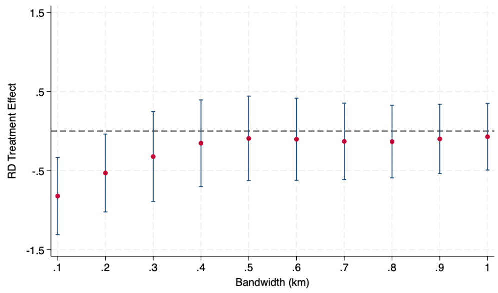
Note. Figure 2.C.1 shows RD estimates from estimating Equation 2.1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 2.1 is estimated using a triangular kernel with a linear fit; the outcome variable is the logarithm of rents. We start with a Bandwidth of 100m and extend by 100m until 1km; we report results for a sample of the pre rent control period (1918-1920) and during rent control (1921-1926). Panel 2.C.1a and 2.C.1b use the distance to the boundary between Republican and Democrat only MCDs; Panel 2.C.1c and 2.C.1d subset the sample for Manhattan only; Panel 2.C.1e and 2.C.1f use the distance to the boundary between majority an non-majority Republican MCDs. Standard errors are clustered at the neighborhood level; vertical bars indicate 95% confidence intervals. We use a triangular kernel with a linear fit.

Figure 2.C.2: Alternative bandwidth - Effect at cut off on residential prices

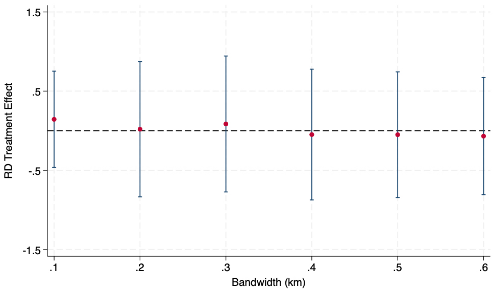
(a) Republican only MCD - 1918-1920



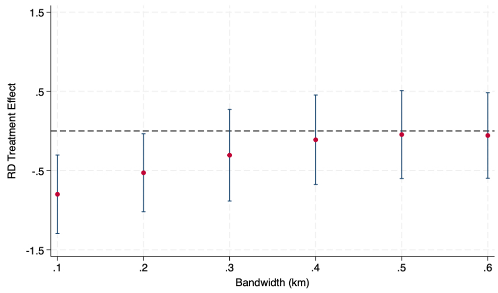
(b) Republican only MCD - 1921-1926



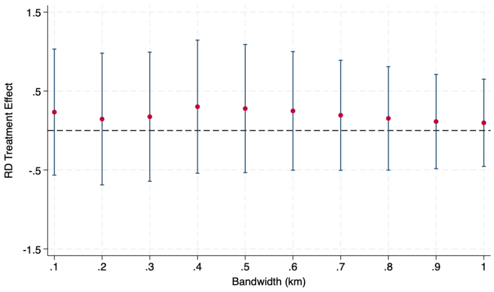
(c) Republican only MCD - Manhattan - 1918-1920



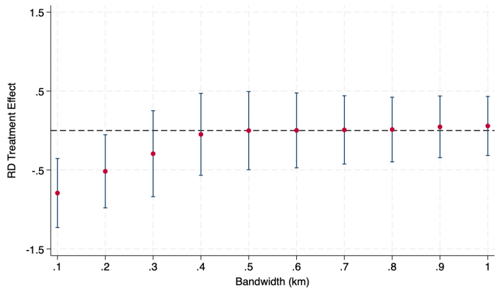
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



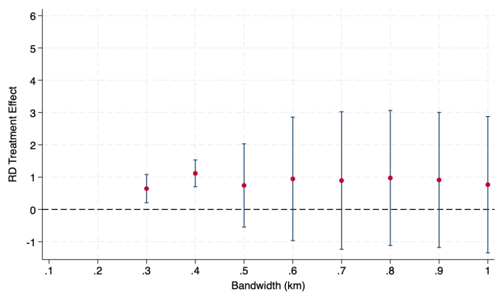
(f) Majority Republican MCD - 1921-1926



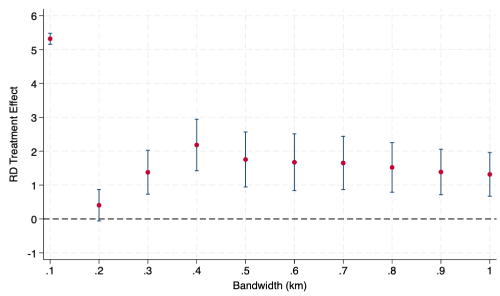
Note. Figure 2.C.2 shows RD estimates from estimating Equation 2.1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 2.1 is estimated using a triangular kernel with a linear fit; the outcome variable is the logarithm of residential transaction prices. We start with a Bandwidth of 100m and extend by 100m until 1km; we report results for a sample of the pre rent control period (1918-1920) and during rent control (1921-1926). Panel 2.C.2a and 2.C.2b use the distance to the boundary between Republican and Democrat only MCDs; Panel 2.C.2c and 2.C.2d subset the sample for Manhattan only; Panel 2.C.2e and 2.C.2f use the distance to the boundary between majority an non-majority Republican MCDs. Standard errors are clustered at the neighborhood level; vertical bars indicate 95% confidence intervals. We use a triangular kernel with a linear fit.

Figure 2.C.3: Alternative bandwidth - Effect at cut off on commercial prices

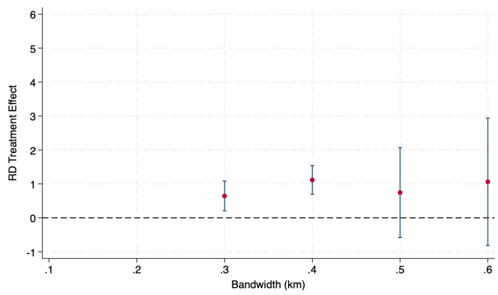
(a) Republican only MCD - 1918-1920



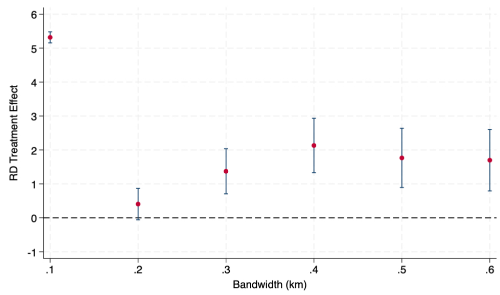
(b) Republican only MCD - 1921-1926



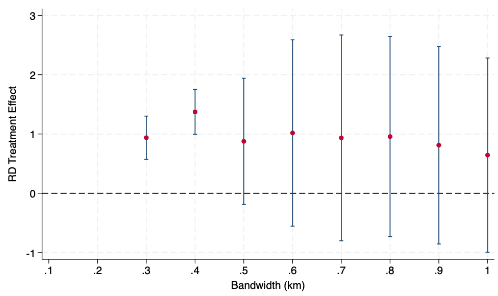
(c) Republican only MCD - Manhattan - 1918-1920



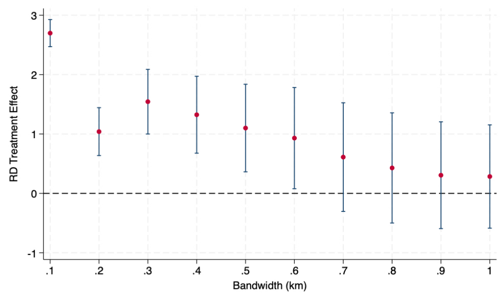
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



(f) Majority Republican MCD - 1921-1926

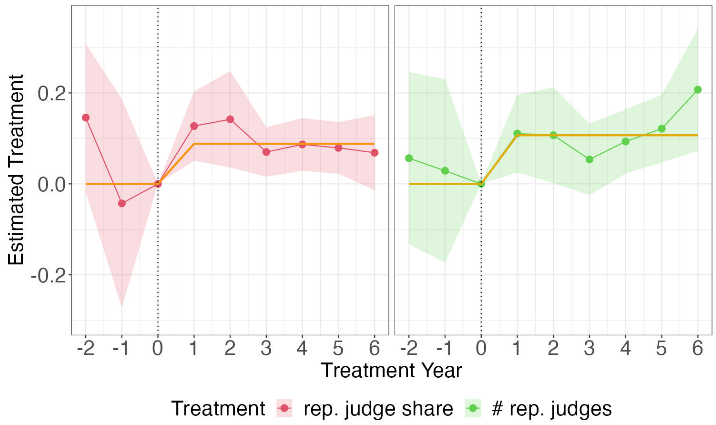


Note. Figure 2.C.3 shows RD estimates from estimating Equation 2.1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 2.1 is estimated using a triangular kernel with a linear fit; the outcome variable is the logarithm of commercial transaction prices. We start with a Bandwidth of 100m and extend by 100m until 1km; we report results for a sample of the pre rent control period (1918-1920) and during rent control (1921-1926). Panel 2.C.3a and 2.C.3b use the distance to the boundary between Republican and Democrat only MCDs; Panel 2.C.3c and 2.C.3d subset the sample for Manhattan only; Panel 2.C.3e and 2.C.3f use the distance to the boundary between majority an non-majority Republican MCDs. Standard errors are clustered at the neighborhood level; vertical bars indicate 95% confidence intervals. We use a triangular kernel with a linear fit.

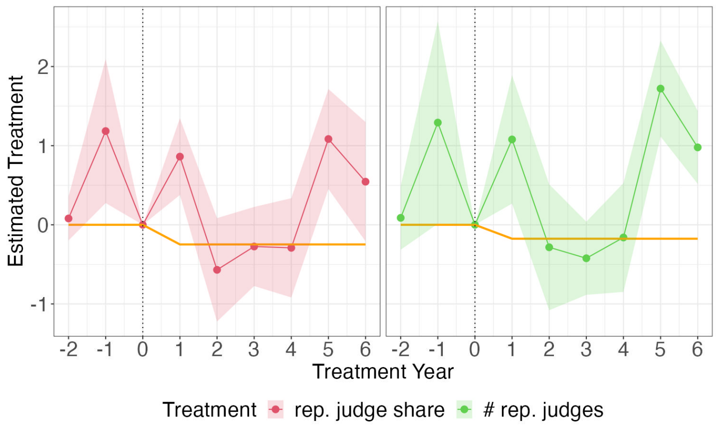
2.C.4 Event study results

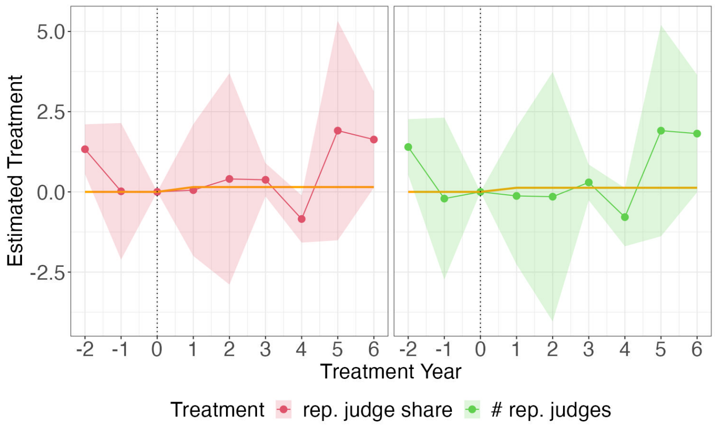
Figure 2.C.4: Effect of binary treatments

(a) Effect on rent prices



(b) Effect on residential prices





(c) Effect on commercial prices

Note. Figure 2.8 reports point estimates for β_τ in Equation 2.2 using the full et of property level controls, year and neighborhood fixed effects; year dummies have interacted with (1) the share of Republican judges in MCD u or (2) the number of Republican judges in MCD u; standard errors are clustered at the neighborhood (NTA) level; the shaded area show the estimated 95% confidence bands; the orange line plots the aggregated average from simple interaction between treatment $T_{t,u}$ and an indicator variable $\mathbb{1}(t > 1920)$. Panel 2.C.4a reports differences for ask rents tracts, Panel 2.8b differences in residential transaction prices and Panel 2.8c differen ces in commercial transaction prices.

Chapter 3

Housing Prices, Costs and Policy. the Housing Supply Equation in Ireland since 1970[1]

Joint with Ronan Lyons

Contribution

My contribution to this study includes preparing the final data set, performing the analyses, preparing the tables and figures for the manuscript, and co-writing the manuscript.

3.1 Introduction

Housing has emerged as one of the most pressing economic, social and political issues across a range of high-income cities and countries in recent decades. Among OECD countries, inflation-adjusted housing prices went from being largely stable in trends to steadily increasing in the third quarter of the 20th century (Knoll **andothers**, 2017). Construction rates have fallen in the same period with, for example, the peak of new homes built in New York City since 2000 (26,400 in 2007) roughly half the 1961-1965 average, despite New York's population rising by one third in the same period. Land use restrictions and other policy-driven barriers to new supply may be at the heart of weak housing supply in many economies (Saiz, 2010).

In this paper, we examine the determinants of new supply in Ireland, a country with a volatile housing system over recent decades, focusing in particular on the responsiveness of supply to prices and costs, using complementary approaches and data from the 1970s. We examine the link between prices, costs and supply,

[1] accepted for publication in *Real Estate Economic*

measured in both stocks and flows, following two approaches in the literature. We find strong evidence of a long-run relationship between flow measures of supply and both housing prices and construction costs. We then use error-correction (ECM) models to estimate that relationship and supplement that with an instrumental variable (IV) specification that uses a series of demand-shifters to validate the ECM results. Our baseline is a quarterly series running from 1975 to 2022, where the outcome of interest is the number of units for which planning permission is granted, conceptually the measure of supply most closely linked to price changes. We find similar results using other measures of supply (commencement of, capital formation in, and completion of new dwellings), Dublin-only data, annual data from 1970 and a panel of Ireland's 26 counties.

In Ireland, as in other economies, there has been much public debate about the required volume of new housing and the low level of completions since the Great Recession. Just 23 homes were built per 1,000 residents during the 2010s, compared to 155 in the 2000s, and an average of 75 in the 1970s, 1980s and 1990s. The lack of new supply occurred at a time of strong increases in prices: having fallen by just over 50% between 2007 and 2012, inflation-adjusted market prices rose by almost two thirds between 2012 and 2020. However, this unconditional correlation – comparing changes in supply and changes in prices – is distinct from the elasticity of housing supply to prices, a conditional correlation measuring how supply responds to prices, other things being equal.

Economic theory suggests the responsiveness, or elasticity, of housing supply in response to outward shifts in demand will be determined not by prices alone but instead by the ratio of prices to costs, i.e. new supply will be built when viable to do so. Further, the supply curve is likely to be kinked at the point of viability, below which housing supply is effectively inelastic (Hilber **and** Mense, 2021). With the existing stock of housing immobile, downward shifts in demand will largely be met with a fall in prices, rather than a fall in quantities. While housing prices (and land costs) vary substantially by location, build costs do not in a geographically small market such as Ireland. This means that the location of the demand curve at regional level, relative to the kink in supply, will determine the supply response.

The key determinants of supply in our analysis are, therefore, the capital value or market price of housing (at national, city, or county level); and the level of construction costs, allowing for relevant tax reliefs that drove trends in net construction costs 1998-2008, discussed in more detail below.[2] For simplicity, we

[2]Arguably, a third determinant is the level of site or land costs. However, land costs are likely to be extremely endogenous. In an extension, we include separately two proxies regional land costs. The first is agricultural land values (which extend back to 1970), while the second, available

refer to the elasticity of housing supply in response to price changes as the housing supply elasticity (HSE). In line with best practice for error correction methods, we concentrate on four empirical specifications of the error correction set-up: the baseline one-step OLS error-correction set-up; an augmented specification with an autoregressive lag structure (ARL); fully-modified OLS (FMOLS), as proposed by (Phillips **and** Hansen, 1990); and dynamic OLS (DOLS), as proposed by Stock **and** Watson (1993). However, despite the lagged nature of the ECM set-up, it is possible that prices are responding to supply. For that reason, we supplement our ECM analysis with an IV approach that uses demand shifters to identify supply responses. In the panel setting, to overcome asymptotic bias in the OLS estimator, we use the Panel Mean Group (PMG) estimator, as well as panel versions of FMOLS and DOLS, as per Pedroni (2000) and Kao **and** Chiang (2000), respectively.

We find strong evidence across all our specifications that housing prices, the cost of construction and measures of new housing supply are cointegrated; we do not find any similar evidence of a relationship between the stock of housing and prices/costs. Under our baseline, using national permits data from the 1970s and a one-step error-correction set-up in OLS, the estimated elasticity of housing supply to prices nationally is 0.9 while the elasticity to costs is roughly twice as large in magnitude (-1.9). We estimate elasticities using not only alternative error-correction methods, but also alternative measures of supply (investment, completions and, in the panel setting, commencements), as well as for Dublin (rather than nationally) and for a 26-county panel from the 1990s. In all ECM specifications and in the IV set-up, the predictions of basic economic theory – that prices positively affect and cost negatively affect new housing supply – are borne out by the analysis. We examine how elasticities vary over time and by location. In particular, we present evidence that responsiveness to prices rose between the 1980s and 1990s, then fell in the 2000s, before rising again in the 2010s. We also document significant heterogeneity in elasticities at the county level, with supply in Dublin among the least responsive to both prices and costs. These findings suggest new avenues for research on the determinants of supply elasticities.

We believe our contribution to the study of housing supply elasticity is as follows. We provide estimates of HSE in Ireland, at both national and regional level, allowing variation over a fifty-year period, for the first time.[3] Our findings

from 2004, is the median listed cost of a residential site at the regional level. Both specifications do not affect our main results; further details are available on request.

[3]While Caldera **and** Johansson (2013) include Ireland in their multi-country analysis, their supply equation is not based on economic theory. Specifically, it does not include the cost of land but does include population, a demand-side factor that should already be captured by prices. Further, they do not state the specific series used for their supply equation, which runs from 1980

imply that the low level of new housing supply in Ireland during the 2010s was not anomalous, but rather explained well by the fundamentals included in the long-run equation, in particular the dramatic rise in after-tax construction costs. While housing prices in 2020 were roughly 20% below their 2007 level, build costs after tax reliefs were between 70% and 90% higher in 2020 than 2007. Studies of other counties suggest lower HSE after 1995; while that is true in the 2000s, HSE was at its highest estimated level in the late 2010s. There is no systematic correlation between estimated price elasticities at county level and either cost elasticities or estimated ease of approval in the planning process. Nonetheless, those counties at either end – including Dublin, the country's most urban county, and Leitrim, arguably its most rural – are instructive. In particular, our findings do not contradict the idea that land-use restrictions or other policy barriers limiting supply have grown in importance over time and are more relevant in Dublin than elsewhere; rather, a more detailed analysis of elasticities across space (and over time) is required.

In the next section, we review the related literature, highlighting four stylized facts that emerge from the growing body of research and distinguishing between a more urban-focused literature that examines housing stocks and a more macro-related literature that analyses housing flows. In Section 3.3, we outline the construction of the dataset, including quarterly and panel series used for supply, prices and costs, as well as our deflators. Section 3.4 establishes our empirical strategy, including the various time series and panel specifications of the error-correction model, while Section 3.5 presents the results of national, Dublin and county-level panel analyses, across supply measures and specifications. The final section concludes by noting limitations as well as implications for both policymakers and the research community.

3.2 Literature Review

The body of research examining the elasticity of housing supply is both long-standing and, in recent years, rapidly growing. The early literature on housing supply, such as Alberts (1962) **and** Grebler **and** Burns (1982), looks at housing construction as a result of firm investment decisions. Their outcomes of interest therefore are housing investment or flows (permits, starts or completions). As argued by Topel **and** Rosen (1988), this literature assumes demand for investment

to 2007 at quarterly frequency, despite quarterly data on investment (their outcome of interest) only being available from 1995. Consequently, it is not possible to replicate their results, which imply a HSE of 0.6, i.e. an increase in housing prices of 10% leads to an increase in investment of 6% in the long run.

to be a small fraction of existing stock such that any new unit will be sold at current prices. Therefore, any new units depend on the level of prices relative to marginal costs. Much of the macroeconomic literature examining housing supply elasticities, exemplified by Caldera **and** Johansson (2013), adopts this approach.

An alternative approach was developed by Mayer **and** Somerville (2000). They use the monocentric city model to show that, in a spatial equilibrium setting, the stock of housing (levels) has to equal prices (levels). Therefore, new units would have been developed only if a city grows which can only happen through the development of new land. Much of the city-level (and typically U.S.-focused) literature more recently has been an extension of this approach, where changes in supply are functions of *changes* in costs. For example, in Green **andothers** (2005), their outcome of interest is the number of housing units for which permits were issued (for each of 45 MSAs in the USA) and their (sole) regressor is the change in housing prices for that MSA.

In general, estimated elasticities of housing supply with respect to prices vary substantially across locations (and time periods). This is a feature that has been shown for different levels of aggregation. Evidence ranges from perfectly elastic supply estimates (Follain, 1979; Stover, 1986; Malpezzi **and** Maclennan, 2001), to estimates ranging from 0.5 to 3 (Poterba, 1984; Mayo **and** Sheppard, 1996; DiPasquale **and** Wheaton, 1994; Harter-Dreiman, 2004; Paixão, 2021). Nonetheless, direct comparisons across studies are complicated, as studies differ by analytical framework and measurement of supply (such as new housing units built, building permits sought or issued, or the level of investment in construction from national accounts), as well as unit and period of coverage.

We highlight here four important stylized facts about HSE that emerge from a review of the literature. Firstly, a number of economic features are associated with responsive housing supply (high HSE). In particular, high housing supply elasticity can be explained by: low population density (Green **andothers**, 2005; Caldera **and** Johansson, 2013); less restrictive building codes (Green **andothers**, 2005; Ihlanfeldt **and** Mayock, 2014); and geographically less diverse regions, with mountainous terrain and water coverage associated with lower HSE (Saiz, 2010; Meen **and** Nygaard, 2011). Further, recent studies using small geographic units show that HSE increases with distance from the urban center, a feature consistent with findings in relation to population density (Baum-Snow **and** Han, 2022). Due to the high correlation and intuitive reasoning, the literature focused on the U.S. often uses measures of geographic characteristics from Saiz (2010) or the Wharton Regulatory Land Use Index (Gyourko, Saiz **andothers**, 2008) as proxies for HSE Glaeser, Gyourko **and** Saiz (2008) **and** Davidoff (2013). However, in the more recent literature these measures have been criticized, as the Saiz instrument can be correlated with other city characteristics such as different industrial compositions Davidoff (2016). To overcome this issue, Guren **andothers** (2021) construct a sensitivity instrument by estimating the systematic historical sensitivity of local housing prices to regional housing cycles and then interacting these historical sensitivity estimates with today's shock to regional housing prices.

The second stylised fact from the literature is that the critical step is overcoming potential endogeneity problems arising from the well-known problem of separating housing demand from housing supply. As noted in Baum-Snow **and** Han (2022), unobserved productivity shocks could affect housing prices by increasing demand and also supply by increasing construction costs, leading to a downward bias in the HES estimate. Aastveit **andothers** (2020) suggest that there is likely reverse causality between prices and supply. Both papers favor instrumental variable strategies, such as Bartik instruments (or at least instruments similar in spirit to or strongly correlated with Bartik shocks) or crime rates, which would isolate the demand-driven component in prices. An alternative approach is to use careful macroeconometric analysis, such as error-correction methods or seemingly unrelated regressions, to allow system-wide data to reveal underlying relationships.

Thirdly, HSE is plausibly related to the severity of housing market cycles. Glaeser, Gyourko **and** Saiz (2008) persuasively argue that the real estate cycle is affected by the HSE: regions with high HSE are less likely to experience bubbles and show less price appreciation compared to regions with low HSE levels. There may be some ambiguity in this relationship, however. On the one hand, it is plausible that a higher HSE led to greater overbuilding during the pre-2007 boom, resulting in greater excess inventory and thus larger price declines in the post-boom period; on the other hand, a higher HSE leads to smaller price increases during the boom, so there is a smaller price correction after the boom. Empirically, this has been supported by Huang **and** Tang (2012) **and** Ihlanfeldt **and** Mayock (2014). The former point to a significant relationship between HSE and price declines during the post-2007 bust, while the latter only find evidence that HSE played a role during the 2000 boom, but not during the subsequent bust. Davidoff (2013) finds no significant relationship between cycle intensity and HSE at the state level, albeit after accounting for state-level fixed effects. In a more recent study, Oikarinen **andothers** (2018) estimate the elasticity of housing prices with respect to income and contrast them with HSE represented by Saiz's measure. They confirm the finding of Glaeser, Gyourko **and** Saiz (2008) that bubble size and duration are inversely related to supply elasticity.

The final stylised fact from the HSE literature builds on the above and relates to its wider importance. Given the importance of housing in both household expenditure and household balance sheets, as well as the wider stock of assets, HSE has implications for wider macroeconomic and financial stability. Accetturo **andothers** (2020) show that demand shocks can have a positive impact on employment and growth in cities where the HSE is higher in Italy. Using a general equilibrium approach, Hsieh **and** Moretti (2019) have shown that a low HSE, due to restrictive land-use conditions, can lead to a welfare loss by preventing workers from accessing highly productive areas due to higher prices.

3.3 Data

In this section, we describe the dataset assembled for the subsequent analysis. We start by describing four related but distinct measures of new housing supply. We then describe price and cost series. We outline our data on mortgage market conditions, before describing other series used, including the deflators. A summary of the main data used in the regression is shown in Appendix 3.A.

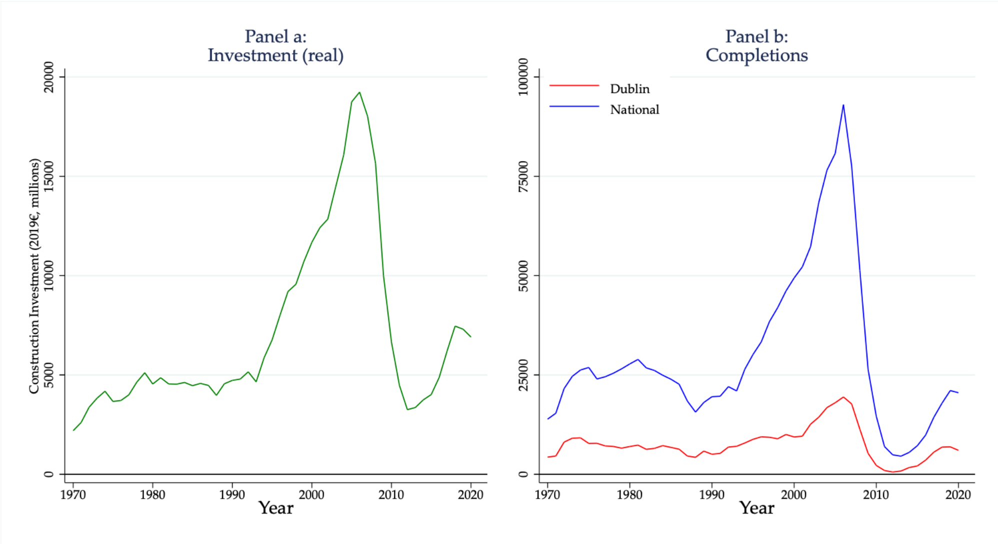
3.3.1 Housing Supply

Our core measure of housing supply at national level is the volume of **fixed capital formation** (FCF) in dwellings, including improvements, during a specified period (year or quarter). We take this from the PxStat database provided by Ireland's Central Statistics Office (CSO), specifically Table N2015, which gives gross fixed capital formation in millions of euro (current) from 1995 to 2020 at annual frequency, and Table NQQ40, which gives it quarterly frequency. Table NQQ40 distinguishes between FCF in the construction of new dwellings and in improvements to existing dwellings, allowing us to devise, for the period from 1995 at both annual and quarterly frequencies, series on FCF with and without improvements. For the period before 1995, only annual data are available. We use PxStat Table NAH15 for FCF in dwellings (in million of euro, current) for the period 1970-1995.[4] The left-hand panel of Figure 3.1 gives fixed capital formation, in millions of 2019 euro, at annual frequency.

Our key alternative measure of housing supply, over longer periods, is the volume of dwellings completed in a particular period. From the first quarter of 2011, this is available from CSO Table NDQ06, at the level of the local authority

[4]Tables A.13 and B.13 of the 1977 edition of National Income and Expenditure provide series back to 1960 on investment in dwellings, nationally at annual frequency, but these are not used here, given the limitations on other data series.

Figure 3.1: Measures of new housing supply, since 1970



and for three types of dwellings: estate or scheme houses; apartments; and one-off houses. For the period 1970-2011, we use connections to the electricity grid to measure completions of new homes.[5] This is available from CSO Table HSA11, again at the level of the local authority, by sector (public or private housing) and from 1994 by type (houses or apartments). We aggregate to national level or to the level of the 26 traditional counties from local authority. The number of local authorities has varied over time, most recently being reduced from 34 to 31 in 2014, but are consistently nested within the 26 traditional counties. The right-hand panel of Figure 3.1 gives the number of new dwellings completed, nationally and for Dublin, from 1970.

Our third measure of supply is commencements, i.e. the number of dwellings commenced in a particular period. This is available from January 2004, at monthly frequency and at the level of the local authority, using CSO Table HSM12 (to February 2014) and Table HSM74 thereafter.[6] As with completions, we aggregate from local authority level (and here from monthly) to county and national level at quarterly frequency.

Our final measures of supply relate to planning permissions. Since 1964, the construction of new dwellings in Ireland has required permission to build from the relevant local authority. Statistics on the number of new homes for which planning permission has been granted are available from CSO Table BHQ05,

[5]Some series for completions, aided by national grants or undertaken by local authorities, are available back to the 1920s, although their comprehensiveness cannot be ascertained.

[6]At the time of writing (October 2021), this table had not been updated since February 2021. For that reason, we used the original source for the CSO, Table A5CB1 published by the Department of Housing, for the period March-June 2021.

at national level and quarterly frequency, from the start of 1975 (split by broad type: houses and apartments/flats). Quarterly figures by local authority are available from CSO Tables BHQ02 (2001Q1-2017Q4) and BHQ12 (from 2018Q1 respectively), for each of three housing types: estate housing, apartments, and one-off dwellings.

Lastly, we use rich spatial data on planning permissions lodged and granted, at the level of the individual site, using the National Planning Application Database (NPAD), which is operated by Ireland's Department of Housing, Local Government and Heritage. In addition to the location and outline of the relevant site, the dataset includes the dates of the receipt of the application and of the local authority's decision, and the outcome of decision, in particular whether (conditionally) accepted or rejected. There are over 410,000 applications from 2010 to mid-2021. These were then aggregated to the level of 'micro-markets', used as part of the Daft.ie Report (explained in more detail below). To link site-level planning data to micro-markets, based on named areas, all official Census 'Small Areas', created by the CSO, were assigned to a micro-market.[7] A spatial join was then used to connect planning permissions and micro-markets. This gives us the number of planning permissions lodged, for each of 389 micro-markets, at quarterly frequency from 2012Q1 to 2021Q2. It should be noted that the scaling is number of permissions, rather than the number of dwellings. For this reason, it is best considered as a measure of the responsiveness of one-off housing (where each permission relates to one and only one dwelling) rather than overall supply and is most comparable to county-level data on permissions for one-off dwellings, rather than all dwellings.

3.3.2 Housing Prices

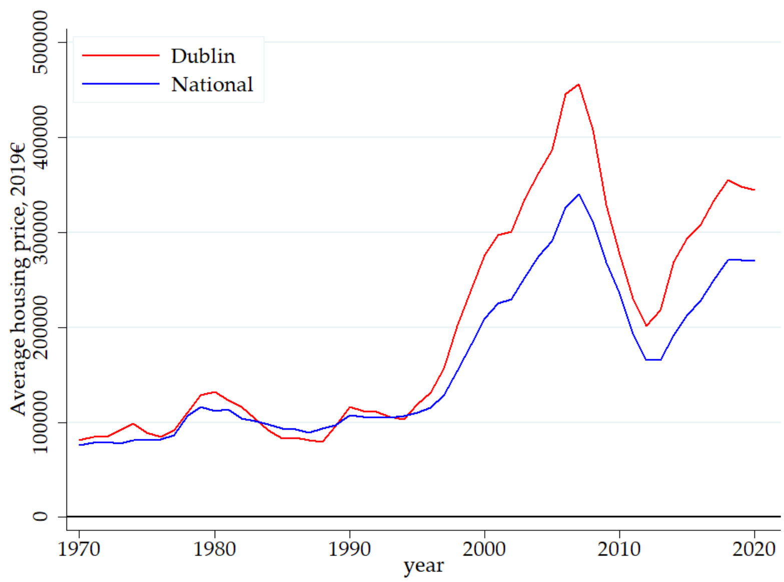
We focus on the sale price of housing, as this reflects the capital value of real estate upon construction. National series on the sale price of housing are generated both for new dwellings and for the mix-adjusted price of all dwellings. For new dwellings, the median price of newly-built homes is available from 2010 at annual frequency, using CSO Table HPA05. This is extended back to 1970 using Department of Environment series on the average price of newly-built dwellings, by year. The data are similar to those underlying CSO Table HSA06 but, in their original published form, included separate averages for new and

[7] The coordinates of over 1 million listings on daft.ie, with assigned micro-market IDs, were projected over the Small Area shapefile. The most commonly occurring micro market, based on listings per small area, was used to assign a small area to a micro market. Once all small areas were assigned a micro market, the shapefiles were dissolved by micro market ID, resulting in a map of all micro-markets with well defined CSO based boundaries.

second-hand dwellings, nationally and for the five city boroughs, at both annual and quarterly frequency. The mix-adjusted price of all dwellings is taken from CSO HPA13 for 2005-2020, extended back to 1996 using the ESRI-PTSB average price, and extended back from 1996 to 1970 using the average of mean prices for both new and second-hand dwellings. For Dublin at annual frequency, and for both national and Dublin series at quarterly frequency (from 1995), the same sources are used.

Mix-adjusted average prices by county are available from 2006Q1 to 2021Q2, using averages calculated from the daft.ie report. While these are listed prices, their treatment – in particular the use of hedonics but also date of initial listings – means that they are very highly correlated with transaction prices, even during volatile market conditions (Lyons, 2019). An alternative is to use the mean price of transactions involving newly-built homes, by aggregating monthly data at the local authority level on the aggregate value of transactions and number of transactions, available from CSO Table HPM05. Daft.ie also provide average (mix-adjusted) price by micro-market, which facilitates the analysis of supply elasticity at quarterly frequency.

Figure 3.2: Inflation-adjusted sale price of housing, since 1970



3.3.3 Housing Costs

The principal series used to measure construction costs are CSO Table HSA09, for annual series, and its monthly equivalent Table HSM09. This was an official index, using a fixed output type (dwelling) and pricing the relevant labour and material inputs used accordingly, and ran at monthly frequency from 1975 to 2017. It is extended back to 1970 at annual frequency using the 'Capital Goods in Building & Construction' sub-component of the Wholesale Price Index and forward to 2021 using percentage changes in the estimate of the rebuilding cost of existing dwellings, per square metre, published by the Society of Chartered Surveyors (SCSI).

This official series suffers from the limitation that it is a fixed-quality basket, while housing quality is likely to have drifted substantially higher during the period being investigated (Lyons, 2014). The SCSI series provides an alternative from 1989, the first year in which it published estimates of the rebuilding cost of a home for insurance purposes. Throughout, a figure for Dublin was published, with additional estimates for Cork (from 1992), Galway (from 1996), Waterford and Limerick (from 2004) and North-West and North-East regions (from 2013). While regional differences in level exist, the trends are very similar, which provides justification for the identifying assumption of an annual trend in construction cost and also similar levels of construction costs outside Dublin. While this series will reflect, for example, any additional costs of improved insulation standards over time, it also suffers from a fixed quantity, given that it is based on per-square-metre costs.

One important adjustment is needed to the series on gross construction costs. For much of the period, various forms of "Section 23" tax reliefs were available with respect to construction costs. In particular, in the decade to July 2008, when all schemes were closed, three principal schemes operated: rural renewal (from June 1998), integrated area urban renewal (from August 1998), and town renewal (from April 2000). As outlined in Revenue Commissioners of Ireland (2020), these schemes were extremely broad and deep. In breadth, the urban schemes applied to 52 different areas in 23 counties, the rural scheme applied to the majority of five north-western counties (including two counties in full), while the town scheme applied to 100 further towns in 23 counties. It was deep, in the sense that all construction costs were eligible to full tax relief, provided the property was rented upon completion, with a minimum qualifying lease of three months. Crucially, the tax relief was not limited to the property itself and reliefs could be set off against any rental income from properties located in Ireland (whether covered by Section 23 or not) in that year or carried forward to future years. In effect, construction costs, net of tax reliefs, were dramatically reduced in almost all parts of the country during the period 1998-2008, even as gross costs rose dramatically.[8]

[8]Indeed, the two phenomenon are likely related. In June 1999, as the Section 23 scheme took full effect, the Chief Executive of the Society of Chartered Surveyors stated that "to get a builder at the moment is very difficult . . . There has obviously been an increase in building costs, as yet

Based on media coverage during the time of the reliefs, we parameterize these reliefs as a 60% discount on construction costs for the period 1998Q3 to 2008Q2; our results are not sensitive to increasing this relatively conservative estimate of the discount associated with Section 23.

The second element of the cost of new homes relates to land (or site) costs. Economic theory predicts that land costs for any given site will be endogenous to the likely price (and build costs) and, in particular, where build costs are determined at a larger scale, such as the unit of the city or wider economy, and prices are determined locally, site values will, in equilibrium, be the difference between these two series. Given this endogeneity, we use lagged median residential site costs, at regional level, where available using micro-data assembled from the daft.ie archive. For five parts of the country – Dublin, its four commuter counties, the rest of the Leinster province, Munster, and the combination of Connacht and the three Ulster counties – annual median site costs were available for the period 2004-2021. For Leinster (outside the greater Dublin area), typical site costs rose from €100,000 in 2004 to €150,000 in 2007, before falling to €55,000 in 2013-2014 and rising to €65,000 by 2020. For the county-level quarterly panel analysis, counties were assigned to one of five regions and annual values were interpolated to quarterly. For the period before 2004, a national series for average agricultural land values, per acre, was used, as contained in Daly **and** Morgan (2022).

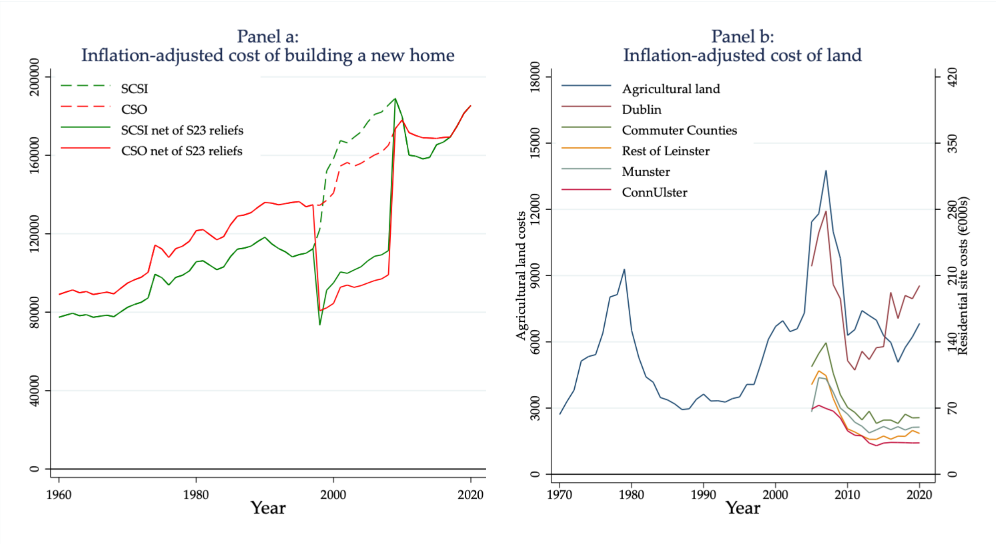
Figure 3.3 presents both elements of housing costs, since 1970. The left-hand panel presents the overall estimate of the cost of building a three-bedroom semi-detached house, by year, using both cost series and accounting for tax reliefs 1998-2008. The right-hand panel presents two alternate measures of site costs: the typical residential site cost, by region, on the left-hand axis and the average per-acre cost of agricultural land, at national level, on the right-hand axis.

3.3.4 Mortgage Market Conditions

We measure mortgage market conditions in three complementary ways. Our preferred measure is the typical loan-to-value (LTV) of first-time buyers (FTBs). As described by Duca **andothers** (2011), this measure captures the injection of new credit into the housing market, as distinct from, for example, the release of existing equity enjoyed by those who already own housing. This is available at quarterly frequency from 2000Q1 and was provided by the Central Bank of Ireland

I couldn't put figures on it but there will be a substantial increase" (Tanney, 1999). The SCSI series of rebuilding costs for Dublin rose 43% between 1998 and 2000, from €90 per square foot to nearly €130, with the period 1997-2001 accounting for half of the €140 increase in per-square-foot rebuilding costs observed 1989-2021.

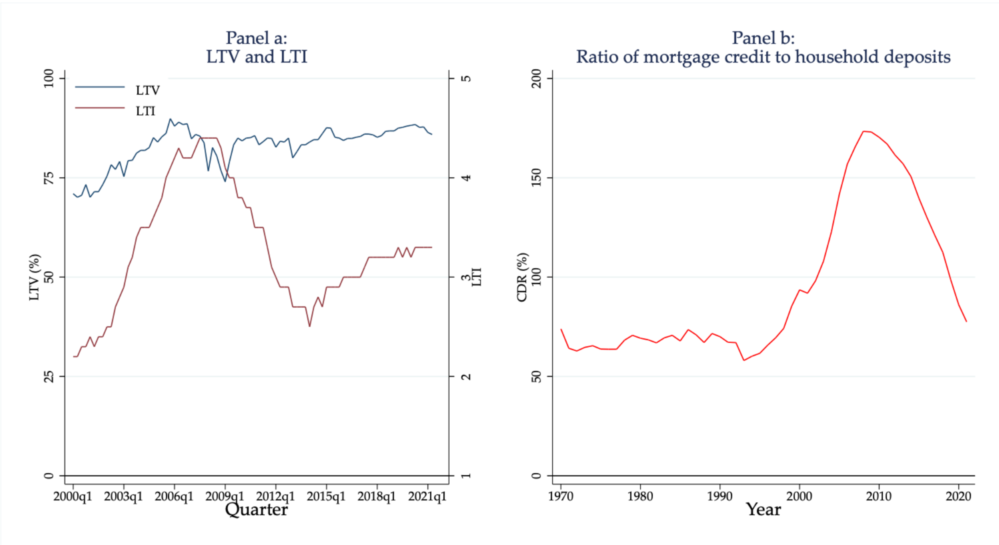
Figure 3.3: Measures of housing costs, since 1970



for the country as a whole, for Dublin, and for the rest of the country, based on underlying loan-level data for financial institutions governed by its supervision rules from 2010, as per Lyons (2018). The same source also provided median loan-to-income (LTI) over the same span and for the same three spatial units. For the county-level panel, loan-to-income is preferred, as this better reflects higher price constraints in the capital: for example, in 2017, the typical deposit of a first-time buyer in Dublin was larger, rather than smaller, than those elsewhere in the country (16 percent rather than 13 percent), although the typical loan to income was also higher (3.5 vs 3.0). The greater deposit may reflect selection into the Dublin market of those with greater ability to rely on gifts or bequests, while the loan-to-income series better reflects the greater credit needed, given earning capacity (which may still be greater for those in the Dublin market).

These data are only available from 2000. Before this, we use data on the system-wide ratio of mortgage credit to household deposits, or credit-deposit ratio (CDR), following Lyons **and** Muellbauer (2015). This captures, at an aggregate level, the changes in non-price credit conditions in the mortgage market, relative to the domestic stock of savings. In particular, as shown in Figure 3.4, it captures the dramatic easing of credit conditions that took place in Ireland between the mid-1990s and the mid-2000s, when the ratio of mortgage credit to deposits rose from approximately 60% to 176%. This reflects the greater extent to which mortgage-issuing institutions increasingly relied on external sources of finance during the so-called "Great Moderation" that preceded the "Great Recession". This series becomes less reliable during the 2010s, as Irish-based mortgage-issuing institutions securitized large portions of their loan books, although these changes are, to the greatest extent possible, accounted for in the series used. More fundamentally, this is a stock measure and – following a dramatic increase in mortgage lending – further and more moderate easing of credit conditions may be less obvious than in the FTB series described above.

Figure 3.4: Credit conditions



3.3.5 Other series

Deflators All euro-denominated series – supply as measured by FCF, housing prices, and build and site costs – are converted into 2019 euro using the personal consumption expenditure deflator (PCE). Specifically, for the period 1995-2020, CSO Tables N2005 and N2006, corresponding to ESA Code P.3 (personal consumption of goods and services), were used to give nominal and real (2019) euro totals. These were extended back to 1970 using CSO Tables NAH05 and NAH06, giving an annual series for the PCE deflator, which increased from 7.42 in 1970 to 100.0 in 2019.[9] For quarterly analysis, the PCE deflator was interpolated from annual series.

Other Despite the nature of the error-correction method, including its one-step formulation, the potential exists for either reverse causality or omitted variable bias, with *inter alia* supply (including new supply) a determinant of price, as well as price determining new supply. To examine this, further data series are needed, in particular to verify the validity of the results from ECM, using

[9]In further robustness checks, not shown below, four alternative deflators were used: GDP, GNI*, GFCF and the headline CPI measure. These did not materially affect the results, nor did they improve the model fit.

Seemingly Unrelated Regressions (SUR) and Instrumental Variable (IV) methods. This includes estimates of population and number of households, which are interpolated from Census frequency where necessary, as well as county-level estimates of household income. These, and the mortgage rate, are sourced from the CSO.

3.4 Empirical Strategy

As described in Section 3.1, the model used in this analysis is given by the typical formulation for the long run supply equation, as a function of prices and costs:

$$y_i = \beta_1 Prices_t + \beta_2 Costs_t + \epsilon_t \tag{3.1}$$

We estimate this supply equation using an error correction framework by applying several estimators that take account of cross-sectional dependence in the data. The baseline error correction equation estimated is the following one-step formulation augmented by lagged terms using OLS:

$$\begin{aligned} \Delta y_t &= \alpha y_{t-1} + \phi_1 Prices_{t-1} + \phi_2 Costs_{t-1} \\ &+ \sum_{i=0}^{n-1} \beta_i \Delta Prices_{t-i} + \sum_{i=0}^{n-1} \gamma i \Delta Costs_{t-i} + \epsilon_t \end{aligned} \tag{3.2}$$

Where α is the speed of adjustment, and ϕ_1 and ϕ_2 give the long run relationship. The number of lags has been informed by the AIC criteria. We choose five lags for prices and one lag for costs, consistent with the process for building new homes, where prices inform the decision to undertake a new project, at which point costs are realized.

In addition, the analysis at country level must reflect concerns about endogeneity and serial correlation. We address these concerns using three specifications for Equation 3.2: firstly, we estimate it using an auto-regressive structure; secondly, using the non-parametric Fully Modified Ordinary Least Squares (FMOLS), as proposed by Phillips **and** Hansen (1990); and thirdly Dynamic Ordinary Least Squares (DOLS), as proposed by Stock **and** Watson (1993). FMOLS accounts for serial-correlation by employing a non-parametric correction using the error term u_t and the first differences of the regressors Δx_t. DOLS is a parametric correction that considers leads and lags of the dependent variable; we can be confident about asymptotic efficiency since in all our specifications we are above the proposed threshold of 60 for FMOLS and DOLS (Banerjee, 1999).

The estimation framework is then extended to the panel setting. As argued by Pedroni (2000), a standard strict exogeneity assumption of regressors in fixed-effects panel OLS does not hold for cointegrated panels because the absence of any dynamic feedback from the regressors at all frequencies is very unlikely. Thus, the OLS estimator is asymptotically biased. To overcome this challenge, we first use the panel mean group (PMG) estimator (Pesaran **andothers**, 1999) which allows the short-run coefficients and error variance to vary across groups, while the long-run coefficients are consatrined to be the same. We further apply panel FMOLS (Pedroni, 2000) and panel DOLS (Kao **and** Chiang, 2000).

In this section, we undertake two parts of our empirical analysis (tests for unit roots and cointegration), as well as tests for panel co-integration as a specification check for the estimated long-run equation and an investigation of the extent of spatial heterogeneity in the long-run parameters. For the variables in the time series and panel environments, we firstly test for unit roots and secondly check the order of integration. Throughout, we allow housing supply to be a function of prices (and costs) in both stocks and flows. Specifically, we examine whether the stock of dwellings (in absolute terms or per capita) or the flow of dwellings responds to prices, using each of the four measures of new housing supply. As there is cointegration exists only between prices and flows of new housing, not between prices and housing stocks, this is discussed in the final part of this section.

Unit root First, we test for the presence of a unit root in our series, employing both time series and panel unit root tests. Using a conventional Augmented Dickey–Fuller (ADF) test, we cannot reject the null of a unit root for our variables in levels, except for housing stock (absolute or per capita). However, all variables are stationary in first differences. These results are robust to the inclusion of linear time trends; more information is given in Appendix 3.C.

Next, we test for a unit root in our panel data set, for which we employ a test developed by Im **andothers** (2003) that averages the ADF statistics across all panel units. The null hypothesis is that each series contains a unit root, whereas the alternative hypothesis is that a proportion of the time series are stationary; thus, rejecting the null hypothesis may only indicate that several of the panel units are stationary. The result is similar to the national series: all our variables are stationary in first differences but not in levels, which is robust to trends. Thus, in order to employ an error correction framework, we proceed by employing appropriate co-integration tests.

Cointegration Next, we perform Engle **and** Granger (1987) (EG) tests for co-integration using Equation 3.1, to assess the validity of our error correction strategy using the long run relationship given. These tests can be performed with or without trends; as above, the series without trends is our baseline. In Table 3.1, we report the results from an EG test using MacKinnon (2010) critical values. In particular without trends, these tests confirm that flow measures of supply – permissions, investment and completions – are co-integrated with prices and costs, while the two related stock measures are not.

Table 3.1: Engle-Granger co-integration test

(Panel A: National)		
	No trend	Trend
Permissions	–37.051*	–36.36
Investment	–36.406*	–38.191
Completions	–38.544**	–37.981
Housing stock HH	12.365	10.318
Housing stock	–24.568	4.023
(Panel B: Dublin)		
Permissions	–5.133***	–5.162***
Completions	–2.316	–1.973

Note. Results from an Engle & Granger cointegration test (Engle **and** Granger, 1987). Critical values are given by MacKinnon (2010). All tests statistics follow the standard normal distribution under the null.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

To test for co-integration in our panel setting, we use the method developed by Pedroni (1999). The test constructs seven diagnostics to test the null of no co-integration: the panel v-statistic, the panel ρ-statistic, the panel t-statistics, the group ρ-statistic and group t-statistics. The group-mean statistics average the results of individual county test statistics, and the 'panel' statistics pool the statistics along the within-dimension. Within both groups, non-parametric (ρ and pp) and parametric (ADF, as well as panel v) statistics are constructed.

Table 3.2: Pedroni Co-integration Test

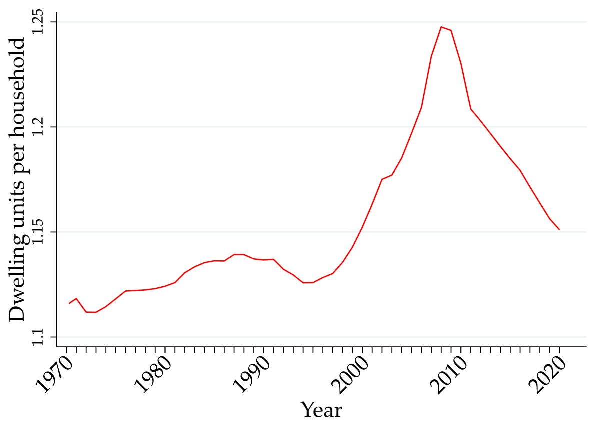
Test Stats.	v	rho	t	adf	group rho	group t	group adf
Permissions	5.221***	-4.021***	-3.465***	-3.631***	-2.509**	-2.949***	-3.841***
Permissions One Offs	8.347***	-30.2***	-21.5***	-13.71***	-30.73***	-25.66***	-14.44***
Commencements	1.559	0.4091	0.03097	0.355	1.472	0.7753	1.26
Completions	-2.796***	-0.3894	-1.265	-0.9473	1.864**	0.02431	0.2914

Note. Results from applying the Pedroni Panel Co-integtaion test using the long run Equation 3.1 for the four outcome variables. Critical values from Pedroni (1999).
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

The results in Table 3.2 clearly show that permissions (overall and of one-off dwellings) are strongly cointegrated with prices and costs. For completions, two test statistics out of seven statistics lead to reject the null, while commencements are not cointegrated.

Why is the stock of housing not cointegrated? The result that housing stocks are not cointegrated with housing prices and costs touches on a broader discussion. As outlined in Mayer **and** Somerville (2000), a long-run equation reflects levels in prices and stock, with changes the result of transitioning from one equilibrium to another. This implies that the within-city housing stock should be co-integrated in a supply equation. Our results indicate that national time series analysis follows, instead, the model of Topel **and** Rosen (1988), similar to Caldera **and** Johansson (2013), who use a similar measure of supply flows in a cross-country analysis.

Figure 3.5: Housing stock per household, since 1970



Ultimately, we do not find any evidence of a cointegrating relationship between the stock of dwellings and our supply shifters, as suggested by the model in Mayer **and** Somerville (2000). This may be due to a violation of the assumption within Mayer-Somerville of fixed consumption of housing and land per person, an assumption that seems implausible over the period we are interested in. Figure 3.5 shows the evolution of number of dwellings per household in Ireland since 1970; note in particular the rise in dwellings per household during the 1990s and 2000s. Conceptually, with housing being a normal good, the sustained increase in incomes over the period would mean greater consumption of housing (both structures and land) per person. An increase in income would be expected to bring about a higher price. In a standard microeconomic setting, any price-cost gap should bring about the additional supply (a flow of new housing) that in turn pushes down prices and restores equilibrium. For a given increase in prices, and assuming no change in costs, a setting where supply is not perfectly elastic means that this price-cost gap may persist over time.

3.5 Results

3.5.1 Quarterly time series

We begin by reporting the regression results where the measure of housing supply is given by the number of units for which planning permission is granted, at quarterly frequency. As described above, this measure of supply is the one most closely connected, theoretically, to changes in prices.

The results are given in Table 3.3 for the four methods of estimation outlined above and for both the National and Dublin datasets. Across three of the specifications (OLS, FMOLS and DOLS), for the national dataset, which extends back to 1975, the principal results are similar. This includes the speed of adjustment from short-run to long-run equilibrium (close to 0.14 per quarter), the elasticity of supply with respect to housing prices (between 0.74 and 1.2), and for the elasticity of supply with respect to housing costs (-1.7 and -2.7). For Dublin (the right-hand panel), the elasticity of supply to prices is larger, while the speed of adjustment is significantly faster. While the estimated elasticities using permits data are greater for Dublin than for the national dataset, the Dublin data on permissions granted only extends back to the late 1990s rather than to the mid-1970s. This suggests that the difference may stem either from time-varying elasticities or from elasticities that differ by location within Ireland: both potential explanations are explored further below.

Given the extent to which more technical specifications support the broad pattern of results for OLS, we report OLS results are our baseline. These suggest that, for Ireland overall since the 1970s, a quarterly speed of adjustment of -0.14 and, for units for which planning is granted, elasticities with respect to prices and costs of +0.9 and -1.9 respectively.

Table 3.3: Error-correction results, based on units for which permits are granted

	National				Dublin			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS-ARDL	FMOLS	PDOLS	OLS	OLS-ARDL	FMOLS	PDOLS
Prices	0.890***	0.673	0.739***	1.221***	2.438***	2.692**	2.897***	2.207**
	0.060	0.598	0.039	0.149	0.354	0.845	0.290	0.678
Costs	-1.929***	-1.203	-1.694***	-2.703***	-1.695***	-1.536*	-1.907***	-1.659**
	0.145	1.324	0.085	0.326	0.272	0.681	0.229	0.551
SOA	-0.144**	-0.068	-0.115***	-0.140**[a]	-0.442***	-0.329**	-0.232**	-0.441***[a]
	0.043	0.035	0.025	0.043	0.085	0.110	0.074	0.084
Adj. R2	0.109	0.200	0.033	0.685	0.310	0.357	0.215	0.466
RMSE	0.200	0.189	0.334	0.404	0.513	0.503	0.629	0.624
Obs.	187.000	184.000	186.000	185.000	89.000	86.000	88.000	87.000

Note. Columns (1) - (4) report results for then national quarterly and Columns (5) - (8) for the Dublin series. Columns (1) - (3) report results from estimating equation 3.2. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results. Column (4) has been estimated using a two step approach with one lead and one lag in the long run DOLS equation. Robust errors are reported in Columns (1) and (3) - (4). Column (2) standard errors are obtained using the Delta method. The same logic applies to Columns (5) - (8).

a Estimate from the second step OLS estimation using five lags for prices and one for costs.

Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Tables 3.4 and 3.5 provide similar information, but where the outcome of interest is, respectively, fixed capital formation in dwellings and the number of dwellings completed, again nationally and at quarterly frequency since the early 1970s. For completions, the broad pattern of results is similar to those for permissions, including speed of adjustment (0.14 per quarter in OLS), price elasticity (+0.65) and the cost elasticity (-2.4). For investment, the speed of adjustment is slower (0.06 per quarter), while the estimated elasticities for prices and costs are closer together in absolute size (+1.4 for prices; -1.2 for costs). The right-hand panel of Table 3.5 presents results for Dublin only. Unlike for permits, there is no clear difference in estimated elasticities.

Table 3.4: Error-correction results, based on fixed capital formation in housing

	(1) OLS	(2) OLS-ARDL	(3) FMOLS	(4) PDOLS
Prices	1.406***	1.316***	1.470***	1.828***
	0.047	0.359	0.032	0.170
Costs	-1.224***	-1.363*	-0.956***	-1.398***
	0.052	0.581	0.033	0.356
SOA	-0.058*	-0.050*	-0.026	-0.056*[a]
	0.027	0.022	0.017	0.027
Adj. R2	0.221	0.261	0.001	0.900
RMSE	0.080	0.077	0.117	0.251
Obs.	202.000	204.000	201.000	205.000

Note. Columns (1) - (3) report results from estimating equation 3.2. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results. Column (4) has been estimated using a two step approach with one lead and one lag in the long run DOLS equation. Robust errors are reported in Columns (1) and (3) - (4). Column (2) standard errors are obtained using the Delta method.

a Estimate from the second step OLS estimation using five lags for prices and one for costs.

Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Table 3.5: Error-correction results, based on units completed

	National				Dublin			
	(1) OLS	(2) OLS-ARDL	(3) FMOLS	(4) PDOLS	(5) OLS	(6) OLS	(7) FMOLS	(8) PDOLS
Prices	0.648***	0.340	0.354***	1.001***	0.795***	1.511	1.025***	0.925
	0.044	0.357	0.035	0.250	0.056	1.054	0.056	0.501
Costs	-2.406***	-2.532***	-1.974***	-2.738***	-2.955***	-3.004*	-2.362***	-3.073***
	0.140	0.645	0.089	0.523	0.190	1.438	0.106	0.722
SOA	-0.139***	-0.096***	-0.055*	-0.137***[a]	-0.090	-0.078*	-0.039	-0.089[a]
	0.038	0.026	0.024	0.038	0.047	0.035	0.024	0.046
Adj. R2	0.140	0.381	0.103	0.615	0.044	0.141	-0.000	0.494
RMSE	0.192	0.162	0.244	0.435	0.273	0.260	0.376	0.711
Obs.	202.000	204.000	201.000	205.000	109.000	106.000	108.000	107.000

Note. Columns (1) - (4) report results for then national quarterly and Columns (5) - (8) for the Dublin series. Columns (1) - (3) report results from estimating equation 3.2. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results. Column (4) has been estimated using a two step approach with one lead and one lag in the long run DOLS equation. Robust errors are reported in Columns (1) and (3) - (4). Column (2) standard errors are obtained using the Delta method. The same logic applies to Columns (5) - (8).

a Estimate from the second step OLS estimation using five lags for prices and one for costs.

Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Table 3.6: Housing stock per household

	(1) OLS	(2) ARDL	(3) FMOLS	(4) PDOLS
Prices	0.351***	0.082***	0.250***	0.055***
	0.000	0.020	0.000	0.009
Costs	-1.015***	-0.108	-0.691***	-0.002
	0.000	0.056	0.001	0.020
SOA	-0.005	-0.007**	-0.008	0.002[a]
	0.005	0.002	0.007	0.008
Adj. R2	0.621	0.890	0.153	0.766
RMSE	0.001	0.001	0.002	0.015
Obs.	202.000	203.000	201.000	204.000

Note. Columns (1) - (3) report results from estimating equation 3.2. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results. Column (4) has been estimated using a two step approach with one lead and one lag in the long run DOLS equation. Robust errors are reported in Columns (1) and (3) - (4). Column (2) standard errors are obtained using the Delta method.
a Estimate from the second step OLS estimation using five lags for prices and one for costs.

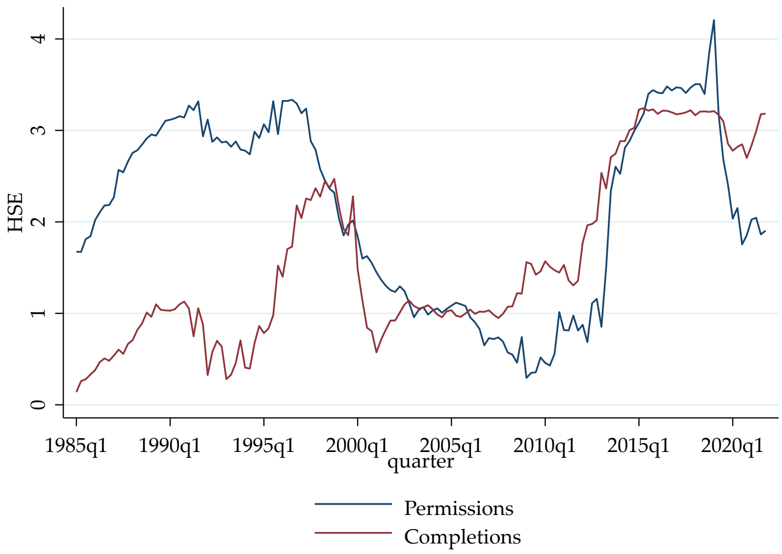
For contrast, the results of a specification where the outcome of interest is housing stock per household, a stock measure rather than a flow, is given in Table 3.6, although as noted above there is very limited evidence of a cointegrating relationship. What is striking is the lack of any systematic adjustment in the short-run to the long-run relationship. Related, the estimated elasticities are often either not statistically significant or very small in magnitude. This supports the discussion earlier that housing supply in Ireland is best modelled as flows, rather than stocks.

As noted above, the estimates for the elasticity of supply differ in the Dublin set of specifications, compared to the national aggregate. This may be due to regional differences in the responsiveness of supply or to the period covered: while national permissions are available at quarterly frequency from 1975, Dublin permissions are only available from 1999. We begin by examining the hypothesis that the responsiveness of supply may change over time. Figure 3.6 presents the estimated elasticity of housing supply to prices (HSE) using rolling 40-quarter (10-year) samples, starting 1975-1985 and and finishing 2012-2022. The 40-quarter basis is designed to allow the elasticity to vary over time while avoiding excessive swings in the underlying estimates of long-run elasticities.[10]

Using both permits and completions, there is evidence of an increase in

[10] Estimates of HSE based on 40-quarter samples using investment and housing stock per household are not shown, due to the volatility of the results, in particular for housing stock where signs of a cointegrating relationship are very weak.

Figure 3.6: Estimates of long-run HSE, from 10-year quarterly samples by rolling window and measure of supply, since 1975



responsiveness to price changes as the sample end-date moves from the mid-1980s to the mid/late-1990s. Throughout this period, the estimated elasticity of permits is greater than of completions, settling at 3 for permits in the 1990s and peaking just above 2 in the late 1990s for completions. In both cases, the responsiveness of supply weakens considerably, such that by 2010, the estimated elasticity of permits is approximately 0.5, while that of completions bottoms out at close to 1. A third phase of responsiveness is evident towards the end of the period, with both measures implying a new peak in estimated elasticities, above 3, in the late 2010s.

3.5.2 Two-stage Least Squares

One concern with interpreting the results presented above as estimates of causal effects relates to endogeneity: housing supply may interact in a nuanced way with other housing-related series. Thus, to further validate our estimates, we start by relaxing the assumption that, after controlling for construction costs, all variations in prices would result from demand shocks. Specifically, we use an instrumental variable (IV) strategy that leverages a demand function to address concerns of endogeneity between housing prices and our outcomes of interest (permissions, investment, completions). The literature on housing supply elasticities uses demand-driving factors, such as income and crime (Accetturo **andothers**, 2020), or market access (Baum-Snow **and** Han, 2022) as IVs. We follow a similar argument by using a demand equation, within a system of demand and supply equations, as our starting point.

More formally, consider the following system of two equations:

$$Prices_t = \gamma_1 Income_t + \gamma_2 HHS_t + \gamma_3 LTV_t + \rho_t \tag{3.3}$$

$$y_t = \beta_1 Prices_t + \beta_2 Costs_t + \epsilon_t \tag{3.4}$$

where Equation 3.3 is our demand equation, where housing prices depends on Inc_t (disposable household income), average household size HHS_t (the ratio of the population to households, to capture demographics) and LTV_t, which measures credit conditions in the mortgage market, through the Loan-to-Value ratio. Plugging (3.3) into (3.4) gives rise to the following reduced form equation:

$$y_t = \beta_1\gamma_1 Income_t + \beta_1\gamma_2 HHS_t + \beta_1\gamma_3 LTV_t + \beta_2 Costs_t + (\beta_1\rho_t + \epsilon_t) \tag{3.5}$$

In our strategy, we use all determinants of demand from Equation (3.3) as instruments for housing prices in Equation 3.4. However, while more instruments can improve efficiency, our model is over-identified as the number of instruments is larger than the number of endogenous regressors. To validate this strategy, all instrumental variables must be orthogonal to omitted supply factors. Where Z is the set of instruments, then the traditional IV conditions for all $z \in Z$ need to be satisfied:

$$Cov(Z_{it}, Prices_t) \neq 0 \qquad \forall\, i \tag{3.6}$$

$$Cov(Z_{it}, \rho_t) = 0 \qquad \forall\, i \tag{3.7}$$

Equation 3.6 is, thus, the relevance condition, stating that the external instrument Z_i must be contemporaneously correlated with local house prices. We build on a long literature in standard macro and housing models that has established that credit conditions, income, and demographics are principal determinants of housing and consumption demand but typically do not affect housing supply directly (Glaeser, Gyourko **and** Saks, 2005; Duca **andothers**, 2011). The second condition in Equation 3.7 – the exogeneity condition – requires the instrument to be not contemporaneously correlated with the omitted supply factors in Equation (3.4).

To validate our IV strategy, in Table 3.7 we report the first-stage (Column [1]) and reduced form estimates (Columns [2-5]). F-test and robust F-test statistics are between 30 and 50, which is significantly above the threshold value in Stock **and** Yogo (2005), suggesting that our instruments are strong. We obtain a very high

χ^2 from the Sargan-test, which rules out issues due to instruments that might fail the condition given in Equation (3.7).

All three demand coefficients accord with theory. The coefficient on household size is statistically significant at conventional levels and positive: more people per dwelling is strongly associated with higher housing prices. Similarly, a one percent increase in disposable household income per person leads to an increase of 1.7 percent in house prices. Our estimate for credit conditions indicates that looser conditions (a higher loan-to-value ratio) increase house prices by nearly the same magnitude (1.5 percent). Turning to the reduced form estimates in columns (2) to (5), the coefficients in columns (2) to (4) align well with general theory. Household size, income, and LTV are strongly and significantly correlated. Lastly, in Column (5), results are given where the outcome of interest is the housing stock; however, in this model, four of five coefficients display signs not aligned with theory, while the fifth (income) has a coefficient that is an order of magnitude smaller than our preferred supply measures.

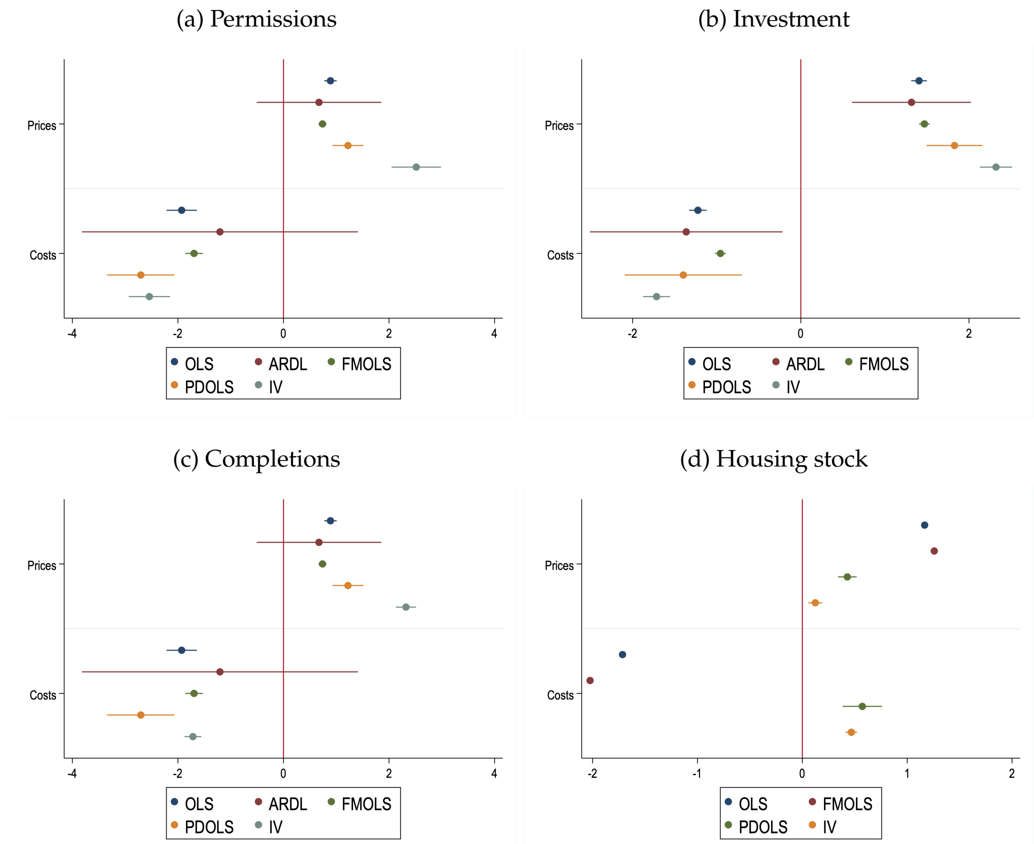
Table 3.7: Validation of IV estimation

	(1) First	(2) Permits	(3) Invest.	(4) Completions	(5) Stock
HHS	9.759***	41.84***	28.00***	41.04***	-2.125***
	(13.05)	(13.18)	(13.60)	(15.42)	(-13.16)
Income	1.780***	3.960***	3.753***	4.366***	0.405***
	(14.98)	(6.74)	(6.81)	(6.55)	(8.37)
LTV	1.574***	4.501**	4.555***	6.363***	-0.201*
	(5.53)	(3.22)	(3.93)	(4.89)	(-2.63)
Costs	-0.0496	-0.842**	-1.284***	-1.747***	0.116***
	(-0.70)	(-2.80)	(-5.49)	(-5.95)	(5.17)
_cons	-21.83***	-88.98***	-72.19***	-96.46***	12.98***
	(-11.41)	(-11.22)	(-13.84)	(-14.31)	(29.30)
CD Wald F	181.8				
SW S stat.	56.26				
Sargan	48.27				
F		77.02	106.3	113.7	277.2
ARW chi2		248.0	342.5	366.1	892.9

Note. Column (1) plots results from a first-stage estimation using Equation 3.3. The F-test and robust F-test assume that under the null, the excluded instruments are not weakly correlated with the endogenous regressors. Columns (2) to (5) show results from estimating the reduced form Equation 3.5. The constant is not reported. Robust heteroskedastic standard errors are shown in parentheses.

We plot the 2SLS estimates in Figure 3.7 and compare them to the estimates obtained using OLS, FOMLS, and PDOLS. Our cost elasticity mirrors closely the results obtained from OLS, FOMLS and DOLS. In particular, in Panels ((a)) and ((b)), the estimator is closely related to the PDOLS estimator. Price elasticities under 2SLS are larger in Panels (a) to (c), compared to other estimates. Lastly, 2SLS corroborates the notion that housing stock and prices do not have a long-run equilibrium relationship; the estimated elasticity is below unity while it implies construction costs are positively related to housing stock.

Figure 3.7: 2SLS estimates in lng run equation



Notes: Figure 3.7 compares coefficient estimates from the long-run Equation 3.1 using the national quarterly time series. Thus, estimates are taken from the previous tables. We compare estimates using standard OLS and OLS in an autoregressive distributed lagged model, FMOLS and PDOLS. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results. The green point estimates are 2SLS estimates from estimating Equation 3.5. Each Panel using th same specification with different outcome variables. For a review of th eoutcome variable see Section 3.3. The horizontal bars indicate 95% confidence intervals.

Table 3.8: Permissions: County-level panel

	(1) PMG	(2) FMOLS	(3) PDOLS
Prices	2.319***	2.354***	3.018***
	.089	.081	.093
Built Cost	-1.839***	-1.902***	-1.578***
	.087	.074	.102
SOA	-.609***	-.613***	-.143***[a]
	.029	.013	.023
Adj. R2	"."	.159	.567
Log Like	-1611.637	.	.
N	2106	80	2002

Note. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results.
a Estimate from the second step OLS estimation with county fixed effects using five lags for prices and one for costs.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

3.5.3 Quarterly panel series

Variation in the supply equation over time is important but just one potential dimension along which it can vary. An additional dimension is location. We start our investigation of how the supply equation varies across space with a panel-based analysis, using permissions and housing prices for each of 26 traditional counties in Ireland from 2001. Table 3.8 presents the results across the three specifications described earlier: panel mean group (PMG), panel FMOLS and panel DOLS.

In all cases, there is evidence of adjustment to the long-run equilibrium – and in the case of PMG and FMOLS, very swift adjustment (roughly 60% per quarter). The estimate of responsiveness of permissions to changes in costs is also relatively consistent across the methods: from an elasticity of -1.6 in PDOLS to -1.9 in FMOLS. For prices, county-level data suggest the absolute value of the elasticity is greater than for costs: around +2.3 in the case of PMG and FMOLS and above +3 for PDOLS. Using more detailed geographic information on prices and supply produces estimates of faster adjustment to equilibrium and greater responsiveness to prices.

Running the same analysis but solely on one-off housing (i.e. single-family housing built outside schemes of 2 or more homes) gives a very similar set of results, which are shown in Table 3.9. This include rapid adjustment in PMG and FMOLS specifications, and cost elasticities that are similar to the full permits sample (around -1.5). The estimated price elasticites are, however, about half the size of the full permits sample: between +1.3 and +1.4. This is consistent with one-off housing often being built with less attention to final capital values, as these are often non-traded, with for example adult children building on family land, but where costs are still relevant.[11]

Table 3.9: Permissions One Offs: County-level panel

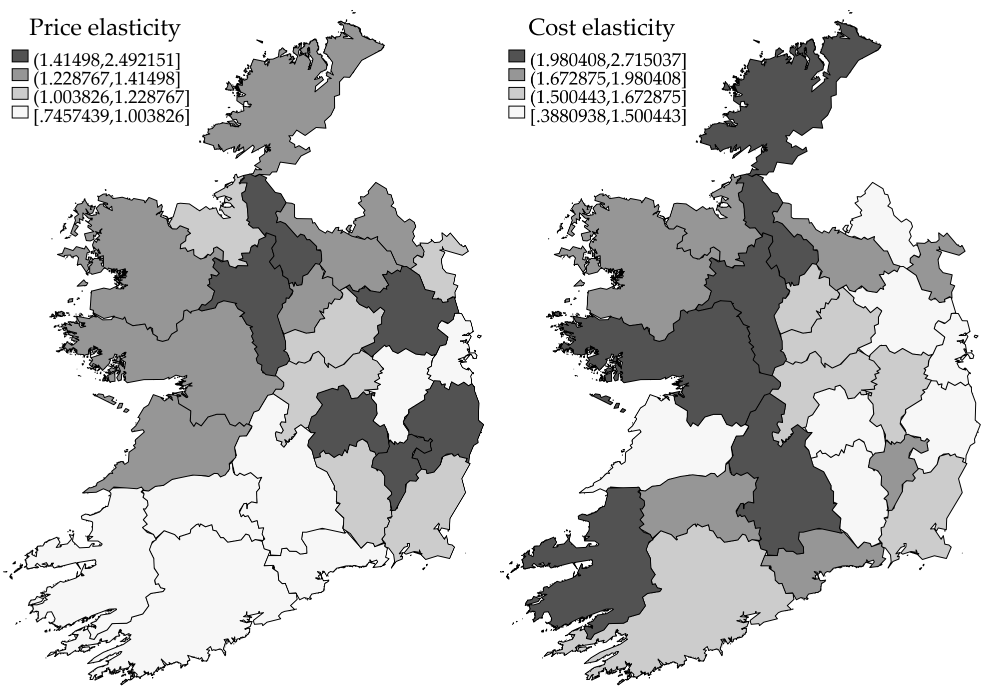
	(1) PMG	(2) FMOLS	(3) PDOLS
Prices	1.272***	1.272***	1.394***
	.049	.039	.057
Costs	-1.492***	-1.663***	-1.641***
	.047	.040	.062
SOA	-.541***	-.554***	-.087***a
	.032	.009	.01
Adj. R2	.	.317	.377
Log Like	-209.671	.	.
N	2106	80	2002

Note. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results.
a Estimate from the second step OLS estimation with county fixed effects using five lags for prices and one for costs.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Figure 3.8 presents the absolute values of the estimated price and cost elasticities of supply, using the permissions measure of supply, for each of the 26 counties using quarterly data from 2001. The estimated price elasticities range from +0.75 to +2.5, with elasticities lowest in Dublin and in the south-west of the country (including Cork, the second largest city). Cost elasticites, shown on the right-hand panel, range in absolute values from 0.4 to 2.7. There is a faint positive correlation between the two elasticities: the north-west county of Leitrim has the highest estimated price and cost elasticties, and Dublin has among the smallest in both. In general, though, there is no obvious link between price and cost elasticities. Two of Dublin's commuter counties (Meath and Wicklow) have weak cost elasticities but higher price elasticities, which is consistent with an overflow of demand from the capital city.

[11] Results for completions and for commencements are shown in the Appendix. In general, results are weaker across both these measures of supply: speed of adjustment is slower, particularly for completions, and in certain cases the estimated coefficients are either not statistically significant and/or not with the sign predicted by theory.

Figure 3.8: Elasticities by county



3.6 Conclusion

The responsiveness of housing supply to demand is a topic of key concern for policymakers in many high-income The responsiveness of housing supply to demand is a topic of key concern for policymakers in many high-income economies. In this paper, we examined the determinants of housing supply in Ireland over the last five decades, using a variety of methods, data series and geographic and temporal scales. Throughout, we found clear evidence not only that housing supply responded to price increases but also that the responsiveness of supply persists to the present. In our baseline, using national data on permits granted at quarterly frequency since the 1970s, and using OLS error-correction methods, the estimated responsiveness of supply to an increase in prices was approximately +0.9, while the elasticity of supply with respect to costs was -1.9. Rolling windows analysis suggests an increase in responsiveness to prices between the 1980s and 1990s, followed by a fall in the 2000s, but with responsiveness recovering to reach a peak in the late 2010s: this pattern is also evident using completions as the measure of supply, albeit with generally lower magnitudes.

What, then, explains the overall lack of new housing supply in Ireland in recent years? The results above reflect conditional elasticities, as is conventional in economics – how supply responds to prices, other factors being equal. The extraordinary tax reliefs on construction costs that applied 1998-2008 appear, however, to have had both short-run and long-run consequences. In the short run, they did indeed have their intended effect, improving viability of new construction and facilitating large increases in housing supply. Almost immediately after their introduction, however, gross construction costs dramatically increased. Despite a small fall during the 2008-2012 crash, build costs – net of any reliefs – were almost twice as high in 2021 as in 2007, while prices were lower.

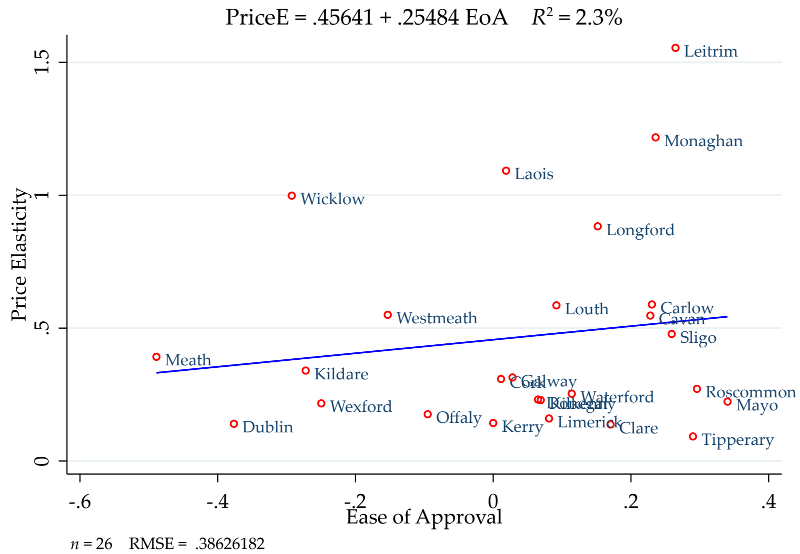
In addition to examining changes in elasticities over time, we also examine spatial variation. While measured price and cost elasticities in Dublin appear to be greater than the national average, this is driven by the shorter availability of Dublin-only series (from 1999, rather than from the 1970s). Indeed, when county-specific estimates of both price and cost elasticities are estimated, Dublin appears to be among the least responsive locations to both prices and costs. This is consistent with the hypothesis that land-use restrictions have dulled the responsiveness of supply in the capital.

This link between elasticity and land-use regulation is a topic of much interest to real estate and urban economists and a promising avenue for future research on the Irish housing market, where the topic is largely unexplored. Figure 3.9 presents a scatterplot of county-specific price elasticities and the estimated ease of approval at county level, taken from **lyons_housing_2015**. There is some evidence of a link, with Dublin and its neighbor Meath having the most restrictive planning systems and lower price elasticities, while Leitrim and Monaghan have among the highest elasticity and greatest ease of approval. Nonetheless, the overall correlation is weak and a number of counties combine relative ease of approval with low estimated price elasticities.

It is important to note the limitations to our analysis. We rely on macroeconometric techniques, in particular error-correction methods, to reveal determinants of housing supply. While we use a number of alternative specifications to examine the solidity of our results, including 2SLS, it remains for future research to embed supply within the broader Irish housing system of equations. National and Dublin time-series results are supported by panel analyses at county levels. An analysis that exploits these spatial differences more explicitly – especially considering site-level permissions data – would shed further light on the determinants of Irish housing supply. A further limitation of this study relates to data: while measures of supply and prices are likely reliable, series capturing build costs are less precise.

Lastly, our findings have implications for public policy. Housing completions in Ireland 2017-2022 averaged just over 20,000 and reached over 30,000 in 2023. While the stated goal of Irish housing policy, in the 2021 *Housing for All* strategy,

Figure 3.9: Price elasticity and Ease-of-Approval (1990-2013)



Notes: Elasticity estimates are the result of individually estimating equation 3.2 for each county for the period 1990 to 2013. Ease-of-Approval values are taken from Lyons (2014) based on a probit regression estimating the probability of a project not being rejected (in other words being approved, conditionally or unconditionally). Ease-of-Approval values are coefficients on local authorities fixed effects indicating the differences across authorities in the likelihood of being approved, holding time constant. Kerry is the control group (coefficient of zero).

was to increase completions to over 30,000 during the 2020s, independent estimates suggest the true housing need to mid-century is likely to be in the range 45,000-60,000 (Horgan-Jones **and** Burns, 2023). The analysis here suggests that, for a 50% increase in supply to happen, prices would need to increase or costs to fall. The baseline estimates here suggest that the elasticity of supply with respect to prices is +0.9 and with respect to costs is -1.9. They imply that, for a 50% increase in construction (+0.41 in log points), an increase in prices of 57% (+0.45 log points) or a fall in costs of 19% (-0.21 log points) would be needed. Assuming housing affordability is a key goal for policymakers, this means that greater cost efficiency should be a priority for housing policy in Ireland over coming years.

Appendix 3.A Data

Table 3.A.1: Ireland Housing Market Dataset

Data	Source	Geographical level	Frequency	Earliest date
Housing supply measures				
Permissions	CSO/Dept. of Env.	National	Quarterly	1974
Permissions	CSO/Dept. of Env.	County	Quarterly	2001
Commencements	CSO	County	Quarterly	2004
Fixed capital formation	CSO	National	Annual	1970
Fixed capital formation	CSO	National	Quarterly	1995
Dwellings completed	CSO/Dept of Envt.	National	Quarterly	1970
Dwellings completed	CSO/Dept of Envt.	County	Quarterly	1994
Housing price measures				
Median price of newly-built homes	CSO/Dept. of Envt.	National/Dublin	Annual	1970
Mix-adjusted price of all dwellings	CSO/ESRI	National/Dublin	Quarterly	1970
Hedonic listed price	Daft.ie	County	Quarterly	1996
Measures of build costs				
Construction cost SCSI	SCSI; CSO pre-1990	National	Quarterly	1970
Construction cost CSO	CSO; SCSI post-2017	National	Quarterly	1970
Construction cost (GB)	ONS	National	Annual	1985

Appendix 3.B Optimal lag length

Table 3.B.1: National quarterly variables

Variable	AIC optimal lag
Investment	6
Housing stock	6
Housing stock HH	2
Permissions	9
Permissions_Dublin	3
Completions	9
Completions_Dublin	12
Prices	5
Prices_Dublin_mix	5
Prices_Dublin_new	5
Build costs	1
Disposable Income	6
Person/HH ratio	2

Note. Table 3.B.1 reports the optimal number of lags by variable using the quarterly national time series. We use the AIC to determine the appropriate number of lags in our time series model; we then select the model with the significant AIC value as the best-fit model; lags are given in Quarters.

Appendix 3.C Unit root tests

Table 3.C.1: Dicky Fuller Test

	No Trend		Trend	
Variable	Levels	1st Δ	Levels	1st Δ
Panel A: National				
Built costs	–1.161	–39.256***	–2.479	–39.158***
Housing stock HH	–1.168	–31.988***	1.585	–31.913***
Housing stock	–3.207**	–31.649***	1.957	–31.57***
Prices	-.262	–36.939***	-.989	–36.847***
Completions	–2.16	–45.318***	–2.208	–45.203***
Investment	–1.457	–41.558***	–1.232	–41.452***
Permissions	–1.962	–35.554***	–1.957	–35.455***
Panel B: Dublin				
Prices	-.516	–37.437***	–1.09	–37.345***
Permissions	–3.603***	–25.379***	–3.695**	–25.225***
Completions	–1.472	–28.153***	–1.668	–28.003***

Notes: ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively. Results from a Dicki-Fuller test against the null hypothesis that there is a unit root. Critical values are given by MacKinnon (2010). All tests statistics follow the standard normal distribution under the null. Panel A uses the quarterly national data set. Panel B uses the quarterly dataset for Dublin only.

Table 3.C.2: IPS Units Root Test

	No Trend		Trend	
Variable	Levels	1st Δ	Levels	1st Δ
Costs	.211	-51.727***	-2.345*	-51.728***
Prices	-8.672***	-17.669***	-4.088***	-18.558***
Commencements	-.737	-24.035***	4.718	-25.296***
Completions	6.274	-20.649***	2.189	-20.676***
Permissions	3.522	-25.097***	3.768	-25.227***
Permissions One Offs	-4.06***	-37.995***	-6.956***	-38.072***

Notes: ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively. Results from an Im–Pesaran-Shin (IPS) panel stationarity test against the null hypothesis that all panels contain unit roots (Im **andothers**, 2003). We use a quarterly panel of the 26 counties of Ireland. We report $\tilde{Z}_{t-bar}$ statistics. Panel A uses the quarterly national data set.

Appendix 3.D Additional tables

Figure 3.D.1: Elasticities by county; permits-based measure

Notes: Figure 3.D.1 reports housing price and cost elasticities at the county level. We obtain elasticities by running county-level quarterly time series from 1989 to 2014. We estimate Equation 3.2 using FMOLS. Short-run prices are lagged by four and costs by one.

Table 3.D.1: Completions: County-level panel

	(1) PMG	(2) FMOLS	(3) PDOLS
Prices	.859**	1.053***	1.649***
	.171	.143	.083
Built Cost	.313	-1.218**	-2.438***
	.324	.248	.084
SOA	-.033***	-.039***	-.018***
	.001	.002	.002
Adj.R2	.	.313	.467
Log Like	2689.991	.	.
N	2522	96	2522

Note. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results.
a Estimate from the second step OLS estimation with county fixed effects using five lags for prices and one for costs.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Table 3.D.2: Commencements: County-level panel

	(1) PMG	(2) FMOLS	(3) PDOLS
Prices	.213	.738*	2.941***
	.463	.299	.107
Built Cost	-.011	-.784	-.698***
	.378	.461	.108
SOA	-.077***	-.084***	-.037***a
	.004	.007	.005
R2	.	.139	.563
Log Like	364.155	.	.
N	1742	66	1638

Note. The selected lag length has been informed by the AIC criteria and is five for prices and one for costs. The FMOLS estimations are based on the Bartlett kernel with Andrews automatic bandwidth selection method. The bandwidth selection method choice does not notably affect the results.
a Estimate from the second step OLS estimation with county fixed effects using five lags for prices and one for costs.
Signif. Codes. ***: 0.01, **: 0.05, *: 0.1.

Conclusion

This thesis presented three chapters exploring the direct and indirect impact of affordable housing policy on urban environments and the responsiveness of housing supply. In this concluding chapter, I summarise their findings and highlight the policy implications they suggest.

This first chapter asked how the construction and expansion of public housing shaped neighborhoods in New York City from 1930 to 2010. Using a staggered difference-in-difference strategy, I found that public housing changed the racial composition of neighborhoods, leading to an influx of whites and an outflow of blacks. I document significant spillover effects, showing a decline in white population in adjacent areas. These effects are driven by "Tower in the Park"-style housing projects, while non-tower projects have significantly lower effects. Additionally, I employed a structural model to quantify the welfare changes associated with these shifts and studied distributional considerations across racial and income groups. The removal of public housing in the model led to significant welfare improvements for both white and black households, driven by lower average rents and reduced segregation. This could indicate that subsequent resorting generates gains for white households, who bid less for specific areas, thereby improving rents for all households. However, these welfare gains were not evenly distributed, with residents within public housing tracts benefiting from removing projects, while those further away benefitted from lower rents due to sorting.

These results carry several policy implications. Firstly, they emphasize the importance of public housing externalities, as welfare gains increased for all households within public housing tracts. This underscores the disparate impacts on welfare and externalities of public housing, highlighting the limitations of policies aimed at revitalizing neighborhoods and benefiting lower-income households. Secondly, the findings suggest that redevelopment can shape the welfare impacts of urban renewal programs, such as public housing demolition. While government-run public housing has often been criticized as inefficient, this paper argues that integrated public housing can provide low-income housing without unintended neighborhood effects. Additionally, mixed-race developments and higher-quality buildings were found to mitigate the negative effects of public housing and benefit the wider area. Finally, efforts to fill large empty green spaces between towers could make places more convenient for different income groups when regenerating public units into mixed-income is costly.

The second chapter examines the effects of the 1920 New York City rent control laws on the city's broader housing market. The evidence presented across various tests indicates that the 1920 rent control laws affected market rents through judge rulings, at least indirectly. Republican judges were found to be more lenient towards landlords than Democrat judges. While a direct link between court rulings and rents could not be established, employing a spatial Regression Discontinuity Design revealed a 10% increase in rents at the border between Republican and Democrat judges. These results are confirmed using an event study design. We propose a mechanism according to which landlords anticipate the costs of lawsuits since they know the partisanship of a judge. Therefore, landlords align with the policy if there is a probability of having a tenant judge. The chapter cannot confirm a similar effect on transaction prices using this methodology. Neither commercial nor residential transaction prices respond to judge types. This result is surprising, at least for residential prices, since rents reflect the landlord's income from residential property. However, rent control might affect landlords' short-term and long-term income expectations differentially.

Finally, this chapter delves into the responsiveness of housing supply to demand in Ireland over the last five decades. Using various methods, data series, and geographic and temporal scales, we uncover clear evidence indicating that housing supply responded to price increases and maintained its responsiveness to the present day. Baseline estimates derived from national data on permits granted since the 1970s reveal an estimated responsiveness of supply to price increases of approximately +0.9, coupled with an elasticity of supply with respect to costs at -1.9. Fluctuations in responsiveness to prices over time were observed through rolling windows analysis, with a notable increase in the late 2010s following a decline in the 2000s. Spatial variation was also explored, particularly in Dublin, where land-use restrictions may have dulled the responsiveness of supply. This chapter underscores the complex interplay between economic factors, policy interventions, and regional dynamics in shaping housing supply trends, offering insights for policymakers and future research endeavors.

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This thesis was typeset using the LaTeX typesetting system created by Leslie Lamport and Donald Knuth, and the memoir class. The body text is set 11pt with Palatino designed by Hermann Zapf, which includes italics and small caps. Other fonts include `Monospace` from Young Ryu's TX Fonts family, Sans from TeX User Group Poland's Latin Modern family, and Helvetica by Max Miedinger and Eduard Hoffmann.