Judge for Yourself? The Impact of Controls on Rents in Interwar New York*

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Abstract

As cities across the high-income world struggle with housing affordability challenges, rent controls have re-emerged as a popular policy tool. A growing body of research examines the effects of these rent controls, although the literature largely focuses on post-World War II rent controls. In 1920, New York became the first city in the US to introduce rent controls, allowing elected judges to evaluate on a case-by-case basis whether proposed increases were reasonable. The effect of these rent controls, however, is unknown. We examine, for the first time, the effects of these rent controls on market outcomes, using a spatial regression discontinuity design and comprehensive datasets of market rents, from newspaper listings, and judicial districts, including judge characteristics, for New York for the period 1918-1926. We find that rent controls cause a spillover in demand, with market rents in "pro-landlord" judge districts 10% higher than just over the border in "pro-tenant" judge districts.

Keywords: Rent control, New York city, 1920s.

JEL codes: O18, R21, R31.

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1 Introduction

The affordability of housing has become an issue of primary political importance in many high-income cities since the Great Recession of the late 2000s. This is particularly true for rents, with urban rents in the U.S. rising by 70 percent between 2010 and 2024, compared to an increase in the overall CPI of 45%.[1] In response, many jurisdictions have in recent years revisited the use of regulatory measures that limit the allowable increase in rents. However, despite their increasing popularity as a policy tool, the economic costs and benefits of these regulations has long been a source of considerable debate amongst academics and policymakers.

Rent controls were widely used internationally after both First and Second World Wars, in the context of high inflation rates due to severe housing shortages. In this paper, we study the impact on market outcomes of 1920 rent control laws in New York City (NYC), the world's largest city at that time. These regulations combined modern "Just Cause Evictions" elements with the legal authority to control prices, giving elected civil court judges the power to determine whether a rent increase was "reasonable". 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 combination of the discretionary nature of the controls and the openly ideological persuasions of judges. Theory suggests that, in a world with non-trivial legal costs, where landlords face the prospect of "pro-tenant" judge, their incentive to increase rents is lower, given that tenants could withhold rents and given the prospect of non-recoverable legal costs.

We implement a Regression Discontinuity Design (RDD) to measure the effects of rent control on market rents, using both the application of controls across tenancies and the binding nature of municipal court district (MCD) boundaries and distance to the boundaries between "pro-tenant" (Democrat) and "pro-landlord" (Republican) judge districts. To measure market rents, our outcome of interest, we assemble a dataset of over 12,000 NYC rental listings, with precise location and listed rent, from the New York Times (NYT) for the period 1918-1926. To measure our treatment, the strictness of rent controls, we collect information on all 125 municipal district court judges, their affiliations and election cycles, from the

[1] These percentage changes are taken from FRED tables CUSR0000SEHA and CPIAUCSL.

NYC Official City Directory. We find that, in line with our theoretical proposition, in Republican-controlled districts, rents at the boundary jumped after the policy was introduced, by about 10%. Supporting our causal interpretation, before the introduction of the policy, rent prices were smooth at the boundary. We complement our baseline RDD analysis, where rent control strictness is categorical, with an event study design, allowing us to exploit the potential for judge share to be non-zero for both parties. The results support our baseline: mixed districts saw market rents that were 6%-8% higher than Democrat-only districts. Supporting a causal interpretation, we also show that the effect of the controls was short-lived: an analysis of rents in 1930, after rent controls were abolished, shows no effect at the boundary.

Our paper is related to the vast literature investigating the economic effects of rent control on rent prices. Kholodilin (2024) provides a recent overview of the literature on the economic effects of rent controls, in particular on eight outcomes. That review of the literature finds that, in the vast majority of cases, while rent controls do moderate increases for protected tenants (e.g. Olsen, 1972; Linneman, 1987), rents in the uncontrolled sector increase as a result (Early and Olsen, 1998). Mobility and the supply of rental housing also suffer because of rent controls (Svarer et al., 2005): studying modern San Francisco controls, Diamond et al. (2019) find that tenants with rent controls stayed in their home longer and that landlords reduced supply by up to 20 per cent through sales, conversion of the building and redevelopment. Similarly, looking the ending of controls in Massachusetts, Sims (2007) finds that landlords lowered the quality and quality of the rental stock (see also Sagner and Voigtländer, 2023). Relevant to our setting, the higher the intensity of rent control, the stronger its effects (Fetter, 2016; Early, 2000; Breidenbach et al., 2019).

Our research is related, secondly, to a body of economic research on judges and their decision-making. 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; in the context of a public utility commission; Lim and Yurukoglu (2018) shows that party affiliation 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 both these literatures. In relation to decision-making by judges, our contribution is evidence that elected officials' political affiliation affects their decision-making, according to the party's ideology. We contribute to the literature on rent control in two ways: firstly, ours is the first using a dwelling-level dataset in a pre-World War 2 setting and we find support. Secondly, in addition to a new setting, we contribute to this literature by investigating a new policy design that works through judges' discretion over rents – related, we propose a mechanism affecting landlords' profit expectations due to costly law proceedings.

The paper is organized as follows. Section 2 describes the historical and institutional context. Section 3 discusses the data sources and provides evidence on judges' decision-making behavior. Section 4 introduces the mechanism and discusses the empirical analysis. In Section 5, we estimate the effect of rent controls, first using a regression discontinuity design and then with an event study approach. Section 6 concludes.

2 Historical and Institutional Context

By the early 20th century, New York City had grown to become the second-largest city in the world, after London, with a population of approximately 5 million people at the outbreak of World War I in 1914. The war, however, had a significant impact on the city's economy and its housing market, especially after the US entered the war. In 1918, less than $40m of new construction projects were authorized, down nearly 80% from almost $200m in 1916. With little new supply, a rapidly rising population and (after the war's end) returning troops, the vacancy rate of housing fell from 5.6% in March 1916 to just 0.2% in February 1921 (Grebler, 1952). With such tight market conditions, housing prices soared; according to Lyons et al. (2024), market rents in New York City rose by 120% between 1916 and 1920. Individual examples support this market-level assessment. For instance, the monthly rent for a small four-room apartment increased by 125% in four months during 1919, from $18.50 in June to $42 by September, while another apartment on Park Avenue near 92nd Street saw its annual rent jump from $2,400 to $5,750 (Fogelson, 2013; New York (State)., 1921).

Such stark increases in market rents brought a response, initially by tenant unions, including through rent strikes, and in turn by politicians. The state government implemented rent control laws initially in April 1920, amending them in September (Fogelson, 2013). The regulation stated that rent increases of more than 25 percent per year were "unjust, unreasonable, and oppressive", in effect strongly discouraging them. However, the ultimate decision in relation to any proposed increase that came before the court fell to MCD judges, who could decide whether the proposed increase was 'reasonable' and also whether an eviction warrant was applicable. Judges could grant stays of up to twelve months and strike down rent increases which were not 'reasonable'. The regulations on rent increases applied to all buildings built before April (later September) 1920, thus exempting new construction.[2] The design of this policy gave judges at the MCD level significant power in relation to rental markets. In effect, by being able to rule on the reasonableness of individual rent increases, they could determine rent ceilings. As discussed in Fogelson (2013), contemporary accounts noted that municipal district judges wielded more power than ever before and their decisions reflected their attitudes.

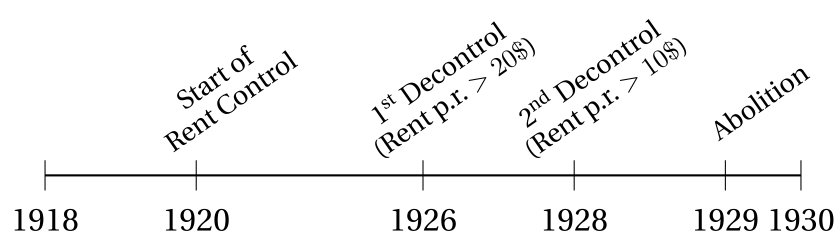
Figure 1 provides an overview of the timeline of rent controls. They were started in 1920 and abolished in 1929. Underpinning that abolition was the wider trend in housing costs in the city during the 1920s. The 1920s saw very high volumes of new rental supply, with over 740,000 new homes built 1920-1929, over twice the number built in the 1910s (and almost four times the number that would be built in the 1930s). With the stock of housing growing by nearly half in the space of a decade, rents in the open market peaked in 1920 and had fallen by 28% by 1930 (Lyons et al., 2024).[3] From the start, the "Emergency" rent laws had been subject to heavy criticism through their existence from several parties, including real estate interest groups such as the Greater New York Taxpayer Association (GNYT). Governor Al Smith appointed an advisory Commission on rent controls, the so-called

[2]A fourth stipulation related to services related to shelter: a landlord who failed to furnish essential services could be charged with a misdemeanor, punishable by a fine of $1,000, a year in prison, or both.

[3]According to the same index, market rents fell by a further 28% in the Great Depression (1930-1934), meaning that in nominal terms market rents had fallen by just over half between 1920 and 1934.

"Stein Commission", which recommended extending the laws in 1923 but, as market conditions changed, in 1925 it recommended "luxury decontrol", i.e. removing the top end of the rental market from the regulations (Fogelson, 2013). In May 1926, the first rent decontrol occurred, removing any dwellings with a monthly rent per room of \$20 or higher. With falling rents, a second phase of rent decontrol took place in 1928, with any dwelling with monthly rents per room over \$10 now excluded from controls, before the regulations expired completely in 1929 (Collins, 2013).

Figure 1: Timeline of Rent Control Events (1918-1930)



The rent controls in New York in the 1920s were far from notional. The Stein Commission outlined statistics on the number of Summary Proceedings instituted in the City of New York in 1920 and 1921 (New York (State)., 1921). Across the city's five boroughs, there were 118,240 summary proceedings in 1920 and 125,856 in 1921. We take this as clear evidence that rent controls were used frequently and thus provided a credible constraint on the behaviour of landlords.

In 1920, New York City had 24 municipal court districts (MCDs); the number of districts was increased in 1924 by one (and again in the 1930s, after our period, to 28). The number of judges per district varied from one to six, with an average of 2.6 judges per MCD. The total number of judges in the city rose from 45 in 1918 to 53 in 1930. Judges were elected and individuals were eligible to run for election if they resided in the district and had served as an Attorney of State for at least five years. They served ten-year terms, earning \$8,000-\$9,000 per year, but could be removed by a two-thirds vote of the State Senate upon the Governor's recommendation. With approximately 50 judges and roughly 120,000 cases per year, a judge would be expected to handle on average 2,400 cases per year, although this number will have varied considerably over time and by district.

The high volume of cases meant both that judges will have had to have used priors, including ideological beliefs, when determining cases rapidly, but also that 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".[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 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 proposed rent increases reduced (and then only by one or two dollars). Unsurprisingly, as per Fogelson (2013), there was a sense that there were "pro-tenant judges" and "pro-landlord judges", a feature that we exploit in our empirical strategy.

3 Data

In this section, we describe the construction of our main outcome of interest, market rents, and our judge-level dataset, which yields our main treatment(s) of interest. We also document evidence from newspaper articles on landlord-tenant cases on the link between judge decisions and party ideology. Figure 2 provides a preview of three kinds of data that we use in our analysis.

3.1 Market Rents

Our data on market rents comes from New York Times listings. A sample of listings was manually digitized, with listings only being included in the dataset if certain

[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.

Figure 2: Examples of data sources

(a) New York Times


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

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

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

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

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

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

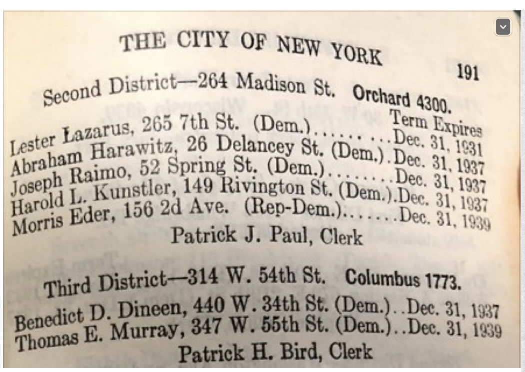
Unfurnished—East Side.

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

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

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

(b) Green Book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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

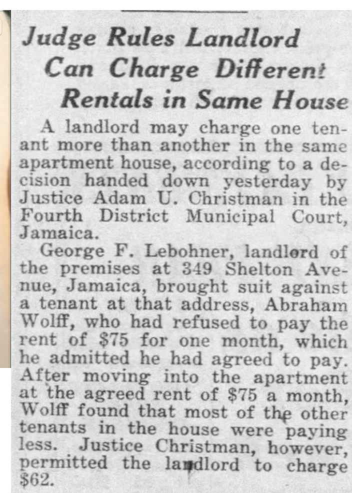
Patrick J. Paul, Clerk

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) 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.

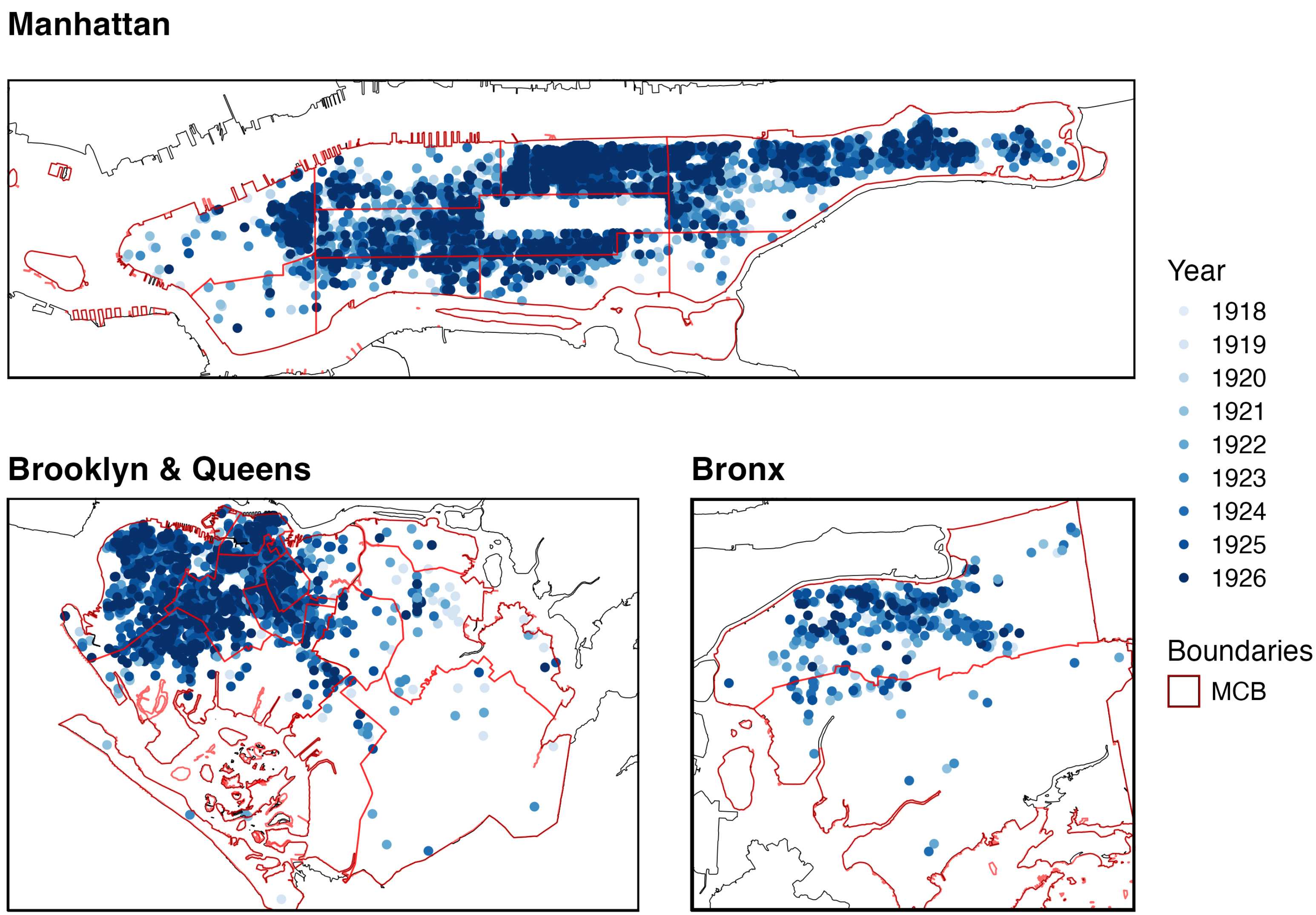
Note. Figure 2 shows example of the main data sources used in the paper. Panel 2a shows a snapshot of the real estate section of the New York Times; Panel 2b displays the Green Book; and Panel 2c shows an example of a landlord tenant case from the Daily News.
Source. New York Times; York (N.Y.) et al. (n.d.); Real Estate Record and Builders' Guide; Green Book; Daily News.

criteria were met in relation to information available. In particular, for a listing to be included, it had to have a (listed) rent, exact address, a measure of size (such as number of rooms or bedrooms), and a property type (house or apartment). Other characteristics, including whether the dwelling was furnished or whether utilities were included in the rent, were also gathered. The typical strategy for choosing listings involved taking the last Sunday for the second month of each quarter from 1918 until 1926, as Sundays had the largest volume of listings. Given the volume of listings on most dates, our data-gathering strategy involved sampling across all columns with relevant listings, thus ensuring that any geographic clustering in the rental listings would not contaminate the dataset. This process yielded a final dataset of 15,398 listings collected across 80 dates between January 1918 and November 1926.

Each listing was then geocoded using the address. This involved a two-step process, with a first-round geolocation using Google Maps API. However, streets numbers and in some cases street names have changed since the sample period, meaning the Google Maps API may not accurately location some listings. For that reason, exact addresses were cross-referenced using street intersections (exploiting the inclusion of streets and avenues in listings) to create coordinates that could address changes to street numbers. In addition, changes in street addresses were handled using Bromley fire insurance maps and the PLUTO 2002 shapefiles.[6] Figure 3 shows the spatial distribution of geocoded rental listings. A key resulting feature of each listing is the MCD in which it is located and the distance (in yards) from the nearest neighboring district. Otherwise similar rental homes within close proximity to the boundary of an MCD with a different composition of judges (by party affiliation) are at the core of our identification strategy.

[6] Figure A.3 in Appendix A shows a detail of manually corrected observations and the underlying lots, addresses, and house numbers.

Figure 3: Spatial distribution of rental properties



In addition to open-market rents for individually located dwellings, we also calculate district-level mix-adjusted average rents. These are done using hedonic price regressions, with year-by-district effects used to generate indices. [More details needed here - how do we use these indices? How do they align with judge districts? And what control variables do we have in the hedonic regressions?]

In our Regression Discontinuity Design approach, outlined later, we include fixed effects for Neighborhood Tabulation Areas (NTAs), as fixed effects at the MCD level would be perfectly multicollinear with our treatment. Our dataset covers NTAs throughout the city, with full coverage of Manhattan and the Bronx and coverage in Brooklyn and Queens also.[7] The resulting indices match the main characteristics of NYC's rental market with, for example, rents lower in the Lower East Side and highest in the Upper West and Upper East Sides. Overall, rents in our dataset are higher than observed in the 1930 Census but this is due to the frequency with which different neighborhoods are observed.[8]

3.2 Judges

Our primary argument is that a judge's party affiliation correlates with their decisions in relation to rent increases (or evictions). 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 base of 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. How-

[7] While there are some observations for Staten Island, these are few. Also, there is no variation in judges in Staten Island: all judges throughout the period are Democrat, giving us little statistical power in the outcome and no variation in the treatment. For that reason, we focus on the four other boroughs of NYC.

[8] 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 B.4. This confirms that higher average rents in our sample largely stem from spatial bias.

ever, 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).

Empirically, our approach is informed 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.

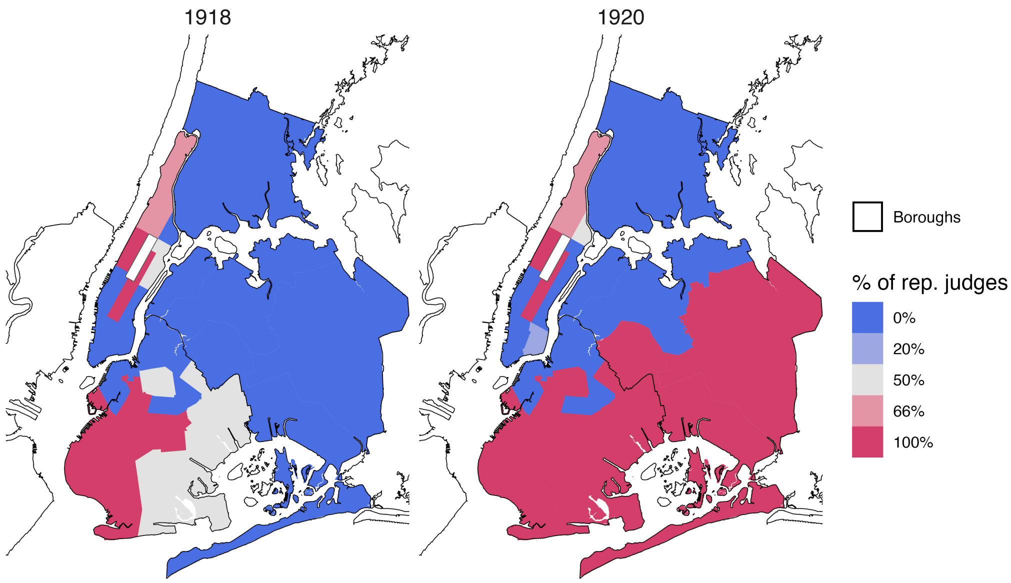
We start by gathering information about each of 125 judges from the NYC Official City directory, known as the *Green Book*. This directory provides each judge's MCD, party affiliation, and re-election date. All judges in our study are affiliated with a political party. The majority are Democrats (93 judges), followed by Republicans (30 judges), one Liberal Party affiliate, and one Socialist Party member. However, during the period under analysis, the share of Republican judges at the district level fluctuated. The share of Republican judges for selected years, by MCD, is shown in Figure 4.

Historical rent case records do not appear to have survived, making it difficult to show how judges' decisions differed by party affiliation. Rather than assume that judges followed political persuasions, we collect information on 72 municipal court cases between landlord and tenant, where the judge is known, as reported in local newspapers and available through newspaper archives. These articles, spanning from 1918 to 1926, provided insights into the stance of 42 judges (23 Democrats and 19 Republicans). Articles were sourced from newspaper archives, using search terms that included each 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. We classified the judges' decisions using three criteria, assigning a dummy variable equal to one if:

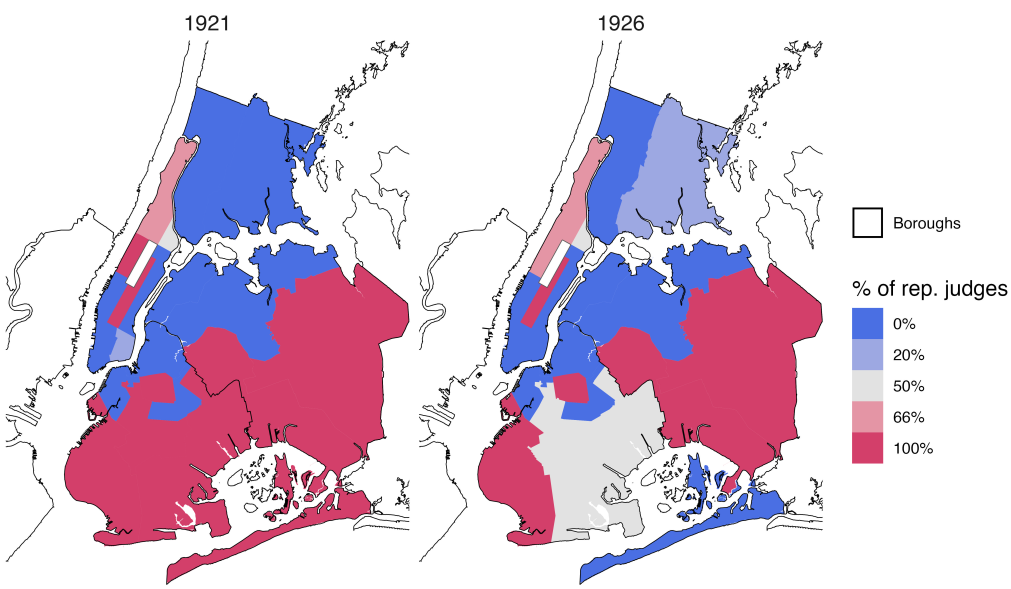
- The judge reduced the rent demanded by the landlord.

Figure 4: Share of Republican judge

(a) Pre rent control



(b) Post rent control



Note. Figure 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 (b); note that there nore changes from 1920 to 1925 in Panel.

- 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 5. For eviction cases, Republican judges granted a stay in 17% of cases, compared to 56% in Democratic districts. This is clear evidence of differences across party lines. 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. It is important to note that the measure here is at the extensive margin: the decision to increase or decrease the rent or not. It is likely, given the result in relation to evictions, that where Democrat judges allowed increases, the increases allowed would have been smaller than for Republican judges (the intensive margin; unobserved).

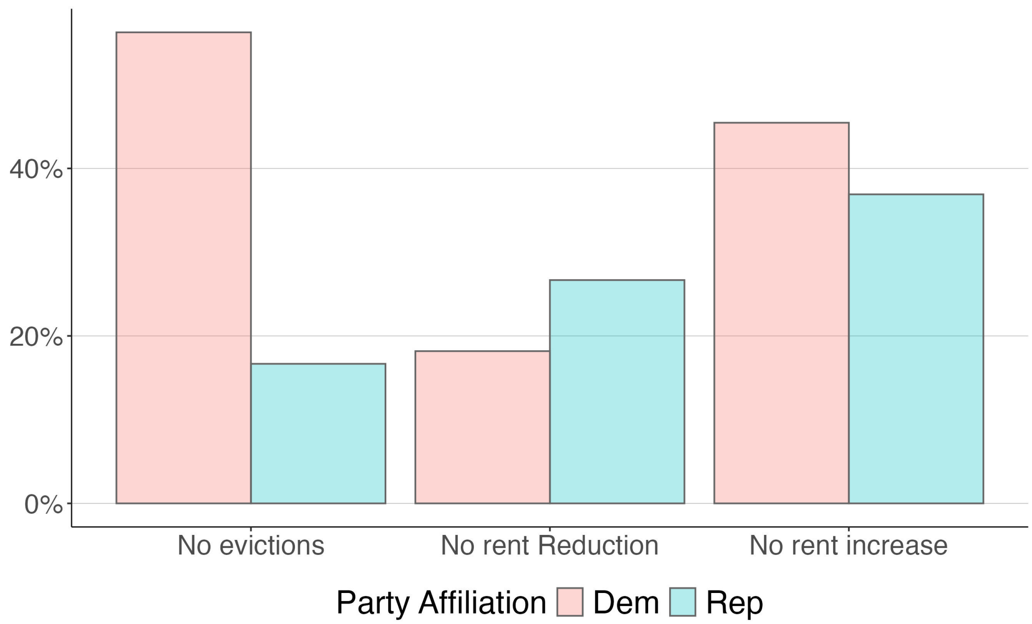
It is important to note the limitations of this dataset. Firstly, we observed only 26 of the 53 judges from 1920 to 1924 in rent cases and 23 of the 58 judges from 1920 to 1926 in eviction cases. The frequency of appearances varied significantly, with some judges appearing once and others up to eight times. The representativeness of judges' decisions is, therefore, uneven, and there may be potential bias due to newspaper reporting, which may favor more prominent cases or judges who seek public attention. Nonetheless, while caution is required, these indicative findings are supportive of the assumption that judges' decisions reflected their political affiliation. The complete list of newspapers used and the classification of judges can be found in Table B.1.

4 Empirical Strategy

4.1 Conceptual framework

We propose a straightforward framework to analyze how rent control may have impacted the housing market in New York City. We begin by assuming that landlords aim to maximize their income, by setting a rent amount denoted as r. In the absence of rent control, this rent would be determined through the market equi-

Figure 5: Judge decisions



Note. Figure 5 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 3.1.

librium, which we denote as r^*. With rent controls, the controlled rent is lower than the market rent: $\bar{r} < r^*$. With rent controls of the type instituted in NYC in the 1920s, landlords continue to choose rents to maximize income, but subject to the constraint that rent controls would be enforced in a Court. For simplicity, we consider two rents for any dwelling, r^* and $\bar{r}$.

If a landlord demands a rent (r^*) that is higher than the controlled rent ($\bar{r}$), the decision may end up before the Municipal Court. This could be because the tenant brings a case or, if the tenant refuses to pay, the landlord would file to evict the tenant. If the landlord loses the case, they incur costs represented by c, which includes hold-up and solicitor costs.[9] With multiple judges per MCD, they face a probability that they encounter a "pro-landlord" judge with a probability p and a "pro-tenant" judge with probability $1-p$. The payoffs for the landlord in choosing r can be expressed as follows:

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

The extreme values of p present simple outcomes. In the case where the probability of facing a pro-landlord judge is $p = 1$, firstly, the expected payoff of setting the rent to the market rent would be greater than the expected payoff of setting it to the controlled rent, $\mathbb{E}(r^*) > \mathbb{E}(\bar{r})$. Where the probability of facing a pro-landlord judge is $p = 0$, however, the expected payoff of setting the rent to the controlled rent minus the cost would be less than the expected payoff of simply setting it to the controlled rent, $\mathbb{E}(\bar{r}-c) < \mathbb{E}(\bar{r})$. However, where the probability of facing a pro-landlord judge is 0.5, the decision by the landlord depends on the relative sizes of the c and the market/control gap ($r^* - \bar{r}$: the landlord will increase the rent to the market level, if the market rent minus the cost exceeds the average rent, $r^* - c > \bar{r}$.

In short, our simple model predicts that 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 (all-in) cost of going to Court. Further, the broad prediction stemming from this simple theoretical framework is that, in general, prevailing rents in the open market will be higher, the greater the share of pro-landlord judges in

[9]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.

an MCD.

4.2 Regression discontinuity

Given the goal of identifying causal effects of rent controls on market outcomes, the main challenge in our setting is that assignment of judge type is not random. For example, the district electorate most likely to elect a pro-landlord judge may also be those where the share of landlords is high, where the housing stock is constrained, or where other demographic factors (such as income levels) may be different from districts electing pro-tenant judges. We can analyze basic Census data to assess differences across MCDs. As shown in Figure B.1 of Appendix B, all-Republican and all-Democrat MCDs are similar across a range of neighborhood characteristics, including population, income, population share by tenure, by race, or by second-generation immigrants. Mixed districts are largely similar, but differ on average in two indicators: total population; and tenure mix. Nonetheless, the potential for omitted variable bias (OVB) remains: there may exist factors that could lead to both high rents and the election of a pro-landlord judge, meaning estimates from standard regression analysis may be biased.

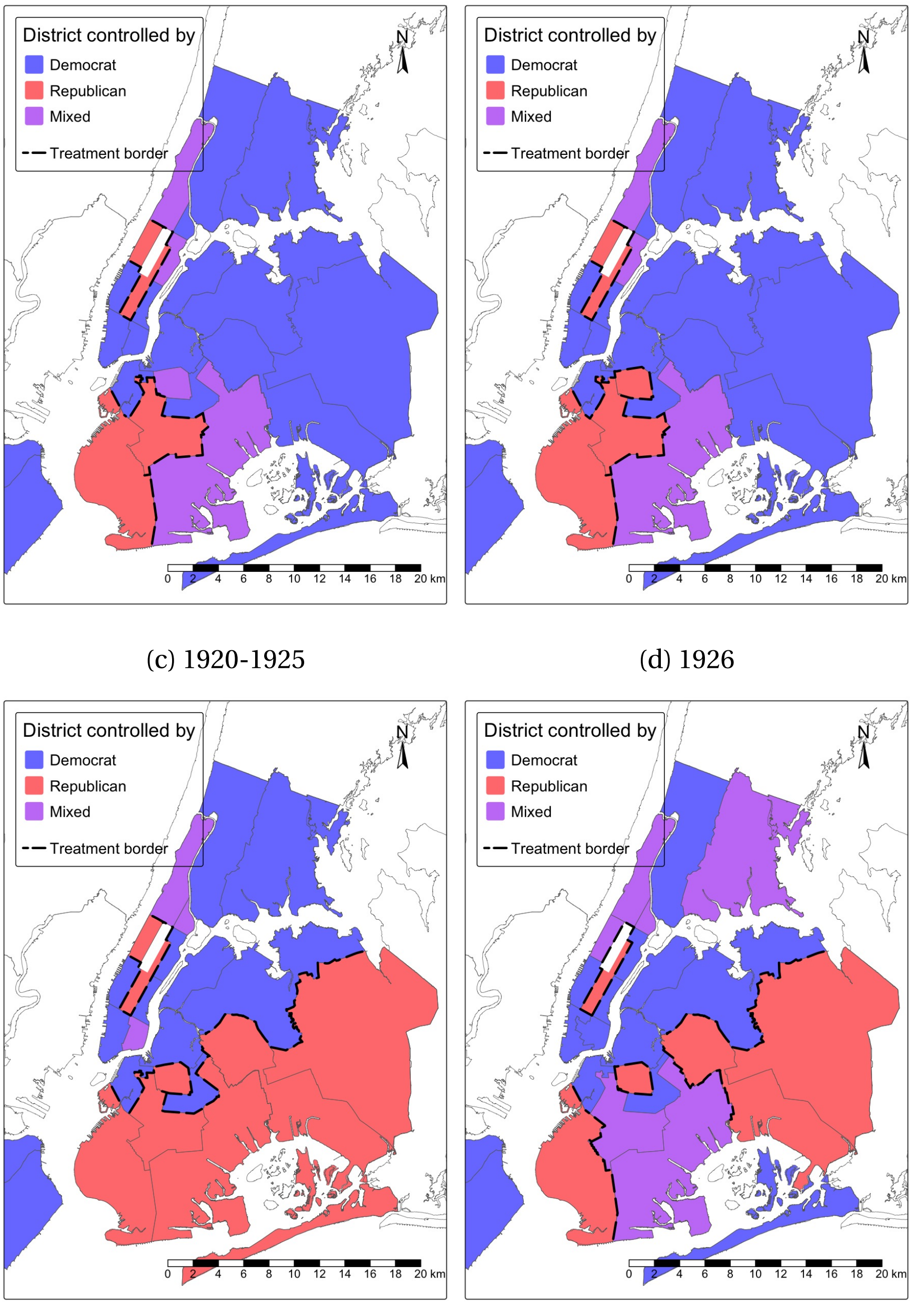
Our empirical strategy exploits the binding nature of MCD boundaries and variation in judge leniency. The same Court, with its set of judges, handled all cases in a district, with each dwelling mapped to one district by its geographical location. Theoretically, as noted above, the more pro-landlord an MCD's judges are, the higher rents will be. Empirically, as shown in Section 3.2, the evidence is that, on balance, judges affiliated to the Democratic Party will judge in favor of tenants, while Republican judges in favor of landlords. In particular, Figure 6 shows (in the dashed black lines) where the identification in the empirical design used comes from: in the RDD set-up, rental listings either side of the border between deep blue and deep red MCDs. As shown in the lower panel of Figure 4, this includes a number of areas in Manhattan and across Brooklyn and Queens.

Our analysis is at the dwelling level. It is in effect a hedonic price regression with a Regression Discontinuity Design (RDD), where we include as the forcing variable distance to the nearest boundary of an MCD with a different political affiliation. The forcing variable is positive within Republican districts and negative for Democrat districts; therefore, the cutoff is $c = 0$. In our baseline, we include

Figure 6: Treatment Boundary

(a) 1918 (b) 1919

(c) 1920-1925 (d) 1926



Note. Figure 6 shows the municipal court districts (MCD) in New York City. Each district has 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 indicates 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 Panels (a) to (d). Note that there are no changes from 1920 to 1925 in Panel (c).

only all-Republican or all-Democrat districts. This gives 18 districts per year and a total sample in the baseline of 11,192 listings.[10]

We estimate the following equation at the dwelling level:

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

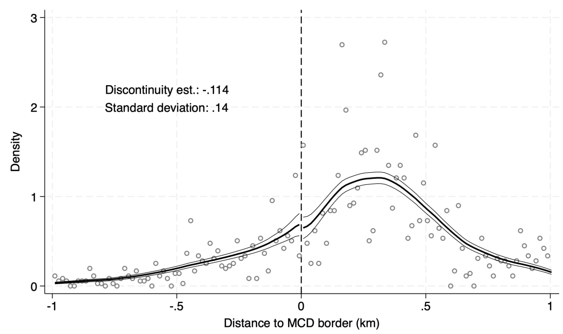
where $y_{i,m,t}$ is the listed rent for dwelling i in MCD m in year t and $distance_i$ measures the distance from property i to the nearest MCD border. $distance_i$ is negative if the MCD is controlled by a Democrat judge and positive otherwise, excluding mixed districts. The two unknown functions f^a and f^b are assumed to be smooth in distance. 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. As is standard in a hedonic set-up, we include a vector of dwelling-level controls, **X**, including size in rooms (included as a vector of categorical variables, one for each room size), whether the property was furnished, whether water and electricity were included in the rent, and property type (apartment or house). We also include two distance-based controls: distance to the coast/river and to the nearest park. We cluster standard errors at the neighborhood level to account for the correlation between nearby properties and present robust bias-corrected confidence intervals, correcting for the fact that confidence intervals are sensitive to bandwidth choice.

The identifying assumption in our RDD set-up is that the error term, $u_{i,t}$, does not jump at the boundary between MCDs. Where that assumption holds, β_i provides an unbiased estimate of the effect of MCDs (and thus rent controls) on a dwelling's rent. Support for the assumption that distance to MCD boundary is continuous at the discontinuity is given in Figure 7, which shows both density tests and histograms of the forcing variable for rents in bins of 12.5 meters before and during rent control. Neither figure reveals any apparent sorting around the discontinuity, and the estimate from the McCrary test is small and statistically insignificant.

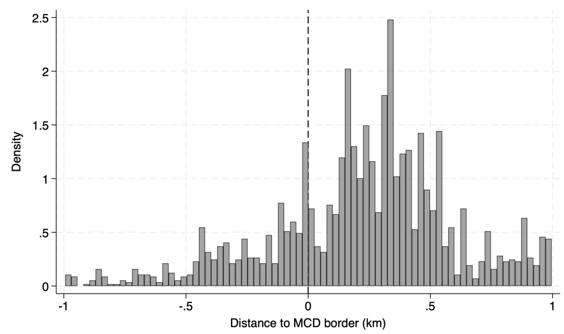
[10]In Appendix C.2, we relax our empirical strategy by including those MCDs in the analysis that had both Republican and Democrat judges. For this exercise, we consider an MCD as treated if the share of republican judges was larger than 50%.

Figure 7: Continuity at Cutoff - Rental Dataset

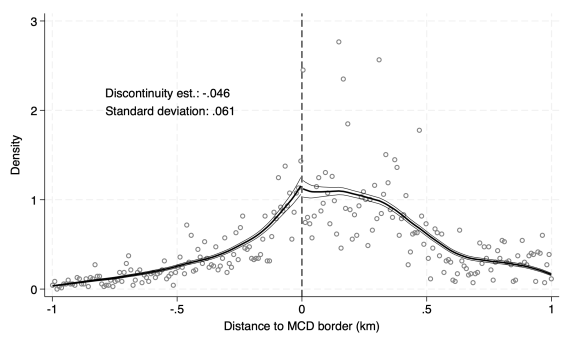
(a) Density test - 1918-1920



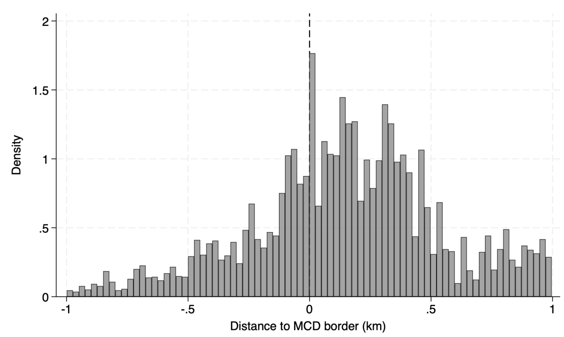
(b) Histogram - 1918-1920



(c) Density test - 1921-1926



(d) Histogram - 1921-1926



Note. Figure 7 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) show 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 assess whether there is a discontinuity in the density of properties at the MCD boundary.

4.3 Event studies

We augment our RDD baseline with an Event Studies specification, by analyzing whether the relationship between rent control and market outcomes varies with the intensity of rent control. In line with the conceptual framework above, we test whether the likelihood of facing a pro-landlord judge incentivizes landlords to increase rents. Specifically, 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 is consistent with the probability of encountering a pro-landlord judge (p), as described above, while the second measure captures something closer to the marginal effect on rents of an additional Republican judge. We use the binary treatments from the RDD in order to check for consistency of results.

Equation 2 gives our event study specification specification:

$$y_{i,m,t} = \sum_{\tau} \beta_\tau \cdot post_{1920} \cdot T_{t,u}(\tau = t - 1920) + \mathbf{X}_{i,m,t} + \gamma_t + \theta_m + u_{i,m,t} \tag{2}$$

where again $y_{i,m,t}$ is the listed rent for observation i in MCD m in year t. The variable $T_{t,u}$ denotes treatment, for which we use one of the two measures mentioned above. We compare the effects of our continuous treatments to the year of rent control implementation in 1920. Dwelling level controls are included in $\mathbf{X}_{i,m,t}$, as per Equation 1, while γ_t and θ_m are time and district-level fixed effects; MCD fixed effects control for otherwise unobserved differences across Court districts. We cluster standard errors at the neighborhood level.

In our event study set-up, our identifying assumption is that, in absence of rent control, the intensity would not matter for rents. In other words, without rent controls, other things being equal, rents in all-Republican or mixed MCDs (i.e. with at least one Republican judge) would have moved parallel to those in all-Democrat districts.

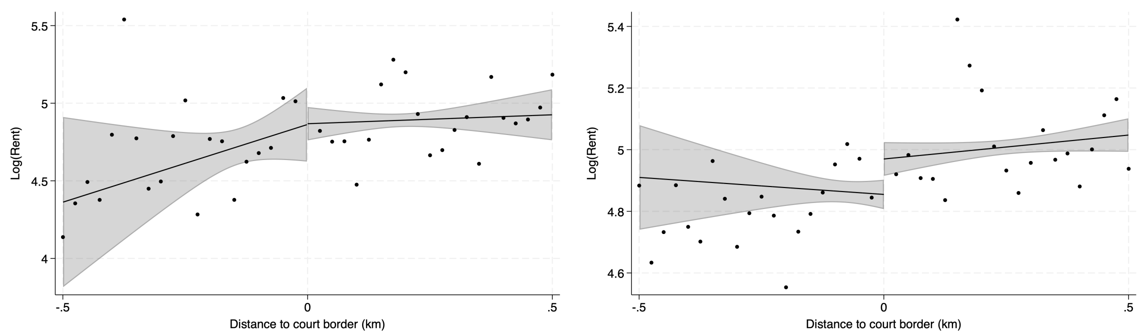
5 Analysis

5.1 Regression Discontinuity

We begin by estimating the RDD equation, Equation 1. A summary of the main results of the RDD is given in Figure 8: Panel 8a shows a smooth relationship of rental prices at the cutoff before the introduction of rent control in April 1920, while Panel 8b shows that, in the rent control period (from April 1920 to May 1926), rents jump discontinuously at the border between MCDs of different judge types. At first pass, in other words, market rents are higher—other things being equal—in all-Republican MCDs.

Figure 8: Effect at cut-off on market rents (RDD)

(a) Before controls (Jan 1918-Mar 1920) (b) During controls (Apr 1920-May 1926



Note: Figure 8 shows the binned scatterplot relationship between rental prices and the RDD running variable (distance to nearest MCD border) using 25 meter bins; Panel (a) shows the relationship before the introduction 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 1; we used a bandwidth of 500m; the shaded area show 95% confidence intervals; standard errors have been clustered at the neighborhood level.

We can examine these results in more detail, presenting regression results in Table 2 and Table 1. These regression results are the output of Equation 1 being estimated for subsamples before and after the introduction of rent control in April 1920. Each table has two panels, one for a linear function and one for quadratic, and four columns. The first column uses the optimal bandwidth, $\hat{b}$, calculated using the Imbens and Kalyanaraman (2012) algorithm, but does not include any controls other than year and NTA FEs. The second column adds dwelling-level controls (as described earlier). The third and fourth columns use half and double the optimal bandwidth, as calculated, to check if effects vary by bandwidth choice.

We start with our period of interest, when Rent Control was in full effect, April 1920–May 1926. Table 1 presents the results of estimating Equation 1 for the sample of listings during the Rent Control period. In each of the eight columns, the coefficient is statistically significant and positive, meaning that there was a jump in rents during the Rent Control period, crossing from an all-Democrat MCD to an all-Republican one. This is true whether a linear or quadratic function is chosen, with and without controls, and whether the bandwidth used is the optimal, half that or twice that. Further, the estimates are similar in magnitude, suggesting a jump of between 10% and 14% at the boundary.

Table 1: Effect at cut-off on rents during Rent Controls (Apr 1920– Nov 1926)

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.097***	0.075**	0.036	0.083***	0.109*	0.089**	0.045	0.090***
	(0.033)	(0.034)	(0.044)	(0.025)	(0.056)	(0.041)	(0.049)	(0.029)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	1.004	0.716	0.358	1.432	1.040	1.375	0.687	2.749
Obs.	9039	8688	8688	8688	9039	8688	8688	8688
R2	0.137	0.304	0.313	0.296	0.137	0.296	0.304	0.294
ci_l_rb	0.021	-0.001	-0.190	0.007	-0.008	0.001	-0.168	0.012
ci_r_rb	0.167	0.145	0.151	0.164	0.244	0.177	0.159	0.175

Note. Table 1 reports regression results for rents using the Rent Control period (April 1920–May 1926); the running variable is the distance from a property to the treatment boundary as shown in Figure 6. 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, $2\hat{b}$, 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 errors in parenthesis have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

Our pre-Rent Control period, from January 1918 to March 1920, sample acts as a placebo test: there should be no relationship between judge composition and rents at the boundary of MCDs in a period when judges have no control over rents.

This is supported by the empirical analysis. Unlike in the Control period, for the pre-Control period (Table 2), there is no evidence of any statistically significant change in market rents at the boundary between all-Democrat and all-Republican MCDs. This is true across all eight specifications: while point estimates vary from 8% to -10%, they are noisy and not statistically significantly different from zero.

Table 2: Effect at cut-off on rents before Rent Controls (Jan 1918– Mar 1920)

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.022	0.038	-0.046	-0.001	0.002	0.003	-0.152	-0.018
	(0.117)	(0.133)	(0.162)	(0.101)	(0.204)	(0.158)	(0.226)	(0.118)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.617	0.469	0.235	0.938	0.722	0.834	0.417	1.668
Obs.	2081	1983	1983	1983	2081	1983	1983	1983
R2	0.152	0.438	0.532	0.413	0.153	0.417	0.461	0.409
ci_l_rb	-0.273	-0.255	-0.430	-0.281	-0.437	-0.318	-0.672	-0.311
ci_r_rb	0.266	0.298	0.465	0.320	0.458	0.321	0.429	0.299

Note: Table 1 reports regression results for rents using the pre-Rent Control period (January 1918–March 1920); the running variable is the distance from a property to the treatment boundary as shown in Figure 6. 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 errors in parenthesis have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

Robustness We test if the effect varies when mixed districts are included and consider a MCD as Republican controlled if the share of Republican judges is larger 50%. We estimate Equation 1 using the same set-up as above. Results are given in Appendix C.2, Table C.3 and Table C.4. As above, there is no evidence for any significant effect of the border before introduction of rent control. For the control period, the broad pattern of results holds, although smaller bandwidth choices render the effect insignificant. We also test for the sensitivity of outcomes to different RDD parameter choices. Appendix C.3 shows that treatment effects are highly stable in magnitude across bandwidths choices before and during rent control (Figure C.1). For each bandwidth choice rent prices after the introduction of rent control are higher by the same factor. Panel C.1c and C.1d in particular show that estimates become significant a bandwidth larger than 300 meters.

5.2 Event Study

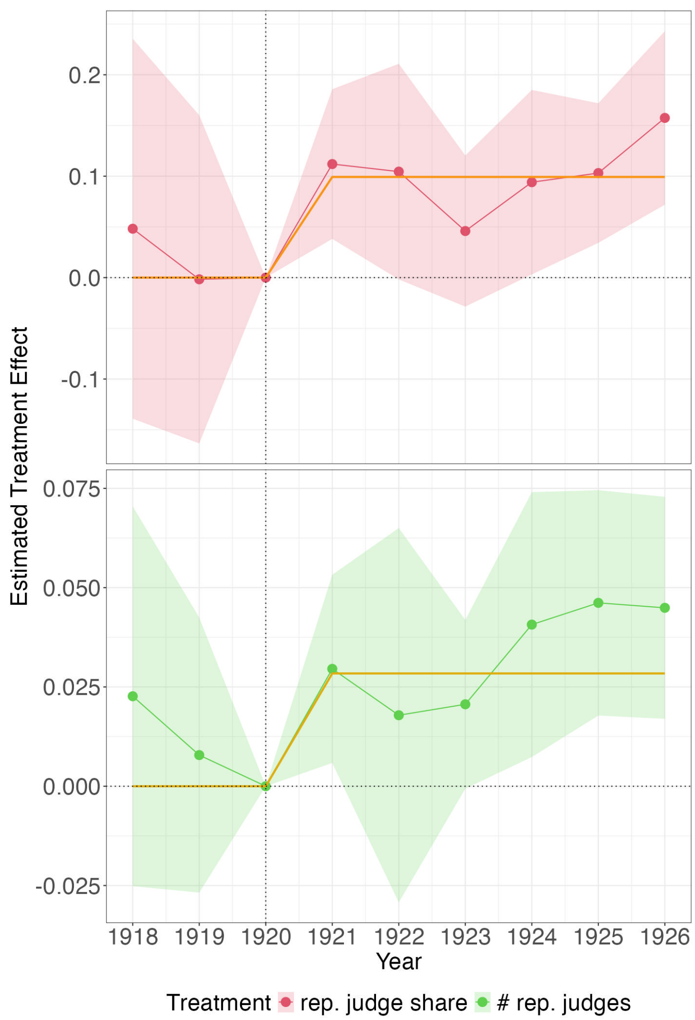
Here, we undertake the Event Study set-up outlined in Section 4.1, in particular Equation 2. As outlined in that section, landlords will seek the market rent if they face a pro-landlord judge with certainty, and they will set the controlled rent if they are sure they will face a pro-tenant judge. If the probability of facing a pro-landlord judge is between 0 and 1, the landlord's choice will depend on the actual payoffs and the cost of the lawsuit. An event-study approach allows for this and for this we use two different measures of treatment at the intensive margin. The first is the share of republican judges within an MCD. Assuming random allocation to cases, this closely proxies the probability of encountering a landlord judge. Second, we use the number of republican judges, which corresponds to the appearance of a judge in a district and his public image, thus signaling the landlord the presence of a landlord judge.

Results from estimating Equation 2 for our rent data are shown in Figure C.2. Again, we find a convincing effect of rent control on rental prices. The difference in market rents between MCDs that are controlled by 0% and 100% by Republican averages at 10%, which closely matches the results we report in Table 1. An additional Republican judge increases rental prices by about 3%. Given that there are on average two Republican judges in an MCD, this would mean 6% higher rents in a typical mixed district. These results are confirmed by using the binary treatments from the RD design in Appendix C.4. The point estimate averages at 10.7% and 8.8% for the Republican-only treatment and majority-Republican treatments while there is no evidence for pretends in rents using either treatment.

5.3 Persistence of Effects

As described earlier, the height of rent control was from 1920 to 1926. In May 1926, all previously controlled properties that were put on the market or which had rents paying more than 20$ per room per month were uncontrolled and in 1928, proper-

Figure 9: Effect of Continuous Treatments



Note. Figure C.2 reports point estimates for β_τ in Equation 2 using the full set of property level controls, year and neighborhood fixed effects. Year dummies are 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 shows the estimated 95% confidence bands, and the orange line plots the aggregated average from the simple interaction between treatment $T_{t,u}$ and an indicator variable $\mathbb{1}(t > 1920)$.

ties renting for more than 10$ per room were uncontrolled. The laws were not renewed in 1929 and expired. This section tests whether rent control's effects lasted beyond their existence, using a dataset of just over 5,000 listings from 1930. Using the same geocoding techniques as described in Section 3, we match those properties to the municipal court district between 1920 and 1926 and take the distance to their respective court border, which we use as a placebo treatment. We show results from estimating Equation 1 in this setup in Table 3.

Table 3: Effect at cut-off on market rents after Control (1930)

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
rdest	0.328***	0.041	-0.064	0.072	0.303*	-0.017	-0.082	0.059
	0.095	0.041	0.071	0.042	0.129	0.061	0.080	0.054
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	582.966	441.717	220.858	883.434	1274.329	850.755	425.377	1701.509
Obs.	5216.000	5077.000	5077.000	5077.000	5216.000	5077.000	5077.000	5077.000
R2	0.205	0.602	0.635	0.590	0.218	0.592	0.606	0.570
ci_l_rb	0.079	-0.099	-0.201	-0.113	0.021	-0.169	-0.288	-0.145
ci_r_rb	0.558	0.105	0.250	0.125	0.569	0.086	0.162	0.128

Note. Table 1 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 6. 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 errors have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

This exercise reveals that there is no evidence that rent prices jump at the border in previously Republican-controlled districts across all specifications. In both linear and quadratic set-ups, there is a difference in rents, when no controls are included, but this effect disappears once controls are included. Across all specifications with controls, the coefficient is noisy and not statistically significant from zero. Thus, the effect of rent control disappeared with its abolition. Similar to the regression for the pre-Control period, these results from after aid a causal interpretation of the results during the Control period. However, given they date from after, they also suggest that the duration of controls did not lead to any income sorting in the rental sector.

6 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.

While the effect on rents confirms that the policy was binding, the lack of any persistent impact at the border reflects 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 do not observe sales or transaction prices, it is unclear if rent control shifted the probability of constructing different types of buildings at the boundary. This 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.

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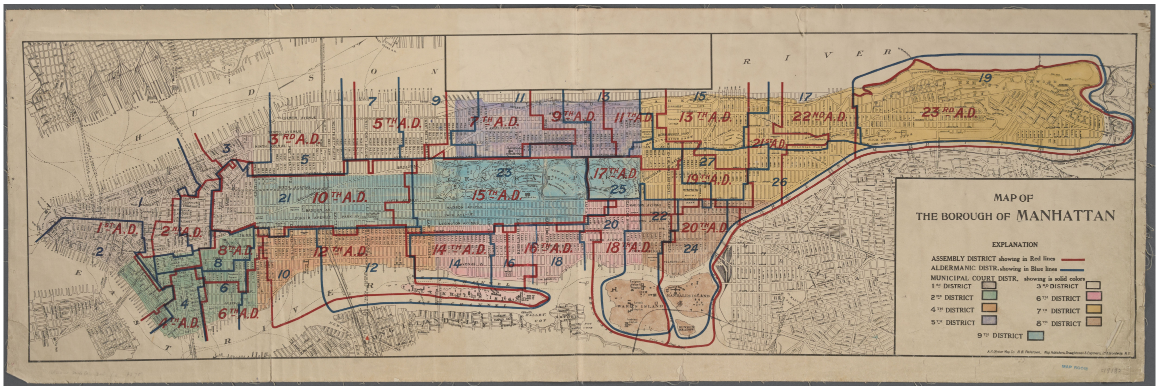
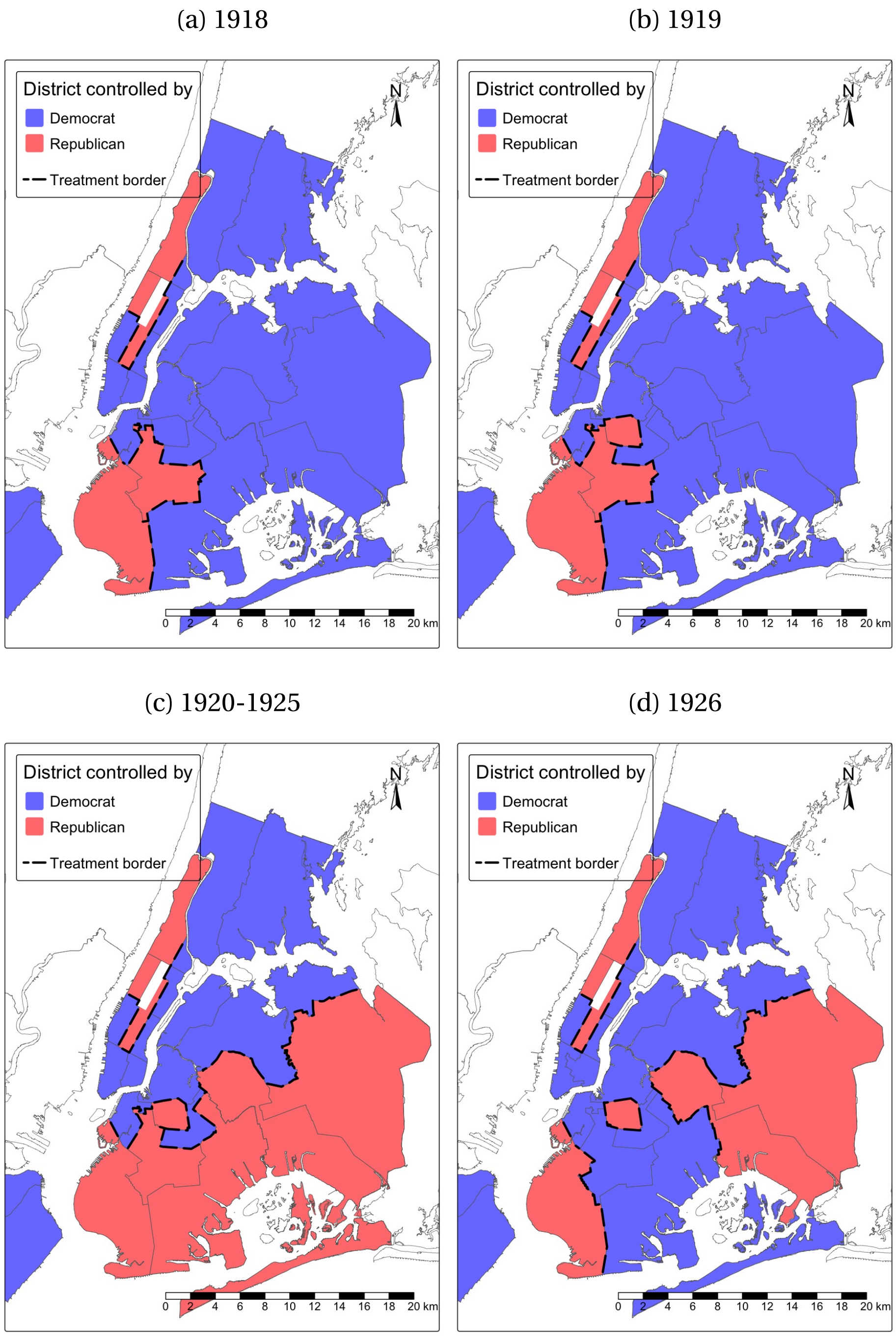
A Supplementary Maps

Figure A.1: Historical Municipal District Courts - Manhattan



Note. Figure 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.

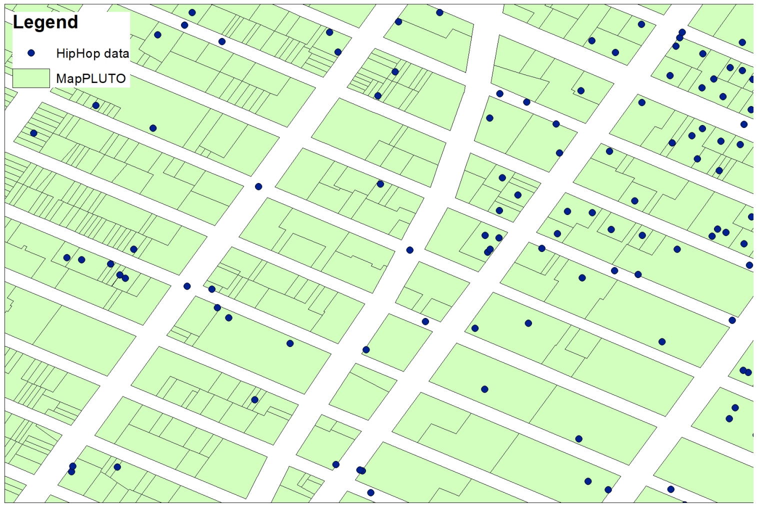
Figure A.2: Alternative treatment boundary



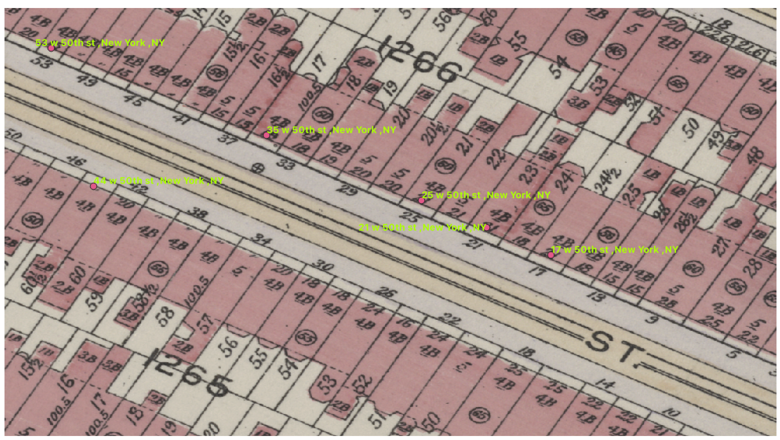
Note. Figure A.2 shows the municipal court districts (MCD) in New York City. Each district had been colored according to the political affiliation of the elected MCD judges. A district is considered as Republican controlled if the share of Republican judges within the MCD is larger than 50%; therfore 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 A.3: Example of manual geocoding

(a) PLoto 2002 lot files



(b) Bromley fire insurance maps



B Descriptive statistics

Table 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

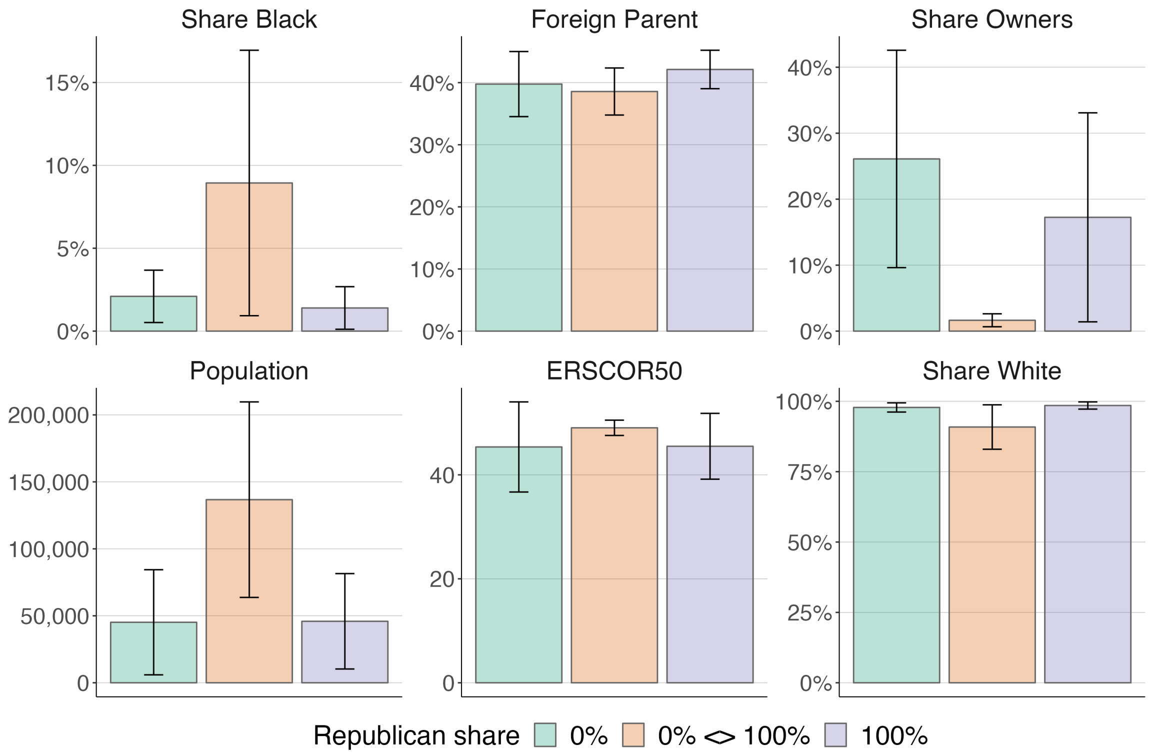
Note. Table B.1 displays the the full list of articles used to classify judge decisions in Chapter 3. It reports the name of the Newspaper as well as the classification of a judge's decisions. Eviction equals to one if a tenant was evicted and zero otherwise, rent decrease equals to one if a judge decided 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 B.2: Descriptive statistics

	1918	1919	1920	1921	1922	1923	1924	1925	1926
	Panel A: Rents								
Monthly rent	149	162	279	186	156	157	133	138	142
	(108.64)	(120.194)	(475.324)	(138.731)	(86.015)	(93.013)	(90.121)	(100.003)	(150.665)
Rooms	5	4	3	4	4	3	4	4	4
	(2.746)	(2.14)	(2.133)	(2.357)	(2.324)	(2.262)	(2.032)	(1.933)	(2.116)
N	906	1587	1037	1876	1832	1734	1984	2332	2110
	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

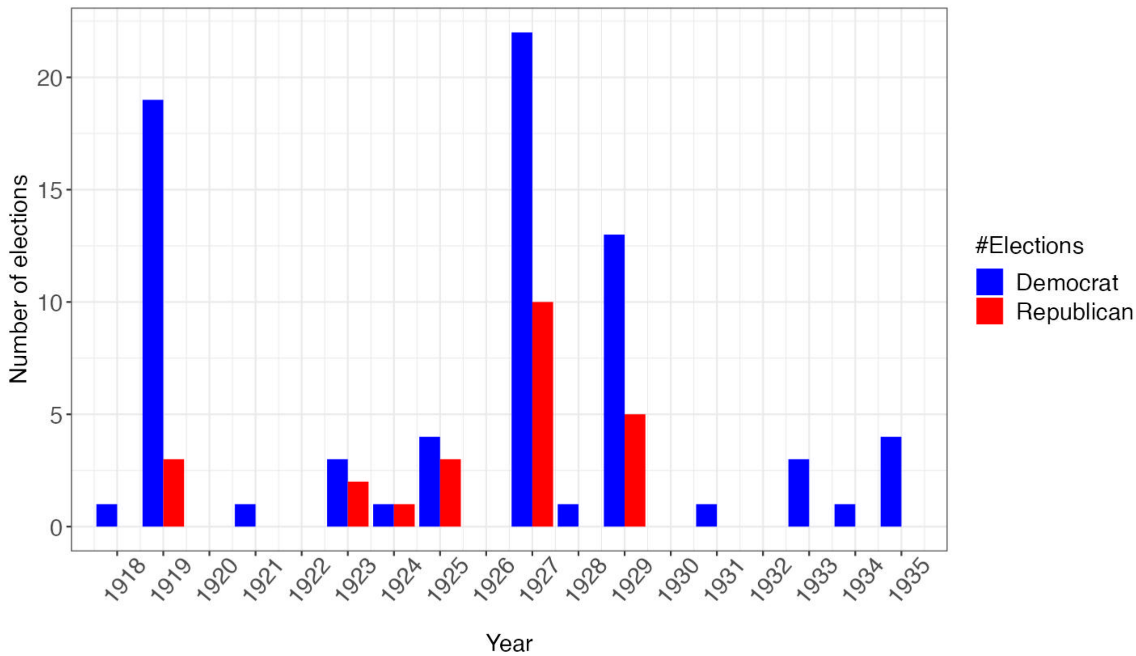
Note. Table B.2 reports means and standard deviations in parentheses. Panel A describes the main outcomes in the rent dataset. Panel B-C describes the transaction price of residential and commercial properties. Panel D displays the average number of (republican) judges by municipal court district. Totals 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 B.1: Differences across MCDs



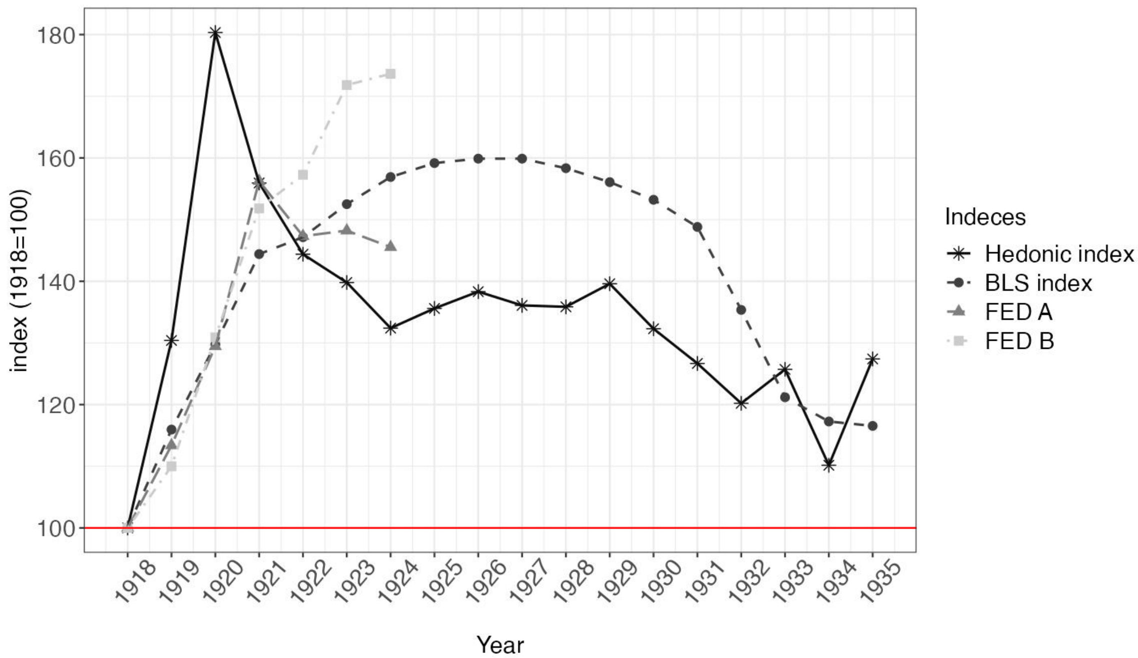
Note. The figure shows census aggregates for MCDs by share of Republican judges. Individual-level data from the 1920 decennial census were aggregated at the enumeration district level. Next, we aggregated NTA aggregates using overlapping area weights. An NTA was counted in an MCD if more than 50% of its area was within the MCD; MCDs were collapsed into 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.

Figure B.2: Judge Elections



Note. Figure B.2 shows the absolute number of elections by year. Elections have been grouped by the political affiliation of the winning judge, including winning incumbent judges. Therefore, the figure includes elections that are either changing or preserving a seat in a court.
Source. York (N.Y.) et al. (n.d.).

Figure B.3: Rent indeces

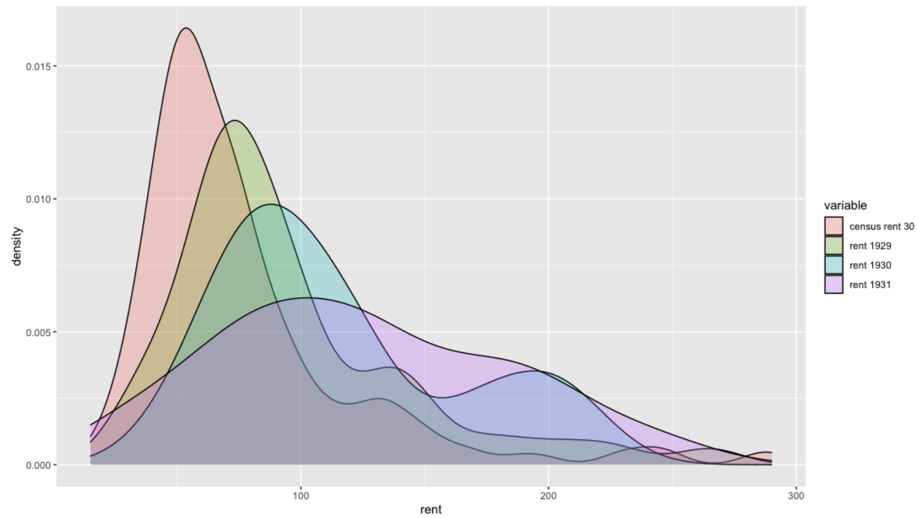


Note. Figure B.3 shows rent indexes for New York City using 1918 as the base year. The black solid line shows a hedonic index using market rents (Hedonic index). The index values have been obtained from the fixed effects of regressing the logarithm of rent on property-level controls and time-fixed effects.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

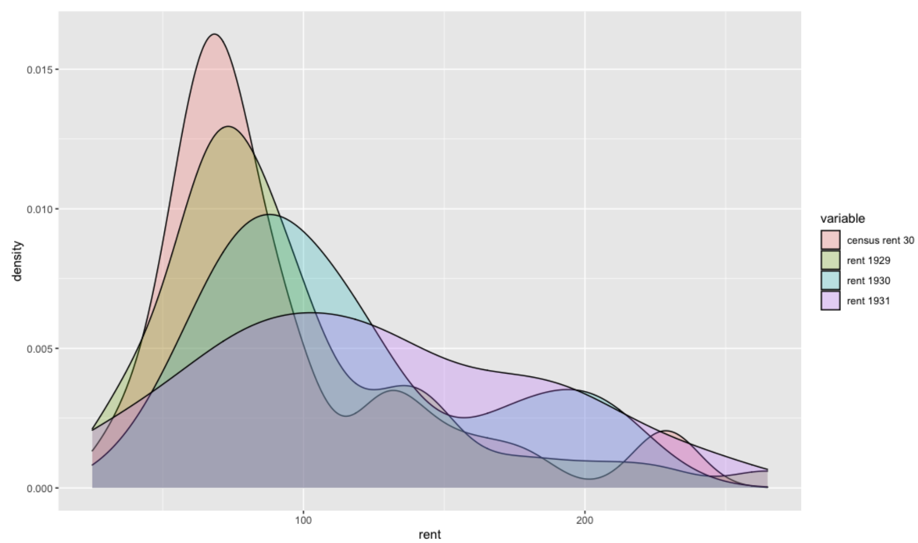
Source. Author's own calculations; United States. Bureau of Labor Statistics (n.d.); (State) (1925).

Figure B.4: Rent distributions

(a) Census and sample distribution



(b) Reweighted census distribution



Note. Figure 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 B.4a plots the rent distribution in the 1930s census vs the sample distributions from 1929 to 1931. Panel 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.

C Additional Results

C.1 RDD estimates for Manhattan

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

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	-0.057	-0.021	0.078	0.054	-0.207	-0.084	0.199	-0.012
	(0.182)	(0.168)	(0.141)	(0.131)	(0.274)	(0.246)	(0.210)	(0.177)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.307	0.244	0.122	0.488	0.376	0.362	0.181	0.723
Obs.	1881	1785	1785	1785	1881	1785	1785	1785
R2	0.450	0.511	0.602	0.412	0.428	0.466	0.551	0.390
ci_l_rb	-0.499	-0.460	-0.099	-0.475	-0.839	-0.633	0.002	-0.582
ci_r_rb	0.351	0.332	0.668	0.318	0.333	0.471	1.064	0.354

Note. Table 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 6. 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 in parenthesis been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

Table C.2: Effect at cut-off on rental prices - 1920-1926 - Manhattan

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.079	0.021	0.079	0.069*	-0.003	0.102	0.202**	0.018
	(0.069)	(0.056)	(0.067)	(0.041)	(0.127)	(0.091)	(0.093)	(0.069)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.338	0.314	0.157	0.628	0.306	0.252	0.126	0.505
Obs.	6046	5726	5726	5726	6046	5726	5726	5726
R2	0.303	0.324	0.295	0.317	0.310	0.329	0.300	0.317
ci_l_rb	-0.100	-0.118	0.022	-0.070	-0.258	-0.064	-0.030	-0.165
ci_r_rb	0.236	0.139	0.381	0.147	0.284	0.319	0.428	0.232

Note. Table C.2 reports regression results for ask rents; the data had been subsetted for the rent control period Apr 1921- Nov 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 6. 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 errors in parenthesis have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

C.2 RDD estimates for alternative treatment boundary

Table C.3: Effect at cut-off on rental prices - 1918-1920 - alternative boundary

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.040	-0.017	-0.134	-0.029	0.059	-0.044	-0.162	-0.068
	(0.103)	(0.100)	(0.104)	(0.077)	(0.163)	(0.110)	(0.143)	(0.083)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.600	0.451	0.225	0.901	0.759	0.893	0.447	1.787
Obs.	2738	2624	2624	2624	2738	2624	2624	2624
R2	0.186	0.469	0.541	0.442	0.185	0.444	0.476	0.426
ci_l_rb	-0.211	-0.245	-0.293	-0.263	-0.301	-0.269	-0.479	-0.300
ci_r_rb	0.245	0.162	0.143	0.179	0.419	0.161	0.125	0.115

Note. Table C.3 reports regression results for ask rents; the data had been subsetted for the pre rent control period Jan 1918- Mar 1920; the running variable is the distance from a property to the treatment boundary as shown in Figure A.2. 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 C.4: Effect at cut-off on rental prices - 1920-1926 - alternative boundary

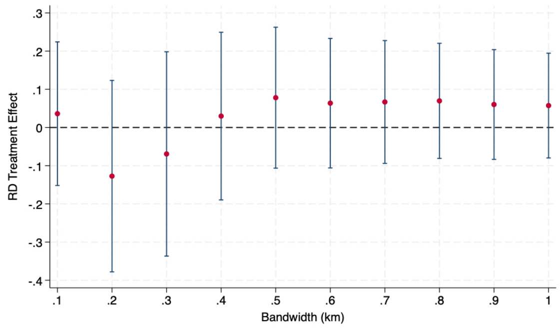
	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.108***	0.066	0.010	0.097***	0.118**	0.085*	0.045	0.118***
	(0.032)	(0.036)	(0.055)	(0.026)	(0.040)	(0.040)	(0.047)	(0.029)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	1.019	0.601	0.301	1.202	1.720	1.461	0.730	2.921
Obs.	12612	12192	12192	12192	12612	12192	12192	12192
R2	0.136	0.307	0.321	0.298	0.134	0.293	0.307	0.276
ci_l_rb	0.047	-0.018	-0.196	-0.007	0.038	-0.006	-0.146	0.034
ci_r_rb	0.190	0.141	0.196	0.164	0.218	0.177	0.164	0.192

Note. Table C.4 reports regression results for ask rents; the data had been subsetted for the rent control period Apr 1921- Nov 1926; the running variable is the distance from a property to the treatment boundary as shown in Figure A.2. 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 errors in parenthesis have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

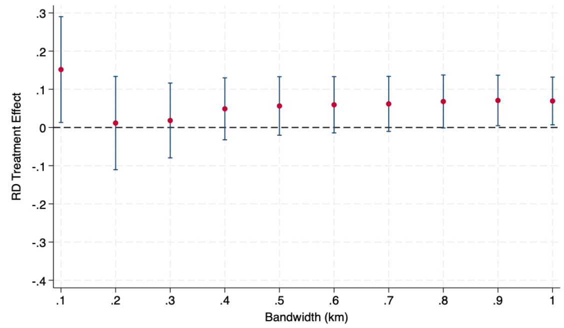
C.3 RDD estimates for Alternative bandwidth choices

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

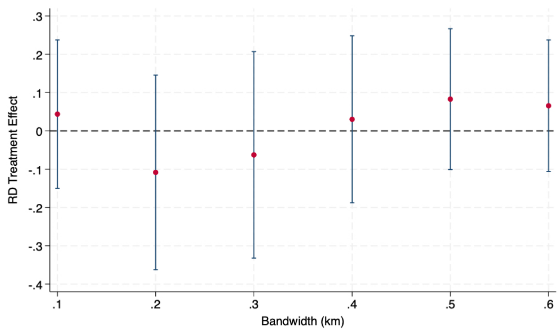
(a) Republican only MCD 1918-1920



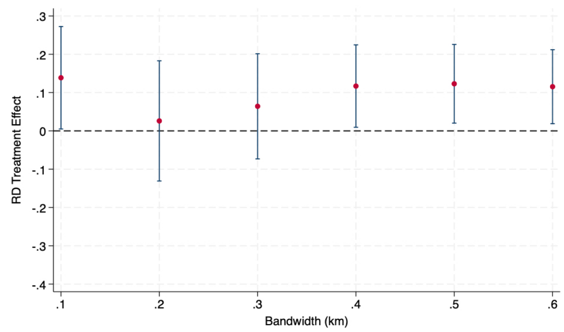
(b) Republican only MCD - 1921-1926



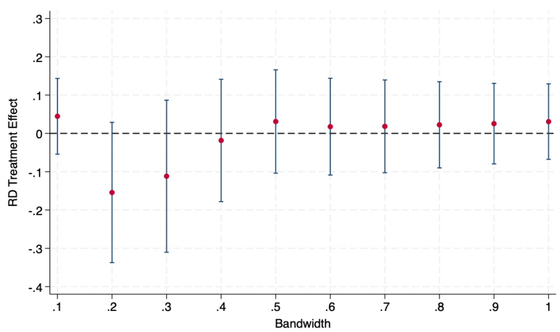
(c) Republican only MCD - Manhattan - 1918-1920



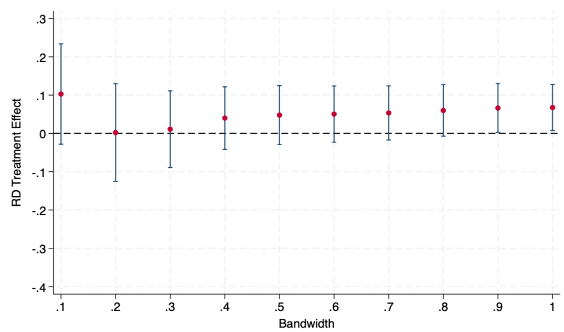
(d) Republican only MCD - Manhattan - 1921-1926



(e) Majority Republican MCD - 1918-1920



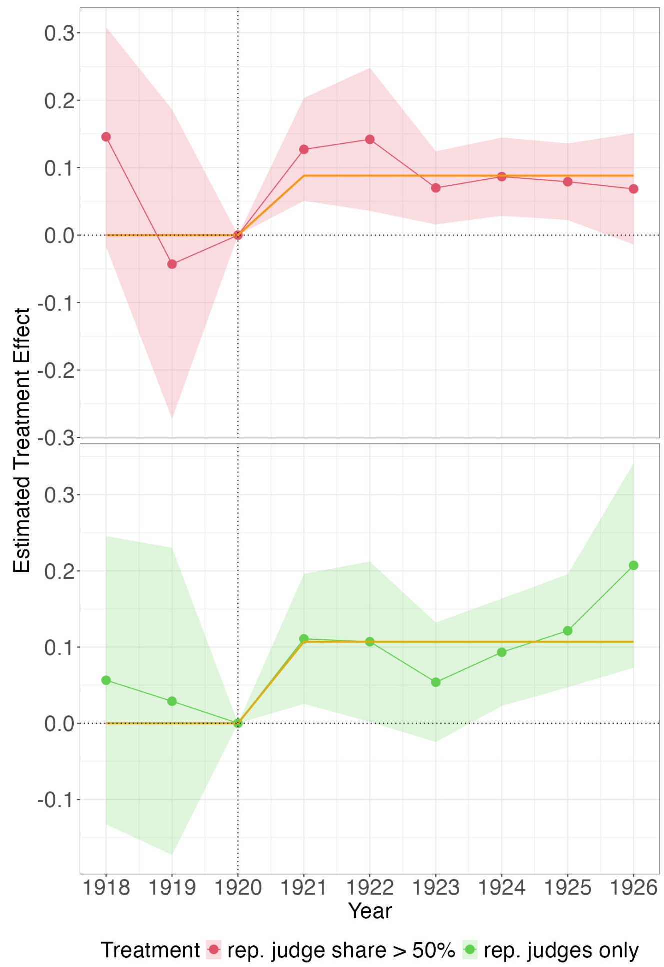
(f) Majority Republican MCD - 1921-1926



Note. Figure C.1 shows RD estimates from estimating Equation 1 for different bandwidth choices using the full et of property level controls, year and neighborhood fixed effects; Equation 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 C.1a and C.1b use the distance to the boundary between Republican and Democrat only MCDs; Panel C.1c and C.1d subset the sample for Manhattan only; Panel C.1e and 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.

C.4 Event study results

Figure C.2: Effect of Continuous Treatments



Note. Figure C.2 reports point estimates for β_τ in Equation 2 using the full set of property level controls, year and neighborhood fixed effects. Year dummies are interacted with a dummy equal to one if (1) the share of Republican judges in MCD u was larger than 50%, or (2) the MCD u was either controlled by republican judges only or not at all - thereby excluding all mixed districts. Standard errors are clustered at the neighborhood (NTA) level. The shaded area shows the estimated 95% confidence bands and the orange line plots the aggregated average from the simple interaction between treatment $T_{t,u}$ and an indicator variable $\mathbb{1}(t > 1920)$.