

Judge for Yourself? The Impact of Controls on the Rental Market in Interwar New York^{*}

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Abstract

This paper examines the impact of early 20th-century rent control laws in New York City, exploiting judicial discretion as a source of variation. The 1920 regulations empowered municipal court judges to decide whether rent increases were “reasonable,” with rulings shaped by partisan affiliation. We assemble a new dataset of over 20,000 rental listings from the New York Times (1918–1930) and more than 7,000 archival building permits, linked to records on 125 district judges. Using a Regression Discontinuity Design at municipal court district boundaries, we find that market rents rose by nearly 10% when crossing from Democrat- to Republican-controlled districts after rent control. To study supply effects, we implement a difference-in-differences analysis and show that residential investment was 65–85% higher in landlord-friendly districts. Together, these findings demonstrate how judicial discretion shaped both prices and investment, leading to systematic differences in profits and construction activity across districts with varying judicial composition.

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, which in the U.S. rose by 70 percent between 2010 and 2024, compared to an increase of 45 percent in the overall CPI.¹ In response, many jurisdictions have revisited the use of regulatory measures that limit allowable rent increases. Despite their increasing popularity, however, the economic costs and benefits of these regulations remain a subject of considerable debate among both scholars and policymakers.

Rent controls were widely used internationally after both World Wars, in the context of severe housing shortages and high inflation. In this paper, we study the introduction of rent controls in New York City in 1920, then the world's largest city. These regulations combined modern "just cause eviction" provisions with direct price-setting authority, empowering elected municipal judges to determine whether rent increases were "reasonable." In practice, this discretion led to sharp ideological divisions: some judges openly identified as "tenant judges," others as "landlord judges" (Rajasekaran et al., 2019; Fogelson, 2013). The institutional setting therefore offers a rare opportunity to examine how political and judicial heterogeneity translated into market outcomes.

We develop a simple two-period theoretical framework to highlight how judicial discretion can shape both market rents (in the short run) and investment in housing (over time). The core of the model is that landlords face litigation risk when attempting to raise rents: if a case landed before a pro-tenant judge, the increase could be rolled back to a lower cap, leaving landlords with costs and lost rents. We show that equilibrium rents rise monotonically in the probability of facing a pro-landlord judge, with pro-tenant courts pushing rents closer to the cap and pro-landlord courts sustaining market increases. As housing is a long-lived asset, developers anticipate these judicial frictions. Embedding landlords' decision rules into a two-period investment model, we show that judicial composition affects expected returns. The more pro-tenant a court, the lower returns and thus the lower investment in new residential construction. The framework delivers two testable predictions: relative to districts where tenant-friendly judges dominate, both market rents and residential investment will be higher in pro-landlord districts.

To take these predictions to the data, we combine three new sets of historical microdata. First, we digitize and geo-code over 20,000 individual rental listings published in the *New York Times* between 1918 and 1930 recording exact addresses, listed rents, and property characteristics. Second, we collect 7,062 building permits from the Metropolitan History Archive, covering virtually all construction projects in Manhattan between 1918 and 1931.

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With information on usage, scale, construction materials, and projected costs, these data offer a window into investment behavior. Finally, we compile information on all 125 municipal court judges in this period, from the NYC Official City Directory, including affiliation, election cycles, and jurisdictional boundaries.

We estimate, firstly, the effect of rent control on market rents using a Regression Discontinuity Design (RDD) at the boundaries between pro-tenant (Democratic) and pro-landlord (Republican) municipal court districts, using our dataset of geo-coded rental listings. We find strong evidence of a discontinuity, with a jump of about 9 percent in rents when crossing from Democratic into Republican districts, consistent with the theoretical framework. Importantly, these discontinuities are absent in the pre-control period (before 1920) and again in 1930, after the controls had lapsed.

Second, we use the digitized building permits to examine the investment response, using a difference-in-differences design with judicial variation at the district court level as a measure of treatment intensity. Since only residential returns were impacted by rent control, we compare residential to non-residential investment. We focus on the intensive margin of investment, that is, the amount of investment in projects (conditional on being permitted), rather than the number of permits. We find that, during the rent control period, adding an additional landlord-friendly judge is associated with a 13 to 17 percent increase in residential investment. Put differently, investment per residential building was about 75 percent higher for residential-only projects in landlord-friendly districts compared to non-residential and mixed-use ones. This pattern is consistent with our hypothesis: the return to mixed-use investment depends on the relative profitability of the residential versus the non-residential component. Lastly, we find supportive evidence in the 1940 Census where, controlling for 1920 conditions, areas with more Republican judges added more homes 1920–1940 than other areas.

Our paper relates to two main strands of the economics literature on rent control, as well as to the literature on judges and their decision-making. The first is the large literature on the price effects of rent control. Reviews such as Kholodilin (2024) show that rent controls typically suppress rents for protected tenants (e.g. Olsen, 1972; Linneman, 1987), but often raise rents in the uncontrolled sector (Early and Olsen, 1998) and reduce mobility and housing quality (Svarer et al., 2005; Diamond et al., 2019; Sims, 2007; Sagner and Voigtländer, 2023). Effects are strongest when controls are more stringent (Fetter, 2016; Early, 2000; Breidenbach et al., 2019). We contribute to this literature by providing the first dwelling-level evidence for a U.S. city in the interwar period and by showing how judicial discretion in enforcing rent laws created sharp distortions in both prices and expectations.

The second strand concerns the supply response to rent control. Most studies document substantial negative effects: Monràs and García-Montalvo (2025) find a 10 percent fall

in rental supply in Spain, while in the U.S., repeal in Cambridge spurred new investment (Autor et al., 2014), and expansion in San Francisco reduced supply via conversions (Diamond et al., 2019). A rare exception is Jofre-Monseny et al. (2023), who find little evidence of reductions in tenancy agreements in Barcelona, though they document anticipation effects and a decline in sales. Recent evidence from Germany (Baye and Dinger, 2024) shows that controls reduced yields on regulated units and shifted investment toward unregulated, higher-end segments, fueling gentrification. Our findings closely parallel these results: in 1920s New York, judicial discretion was associated with pronounced differences in investment patterns across districts, with relatively higher residential construction in areas with more landlord-friendly judges. By focusing directly on investment flows using building permits, rather than contracts or occupancy, we provide new evidence on how enforcement mechanisms influenced the spatial allocation of housing investment within the city.

The third strand of literature concerns judges and their decision-making. Prior work shows that elected judges behave systematically differently from appointed ones: they hand down longer sentences (Gordon, 2007; Lim, Snyder, and Strömberg, 2015), and partisan judicial elections often mirror partisan politics, with voters using party labels as the main signal guiding voter choice (Lim, Snyder, and Strömberg, 2015; Lim and Yurukoglu, 2018). Judges' political affiliation has also been shown to shape decisions in areas such as utility regulation and criminal justice (Mueller-Smith, 2015). We add to this literature by showing that partisan affiliation mattered not only for sentencing or regulatory rulings, but also for the enforcement of rent control. In our setting, judicial ideology directly translated into differential rent outcomes and, in turn, into investment incentives.

The remainder of the paper begins with Section 2, which presents the historical and institutional background of our setting—rent controls in 1920s New York. Informed by this, we outline in 3 our conceptual framework, giving us predictions from theory, in relation to rents and investment in new rental housing, that we can bring to our setting. Section 4 introduces our newly-assembled datasets on municipal court judges, on market rents, and on building permits, while Section 5 describes the empirical strategies we use when combining our data with the predictions of our models. In Section 6, we present our results: first the effect of rent control on market rents, using a regression discontinuity design; and second, the impact on investment and supply, using a difference-in-differences framework and building permits data.

2 Historical and Institutional Context

By the early 20th century, New York City had grown to become one of the largest cities in the world, 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 housing market, particularly after the U.S. entered the conflict. In 1918, less than \$40m of new construction projects were authorized, down nearly 80 percent from almost \$200m in 1916. With little new supply, a rapidly rising population, and returning troops after the war's end, the housing vacancy rate fell from 5.6 percent in March 1916 to just 0.2 percent 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 percent between 1916 and 1920. Individual examples reinforce this citywide trend. For instance, the monthly rent for a small four-room apartment increased by 125 percent from \$18.50 in June 1919 to \$42 in 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).

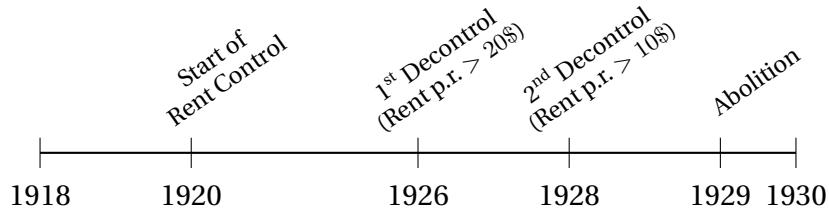
Such sharp increases in rents prompted responses, first from tenant unions, through rent strikes, and later from policymakers. The state government implemented rent control laws initially in April 1920, amending them in September (Fogelson, 2013). The regulation declared that rent increases above 25 percent per year were “unjust, unreasonable, and oppressive,” effectively discouraging them. However, the ultimate decision in relation to any proposed increase fell to Municipal Courts. Judges for each Municipal Court District (MCD) could decide whether any proposed increase that before them was ‘reasonable’ and also whether an eviction warrant was applicable. Judges could grant stays of up to twelve months and strike down rent increases that were judged unreasonable. 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 noted by Fogelson (2013), contemporary observers remarked that municipal court judges wielded unprecedented power, and their decisions reflected clear ideological leanings.

Figure 1 provides an overview of the timeline of rent controls. They were started in 1920 and abolished in 1929. The eventual abolition reflected broader trends in housing prices 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

²A 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.

nearly half in the space of a decade, rents in the open market peaked in 1920 and had fallen by 28 percent by 1930 (Lyons et al., 2024).³ The “Emergency” rent laws faced persistent criticism from their inception, particularly from real estate interests such as the Greater New York Taxpayers Association. Governor Al Smith appointed an advisory commission on rent controls, the so-called “Stein Commission”, which recommended extending the laws in 1923. In 1925, however, as market conditions changed, it recommended “luxury decontrol”, i.e. removing the top end of the rental market from the regulations (Fogelson, 2013); as a result, 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 1920s New York 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. Further, as confirmed by a State Supreme Court decision in April 1921, rent controls applied across tenancies: landlords were not allowed to “exact from new tenants a rental in excess of that paid by a former tenant” (New York Times, 1921). This provides clear evidence that rent controls were both widely enforced and binding across the rental market, serving as a credible constraint on landlords' behavior. Further, and in line with the social history of tenant organizations and rent strikes during this period, we believe rent controls were well understood across the rental market, and not limited to (for example) higher-income or English-speaking tenants (Fogelson, 2013).

At any one time, there were between 45 and 53 Municipal Court judges in the city. These 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

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

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 that judges likely relied on prior beliefs, including ideological predispositions, when ruling quickly. Judges were also prominent public figures, with their appearances, opinions, and rulings 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 Jacob Strahl of the 4th District Court in Brooklyn, were widely regarded as "tenants' friends." 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.⁴ 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) (Fogelson, 2013). Unsurprisingly, as Fogelson (2013) documents, the public perceived a clear divide between "pro-tenant" and "pro-landlord" judges—a distinction central to our empirical strategy.

3 Conceptual Framework

Rent Control under Judicial Uncertainty In this section we develop a simple framework to understand the impact of rent controls when operated through judicial rulings. At its core is the feature that the regulation did not mechanically fix rents, as in some other rent control systems, but instead delegated substantial discretion to judges in municipal courts. As a result, the economic consequences of rent control depended not only on statutory rules, but also on the composition of judges assigned to resolve landlord–tenant disputes. Suppose the city is partitioned into municipal court districts $j \in J$, and that in each district market rents evolve deterministically as

$$r_{j,t} = r_{j,t-1} + g,$$

with $g > 0$ capturing the steady upward drift in rents during the postwar boom. This abstracts from short-run fluctuations, highlighting that in a growing market, the realized

⁴Move to Disbar Justice Strahl on Campaign Cartoon Showing Him as Foe of Rent Profiteer. (1922, April 21). The Evening World.

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

rent path depends on whether landlords can have increases approved in court.

Under rent control of this nature, each district's court enforces a cap r_j^c , which we treat as district-specific and potentially arbitrary. This reflects the historical reality of the early 1920s, when municipal judges had wide discretion in interpreting the rent laws and could reset rents to different benchmarks. In some districts the cap was close to prevailing market rents; in others it was set much lower. In addition, when enforcement was triggered, landlords faced a common litigation cost c_ℓ .⁶

When a landlord attempts to raise rents, the outcome depends on judicial composition. With probability p_j , the case is assigned to a landlord-friendly judge who upholds the market rent $r_{j,t}$. With probability $1 - p_j$, the case goes to a tenant-friendly judge who resets the rent to $r_j^c - c_\ell$. The expected rent is therefore

$$r_{j,t}^e(p_j) = p_j r_{j,t} + (1 - p_j)(r_j^c - c_\ell). \quad (1)$$

Proposition 1 (Landlord rent setting). *If $p_j = 0$ (all judges tenant-friendly), the landlord never raises and sets $r_{j,t} = r_{j,t-1}$. If $p_j = 1$ (all judges landlord-friendly), the landlord always raises and sets $r_{j,t} = r_{j,t-1} + g$. For interior $p_j \in (0, 1)$, the landlord raises rent in period t if and only if*

$$p_j > \frac{r_{j,t-1} - r_j^c + c_\ell}{r_{j,t} - r_j^c + c_\ell}.$$

The proof of Proposition 1 is given in Appendix A.

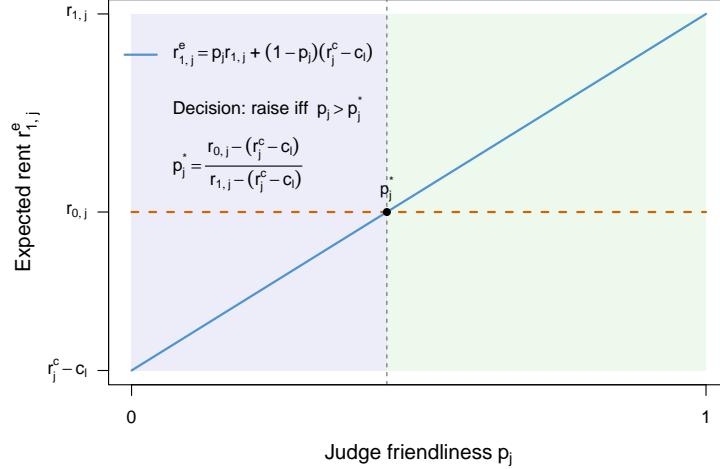
Figure 2, Panel (a), illustrates the landlord's decision. At the start of each year, the landlord compares the expected return from raising rents to $r_{j,t}$ with the safe option of keeping last year's rent $r_{j,t-1}$. When $p_j = 0$, the landlord never raises; when $p_j = 1$, they always raise. For intermediate probabilities, raising is optimal only if p_j exceeds a threshold p_j^* . This threshold depends on both the growth increment g and the district-specific cap r_j^c , as the larger the gap between market rents and the cap, the more reliant landlords are on favorable judges to justify raising rents.

Developers and investment While landlords' decisions determine short-run rent trajectories, developers face the long-run choice of how much to invest in new residential housing. A developer chooses a project scale $k \geq 0$ once, paying a convex construction cost $c(k)$ and producing housing services $h(k)$ with diminishing returns. In period 0, rents are given by $r_{j,0}$. In period 1, rent control may apply with probability q . If it does, the

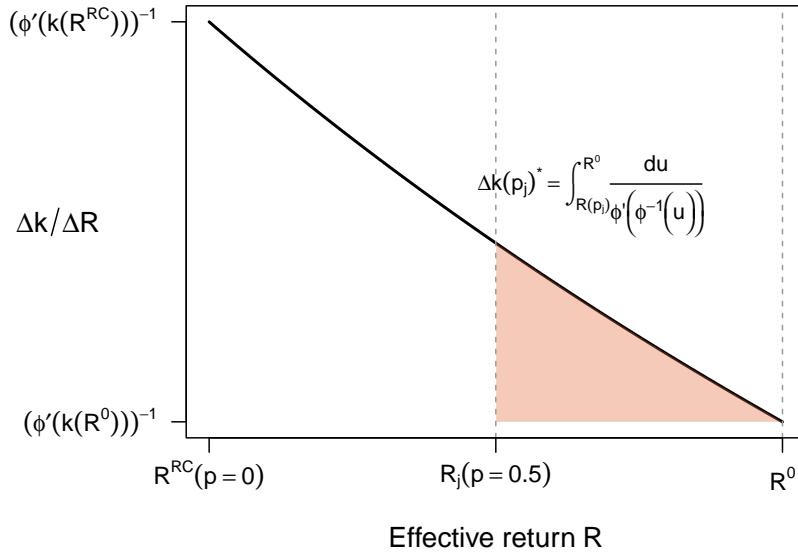
⁶We model c_ℓ as a landlord-side cost because, historically, tenants faced relatively low barriers to initiating disputes. Resistance could occur through both the courts and collective action, such as rent strikes, which were widespread across class and immigrant groups in interwar New York (Day, 1999). By contrast, landlords risked forgone rent, property damage, and legal expenses if a case was contested. While landlords may have wished to screen for "low-risk" tenants, the breadth of tenant activism meant this was rarely feasible in practice.

Figure 2: Judicial discretion and housing market responses.

(a) Landlord rent-setting decision (short run)



(b) Developer investment response (long run)



Note. Figure 2 illustrates how judicial discretion shapes rents and investment. Panel (a) shows landlords' one-period rent-setting decision: expected rents $r_{j,t}^e$ rise linearly in the probability p_j of drawing a landlord-friendly judge. At $p_j = 0$, rents collapse to the cap net of costs $(r_j^c - c_l)$; at $p_j = 1$, they reach the market rent $r_{j,1}$. A rent increase occurs only if p_j exceeds the cutoff p_j^* . Panel (b) plots the marginal investment response dk/dR against returns. The shaded area between $R_j(p_j = 0.5)$ and R^0 gives the investment shortfall $\Delta k(p_j)$ from Proposition 2. The parametrization assumes $c(k) = \frac{1}{2}k^2$ and $h(k) = k^\alpha$, so that $\phi(k) = c'(k)/h'(k)$ is strictly increasing and invertible.

return again depends on judicial composition: with probability p_j a landlord-friendly judge enforces the market rent $r_{j,1}$, while with probability $1 - p_j$ a tenant-friendly judge imposes the cap r_j^c net of litigation costs. The effective two-period return is therefore

$$R_j(p_j) = r_{j,0} + \beta \left[q r_{j,1}^e(p_j) + (1 - q) r_{j,1} \right],$$

with $r_{j,1}^e(p_j)$ defined in [Equation 1](#).

The developer's profit maximization problem is

$$\max_{k \geq 0} \pi_j(k; p_j) = R_j(p_j) h(k) - c(k).$$

The first-order condition is $\phi(k) = R_j(p_j)$, where $\phi(k) \equiv c'(k)/h'(k)$ is the cost-benefit ratio of investment. Because ϕ is strictly increasing and invertible, the unique optimal scale is

$$k_j^*(p_j) = \phi^{-1}(R_j(p_j)). \quad (2)$$

Proposition 2 (Investment gap). *Optimal investment $k_j^*(p_j)$ is strictly positive whenever $R_j(p_j) > \phi(0)$, and strictly increasing in the probability of drawing a landlord-friendly judge p_j when the cap binds. Relative to the no-control benchmark $R^0 = r_{j,0} + \beta r_{j,1}$, the investment shortfall is*

$$\Delta k(p_j) = k^*(R^0) - k_j^*(p_j) = \int_{R_j(p_j)}^{R^0} \frac{du}{\phi'(\phi^{-1}(u))}.$$

Equivalently, the more tenant-friendly the judiciary in district j (lower p_j), the larger the gap between actual investment and the no-control benchmark.

The proof of Proposition 2 is given in [Appendix A](#).

[Figure 2](#) illustrates this mechanism. Panel (b) shows how judicial composition shapes effective returns collapsing to R^{RC} under fully tenant-friendly courts and rising to R^0 under fully landlord-friendly courts. The shaded area corresponds to $\Delta k(p_j)$ in [Proposition 2](#), making clear how reduced expected returns in tenant-friendly districts translate into lower optimal investment.

In sum, the model highlights two distinct but related effects of judicial uncertainty in rent control regimes. [Proposition 1](#) shows that landlords' willingness to raise rents depends not only on statutory rules but also on the probability of drawing a landlord-friendly judge: rents remain frozen under fully tenant-friendly courts, rise steadily under fully landlord-friendly courts, and are conditionally raised only when judicial probabilities exceed a threshold. [Proposition 2](#) extends this logic to developers, showing that expected returns

on new housing are likewise shaped by judicial composition, with investment levels rising monotonically in the share of landlord-friendly judges and falling short of the no-control benchmark whenever tenant-friendly rulings are likely. Together, these results underscore how heterogeneity in local judicial enforcement maps into both short-run rent trajectories and long-run investment outcomes.

4 Data

In this section, we begin by describing our judge-level dataset, which yields our main treatments of interest in the empirical analysis. We also document evidence from newspaper articles on landlord–tenant cases linking judges’ decisions to party ideology. We then proceed by describing the construction of our two main outcomes of interest, market rents and investment. Figure C.1 provides examples of rental listings, judge details, and court cases. We provide a full summary table of the three datasets in Table C.1.

4.1 Judges

Our main hypothesis is that a judge’s party affiliation correlates with decisions on rent increases and evictions. Historically, Republicans aligned with business interests (Link, 1959) and opposed redistributive legislation (Nelson, 2001), suggesting that Republican judges favored landlords. Democrats, by contrast, drew support from a progressive urban electorate and a conservative rural base (Link, 1959), implying greater tenant support. Judges were public figures, frequently covered in newspapers for their appearances at union meetings, dinners, and festivals. Partisan elections gave them strong incentives to mobilize voters by taking positions on rent laws. Still, party lines were not absolute. New York Democrats were linked to Tammany Hall corruption, while some Republicans, such as Fiorello La Guardia, advanced social welfare policies (Williams, 2014).

Empirically, our approach builds on the literature on judges. First, the method of selection influences judicial behavior: both Gordon (2007) and Lim, Snyder, and Strömberg (2015) find that elected judges impose longer sentences than appointed ones. Second, partisan judicial elections mirror political outcomes. Lim and Snyder (2015) show that in partisan elections, the correlation between Democratic vote share in political and judicial contests exceeds 0.9, compared to below 0.5 in nonpartisan elections.

We collect information on 125 judges from the NYC Official City Directory, known as the *Green Book* (City of New York, 1918–1931). This directory provides each judge’s municipal court district (MCD), party affiliation, and re-election date. All judges in our study were politically affiliated: the vast majority were Democrats (93), followed by Re-

publicans (30), with one Liberal and one Socialist. Since judges were elected, there was variation in the distribution of Republican and Democratic judges across time and space ([Figure B.2](#)). Although a major realignment occurred in 1919, when many Republicans replaced Democrats, the rent control period (1920–1926) was marked by striking stability. Not a single MCD flipped from a Republican to a Democratic majority during these years. This stability in judicial composition is illustrated in [Figure C1.1](#), which documents the persistence of party distributions among judges.

To the best of our knowledge, historical rent case records did not survive, hindering a direct test of the link between judge decisions and party affiliation. Rather than assume judges followed partisan lines, we used newspaper archives to construct a dataset of municipal landlord–tenant cases in which the presiding judge is identified. These articles, covering 72 cases 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”. The complete list of newspapers used and the classification of judges can be found in [Table C1.1](#).

We focus on two types of cases reported in newspapers: those involving rent disputes and those concerning 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.
- The judge refused any rent increase.
- 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 3](#) and show clear evidence of differences across party lines. For eviction cases, Republican judges granted a stay in 17 percent of cases, compared to 56 percent in Democrat districts. Similarly, regarding rental reductions, Republican judges refused to reduce the rent demanded by landlords in 27 percent of cases, compared to 18 percent for Democrat judges. Finally, Republican judges allow rent increases more often than their Democrat counterparts: 45 percent of cases compared to 37 percent. Note that our measure captures only the extensive margin—the decision to increase, decrease, or maintain rent levels. Given the eviction results, it is likely that when Democratic judges allowed increases, these were smaller than those permitted by Republican judges (the unobserved intensive margin).

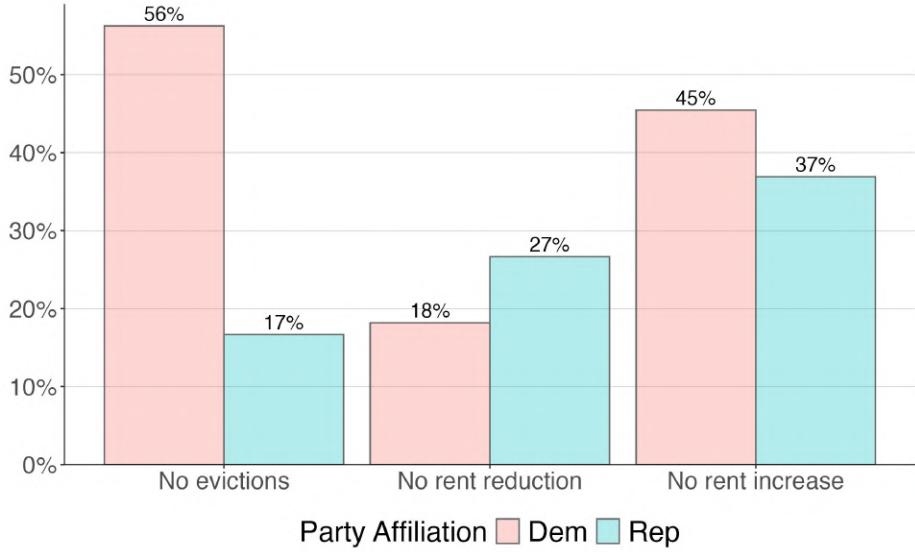


Figure 3: Judge decisions

[Figure 3](#) 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 4.2.

This dataset has clear limitations. Firstly, we observed only 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, we believe that these cases and the findings outlined above support the assumption that judges' decisions reflected their political affiliation.

4.2 Market Rents

We turn next to our first outcome of interest: rents. We collect a novel dataset on market rents in New York City from New York Times (NYT) classified listings between 1918 and 1926, with a further sample for 1930. The dataset includes 15,398 digitized rental advertisements across 80 dates 1918–1926 and 5,216 listings in 1930. Each entry contains the advertised rent, address, unit size, and property type. Full details on sampling, inclusion criteria, and digitization are provided in Appendix C2.

All listings were geocoded to historical addresses. Because street numbering and naming conventions have changed since the 1920s, automated matching alone would often

misplace properties. To address this, we combined automated geolocation with manual corrections based on historical maps and cross-street references in the listings. This procedure produces reliable coordinates for all addresses, allowing us to link each property to its municipal court district and surrounding neighborhoods.⁷

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 across the four most populous boroughs of the city: Manhattan, the Bronx, Brooklyn, and Queens. The spatial distribution of rents, shown in [Figure 4](#), is consistent with well-known patterns: lower coverage in the Lower East Side and higher in the Upper East and West Sides. We establish that while average rents in the sample exceed those reported in the 1930 Census, this difference stems from the neighborhoods more frequently represented in the NYT listings rather than systematic bias.⁸ In Appendix C2, we further validate the dataset by showing that our rent indices closely track other historical benchmarks, underscoring both its representativeness and reliability for studying rental dynamics during this period.

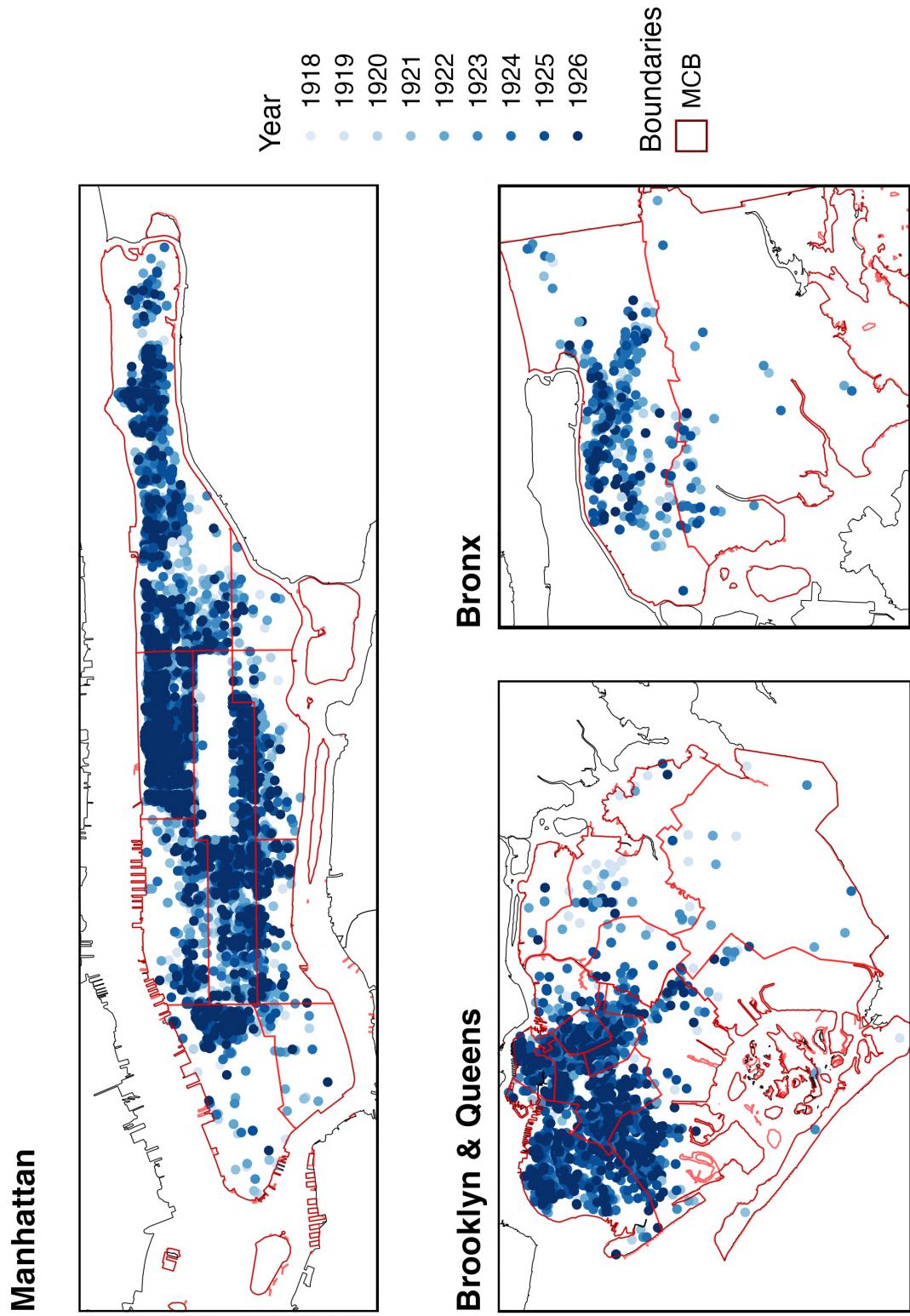
4.3 Building Permits

Our second main outcome concerns investment in new construction. We collect 7,209 building permit records from the Office for Metropolitan History ([2024](#)) website. These records primarily cover Manhattan and provide rich detail on proposed projects, including the number of buildings, intended use (residential, commercial, storage, or industrial), construction materials and features, project address, and estimated development cost. We take reported development cost as our primary measure of investment. To locate projects, we geocode addresses using the same two-step procedure as for the rental listings, combining automated matching with historical maps to account for changes in street numbers and names. [Figure 5](#) shows the spatial distribution of permits, highlighting the strong concentration in Manhattan.

⁷We describe the geocoding procedure in Appendix 4.2, including the manual corrections applied to observations using underlying lots, addresses, and house numbers.

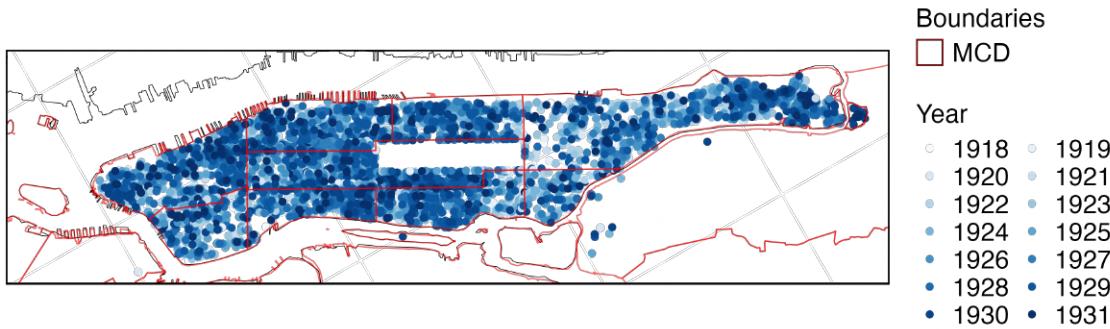
⁸To assess whether this bias stems from the fact that we only observe part of the city's neighborhoods, we calculate frequency weights as the number of observations within a neighborhood divided by the total number of rental observations in [Figure C2.2](#). This confirms that higher average rents in our sample largely stem from spatial bias.

Figure 4: Spatial distribution of rental properties



A natural concern is the representativeness of our permit data. While we lack comprehensive permit counts for all of Manhattan, we benchmark our data against completed buildings with more than three dwellings in [Figure C3.2](#) in Appendix C3. The trends in permitted residential projects closely track those of completed multi-family buildings, with a lag of roughly one year corresponding to the likely lag between permission and completion. This alignment suggests that the permit data captures meaningful variation in construction activity. Finally, apart from the year 1918, residential, commercial, storage, and industrial projects account for the bulk of total investment (see Appendix C3, [Figure C3.1](#)).

Figure 5: Spatial distribution of building permits



5 Empirical Strategy

In this section, we combine the predictions of the theoretical framework outlined in Section 3 with the features of the dataset described in Section 4 to guide our empirical strategy. In particular, the model generates two central propositions. Proposition 1 predicts that, in districts with landlord-friendly judges, rents will be higher, reflecting that landlords' willingness to raise rents depends not only on statutory rules but also on the probability of drawing a landlord-friendly judge. Proposition 2 extends this logic to developers, showing that expected returns on new housing are also shaped by judicial composition: investment in new residential construction will be higher where the share of landlord-friendly judges is higher. We evaluate these predictions using two complementary empirical designs: a Regression Discontinuity Design (RDD) at MCD boundaries to test Proposition 1, and

a difference-in-differences (DiD) framework comparing residential and non-residential construction to test Proposition 2.

5.1 Impact on Rents (RDD)

To test Proposition 1, we exploit the fact that judicial assignment is determined by municipal court district (MCD) boundaries. The main empirical challenge is that the election of judges is not random: municipal court districts (MCDs) that elect pro-landlord judges may also be areas with higher landlord shares, more constrained housing stock, or different demographic composition. As shown in Appendix C, all-Republican and all-Democratic MCDs look broadly similar across population, income, tenure, race, and immigrant share, though mixed districts differ somewhat in size and tenure composition. Nonetheless, standard regression estimates may still suffer from omitted variable bias if unobserved factors both drive rents and influence the election of pro-landlord judges.

To address this concern, we implement a Regression Discontinuity Design (RDD) that exploits the different assignment of properties to MCDs across geographic boundaries. Each dwelling is uniquely mapped to one MCD, and all rental disputes in that MCD are handled by the same set of judges. Empirically, as shown in Section 4.1, Democratic judges were more likely to rule in favor of tenants, while Republican judges favored landlords. Identification comes from properties located close to the boundary between all-Republican and all-Democratic MCDs, where otherwise similar dwellings fall under courts with systematically different partisan composition. Figure 6 highlights these boundary segments (dashed black lines), with rental listings on either side.

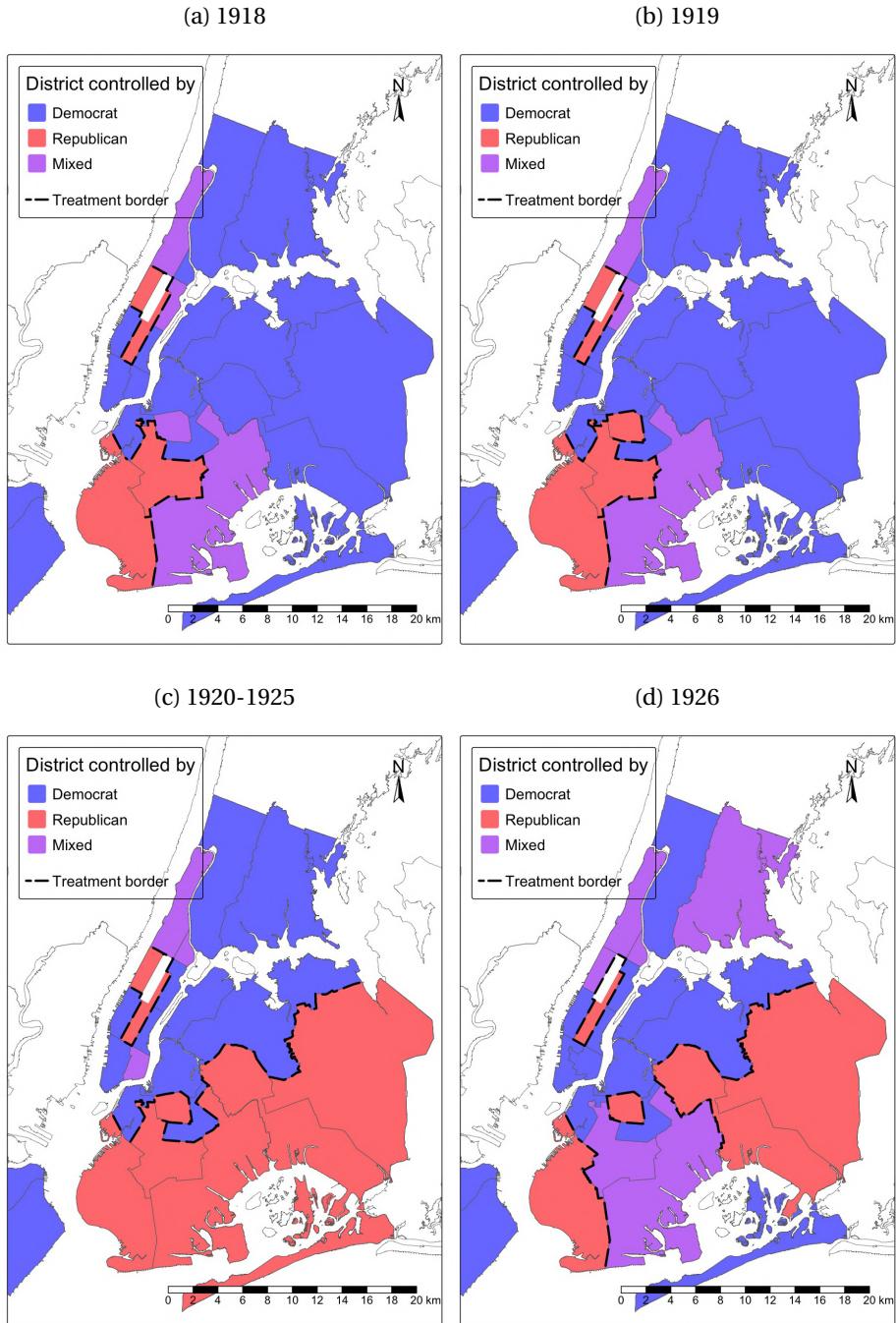
Our analysis is at the dwelling level and can be interpreted as a hedonic price regression with a spatial RDD component. The forcing variable is the shortest distance to the nearest MCD boundary, defined as positive in Republican districts and negative in Democratic districts. Our baseline sample includes only all-Republican and all-Democratic MCDs, yielding 18 districts per year and 11,192 listings in total.⁹ We estimate the following specification:

$$y_{i,m,t} = \beta_{rdd} \cdot 1(distance_i > 0)_{i,t} + f^a(distance_i) + f^b(distance_i) \cdot 1(distance_i > 0)_{i,t} + \mathbf{D}_{i,t} + \mathbf{X}_{i,t,m} + \gamma_t + \gamma_m + u_{i,t} \quad (3)$$

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.

⁹Appendix D2 shows results when including mixed MCDs, treated as Republican if the majority of judges are Republicans.

Figure 6: Treatment Boundary



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).

The two unknown functions f^a and f^b are assumed to be smooth in distance. We use a local non-parametric approach, with a triangular kernel and 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, \mathbf{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 ($D_{i,t}$): 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 report robust bias-corrected confidence intervals, which account for bandwidth sensitivity.

The identifying assumption is that if unobserved determinants of rents vary smoothly across MCD boundaries, then β_{rdd} (the discontinuity) provides an unbiased estimate of the effect of judicial composition (and thus rent control stringency) 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.

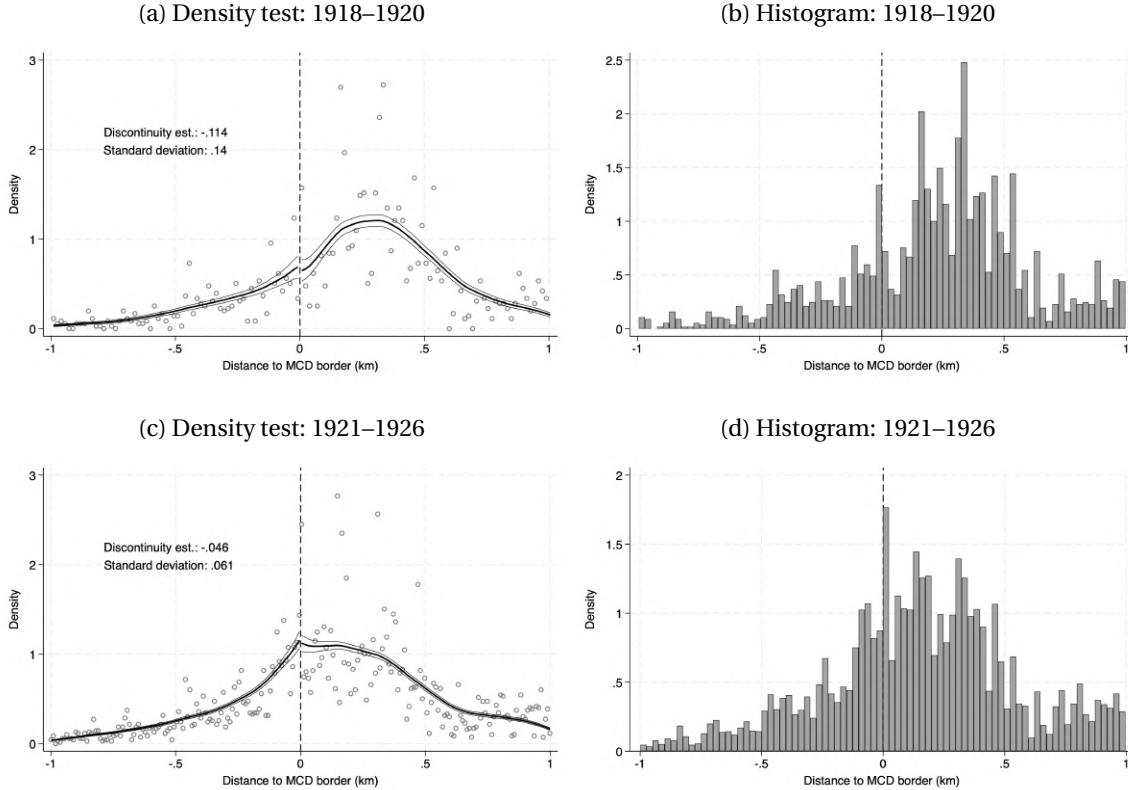
5.2 Impact on Investment (DiD)

The second proposition of the model is that a more landlord-friendly judge composition leads to higher residential investment. To test this, we complement the RDD with a Difference-in-Differences (DiD) strategy that exploits temporal and sectoral variation in building activity. Rent control directly applied only to residential rents, while commercial and industrial rents were not subject to judicial enforcement. This allows us to use non-residential investment as a counterfactual for residential investment.

We define two treatment groups: (i) residential-only permits, i.e. permits issued exclusively for the construction of residential buildings, and (ii) mixed-use permits that combine a residential component with predominantly commercial, industrial, or storage space. To benchmark these against unaffected sectors, we construct three main control groups. First, *all* investment other than residential provides the broadest counterfactual. It captures the full set of permits that were not subject to rent control, meaning that the comparison is against the overall non-residential construction market. Second, private-sector non-residential investment — including commercial, industrial, or storage projects — narrows the comparison to private-sector activity most similar to residential building in terms of being market-driven (as opposed to public or infrastructure projects). This helps ensure that differences are not driven by sectoral shifts into public works or infrastructure. The

third control group we use in our analysis is the set of non-residential projects excluding commercial, which may have had similar financing structures and location dynamics to (multifamily) rental housing. For that reason, comparing residential to non-commercial sectors may isolate the effect of rent control from broader trends in construction technology, financing, or urban development.¹⁰ The identifying assumption is that, conditional on observables, residential and non-residential investment would have followed parallel trends absent rent control.

Figure 7: Continuity at Cutoff – Rental Dataset



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 1 km 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.

Our baseline Equation 4 compares treated and control permits before and after rent

¹⁰If residential and commercial projects are strong complements—for example, because new housing creates demand for retail and service space, or because mixed-use development bundles both together—then rent control's dampening effect on residential construction could also indirectly depress commercial investment. Conversely, if they are substitutes—for instance, if developers shift resources away from less profitable residential projects into commercial buildings—then one would expect to see a relative increase in commercial activity when rent controls are in force.

control, allowing effects to vary with district-level exposure, measured by the Republican share of judges or the number of rent cases filed.

$$y_{i,m,t} = \theta_{Post}^{Treat} \cdot (Post_t^{20-28} + Post_t^{29-31}) \\ \cdot (Treat_i^{res/mix} \times Intensity_m) + \mathbf{D}_{i,t} + \mathbf{C}_i + \mathbf{U}_i + \gamma_t + \gamma_m + \nu_{i,m,t}, \quad (4)$$

where $y_{i,m,t}$ is the log project cost of permit i in MCD m and year t , $Post_t^{20-28}$ and $Post_t^{29-31}$ are indicators for the main rent control periods, and $Treat_i^{res/mix}$ indicates whether the permit is residential or mixed-use. $Intensity_m$ measures the extent of exposure to rent control in district m , captured either by the Republican share of judges or the number of rent cases filed in the MCD. The specification further includes distance controls ($\mathbf{D}_{i,t}$), construction-material fixed effects (\mathbf{C}_i), usage-mix controls (\mathbf{U}_i), year fixed effects (γ_t), and neighborhood fixed effects (γ_m).

This DiD framework provides a direct test of Proposition 2 by asking whether residential investment rose or fell relative to comparable non-residential investment, particularly in districts more exposed to rent control. Together with the RDD results, which isolate rent discontinuities at MCD boundaries, the DiD results show how judicial composition shaped both rents and investment in New York City's housing market.

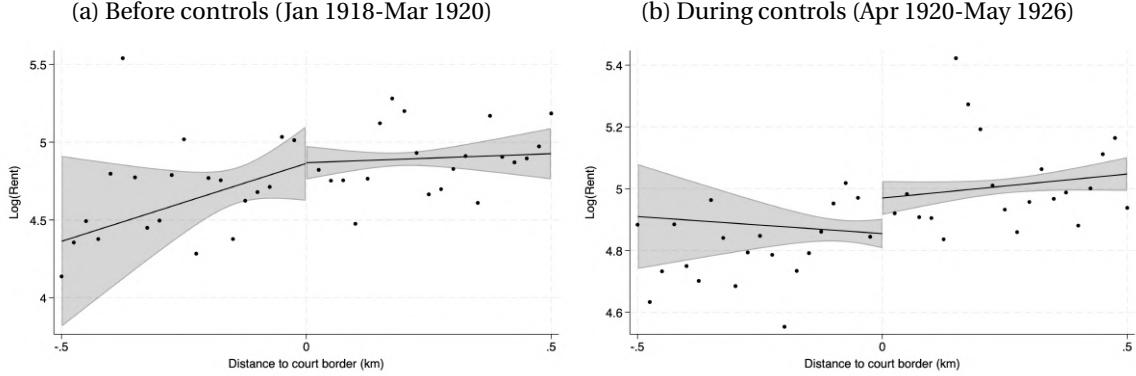
6 Analysis

6.1 Effects on Rents

We begin by estimating the RDD equation, [Equation 3](#). A summary of the main RDD results is shown in [Figure 8](#), corresponding to the regression results in the first column of [Table 2](#) and [Table 1](#). 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. These results, which include year and NTA fixed effects but exclude other dwelling-level controls, indicate that, at first pass, market rents were higher in all-Republican MCDs.

We examine these findings in greater detail in [Table 1](#) and [Table 2](#). These regression results are the output of [Equation 3](#) being estimated for samples before and after the introduction of rent control in April 1920. Each table has two panels, one for a linear function and one for a quadratic, and four columns. The first column uses the optimal bandwidth, \hat{b} , calculated using the Imbens and Kalyanaraman ([2012](#)) algorithm, but does

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



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 3; we used a bandwidth of 500m; the shaded area show 95 percent confidence intervals; standard errors have been clustered at the neighborhood level.

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 controls were in full effect, April 1920–May 1926. Table 1 presents the results of estimating Equation 3 for the sample of listings during the Rent Control period. In each of the eight columns, the coefficient is positive and in six, it is statistically significant, meaning that during the Rent Control period there was a jump in rents 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 statistical significance only fails where half the optimal bandwidth is used (although the point estimate is still positive). Using the optimal bandwidth and including dwelling-specific controls, the estimated jump in market rents at the boundary is between 8 percent and 9 percent.

The pre-Rent Control period, from January 1918 to March 1920, serves as a placebo test: there should be no relationship between judge composition and market outcomes when judges had no authority over rents. This is supported by the empirical analysis. Unlike during the Control period, in the pre-Control years (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 percent to -10 percent, they are noisy and in no instance is the point estimate statistically significantly different from zero.

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*** (0.033)	0.075** (0.034)	0.036 (0.044)	0.083*** (0.025)	0.109* (0.056)	0.089** (0.041)	0.045 (0.049)	0.090*** (0.029)
Controls	X	✓	✓	✓	X	✓	✓	✓
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 ^u _{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 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, $2\hat{b}$, 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.

Robustness We test whether the effect varies when mixed districts are included, considering an MCD Republican-controlled if the share of Republican judges exceeds 50 percent. We estimate [Equation 3](#) using the same set-up as above. Results are given in [Table D2.1](#) and [Table D2.2](#) of Appendix D2. As above, there is no evidence for any significant effect of the border before introduction of rent control. During rent control, the broad pattern of results persists, though smaller bandwidth choices render the effect insignificant. We also test for the sensitivity of outcomes to different RDD parameter choices. [Figure D3.1](#) in Appendix D3 shows that treatment effects are highly stable in magnitude across bandwidth choices before and during rent control. For each bandwidth choice, rents after the introduction of rent control are higher by a similar factor. Panel D3.1c and D3.1d in particular show that estimates become significant a bandwidth larger than 300 meters.

Placebo Persistence As described earlier, rent controls were gradually rolled back from 1926 and expired in 1929. To test whether their effects persisted beyond abolition, we estimate [Equation 3](#) using a dataset of just over 5,000 listings from 1930. Properties were geocoded as in Section 4 and matched to their pre-1926 MCD boundaries, with distance to the court border serving as a placebo treatment. Results are reported in [Table D6.1](#).

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 (0.117)	0.038 (0.133)	-0.046 (0.162)	-0.001 (0.101)	0.002 (0.204)	0.003 (0.158)	-0.152 (0.226)
Controls	X	✓	✓	✓	X	✓	✓	✓
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 ^u _{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 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, 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.

Across specifications, there is no evidence of a discontinuity in rents at the former boundary once dwelling-level controls are included. While linear and quadratic estimates without controls yield small differences, these vanish once controls are added. With controls, coefficients are imprecise and statistically indistinguishable from zero. This suggests that rent control’s effects ended with its repeal, with no evidence of persistent impacts such as sorting or longer-term shifts in the rental market.

Event Study We also estimate an event-study specification in Appendix D5, interacting year dummies with continuous measures of judicial composition. We report these results in [Figure D5.1](#). Both treatments — the share and the number of Republican judges in an MCD — yield consistent effects. Rents in fully Republican districts are about 10 percent higher than in fully Democratic districts, closely matching the RDD results in [Table 1](#). Adding one Republican judge raises rents by roughly 3 percent, implying about 6 percent higher rents in a typical mixed district. Results using binary treatments from the RD design (Panels (c) and (d) in [Figure D5.1](#)) confirm this pattern, with point estimates of 10.7 percent and 8.8 percent and no evidence of pre-trends.

6.2 Effect on Investment

Here, we test Proposition 2 of the model, which predicts that more stringent rent controls, as proxied by the political composition of judges, lower the effective return to residential projects, depressing new residential construction. Table 3 reports the results from our difference-in-differences design. The temporal difference compares investment per permit during ($Post_{20-28}$) and after ($Post_{29-31}$) the policy to the pre-period (1918–1919). The cross-sectional difference compares residential ($Post \times Res$) and mixed-use permits ($Post \times Mix$) to non-residential permits, measured—as described above—in three ways (any non-residential; private-sector non-residential only; and excluding commercial). Following the conceptual model, the baseline treatment is the number of Republican judges in the MCD.

Table 3: Effect of Rent Control on Investment

	Any	Private	No Com.
$Post_{20-28} \times Res$	0.132*** (0.047)	0.141** (0.053)	0.167*** (0.036)
$Post_{29-31} \times Res$	0.013 (0.048)	0.005 (0.050)	0.072* (0.039)
$Post_{20-28} \times Mix$	-0.059 (0.091)	-0.071 (0.098)	-0.034 (0.083)
$Post_{29-31} \times Mix$	-0.086 (0.112)	-0.113 (0.110)	-0.049 (0.104)
Distance	✓	✓	✓
Material FE	✓	✓	✓
Usage FE	✓	✓	✓
NTA FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	7,098	6,314	5,242
R ²	0.52	0.51	0.59

Table 3 reports difference-in-differences regressions of log project cost on rent control exposure, following Equation 4. Treatment groups are residential-only permits and mixed-use permits with a residential component. Control groups vary across columns between (i) all non-residential permits (Any), (ii) private non-residential permits (Private), and (iii) non-commercial private permits (No Commercial). Post₂₀₋₂₈ and Post₂₉₋₃₁ are indicators for the rent control periods 1920–1928 and 1929–1931. Exposure is measured as the number of Republican judges in an MCD. All specifications include distance controls, material fixed effects, usage fixed effects, neighborhood (NTA) fixed effects, and year fixed effects. Standard errors in parentheses are clustered at the neighborhood (NTA) level. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

For residential permits, we find large and robust effects during the rent control period, but not afterward. Coefficients are consistently positive and statistically significant across specifications, implying that each additional Republican judge is associated with a 13 to 17 percent increase in investment per residential building. In the later period (1929–1931),

after rent controls were phased out and abolished, the estimates are small, imprecise, and not statistically significant. This pattern indicates that the contraction in residential investment, where rent controls were most strictly applied, was confined to the years when those controls were actively enforced. We use the share of Republican judges as an alternative treatment and report these results in [Table D4.1](#) in Appendix [D4](#). This confirms our results, showing between 65 and 85 percent higher investment per residential building in districts with only Republican judges compared to districts with none during the height of rent control. This closely matches the result using the single-judge specification. Given that there could be up to three—and in one MCD, four—Republican judges in certain districts, an effect size of 0.17 closely matches the results using the share.^{[11](#)}

For mixed-use permits that included both non-residential and residential components, the estimates are not statistically significant, both during and after rent control. The point estimates are negative, but small in magnitude. This is consistent with such projects being only partially exposed to rent control, thus potentially offering a margin of adjustment for developers in districts where strict rent controls were feared. The net effect on investment would therefore depend on how much of the project's expected revenue came from the controlled residential portion versus the uncontrolled commercial/industrial portion.

Taken together, these results demonstrate that rent control not only led to a distortion of rental prices during control – in line with the established literature – but also influenced the spatial allocation of investment in those districts where controls were perceived to be strongest, based on the political composition of judges. The combination of price effects ([Proposition 1](#)) and investment effects ([Proposition 2](#)) aligns closely with the model's predictions: by lowering expected returns for landlords, judicial enforcement of rent regulation is consistent with lower rents and relatively less new residential construction in more tightly controlled districts.

Event Study We estimate an event-study specification for investment ([Appendix D5](#)) that interacts year dummies with measures of judicial composition, using residential permits as the treatment group and private non-residential permits as the control. [Figure D5.2](#) reports the estimates for residential-only permits. The coefficients show no evidence of differential pre-trends prior to 1920, although the smaller volume of permits in 1918 means a large confidence interval. Once rent controls come into effect, investment in less controlled districts — proxied by the share and number of Republican judges — rises sharply, in line with the DiD results, and spikes again after the 1926 extension. These effects vanish after 1929, when controls are phased out. The same pattern holds across alternative controls —

¹¹The scaling assumes the coefficient represents a semi-elasticity in log terms. If the per-judge coefficient is β , the implied effect of three judges relative to none is $\exp(3\beta) - 1$. Using $\beta = 0.13 - 0.17$ yields a combined effect of approximately 45–66 percent higher investment.

including all non-residential investment and private non-residential investment excluding commercial. [Figure D5.3](#) presents the mixed-use results: effects are weaker and imprecise across specifications. Across all control groups, the consensus result is that more landlord-friendly judges significantly increased investment in residential-only projects, while the effects on mixed-use projects are weaker, less precisely estimated, and not orthogonal across specifications.

Evidence from Census Housing Counts The 1920s saw the largest addition to the city's housing stock of any decade in the 20th century. Thus, if areas exposed to more intense rent controls experienced lower levels of residential investment during the 1920s, this may be reflected in the housing stock. We close our empirical analysis by examining whether there is any evidence of this relationship in the 1940 Census, the first systematic post-rent-control tally of dwelling units. Specifically, we use tract-level data from the 1940 Census, obtained from IPUMS NHGIS ([Schroeder et al., 2025](#)); for NYC in the 1940 Census, housing counts are by health district, which we match to MCDs. As some health districts span multiple MCDs, we construct health district–MCD overlap shares and run weighted OLS regressions, using these overlap shares as weights.¹² Since rent control may have broader general equilibrium effects, we do not control for contemporaneous variables such as income or population in 1940. Instead, we include baseline controls from the 1920 Census (conducted just before rent control was introduced), which are exogenous to the policy but likely influenced subsequent construction activity.

[Table 4](#) reports the results from these cross-sectional regressions where (log) housing units in the 1940 is the outcome and our regressor of interest is judicial composition during the rent control era, measured by the number of Republican judges in the local MCD. Column (1) reports a positive unconditional correlation across the 521 tracts, an effect that is somewhat larger and more precisely estimated when controls are included for 1920 housing stock, population and black share of the population. Including NTA fixed effects reduces the point estimate slightly but, with or without controls, it is still statistically significant at conventional levels. Allowing for any link between the existing population in 1920 and new construction from 1920 to 1940, each additional Republican judge in an area was associated with 4 percent more housing in 1940.

We do not claim that this cross-sectional result is a neatly identified causal effect. However, the result is consistent with our theoretical framework and earlier findings: more strictly-enforced rent controls (as measured by fewer Republican judges) saw lower rents during the controls, but also lower investment in new housing, with evidence that

¹²[Figure B.5](#) in Appendix B shows the 1940 health districts overlaid with municipal court district (MCD) boundaries.

the effects were visible in diminished housing stock in 1940 relative to districts where controls were less enforced. Together, the permit and census results are evidence in favor of the mechanism in Proposition 2: judicial discretion lowered expected returns in tenant-friendly districts, depressing residential investment in the 1920s and leaving visible gaps in the housing stock nearly two decades later.

Table 4: Effect of Rent Control on 1940 Housing Stock

	(1)	(2)	(3)	(4)
#Rep	0.066** (0.027)	0.071*** (0.026)	0.042** (0.017)	0.043** (0.018)
$\log(HU_{20})$	0.031 (0.040)		-0.065 (0.095)	
$\log(POP_{20})$	0.103** (0.044)		0.228** (0.095)	
$\log(BLACK_{20})$	-0.016 (0.011)		-0.017 (0.015)	
NTA FE	x	x	✓	✓
Observations	521	507	521	507
R ²	0.020	0.108	0.536	0.583

Note. Table 4 reports weighted OLS regressions of log housing units in 1940 Census health districts (from IPUMS NHHGS) on the number of Republican judges in the corresponding municipal court district (MCD) during the rent control era. Health districts overlapping multiple MCDs are matched using area shares, which serve as weights. Columns (1)–(2) exclude neighborhood fixed effects; Columns (3)–(4) include neighborhood (NTA) fixed effects. Controls include baseline housing stock (HU_{20}), population (POP_{20}), and the Black population share ($BLACK_{20}$) from the 1920 Census, conducted before rent control was introduced. Standard errors (in parentheses) clustered at the NTA level. ***, **, * denote significance at the 1, 5, and 10 percent levels, respectively.

7 Conclusion

While many studies have examined rent control and its effects, strong causal identification methods have only recently been applied across multiple settings. This paper has examined the effects of interwar rent control in New York City, focusing on the distinctive role of judicial discretion in enforcement. Using a combination of theory and newly assembled microdata, we showed that pro-landlord courts sustained higher rents during the 1920s, generating discontinuities of around 10% across municipal court district boundaries, while investment per residential building was significantly higher in landlord-friendly districts by more than 75%. These findings highlight how judicial composition shaped both short-run prices and long-run supply through courts' decisions to uphold or roll back rent increases. In districts with pro-tenant judges, litigation risk effectively lowered expected returns, pushing rents closer to the cap and disincentivizing new residential investment. This

provides evidence that the enforcement mechanism of rent control — through partisan judicial discretion — distorted housing markets.

Our study is not without limitations. The classification of judges relies partly on newspaper accounts; our rental data are drawn from the New York Times and are not fully representative of the entire housing stock; and the permits data cover only Manhattan. Yet the consistency of results across two distinct empirical strategies strengthens confidence in the main conclusions.

For policymakers, the results are consistent with evidence that rent controls can influence market outcomes and the allocation of residential housing supply. Our findings also underline the risk associated with decentralized or discretionary enforcement mechanisms for those controls. Designing rental regulations requires attention not only to statutory provisions but also to the institutions charged with applying them.

Our focus is on price and investment outcomes in the rental market, but the nature of the rent controls suggests a number of rich potential avenues for future research. These include potential spillovers in the sales segment, particularly as mortgage markets were evolving in the 1920s to something closer to modern mortgage systems. In addition to effects on prices and investment, the rent controls may also have affected mobility. With the evolution of credit markets, rent controls may have encouraged changes in the tenure mix which, combined with new transport technologies, could have led to a boom in owner-occupied housing construction. As rent controls regain political popularity, understanding the wider effects of episodes such as New York's 1920s controls has a wider relevance.

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Online Appendix for “Judge for Yourself? The Impact of Controls on Rents in Interwar New York”

Maximilian Guennewig-Moenert and Ronan Lyons

Appendix Contents

A	Model	2
B	Supplementary Maps	4
C	Data	9
	C1 Judges	11
	C2 Listing Rents	17
	C3 Building Permits	20
D	Additional Results	23
	D1 RDD estimates for Manhattan	23
	D2 RDD estimates for alternative treatment boundary	25
	D3 RDD estimates for Alternative bandwidth choices	27
	D4 Difference-in-Differences	28
	D5 Event study results	28
	D5.1 Rent Price Effects	28
	D5.2 Investment Effects	29
	D6 Persistence of Effects	34

A Model

Proof of Proposition 1. At the start of period t , the landlord chooses between *hold* (keep last period's rent) and *raise* (demand the market rent and risk adjudication).

Payoffs. If the landlord holds, the realized rent equals $r_{j,t-1}$. If the landlord raises, the expected realized rent is

$$\mathbb{E}[r_{j,t} \mid \text{raise}] = p_j r_{j,t} + (1 - p_j) (r_j^c - c_\ell) = p_j (r_{j,t} - r_j^c + c_\ell) + (r_j^c - c_\ell).$$

Decision rule. Raising is optimal iff $\mathbb{E}[r_{j,t} \mid \text{raise}] > r_{j,t-1}$, i.e.

$$p_j (r_{j,t} - r_j^c + c_\ell) + (r_j^c - c_\ell) > r_{j,t-1} \iff p_j > \frac{r_{j,t-1} - r_j^c + c_\ell}{r_{j,t} - r_j^c + c_\ell}.$$

Using $r_{j,t} = r_{j,t-1} + g$ with $g > 0$ shows the right-hand side is well-defined and strictly less than 1 whenever the cap binds (i.e. $r_{j,t} > r_j^c - c_\ell$), because then $r_{j,t} - r_j^c + c_\ell > r_{j,t-1} - r_j^c + c_\ell$.

Corner cases. If $p_j = 0$, then $\mathbb{E}[r_{j,t} \mid \text{raise}] = r_j^c - c_\ell \leq r_{j,t-1}$ when the cap binds, hence *hold*. If $p_j = 1$, then $\mathbb{E}[r_{j,t} \mid \text{raise}] = r_{j,t} > r_{j,t-1}$, hence *raise*. \square

Proof of Proposition 2. The developer chooses $k \geq 0$ to maximize

$$\pi_j(k; p_j) = R_j(p_j) h(k) - c(k),$$

with $h'(k) > 0$, $h''(k) < 0$, $c'(k) > 0$, $c''(k) > 0$, and $R_j(p_j) = r_{j,0} + \beta [q r_{j,1}^e(p_j) + (1 - q)r_{j,1}]$ where $r_{j,1}^e(p_j) = p_j r_{j,1} + (1 - p_j)(r_j^c - c_\ell)$.

1. *Concavity and FOC.* Since $-c$ is strictly concave and h is concave, $\pi_j(\cdot; p_j)$ is strictly concave. The first-order condition (FOC) for an interior optimum is

$$h'(k) - c'(k) + R_j(p_j) = 0 \iff \phi(k) = R_j(p_j),$$

where $\phi(k) \equiv \frac{c'(k)}{h'(k)}$.

2. *Monotonicity and invertibility of ϕ .* Differentiating,

$$\phi'(k) = \frac{c''(k)h'(k) - c'(k)h''(k)}{[h'(k)]^2} > 0,$$

because $c''(k) > 0$, $h'(k) > 0$, and $-h''(k) > 0$ with $c'(k) > 0$. Hence ϕ is strictly increasing and continuously differentiable, so it admits a (continuously differentiable) inverse on its

image. Therefore the unique optimizer is

$$k_j^*(p_j) = \phi^{-1}(R_j(p_j)),$$

with the corner $k_j^*(p_j) = 0$ if $R_j(p_j) \leq \phi(0)$.

3. Comparative Statics in p_j . When the cap binds, $r_{j,1} > (r_j^c - c_\ell)$ and thus

$$\frac{\partial r_{j,1}^e}{\partial p_j} = r_{j,1} - (r_j^c - c_\ell) > 0 \implies \frac{\partial R_j}{\partial p_j} = \beta q[r_{j,1} - (r_j^c - c_\ell)] > 0.$$

By the implicit-function theorem,

$$\frac{\partial k_j^*}{\partial p_j} = \frac{1}{\phi'(k_j^*(p_j))} \frac{\partial R_j}{\partial p_j} > 0,$$

since $\phi'(k) > 0$. Hence $k_j^*(p_j)$ is strictly increasing in p_j under a binding cap.

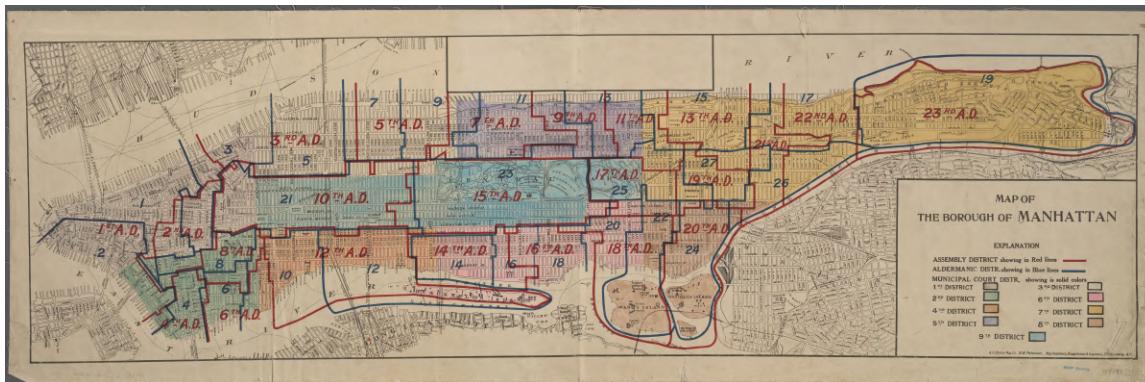
4. Investment gap and integral identity. Let $R^0 \equiv r_{j,0} + \beta r_{j,1}$ denote the no-control return. Because $r_{j,1}^e(p_j) \leq r_{j,1}$ with strict inequality when the cap binds and $p_j < 1$, we have $R_j(p_j) \leq R^0$ (strictly < when binding and $p_j < 1$). Using $k^*(R) = \phi^{-1}(R)$ and $(\phi^{-1})'(u) = 1/\phi'(\phi^{-1}(u))$,

$$k^*(R^0) - k_j^*(p_j) = \phi^{-1}(R^0) - \phi^{-1}(R_j(p_j)) = \int_{R_j(p_j)}^{R^0} \frac{du}{\phi'(\phi^{-1}(u))}.$$

This expression shows that the investment shortfall can be interpreted as the area under the inverse marginal technology schedule between the actual return $R_j(p_j)$ and the no-control benchmark R^0 . Because the integrand is strictly positive, the gap is always positive whenever $R_j(p_j) < R^0$, that is, whenever the cap binds and $p_j < 1$. Taken together, the results establish three points: developers invest a positive amount whenever expected returns exceed the marginal cost at zero scale, investment rises monotonically with the probability of drawing a landlord-friendly judge, and the difference between actual and benchmark investment admits an exact integral representation. \square

B Supplementary Maps

Figure B.1: Historical Municipal District Courts - Manhattan

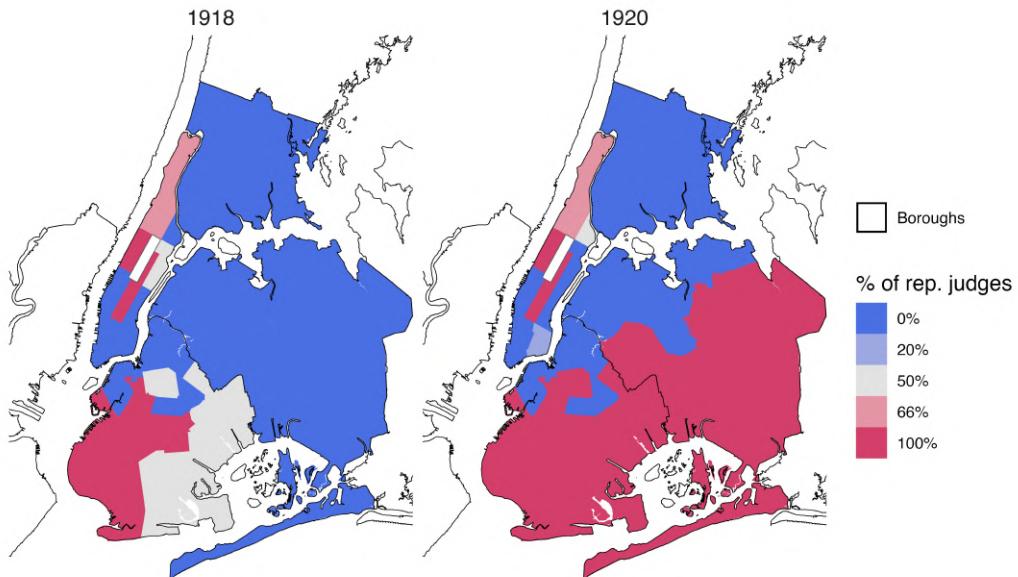


Note. [Figure B.1](#) shows the Borough of Manhattan, the Assembly, Aldermanic, and Municipal Court Districts in 1918.

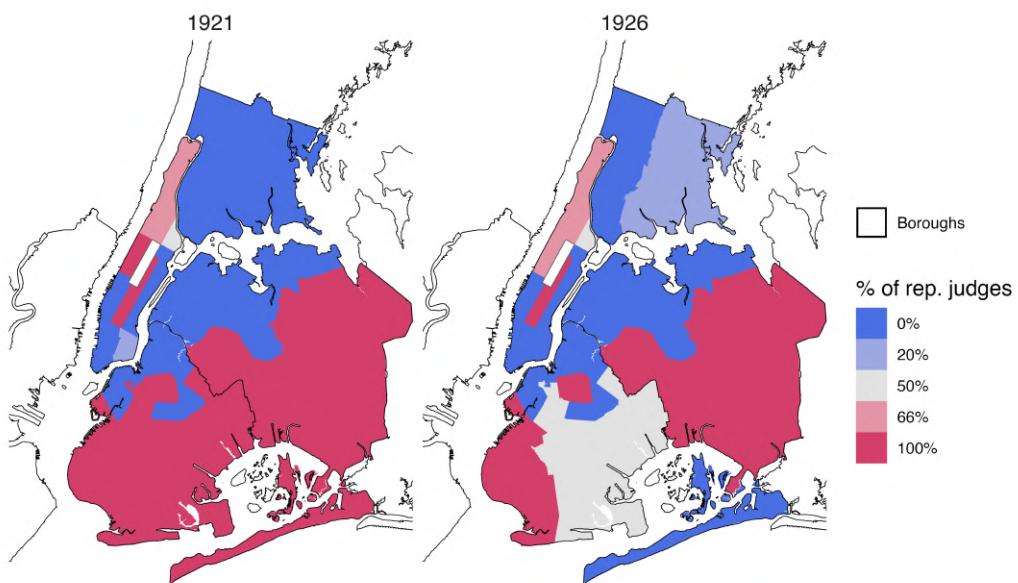
Source. Lionel Pincus and Princess Firyal Map Division, The New York Public Library (1918). Map of the Borough of Manhattan, showing the Assembly, Aldermanic, and Municipal Court Districts.

Figure B.2: Share of Republican judge

(a) Pre rent control

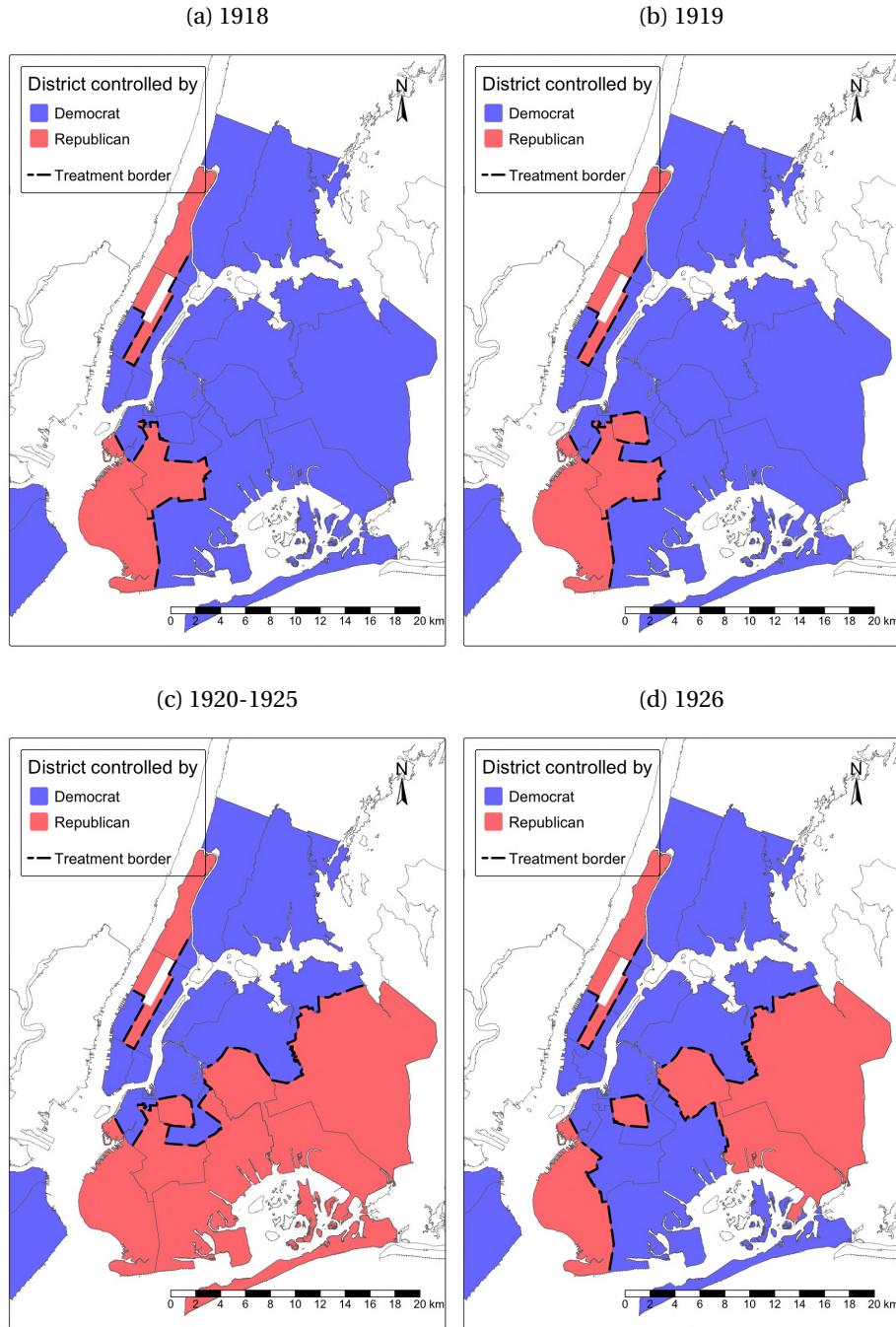


(b) Post rent control



Note. Figure B.2 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 were no changes from 1920 to 1925 in Panel.

Figure B.3: Alternative treatment boundary



Note. Figure B.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. A district is considered as Republican controlled if the share of Republican judges within the MCD is larger than 50%; therefore 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 were changes from 1920 to 1925 in Panel (c).

Figure B.4: Example of manual geocoding

(a) PLuto 2002 lot files



(b) Bromley fire insurance maps

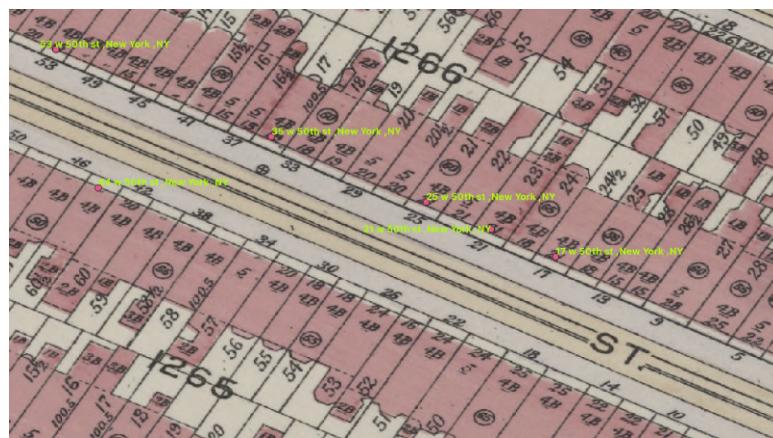
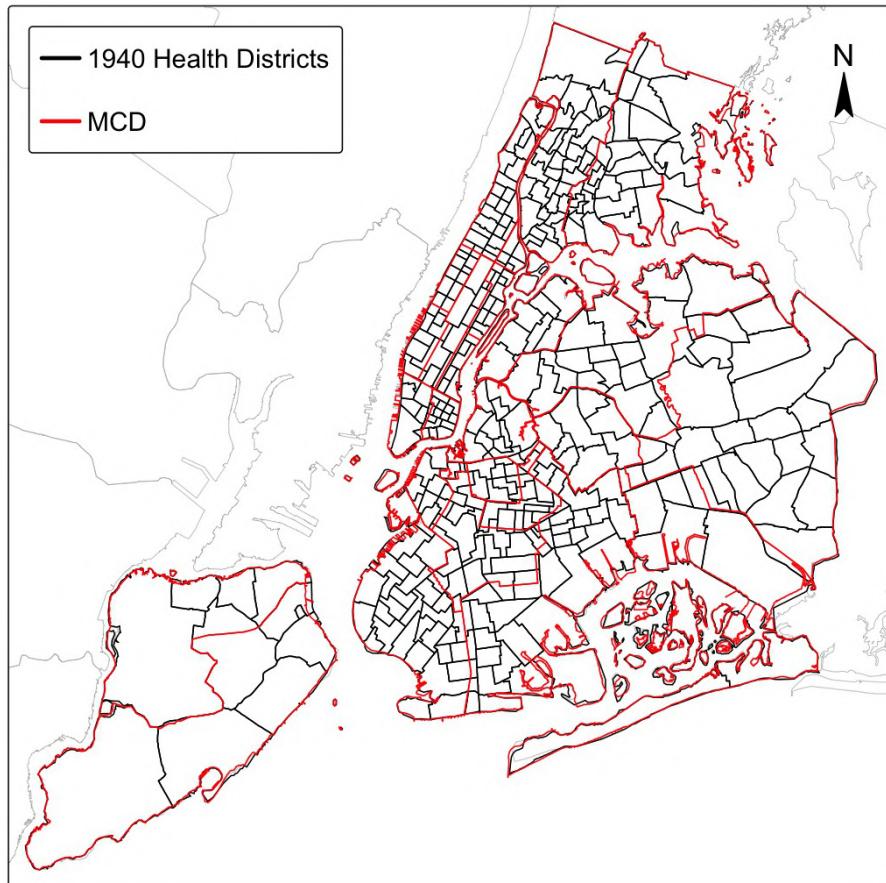


Figure B.5: 1940 Health Districts and Municipal Court Districts in NYC



Note. Figure B.5 overlays the 347 1940 Census health districts (black boundaries) with municipal court districts (MCDs, red boundaries). Because some health districts overlap multiple MCDs, we construct health district–MCD shares and use these as weights in our regression analysis in 6.2.

Source. IPUMS NHGIS (Schroeder et al., 2025).

C Data

Figure C.1: Examples of Data Sources

(a) New York Times

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

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

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

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 Rooms, \$2,400.
8 Rooms (Corner) \$3,200.
Apply on premises or
NASSOIT & LANNING,
Way & 55th St. Tel. 3380 Riverside

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

690 RIVERSIDE DRIVE,
Cor. 140th Street, elevator apartment.
large rooms; immediate possession.
Rent \$2100. Apply on Premises.

Unfurnished—East Side.

SEVEN ROOMS
AND TWO BATHS
1109-1111 Madison Ave.
CORNER 81D ST.
Elegant high-class apartment. All
bills included. Possession. Rent \$3,500
per annum.
JOHN A. SCHOEN, 115 Little House,
Tel. Stuy. 7695

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

THE CITY OF NEW YORK
Second District—264 Madison St. Orchard 4300. 191
Lester Lazarus, 265 7th St. (Dem.).....Term Expires Dec. 31, 1931
Abraham Harawitz, 26 Delancey St. (Dem.) Dec. 31, 1931
Joseph Raimo, 52 Spring St. (Dem.).....Dec. 31, 1937
Harold L. Kunstler, 149 Rivington St. (Dem.) Dec. 31, 1937
Morris Eder, 156 2d Ave. (Rep-Dem.).....Dec. 31, 1939
Patrick J. Paul, Clerk

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

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 C.1 shows examples of three of the main data sources used in the paper. Panel (a) shows a snapshot of the *New York Times* real estate section; Panel (b) displays the Green Book; and Panel (c) shows a landlord-tenant case from the *Daily News*.

Source. Panel (a): *New York Times* Real Estate Section; Panel (b): City of New York (1918–1931); Panel (c): *Daily News*.

Table C.1: Descriptive statistics

	1918	1919	1920	1921	1922	1923	1924	1925	1926	1927	1928	1929	1930	1931	Year
Panel A: Rent Data															
Monthly Rent	148.84 (170.226)	162.31 (141.927)	279.26 (457.945)	185.98 (144.968)	156.38 (143.868)	157.34 (135.306)	133.12 (115.009)	137.94 (103.971)	141.78 (148.028)						135.20 (134.335)
Rooms	5.29 (2.684)	3.77 (2.136)	3.43 (2.104)	4.09 (2.310)	3.72 (2.157)	3.49 (2.226)	4.06 (1.958)	4.10 (1.892)	3.54 (2.046)						2.94 (1.933)
# Listings	906	1587	1037	1876	1832	1734	1984	2332	2110						5688
Panel B: Investment (in \$1,000s) & Building Permits															
All	45.6 (99.7)	227.1 (542.0)	302.0 (682.1)	232.0 (984.7)	334.6 (2,059.0)	311.7 (691.4)	410.7 (1,184.2)	577.1 (1,188.2)	535.7 (930.5)	605.6 (1,110.1)	980.9 (1,169.0)	540.2 (1,812.8)	531.0 (1,258.7)	531.0 (1,923.3)	
# Permits	163	343	428	557	630	626	695	765	626	536	586	597	324	186	
Resid. Only	165.9 (181.5)	325.0 (394.5)	426.1 (735.7)	278.1 (322.5)	321.8 (306.3)	349.5 (309.1)	499.7 (503.7)	538.8 (508.0)	510.6 (650.6)	580.9 (733.3)	586.8 (653.2)	838.3 (775.2)	648.2 (668.9)	372.0 (451.4)	
# Resid. Only	11	48	39	109	153	210	214	213	175	142	213	205	68	33	
Resid. Mixed	42.7 (50.0)	272.2 (262.2)	378.8 (776.9)	211.0 (535.2)	386.2 (1062.6)	509.9 (637.0)	561.7 (986.8)	865.5 (1421.0)	836.7 (704.9)	576.8 (1091.8)	790.9 (1746.3)	1471.6 (466.3)	635.7 (447.7)	82.2	
# Resid. Mixed	3	11	11	30	47	23	27	55	64	38	48	69	30	4	
Panel C: Judges															
Judges	2.33 (1.022)	2.35 (0.994)	2.48 (1.243)	2.49 (1.214)	2.49 (1.214)	2.49 (1.214)	2.46 (1.220)	2.46 (1.220)	2.46 (1.220)	2.46 (1.220)	2.46 (1.220)	2.46 (1.220)	2.46 (1.220)	2.97 (1.472)	
# Judges	45	46	47	47	47	48	48	48	48	48	48	48	53	61	
Rep. Judges	0.93 (1.338)	1.11 (1.524)	1.04 (1.349)	1.02 (1.343)	1.02 (1.343)	1.00 (1.337)	0.94 (1.262)	0.85 (1.220)	0.62 (1.178)	0.62 (1.178)	0.62 (1.178)	0.38 (1.078)	0.38 (1.078)	0.66 (1.797)	
# Rep. Judges	15	17	20	20	20	20	19	17	16	11	11	6	6	8	

Note. Table C.1 reports means, with standard deviations in parentheses. Panel A summarizes the main outcomes from the rent dataset. Panel B reports investment measures: total investment (All), residential-only investment (Resid. Only), and residential mixed-use investment (Resid. Mixed), along with the corresponding number of observations in each category. Panel C shows the average number of Republican judges by municipal court district. Sample sizes are indicated by #. All prices are deflated using the CPI and expressed in 1918 dollars.

Source. Property price data from the *New York Times* real estate section; judge information from City of New York (1918–1931); building permit data from Office for Metropolitan History (2024).

C1 Judges

In this section we provide additional details on the construction of the judge-level dataset. Historical boundaries of Municipal Court Districts (MCDs) were reconstructed using archival maps and planning documents. An example can be seen in [Figure B.1](#). These boundaries were digitized to allow linkage with demographic, property-, and permit-level data. Information on judges was gathered from the *Green Book* (New York City Official Directory; City of New York, [1918–1931](#)), which lists each judge's name, party affiliation, district assignment, and re-election year. All 125 judges in our study were affiliated with a political party, primarily Democrats and Republicans.

To complement this administrative information, we collected case-level data from digitized newspaper archives. Articles were identified through keyword searches that combined the judge's full name and common variants (e.g., "Judge Morris", "Justice Morris"). We restricted attention to landlord–tenant cases related to rent increases or evictions, reported between 1918 and 1926. A total of 72 unique cases were coded, covering 42 judges. The classification of judges from these articles is reported in [Table C1.1](#), where we also document the publication date and the newspaper from which the information was obtained.

Each case was hand-coded into a binary outcome based on the judge's ruling. A decision was classified as tenant-favorable if:

1. The judge reduced the rent demanded by the landlord;
2. The judge refused a rent increase; or
3. The judge denied an eviction demand.

For each judge, we averaged these binary outcomes across observed cases. These averages were then aggregated by party affiliation, providing a measure of partisan differences in judicial behavior.

Table C1.1: Judge Coding

Name	Newspaper	Year	Month	Day	Reduction of rent	No increase	Tenant not evicted
O. 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 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 E. 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 C1.1 displays the full list of articles used to classify judge decisions in Chapter 4. It reports the name of the Newspaper as well as the classification of a judge's decisions. Eviction equals 1 if a tenant was evicted and 0 otherwise; Rent Decrease equals 1 if the judge reduced the amount demanded by the landlord; No Increase equals 1 if the judge denied any requested rent increase.

Next, we explore the election cycles of judges. Having the election year for each judge, we can track changes in the political composition of Municipal Court Districts (MCDs) over time in Figure C1.1. We begin by reporting the number of elections in a given year, separately for Democratic and Republican incumbents. Major election years were 1919, 1927, and 1929, each of which saw a large number of judicial contests (Panel (a)).¹³

We then turn to the consequences of these cycles for the stability of partisan control in MCDs. To capture stability, we construct a centered index that reflects whether districts tend to shift toward Republicans or Democrats relative to the previous year. For each district d and year t , we compute the share of Republican judges, $\text{share}_{d,\text{Rep},t}$, and take its year-to-year change:

$$\Delta_{d,t} = \text{share}_{d,\text{Rep},t} - \text{share}_{d,\text{Rep},t-1}.$$

We then average this change across all districts that can be observed in both $t - 1$ and t :

$$\bar{\Delta}_t = \frac{1}{N_t} \sum_{d \in \mathcal{D}_t} \Delta_{d,t},$$

where N_t is the number of districts observed in both years. Finally, we rescale this measure to be centered at 0.5:

¹³Though rare, some judges entered office through administrative appointment rather than election. For example, in 1925 Thomas J. Whalen was appointed by the Mayor to the 5th MCD to succeed William Young, who had become a Justice of the Children's Court. Similarly, Joseph Raimo was appointed by the Mayor to the 2nd MCD in 1927 to replace William Blau, who had resigned.

$$\text{CenteredStability}_t = 0.5 + \frac{\overline{\Delta}_t}{2}, \quad \text{with } \text{CenteredStability}_t \in [0, 1].$$

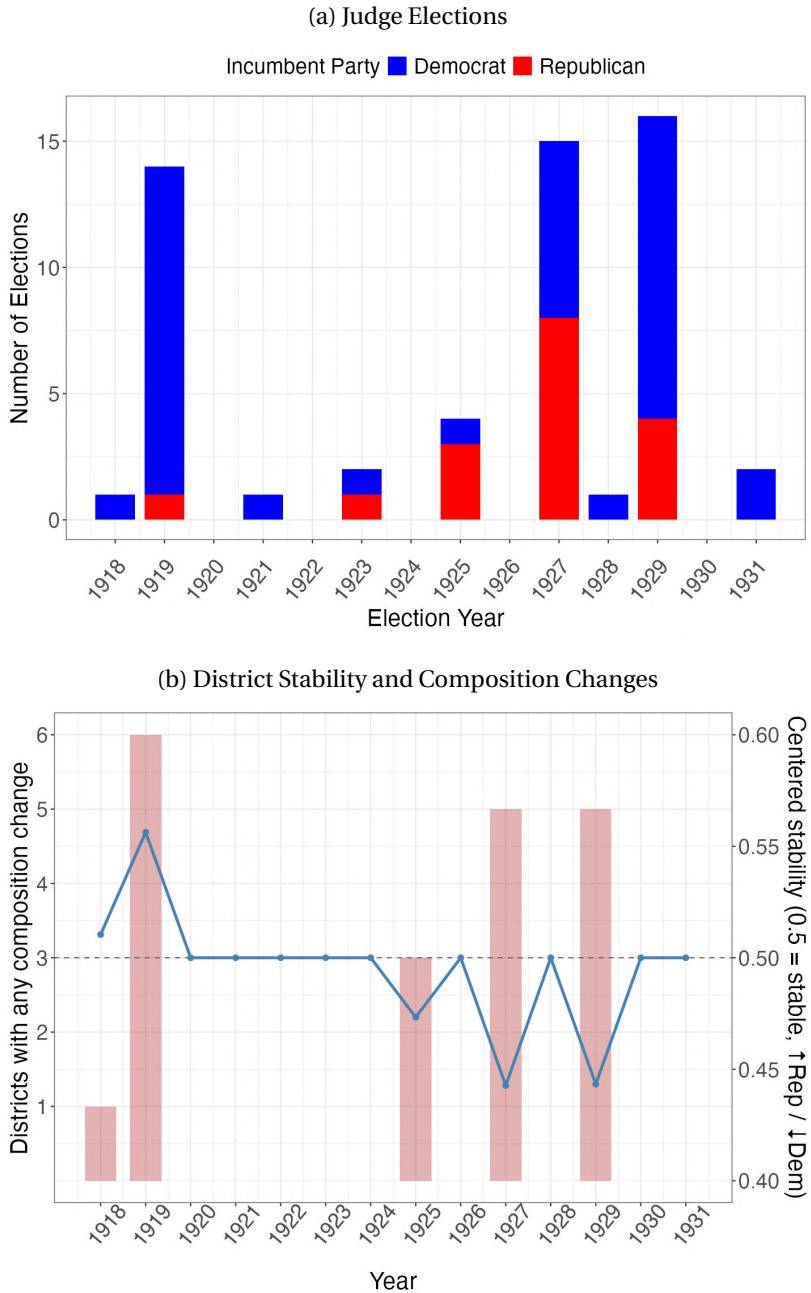
In this index, values above 0.5 indicate that, on average, districts are shifting toward Republicans, while values below 0.5 indicate shifts toward Democrats. A value of exactly 0.5 corresponds to no net directional change in partisan composition.

Panel (b) plots the centered stability index (blue line, right axis) together with the number of districts that experience any change in partisan composition in a given year (bars, left axis). The figure shows that while most years exhibit relatively little movement, major election years feature both a higher number of districts undergoing change and systematic shifts in partisan balance. For example, in the critical election year 1919, a substantial share of MCDs shifted towards Republican control. By contrast, the large election cycles of 1927 and 1929 were characterized by widespread shifts in the opposite direction, with districts moving into Democratic control. Importantly, during the height of rent control from 1920 to 1926, the electoral system was remarkably stable: few districts changed composition, and the centered stability index remained close to 0.5, indicating little systematic partisan drift. This pattern underscores that partisan reshuffling of MCDs occurred in bursts tied to major election years rather than continuously throughout the rent control period.

Finally, to assess how different MCDs were regarding their socio-demographic composition, we rely on the 1920 Decennial Census, since no annual statistics are available at a sufficiently small geographical scale. Individual-level census records were first aggregated to the enumeration district (ED) level and then to Neighborhood Tabulation Areas (NTAs), using overlapping area weights to handle cases where EDs straddled NTA boundaries. Each NTA was assigned to the MCD in which more than half of its area lay, and we then aggregated NTAs to the MCD level. Using this mapping, we constructed MCD-level averages for key socio-demographic indicators, including population size, income (ERSCOR50), tenure status (share owners), and the population shares of Blacks, Whites, and second-generation immigrants.

Next, we grouped MCDs by their judicial composition: all-Democrat (0% Republican judges), all-Republican (100% Republican judges), and mixed districts with an intermediate Republican share. For each group, we calculated the mean of each socio-demographic indicator and report these in Figure C1.2, with vertical bars representing one standard deviation. The figure shows that districts with 100 percent Republican judges and those with 0 percent Republican judges are broadly similar in their average socio-demographic composition. In 1920, these two groups do not differ systematically across indicators such as population size, income, tenure, or racial and immigrant composition. By contrast,

Figure C1.1: Judge Elections, 1918–1931

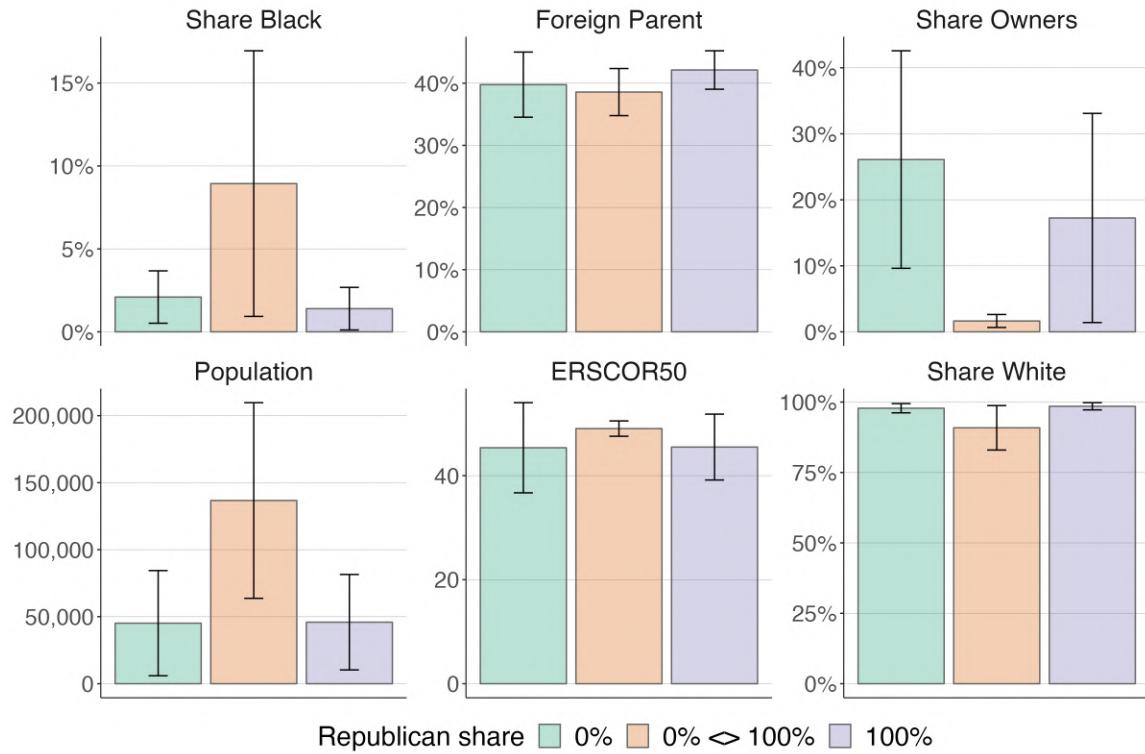


Note. Figure C1.1 summarizes judicial elections by year. Panel (a) reports the absolute number of elections, grouped by the political affiliation of the incumbent judges. Panel (b) shows the number of districts experiencing any partisan change in composition (bars, left axis), together with a stability index centered at 0.5 (line, right axis; zoomed to 0.4–0.6). When a district's partisan composition changed, we attribute the shift to the year in which the election or appointment took place. Thus, for elections, the change is recorded in the election year (e.g. 1926), even if the new judge formally took office the following year; for appointments, the change is recorded in the year of appointment (e.g. a judge appointed in 1926 is treated as a flip in 1926, relative to 1925).

Source. City of New York (1918–1931).

mixed districts stand out: they had a higher Black population share, lower homeownership rates, and substantially larger populations on average.

Figure C1.2: 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 to NTAs 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, Black and White residents, homeownership, income, and population by the share of Republican judges. The vertical lines represent one standard deviation.

Source. Authors' calculations using data from the 1920 U.S. Federal Census, obtained via IPUMS NHGIS (Schroeder et al., 2025).

C2 Listing Rents

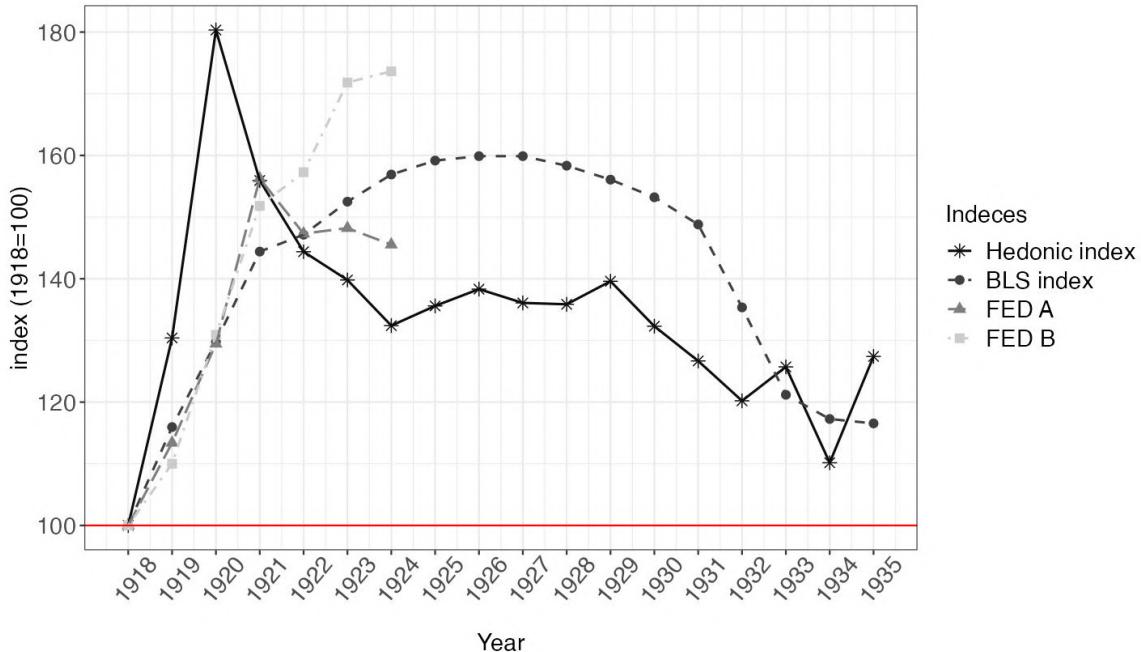
Our data on market rents are drawn from New York Times classified listings. A sample of advertisements was manually digitized, with inclusion based on a set of pre-specified criteria. Each listing had to report (i) an advertised rent, (ii) an exact street address, (iii) a measure of unit size (such as the number of rooms or bedrooms), and (iv) a property type (house or apartment). Additional attributes—such as whether the unit was furnished or whether utilities were included—were also recorded when available.

Listings were sampled on the last Sunday of the second month of each quarter, from January 1918 through November 1926, since Sunday editions contained the largest volume of advertisements. On each sampling date, listings were drawn across all columns of the newspaper to avoid geographic clustering. This procedure produced 15,398 listings across 80 dates. An additional 5,216 listings were collected for 1930, using the same procedure.

Each address was geocoded using a two-step process. Initial coordinates were obtained through the Google Maps API. Because street numbering and, in some cases, street names have changed since the 1920s, a second round of corrections was conducted. This involved cross-referencing street intersections mentioned in the advertisements and consulting historical sources, including Bromley fire insurance maps and PLUTO 2002 shapefiles. [Figure B.4](#) illustrates examples of manually corrected geocodes relative to underlying lots, addresses, and house numbers. [Figure 4](#) shows the spatial distribution of the final set of geocoded rental listings.

To assess the representativeness of our rental listings, we compare the constructed indices to alternative measures of rents available for the same period. As shown in [Figure C2.1](#), our hedonic rent index closely tracks other contemporary series, including the Bureau of Labor Statistics (BLS) rent index and two indices produced by the Federal Reserve. All series exhibit a pronounced increase in rents in the immediate post–World War I years, followed by stabilization and gradual decline in the late 1920s and early 1930s. While the exact timing and magnitude of changes differ across indices, the overall trends are highly consistent. This comparison suggests that our digitized sample of newspaper listings provides a reliable and broadly representative measure of the underlying rental market dynamics in New York City during the interwar period.

Figure C2.1: Rent Indeces



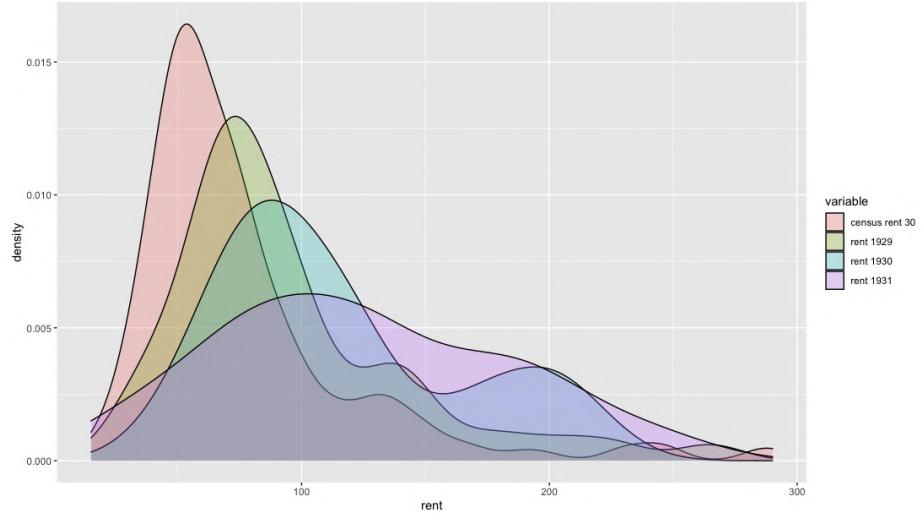
Note. Figure C2.1 shows rent indexes for New York City using 1918 as the base year. The black solid line shows a hedonic index using market rents (Hedonic index). The index values have been obtained from the fixed effects of regressing the logarithm of rent on property-level controls and time-fixed effects. 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 indices are taken from Table 4 in New York (State) (1925).

Source. Authors' own calculations; United States. Bureau of Labor Statistics (n.d.); New York (State) (1925).

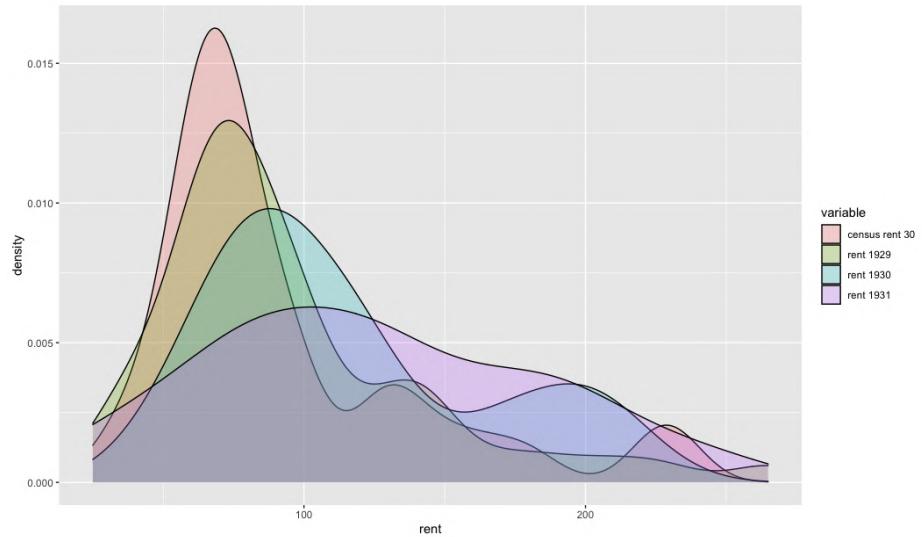
The geographic coverage of the dataset aligns with key features of New York City's rental market. For example, rental coverage is lower in the Lower East Side and higher in the Upper East and West Sides. On average, rents in our sample exceed those reported in the 1930 Census. This difference primarily reflects variation in the frequency with which neighborhoods are sampled. To assess the extent of this spatial bias, we construct frequency weights based on the number of listings observed in each neighborhood relative to the total number of listings. Figure C2.2 confirms that higher average rents in the sample stem largely from differences in neighborhood coverage.

Figure C2.2: Rent distributions

(a) Census and sample distribution



(b) Reweighted census distribution



Note. Figure C2.2 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 C2.2a plots the rent distribution in the 1930s census vs the sample distributions from 1929 to 1931. Panel C2.2b 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 Census average a higher weight when a neighborhood is overrepresented in the Census relative to our sample, and reduce the weight when it is underrepresented.

Source. Author's own calculations; US Federal Census.

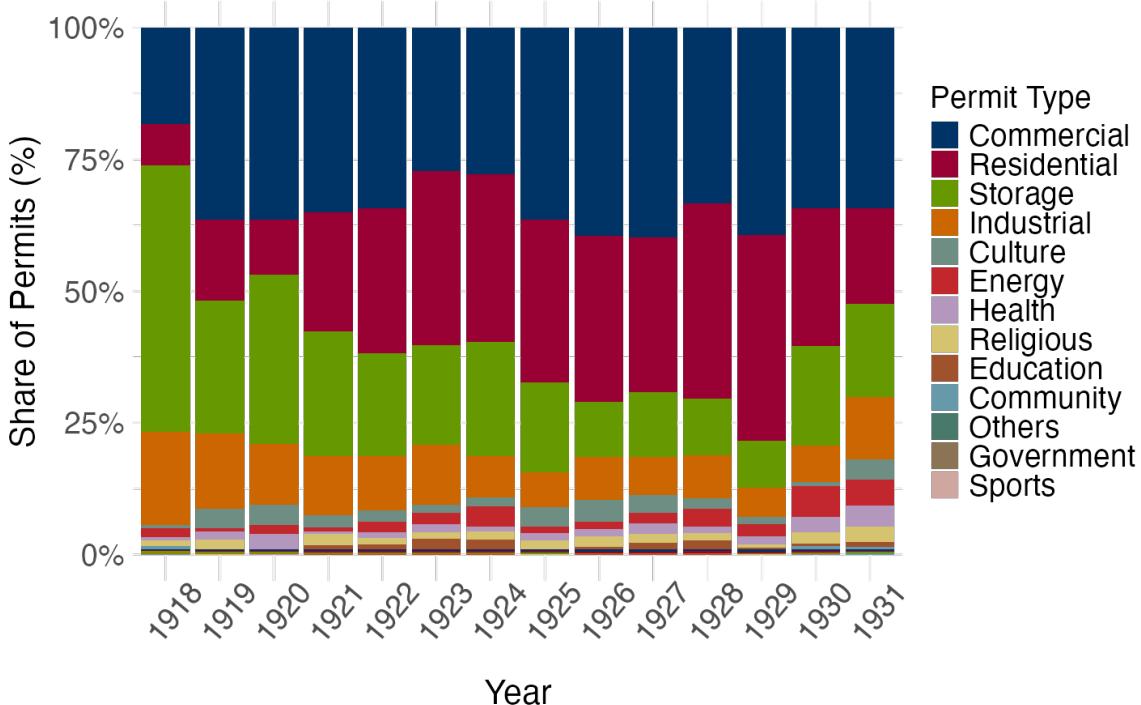
C3 Building Permits

We construct our dataset of building activity by scraping permit records from the Office for Metropolitan History ([2024](#)) website. These records provide rich detail at the project level, including the number of buildings, intended use, free-text descriptions, materials, features (elevators, skylights), the project address, and the estimated cost of development. We use the reported development cost as our primary measure of investment.

Each permit was geocoded using the same two-step procedure as for our rental listings. In a first pass, addresses were located with the Google Maps API. Because many street numbers and some street names have changed since the period under study, these automated matches were sometimes inaccurate. To correct them, we cross-referenced addresses with nearby street intersections, and relied on Bromley fire insurance maps to recover stable coordinates. This procedure ensures that each project is assigned to a historically consistent location, even in the presence of renumbering or street realignments.

Next, we classify permits based on the free-text descriptions of proposed structures. Using a set of keyword dictionaries, we group projects into broad categories such as residential, commercial, industrial and warehousing, energy and fuel, infrastructure, cultural and entertainment, public and health, religious, sports and recreation, government, community, storage and outbuildings, and education. Descriptions that do not match any of these categories are marked as “unclassified.” We further extract information on building materials and features, such as brick, stone, concrete (including reinforced concrete), cement, iron, steel, limestone, terra cotta, cornices, roofing types, and the inclusion of elevators, skylights, or steam heating. These variables provide a useful window into construction technologies and quality upgrades over time. [Figure C3.1](#) shows the composition of permits by year.

Figure C3.1: Composition of Building Permits

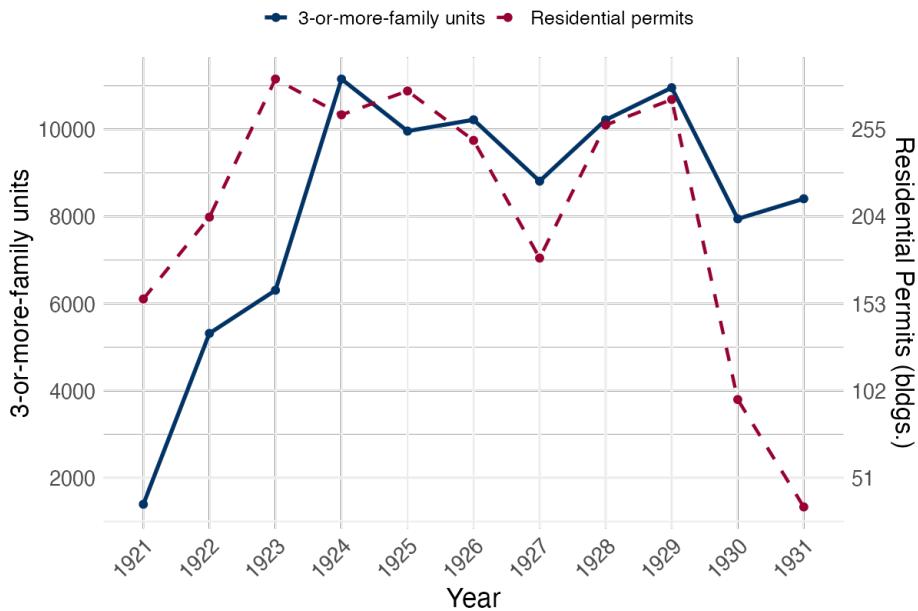


Note. Figure C3.1 shows the composition of building permits by category in New York City, 1918–1931. Bars show the share of each permit type in total permits issued each year. Three categories dominate throughout the period: the first segment remains consistently large, the second expands notably after 1921, and the third declines relative to its prominence in the late 1910s. Smaller categories, shown in thinner bands near the bottom, contribute only marginally to the overall distribution.

Source. Office for Metropolitan History (2024).

Finally, we benchmark our permit counts against completed multi-family buildings with more than three dwellings. The trends in permitted residential structures closely track completions, with the expected lag between authorization and construction. This comparison lends confidence that the permit data capture meaningful variation in building activity. With the exception of the year 1918, residential, commercial, storage, and industrial projects account for the bulk of total investment during our study period.

Figure C3.2: Residential Permits (bldgs.)



Note. Figure C3.2 shows the annual number of newly constructed 3-or-more-family buildings (left axis, solid blue line) and residential building permits (right axis, dashed red line), New York City, 1921–1932.

Source. 3-or-more-family building counts from Grebler (2019); residential building permits from Office for Metropolitan History (2024).

D Additional Results

D1 RDD estimates for Manhattan

Table D1.1: 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 (0.182)	-0.021 (0.168)	0.078 (0.141)	0.054 (0.131)	-0.207 (0.274)	-0.084 (0.246)	0.199 (0.210)	-0.012 (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 ^u _{rb}	0.351	0.332	0.668	0.318	0.333	0.471	1.064	0.354

Note. Table D1.1 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 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, 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 D1.2: 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 (0.069)	0.021 (0.056)	0.079 (0.067)	0.069* (0.041)	-0.003 (0.127)	0.102 (0.091)	0.202** (0.093)	0.018 (0.069)
Controls	X	✓	✓	✓	X	✓	✓	✓
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 ^u _{rb}	0.236	0.139	0.381	0.147	0.284	0.319	0.428	0.232

Note. Table D1.2 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 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, 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.

D2 RDD estimates for alternative treatment boundary

Table D2.1: 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 (0.103)	-0.017 (0.100)	-0.134 (0.104)	-0.029 (0.077)	0.059 (0.163)	-0.044 (0.110)	-0.162 (0.143)	-0.068 (0.083)
Controls	X	✓	✓	✓	X	✓	✓	✓
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 ^u _{rb}	0.245	0.162	0.143	0.179	0.419	0.161	0.125	0.115

Note. Table D2.1 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 B.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, 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.

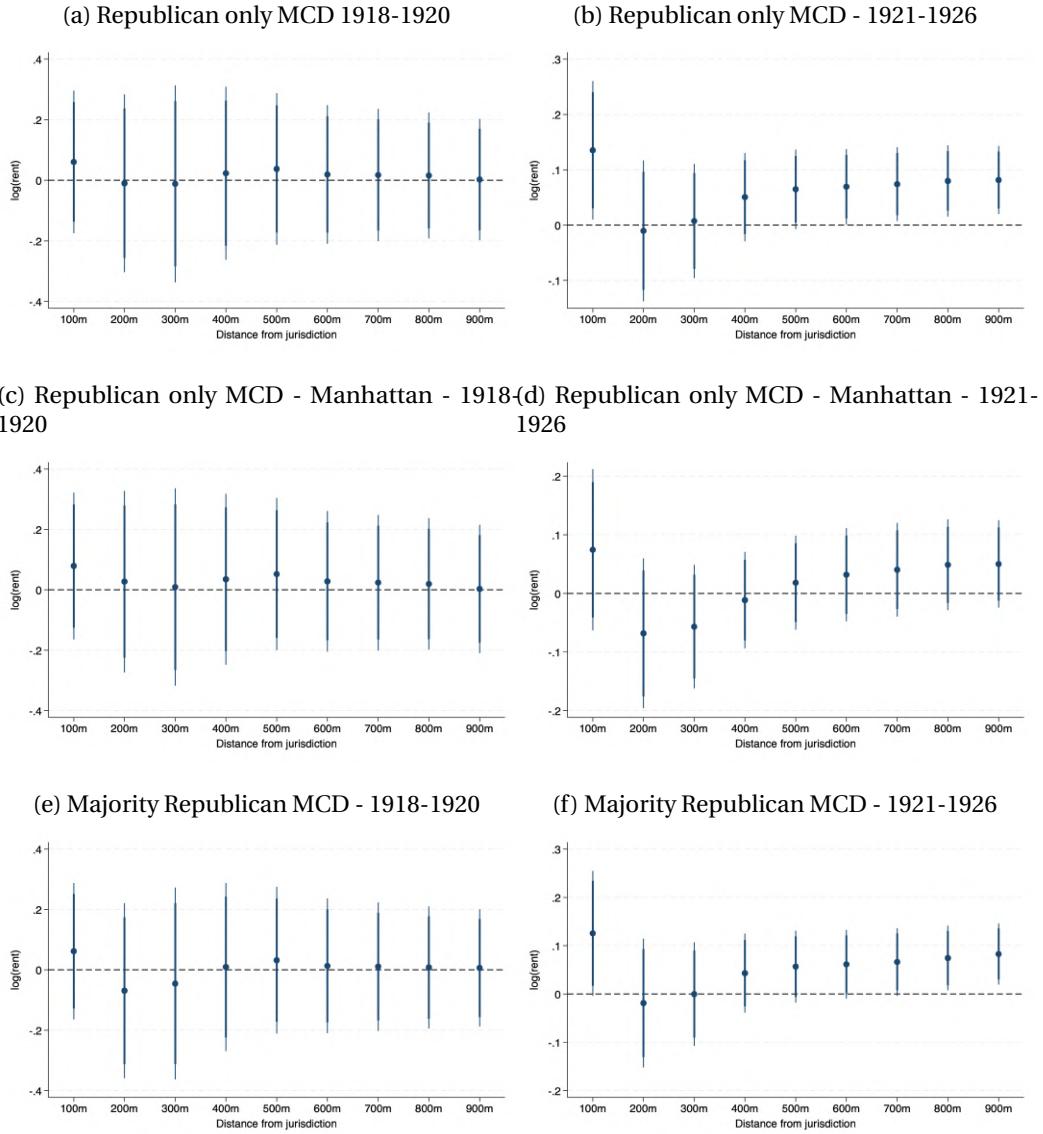
Table D2.2: Effect at cut-off on rental prices (1920–1926): alternative boundary

	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*** (0.032)	0.066 (0.036)	0.010 (0.055)	0.097*** (0.026)	0.118** (0.040)	0.085* (0.040)	0.045 (0.047)	0.118*** (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 ^u _{rb}	0.190	0.141	0.196	0.164	0.218	0.177	0.164	0.192

Note. Table D2.2 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 B.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, 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.

D3 RDD estimates for Alternative bandwidth choices

Figure D3.1: Alternative bandwidth - Effect at cut off on rental price



Note. Figure D3.1 shows RD estimates from estimating [Equation 3](#) for different bandwidth choices using the full set of property level controls, year and neighborhood fixed effects; [Equation 3](#) 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 D3.1a and D3.1b use the distance to the boundary between Republican and Democrat only MCDs; Panel D3.1c and D3.1d subset the sample for Manhattan only; Panel D3.1e and D3.1f use the distance to the boundary between majority and 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.

D4 Difference-in-Differences

Table D4.1: Effect of Rent Control on Investment: Share Repub. Judges

	Any	Private	No Com.
$Post_{20-28} \times Res$	0.514*** (0.154)	0.558*** (0.173)	0.613*** (0.122)
$Post_{29-31} \times Res$	0.096 (0.206)	0.058 (0.210)	0.329* (0.171)
$Post_{20-28} \times Mix$	-0.203 (0.321)	-0.236 (0.345)	-0.148 (0.293)
$Post_{29-31} \times Mix$	-0.310 (0.441)	-0.435 (0.424)	-0.165 (0.399)
Distance	✓	✓	✓
Material FE	✓	✓	✓
Usage FE	✓	✓	✓
NTA FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	7,098	6,314	5,242
R ²	0.52	0.51	0.59

Table D4.1 table reports difference-in-differences regressions of log project cost on rent control exposure, following Equation 4. Treatment groups are residential-only permits and mixed-use permits with a residential component. Control groups vary across columns between (i) all non-residential permits (Any), (ii) private non-residential permits (Private), and (iii) non-commercial private permits (No Commercial). Post₂₀₋₂₈ and Post₂₉₋₃₁ are indicators for the rent control periods 1920–1928 and 1929–1931. Exposure is measured as the share of Republican judges in an MCD. All specifications include distance controls, material fixed effects, usage fixed effects, neighborhood (NTA) fixed effects, and year fixed effects. Standard errors in parentheses are clustered at the neighborhood (NTA) level. ***, **, * indicate significance at the 1, 5, and 10 percent levels, respectively.

D5 Event study results

D5.1 Rent Price Effects

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 D.1 gives our event study specification specification:

$$y_{i,m,t} = \sum_{\tau} \beta_{\tau} \cdot post_{1920} \cdot Treat_{t,u}(\tau = t - 1920) + \mathbf{D}_{i,t} + \mathbf{X}_{i,t} + \gamma_t + \gamma_m + u_{i,m,t} \quad (\text{D.1})$$

where again $y_{i,m,t}$ is the listed rent for observation i in MCD m in year t . The variable $Treat_{t,u}$ 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 $\mathbf{X}_{i,t}$, as per Equation 3, while γ_t and γ_m are time and neighborhood (NTA) fixed effects. 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.

Results from estimating Equation D.1 for our rent data are shown in Figure D5.1. 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 Panels (c) and (d). 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.

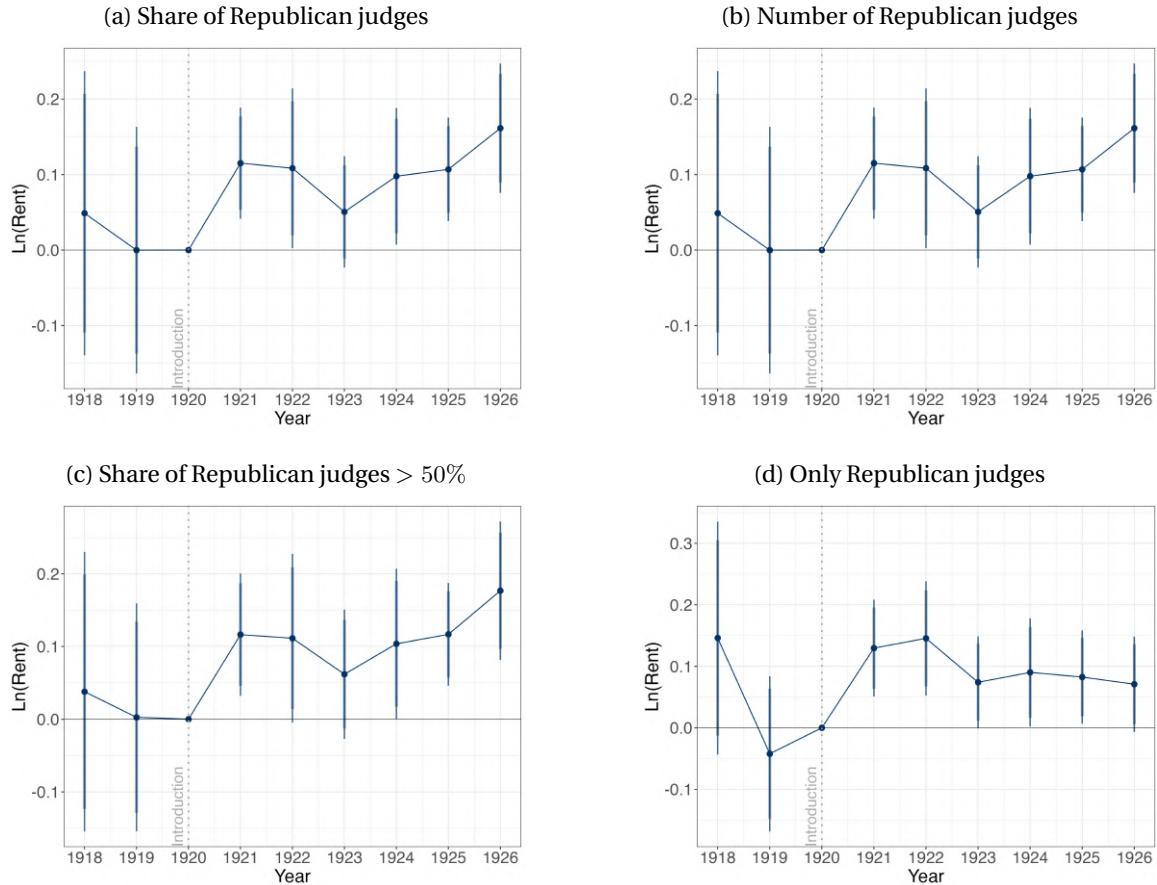
D5.2 Investment Effects

To complement the DiD results, we also estimate an event-study specification for investment, interacting year dummies with continuous measures of judicial composition. Specifically, we estimate equations of the form

$$\begin{aligned} y_{i,m,t} = & \sum_{\tau \neq 1920} \beta_{\tau} \mathbf{1}(t = \tau) \times Treat_i \times Exposure_i \\ & + \mathbf{D}_{i,t} + \mathbf{C}_i + \mathbf{U}_i + \gamma_t + \gamma_m + u_{i,m,t}. \end{aligned} \quad (\text{D.2})$$

where $y_{i,m,t}$ is log project cost, $Treat_i$ indicates whether the permit is residential (or mixed-use with a residential component), and $Exposure_m$ measures judicial composition

Figure D5.1: Effect of Residential-only Investment Treatments



Note. Figure D5.1 reports point estimates for β_τ in Equation D.1, estimated with the full set of property-level controls, year fixed effects, and neighborhood (NTA) fixed effects. Panels (a) and (b) use the share and the number of Republican judges in an MCD as continuous treatments, including all districts (also those with mixed partisan composition). Panels (c) and (d) use binary treatments: in panel (c) year dummies are interacted with an indicator equal to one if the share of Republican judges in an MCD exceeded 50%, and in panel (d) with an indicator equal to one if an MCD was either fully Republican or fully Democratic, thereby excluding mixed districts. Standard errors are clustered at the neighborhood (NTA) level. The bars show 90% and 95% confidence bands.

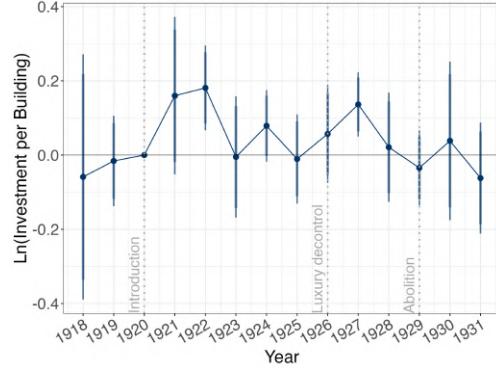
either as the share or the number of Republican judges in MCD. All specifications include distance controls, material and usage fixed effects, as well as year and neighborhood (NTA) fixed effects.

Figure D5.2 shows the main results for residential permits, using any non-residential and private non-residential permits excluding commercial ones as the control group. The estimates demonstrate that residential investment increased sharply in Republican districts during the rent control years, with effects consistent across both exposure measures. Fully Republican districts saw investment levels around 60–80 percent higher than fully Democratic districts, while each additional Republican judge is associated with a 13–17 percent increase in residential investment. Importantly, the coefficients flatten out and lose significance once rent controls were phased out after 1929, underscoring that the effect was confined to the rent control era.

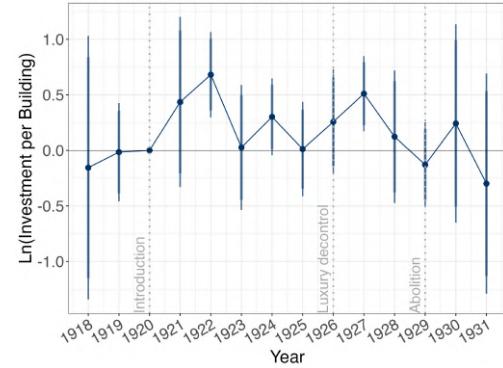
Additional results for mixed-use permits and alternative control groups are reported in Figure D5.3. These estimates are generally smaller, negative, and imprecise, consistent with the fact that mixed-use projects were only partially exposed to rent control. Together, the main table and appendix results reinforce the model's Proposition 2: by lowering the expected return on residential projects, rent control led to a pronounced increase in housing investment in districts with landlord friendly judges during the 1920s, with no evidence of persistent effects after repeal.

Figure D5.2: Effect of Residential-only Investment Treatments

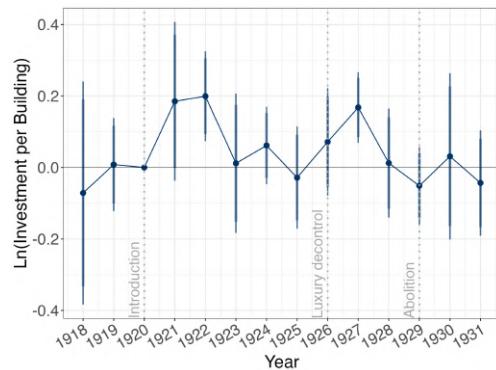
(a) Number of Republican judges, Any control group



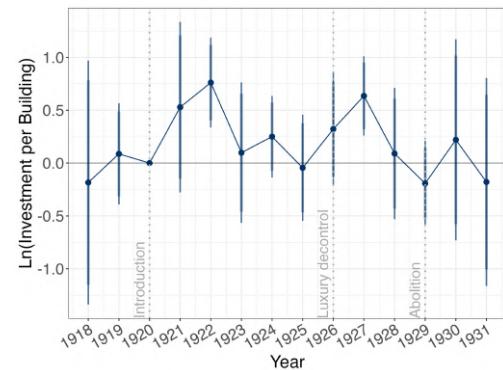
(b) Share of Republican judges, Any control group



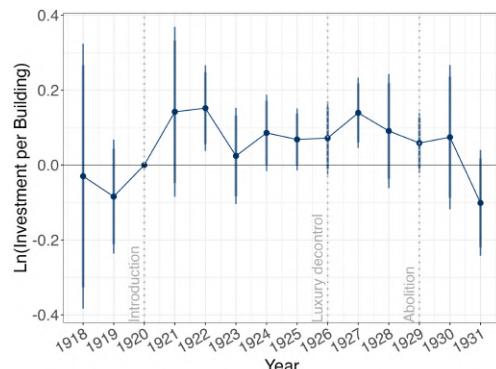
(c) Number of Republican judges, Private control group



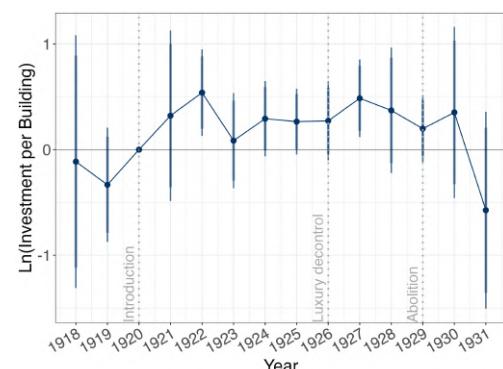
(d) Share of Republican judges, Private control group



(e) Number of Republican judges, No Commercial control group



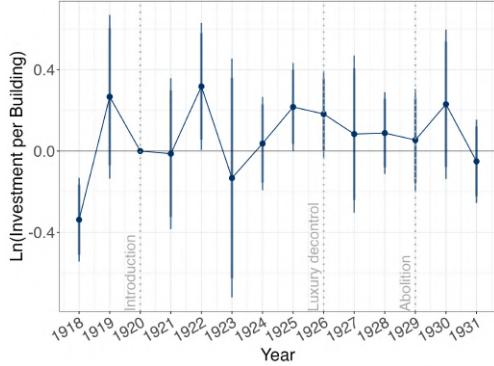
(f) Share of Republican judges, No Commercial control group



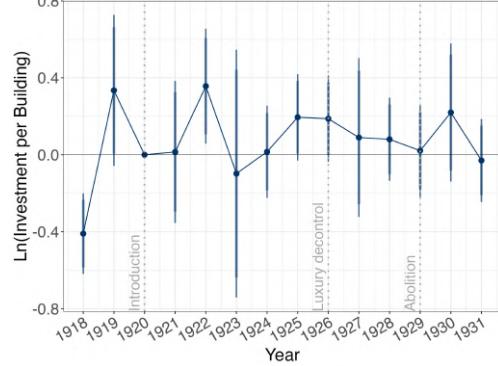
Note. Figure D5.2 reports event-study estimates of the effect of rent control on residential investment using alternative sets of private non-residential permits as controls. Panels (a) and (b) use all non-residential permits as the control group; Panels (c) and (d) use private non-residential permits; Panels (e) and (f) use private non-residential permits excluding commercial. Panels (a),(c) and (e) use the number of Republican judges in an MCD as the continuous treatment; Panels (b),(d) and (f) use the share of Republican judges. All specifications include project-level controls, year and neighborhood (NTA) fixed effects. Standard errors are clustered at the neighborhood level. The bars show 90% and 95% confidence bands.

Figure D5.3: Effect on Mixed-Use Investment

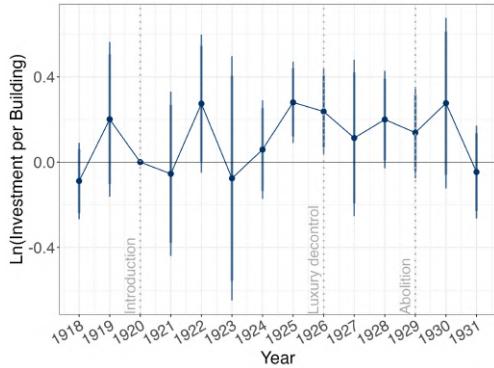
(a) Number of Republican judges, Any control group



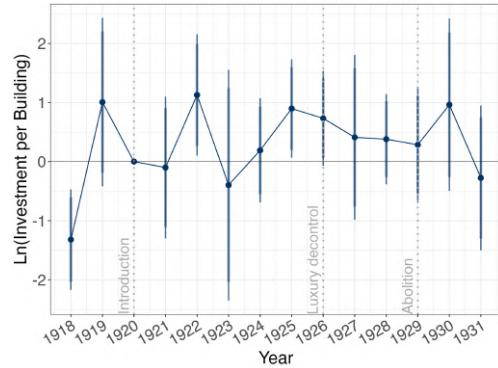
(b) Share of Republican judges, Private control group



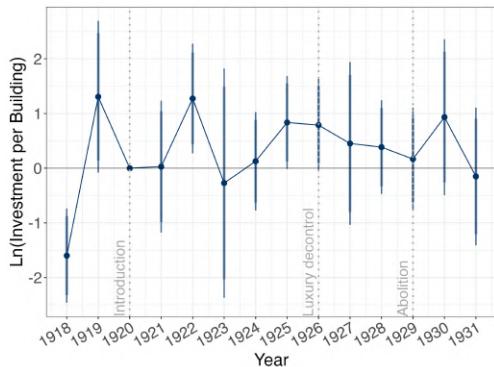
(c) Number of Republican judges, No Commercial control group



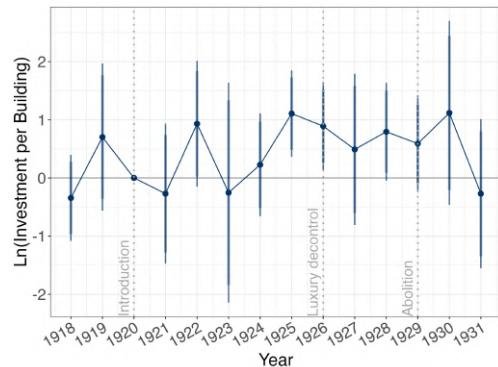
(d) Share of Republican judges, Any control group



(e) Number of Republican judges, Private control group



(f) Share of Republican judges, No Commercial control group



Note. Figure D5.3 reports event-study estimates of the effect of rent control on mixed-use investment, with mixed-use permits as the treatment group and alternative sets of private non-residential permits as controls (Any, Private, and No Commercial). Panels (a), (c) and (e) use the number of Republican judges in an MCD as the continuous treatment, while Panels (b), (d) and (f) use the share of Republican judges. All specifications include project-level controls, year and neighborhood (NTA) fixed effects. Standard errors are clustered at the neighborhood level. The bars show 90% and 95% confidence bands.

D6 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, properties 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 4, 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 3 in this setup in Table D6.1.

Table D6.1: 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}	$\hat{b}/2$	$\hat{b} * 2$
β_{rdd}	0.328*** 0.095	0.041 0.041	-0.064 0.071	0.072 0.042	0.303* 0.129	-0.017 0.061	-0.082 0.080	0.059 0.054
Controls	X	✓	✓	✓	X	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	582.966	441.717	220.858	883.434	1274.329	850.755	425.377	1701.509
Obs.	5216	5077	5077	5077	5216	5077	5077	5077
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 ^u _{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 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, 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, where dwelling-level controls are included, there is no evidence that rent prices jump at the border between previously Democrat-controlled and previously Republican-controlled districts. 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 the end of rent controls, aid a causal interpretation of the results during the Control period.

They also suggest that the duration of controls did not lead to longer-lasting effects, such as income sorting, in the rental sector.