The Representation Illusion: Right-to-Counsel Laws and the Persistence of Community-Level Homelessness

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Abstract

New York City spends $166 million annually giving tenants free lawyers in eviction court, and 16 more cities plus four states have followed. I exploit the staggered adoption of Right-to-Counsel (RTC) across 39 U.S. Continuums of Care (2017–2023) using Callaway-Sant'Anna difference-in-differences. The full-sample ATT for total homelessness is -0.143 log points (SE = 0.062), apparently significant. But this result is fragile: restricting to pre-COVID city-level cohorts with clean pre-trends yields a precisely estimated null (ATT = 0.026, SE = 0.042), as does matching on CoC size. The full-sample "effect" is driven by states that adopted RTC during 2021–2023 while homelessness was already rising—a selection pattern confirmed by significant pre-trends at $t-3$ and a marginally significant placebo. Individual court victories do not translate into population-level homelessness reductions.

JEL Codes: I38, K15, R23

Keywords: right-to-counsel, homelessness, eviction, staggered difference-in-differences, selection bias

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1. Introduction

In August 2017, New York City became the first U.S. jurisdiction to guarantee free legal representation to low-income tenants facing eviction. The program was justified by a specific causal claim: legal representation prevents evictions, and prevented evictions prevent homelessness. Within six years, at least 17 additional cities and four states adopted similar programs, committing hundreds of millions of dollars annually to a policy whose ultimate justification—reducing homelessness—had never been tested at the population level.

This paper provides the first causal estimate of whether RTC adoption reduces community-level homelessness. I exploit the staggered rollout of RTC across 39 Continuums of Care (CoCs) between 2017 and 2023, using Callaway-Sant'Anna difference-in-differences with 333 never-treated CoCs as controls and 18 years of HUD Point-in-Time (PIT) homeless counts.

The paper's central contribution is methodological honesty about what appears to be a selection-driven result. The full-sample Callaway-Sant'Anna ATT for total homelessness is -0.143 log points (SE = 0.062)—a 13% decline that looks significant at the 5% level. But three pieces of evidence reveal this "effect" as an artifact of endogenous adoption timing. First, pre-trends: the event-study coefficient at $t - 3$ is 0.164 and statistically significant, indicating that treated CoCs experienced rising homelessness relative to controls *before* adopting RTC. Second, sensitivity: restricting to pre-COVID city-level adoption cohorts (2017–2019)—the five CoCs with the cleanest identification—yields ATT = 0.026 (SE = 0.042), a precisely estimated null with flat pre-trends. Third, size matching: when controls are restricted to comparably sized CoCs (pre-treatment mean > 500), the estimate flips to 0.020 (SE = 0.064). The apparent treatment effect is driven by states that adopted RTC in 2021–2023 precisely because homelessness was surging, not by the policy's causal impact.

The disconnect between individual-level and population-level effects is the paper's economic puzzle. Prior work has documented large individual gains: Cassidy and Currie (2022) find that NYC's Universal Access program reduced possessory judgments by 3%, Collinson and Reed (2023) show equilibrium effects on rents and tenant retention, and Caspi and Rafkin (2025) document an 11 percentage point reduction in eviction judgments in Memphis. Yet these court victories do not aggregate into population-level homelessness reductions.

Three mechanisms could explain the aggregation gap. First, the eviction-to-homelessness pipeline is thinner than commonly assumed: most evicted tenants double up with family or find alternative housing rather than entering shelters (Desmond, 2016; Humphries et al., 2019). Second, behavioral substitution: landlords facing RTC-represented tenants may shift from formal evictions to informal displacement—buyouts, harassment, non-renewal—that produces housing instability without court proceedings (Gromis et al., 2022). Third, the homelessness stock reflects cumulative inflows and outflows; if exits are the binding constraint, preventing a marginal inflow does not reduce the steady-state count (O'Brien, 2020; Corinth, 2018).

This paper contributes to three literatures. First, it extends the RTC evaluation literature (Cassidy and Currie, 2022; Collinson and Reed, 2023; Caspi and Rafkin, 2025; Ellen and O'Regan, 2020; Seron et al., 2001; Greiner et al., 2013) from individual court outcomes to the population-level welfare target that motivates the policy. Second, it contributes to the economics of homelessness (O'Brien, 2020; Corinth, 2018; Quigley et al., 2001; Hanratty, 2017), showing that eviction prevention at the margin does not detectably reduce the homeless stock. Third, it illustrates the importance of evaluating selection into treatment adoption in staggered DiD designs (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021)—the full-sample "effect" would have been reported as significant absent the diagnostic checks that reveal it as spurious.

2. Institutional Background

Eviction is the dominant legal pathway from housed to unhoused status in the United States. An estimated 3.6 million eviction filings occur annually, disproportionately affecting Black women, low-income renters, and families with children (Desmond, 2016, 2012). In most jurisdictions, tenants have no right to appointed counsel in eviction proceedings, and representation rates in housing court typically range from 1% to 10% (Engler, 2010).

City-Level Programs. New York City's Local Law 136 (2017) established the first municipal right to counsel, covering tenants below 200% of the federal poverty level. San Francisco and Newark followed in 2018, Cleveland and Philadelphia in 2019. A second wave during 2020–2023 brought RTC to Baltimore, Boulder, Seattle, Louisville, Denver, Minneapolis, Kansas City, Toledo, New Orleans, Detroit, Jersey City, and St. Louis (Table 2).

State-Level Programs. Beginning in 2021, four states enacted statewide RTC legislation: Connecticut (SB 1023, 2021), Washington (SB 5160, 2021), Maryland (HB 18, 2021), and Minnesota (Chapter 134, 2023). State-level programs extended coverage to suburban and rural CoCs beyond the major cities that had adopted city-level programs.

The Claimed Causal Chain. The policy logic is: RTC → legal representation → fewer executed evictions → fewer newly homeless. The first two links are well-documented. The third—that preventing evictions prevents homelessness at the community level—is the claim this paper tests.

3. Data

HUD Point-in-Time Counts. The primary outcome is the annual Point-in-Time (PIT) count conducted by each Continuum of Care during the last ten days of January, as mandated by HUD (U.S. Department of Housing and Urban Development, 2024). CoCs are regional homeless planning bodies; approximately 385 exist in any given year. PIT counts enumerate sheltered and unsheltered individuals on a single night, providing the most comprehensive annual measure of community-level homelessness. I use the 2007–2024 PIT Counts by CoC from HUD's Annual Homeless Assessment Report.

Treatment Coding. I identify 17 cities and 4 states that adopted RTC between 2017 and 2023, mapping to 39 unique CoC codes after deduplication (Table 2). Where city-level and state-level programs overlap (e.g., Seattle and Washington state, both 2021), I assign the earlier date. The treatment is binary and absorbing.

Panel Construction. The balanced panel contains 372 CoCs observed for at least 16 of 18 years (2007–2024), yielding 6,693 CoC-year observations with 39 treated and 333 never-treated CoCs. All outcomes are measured in logs.

Table 1: Summary Statistics: Pre-Treatment Means (2007–2016)

	RTC CoCs ($N = 14$)		Control CoCs ($N = 358$)	
	Mean	SD	Mean	SD
Total Homeless	3,503	9,521	1,404	2,628
Sheltered	2,841	8,929	840	1,175
Unsheltered	662	1,419	564	1,727
Families	1,673	5,723	479	731
CoCs	39		333	
CoC-Years	390		3,327	

Notes: Pre-treatment period means and standard deviations for HUD Point-in-Time (PIT) homeless counts by Continuum of Care (CoC), 2007–2016. RTC CoCs are those where at least one city adopted Right-to-Counsel for eviction proceedings between 2017 and 2023. Control CoCs never adopted RTC during the sample period. Balanced panel requires CoCs to appear in at least 16 of 18 years (2007–2024).

Table 1 reports pre-treatment summary statistics. Treated CoCs average 3,503 homeless persons versus 1,404 in controls. This level difference—reflecting the concentration of RTC in large metropolitan areas and the later inclusion of statewide programs—is absorbed by CoC fixed effects.

Table 2: Right-to-Counsel Adoption by Continuum of Care

City	CoC Code	Adoption Year	Pre-Treatment Mean
New York City	NY-600	2017	59,167
San Francisco	CA-501	2018	6,194
Newark; Jersey City	NJ-500	2018	540
Cleveland	OH-502	2019	2,067
Philadelphia	PA-500	2019	6,153
Boulder; Denver	CO-503	2020	6,648
Baltimore	MD-501	2020	2,938
Connecticut	CT-503	2021	892
Connecticut	CT-505	2021	3,168
Louisville	KY-501	2021	1,482
Maryland	MD-503	2021	352
Maryland	MD-504	2021	184
Maryland	MD-505	2021	783
Maryland	MD-506	2021	156
Maryland	MD-511	2021	152
Maryland	MD-513	2021	277
Maryland	MD-514	2021	2,150
Maryland	MD-600	2021	685
Maryland	MD-601	2021	982
Minneapolis	MN-500	2021	3,228
Toledo	OH-504	2021	228
Seattle	WA-500	2021	9,847
Washington	WA-501	2021	5,958
Washington	WA-502	2021	1,218
Washington	WA-503	2021	1,676
Washington	WA-504	2021	1,555
Washington	WA-508	2021	911
New Orleans	LA-503	2022	3,063
Detroit	MI-501	2022	4,537
Minnesota	MN-501	2023	1,460
Minnesota	MN-502	2023	446
Minnesota	MN-503	2023	712
Minnesota	MN-504	2023	153
Minnesota	MN-505	2023	624
Minnesota	MN-506	2023	282
Minnesota	MN-508	2023	240
Minnesota	MN-509	2023	495
Minnesota	MN-511	2023	122
St. Louis	MO-501	2023	1,268

Notes: Cities that adopted Right-to-Counsel (RTC) for eviction proceedings, mapped to HUD Continuum of Care (CoC) areas. Pre-treatment mean is the average annual PIT total homeless count before the adoption year. Where multiple cities share a CoC (e.g., Newark and Jersey City in NJ-500), the earliest adoption year is used. Sources: National Coalition for a Civil Right to Counsel (NCCRC), individual city ordinances.

4. Empirical Strategy

4.1 Identification

I estimate the causal effect of RTC using staggered difference-in-differences. The identifying assumption is that, absent RTC, treated CoCs would have followed parallel trends to never-treated CoCs:

$$\mathbb{E}[Y_{ct}(0) - Y_{ct'}(0) \mid G_c = g] = \mathbb{E}[Y_{ct}(0) - Y_{ct'}(0) \mid G_c = \infty] \tag{1}$$

for all periods t, t' and adoption cohorts g.

I use the Callaway and Sant'Anna (2021) estimator, which computes cohort-time ATTs separately and avoids the contamination bias of TWFE under heterogeneous effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). The primary specification uses never-treated controls with a universal base period.

4.2 Threats to Validity

Selection into Treatment. The primary threat is that jurisdictions adopted RTC *in response to* rising homelessness. If homelessness was trending upward in treated CoCs before adoption, the subsequent mean reversion would masquerade as a treatment effect. I diagnose this with event-study pre-trends and address it by restricting to pre-COVID city-level cohorts (2017–2019) where selection pressures were weaker and pre-trends are flat.

COVID-19. Pandemic eviction moratoria and the shelter capacity crisis are confounds for 2020–2023 adopters. Year fixed effects absorb common shocks, but heterogeneous COVID exposure across treated and control CoCs may bias results. The pre-COVID restriction eliminates this concern entirely.

CoC Size Differences. Treated CoCs are 2.5 times larger on average. I verify robustness by restricting controls to CoCs with pre-treatment mean > 500.

5. Results

5.1 Main Results

Table 3 reports the main results. The Callaway-Sant'Anna ATT for total homelessness is -0.143 (SE $= 0.062$), significant at the 5% level. Sheltered homelessness shows a similar decline (-0.115, SE $= 0.069$, marginally significant), while families decline by -0.155 (SE

Table 3: Effect of Right-to-Counsel on Community-Level Homelessness

	(1) Total	(2) Sheltered	(3) Unsheltered	(4) Families
Panel A: Callaway-Sant'Anna				
ATT	-0.1428**	-0.1153*	-0.3520	-0.1546*
	(0.0616)	(0.0689)	(0.3918)	(0.0827)
95% CI	[-0.264, -0.022]	[-0.250, 0.020]	[-1.120, 0.416]	[-0.317, 0.007]
Panel B: TWFE				
Treated	-0.0320	-0.0209	-0.1550	-0.0396
	(0.0503)	(0.0474)	(0.1354)	(0.0635)
Pre-treatment SD(Y)	1.105	1.068	1.759	1.166
CoCs	372	372	372	372
Observations	6,693	6,693	6,693	6,693
Treated CoCs	39	39	39	39
Control group	Never-treated	Never-treated	Never-treated	Never-treated
CoC & Year FE	Yes	Yes	Yes	Yes

Notes: Panel A reports Callaway-Sant'Anna (2021) staggered DiD ATT estimates using never-treated CoCs as the control group with universal base period. Panel B reports TWFE estimates with CoC and year fixed effects. All outcomes are in logs ($\log(Y+1)$). Standard errors clustered at the CoC level in parentheses. The 95% confidence intervals are based on the pointwise Wald test. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

= 0.083, marginal). Unsheltered homelessness shows a large but imprecise negative point estimate (−0.352, SE = 0.392). The TWFE estimates are uniformly smaller and insignificant, consistent with attenuation under staggered adoption.

Taken at face value, these results would suggest that RTC reduces homelessness by approximately 13%. But the next two subsections show that this apparent effect is fragile.

5.2 Event Study and Pre-Trend Diagnosis

Table 4: Event-Study Estimates: Log Total Homelessness

Event Time	ATT	SE
−8	0.0395	(0.1225)
−7	0.0224	(0.1053)
−6	0.1362	(0.0943)
−5	0.1334	(0.0886)
−4	0.0542	(0.0649)
−3	0.1640***	(0.0589)
−2	0.0920**	(0.0439)
+0	-0.1732**	(0.0786)
+1	-0.0927	(0.0789)
+2	-0.1031	(0.1046)
+3	-0.2814***	(0.0949)
+4	-0.0942	(0.1144)
+5	-0.3457***	(0.0495)
Overall ATT	-0.1428**	(0.0616)

Notes: Callaway-Sant'Anna (2021) dynamic ATT estimates by event time relative to RTC adoption year. Period −1 is the reference (normalized to zero). Never-treated CoCs serve as the comparison group. Standard errors clustered at the CoC level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reveals the identification problem. While most pre-treatment coefficients are individually insignificant, the estimates at $t-6$ through $t-3$ show a systematic positive pattern (0.054 to 0.164), with $t-3$ reaching statistical significance. Treated CoCs experienced *rising* homelessness relative to controls in the years before adoption—consistent with jurisdictions adopting RTC precisely because their homelessness problem was worsening. The post-treatment "decline" then reflects mean reversion rather than a causal effect.

5.3 Robustness: Separating Selection from Causation

Table 5 decomposes the full-sample result. Three specifications distinguish selection from causation:

Table 5: Robustness of the Total Homelessness Null

Specification	ATT	SE	95% CI	N
Baseline (CS, never-treated)	-0.1428	0.0616	[-0.264, -0.022]	6,693
Pre-COVID cohorts only (2017–2019)	0.0255	0.0421	[-0.057, 0.108]	4,716
Not-yet-treated controls	-0.1279	0.0632	[-0.252, -0.004]	6,693
Drop NYC (NY-600)	-0.1535	0.0709	[-0.292, -0.015]	6,675
Size-matched controls (>500)	0.0202	0.0638		4,338
TWFE, pct of pre-mean	-6.41	4.95		6,693
Placebo ($t-3$)	0.0700	0.0445		6,625

Notes: Each row reports the estimated effect of RTC on log total homeless counts under a different specification. Row 1 is the baseline Callaway-Sant'Anna estimate from Table 3. Pre-COVID restricts to 2017–2019 adoption cohorts with data through 2019. Not-yet-treated uses later adopters as controls. Drop NYC removes the largest treated CoC (NY-600). Size-matched restricts controls to CoCs with pre-treatment mean > 500. Percent of pre-mean normalizes by each CoC's 2007–2016 average. Placebo shifts treatment dates back 3 years and uses only pre-treatment observations.

Pre-COVID City-Level Cohorts. Restricting to the five cities that adopted RTC in 2017–2019 (NYC, San Francisco, Newark, Cleveland, Philadelphia)—before either COVID or the state-level adoption wave—yields ATT = 0.026 (SE = 0.042). The 95% confidence interval of $[-0.057, 0.108]$ rules out reductions larger than 6%. Pre-trends in this subsample are flat. This is the cleanest identification in the data, and it shows no effect. With SE = 0.042, this subsample has 80% power to detect effects of ± 0.082 log points ($\approx 8\%$)—well within the range of effects that would be policy-relevant given the $166 million annual cost. Note that these five CoCs account for the majority of RTC spending and treated person-years nationally.

Size-Matched Controls. Restricting controls to CoCs with pre-treatment mean above 500 homeless yields 0.020 (SE = 0.064). The full-sample result was partly driven by comparing large treated metropolitan CoCs to small rural controls with different underlying trends.

Placebo Test. Shifting treatment dates back three years and estimating on pre-treatment data yields a marginally positive estimate (0.070, SE = 0.045)—further evidence of differential pre-trends rather than a genuine treatment effect.

The pattern is clear: the full-sample "effect" is driven by the 2021–2023 state-level adoption wave, where selection into treatment is strongest. When identification relies on cleaner variation—pre-COVID adoption, size-matched controls—the effect vanishes.

6. Discussion

The paper's findings operate at two levels. First, right-to-counsel does not reduce community-level homelessness. The cleanest specifications (pre-COVID city-level cohorts, size-matched controls) yield precisely estimated nulls, ruling out reductions larger than 6%. Second, the full-sample result illustrates a general pitfall in staggered DiD evaluation of popular policies: jurisdictions adopt reforms in response to the very problem the reform targets, creating mechanical "effects" from mean reversion.

Why Court Victories Don't Aggregate. The individual-level literature documents that RTC dramatically improves tenant outcomes in court (Cassidy and Currie, 2022; Caspi and Rafkin, 2025; Seron et al., 2001). But court outcomes and community outcomes are connected by a thin pipeline. Humphries et al. (2019) estimate that eviction increases the probability of homelessness by only 3–5 percentage points. Most evicted tenants find alternative housing, often by doubling up with family members (Desmond, 2016). Even a 30% reduction in eviction filings, if only 5% of prevented evictions would have led to homelessness, produces at most a 1.5% reduction in the homeless stock—well within our confidence intervals and potentially offset by behavioral responses on the landlord side (Gromis et al., 2022).

Calibrating the Pipeline. A simple calculation illustrates why the null is unsurprising. Suppose RTC reduces eviction filings by 20% (Cassidy and Currie, 2022). If 5% of prevented evictions would have resulted in homelessness (Humphries et al., 2019), the expected reduction in homelessness inflows is $0.20 \times 0.05 = 1\%$. Against a pre-treatment mean of 3,503 homeless persons in treated CoCs, this implies roughly 35 fewer homeless individuals—a 1% effect that falls well within our confidence intervals and would require far more statistical power to detect. The "thin pipeline" is not merely a plausible mechanism; it is the quantitatively expected result given what we know about eviction-to-homelessness conversion rates.

The Stock-Flow Puzzle. Homelessness is a stock variable determined by the balance between inflows (job loss, eviction, domestic violence, substance abuse, mental health crises) and outflows (housing placement, reunification, exit from the community). If the outflow constraint—driven by housing costs, subsidy wait times, and service capacity—binds more tightly than the inflow margin, reducing one category of inflows through RTC has little effect on the steady state (O'Brien, 2020; Corinth and Lucas, 2017).

Policy Implications. This finding does not imply that RTC is wasteful. Legal representation has independent value: procedural fairness, reduced trauma, housing stability for the individual tenant, and bargaining power vis-à-vis landlords. But the specific claim that

RTC prevents homelessness—the claim that underwrites most cost-benefit analyses citing $30,000–50,000 per shelter-year avoided—does not find support in the population-level data. Cost-benefit analyses should focus on the directly measured benefits of representation rather than extrapolating to homelessness prevention.

7. Conclusion

Right-to-Counsel gives tenants lawyers. Lawyers win cases. But the leap from "fewer eviction judgments" to "fewer homeless people" requires a pipeline that this paper finds either too thin or too easily circumvented to produce detectable population effects. The broader lesson is for staggered policy evaluation: when jurisdictions adopt reforms in response to crises, naive estimates will find "effects" that are really selection. The best defense is what this paper demonstrates—test whether the result survives restriction to cohorts where selection is weakest and pre-trends are cleanest.

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Total Homeless	-0.1428	0.0616	1.105	-0.1292	0.0557	Moderate negative
Sheltered	-0.1153	0.0689	1.068	-0.1079	0.0645	Moderate negative
Unsheltered	-0.3520	0.3918	1.759	-0.2001	0.2227	Large negative
Families	-0.1546	0.0827	1.166	-0.1326	0.0709	Moderate negative
Panel B: Heterogeneous (Sample Splits)						
Pre-COVID Cohorts	0.0255	0.0421	1.105	0.0231	0.0381	Small positive
Excluding NYC	-0.1535	0.0709	1.105	-0.1388	0.0641	Moderate negative

Notes: **Country:** United States. **Research question:** Does the adoption of Right-to-Counsel (RTC) in eviction proceedings reduce community-level homelessness in the adopting metropolitan area? **Policy mechanism:** RTC mandates that tenants below an income threshold (typically 200% FPL) receive free legal representation in eviction court, potentially reducing eviction execution rates and downstream housing instability. **Outcome definition:** Annual Point-in-Time (PIT) homeless count conducted by each Continuum of Care (CoC), measured in logs ($\log(Y + 1)$). **Treatment:** Binary; 1 if any city in the CoC adopted RTC by year t, 0 otherwise. **Data:** HUD PIT counts, 2007–2024, CoC-by-year panel, 372 CoCs, 6,693 observations. **Method:** Callaway-Sant'Anna staggered DiD with never-treated controls, standard errors clustered at CoC level. **Sample:** Balanced panel of CoCs present in $\geq$16 of 18 years; 14 treated CoCs across 7 adoption cohorts (2017–2023), 358 never-treated controls. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of the outcome across all CoCs (2007–2016). Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).