State Preemption of Rent Control: Adoption, Diffusion, and Effects on Housing Supply

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September 2026

Working paper

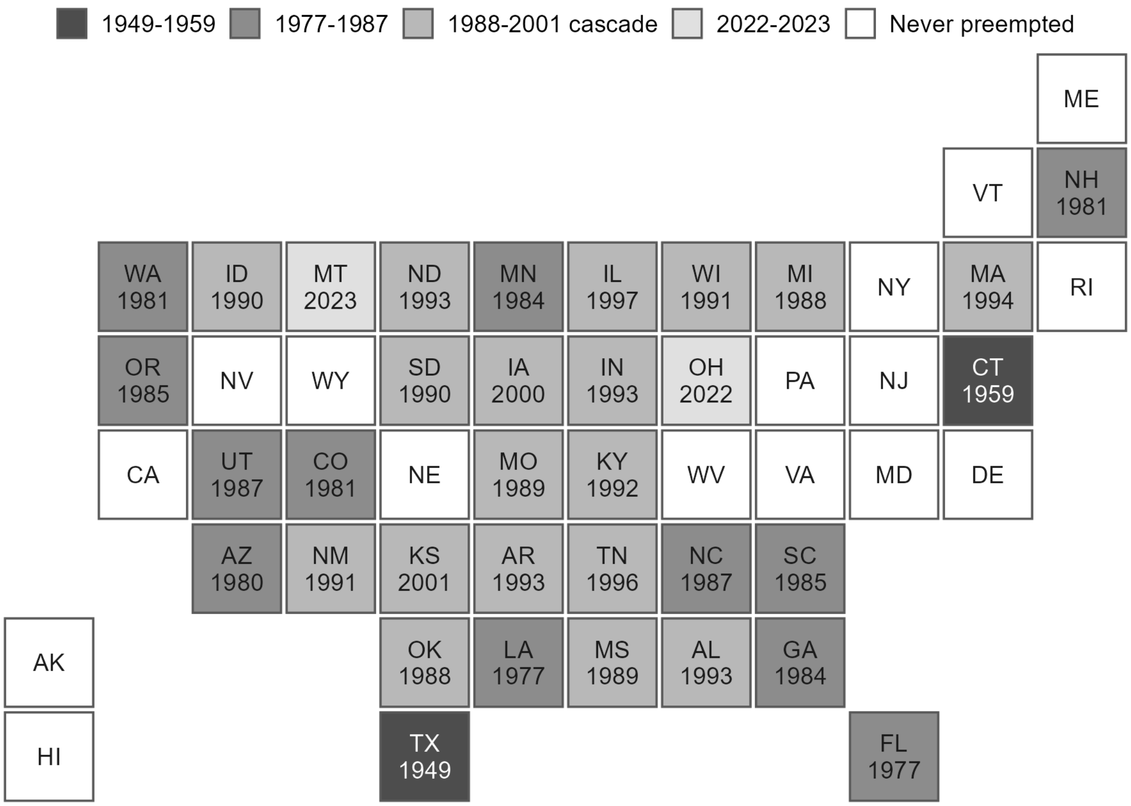
Abstract

I seek to answer why states have preempted local rent control, among other housing preemptions, and I find that adoption of rent control preemptions spreads through neighboring states rather than party affiliation or ideology, and that while rent control preemption has no detectable effect, other housing preemptions do have a detectable effect on the number of units permitted.

Chapter 3 of the author's dissertation, Second Generation Rent Control and New Construction, The Graduate Center, City University of New York, 2026. Page numbers below are the chapter's own.

1 Introduction

Thirty-four states have prohibited local rent control (Figure 1); these preemptions spread across the United States in waves (Figure 2). Rent control preemption laws could reasonably be expected to be partisan or ideologically driven efforts to protect or secure local landlords and developers from municipalities and to increase investment. Following Goodman and Hatch (2023a), I extend the window over which preemption adoption is estimated, and add outcome measures to examine the effects after passage. I find that states are more likely to adopt after nearby states have already preempted rent control. Party control and ideology have no statistically significant relationship with rent-control adoption that survives bootstrap inference, although the neighbor result depends on the risk-set rule, which states count as still able to adopt. Preemption is already used across policy areas, so rent control is one part of a broader conflict between state and local authority. State politics is often described as nationalized and partisan. A law that instead moves across borders points in a different direction. I find no detectable effect of preempting rent control on new housing construction, and a marginally significant effect on the real value of construction permitted per capita. The later preemptions of short-term rentals, inclusionary zoning and source of income look different: they arrive after 2011, the vast majority in states with both Republican legislatures and those that had already preempted rent control. These non-rent control preemptions, perhaps unexpectedly, are the one place I detect a clear housing construction response; I estimate that preemptions of this kind increase new housing construction. This paper combines an empirical analysis of policy adoption with an analysis of that policy's effects, which I am not aware of any prior state-level study doing for preemption. The empirical literature on law propagation is long-standing (Berry and Berry 1990; Mallinson 2021), and Fowler and Witt (2019), Flavin and Shufeldt (2020) and Goodman and Hatch (2023b) measure effects on preemption more broadly (other non-rent control preemption laws), but the literature on rent control preemption in particular remains thin beyond Goodman and Hatch (2023a).



Each square is one state, equally weighted: the hazard model treats every state as a single observation, so land area would be the wrong encoding. Onset years are session-law verified.

Figure 1: Rent-control preemption by state and adoption year.

Notes: Alaska and Hawaii appear on the map but not in the estimates because the share of neighboring states that have adopted is undefined for states without land neighbors (Berry and Berry 1990, 405).

2 Preemption spread

Rent control preemption became law in three ways in the United States: through state legislatures passing a preemption law (31), through judicial decision (2), and through ballot question (1). These are meaningfully different in this analysis because the incentives associated with these three routes might be markedly different. The base case in this paper is taken to be the state legislature enactment, as this is by far the most common case and because, in particular, the judicial route could be largely insulated, and more independent from typical economic and political forces. I relax these assumptions in the empirical work for the sake of robustness and sensitivity analysis.

To verify the dates of preemption adoption, I check each of the 34 against records of state law with original reference from National League of Cities (2019). Table 5 reports the legal citation. One contribution of this paper is this attributable list and corresponding dates of rent control preemption laws in the United States. There is some disagreement among sources: Caughey and Warshaw (2016) codes rent control preemption as beginning in 1978, but the code itself does not mention rents themselves. These disagreements among sources on interpretation of the applicability of yet-untested laws are an on-going limitation and consideration in this literature. An additional consideration is the appropriate timing: an act being passed does not always coincide with when it practically becomes enforceable, and so discrepancies of a year in either direction do occur in these data.

3 Data

The main data are compiled from several sources to make a panel-year dataset balanced across all 50 states from 1969 to 2023. There are several states which never adopt during this window, two (Texas and Connecticut) which adopted prior to this window. In estimating diffusion models, noncontiguous states of Hawaii and Alaska are excluded. The three main components are the preemption adoption dates, a suite of state-level economic and demographic variables, and variables

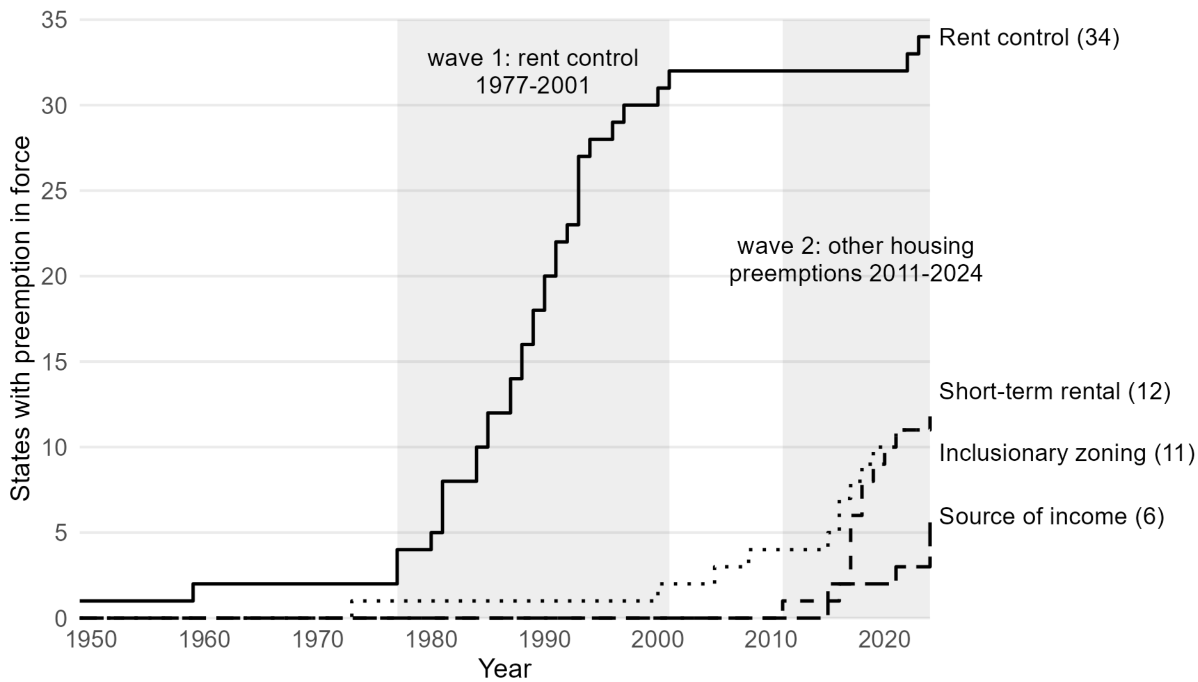


Figure 2: Cumulative state housing preemption, 1949 to 2024. Shaded bands mark the waves.

for state government political control and ideology. Construction outcomes come from the Census Building Permits Survey: total units and units in structures with five or more units per 1,000 residents, the multifamily share, and log real permit value per capita. Political control variables include legislative and gubernatorial party and trifecta control from Klarner's state partisan balance data (Klarner 2013), extended through 2023 from NCSL partisan-composition records. This panel uses similar state-level political data to Shor and McCarty (2011) from Berry (Berry et al. 1998), extending Goodman and Hatch's comparison twenty-four years earlier.

The Dillon's-Rule states, which effectively have rent control preemption through a different legal route, are not clean controls and are removed from the donor pool in a sensitivity rather than appearing as real never-adopters.

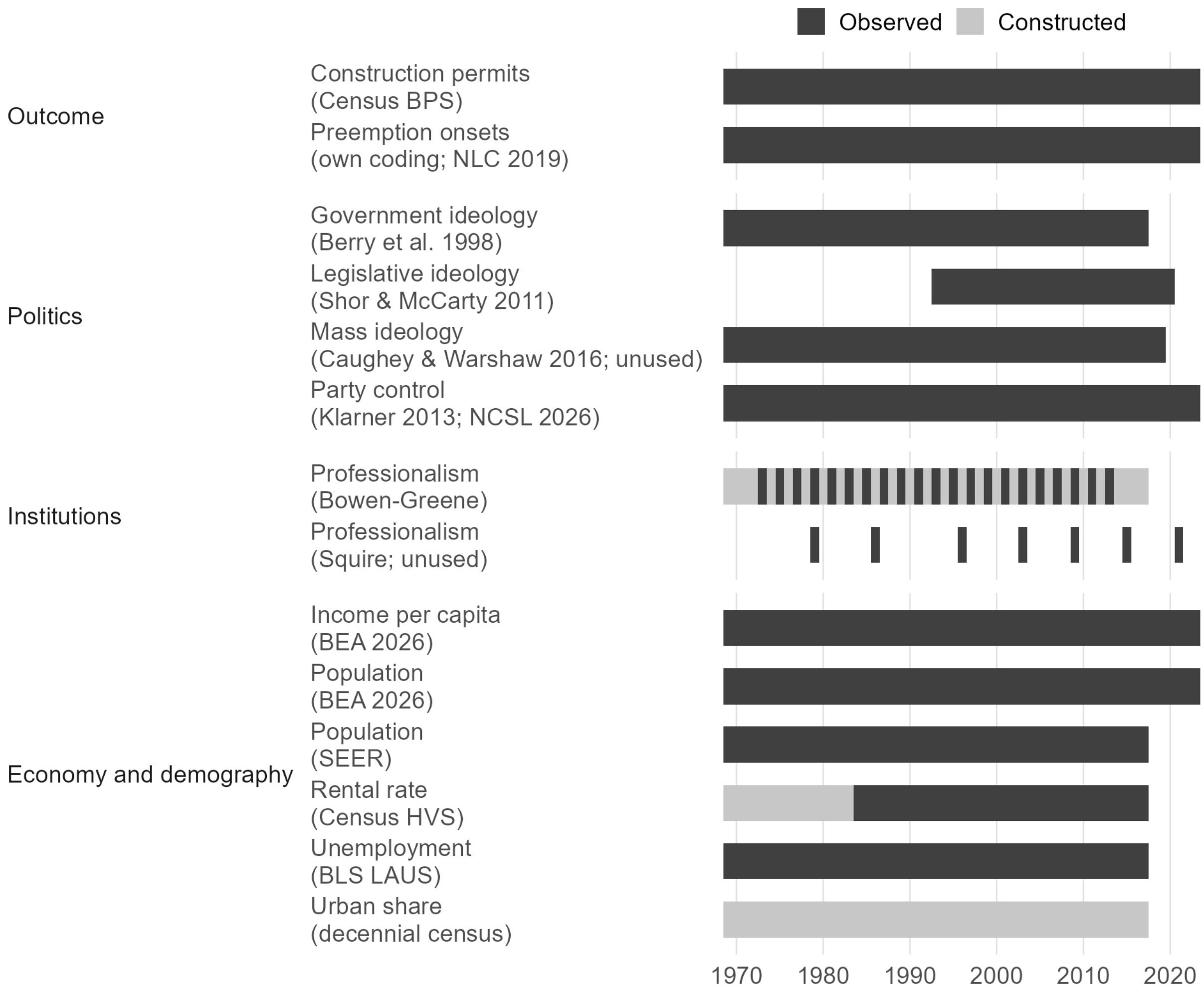


Figure 3: Data sources by year of coverage.

Notes: Each bar names a series and the source it comes from, with dark segments marking years the source supplies directly and light segments years constructed from adjacent observations. The figure is the chapter's statement of provenance: it shows which windows each specification can be estimated on, and why. Government ideology ends in 2017 and legislative ideology begins in 1993, which together determine the windows reported in the adoption models. Party control is spliced, Klarner through 2011 and the National Conference of State Legislatures tables thereafter; the two series marked unused were acquired but enter no specification.

4 Why states preempt

4.1 Empirical strategy

The adoption models I estimate are discrete-time hazard logits (Beck et al. 1998), estimated on the states that have not yet preempted, with standard errors clustered by state (Table 1). There are two samples for this study: one with twenty-nine events, based on statutory preemption, and another with thirty-two. The thirty-two are composed of the twenty-nine statutory cases and two non-legislative: New Hampshire in 1981, by established case law, and Massachusetts in 1994, by ballot, and the third, while legislative, is indeterminate: Louisiana in 1977 passed a law governing lease rights, but that law makes no mention of rent control. Connecticut adopted in 1959, and remains preempted throughout, so does not contribute an adoption nor is at risk of adoption.

The main argument to distinguish these two samples is that state legislatures which adopt preemption face a different set of pressures and influencing factors than do states which adopt preemption by some other means. For this reason I proceed with the statutory-only sample as my main specification rather than include the additional three. Sample exclusions do have some effect on the interpretation of these results, depending on which states are dropped, but there is a defensible reason for the sample I have chosen as headline in this paper. Goodman and Hatch's (2023a) predictors cannot support such a small number of adoptions as would be present in the case of using Shor and McCarty (2011), and thus replacement with Berry's ideological measures (Berry et al. 1998) is a practical concern of expanding the data window available to 1969, capturing the wave of rent control preemptions in the 1980s (Figure 3).

This extends to modeling diffusion with neighbor share: a state receives preemption pressure only from neighbors whose legislatures enacted rent control preemption. A court decision or a ballot question binds its own state without serving as an example to follow for a neighboring legislature. Were I modeling judicial preemption, or ballot initiative diffusion, I would similarly not expect neighboring state legislatures to exert measurable influence in judicial affairs or public ballot results.

This assumption matters most in the Northeast, where all of the three non-legislative preemptions reside.

4.2 Results

Democrats held the legislature in seventeen of the thirty-two adoptions of rent control preemption in the window this paper studies. Republicans, eleven (ten statutory), and split legislatures, four. The party counts of preemption adoption really indicate a rate of exposure to nearby states with preemption, and so cannot be taken as an indication of a causal relationship.

There is no specification where party is detectable as affecting the rate of adoption of rent control preemption laws at the five percent level. Proximity is detectable as an effect but only in cases where the structure of the estimator, or controls, does not absorb the effect of proximity in its construction (Table 1).

State preemption has occurred in two main waves (Figure 2): a rent control preemption wave from 1977 to 2001, and then a later wave 2011 onwards which included preemptions of three other varieties: short-term rentals, inclusionary zoning and source of income. These two waves can be characterized as distinct phenomena that correlate with two different sets of variables. The rent control preemption wave clustered by geographic proximity, and was also concentrated in states with lower incomes. What has been estimated in Goodman and Hatch (2023a), which combines these waves into an aggregate from 1993–2018, suggests a more clearly political or ideological tilt to preemption generally. I find in Table 1 column (1) the likelihood of spread in this first wave can largely be explained by a combination of the share of neighboring states having preempted, and by log of income per capita, although only the neighbor share holds its significance under each way I have modeled time (Table 2).

For sensitivity, I run forty-two single-state drops, one for each state in the headline sample, and the diffusion coefficient is always positive. None of these pushes the p value above five percent; the largest is .0002.

Wave 2 had more of an explicit political tilt; this wave is dominated by adoptions in states and years with Republican legislatures and in states that have already preempted rent control. These criteria alone describe 14 of the total 16 non-rent control housing preemption adoption events. In Table 1 column (2) I estimate this specification (Kreitzer and Boehmke 2016), and find a Republican legislature variable that suggests a positive result, one that clears the five percent threshold under randomization inference (p = .019) but not with the cluster-robust test (p = .055). The randomization reallocates the states' entire 2011–2023 party series among them at random, holding adoption dates, risk sets and every other covariate fixed; then this refit of the model compares the observed coefficient with 2,000 such draws, two-sided (restricting the reallocation to states in the same Census region gives .020). The meaningful variation that this model is estimated on is coming from 16 adoptions from 2011 to 2023, and thus future estimations of this model with more available data may improve these estimates.

While the neighbor term shows that prior neighboring adoption predicts a state's own, it cannot discount a version of history where rent control preemption is naturally correlated with time (Table 2). The estimate survives with every flexible form of the time control and a fully nonparametric baseline, though none of these absorbs a regional shock.

Table 3 places Goodman and Hatch's published estimates beside a replication of their model and design and with this paper's adoption dates (although both are from the same source). Using the same 1993 to 2018 window, their legislative-ideology coefficient of 0.061 becomes 0.105, with a cluster-robust p of .056 and a wild-bootstrap p of .063 (MacKinnon and Webb 2017). As far as the other covariates, ten of the nineteen coefficient signs in their table are the same with the replication that I estimate. I was not able to reproduce the 759 state-years from the sources they cite (Local Solutions Support Center 2026); the replication holds 364 (I can infer from this that the dates of adoption I have recorded are earlier than those that they use, but I was unable to find all of the exact points of disagreement). The window itself cannot be used to reliably estimate the

Table 1: Estimating statutory adoption of state housing-related preemptions

	(1) Wave 1: rent control	(2) Wave 2: other housing preemptions
Share of neighboring states preempting	3.6564** (0.7137)	-2.0803 (3.4877)
CR2 p-value; odds ratio per SD	0.000; 2.67	0.561; 0.76
Republican legislature	0.3413 (0.3949)	2.0135+ (0.9664)
Prior rent-control preemption	–	0.9600 (0.7112)
Log population density	0.1973 (0.1532)	–
Log income per capita	-3.5430* (1.3615)	–
Adoptions under Republican legislature	10 of 29	15 of 16
Adoptions by legislature and prior rent-control preemption: Republican with; Republican without; other with; other without	–	14; 1; 0; 1
Randomization p for party, two-sided: all states; within region	0.580; 0.496	0.019; 0.020
Randomization p for neighbor share, graph permutation, two-sided centered on the null median; null center	0.006; +0.44	–
Time controls	Linear trend	Linear trend; policy FE
Window	1969–2023	2011–2023
Inference	State cluster, CR2	State cluster, CR2
N; states; adoptions	1,393; 42; 29	1,239; 43; 16

Notes: (1) Discrete-time hazard logit measuring the effect on the likelihood of a state that has not yet adopted passing rent control preemption. (2) The same model for inclusionary zoning, short-term rental, and source-of-income preemption, with each state only at risk for each policy from that policy's first US adoption, and policy fixed effects; State-clustered standard errors in parentheses; stars from the CR2 test. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 2: Sensitivity analysis of the wave 1 diffusion estimate to the time control.

Time control	Share of neighboring states preempting	Log income per capita	Republican legislature
Linear trend (Table 1)	3.6564** (0.7137)	-3.5430* (1.3615)	0.3413 (0.3949)
Quadratic trend	2.6634** (0.7560)	-3.8144* (1.5069)	0.4508 (0.3995)
Cubic trend	3.0922** (0.8294)	-2.8786 (1.6163)	0.8176+ (0.4229)
Natural spline, 4 df	3.0862** (0.8406)	-2.9112 (1.6181)	0.8049+ (0.4074)
Six period dummies	2.6600** (0.8197)	-2.5385 (1.4616)	0.5743 (0.4344)
Nonparametric baseline (Cox; jackknife SE)	2.8643** (1.0585)	-2.9838 (1.9352)	0.8651 (0.5170)
Randomization p, linear trend, two-sided centered on the null median; null center	0.006; +0.44	–	–
Randomization p, nonparametric baseline, two-sided centered on the null median; null center	0.005; -0.09	–	–
N; states; adoptions	1,393; 42; 29	1,393; 42; 29	1,393; 42; 29

Notes: Each row estimates the model in Table 1 column (1) with the time control named in the row, and the statutory stock counts only legislative preemptions as neighbor pressure. Log income and Republican legislature are from the statutory-stock fit in the same row. The p-values are from randomizations of each state's neighbors while keeping each state's number of neighbors fixed, two-sided against the permutation null centered on its median; the null center is the average coefficient under random geography. Standard errors in parentheses: state-clustered CR1 on the logit rows, delete-one-state jackknife on the Cox row. Stars from the CR2 Satterthwaite test on the logit rows and from the jackknife on the Cox row: ** $p < .01$, * $p < .05$, + $p < .10$.

Table 3: Goodman and Hatch (2023a) replication and window sensitivity.

	Goodman and Hatch (2023a)	Replication	Replication, 1993–2010	Replication, 2005–2018
Avg. legislative chamber ideology	0.0607** (0.0220)	0.1047+ (0.0498)	0.2540* (0.0823)	-0.1570 (0.1514)
Legislative professionalism	-0.3220* (0.1374)	0.5196 (0.3368)	-2.2163 (1.2983)	-2.0975 (2.9545)
Folded Ranney index	-0.1005 (0.1123)	-0.0087 (0.1830)	0.1699 (0.4846)	7.3261* (2.9664)
Electoral competition	0.0001 (0.0011)	0.0031 (0.0038)	0.0104 (0.0069)	-0.0278* (0.0100)
Rental rate	-0.9182* (0.4339)	-0.2243 (1.4635)	-3.5316+ (1.5518)	1.0503 (4.5588)
Emp. single-family construction p.c.	-13.2225 (9.8335)	21.2239 (15.5459)	40.6918* (17.0149)	229.8817 (136.9684)
Emp. real estate p.c.	-0.8590 (38.1683)	-28.2093 (73.2277)	-134.6159 (177.5984)	48.8357 (121.1955)
House price index (FHFA)	-0.0002 (0.0001)	-0.0003+ (0.0001)	-0.0002 (0.0002)	-0.0021 (0.0013)
Housing/comm. dev. expend. p.c.	-0.1267 (0.4353)	26.5292 (20.8485)	2.9167 (27.2741)	-4.6585 (77.4050)
Population (1000s)	0.0000 (0.0000)	-0.0001** (0.0000)	0.0000 (0.0001)	-0.0003* (0.0001)
Personal income p.c.	0.0017 (0.0041)	0.0182 (0.0103)	0.0368 (0.0434)	0.0330 (0.0288)
Population density	0.0006 (0.0010)	0.0071** (0.0020)	-0.0018 (0.0191)	0.0010 (0.0078)
Percent urban	0.1993 (0.1788)	-9.4730** (2.0254)	-14.5146 (8.4074)	18.8636 (12.3651)
Percent 65 and older	-0.0625 (2.2241)	13.0025** (4.2537)	-20.9070 (16.9091)	14.0326 (49.7313)
Percent 19 and younger	-2.8583 (2.1038)	3.3765 (3.2939)	20.9112 (12.8931)	24.4698 (65.3523)
Percent with BA or higher	0.5344 (0.4988)	-0.6498 (1.8608)	-0.0345 (3.1716)	1.3454 (5.7331)
Ethnic fractionalization	1.7805* (0.8254)	10.5322** (2.5826)	13.1371 (8.4802)	70.1386* (26.7473)
Share of neighbors preempting	0.0416 (0.1148)	-0.5958* (0.2414)	-1.3209** (0.3473)	–
Number of previous preemptions	-0.0492* (0.0189)	-0.2216** (0.0624)	-0.5387** (0.0771)	-0.4633** (0.1152)
Time controls / fixed effects	State + year FE	State + year FE	State + year FE	State + year FE
Dependent variable	Any of four preemptions	Any of four preemptions	Any of four preemptions	Any of four preemptions
Window	1993–2018	1993–2018	1993–2010	2005–2018
Event rule	Legislative only	Legislative only	Legislative only	Legislative only
Sample rule	Ever-adopters	Ever-adopters	Ever-adopters	Ever-adopters
N; states; events	759; –; –	364; 14; 21	162; 9; 9	112; 8; 12
Ideology p, cluster-robust	< 0.01	0.056	0.015	0.334
Ideology p, wild cluster bootstrap	–	0.063	0.174	0.452
Diffusion p, cluster-robust	0.718	0.028	0.005	–
Diffusion p, wild cluster bootstrap	–	0.096	0.040	–

Notes: Column 1 is a reprint of table 2 in Goodman and Hatch (2023a). Their table prints stars rather than p-values, so the two p-values shown in that column are derived, the ideology p from its stars and the diffusion p from the coefficient and the standard error that they report. Column 2 replicates the method data source and window exactly but differs on the number of adoptions in that window. Column 3 and 4 are replications with only the years changed 1993-2010 for column 3 and 2005-2018 for column 4. A dash takes the place of missing information. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

bulk of the first wave, because the Shor and McCarty ideology series (Shor and McCarty 2011) begins in 1993 and 23 of the 34 rent-control preemptions had already been adopted. Split into 1993–2010 and 2005–2018, the ideology coefficient runs from 0.254 in the early years (cluster p .015, bootstrap .174) to -0.157 in the later ones (p .33), on nine and twelve adoptions. Using this method of estimation keeps only the states that adopt at some point within the data. The neighbor share (diffusion) in replication has a coefficient of -0.596, where theirs is positive. The sign is sensitive because of the risk-set dependence noted in the introduction, and because this window looks primarily at the second wave, which is less geographically determined than the first. The fixed-effects design identifies from within-state movement among the states which adopt within the window. By contrast, the hazard model in Table 1 identifies from the contrast between adopters and never-adopters, which is one of the bases for the modeling differences in the broader policy adoption literature.

5 Effects on construction

Preemption is presumably implemented as a reassurance to local landlords and developers and subsequently increases the amount of construction, particularly multifamily housing. I test this with the Callaway-Sant'Anna (2021) estimator, which makes use of never-treated controls (Figure 4).

Table 4 reports that for rent control preemption, total units permitted rise by 0.79 per 1,000 residents against never-treated states, with a standard error of 0.68, and shrink to 0.58 against not-yet-treated states. Neither of these is distinguishable from zero and the multifamily share does not move under either comparison group. The real value permitted per capita rises by 0.19 log points compared to the never-treated states, which is marginally significant, and then by 0.16 for the not-yet-treated states. In the panel of the other (non-rent control) housing preemptions, things look quite different: the total units rise by about 1.0 per 1,000 residents, and the multifamily units rise by 0.33 and both are significant at five percent under either comparison control group. Twelve states supply the treatment and they adopt from 2011 to 2021. Eleven of the twelve that adopt other housing preemptions

Table 4: Overall construction effects under two comparison groups. The two approaches point the same way for every outcome.

	Never-treated	Not-yet-treated
A. Rent-control preemption		
Total units permitted per 1,000	0.7898 (0.6766)	0.5822 (0.6816)
Multifamily units permitted per 1,000	0.1209 (0.3249)	0.0649 (0.3106)
Multifamily share of units permitted	-0.0258 (0.0292)	-0.0235 (0.0268)
Log real value permitted per capita	0.1926+ (0.1085)	0.1561 (0.1020)
Comparison groups	34 treated; 14 never-treated; 2,640 state-years	
B. Other housing preemptions		
Total units permitted per 1,000	1.0245* (0.4272)	0.9925* (0.4230)
Multifamily units permitted per 1,000	0.3349* (0.1644)	0.3224* (0.1609)
Multifamily share of units permitted	-0.0069 (0.0157)	-0.0074 (0.0154)
Log real value permitted per capita	0.1695* (0.0800)	0.1627* (0.0765)
Comparison groups	12 treated; 34 never-treated; 2,530 state-years	

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

had already preempted rent control. When I restrict the controls to the twelve states without either type of preemption, it leaves total units at 1.07, and takes multifamily below the threshold for significance. Ending the outcome window in 2019, to test for sensitivity to pandemic-era effects, leaves total units at 0.80 with multifamily again short of significance.

The total-units interval runs from -0.5 to +2.1 units per 1,000 residents, so I can rule out substantial declines, but not large increases, which limits the strength of the claims that can be made about new construction's response to preemption adoption.

Figure 5 plots Panel B's clean-control estimate year by year in an event study diagram. The nine pre-adoption terms center on zero, and the effect builds steadily from the adoption year to about two units per 1,000 residents by the eighth, although only four of the twelve treated states reach that full time horizon.

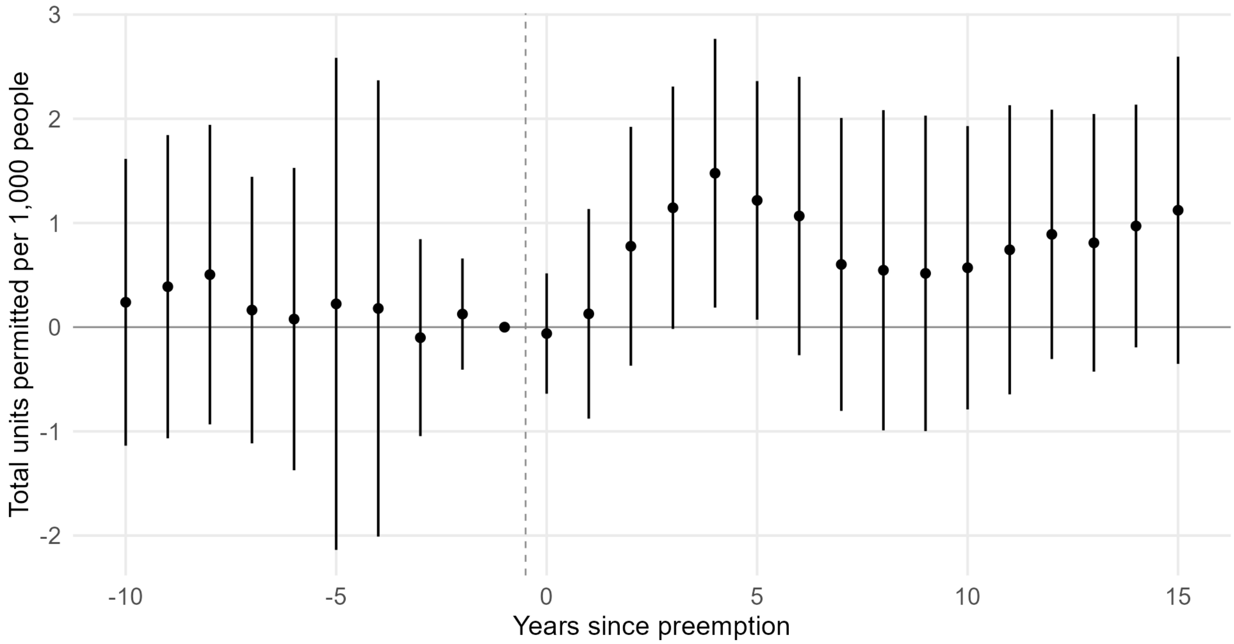


Figure 4: Event study for total units permitted per 1,000 residents.
Notes: Year-by-year treatment effects relative to the final pre-adoption year, which is zero by construction, with never-treated states as the comparison and 95 percent pointwise intervals.

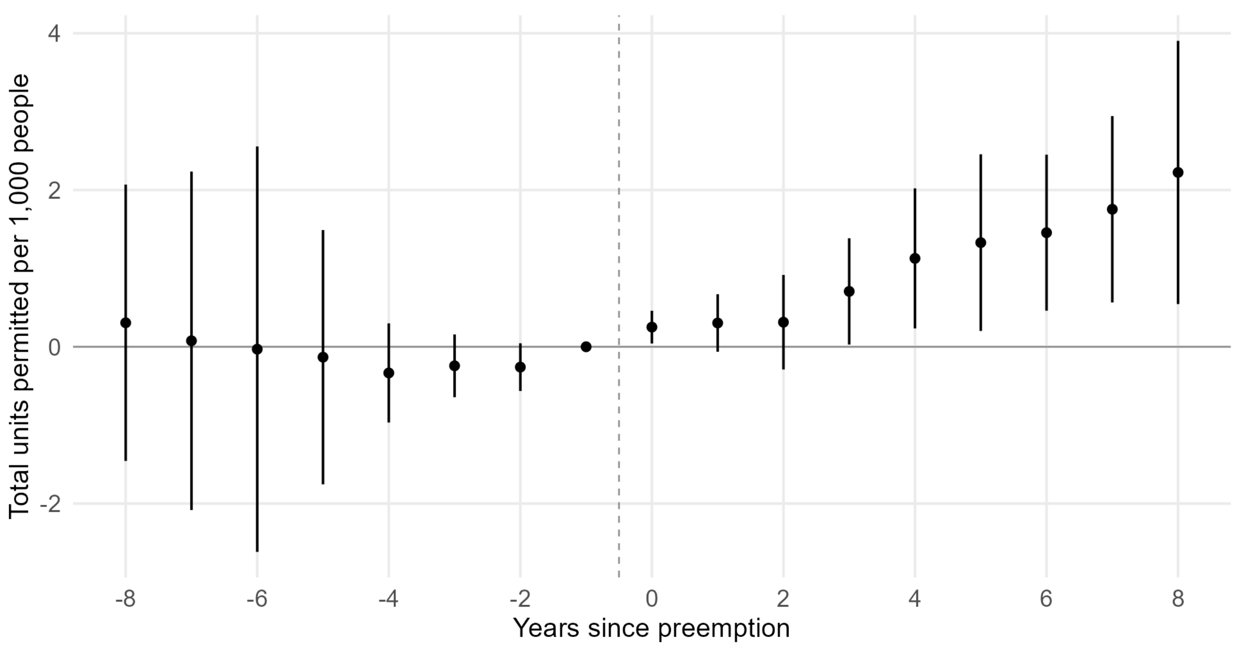


Figure 5: Inclusionary Zoning, Source of Income and Short-Term Rental Preemptions on New Housing Units. Event study figure using the estimation in Table 4 using all clean on both forms of preemption as controls.

6 Conclusion

States with neighbors with preemption laws are more likely to adopt rent control preemption. Contrary to popular conception, I do not find that ideology explains which states adopt rent control preemption in any specification I estimate that survives bootstrap inference, nor does party control. This is in contrast with the public perception of rent control and its opposition as largely an ideological and political struggle in the United States context. The non-rent-control preemptions that came later, and are still on-going, arrive more frequently under Republican legislatures and that association clears conventional thresholds under randomization inference. Given that rent control might reduce lifetime earnings of rental properties, it might be intuitive that preemption of rent control, which protects landlords and developers from this risk, would increase construction. I find no detectable effect of rent control preemption on new housing construction after implementation, nor a shift toward multifamily housing; the one exception is a marginally significant increase in the real value of construction permitted per capita, one marginal result among four outcomes.

One interpretation of this evidence is that rent control preemption represents position-taking or formalized commitment, while not having a credible commitment device. Preemption binds localities rather than the legislature that enacted it: Oregon preempted local rent control in 1985 and then imposed statewide rent control through SB 608 in 2019 (Chapter 1). Preemption therefore has little effect on the incentives of participants in the housing market. Alternatively, the laws may be redundant, adopted where there was little genuine risk of rent control in the first place.

A Appendix: estimation detail

The appendix gives the session-law citation and adoption date for each verified onset.

Table 5: Session-law citations.

State	Onset	Session law
Rent-control preemption		

Texas	1949	Tex. Loc. Govt Code 214.902, orig. V.A.C.S. art. 1269l-1 s.1a (Acts 1949 51st Leg ch.472, HB 808)
Connecticut	1959	Old Colony Gardens v. Stamford, 147 Conn. 60 (Dec. 8, 1959)
Florida	1977	Fla. Stat. 166.043 and 125.0103 (Ch. 77-50, CS/SB 403, filed 5/21/1977; renumbered 1979 ch.79-400)
Louisiana	1977	La. R.S. 9:3258 (Acts 1977 No.655 s.1, eff. 7/20/1977); indeterminate
Arizona	1980	A.R.S. 33-1329 (Laws 1980 ch.235 s.2, HB 2061, appr. 4/23/1980)
Colorado	1981	C.R.S. 38-12-301 (SL 1981 ch.437 p.1818 s.1, HB 1604, law without signature 6/23/1981)
New Hampshire	1981	Girard v. Town of Allenstown, 121 N.H. 268 (Apr. 3, 1981)
Washington	1981	RCW 35.21.830 (Laws 1981 ch.75 s.1, SHB 264); counties RCW 36.01.130 s.2
Georgia	1984	O.C.G.A. 44-7-19 (Ga. L. 1984 No.1226 / HB 594, p.1079, appr. 3/29/1984)
Minnesota	1984	Minn. Stat. 471.9996 (Laws 1984 ch.551 s.1, SF 1683, appr. 4/25/1984)
Oregon	1985	ORS 91.225 (Or. L. 1985 ch.335 s.2)
South Carolina	1985	S.C. Code 27-39-60 (1985 Act No.184 s.1, R282 H.2545, eff. 6/21/1985)
North Carolina	1987	N.C.G.S. 42-14.1 (S.L. 1987-458, HB 1025, ratified 6/23/1987)
Utah	1987	Utah Code 57-20-1 (Laws of Utah 1987 ch.186; am. 2006 ch.365)
Michigan	1988	MCL 123.411 (PA 226 of 1988, Imd. Eff. 7/5/1988)
Oklahoma	1988	11 O.S. 14-101.1 (Laws 1988 ch.38 s.1, HB 1647, emerg. eff. 3/21/1988)
Mississippi	1989	Miss. Code 21-17-5(2)(h) (Laws 1989 ch.526 s.1; continued Laws 1990 ch.418 s.1)
Missouri	1989	Mo. Rev. Stat. 441.043 (L. 1989 H.B. 602 s.2, eff. 8/28/1989)
Idaho	1990	Idaho Code 55-306, formerly 55-307(2) (1990 ch.185 s.1 p.414, HB 555; recodified 2025 ch.65)
South Dakota	1990	SDCL 6-1-13 (SL 1990 ch.51 s.2)
New Mexico	1991	N.M. Stat. 47-8A-1 (Laws 1991 ch.23 s.1)
Wisconsin	1991	Wis. Stat. 66.1015, ORIGINALLY 66.375 (1991 Act 39 s.1689j; renumbered 1999 Act 150 s.377)
Kentucky	1992	KRS 65.875 (Acts 1992 ch.242)
Alabama	1993	Ala. Code 11-80-8.1 (Acts 1993 No.93-421, S.208, appr. 5/12/1993)

Arkansas	1993	Ark. Code 14-54-1409 (municipalities) AND 14-16-601 (counties) (Act 545 of 1993, SB 208, appr. 3/16/1993)
Indiana	1993	Ind. Code 32-31-1-20, orig. IC 32-7-1-19 (P.L. 234-1993 s.1, S.501, appr. 4/20/1993)
North Dakota	1993	N.D.C.C. 47-16-02.1 (S.L. 1993 ch.451, SB 2281)
Massachusetts	1994	Acts 1994 ch.368 (Question 9, approved by the people 11/8/1994; eff. 1/1/1995)
Tennessee	1996	Tenn. Code 66-35-102 (Acts 1996 ch.623 s.1, Pub.Ch.623, SB 2683, eff. 7/1/1996)
Illinois	1997	50 ILCS 825 Rent Control Preemption Act (P.A. 90-313, SB 531, appr. and eff. 8/1/1997)
Iowa	2000	Iowa Code 364.3(9) (2000 Acts ch.1083 s.2, SF 428, appr. 4/13/2000)
Kansas	2001	K.S.A. 12-16,120 (L. 2001 ch.134 s.2, eff. 7/1/2001)
Ohio	2022	Ohio Rev. Code 5321.19 (H.B. 430, 134th G.A., eff. 9/23/2022)
Montana	2023	MCA 7-1-111(26) (2023 Mont. Laws, SB 105, 68th Leg. s.1)
Inclusionary-zoning preemption		
Virginia	1973	Judicial: Bd. of Supervisors of Fairfax County v. DeGroff Enterprises, 214 Va. 235, 198 S.E.2d 600 (Va. Aug. 30, 1973)
Colorado	2000	Judicial: Town of Telluride v. Lot Thirty-Four Venture, 3 P.3d 30 (Colo. June 5, 2000)
Texas	2005	Tex. Loc. Govt Code 214.904, now 214.905 (Acts 2005, 79th Leg. R.S., ch. 1103, HB 2266, eff. 9/1/2005); narrow
Idaho	2008	Judicial: Mountain Central Bd. of Realtors v. City of McCall, No. CV 2006-490-C (Idaho 4th Jud. Dist., Feb. 19, 2008), applying former Idaho Code 55-307(2); unpublished, secondary source only
Arizona	2015	A.R.S. 9-461.16 and 11-819 (Laws 2015, ch. 140, SB 1072, appr. 4/1/2015)
Kansas	2016	K.S.A. 12-16,120(d) (L. 2016, ch. 104, SB 366)
Tennessee	2016	T.C.A. 66-35-102(b) (Pub. Ch. 822 of 2016, SB 1636, passed 4/7/2016; broadened by Pub. Ch. 685 of 2018)
Indiana	2017	IC 32-31-1-20 as amended (P.L. 266-2017, SEA 558, appr. 5/2/2017)
Wisconsin	2018	Wis. Stat. 66.1015(3) (2017 Wis. Act 243, AB 770, enacted 4/3/2018)
Florida	2019	Fla. Stat. 125.01055(2) and 166.04151(2) (Ch. 2019-165, Laws of Fla., HB 7103); narrow
Montana	2021	MCA 76-2-114 (HB 259, 67th Leg. 2021)

Short-term-rental preemption		
Florida	2011	Fla. Stat. 509.032(7)(b) (Ch. 2011-119, Laws of Fla., HB 883, appr. 6/2/2011)
Arizona	2016	A.R.S. 9-500.38 and 11-269.15, now 9-500.39 and 11-269.17 (Laws 2016, ch. 208, SB 1350, appr. 5/12/2016)
Idaho	2017	Idaho Code 67-6539 (2017 Sess. Laws ch. 239, HB 216, eff. 1/1/2018)
New Hampshire	2017	RSA 48-A:2 as amended (2017 Laws ch. 249, HB 654, appr. 7/18/2017); narrow
Utah	2017	Utah Code 10-8-85.4 and 17-50-338 (2017 Laws ch. 335, HB 253, signed 3/24/2017); narrow
Wisconsin	2017	Wis. Stat. 66.1014 (2017 Wis. Act 59, AB 64, enacted 9/21/2017)
Indiana	2018	IC 36-1-24 (P.L. 73-2018, HEA 1035, signed 3/14/2018)
Tennessee	2018	T.C.A. 13-7-601 to -606 (2018 Pub. Acts ch. 972, HB 1020, appr. 5/17/2018); narrow
Nebraska	2019	Neb. Rev. Stat. 18-1758 (Laws 2019, LB 57, appr. 3/7/2019)
Iowa	2020	Iowa Code 331.301(18) and 414.1(1)(e) (2020 Iowa Acts ch. 1118, HF 2641, appr. 6/29/2020)
Nevada	2021	AB 363 (Stats. 2021, ch. 388, appr. 6/4/2021)
Virginia	2024	Va. Code 15.2-983(D) (Acts 2024, ch. 700, HB 1461, appr. 4/8/2024; and ch. 792, SB 544); narrow
Source-of-income preemption		
Indiana	2015	IC 36-1-3-8.5 (P.L. 101-2015, HEA 1300, sec. 3)
Texas	2015	Tex. Loc. Govt Code 250.007 (Acts 2015, 84th Leg. R.S., ch. 1140, SB 267, eff. 9/1/2015)
Iowa	2021	Iowa Code 331.304(13) and 364.3(13), now (16) (2021 Iowa Acts ch. 49, SF 252)
Idaho	2024	Idaho Code 55-307(2) as amended (2024 Sess. Laws ch. 257, HB 545, signed 4/1/2024)
Kentucky	2024	New section of KRS ch. 65 (2024 Ky. Acts ch. 3, HB 18, veto overridden 3/6/2024)
North Carolina	2024	G.S. 42-14.1(b) (S.L. 2024-47, HB 556, law over veto 9/9/2024)

Notes: These are the dates used to construct the panel, and the underlying laws allow the coding to be checked state by state. Tags: narrow marks a statute that limits rather than completely prohibits the local policy; Louisiana 1977 is indeterminate because its lease-rights statute does not mention rent control.

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