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

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Appendix Contents

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

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

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

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

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 $\mathbb{E}[r_{j,t} \mid \text{raise}] = r_j^c - c_\ell \le r_{j,t-1}$ 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*. □

Proof of Proposition 2. The developer chooses $k \ge 0$ 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 $R_j(p_j) = r_{j,0} + \beta\big[q\, r^e_{j,1}(p_j) + (1-q) r_{j,1}\big]$ where $r^e_{j,1}(p_j) = p_j r_{j,1} + (1-p_j)(r_j^c - c_\ell)$.

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

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

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

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

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

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

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

By the implicit-function theorem,

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

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

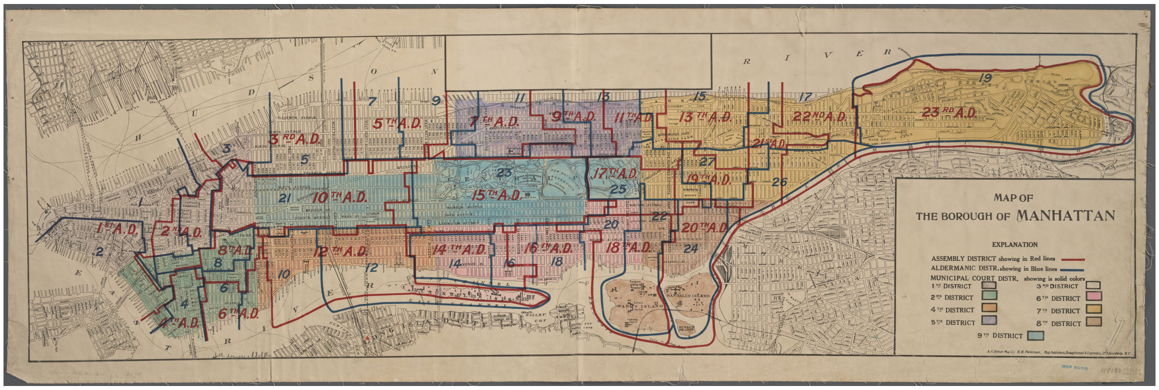
4. Investment gap and integral identity. Let $R^0 \equiv r_{j,0} + \beta r_{j,1}$ denote the no-control return. Because $r_{j,1}^e(p_j) \leq r_{j,1}$ with strict inequality when the cap binds and $p_j < 1$, we have $R_j(p_j) \leq R^0$ (strictly < when binding and $p_j < 1$). Using $k^*(R) = \phi^{-1}(R)$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

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

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. □

B Supplementary Maps

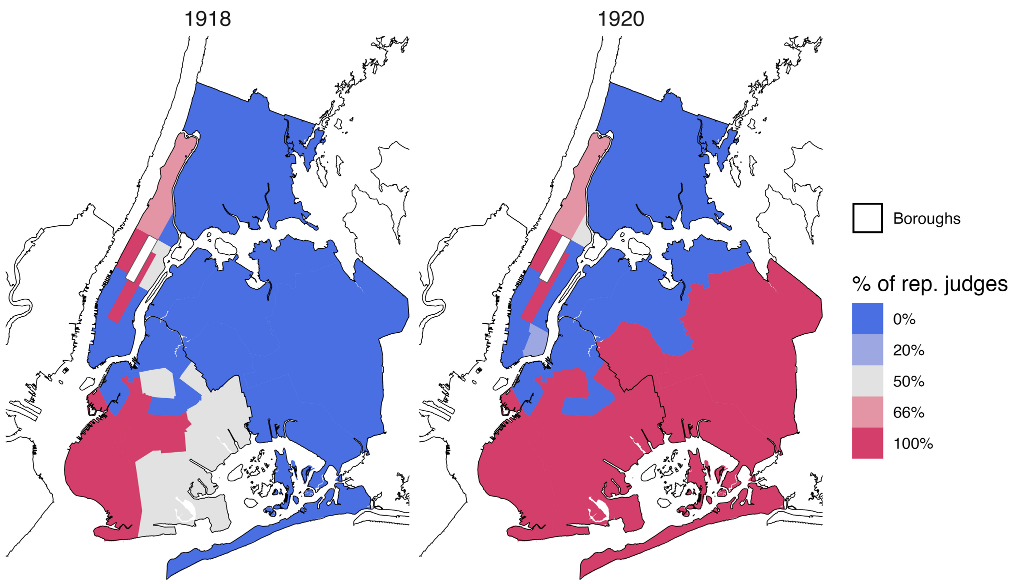
Figure B.1: Historical Municipal District Courts - Manhattan



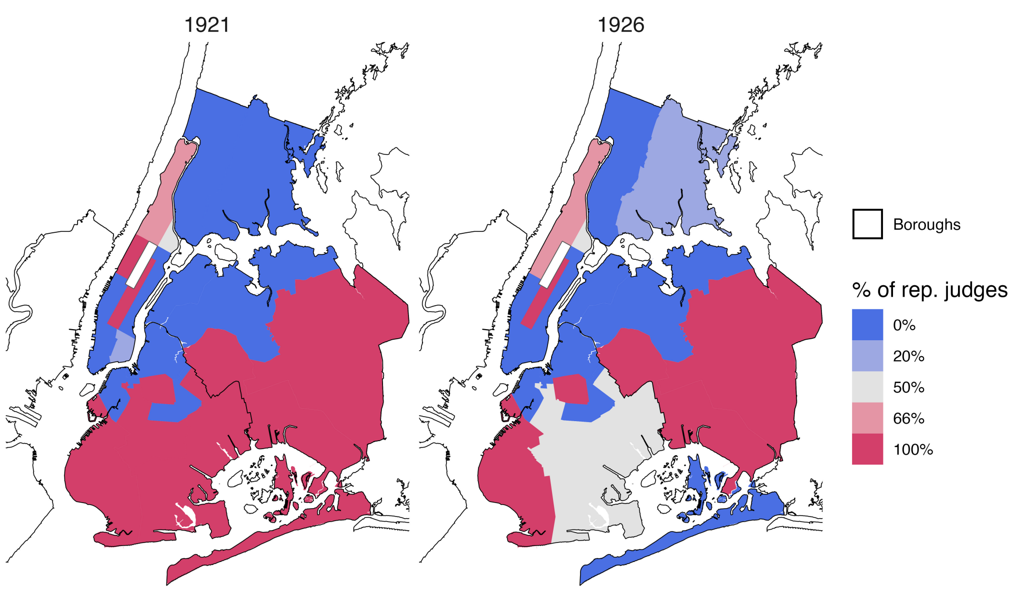
Note. Figure B.1 shows the Borough of Manhattan, the Assembly, Aldermanic, and Municipal Court Districts in 1918.
Source. Lionel Pincus and Princess Firyal Map Division, The New York Public Library (1918). Map of the Borough of Manhattan, showing the Assembly, Aldermanic, and Municipal Court Districts.

Figure B.2: Share of Republican judge

(a) Pre rent control

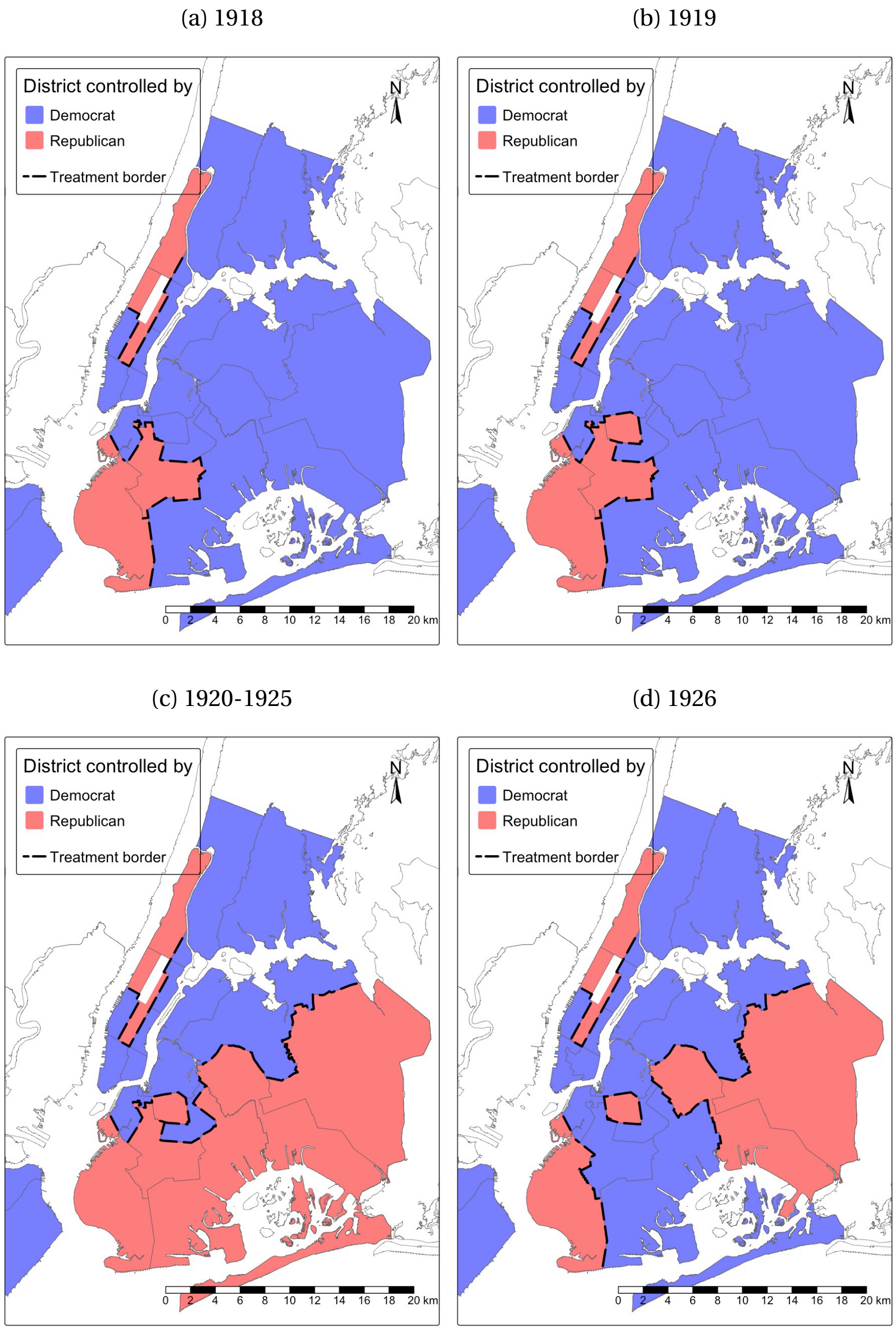


(b) Post rent control



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 nore changes from 1920 to 1925 in Panel.

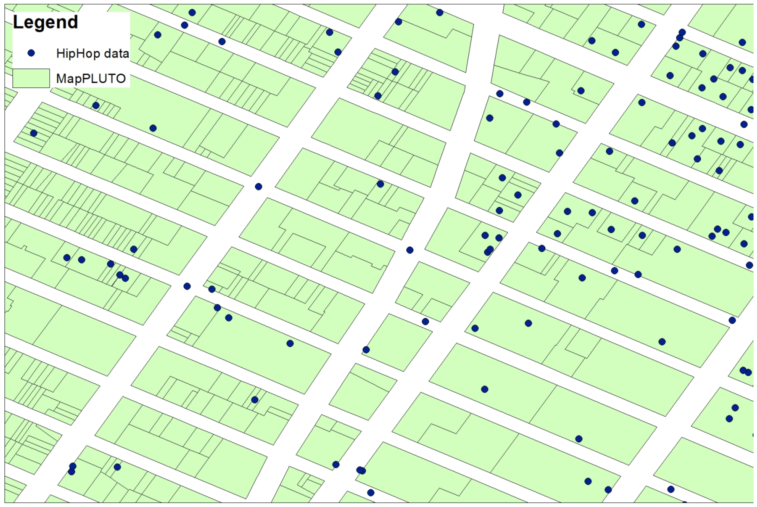
Figure B.3: Alternative treatment boundary



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%; therfore there are no mixed colored districts. The dotted line gives our treatment boundary; in our baseline treatment, we consider the distance to majority Republican and majority Democrat MCDs; since elections alter the spatial distribution of judges, we plot the variation in treated and control MCDs in Panel (a) to (d); note that there nore changes from 1920 to 1925 in Panel (c).

Figure B.4: Example of manual geocoding

(a) PLuto 2002 lot files



(b) Bromley fire insurance maps

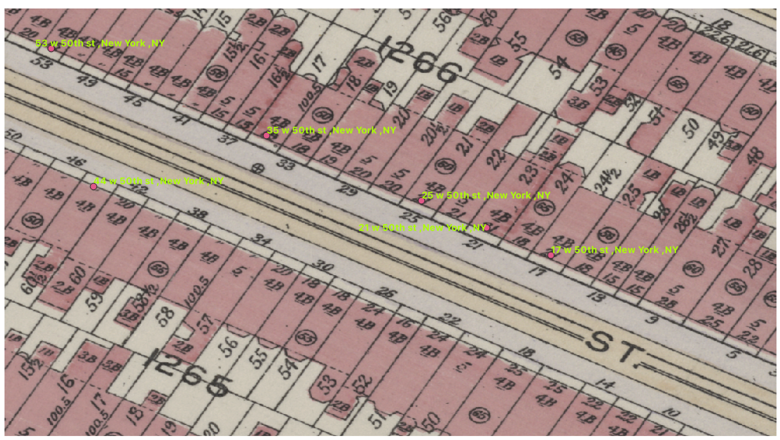
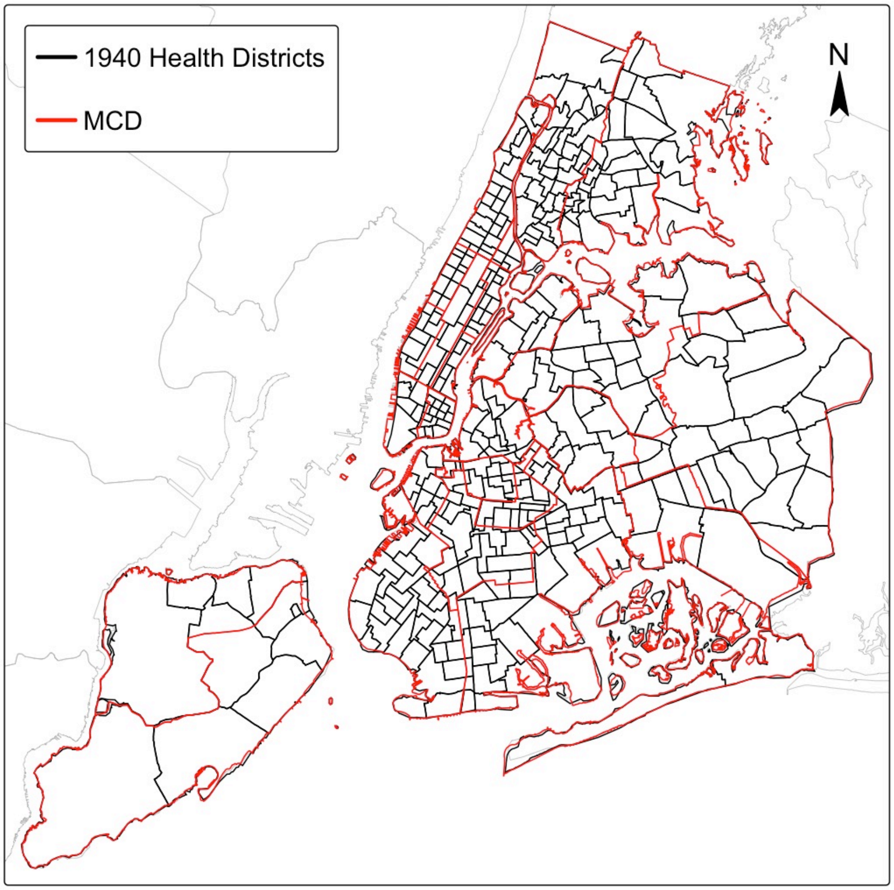


Figure B.5: 1940 Health Districts and Municipal Court Districts in NYC



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 & Bath $100 A Month

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

55 West 86th St. JUST COMPLETED

High class housekeeping, kitchenette or bachelor apartments. Exceptionally large, light rooms with unusually spacious closets.

Living room 13x23, all outside, extra large rooms, elevator; new and thoroughly modern.

SENIOR & ALLEN, Inc. 505 Fifth Ave.

4 ROOMS $65.00

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

SEVEN ROOMS AND TWO BATHS

1109-1111 Madison Ave. CORNER 83D ST.

Elegant high-class apartment. All light rooms. Possession. Rent $3,500 per annum.

JOHN A. SCHOEN, 115 Bible House. Tel. Stuy. 7695.

Hendrik Hudson Annex

110th Street & Broadway Northwest Corner. 7 Rooms, $3,100. 8 Rooms (Corner) $3,600.

The Rockfall

545 West 111th St. Northeast Corner Broadway. 6 and 7 Rooms, $2,500. 8 Rooms (Corner) $3,200. Apply on premises, or

NASSOIT & LANNING, B'way & 89th St. Tel. 8380 Riverside

56 ST.—342 WEST

ONE BLOCK FROM BROADWAY. High-Class Elevator Apartment House. 3 ROOMS AND BATH. APPLY SUPT. ON PREMISES.

690 RIVERSIDE DRIVE,

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

Unfurnished—East Side.

Only 2 Apartments Left

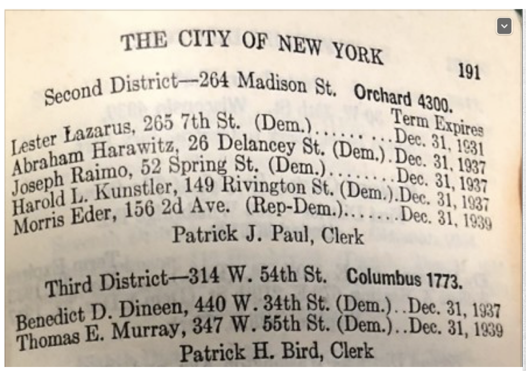
Belgrave Block

Madison Ave., 49th to 50th St. 2 - 3 Rooms—$900 to $1,500

Cruikshank Company 141 Broadway Rector 4100

Worthington Whitehouse, Inc., 446 Madison Av. Plaza 4600.

(b) Green Book



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Second District—264 Madison St. Orchard 4300.

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

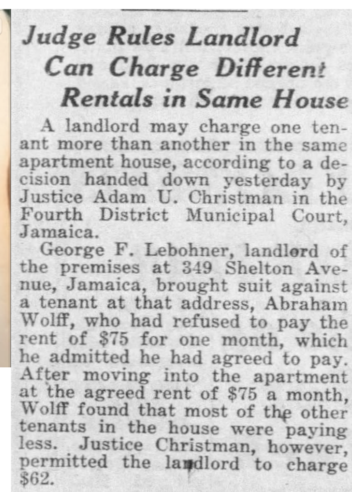
Patrick J. Paul, Clerk

Third District—314 W. 54th St. Columbus 1773.

Benedict D. Dineen, 440 W. 34th St. (Dem.)	Dec. 31, 1937
Thomas E. Murray, 347 W. 55th St. (Dem.)	Dec. 31, 1939

Patrick H. Bird, Clerk

(c) Daily News



Judge Rules Landlord Can Charge Different Rentals in Same House

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

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

Note. Figure 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

	Year													
	1918	1919	1920	1921	1922	1923	1924	1925	1926	1927	1928	1929	1930	1931
Panel A: Rent Data														
Monthly Rent	148.84	162.31	279.26	185.98	156.38	157.34	133.12	137.94	141.78				135.20	
	(170.226)	(141.927)	(457.945)	(144.968)	(143.868)	(135.306)	(115.009)	(103.971)	(148.028)				(134.335)	
Rooms	5.29	3.77	3.43	4.09	3.72	3.49	4.06	4.10	3.54				2.94	
	(2.684)	(2.136)	(2.104)	(2.310)	(2.157)	(2.226)	(1.958)	(1.892)	(2.046)				(1.933)	
# Listings	906	1587	1037	1876	1832	1734	1984	2332	2110				5688	
Panel B: Investment (in $1,000s) & Building Permits														
All	45.6	227.1	302.0	232.0	334.6	311.7	410.7	577.1	535.7	535.6	605.6	980.9	540.2	531.0
	(99.7)	(542.0)	(682.1)	(984.7)	(2,059.0)	(691.4)	(1,184.2)	(1,188.2)	(930.5)	(1,110.1)	(1,169.0)	(1,812.8)	(1,258.7)	(1,923.3)
# Permits	163	343	428	557	630	626	695	765	626	536	586	597	324	186
Resid. Only	165.9	325.0	426.1	278.1	321.8	349.5	499.7	538.8	510.6	580.9	586.8	838.3	648.2	372.0
	(181.5)	(394.5)	(735.7)	(322.5)	(306.3)	(309.1)	(503.7)	(508.0)	(650.6)	(733.3)	(653.2)	(775.2)	(668.9)	(451.4)
# Resid. Only	11	48	39	109	153	210	214	213	175	142	213	205	68	33
Resid. Mixed	42.7	272.2	378.8	211.0	386.2	509.9	561.7	865.5	836.7	576.8	790.9	1471.6	635.7	82.2
	(50.0)	(262.2)	(776.9)	(322.7)	(535.2)	(1062.6)	(637.0)	(986.8)	(1421.0)	(704.9)	(1091.8)	(1746.3)	(466.3)	(44.7)
# Resid. Mixed	3	11	11	30	47	23	27	55	64	38	48	69	30	4
Panel C: Judges														
Judges	2.33	2.35	2.48	2.49	2.49	2.49	2.46	2.46	2.46	2.46	2.46	2.46	2.55	2.97
	(1.022)	(0.994)	(1.243)	(1.214)	(1.214)	(1.214)	(1.220)	(1.220)	(1.220)	(1.220)	(1.220)	(1.220)	(1.153)	(1.472)
# Judges	45	46	46	47	47	47	48	48	48	48	48	48	53	61
Rep. Judges	0.93	1.11	1.04	1.02	1.02	1.02	1.00	0.94	0.85	0.85	0.62	0.62	0.38	0.66
	(1.338)	(1.524)	(1.349)	(1.343)	(1.343)	(1.343)	(1.337)	(1.262)	(1.220)	(1.220)	(1.178)	(1.178)	(1.078)	(1.797)
# Rep. Judges	15	17	20	20	20	20	20	19	17	16	11	11	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
0. Grant Esterbrook	New York Tribune	1920	Jul	24	0	0	
Aaron J. Levy	Daily News	1922	Jun	21			1
Abram Ellenbogen	The Evening World	1920	Jan	14			0
Abram Ellenbogen	New York Times	1920	April	21			0
Adam Christmann, Jr.	Daily News	1921	Nov	12	1	0	
Benjamin Hoffman	New York Times	1920	Apr	13	1	1	0
Benjamin Hoffman	The Sun	1920	Apr	13	1	1	0
Charles B. Law	The Evening World	1921	Sat	8	1	1	
Charles J. Carroll	Daily News	1926	Sep	29			0

Edgar F. Hazelton	The Brooklyn Daily Eagle	1920	Oct	29	1	1	
Edgar F. Hazelton	The Brooklyn Daily Eagle	1920	Oct	29	0	0	
Edgar F. Hazelton	The Brooklyn Daily Eagle	1921	Aug	24			1
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar F. Hazelton	Standard Union	1922	Aug	11			0
Edgar J. Lauer	New York Herald	1921	May	13	0	0	0
Edgar M. Doughty	The Brooklyn Daily Eagle	1921	Jun	22	1	1	
Edgar M. Doughty	Standard Union	1922	Apr	16			1
Edgar M. Doughty	Standard Union	1923	Aug	20	1	0	
Frank J. Coleman, Jr.	New York Herald	1921	Jan	18	1	1	
George L. Genung	The Evening World	1921	Feb	4	1	1	
George L. Genung	New York Times	1921	Oct	22	0	0	
Harrison C. Glore	Standard Union	1921	May	13			0
Harry Robitzek	New York Herald	1922	Jan	26			0
Harry Robitzek	The Evening World	1922	Mar	14	1	0	
Harry Robitzek	Daily News	1920	Apr	9	0	0	
Harry Robitzek	New York Times	1920	Apr	29	0	0	0
Harry Robitzek	New York Times	1923	Jan	24	1	0	
Jacob Marks	Evening World	1921	Apr	28			
Jacob Marks	New York Times	1922	Apr	16			1
Jacob Panken	New York Tribune	1920	May	7			1
Jacob Panken	New York Herald	1922	Nov	24			1
Jacob S. Strahl	New York Times	1920	Jan	1			1
Jacob S. Strahl	New York Times	1920	Jan	1			1
Jacob S. Strahl	The Evening World	1920	Sep	20	1	1	
Jacob S. Strahl	New York Herald	1922	May	9			1
James A. Dunne	Standard Union	1922	Jan	4			1
James A. Dunne	New York Herald	1921	May	3			1
James A. Dunne	Standard Union	1921	Dec	18	0	0	
James A. Dunne	The Evening World	1922	Jan	14	1	0	
John G. McTigue	Daily News	1921	Sep	16	1	1	
John Hetherington	Brooklyn Times	1922	Jan	25			0
John Hetherington	New York Times	1922	Jul	2			1
John M. Cragen	Brooklyn Times	1921	Dec	11			0
John M. Cragen	Brooklyn Times	1922	Jan	25			1
John R. Davies	New York Tribune	1921	Nov	25	1	1	
John R. Davies	New York Times	1920	Apr	21	1	0	
John R. Farrar	The Brooklyn Daily Eagle	1922	Jun	22	1	1	
John R. Farrar	The Brooklyn Daily Eagle	1922	Jun	22	1	1	
Leopold Prince	New York Times	1920	Apr	29	1	0	
Leopold Prince	New York Times	1924	Jan	27	1	1	
Michael J. Scanlan	Evening World	1920	Sep	9	1	0	
Michael J. Scanlan	Daily News	1920	Sep	3	1	0	
Michael J. Scanlan	New York Tribune	1920	May	7	1	0	
Samson Friedlander	New York Herald	1921	Oct	27	1	0	

Samson Friedlander	New York Tribune	1920	May	7			0
Thos. E. Murray	New York Tribune	1920	May	8			0
Timothy A. Leary	New York Times	1922	Jun	20			1
William Blau	New York Tribune	1920	Aug	1	1	0	
William Blau	New York Tribune	1920	Aug	1			0
William C. Wilson	New York Times	1920	April	21	1	0	
William E. Morris	New York Tribune	1920	May	8	1	0	
William E. Morris	New York Herald	1922	Apr	13			1
William E. Morris	Democrat and Chronicle	1920	Aug	10	1	1	1
William F. Moore	The Evening World	1921	Sep	6	1	1	
William J. A. Caffrey	Daily News	1921	Dec	11			1
William J. Bogenshutz	Standard Union	1923	Nov	5	0	0	0
William J. Bogenshutz	Standard Union	1922	May	14	0	0	
William Young	New York Times	1921	Apr	10	0		0

Note. Table 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)).[14]

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, $share_{d,\text{Rep},t}$, 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:

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

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

[14] Though 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.

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

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.

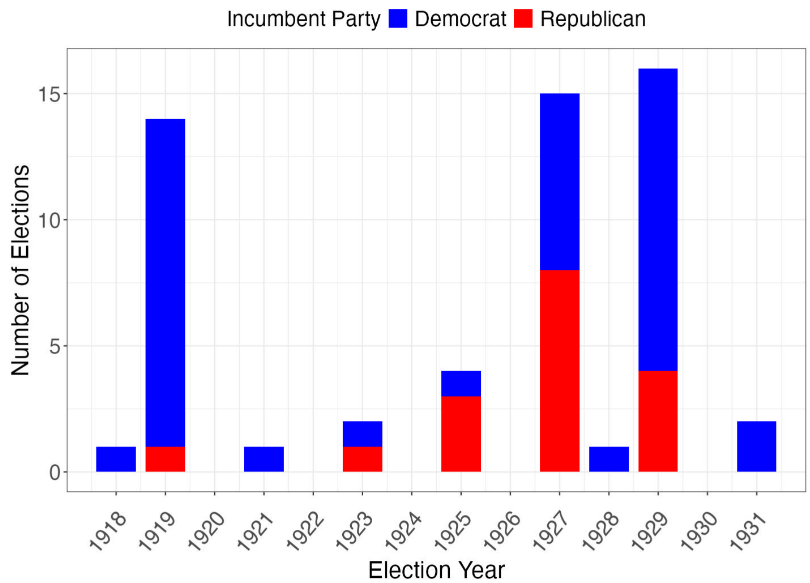
Panel (b) plots the centered stability index (blue line, right axis) together with the number of districts that experience any change in partisan composition in a given year (bars, left axis). The figure shows that while most years exhibit relatively little movement, major election years feature both 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.

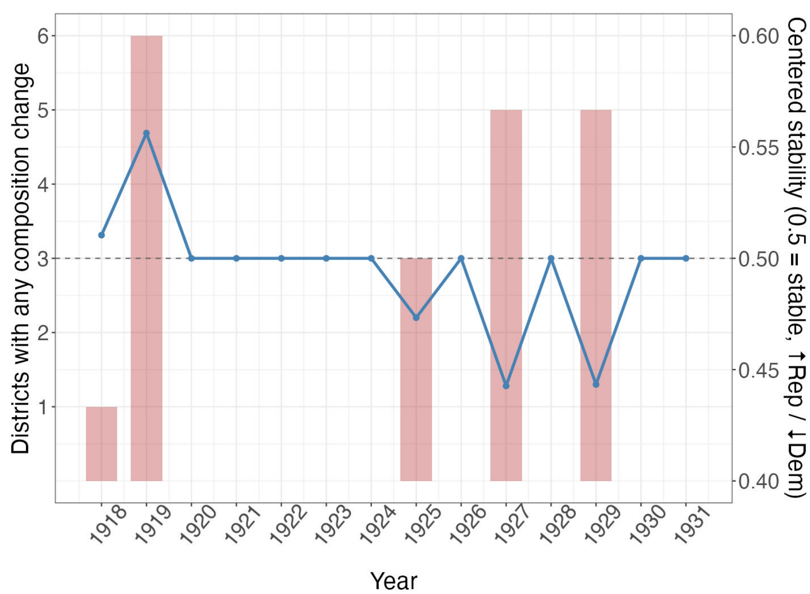
Next, we grouped MCDs by their judicial composition: all-Democrat (0% Republican judges), all-Republican (100% Republican judges), and mixed districts with an intermediate Republican share. For each group, we calculated the mean of each socio-demographic indicator and report these in Figure C1.2, with 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

(a) Judge Elections



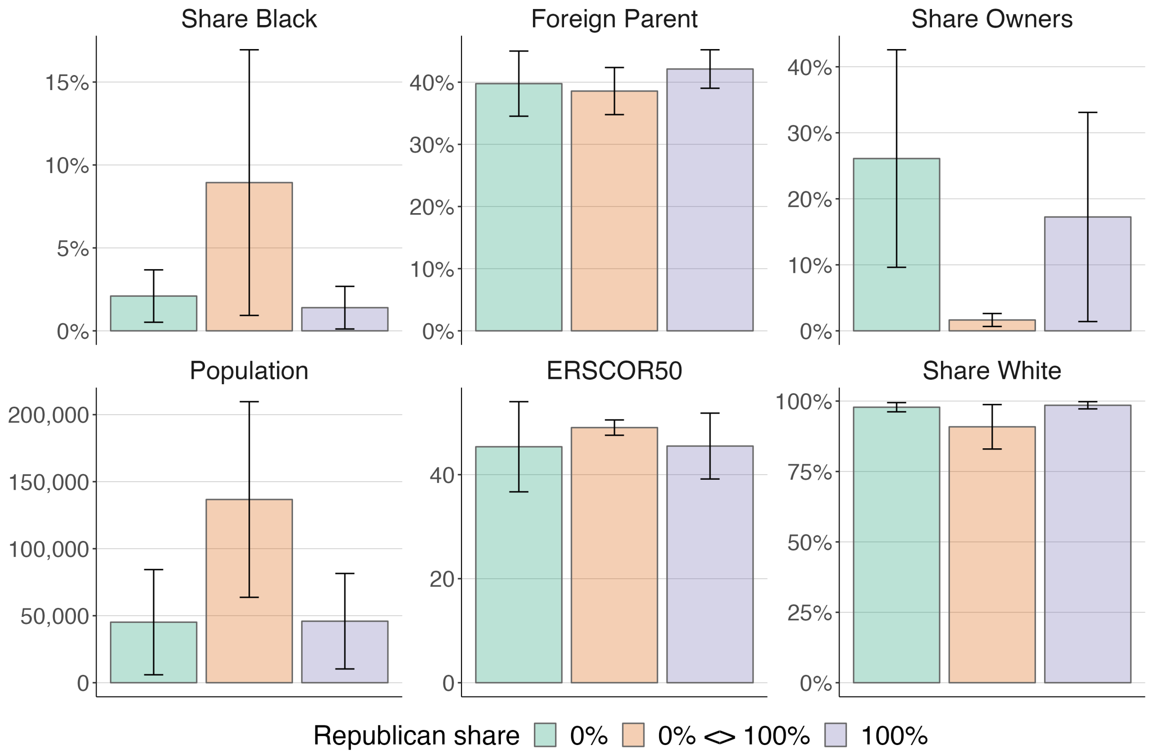
(b) District Stability and Composition Changes



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

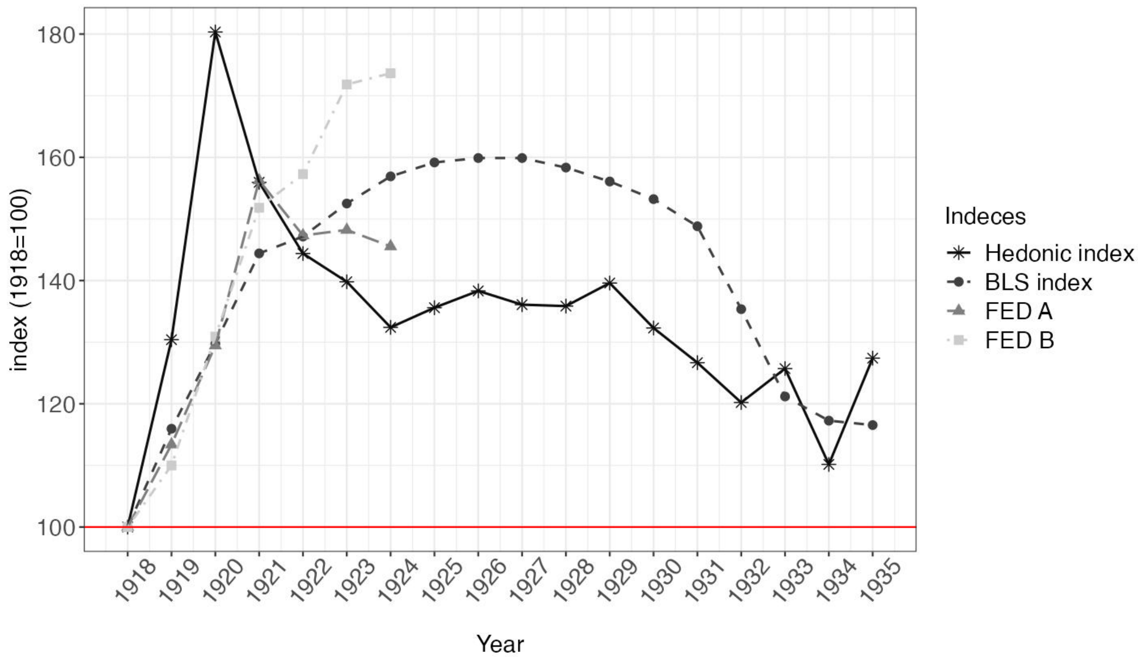
Our data on market rents are drawn from New York Times classified listings. A sample of advertisements was manually digitized, with inclusion based on a set of pre-specified criteria. Each listing had to report (i) an advertised rent, (ii) an exact street address, (iii) a measure of unit size (such as 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).

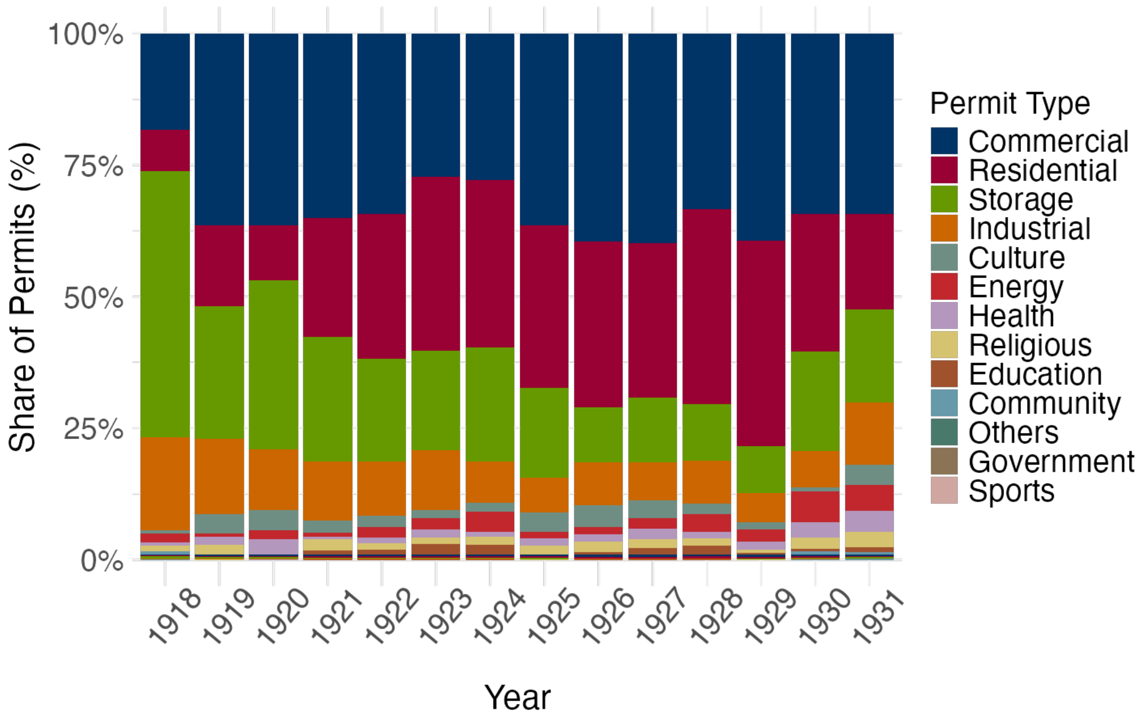
C3 Building Permits

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.

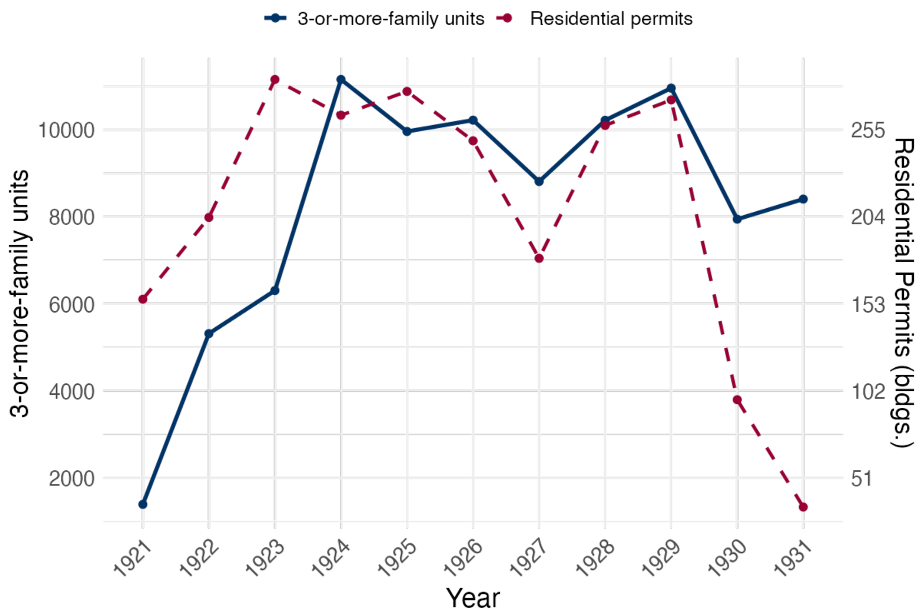
Figure C3.1: Composition of Building Permits



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 Placebo Test: Pre–Rent Control

Table D1.1: Effect at cut-off on rents before Rent Controls (Jan 1918–Mar 1920)

	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.022	0.038	-0.046	-0.001	0.002	0.003	-0.152	-0.018
	(0.117)	(0.133)	(0.162)	(0.101)	(0.204)	(0.158)	(0.226)	(0.118)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.617	0.469	0.235	0.938	0.722	0.834	0.417	1.668
Obs.	2081	1983	1983	1983	2081	1983	1983	1983
R2	0.152	0.438	0.532	0.413	0.153	0.417	0.461	0.409
CI^{l}_{rb}	-0.273	-0.255	-0.430	-0.281	-0.437	-0.318	-0.672	-0.311
CI^{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 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens and Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard errors in parenthesis have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

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	-0.017	-0.134	-0.029	0.059	-0.044	-0.162	-0.068
	(0.103)	(0.100)	(0.104)	(0.077)	(0.163)	(0.110)	(0.143)	(0.083)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	0.600	0.451	0.225	0.901	0.759	0.893	0.447	1.787
Obs.	2738	2624	2624	2624	2738	2624	2624	2624
R2	0.186	0.469	0.541	0.442	0.185	0.444	0.476	0.426
$\text{CI}^{l}{}_{\text{rb}}$	-0.211	-0.245	-0.293	-0.263	-0.301	-0.269	-0.479	-0.300
$\text{CI}^{u}{}_{\text{rb}}$	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 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens and Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

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

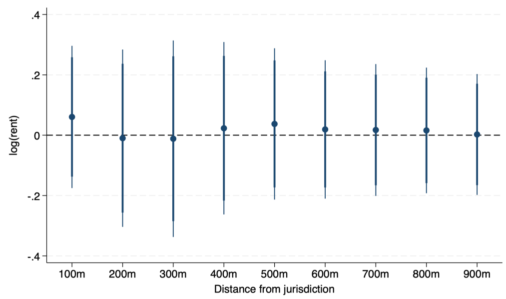
	linear				quadratic			
	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$	$\hat{b}$	$\hat{b}$	$\hat{b}/2$	$\hat{b}*2$
β_{rdd}	0.108***	0.066	0.010	0.097***	0.118**	0.085*	0.045	0.118***
	(0.032)	(0.036)	(0.055)	(0.026)	(0.040)	(0.040)	(0.047)	(0.029)
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	1.019	0.601	0.301	1.202	1.720	1.461	0.730	2.921
Obs.	12612	12192	12192	12192	12612	12192	12192	12192
R2	0.136	0.307	0.321	0.298	0.134	0.293	0.307	0.276
CI^{l}_{rb}	0.047	-0.018	-0.196	-0.007	0.038	-0.006	-0.146	0.034
CI^{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 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens and Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard errors in parenthesis have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals. ***, **, * indicate significance at the 1 per cent, 5 per cent and 10 per cent level respectively.

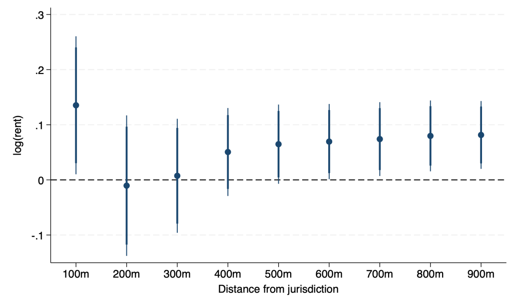
D3 RDD estimates for Alternative bandwidth choices

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

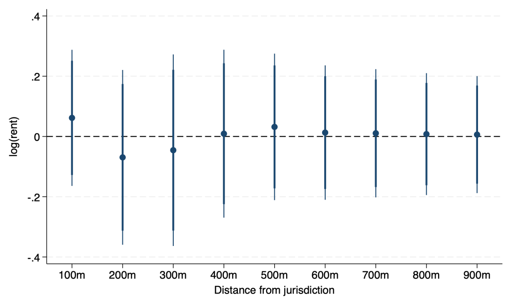
(a) Republican only MCD 1918-1920



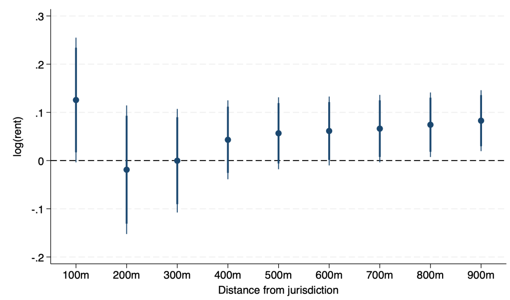
(b) Republican only MCD - 1921-1926



(c) Majority Republican MCD - 1918-1920



(d) Majority Republican MCD - 1921-1926



Note. Figure D3.1 shows RD estimates from estimating Equation 3 for different bandwidth choices using the full et 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 use the distance to the boundary between majority an non-majority Republican MCDs. Standard errors are clustered at the neighborhood level; vertical bars indicate 95% confidence intervals. We use a triangular kernel with a linear fit.

D4 Event Study - Rent Prices

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:

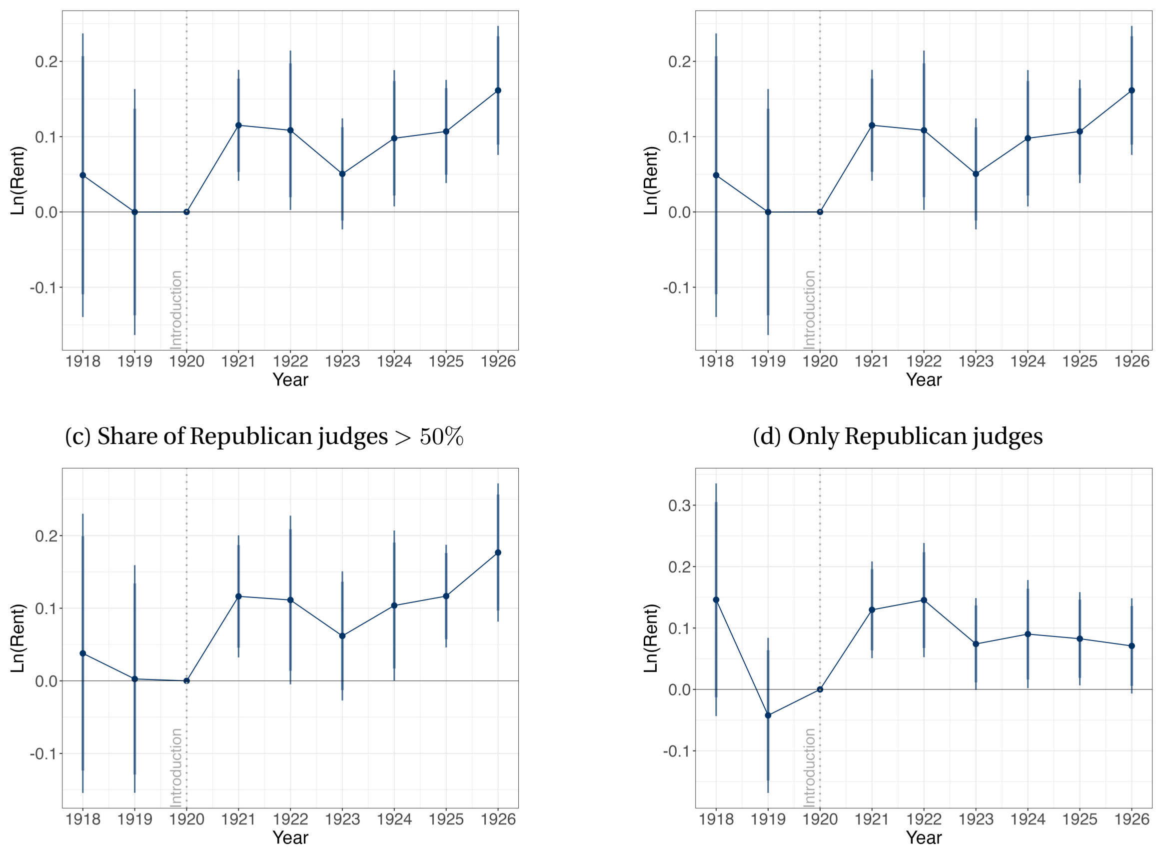
$$y_{i,m,t} = \sum_{\tau \neq 1920} \beta_\tau \cdot Exposure_{t,i(m)} + \mathbf{D}_{i,t} + \mathbf{X}_{i,t} + \gamma_t + \gamma_m + u_{i,m,t} \tag{D.1}$$

where again $y_{i,m,t}$ is the listed rent for observation i in MCD m in year t. The variable $Exposure_{t,i(m)}$ 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 using our rent data are shown in Figure D4.1. We again find a convincing effect of rent control on rental prices. The difference in market rents between MCDs that are controlled by 0% versus 100% Republican judges averages 10 percent, closely matching the results reported in Table 1. An additional Republican judge increases rental prices by about 3 percent. Given that there are, on average, two Republican judges in an MCD, this implies 6 percent higher rents in a typical mixed district. These results are corroborated using the binary treatments from the RD design in Panels (c) and (d). The average point estimates are 10.7 percent and 8.8 percent for the Republican-only and majority-Republican treatments, respectively, and there is no evidence of pre-trends in rents under either treatment.

Figure D4.1: Effect of Residential-only Investment Treatments



(a) Share of Republican judges

(b) Number of Republican judges

(c) Share of Republican judges $> 50\%$

(d) Only Republican judges

Note. Figure D4.1 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.

D5 Difference-in-Differences

In this appendix section, we report additional difference-in-differences results that complement the baseline analysis in Section 6.2. Instead of measuring judicial exposure by the absolute number of Republican judges in a municipal court district (MCD), we use the *share* of Republican judges as an alternative treatment variable. This specification captures the relative partisan composition of the court and provides a robustness check on whether our results are sensitive to the scale at which judicial exposure is measured.

Extensive margin. Table D5.1 presents the corresponding extensive-margin results, where the outcomes are the log number of residential-only permits, mixed-use residential permits, and total housing listings aggregated to the neighborhood (NTA) level. During the rent control period, neighborhoods exposed to a higher share of Republican judges experienced significantly more residential construction activity. Moving from a fully Democratic to a fully Republican court is associated with approximately a 54 percent increase in residential-only permits and a 23 percent increase in mixed-use residential permits relative to the pre-period. By contrast, the estimated effect on the total number of housing listings is small and statistically insignificant.

Table D5.1: Extensive Margins

	Res. only	Res. mixed	Listings
$Post_{20-28} \times \%Rep$	0.537***	0.230***	-0.056
	(0.150)	(0.082)	(0.080)
$Post_{29-31} \times \%Rep$	0.147	0.002	-0.156
	(0.207)	(0.116)	(0.121)
NTA FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	647	478	2,692
R^2	0.679	0.580	0.803

Note. Table D5.1 reports weighted difference-in-differences regressions of the log number of residential-only permits, mixed-use-only residential permits, and total housing listings at the neighborhood (NTA) level, estimated following Equation 4. Treatment is measured by the share of Republican judges in the corresponding municipal court district (MCD). NTAs overlapping multiple MCDs are matched using area shares, which serve as regression weights. All specifications include NTA and year fixed effects. Standard errors (in parentheses) are clustered at the NTA level. ***, **, * denote significance at the 1, 5, and 10 percent levels, respectively.

Intensive margin. Table D5.2 reports neighborhood-level DiD estimates for investment outcomes, using the log of investment per building and the log of total investment as dependent variables. During the rent control period (1920–1928), a higher share of Republican judges is associated with significantly higher residential investment. For residential-only permits, moving from a fully Democratic to a fully Republican court is associated with approximately a 28 percent increase in investment per building and an 85 percent increase in total residential investment. The corresponding estimates for mixed-use residential projects are positive but smaller in magnitude and less precisely estimated, consistent with their partial exposure to rent regulation.

In the post-control period (1929–1931), the estimated coefficients are substantially attenuated and statistically indistinguishable from zero across all outcomes and permit types. This mirrors the baseline results using the number of Republican judges and indicates that the relationship between judicial composition and residential investment intensity was specific to the period in which rent control was actively enforced.

Table D5.2: Effect on Investment

	Per Building		Total	
	Res. only	Res. mixed	Res. only	Res. mixed
$Post_{20-28} \times$ % Rep	0.284**	0.238	0.846***	0.491*
	(0.109)	(0.227)	(0.238)	(0.256)
$Post_{29-31} \times$ % Rep	-0.115	-0.294	0.074	-0.229
	(0.204)	(0.249)	(0.305)	(0.289)
NTA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	647	478	647	478
R^2	0.491	0.541	0.629	0.617

Note. Table D5.2 reports weighted difference-in-differences estimates of the effect of rent control on construction investment at the neighborhood (NTA) level, following Equation 4. Columns (1) and (2) use the log of average project cost per permitted building as the outcome for residential-only and mixed-use permits, respectively (intensive margin). Columns (3) and (4) report results for the log of total investment in residential-only and mixed-use construction (extensive margin). Treatment intensity is measured by the share of Republican judges in the corresponding municipal court district (MCD). NTAs overlapping multiple MCDs are matched using area shares, which serve as regression weights. All specifications include neighborhood (NTA) and year fixed effects. Standard errors (in parentheses) are clustered at the NTA level. ***, **, * denote significance at the 1, 5, and 10 percent levels, respectively.

D6 Tripple Difference-in-Differences

While the neighborhood-level DiD design in Section 6.2 captures how aggregate construction activity Responds to judicial composition, the model also delivers predictions at the level of individual projects. Because rent control applied exclusively to residential rents, variation in judicial enforcement should affect the expected returns - and hence the optimal scale - of residential projects, but not non-residential ones.

To isolate this mechanism, we estimate a permit-level triple-difference (DDD) specification that exploits three sources of variation: time variation across policy regimes, cross-sectional variation in judicial exposure, and sectoral variation in exposure to rent control. Relative to the baseline DiD, the DDD design differences out all neighborhood-year shocks common to construction activity and identifies how judicial enforcement differentially affects residential projects relative to non-residential ones.

We classify permits into two treatment groups: residential-only permits and mixed-use permits that include a residential component. Mixed-use projects are only partially exposed to rent control and therefore represent an intermediate treatment intensity. Non-residential permits serve as the comparison group, with alternative definitions used to assess robustness.

Formally, we estimate:

$$y_{i,m,t} = \Big(\theta_{20\text{–}28} \cdot Post_{20\text{–}28} + \theta_{29\text{–}31} \cdot Post_{29\text{–}31}\Big) \times \mathbb{1}\{Type_i = s\} \times Exposure_m + \gamma_{tm} + \gamma_{ts} + \gamma_{sm} + \mathbf{U}_i + \mathbf{D}_{i,t} + \mathbf{C}_i + \varepsilon_{i,m,t}, \quad \text{(D.2)}$$

where $y_{i,m,t}$ is log investment per building for permit i in neighborhood m and year t. Neighborhood-by-year fixed effects (γ_{tm}) absorb all local shocks to construction activity, including demand shifts, zoning changes, and land-use dynamics. Year-by-type fixed effects (γ_{ts}) allow investment trends to differ flexibly across permit types, while neighborhood-by-type fixed effects (γ_{sm}) absorb time-invariant spatial sorting of project types. The remaining variation therefore comes from differential post-period responses of investment to judicial exposure across permit types within the same neighborhood and year.

The specification further includes distance controls ($\mathbf{D}_{i,t}$), construction-material fixed effects ($\mathbf{C}_i$), and usage-mix controls ($\mathbf{U}_i$), which flexibly hold constant project location, construction technology, and intended building use at the permit level. As a result, the estimates isolate changes in project scale rather than shifts in project composition.[15]

The coefficients on the triple interaction terms capture differences in the sensitivity of project-level investment to rent control enforcement, rather than level effects on construction activity. In the context of the model, they measure how developers reoptimize project scale in response to changes in expected returns induced by judicial enforcement, holding other permit characteristics fixed.

[15] Distance controls include the permit's distance to the shoreline and to the nearest major park. Construction-material fixed effects consist of indicator variables for the primary structural materials specified in the permit—brick, stone, concrete, cement, iron, and steel—as well as an indicator for elevator installation, capturing differences in building technology and vertical scale. Usage-mix controls are a full set of indicator variables for the intended primary use of the structure, including residential, commercial, industrial, energy, infrastructure, cultural, health, religious, sports, government, community, storage/outbuildings, education, and other uses. Together, these controls condition on project location, construction technology, and intended use as recorded in the permit, ensuring that the estimated effects reflect changes in investment intensity within projects rather than changes in the composition of permitted construction.

Identification relies on the assumption that, absent rent control, residential and non-residential investment would have followed parallel trends across districts with different judicial compositions. Together with the neighborhood-level DiD results, the permit-level DDD estimates provide complementary evidence on both the extensive and intensive margins of investment predicted by the model.

Table D6.1: Investment per Building - Permits

	Any	Private	No Com.
θ_{20-28}	0.139***	0.135***	0.161***
	(0.052)	(0.049)	(0.048)
θ_{29-31}	-0.035	-0.003	-0.052
	(0.053)	(0.058)	(0.054)
Distance Controls	✓	✓	✓
Material FE	✓	✓	✓
Usage FE	✓	✓	✓
NTA × Type FE	✓	✓	✓
Year × Type FE	✓	✓	✓
Year × NTA FE	✓	✓	✓
Observations	7,098	6,314	5,242
R^2	0.567	0.574	0.642

Note. Table D6.1 reports permit-level triple-difference (DDD) regressions of log investment per building on judicial exposure to rent control, estimated following Equation D.2. Treatment is measured by the number of Republican judges in the corresponding municipal court district (MCD). Columns differ by the comparison group used: all non-residential permits (Any), private-sector non-residential permits (Private), and non-residential permits excluding commercial construction (No Commercial). Standard errors (in parentheses) are clustered at the neighborhood (NTA) level. ***, **, * denote significance at the 1, 5, and 10 percent levels, respectively.

D7 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 D7.1.

Table D7.1: 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***	0.041	-0.064	0.072	0.303*	-0.017	-0.082	0.059
	0.095	0.041	0.071	0.042	0.129	0.061	0.080	0.054
Controls	✗	✓	✓	✓	✗	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
NTA FE	✓	✓	✓	✓	✓	✓	✓	✓
BWS	582.966	441.717	220.858	883.434	1274.329	850.755	425.377	1701.509
Obs.	5216	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 gives RD estimates using a linear specification. In column (1)-(2) the sample had been restricted to a bandwidth of $\hat{b}$, determined by the Imbens and Kalyanaraman (2012) algorithm. Columns 5–8 are alternative RD specifications using half,$\hat{b}/2$, and double, $\hat{b}*2$, the optimal bandwidth. Columns 5–8 give RD estimates using a quadratic specification; controls include the distance to the coastal line and the nearest park, the total room, and a set of dummies indicating if the property was furnished, had water and electricity included, and a dummy if it was a flat or a house. All specifications include year and neighborhood (NTA) fixed effects; standard errors have been clustered at the neighborhood (NTA) level; we additionally report robust bias-corrected confidence intervals.

This exercise reveals that, 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.