Chapter 2

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

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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 et al., 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 et al., 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 et al., 2011; Arnott and Igarashi, 2000). However, this view is not unchallenged in the literature. Most notably, Jofre-Monseny et al., 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 et al., 2019; Dolls et al., n.d.). 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 et al., 2014) or even attract crime (Autor et al., 2017).

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

[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

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, et al., 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.
Also 3 rooms and bath.
Living room 18 ft. x 26 ft.
19 & 21 West 31st St.
Strictly High-Class Fireproof Apartment

55 West 86th St.
JUST COMPLETED

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

4 ROOMS $65.00
569 WEST 125TH ST.

SEVEN ROOMS
AND TWO BATHS
1109-1111 Madison Ave.
JOHN A. SCHOEN

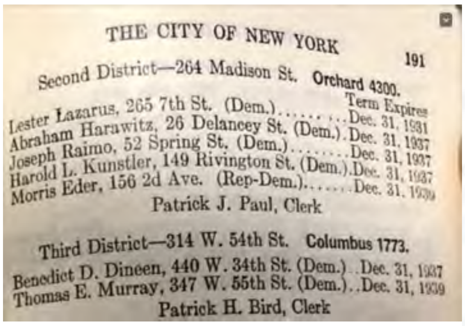
Hendrik Hudson Annex
110th Street & Broadway
The Rockfall
545 West 111th St.
NASSOIT & LANNING

56 ST.—342 WEST
3 ROOMS AND BATH.
APPLY SUPT. ON PREMISES.

690 RIVERSIDE DRIVE,
Rent $2100. Apply on Premises.

Only 2 Apartments Left
Belgrave Block
Madison Ave., 49th to 50th St.
2-3 Rooms—$900 to $1,500
Cruikshank Company
Worthington Whitehouse, Inc.

(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, 1937
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

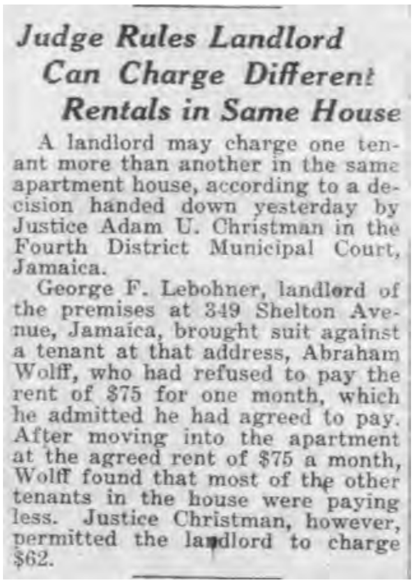
BEEKMAN pl., e. s., 20 s. 50th st., 20x100, h. & l. John Wendel to James D. Sherwood. Oct. 28 38,000

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.

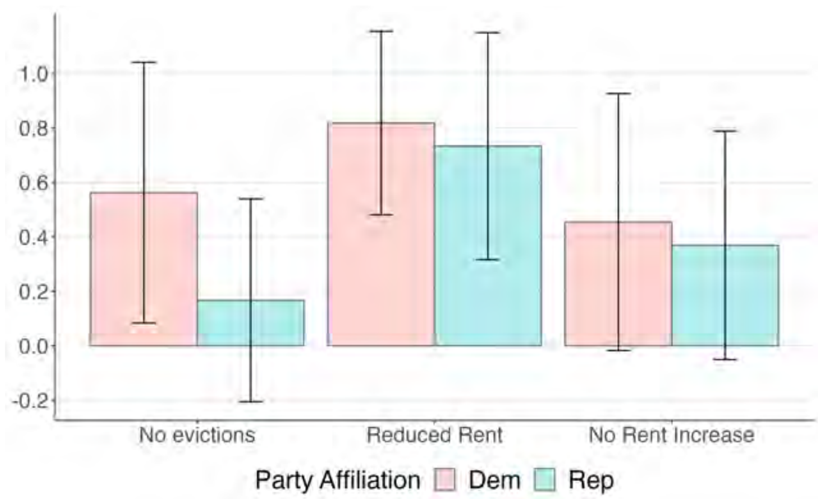
Judges may also have been incentivized to take sides in their rulings for various reasons. As public figures, judges' appearances and opinions were often covered by newspapers at trade unions, dinners, and festivals. Given that judges were elected in partisan elections, they could mobilize voters by 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.

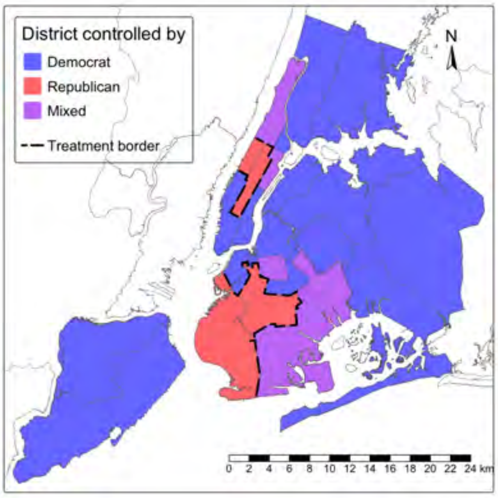
We follow the reasoning in Section 2.4.1 and consider only districts where only Republican or Democrat judges are elected. For each year, we combine all Republican and Democrat-only court districts to exploit the distance to the nearest joint MCD boundary as displayed by Figure 2.3. Thus, for the primary 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

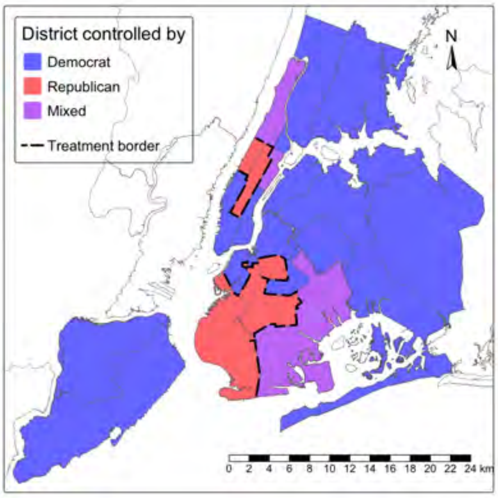
had Republican and Democrat judges. For this exercise, we consider an MCD as treated if the share of republican judges was larger than 50%.

Figure 2.3: Treatment boundary

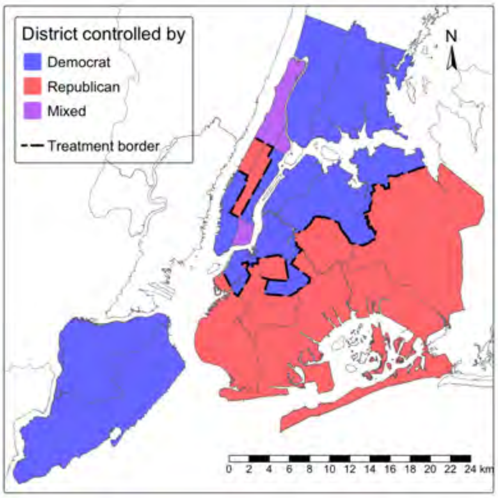
(a) 1918



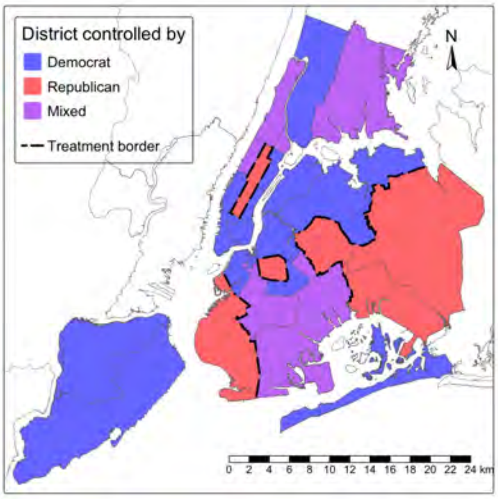
(b) 1919



(c) 1920-1925



(d) 1926



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, $\mathbf{X}$ 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.

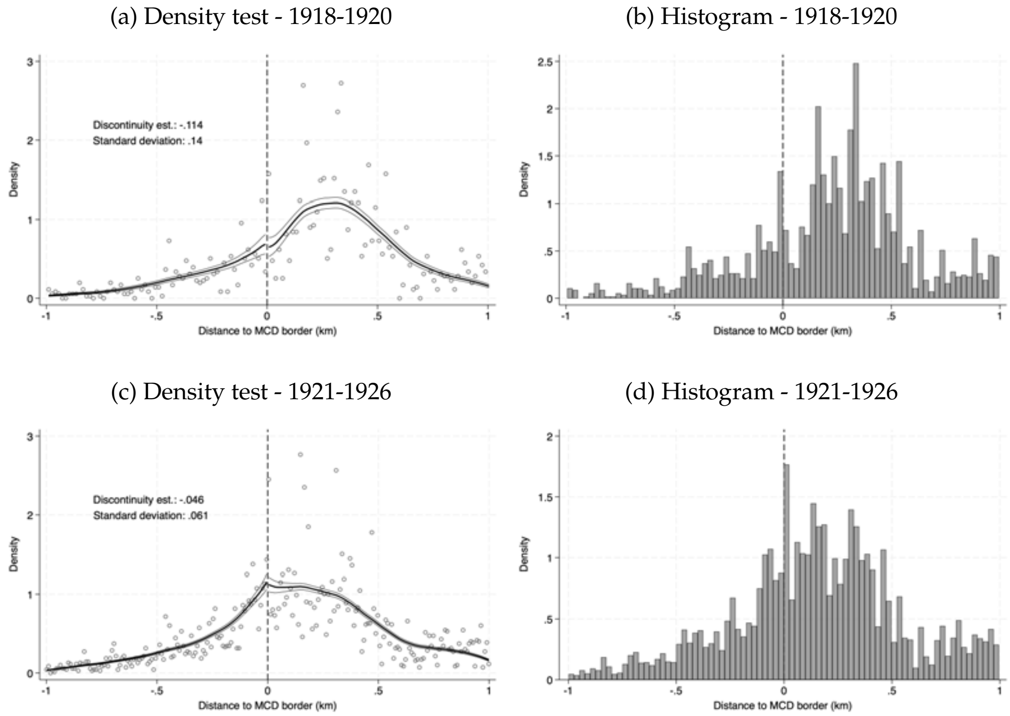
The identification assumption is that there is no change in the density of the running variable at the cutoff. First, we examine whether the density of the forcing variable, the distance to the MCD boundary, is continuous at the discontinuity. Figure Figure 2.4 and Figure 2.5 show histograms of the forcing 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



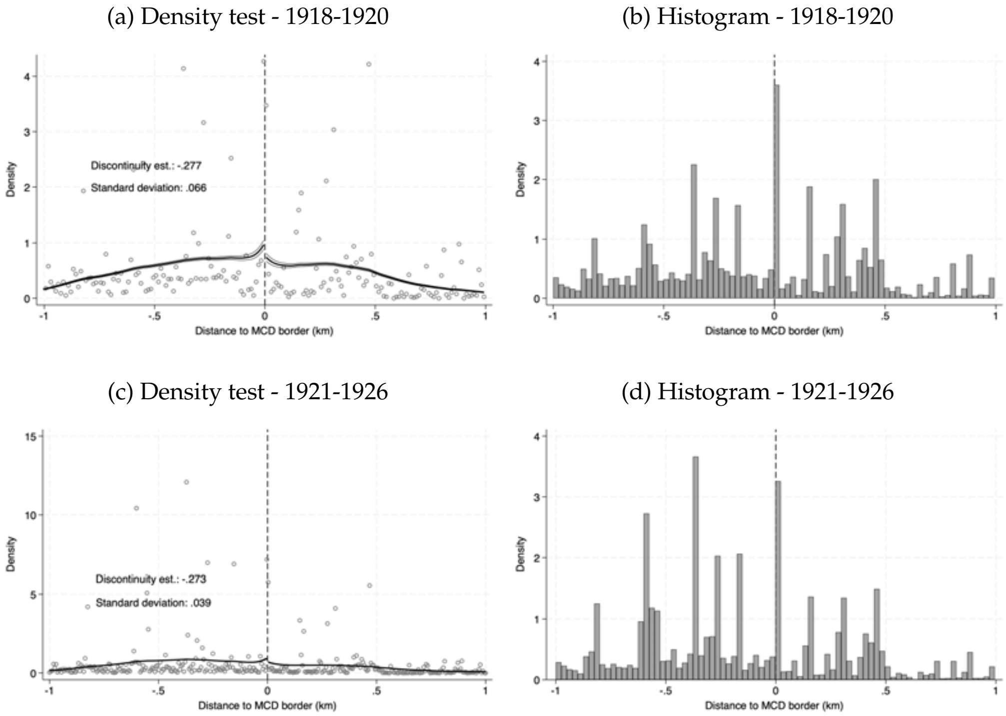
Note. Figure 2.4 presents results from testing if the continuity assumption at the threshold holds. We report tests for the period before and during rent control—panel (b) and (d) the distribution of the running variable. Bins are 12.5 meters in a 1km bandwidth around the cutoff at 0. Panels (a) and (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

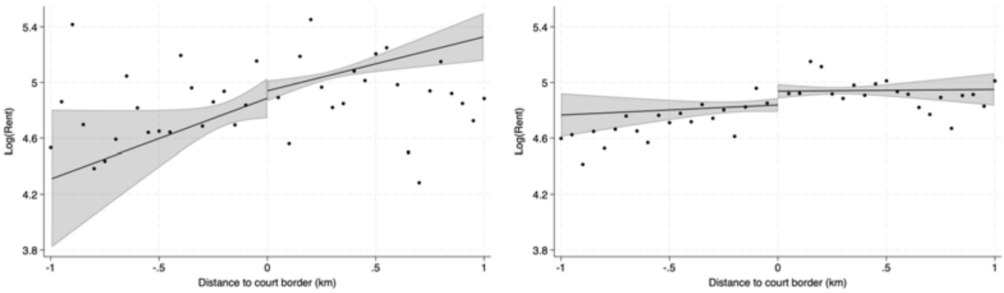
In this section, we first concentrate on market rents and present the results of estimating Equation 2.1. Next, we test wether rent control effected residential and commercial transaction prices.

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.

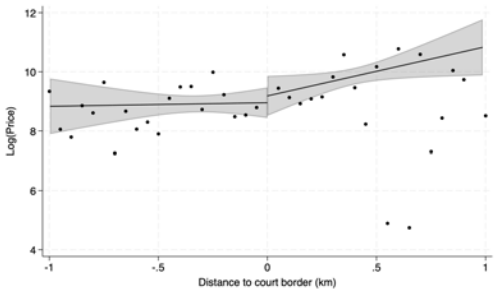
2.5.2 Effect on residential & commercial transaction prices

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.

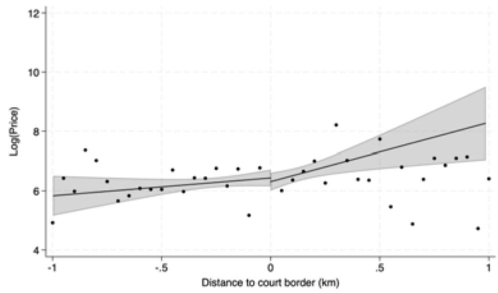
We investigate these effects further for resitdential transaction prices in

Figure 2.7: Effect at cut off on real prices

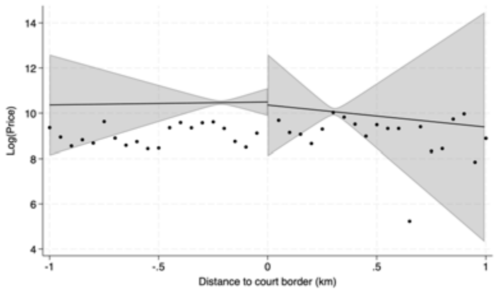
(a) pre 1920 rep50



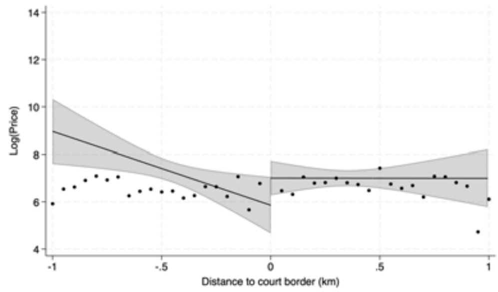
(b) 1920-1926 rep50



(c) pre 1920 rep50



(d) 1920-1926 rep50



*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.

Comprainng 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.

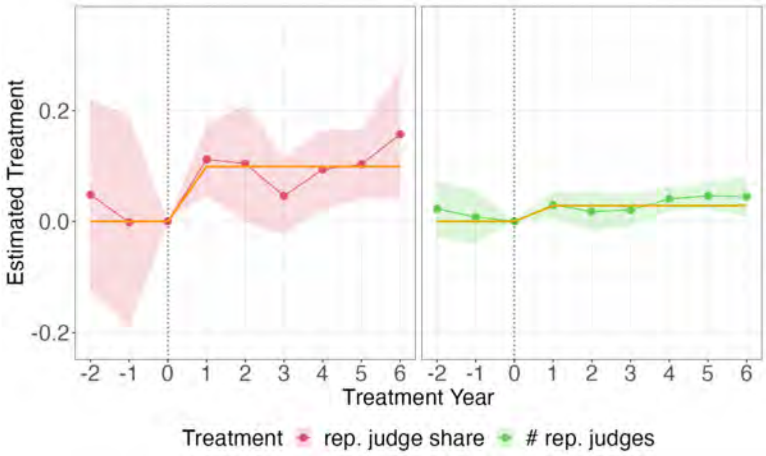
2.6 Event study results

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.

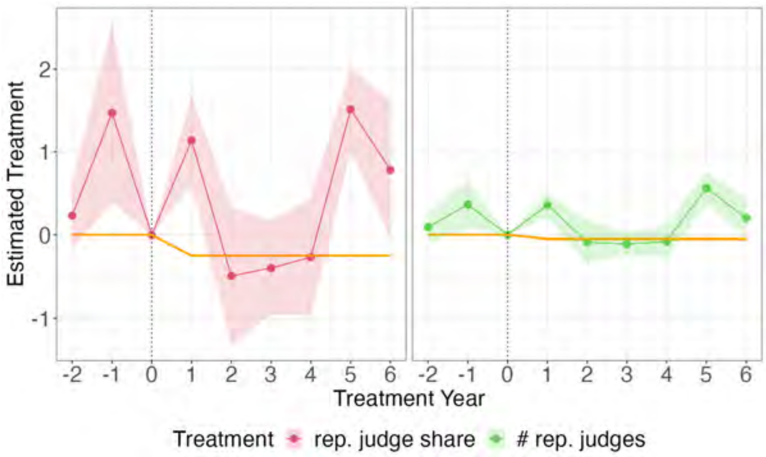
These resulst are confirmed by using the binary treatments from the the RD design in Appendix 2.C.4. There is no evidence for pretends in rent prices using the either the Republican only vs. Democrat only treatment or majority Republican districts (Figure 2.C.4). Point estinate average at 10.7% and 8.8%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

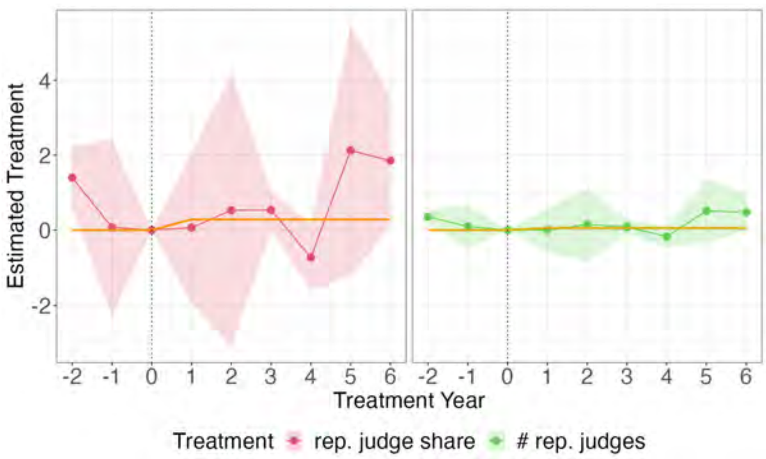
(a) Effect on rent prices



(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.

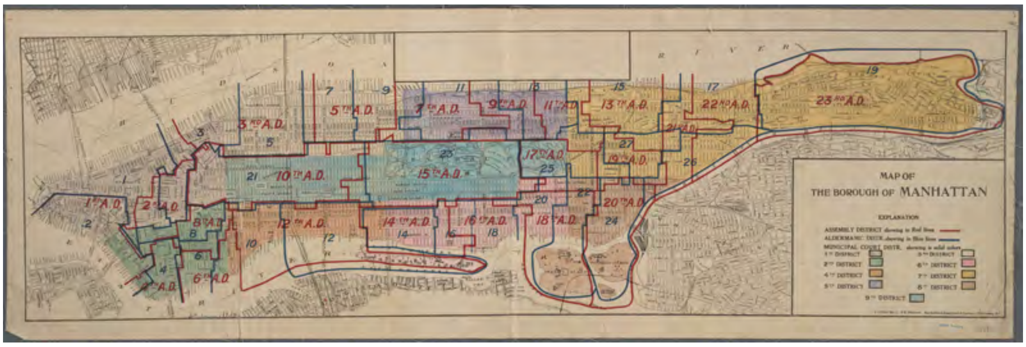
Overall, we find evidence across a variety of tests that the 1920 rent control laws were affecting market rents through judge rulings, at least indirectly. We establish that Republican judges were more lenient towards landlords than Democrat judges. While we cannot establish a direct link between court 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

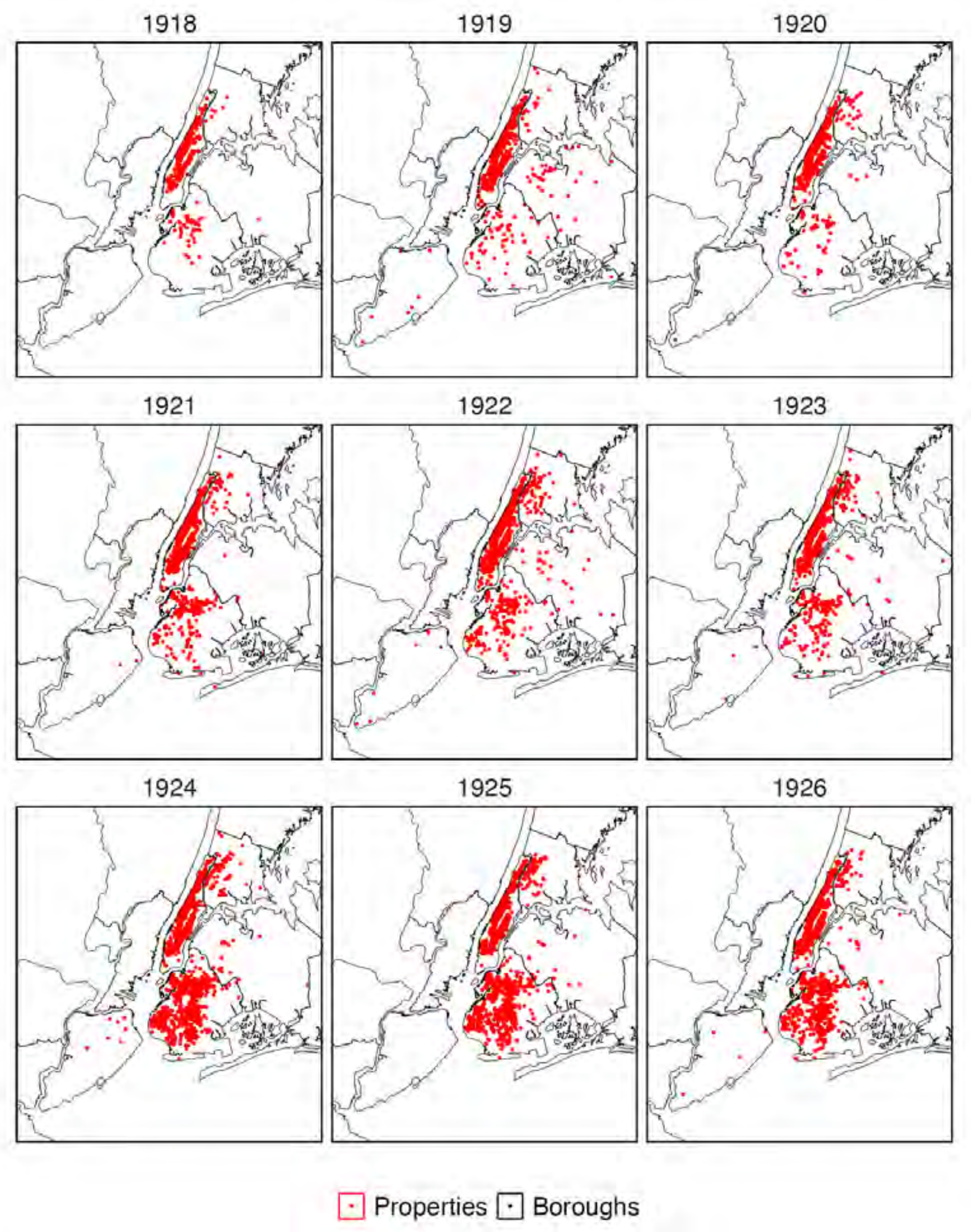


Figure 2.A.3: Spatial distribution of conveyances

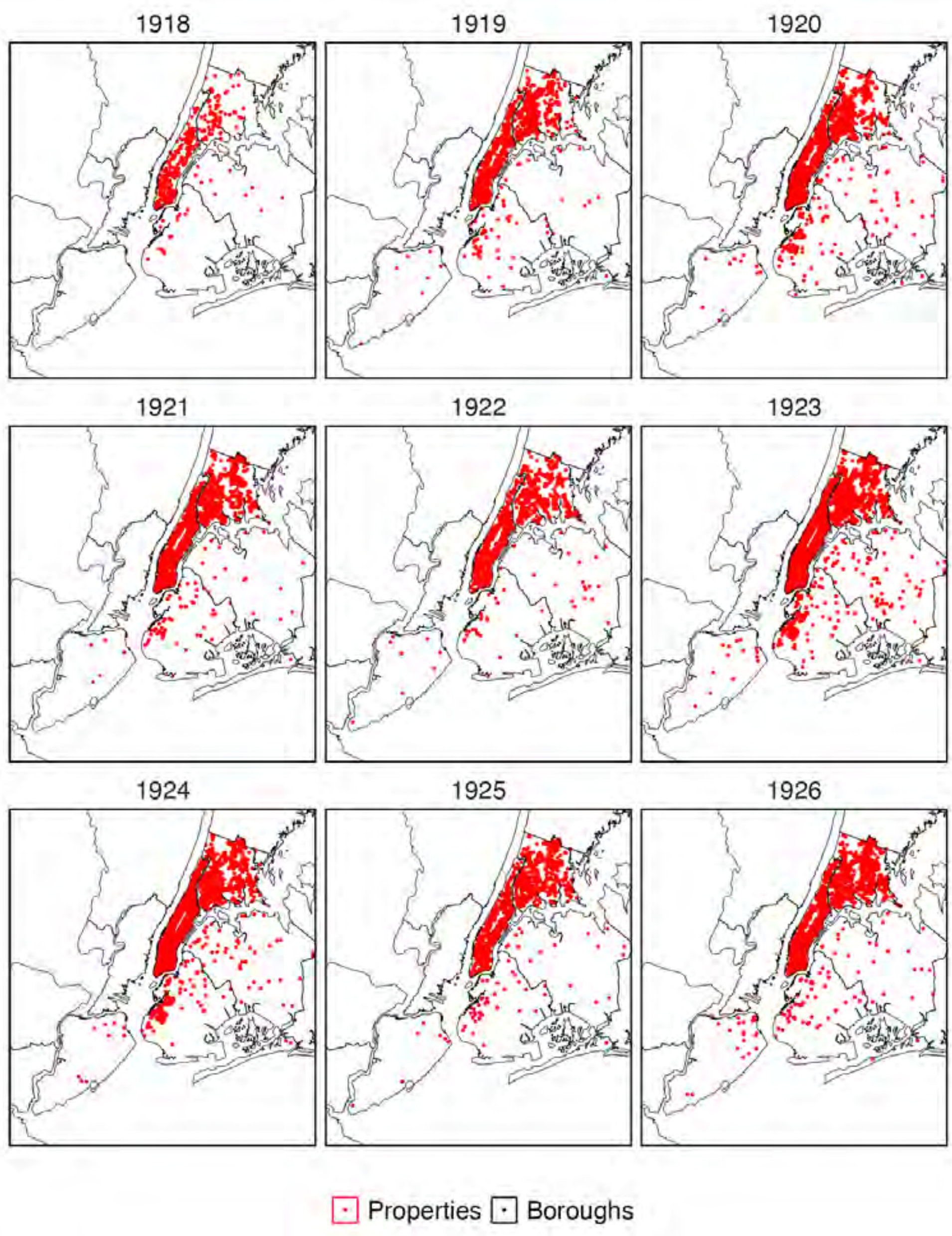
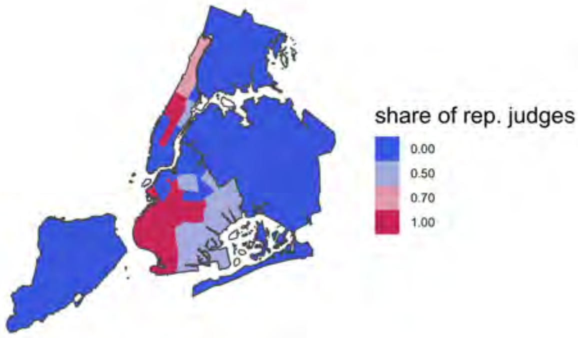
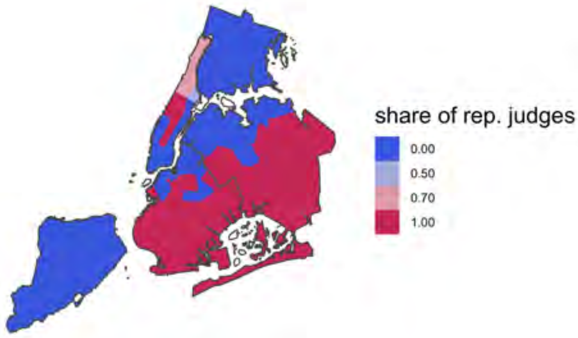


Figure 2.A.4: Share of Republican judge

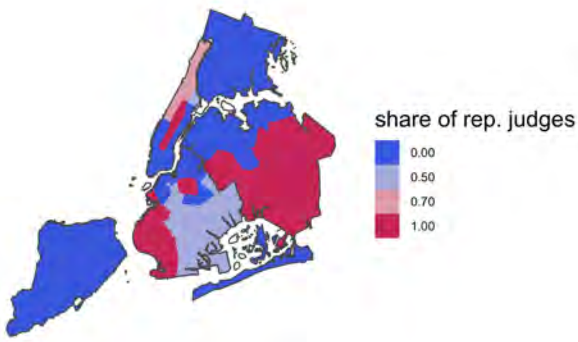
(a) 1918



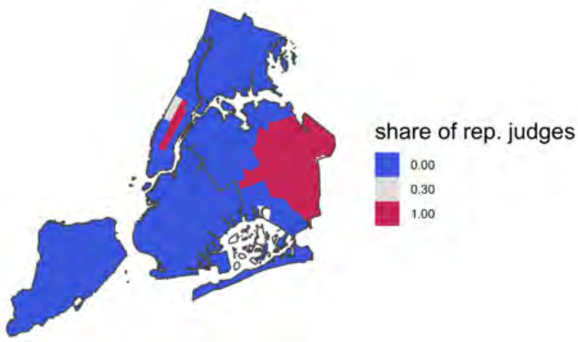
(b) 1922



(c) 1927



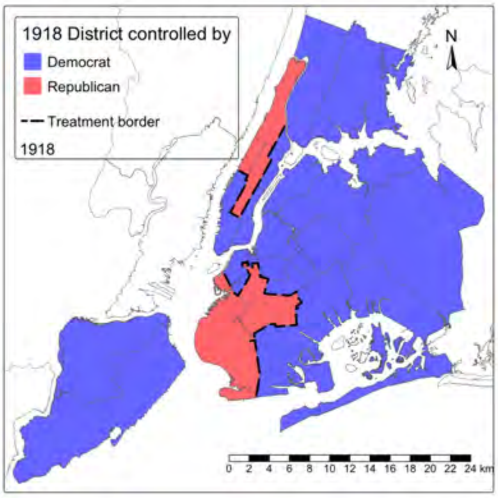
(d) 1931



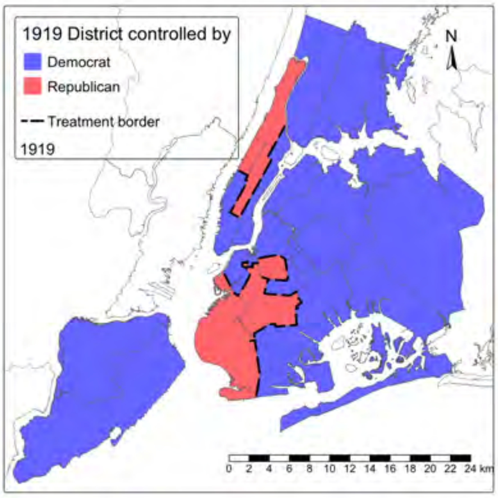
Note. Figure 2.A.4 shows the municipal court districts (MCD) in New York City. Each district had been colored according to the share of Republican judges elected at each point in time; we plot the variation in judge shares in MCDs in Panel (a) to (d); note that there nore changes from 1920 to 1925 in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

Figure 2.A.5: Alternative treatment boundary

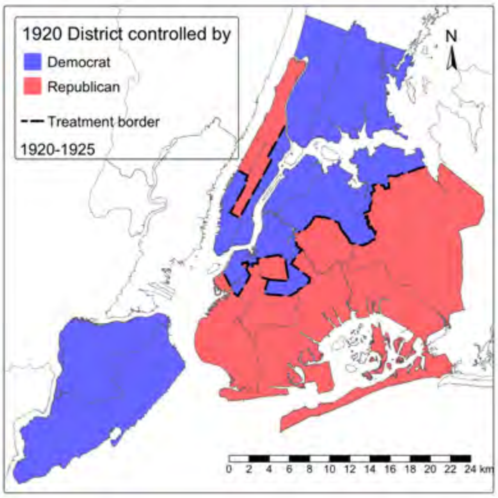
(a) 1918



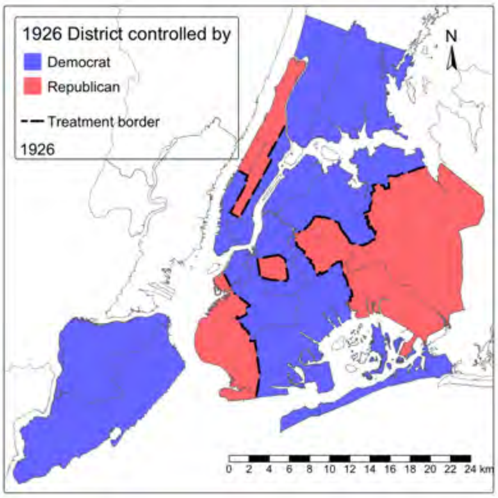
(b) 1919



(c) 1920-1925



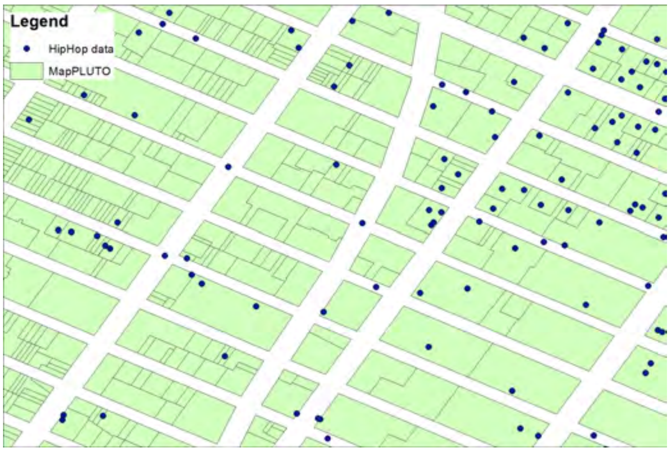
(d) 1926



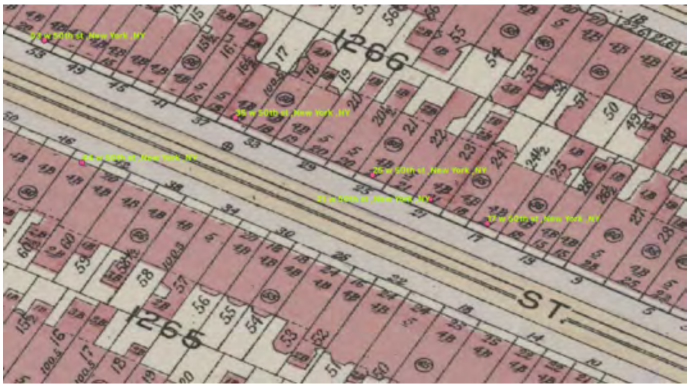
Note. Figure 2.A.5 shows the municipal court districts (MCD) in New York City. Each district had been colored according to the political affiliation of the elected MCD judges. A district is considered as Republican controlled if the share of Republican judges within the MCD is larger than 50%; therfore 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

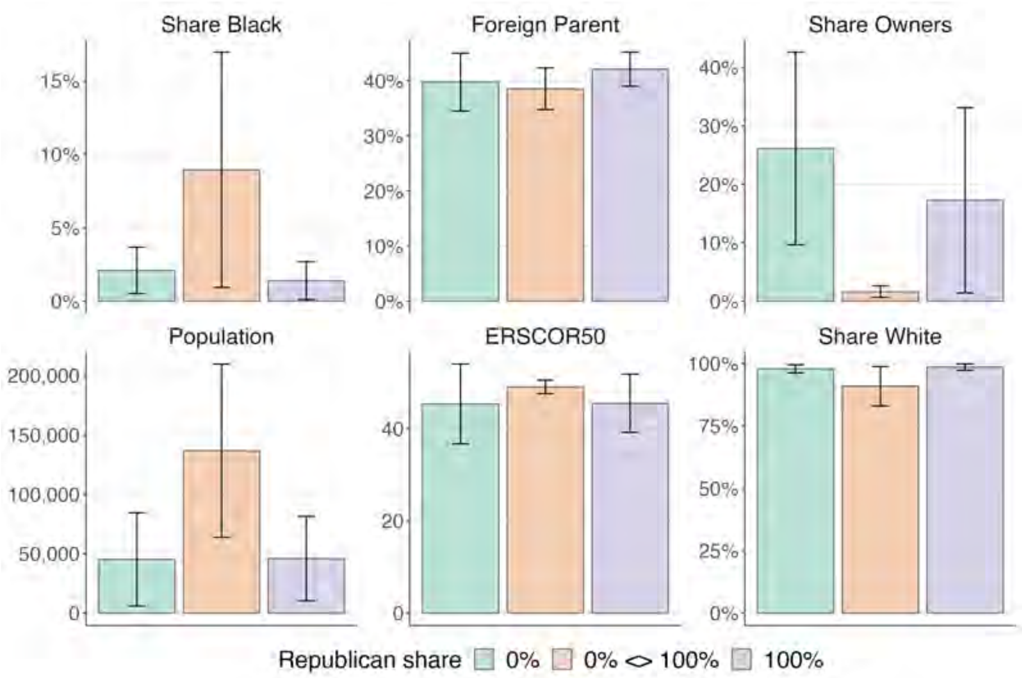


(b) Bromley fire insurance maps



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

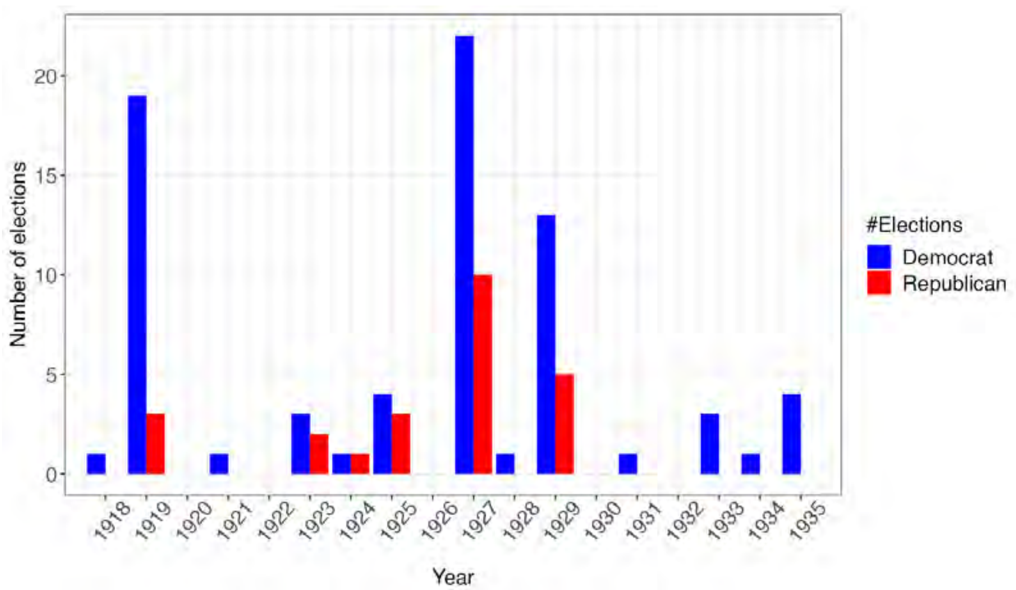
Note. Table 2.B.1 displays the teh full list of articles used to classify judge descisions in Chpater 2.3.1. It reports the name the Newspaper as well as the classification fo a judge' descisions. Eviction equals to one if a tenant was evicted and zero otherwise, rent decrease equals to one if a judg 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

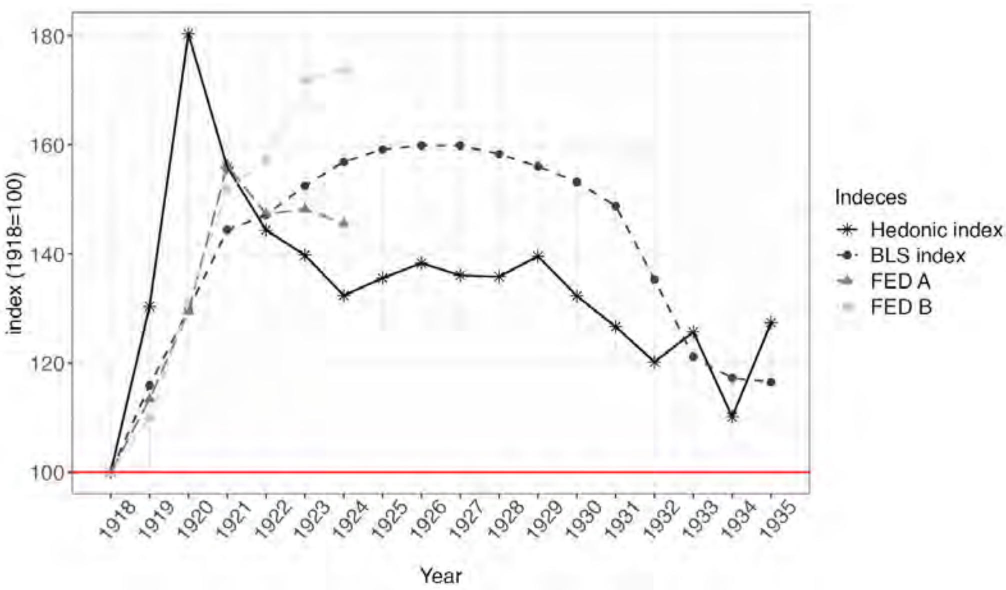
Note. Table 2.B.2 reports means and standard deviations in parentheses. Panel A describes the main outcomes in the rent dataset. Panel B-C describes the transaction price of residential and commercial properties. Panel D displays the average number of (republican) judges by municipal court district. 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).

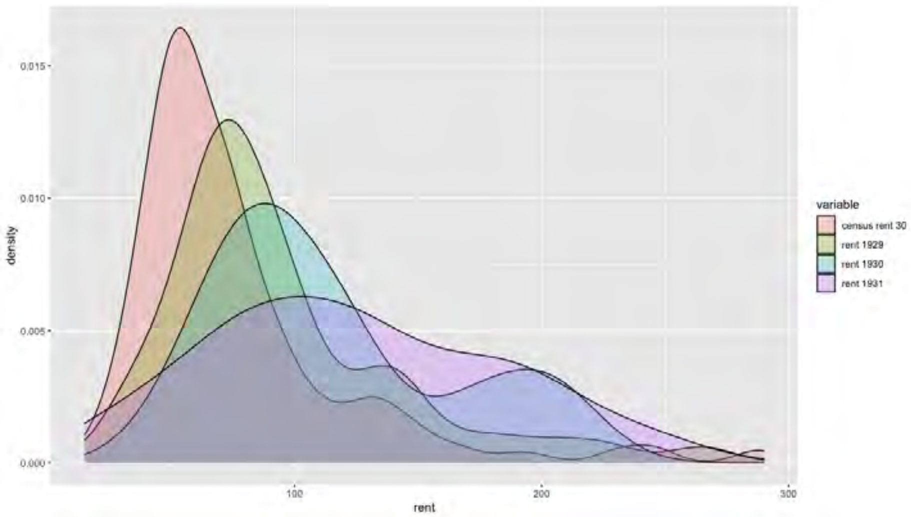
Figure 2.B.3: Rent indeces



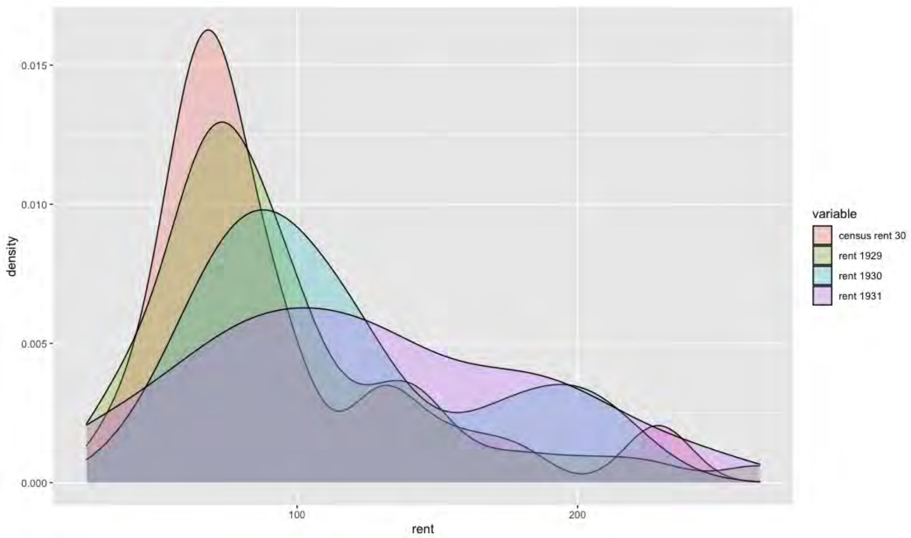
Note. Figure 2.B.3 shows rent indexes for New York City using 1918 as the base year. The black solid line shows a hedonic index using market rents (Hedonic index). The index values have been obtained from the fixed effects of regressing the logarithm of rent on property-level controls and time-fixed 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

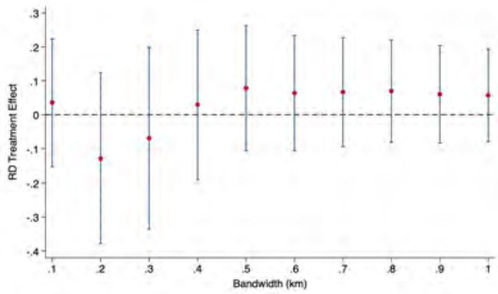
	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.310	0.461	1.335***	0.264	0.390	0.712	0.828	0.275
	0.281	0.471	0.325	0.357	0.350	0.607	0.494	0.502
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.788	0.779	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.

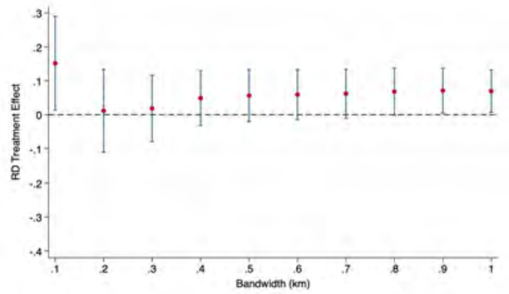
2.C.3 RDD estimates for Alternative bandwidth choices

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

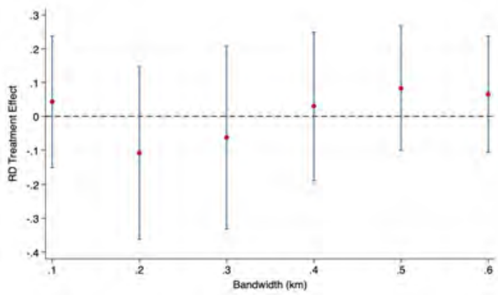
(a) Republican only MCD 1918-1920



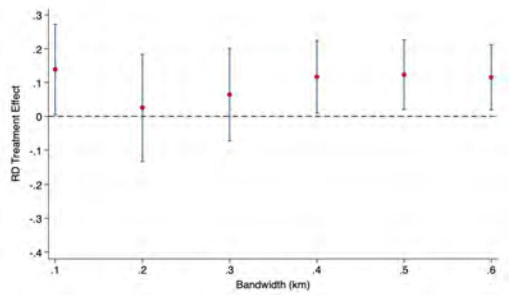
(b) Republican only MCD - 1921-1926



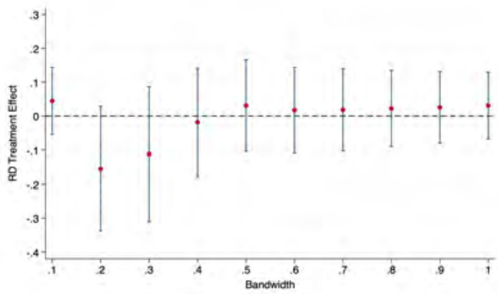
(c) Republican only MCD - Manhattan - 1918-1920



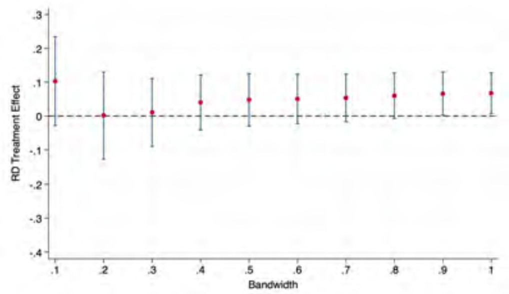
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



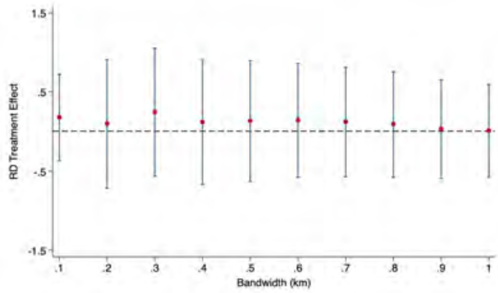
(f) Majority Republican MCD - 1921-1926



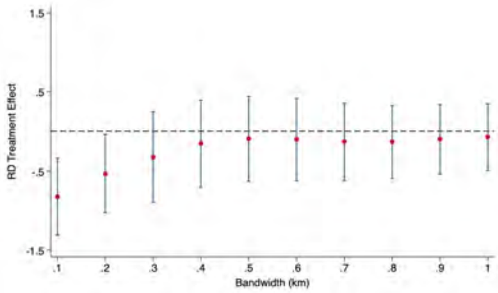
Note. Figure 2.C.1 shows RD estimates from estimating Equation 2.1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 2.1 is estimated using a triangular kernel with a linear fit; the outcome variable is the logarithm of rents. 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

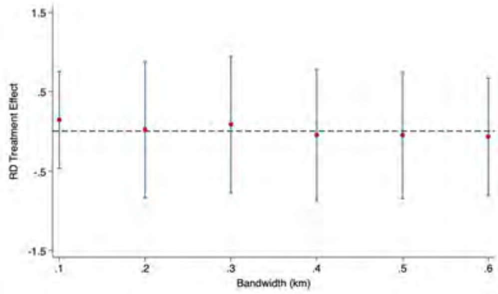
(a) Republican only MCD - 1918-1920



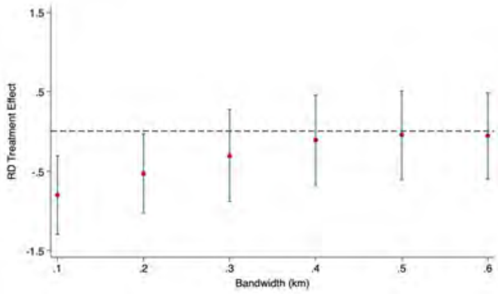
(b) Republican only MCD - 1921-1926



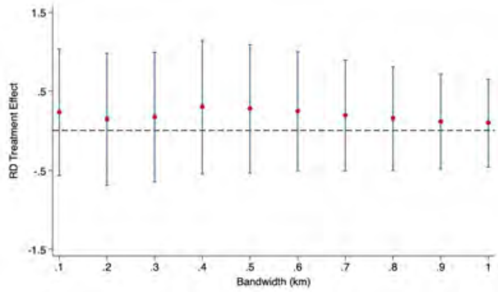
(c) Republican only MCD - Manhattan - 1918-1920



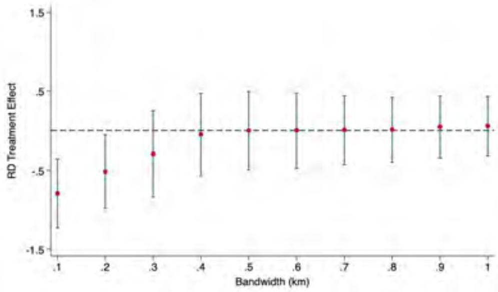
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



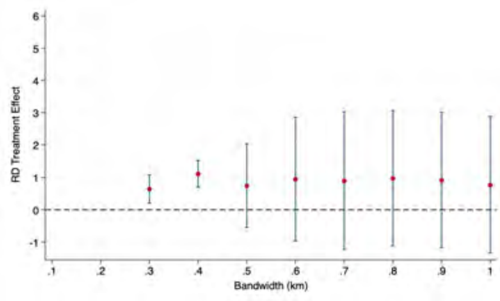
(f) Majority Republican MCD - 1921-1926



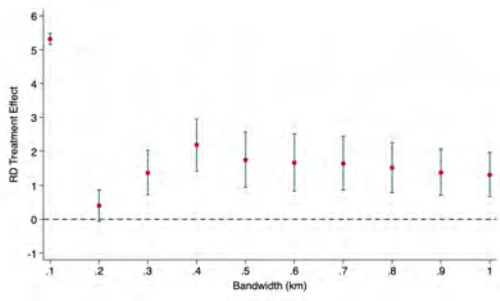
Note. Figure 2.C.2 shows RD estimates from estimating Equation 2.1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 2.1 is estimated using a triangular kernel with a linear fit; the outcome variable is the logarithm of residential 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

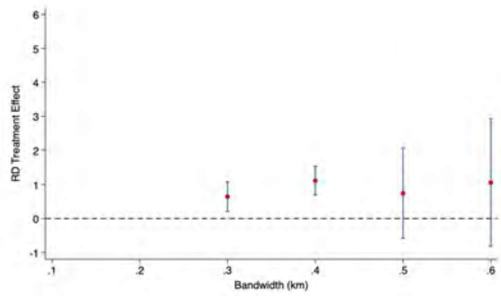
(a) Republican only MCD - 1918-1920



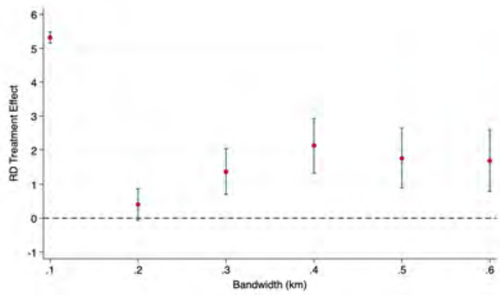
(b) Republican only MCD - 1921-1926



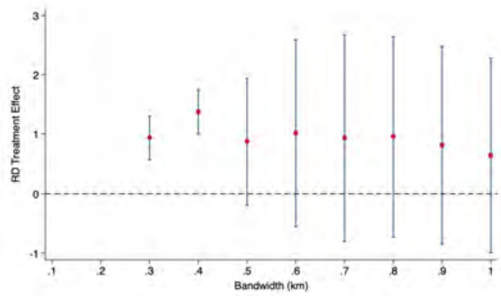
(c) Republican only MCD - Manhattan - 1918-1920



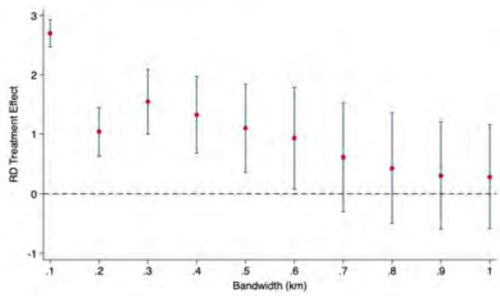
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



(f) Majority Republican MCD - 1921-1926

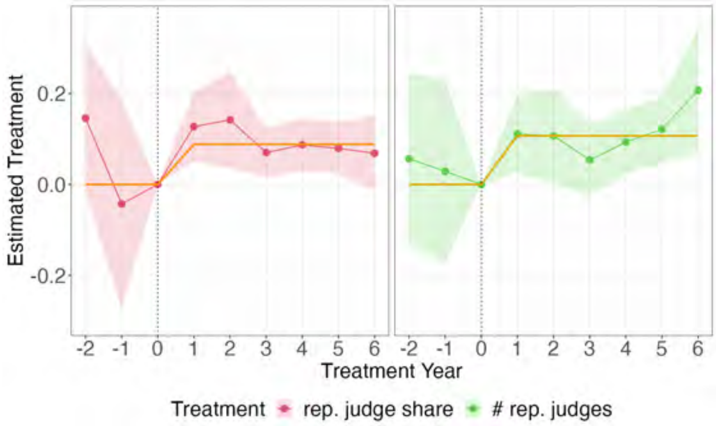


Note. Figure 2.C.3 shows RD estimates from estimating Equation 2.1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 2.1 is estimated using a triangular kernel with a linear fit; the outcome variable is the logarithm of commercial 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.

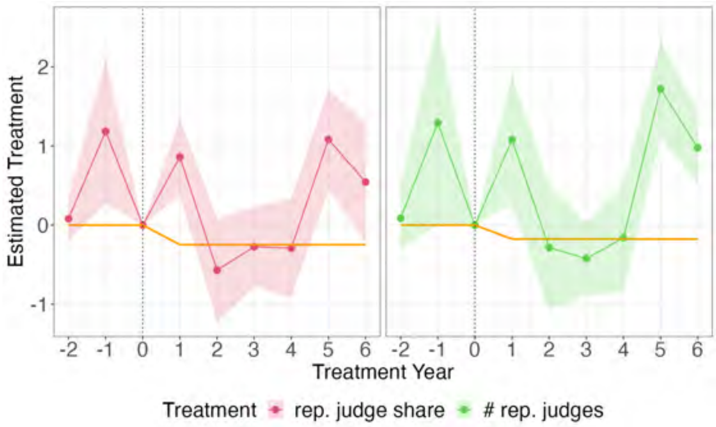
2.C.4 Event study results

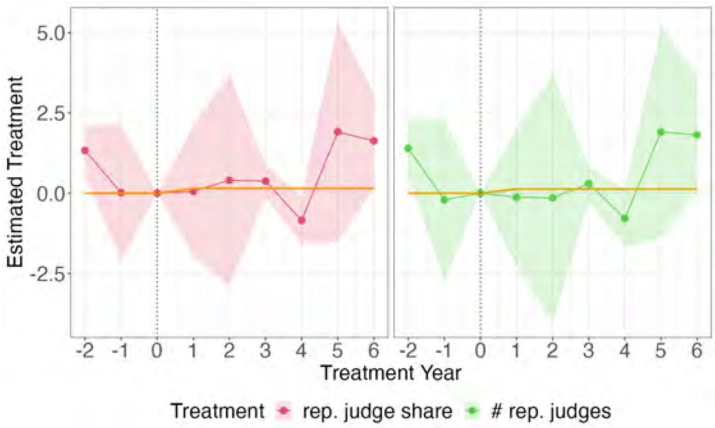
Figure 2.C.4: Effect of binary treatments

(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.