Voucher Household Mobility and Rental Market Dynamics: Evidence from Source of Income Protection Laws

Luke Maddock

Department of Economics, Colorado State University, Fort Collins, CO 80523

Luke.Maddock@colostate.edu

October 8, 2025

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Abstract

I study how source-of-income (SOI) protections affect where voucher households live and rents in the segments where vouchers transact. I assemble an annual panel of incorporated places with ≥65,000 residents from 2011–2019 and exploit staggered adoption across 57 jurisdictions (first effective dates 2013–2018). Identification follows Callaway & Sant'Anna with doubly-robust adjustment using 2012 baseline covariates and never-adopters as the counterfactual. Across four measures of voucher household geographic concentration, event-study profiles show flat pre-trends and no detectable post-adoption change. By contrast, lower-tier rents rise after adoption: the 25th-percentile contract rent increases by 0.049 log points (about 5%), the 75th percentile is unchanged, and the stock share affordable at ≤30% of HAMFI falls by 1.8 percentage points. Effects are larger under stronger laws (greater enforcement, fewer exemptions) and in tighter rental markets (lower baseline vacancy). Results are robust to a synthetic staggered DiD estimator and to allowing one year of anticipation. The pattern is consistent with pass-through of compliance and screening costs in voucher-relevant segments without re-sorting of voucher households within places. Legal access alone appears insufficient to generate neighborhood gains absent reduced leasing frictions and expanded low-rent supply.

Keywords: Housing Choice Vouchers, Source of Income Protection, Tenant Protections

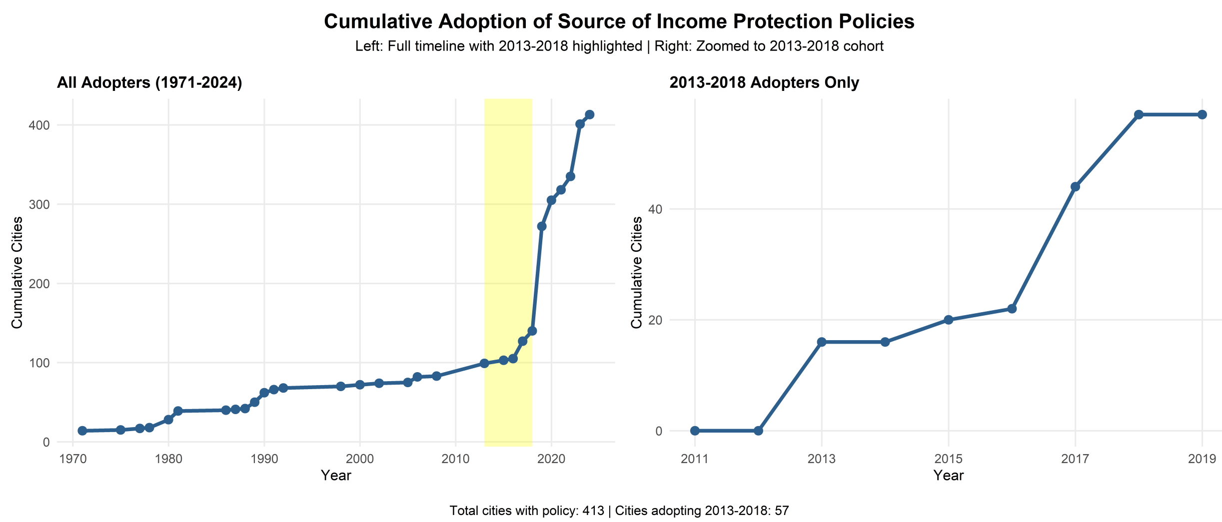
JEL Codes: R38, R31, I38, H75

I am grateful to Anita Pena, Ray Miller, Tim Komarek, and Jesse Burkhardt for invaluable guidance and feedback. I thank Stephan Weiler, Mo Alloush, Thomas Gifford, Greg Miller, Sean Callahan, Sophie McKee, and additional conference attendees for helpful comments. All errors are my own.

1 Introduction

The Housing Choice Voucher (HCV) program serves about 2.3 million low-income households annually (U.S. Department of Housing and Urban Development, 2025), yet landlords routinely reject voucher applicants before any assessment of tenant quality.[1] In response, many jurisdictions adopted source-of-income (SOI) protections prohibiting categorical rejection of voucher users. Among census-designated places with populations exceeding 65,000, the cumulative number covered by SOI protections grew from 140 in 2018 to 413 by 2024. Adoption has proceeded unevenly, producing a patchwork of coverage and scope that varies widely in timing, enforcement strength, and exemptions. The cumulative adoption timing across all passed laws and those within the sample period of this paper can be seen in Figure 1. Figure 2 illustrates the geographic distribution of this policy diffusion, showing that early adoption (2013-2016) occurred in scattered jurisdictions across the country, while the 2017 to 2018 period saw concentrated adoption along the east and west coasts.

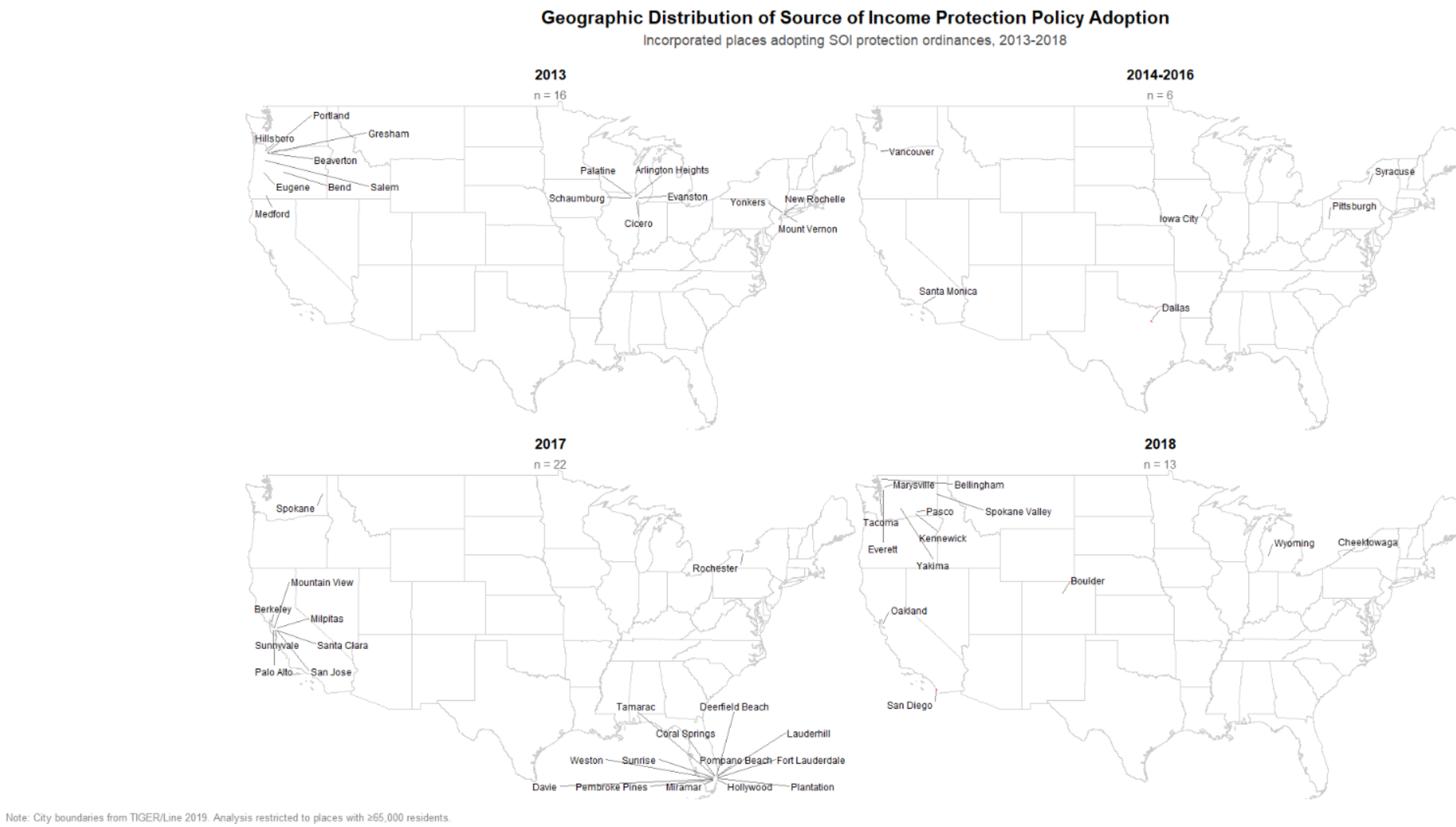
Figure 1: Adoption of SOI Protection Laws in US Cities



The barrier is practical as well as legal. In jurisdictions without protections, voucher seekers encounter non-response, categorical refusals, and steering, often before any assessment of tenant quality. Field and administrative evidence point to high rejection rates for voucher applicants, slow lease-up, and concentration of successful placements in familiar, higher-poverty submarkets that openly solicit voucher tenants (Tighe et al., 2016; Phillips, 2017; Cunningham et al., 2018). These frictions interact with time limits, inspection scheduling, and rent-reasonableness requirements to compress search into segments where acceptance is predictable, undermining the program's deconcentration goals. Furthermore, many newly issued vouchers

[1] See Figure 10 as an example.

Figure 2: Geographic Distribution of SOI Protection Law Adoption from 2013 to 2018



Notes: Maps represent census-designated places above 65,000 population that passed a Source of Income Protection law at any point between 2013 and 2018. Place information is gathered from ACS 1 Year surveys. Point geometry is gathered from TIGRIS boundary files.

are never successfully used within standard search windows, underscoring the depth of frictions at the lease-up margin. Using HUD administrative records across 200 PHAs (2014–2022), Ellen et al. (2025) estimates that only about 60 percent of households issued a voucher successfully lease a unit within six months. This take-up shortfall coexists with continuing concentration of successful lease-ups in higher-poverty submarkets.

SOI protections target the explicit refusal margin by making "No HCV", "No Voucher", or "No Section 8"[2] policies unlawful. Whether those protections alter outcomes depends on details that vary across places. This variation is central to both the empirical strategy and the interpretation of effects.

This paper studies two questions that follow directly from these channels. First, do SOI protections expand neighborhood choice for voucher households, as reflected in changes to where voucher households live across tracts and how they are distributed within places? Second, do SOI protections generate broader general-equilibrium effects in rental markets, particularly at the lower end of the rent distribution where voucher-affordable units are concentrated?

The empirical setting uses staggered SOI adoption across incorporated places with populations above 65,000 during 2011–2019, focusing on first effective dates from 2013 to 2018 to avoid confounding variation from the Great Recession peak and the COVID-19 pandemic. The panel links detailed policy features to two sets of outcomes. The first set captures voucher mobility and spatial distribution at the place level: weighted tract poverty exposure, the average number of voucher households per occupied tract, the percentage of all census tracts with any HCV households, and a normalized Herfindahl index of voucher concentration. The second set captures rental market conditions: the twenty-fifth and seventy-fifth percentiles of contract rent, the rental vacancy rate, and the share of the rental stock affordable at or below 30 percent of Area Median Family Income.

Identification follows Callaway and Sant'Anna (2021). Treated units are places with first effective dates in 2013–2018; the counterfactual is the set of places that do not adopt within this window. Dynamic event-time estimates summarize the evolution of outcomes before and after adoption and allow inspection of pre-trends. Because eventual adopters and never-adopters differ systematically on observable characteristics that relate to both adoption and outcomes, dynamic treatment effects are estimated with a doubly robust approach that conditions on 2012 baseline covariates, mitigating bias from pre-policy differences while preserving the staggered-adoption design. Because policy design and market tightness plausibly shape the magnitudes of the dynamic effects, heterogeneity is examined along three dimensions emphasized by institutions and

[2]The Housing Choice Voucher program is commonly referred to as "Section 8," referencing Section 8 of the Housing Act of 1937 that originally established the federal housing assistance framework. While the current voucher program was created by the Housing and Community Development Act of 1974 and substantially reformed in 1998, the "Section 8" terminology persists in both policy discussions and popular usage. Landlord advertisements stating "No Section 8" typically refer to rejection of Housing Choice Voucher recipients.

prior evidence: stronger versus weaker enforcement, fewer versus more exemptions, and lower versus higher baseline (2012) rental vacancy as a proxy for market tightness.

Across all mobility measures there is no detectable change in the neighborhoods where voucher households live or in the spatial dispersion of voucher households within places following SOI adoption. By contrast, lower-tier rents rise after adoption, with effects concentrated where enforcement is stronger, exemptions are fewer, and baseline vacancy is low. The twenty-fifth percentile rent rises by about 4.9 percent on average (5.5 percent in strong-law places and 5.2 percent in low-vacancy places), while the share of units affordable at or below 30 percent of Area Median Family Income declines by 1.8 percentage points on average (2.1 percentage points in strong-law places and 1.9 percentage points in low-vacancy places). The pattern is consistent with adjustments in tight, voucher-relevant segments that shift prices without materially altering the geography of voucher households. These results remain robust under alternative empirical specifications: synthetic difference-in-differences estimation as an alternative to the Callaway & Sant'Anna approach, and incorporating one year of anticipation effects to account for forward-looking behavior by housing providers and HCV households.

This study contributes in three ways. First, it examines market-wide rental price effects of SOI protections using a staggered-adoption design, providing evidence on a dimension that remains underexplored. Second, it aligns outcome definitions with policy scope by distinguishing mover-focused findings in prior work from place-level stock measures, and by evaluating multiple mobility and spatial concentration metrics rather than a single neighborhood indicator. Third, it investigates heterogeneity along policy and market dimensions highlighted by institutional context and prior evidence (enforcement strength, exemptions, and baseline market tightness), offering insight into where and why effects are larger.

These findings speak to SOI protection and broader tenant protection policy design. Reducing explicit refusal may be insufficient to deliver neighborhood gains in the presence of early-stage screening frictions and limited short-run supply at the low end. Complementary tools, such as faster inspections and lease-up processes, targeted landlord engagement or bonuses, payment-standard reforms, and additional supply in voucher-affordable segments, may be necessary for translating legal protections into improved residential outcomes without amplifying rent pressure.

The remainder of the paper proceeds as follows. Section 2 reviews related literature on voucher discrimination and SOI protections. Section 3 describes institutional background and policy measurement. Section 4 documents data sources, outcome definitions, and baseline covariates. Section 5 outlines the estimation strategy and heterogeneity design. Section 6 presents the main and subgroup results. Section 7 interprets mechanisms and policy implications, and Section 8 concludes.

2 Literature Review

2.1 Voucher search frictions and neighborhood access (direct channel)

A large literature documents persistent barriers that constrain Housing Choice Voucher recipients' residential choices. Early work established that voucher households disproportionately lease in higher-poverty neighborhoods, limiting progress toward deconcentration goals (Devine et al., 2003; Pendall et al., 2014). Subsequent studies confirm that these patterns have proven durable despite program refinements (McClure et al., 2015; Ellen, 2020). Qualitative research underscores that clustering reflects constraints rather than preferences: voucher households routinely encounter categorical refusals, informational gaps, tight timelines, and administrative frictions that compress searches into familiar submarkets (DeLuca, 2014; Graves, 2016; Galvez, 2010). These frictions interact with inspection scheduling and rent reasonableness to tilt successful lease-up toward segments where acceptance is predictable.

2.2 Evidence on source-of-income discrimination

2.2.1 Prevalence and geographic variation

Audit and correspondence studies consistently find substantial discrimination against voucher users. A HUD-sponsored multi-metro audit documented wide acceptance rate variation across markets and neighborhood types, with rejection especially pronounced in low-poverty areas (Cunningham et al., 2018). Correspondence designs reach similar conclusions in diverse settings: landlords frequently fail to respond to inquiries that disclose voucher use or impose conditions that effectively exclude voucher households (Turner et al., 1999; Phillips, 2017; Aliprantis et al., 2019; Hangen, 2022). These patterns suggest that, absent legal protections, explicit source-of-income screening is pervasive and systematically narrows the effective search set for voucher recipients.

2.2.2 Landlord responses under regulation (screening, pricing, timing)

Evidence also indicates that landlord behavior adapts as legal constraints evolve. Observational and practitioner reports describe early-funnel strategies—non-response, discouragement, and shifts to minimum income and credit thresholds—that are difficult to detect in complaint-driven systems (Unlock NYC et al., 2022). Correspondence experiments around application-screening reforms in Minneapolis show substitution toward earlier-stage discrimination against specific groups, highlighting how enforcement gaps at initial contact can blunt policy intent (Gorzig and Rho, 2025). Relatedly, field evidence that monetary incentives increase landlord willingness to consider vouchers points to acceptance margins that respond to expected returns and perceived costs (Aliprantis et al., 2019; Collinson and Ganong, 2018; Desmond and Perkins, 2016; Rosen, 2014). Recent work also documents landlord strategy adjustment under SOI protection regimes, such as adapting to screening on more restricive credit requirements or strategically reducing the chances of a successful housing authority inspection (Lucio and Cho, 2025; Cho and Lucio, 2025). Together, these studies map the margins (screening, pricing near payment standards, and timing) along which owners can comply formally while maintaining effective control over tenant selection.

2.3 Effects of SOI protections on voucher mobility and location

Research on the impacts of SOI protections has focused on utilization and locational outcomes, with a growing distinction between mover-specific effects and changes in the stock distribution. Early difference-in-differences analyses reported higher voucher utilization following adoption (Freeman, 2012) and modest improvements in neighborhood characteristics where voucher households reside (Freeman and Li, 2014). More recent work using administrative records and larger samples emphasizes temporal dynamics and mover focus: voucher movers experience greater reductions in tract poverty after adoption, with effects that tend to materialize several years post-enactment (Ellen et al., 2022). Event-study evidence on families with children similarly finds rising access to low-poverty areas with effects emerging over a longer horizon, and with larger gains for Black families (Teles and Su, 2022). Syntheses conclude that SOI protections yield meaningful but gradual improvements, conditioned by local design features and enforcement capacity, and that discrimination persists through less visible channels (Galvez and Knudsen, 2024; Lens et al., 2011; Basolo and Nguyen, 2005). A notable gap in this literature concerns the effectiveness of enforcement practices and the extent to which exemptions limit coverage, issues that motivate attention to heterogeneity by enforcement strength and exemptions.

2.4 Research gap: market-wide rent effects

While several studies examine rents and affordability in voucher-accessible units, little evidence directly links SOI protections to market-wide price dynamics. Theoretically, landlord adjustments to higher expected administrative or legal costs, combined with short-run inelastic supply in low-tier segments, could generate upward pressure on rents; related policy contexts document such equilibrium responses (Abramson, 2024; Coulson et al., 2025). Whether similar general-equilibrium effects arise following SOI adoption remains largely untested, motivating explicit examination of lower-tier rents and the supply of affordable units alongside neighborhood outcomes.

2.5 Contribution of this study

This study extends the literature in three ways. First, it examines market-wide rental price effects of SOI protections using a staggered-adoption design, contributing evidence on a dimension that remains underexplored. Second, it aligns outcome definitions with policy scope by distinguishing mover-focused findings in prior work from place-level stock measures, and by evaluating multiple mobility and spatial concentration metrics rather than a single neighborhood indicator. Third, it investigates heterogeneity along policy and market dimensions highlighted by institutional context and prior evidence—enforcement strength, exemptions, and baseline market tightness—providing insight into where and why effects are likely to be larger.

3 Institutional Background

3.1 The Housing Choice Voucher Program

The HCV program represents the cornerstone of federal rental assistance policy in the United States. Administered by approximately 2,200 local Public Housing Authorities (PHAs) under federal oversight from the Department of Housing and Urban Development (HUD), the program currently serves 2.3 million low-income households with portable rental subsidies. Unlike the place-based public housing developments that dominated earlier federal housing policy, vouchers embody a market-oriented approach that allows recipients to seek housing throughout the private rental market.

Eligibility for the program is typically restricted to households earning below 30 percent of area median income, with 75 percent of new vouchers reserved for extremely low-income families below this threshold. Once a household receives a voucher, they contribute approximately 30 percent of their adjusted income toward rent, while the PHA pays the landlord the difference up to a locally determined payment standard. These payment standards are generally set near the 40th percentile of area rents, providing recipients with access to a substantial portion of the local rental market in theory. PHAs typically set payment standards within a band around the FMR (for example, 90–110 percent by default, with higher levels subject to approval), and some jurisdictions use Small Area FMRs that vary by ZIP code (Ellen, 2020).

The voucher utilization process involves several critical steps that create potential friction points. After receiving a voucher, families typically have 60 to 90 days to locate suitable housing, though PHAs may grant extensions in difficult market conditions. The chosen unit must pass HUD's Housing Quality Standards inspection, and the landlord must agree to sign a Housing Assistance Payment (HAP) contract with the PHA. Only after these requirements are met can the family move in and begin receiving rental assistance. This timeline pressure is intensified by PHA performance incentives: housing authorities are expected to maintain utilization rates of 90-98 percent of their authorized vouchers, with funding consequences for those that fall significantly below these targets. The shortness of lease-up windows, inspections and rent-reasonableness determinations must be completed before move-in, and vouchers can expire absent extensions, which compresses search into submarkets with predictable acceptance (Tighe et al., 2016; Cunningham et al., 2018; Ellen, 2020).

Despite the program's market-based design, voucher holders face substantial barriers that limit their housing choices. Research consistently shows that many recipients struggle to use their vouchers within the allotted timeframe, with success rates varying dramatically across metropolitan areas and demographic groups. Those who do successfully lease units often find themselves concentrated in higher-poverty neighborhoods, undermining the program's poverty deconcentration objectives.

3.2 Source of Income Protection Laws

3.2.1 Legal Framework and Geographic Variation

Source-of-income protection laws target a central barrier faced by voucher households: categorical refusal by owners to consider tenants who pay with housing subsidies. SOI is not a protected class under the federal Fair Housing Act; coverage is provided by state and local law (Schwemm, 2016). These SOI protection laws add "source of income" to state or local fair housing statutes and make it unlawful to reject an applicant solely because rent would be paid in part by a Housing Choice Voucher. Where covered, owners are expected to apply the same screening criteria used for other applicants—credit, rental history, and other non-voucher factors—rather than exclude voucher users per se.

However, statutory language differs in whether housing vouchers are explicitly included, implied, or excluded from the protected class described in each law. Recent statewide adoptions include Hawaii, which now prohibits discrimination based on participation in voucher programs, and Delaware, which enacted statewide protections in 2024 with renter coverage taking effect in 2026. By contrast, Wisconsin's state law has been interpreted not to treat federal vouchers as a protected "lawful source of income," leaving acceptance voluntary under state law unless a local ordinance applies. In several states, legislatures have preempted local governments from adopting their own SOI ordinances; Iowa and Texas are prominent examples.

Coverage also reflects political and market conditions. Jurisdictions adopting SOI protections tend to be higher-income, with an older and more college-educated population, and statutes differ in the breadth of units covered via exemptions and carve-outs (Cho and Lucio, 2025). In the period relevant for the empirical analysis, 2013–2019, adoption accelerated. Within the estimation sample of large incorporated places, 57 census-designated places adopted SOI protections with first effective dates between 2013 and 2018. This variation in timing, enforcement strength, and exemptions motivates the staggered research design and the heterogeneity analyses reported below.

3.2.2 Policy Mechanisms and Implementation Challenges

SOI protections are enforced through a mix of administrative and judicial channels. Most jurisdictions rely on complaint-driven processes administered by civil rights or human rights agencies; some provide for private rights of action with damages and fee-shifting. Enforcement capacity and remedies vary, and so does effective coverage: many laws include exemptions for specific property types or owners (for example, owner-occupied small buildings, certain small-landlord thresholds, or units already governed by other programs), which can leave a substantial share of the stock outside the rule's reach. For measurement, the analysis distinguishes enforcement strength (agency authority, investigative tools or testing, penalty structure, private right of action) and the prevalence of exemptions; Section 4 details the coding rubric.

Implementation faces several structural challenges. First, early-funnel behavior is difficult to police in complaint-driven systems: non-response to inquiries, discouragement, or shifting to minimum-income and credit thresholds can sustain effective exclusion even where explicit refusal is unlawful (Unlock NYC et al., 2022; Cunningham et al., 2018; Phillips, 2017). Second, payment standards and rent-reasonableness tests cap what a voucher household can transact, so units priced even slightly above the standard may remain effectively out of reach despite formal protections (Ellen et al., 2025). Third, administrative requirements, such as inspections, Housing Assistance Payment contracts, and sequencing with PHA approval, impose time and process costs that can deter participation independent of legal obligations to consider voucher applicants. These features imply that the same statute may bind strongly in jurisdictions with robust enforcement and few exemptions, and bind weakly where coverage is narrow or markets are tight, motivating the focus on heterogeneity by enforcement, exemptions, and baseline vacancy (Lucio and Cho, 2025).

3.3 Conceptual Framework

SOI protection laws operate through two primary channels that can produce both intended benefits for voucher holders and unintended consequences for the broader rental market. This section outlines the theoretical mechanisms through which SOI policies affect housing market outcomes, focusing on the behavioral responses of both voucher recipients and landlords.

3.3.1 Voucher Holder Response Channel

The first channel through which SOI laws operate is by expanding the effective choice set available to voucher recipients. Prior to policy adoption, voucher holders face a constrained search process where a substantial portion of the rental market is explicitly off-limits due to "No Section 8" policies. Rational voucher holders, aware of this discrimination, may limit their search efforts to landlords known to accept vouchers or neighborhoods with high concentrations of subsidized housing, even if they would prefer to live elsewhere.

When SOI protections are enacted, voucher holders gain legal recourse against discriminatory denials and can reasonably expect fairer treatment from a broader set of landlords. This expansion of the effective choice set should induce voucher holders to search more widely across neighborhoods and property types, including areas they might have previously avoided due to anticipated discrimination. The magnitude of this response depends on several factors: voucher holders' awareness of the new legal protections, their confidence in enforcement mechanisms, and their preferences for neighborhood characteristics versus the costs and uncertainty of expanded search.

For SOI laws to generate measurable effects on voucher holder outcomes, this behavioral response is necessary but not sufficient. Voucher holders must actually expand their search patterns and apply to landlords who would have previously rejected them outright. If recipients continue to limit their search to the same subset of accommodating landlords, the policy would have little impact regardless of its legal force.

3.3.2 Landlord Response Channel

The second channel operates through landlord behavioral responses to the new legal constraint on their tenant selection process. Prior to SOI adoption, landlords who preferred not to rent to voucher holders could simply refuse such applicants without legal consequence. This allowed them to avoid perceived costs or risks associated with voucher tenants—whether real administrative burdens, unfounded stereotypes, or preferences for particular tenant types[3].

SOI laws eliminate landlords' ability to categorically exclude voucher applicants, forcing them to evaluate such applicants using the same criteria applied to all prospective tenants. However, landlords retain considerable discretion in how they respond to this constraint, leading to several possible equilibrium adjustments.

First, some landlords may comply with the law's intent by genuinely evaluating voucher applicants on

[3]HUD issued a fact sheet about the HCV program to attempt to change landlord perception on voucher tenants - Fact Sheet

their merits. These landlords might discover that their previous aversion to voucher tenants was unfounded, potentially leading to increased acceptance rates and improved outcomes for voucher holders.

Second, landlords who continue to view voucher tenants as higher-cost or higher-risk may adjust their rental terms to reflect these perceived costs. This could manifest as higher security deposits, stricter screening criteria that disproportionately affect voucher holders, or most importantly for our analysis, higher rental prices. If landlords anticipate increased costs from a tenant pool that now includes more voucher holders, they may raise rents preemptively to maintain their expected returns.

Third, some landlords may attempt to circumvent the law through subterfuge, using pretextual reasons to reject voucher applicants or steering them away through discouraging behavior. The prevalence of such evasion depends critically on enforcement strength and the penalties for violations.

Finally, landlords operating in high-demand markets or luxury segments may find that SOI laws have little practical impact on their tenant selection. If these landlords receive numerous applications from qualified non-voucher tenants, they may rarely encounter voucher applicants in the first place, or may legitimately reject them based on income requirements that exceed voucher payment standards.

3.3.3 Market-Level Implications

The interaction of these two channels produces aggregate effects that may extend beyond the voucher population. If SOI laws successfully increase the effective demand for rental housing by enabling more voucher holders to compete for units throughout the market, and if landlords respond by raising rents to offset perceived costs, the result could be upward pressure on rental prices that affects all low-income renters.

However, rent effects may be limited or absent if SOI laws fail to meaningfully constrain landlord behavior in practice. Landlords may continue excluding voucher holders through several mechanisms that render the legal prohibition ineffective. Weak enforcement capacity or complaint-driven systems may create insufficient deterrence, allowing continued discrimination with minimal consequences. The prevalence of legal exemptions—such as owner-occupied properties or small landlords—may preserve discriminatory exclusion for substantial portions of the rental market. Additionally, landlords in high-demand markets with numerous applications may rarely encounter situations where they must choose between renting to voucher holders or leaving units vacant, effectively nullifying the policy's binding constraint. In such contexts, SOI laws would produce limited behavioral change among landlords, resulting in minimal rent adjustments and smaller improvements in voucher holder access.

This rent effect could be particularly pronounced in the lower tier of the rental market, where voucher holders are most likely to compete with other low-income households. Higher rents in this segment could price out some non-voucher low-income households, potentially offsetting some of the gains achieved by voucher recipients. The net welfare effect would depend on the relative magnitudes of improved voucher utilization versus the displacement of other low-income renters.

The theoretical framework thus suggests that SOI laws should improve outcomes for voucher holders who successfully benefit from expanded access, while potentially generating negative spillovers for other market participants through rent increases. The empirical analysis that follows tests these predictions and quantifies the relative importance of these competing effects.

4 Data and Empirical Strategy

4.1 Policy Data

SOI policy information is assembled from the Urban Institute's State and Local Voucher Protection Laws database and the National Low Income Housing Coalition's Tenant Protection Database. Each statute or ordinance is matched to a census place via exact and fuzzy name matching verified against state and county identifiers, and time-stamped by the first effective date (passage dates are used only when coincident with effectiveness).

Within the estimation window, 57 census-designated places enter coverage between 2013 and 2018. Table 1 lists treated places, first effective year, and the adoption layer (state, county, municipal). Adoption-layer shares are summarized beneath the table. Definitions and sources for policy variables used below appear in Table 2.

4.2 Data Construction and Sample

Evidence from Cho and Lucio (2025) shows that SOI adoption may not be random: eventual adopters differ from never-adopters along observables tied to housing markets and local institutions, including higher incomes and rents, greater college attainment, and denser tenant-protection environments. These differences raise concerns about selection bias in a simple comparison of adopters versus never-adopters, since the same factors that drive SOI adoption may also influence housing outcomes independently of the policy itself.

To address this challenge, I assemble a comprehensive set of baseline place characteristics measured in 2012, before any place in the sample adopts SOI protections during the analysis window. The covariate set is designed to capture observable factors that theory and prior evidence suggest influence both the propensity to adopt SOI laws and the housing outcomes of interest. These include housing market fundamentals (median household income, median contract rent, rental vacancy rate), demographic composition (college-

Table 1: Places Adopting Source of Income Protection Policies, 2013–2018

Place	First Year	Level	Place	First Year	Level
Berkeley, CA	2017	Place	Mount Vernon, NY	2013	County
Milpitas, CA	2017	County	New Rochelle, NY	2013	County
Mountain View, CA	2017	County	Rochester, NY	2017	Place
Oakland, CA	2018	Place	Syracuse, NY	2016	Place
Palo Alto, CA	2017	County	Yonkers, NY	2013	County
San Diego, CA	2018	Place	Beaverton, OR	2013	State
San Jose, CA	2017	County	Bend, OR	2013	State
Santa Clara, CA	2017	County	Eugene, OR	2013	State
Santa Monica, CA	2015	Place	Gresham, OR	2013	State
Sunnyvale, CA	2017	County	Hillsboro, OR	2013	State
Boulder, CO	2018	Place	Medford, OR	2013	State
Coral Springs, FL	2017	County	Portland, OR	2013	State
Davie, FL	2017	County	Salem, OR	2013	State
Deerfield Beach, FL	2017	County	Pittsburgh, PA	2015	Place
Fort Lauderdale, FL	2017	County	Dallas, TX	2016	Place
Hollywood, FL	2017	County	Bellingham, WA	2018	State
Lauderhill, FL	2017	County	Everett, WA	2018	State
Miramar, FL	2017	County	Kennewick, WA	2018	State
Pembroke Pines, FL	2017	County	Marysville, WA	2018	State
Plantation, FL	2017	County	Pasco, WA	2018	State
Pompano Beach, FL	2017	County	Spokane, WA	2017	Place
Sunrise, FL	2017	County	Spokane Valley, WA	2018	State
Tamarac, FL	2017	County	Tacoma, WA	2018	State
Weston, FL	2017	County	Vancouver, WA	2015	Place
Arlington Heights, IL	2013	County	Yakima, WA	2018	State
Cicero, IL	2013	County			
Evanston, IL	2013	County			
Palatine, IL	2013	County			
Schaumburg, IL	2013	County			
Iowa City, IA	2015	Place			
Wyoming, MI	2018	Place			
Cheektowaga, NY	2018	County			

Notes: This table lists all 57 places that adopted Source of Income Protection policies between 2013 and 2018. Sample includes all incorporated cities and towns with population $\geq$ 65,000 based on ACS 2019 estimates that enacted SOI legislation during the analysis window. First Year indicates the first year in which the policy was active at the place level. Level indicates the governmental level at which the policy was initially enacted (Place = local ordinance, County = county ordinance, State = state legislation).

educated share, poverty rate, median age, Black population share, total population), economic conditions (unemployment rate, SNAP participation rate), the existing tenant protection environment (indicators for pre-2013 anti-retaliation protections, limits on fees, just-cause eviction standards, legal-defense funds, and right to counsel), voucher program presence (HCV units per 1,000 residents), and broader policy context (count of other state-level tenant policies active before 2013). Complete definitions and data sources for all variables appear in Table 2.

These covariates are then used to assess pre-treatment balance of observables. The analysis builds an annual panel for 2011–2019 at the census "place" level (incorporated cities and towns) using ACS one-year geographies. The sample frame includes all places with population at least 65,000 on a fixed baseline (2012), yielding 521 places in 2012, of which 57 adopt a source-of-income (SOI) protection with first effective dates between 2013 and 2018, and 464 that never adopt within the estimation window.[4] Places with SOI protections effective before 2011 are excluded to ensure a pre-policy baseline. Place boundaries follow the ACS one-year "place" geography (2010 vintage).

Table 3 documents the extent of pre-treatment imbalance by comparing means and standard deviations across eventual adopters and never-adopters in 2012. To quantify meaningful differences, I calculate standardized mean differences (SMD) defined as:

$$\text{SMD} = \frac{\bar{X}_{\text{treated}} - \bar{X}_{\text{control}}}{\sqrt{\frac{(n_T-1)s_T^2+(n_C-1)s_C^2}{n_T+n_C-2}}},$$

where $\bar{X}_{\cdot}$ and $s_{\cdot}$ are group means and standard deviations and n_T, n_C are the treated and control sample sizes. Balance is a property of the realized sample, so hypothesis tests and p-values are poor diagnostics because they fluctuate with sample size rather than substantive imbalance Ho et al. (2007). Standardized mean differences are scale-free, directly interpretable in SD units, and recommended as the primary check; a common rule of thumb treats $SMD \geq 0.15$ as large. Using SMDs focuses the diagnostic on imbalance itself, which is what drives bias and model dependence.

The results confirm substantial baseline differences between eventual adopters and never-adopters. Several variables exceed the 0.15 threshold, including state-level tenant-policy activity (SMD = 0.671), college-educated share (SMD = 0.429), median rent (SMD = 0.414), median age (SMD = 0.335), anti-retaliation protections (SMD = 0.268), and median household income (SMD = 0.188). These patterns indicate that places eventually adopting SOI protections were systematically different in 2012: they had higher rents and incomes, more college-educated populations, older median ages, greater prevalence of existing tenant protections, and operated in states with more active tenant-policy environments.

These baseline differences motivate the doubly robust estimation approach described in Section 5, which combines outcome regression with inverse probability weighting to condition on the full set of 2012 covariates. This adjustment helps ensure that the estimated treatment effects reflect the causal impact of SOI adoption rather than pre-existing differences between adopting and non-adopting places.

[4] "Never-adopter within window" denotes places without an effective SOI date through 2019.

Table 2: Variable definitions and data sources

Variable	Definition	Source
Anti-retaliation ordinance (pre-2013)	Anti-retaliation protection in force before 2013 (1 = yes)	NLIHC
Limits on fees (pre-2013)	Caps or bans on application/late fees before 2013 (1 = yes)	NLIHC
Ability to expunge eviction records (pre-2013)	Tenants can seal or expunge eviction filings prior to 2013 (1 = yes)	NLIHC
Just-cause eviction standards (pre-2013)	Requirement that landlords cite "just cause" before 2013 (1 = yes)	NLIHC
Legal-defense fund (pre-2013)	Public fund covering tenant legal defense before 2013 (1 = yes)	NLIHC
Right to counsel (pre-2013)	Guaranteed right to counsel in eviction cases before 2013 (1 = yes)	NLIHC
HCV Units per 1,000 Total Population	Number of Housing Choice Voucher units per 1,000 Residents	HUD PSH
Median contract rent (2019 $)	Median monthly contract rent	ACS 1 Year Surveys
Median household income (2019 $)	Median household income	ACS 1 Year Surveys
Rental vacancy rate (%)	Share of rental units that are vacant	ACS 1 Year Surveys
Unemployment rate (%)	Civilian unemployment rate	ACS 1 Year Surveys
College degree share (%)	Adults (25+) with bachelor's degree or higher	ACS 1 Year Surveys
SNAP participation rate (%)	Households receiving SNAP benefits	ACS 1 Year Surveys
Poverty rate (%)	Individuals below the federal poverty line	ACS 1 Year Surveys
Median age (years)	Median age of residents	ACS 1 Year Surveys
Black population share (%)	Residents identifying as Black or African American	ACS 1 Year Surveys
Total population	Total resident population	ACS 1 Year Surveys
Other state SOI laws	Count of SOI protections active in the same state before 2013	Policy databases

Note: SOI = Source of Income; HCV = Housing Choice Voucher; ACS = American Community Survey, HUD PSH = HUD Picture of Subsidized Households. Data is gathered to observe balance between treated and never treated groups at baseline 2012 values. Policy variables denote the presence of a policy at any point before 2013, not just within the sample period. Other SOI Laws include SOI protections present at the city or county level in areas within the same state as the city of observation.

Table 3: Baseline characteristics stratified by treatment status, 2012 Values

	Stratified by treated			
Variable	**Overall**	**Never Treated**	**Treated**	**SMD**
Groups (unique census designated places)	521	464	57	–
Anti-retaliation protection, pre-2013 (mean, SD)	48.0 (50.0)	46.0 (50.0)	60.0 (49.0)	0.268(*)
Limits on fees, pre-2013 (mean, SD)	1.0 (10.0)	1.0 (9.0)	0.0 (0.0)	0.075
Just-cause eviction standards, pre-2013 (mean, SD)	3.0 (18.0)	3.0 (17.0)	5.0 (23.0)	0.112
Eviction legal-defense fund, pre-2013 (mean, SD)	2.0 (9.0)	2.0 (14.0)	2.0 (9.0)	< 0.001
Right to counsel, pre-2013 (mean, SD)	11.0 (67.0)	10.0 (56.0)	12.0 (45.0)	0.04
HCV Units per 1,000 Total Population (mean, SD)	9.11 (5.42)	8.97 (5.24)	9.21 (5.87)	0.077
Median rent, 2019 $ (mean, SD)	869.55 (287.94)	855.04 (279.10)	981.67 (330.71)	0.414(*)
Median household income, 2019 $ (mean, SD)	53,593.02 (18,495.00)	53,190.59 (18,430.24)	56,702.71 (18,869.29)	0.188(*)
Vacancy rate (%, mean, SD)	9.55 (5.44)	9.59 (5.43)	9.29 (5.59)	0.055
Unemployment rate (%, mean, SD)	10.25 (3.76)	10.26 (3.85)	10.18 (2.98)	0.021
College-educated population (%, mean, SD)	30.89 (13.84)	30.16 (13.29)	36.59 (16.57)	0.429(*)
SNAP participation rate (%, mean, SD)	13.68 (7.78)	13.56 (7.66)	14.62 (8.65)	0.129
Poverty rate (%, mean, SD)	17.62 (7.90)	17.74 (8.01)	16.75 (7.03)	0.130
Median age (mean, SD)	34.79 (4.35)	34.63 (4.36)	36.05 (4.13)	0.335(*)
Black population share (mean, SD)	0.14 (0.16)	0.15 (0.16)	0.13 (0.16)	0.109
Total population (mean, SD)	179,790.70 (261,944.95)	179,207.45 (262,280.87)	184,202.13 (261,744.19)	0.019
State-level tenant-policy count (mean, SD)	0.92 (1.47)	0.76 (1.10)	2.21 (2.85)	0.671(*)

Notes. This table compares means and standard deviations (in parentheses) of key covariates in 2012 for cities that never adopt an SOI law by 2018 and those that will adopt between 2013–2018. The column *SMD* reports the standardized mean difference. An SMD above 0.15 indicates a meaningful imbalance, marked by (*).

4.3 Outcome Variables

The outcome groups are chosen to align with the two channels through which SOI protections operate. For HCV mobility, seen in in Table 4, the goal is to track whether legal access translates into changes in where voucher households live within a place. Voucher household-weighted tract poverty exposure provides a standard measure of neighborhood economic conditions experienced by HCV households. The normalized Herfindahl index summarizes spatial concentration in a scale-free way that is robust to the number of tracts and the distribution of HCV presence across them, making it the preferred concentration metric when comparing places of different sizes. Two supplementary dispersion statistics—the share of tracts with any HCV presence and the average number of HCV households per occupied tract—help interpret movements in the primary indicators by distinguishing diffusion across new tracts from reallocation within already-active tracts.

Figure 3 visualizes tract-level changes between 2012 and 2019 in (i) poverty rate and (ii) the share of households using HCVs for two adopters (Portland, OR—SOI 2013; Dallas, TX—SOI 2016). Blues denote increases and reds denote decreases; color scales are symmetric across cities. These panels are descriptive only and serve to clarify the construction and interpretation of the HCV-weighted tract poverty outcome used below.

For market-wide rental conditions, seen in Table 5, the focus is on the price segment where vouchers transact and on an affordability quantity that should co-move with those prices. The twenty-fifth percentile of contract rent targets the lower tier of the market near payment-standard constraints, where small price adjustments are most likely to bind for voucher households. The seventy-fifth percentile provides a natural falsification outcome that should be largely insensitive if effects are concentrated in the voucher-relevant segment. The share of units affordable at or below 30 percent of HAMFI serves as an extensive-margin complement to the price measure, indicating whether shifts in low-tier rents translate into changes in the available stock of very low-rent units. Finally, the rental vacancy rate is reported to gauge short-run slack consistent with price movements, but it is treated as a mechanism rather than a primary target of policy effects.

Summary statistics for 2012 pre-treatment values of all outcome variables can be seen in Table 6.

Figure 3: Changes in Poverty Rate and HCV Household Share by Census Tract, 2012 → 2019 (Portland above; Dallas below).

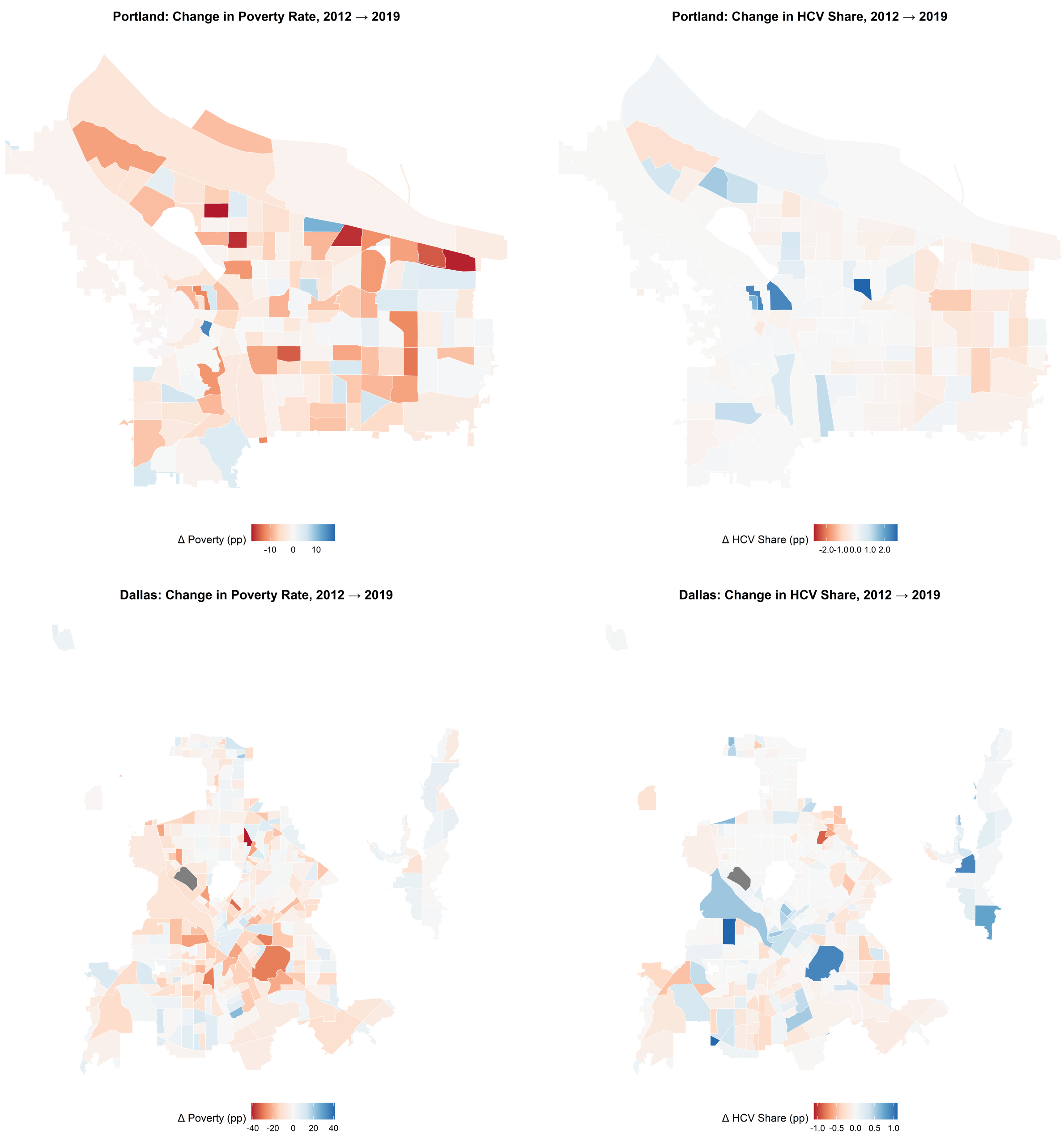


Table 4: HCV Mobility Outcomes: Definitions and Sources

Variable	Definition	Source
Weighted HCV Household Tract Poverty Rate	Voucher-weighted tract poverty exposure: $\frac{\sum_i H_i \cdot \text{pov}_i}{\sum_i H_i}$, where H_i is the number of HCV households in tract i and pov_i is the tract poverty rate.	HUD Picture of Subsidized Households
Mean HCV Households per Occupied Census Tract	Average number of HCV households per occupied tract, H/n_{active}, where n_{active} is the count of tracts with ≥ 1 HCV household.	HUD Picture of Subsidized Households
Normalized Hirschman-Herfindahl Index	Normalized HHI of HCV spatial concentration: $\frac{\text{HHI} - 1/n}{1 - 1/n} \times 100$, with $\text{HHI} = \sum_i s_i^2$ and $s_i = H_i/H$. Ranges 0 (uniform) to 100 (complete concentration).	HUD Picture of Subsidized Households
Occupied Tract Share Percentage	Share of tracts with any HCV presence: $n_{\text{active}}/n_{\text{total}}$, where n_{total} is the total number of tracts in the place.	HUD Picture of Subsidized Households

Notes: Data comes from census tract level measures aggregated to the place level provided by the HUD Picture of Subsidized Households. Weighted HCV Household Tract Poverty is calculated and provided by HUD PSH. All other variables are the author's calculations. HCV = Housing Choice Vouchers. HHI - Herfindahl-Hirschman Index.

Table 5: Rental Market Outcomes: Definitions and Sources

Variable	Definition	Source
Ln(25th Percentile Rent)	Log of the 25th percentile (P25) of the contract-rent distribution for renter-occupied and vacant-for-rent units at the place level; contract rent excludes utilities; CPI-U adjusted to 2019 dollars.	ACS 1-year
Ln(75th Percentile Rent)	Log of the 75th percentile (P75) of the contract-rent distribution; contract rent excludes utilities; CPI-U adjusted to 2019 dollars.	ACS 1-year
Rental Inventory Share Affordable at $\leq$ 30% HAMFI	Share of total rental inventory (occupied + vacant-for-rent) with gross rent $\leq$ 30% of HUD Area Median Family Income (HAMFI), using the 30% rent-to-income threshold.	HUD CHAS
Rental Vacancy Rate	Rental vacancy rate: vacant-for-rent units divided by total rental units.	ACS 1-year

Notes: HCV = Housing Choice Vouchers. HAMFI = HUD Area Median Family Income. Contract rent excludes utilities. CHAS data uses gross rent (contract rent + utilities) with HUD affordability thresholds based on 30% rent-to-income with HAMFI. Means and standard deviations calculated using 2012 values (pre-treatment for all units).

Table 6: Summary Statistics for Outcome Variables - 2012 values

Variable	N	Mean	SD	Min	Max
Tracts with any HCV (%)	521	80.26	20.42	4.00	100.00
HCV HH per occupied tract	521	41.28	25.81	1.00	218.44
HCV-weighted tract poverty (%)	521	20.50	8.96	4.00	60.00
Normalized HHI (0–100)	521	11.69	12.10	0.22	100.00
P25 rent ($2019)	521	698.11	288.46	295.00	2080.00
P75 rent ($2019)	521	1542.77	562.98	945.00	3350.00
Affordable share $\leq$ 30% HAMFI (%)	521	9.28	5.09	0.51	46.66
Rental vacancy rate (%)	521	9.34	5.54	0.36	39.03

Notes: All measures are calculated as the means of both treated and untreated groups for 2012 values. HCV = Housing Choice Vouchers. HHI = Herfindahl Index. HAMFI = HUD Area Median Family Income. Contract rent excludes utilities. CHAS data uses gross rent (contract rent + utilities) with HUD affordability thresholds based on 30% rent-to-income with HAMFI. Means and standard deviations calculated using 2012 values (pre-treatment for all units).

5 Empirical Strategy

5.1 Design and Estimator

The analysis follows the staggered difference-in-differences framework of Callaway and Sant'Anna (2021). Cohorts are defined by the first effective year of SOI coverage, $g \in \{2013, \ldots, 2018\}$, and in each calendar year t I estimate group-time average treatment effects on the treated relative to places that are never treated within 2011–2019. Not-yet-treated places are not used as controls after their effective dates. Standard errors are clustered at the place level.

Because eventual adopters and never-adopters differ on observables at baseline, I implement the doubly robust version of the estimator. Covariates are measured once, in 2012, as described in Section 4, and held fixed to avoid post-treatment conditioning. The estimator combines outcome regression with inverse-probability weighting so that consistency obtains if either the regression model or the propensity score is correctly specified. The weighting step improves overlap and covariate balance between treated cohorts and never-adopters, which is important given the baseline differences documented in the balance tests.

5.2 Heterogeneity

Mechanisms are examined in two prespecified breakouts that map policy design and market tightness into treatment intensity. First, a "strong-law" classification combines the enforcement and exemptions indicies provided by the Urban Institute, summarized in Table 7: places with an enforcement score of at least three and an exemptions score of at least 3 at adoption are labeled strong-law; all others are weak-law. I re-estimate the event study separately for strong- and weak-law adopters against the same pool of never-adopters, using the identical 2012 covariates.

Second, I stratify treated places by baseline tightness using the 2012 rental vacancy rate, defining low- and high-vacancy markets at the treated and non-treated sample median. Heterogeneity focuses on the outcomes that most directly test the two channels. For HCV mobility, I use the normalized HHI of voucher spatial concentration and the voucher household-weighted tract poverty exposure, which are the most robust measures of concentration and neighborhood quality relative to more mechanically sensitive dispersion statistics. For rental markets, I use the log of the twenty-fifth percentile of contract rent and the share of units affordable at or below 30 percent of HAMFI, where the former targets the low-rent segment relevant for voucher transactions and the latter provides a complementary extensive-margin check on affordability.

As shown in Table 8, the distribution across these categories is relatively balanced, with 18 places in the Strong-High quadrant, 19 in Strong-Low, 9 in Weak-High, and 11 in Weak-Low. This variation provides sufficient power to test the hypothesis that SOI effects should be larger where laws are stronger and markets are tighter.

Table 7: SOI Protections Policy Strength Scoring Framework

Dimension	Component	Points	Description
Enforcement	Private Right of Action	+1	Allows private parties to file civil lawsuit directly in court
	Civil Damages	+1	Allows court to order monetary relief to winning plaintiff
	Attorney's Fees	+1	Allows court to order losing defendant to reimburse legal costs
	Criminal Penalties	+1	Allows court to impose fines or sentences in criminal cases
	Maximum	*4*	
Exemptions	Good Faith Business Decision	-1	Allows landlord to deny housing based on reasonable business judgment
	Minimum Income Requirement	-1	Allows landlord to require minimum income from non-voucher sources
	Owner Occupied/Property Size	-1	Exempts small properties or owner-occupied units
	Religious/Nonprofit Owner	-1	Exempts properties owned by religious or nonprofit organizations
	Maximum	*4*	

Notes: This table summarizes the scoring framework for SOI protection policy strength developed by the Urban Institute. The framework evaluates two key dimensions of policy design: enforcement mechanisms available and exemptions that weaken coverage. The scoring allows for systematic comparison of SOI protections policy strength across jurisdictions. Enforcement begins at a score of 0 and receives a point for each of the standardized components listed in the table. Exemptions begin at a score of 4 and lose a point for each of the standardized components that are present by place of adoption.

Table 8: Places Adopting SOI Protection Policies by Law Strength and Vacancy Level, 2013–2018

Strong Laws, High Vacancy	**First Year**	**Strong Laws, Low Vacancy**	**First Year**
Berkeley, CA	2017	Milpitas, CA	2017
Oakland, CA	2018	Mountain View, CA	2017
Coral Springs, FL	2017	Palo Alto, CA	2017
Davie, FL	2017	Santa Monica, CA	2015
Deerfield Beach, FL	2017	Sunnyvale, CA	2017
Fort Lauderdale, FL	2017	Tamarac, FL	2017
Hollywood, FL	2017	Weston, FL	2017
Lauderhill, FL	2017	Arlington Heights, IL	2013
Pembroke Pines, FL	2017	Evanston, IL	2013
Plantation, FL	2017	Palatine, IL	2013
Pompano Beach, FL	2017	Schaumburg, IL	2013
Sunrise, FL	2017	Beaverton, OR	2013
Cicero, IL	2013	Bend, OR	2013
Syracuse, NY	2016	Eugene, OR	2013
Medford, OR	2013	Gresham, OR	2013
Pittsburgh, PA	2015	Hillsboro, OR	2013
Spokane, WA	2017	Portland, OR	2013
Tacoma, WA	2018	Salem, OR	2013
		Miramar, FL	2017
Weak Laws, High Vacancy	**First Year**	**Weak Laws, Low Vacancy**	**First Year**
San Diego, CA	2018	Boulder, CO	2018
Cheektowaga, NY	2018	Iowa City, IA	2015
Mount Vernon, NY	2013	Wyoming, MI	2018
New Rochelle, NY	2013	San Jose, CA	2017
Rochester, NY	2017	Santa Clara, CA	2017
Yonkers, NY	2013	Bellingham, WA	2018
Dallas, TX	2016	Everett, WA	2018
Kennewick, WA	2018	Marysville, WA	2018
Yakima, WA	2018	Pasco, WA	2018
		Spokane Valley, WA	2018
		Vancouver, WA	2015

Notes: This table cross-classifies the 57 places adopting SOI protections between 2013–2018 by law strength and baseline rental market tightness. Strong Laws have both enforcement and exemption scores ≥ 3 from the Urban Institute policy database; Weak Laws have at least one score ≤ 3. High Vacancy places had above-median rental vacancy rates in 2012 among treated and never-treated places; Low Vacancy places had below-median rates. The classification allows examination of heterogeneity by policy design features and market conditions that theory suggests should influence treatment intensity.

5.3 Robustness

5.3.1 Synthetic Staggered Difference in Differences

As a robustness check, I implement a synthetic staggered difference-in-differences (SDID) estimator. SDID blends the strengths of synthetic control and DiD by learning unit weights (across untreated donors) and time weights (across periods) that make the counterfactual path of treated units closely track their pre-treatment outcomes, then comparing appropriately weighted post-treatment means. This relaxes reliance on conventional parallel-trends and improves fit when unobserved confounding evolves through latent unit×time factors, a setting where SDID has attractive robustness properties and good empirical performance. The method is first proposed from Arkhangelsky et al. (2021) and formalized by Porreca (2022).

In staggered-adoption panels, SDID is applied cohort-by-cohort (first treated in year g versus never-treated donors) and the cohort estimates are then aggregated, yielding a single ATT that preserves the staggered structure and avoids problematic comparisons to later-treated units. This cohort-wise synthetic construction addresses concerns about poor counterfactual matches or compositional issues in multi-period TWFE designs. When rich pre-policy covariates are available, a standard approach is to residualize outcomes on the vector of pre-treatment covariate values, then run SDID on the residual values. This absorbs systematic X-related differences while preserving SDID's outcome-based balancing (Porreca, 2022). In doing so, this approach mitigates the concerns of selection bias from observable characteristics as mentioned previously.

SDID supports several variance estimators. In practice, bootstrap, placebo, and jackknife are common, with coverage documented in simulations (Arkhangelsky et al., 2021; Porreca, 2022). I use placebo inference. For each adoption cohort g, fixing the pre/post split at g and the never-treated donor pool, I draw R placebo replications by (i) selecting a pseudo-treated subset of donors with the same cardinality as the actual cohort, (ii) assigning them placebo exposure $W_{it} = \mathbf{1}\{t \geq g\}$, (iii) re-estimating SDID on that placebo block (re-learning unit and time weights from pre-g outcomes), and (iv) recording $\tilde{\tau}_g^{(r)}$. The cohort-level standard error is the dispersion of the placebo distribution, $\widehat{\text{se}}(\hat{\tau}_g) = \text{sd}(\{\tilde{\tau}_g^{(r)}\}_{r=1}^{R})$, yielding Wald 95% CIs $\hat{\tau}_g \pm 1.96\,\widehat{\text{se}}(\hat{\tau}_g)$; a randomization p-value is $\hat{p} = \frac{1+\sum_{r=1}^{R}\mathbf{1}\{|\tilde{\tau}_g^{(r)}| \geq |\hat{\tau}_g|\}}{R+1}$. This design respects cohort timing, matches the panel's dependence structure, and avoids contamination by restricting donors to never-treated units.[5]

5.3.2 Anticipation Effects

A potential threat to identification is that landlords and PHAs may learn about, prepare for, or partly comply with SOI protections before their first effective date that the policy is active for. In many jurisdictions, ordinances are passed months before they become enforceable, accompanied by public guidance, agency outreach, and media coverage. Owners may adjust screening or list prices in anticipation to avoid future frictions (inspection sequencing, rent-reasonableness negotiations, complaint risk). If such pre-treatment responses are material, dynamic effects estimated with the last pre-period year (e.g., $e = -1$) in the identifying set could understate true post-adoption changes or blur the timing of effects.

To assess this, I re-estimate the Callaway–Sant'Anna event studies, allowing an anticipation window of one year. Concretely, for each outcome I compute cohort- and time-specific doubly-robust ATT with the year immediately prior to first effectiveness dropped from the set of periods used to construct counterfactuals. To properly re-estimate the dynamic effects given baseline observable imbalances, I re-define all baseline

[5] Implemented via `synthdid_se(method="placebo")`. R is set to $R = 400$ in the main results.

covariates using 2011 mean values, which is the last period that is untreated and unanticipated for the earliest cohort (g=2013). I then aggregate to dynamic event time and cluster standard errors at the place level in the same manner as is done for the main results.

5.4 Identification and Validity

The causal interpretation of the results depends critically on the conditional parallel trends assumption: that treated and never-treated places would have followed similar outcome trajectories absent SOI adoption, conditional on the 2012 baseline covariates. This assumption is non-trivial given the substantial pre-treatment differences documented in Table 3, where several covariates exhibit standardized mean differences exceeding 0.4, indicating that eventual adopters were systematically different places with higher incomes, rents, education levels, and more active tenant-protection environments.

The doubly robust estimator addresses this challenge by combining outcome regression with inverse probability weighting to condition on the full vector of 2012 baseline characteristics. This approach provides protection against model misspecification: consistent estimates obtain if either the outcome regression or the propensity score model is correctly specified. The rich covariate set is designed to capture observable factors that theory and prior evidence suggest influence both adoption propensity and housing market outcomes. While this adjustment cannot eliminate bias from unobserved time-varying confounders, the event-study profiles provide evidence on pre-treatment trends. Joint tests of lead coefficients assess whether differential trends existed before adoption; flat pre-period profiles support the conditional parallel trends assumption.

Several design features strengthen the identification strategy. The focus on incorporated places eliminates partial treatment concerns that would arise from using larger geographic units like metropolitan areas or counties, where SOI laws might cover only portions of the boundary. By matching policy coverage precisely to outcome measurement boundaries, the analysis captures the full intensity of treatment exposure. The staggered timing across 57 places and six adoption years (2013-2018) provides multiple quasi-natural experiment settings, reducing dependence on any single cohort or time period. The number of contributing treated place-year units across event times can be seen in Figure 13.

The robustness check using SDID provides some reassurance by employing a different identification strategy that constructs counterfactuals through outcome-based matching rather than conditioning on pre-specified covariates. The consistency of results across estimators strengthens confidence in the findings, though both approaches share core identifying assumptions about the comparability of treated and control units. The current results should be interpreted as the causal effects of SOI adoption under the assumption that the rich baseline controls successfully account for systematic differences between adopting and non-adopting places.

6 Results

6.1 Overall ATTs

Table 9: Summary of Average Treatment Effects on the Treated (ATT)

Outcome	ATT	Std. Error	95% Conf. Int.
HCV Mobility Outcomes			
Occupied Tract Share (%)	-0.23	3.92	[-4.92, 4.45]
HCV HH per Occupied Tract	0.27	1.60	[-2.87, 3.41]
Normalized HHI (0-100)	0.53	0.84	[-1.12, 2.18]
HCV-Weighted Tract Poverty (%)	-0.20	0.39	[-0.97, 0.57]
Rental Market Outcomes			
Ln(25th Percentile Rent)	0.050**	0.014	[0.026, 0.078]
Affordable Share $\leq$ 30% HAMFI (%)	-1.70**	0.32	[-2.20, -0.87]
Ln(75th Percentile Rent)	0.010	0.018	[-0.026, 0.046]
Rental Vacancy Rate (%)	-0.60	0.42	[-1.43, 0.23]

Notes: This table reports overall average treatment effects on the treated (ATT) from the Callaway and Sant'Anna (2021) doubly robust estimator with event-study aggregation. Effects are estimated relative to never-adopters, conditioning on 2012 baseline covariates. Standard errors are clustered at the place level. ** indicates statistical significance at the 5% level (confidence interval does not include zero). HCV = Housing Choice Vouchers; HHI = Herfindahl-Hirschman Index; HAMFI = HUD Area Median Family Income.

The pooled treatment effects reveal a clear pattern consistent with the event-study findings. Across all four HCV mobility measures, estimates are small and statistically indistinguishable from zero—weighted tract poverty (0.20 p.p.; 95% CI: 0.97, 0.57), normalized HHI (0.53 points; 95% CI: 1.12, 2.18), share of tracts with any HCV presence (0.23 p.p.; 95% CI: 4.92, 4.45), and HCV households per occupied tract (0.27; 95% CI: 2.87, 3.41)—indicating no detectable within-place change in voucher geography after SOI adoption. By contrast, rental market outcomes show effects concentrated at the low end: the 25th-percentile contract rent rises by about 5.0% (0.050 log points; 95% CI: 0.026, 0.078), and the share of units affordable at or below 30% of HAMFI falls by 1.7 p.p. (95% CI: 2.20, 0.87). Upper-tier rents remain unchanged (75th percentile: 0.010 log points; 95% CI: 0.026, 0.046), and the rental vacancy rate is negative but imprecise (0.60 p.p.; 95% CI: 1.43, 0.23). Taken together, SOI protections appear to generate price pressure at the low end of the rent distribution without materially altering the spatial distribution of voucher households within places.

The following figures denote the dynamic effects relative to time in years before and after treatment occurs. Across figures, two overarching features bear emphasis. First, for the majority of the dynamic effect estimates, the lead coefficients in all panels are centered near zero, providing graphical support for parallel pre-trends. Second, the post-adoption dynamics for rents and affordability are gradual rather than instantaneous, with effects building over one to several years and showing little sign of reversal within the observed horizon, whereas the mobility outcomes remain flat throughout.

6.2 HCV Mobility Results

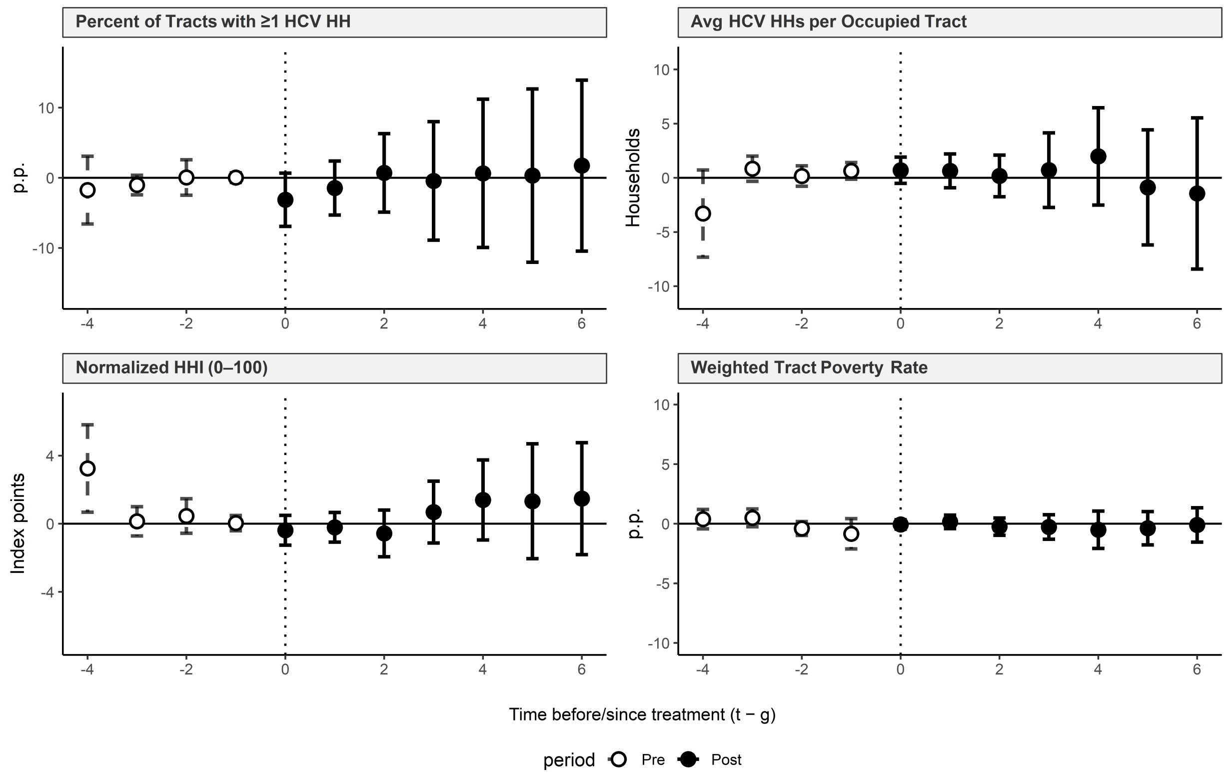
The HCV mobility event studies in Figure 4 show flat pre-trends and no discernible post-adoption movement across all four measures. Voucher-weighted tract poverty remains centered near zero (pooled ATT: 0.20 p.p.; 95% CI: 0.97, 0.57), indicating no detectable shift toward lower-poverty neighborhoods following SOI adoption. The normalized HHI exhibits the same pattern (0.53 index points; 95% CI: 1.12, 2.18), with no systematic rise or decline in concentration. The two dispersion statistics are noisier post-adoption but likewise show no persistent changes—share of tracts with any HCV presence: 0.23 p.p. (95% CI: 4.92, 4.45); HCV households per occupied tract: 0.27 (95% CI: 2.87, 3.41). Together, these profiles suggest that, within places, legal protections did not translate into measurable re-sorting of voucher households across tracts over the study horizon, and the null mobility findings are not an artifact of a single metric.

The stratified event studies for voucher neighborhood quality (Figure 5) and voucher concentration (Figure 6) mirror the pooled mobility results. By the strong- versus weak-law split, post-adoption effects for tract poverty and for the normalized HHI are statistically indistinguishable from zero in every event year (all 95% CIs span zero), with estimates hovering near zero before and after adoption. Splitting on baseline tightness yields visually opposite drifts in tract poverty—mildly downward in low-vacancy places and upward late in high-vacancy places—but the low-vacancy series shows non-flat leads and both series have wide post-period intervals (again, 95% CIs include zero throughout), so these patterns are not interpreted as causal. Taken together, the heterogeneous cuts provide no credible evidence of within-place re-sorting by voucher households; any vacancy-related divergence is suggestive at most and lacks robust pre-trend support.

6.3 Rental Market Results

Figure 7 turns to rental conditions and reveals a sharp split between the lower and upper tiers of the market. The log 25th-percentile contract rent drifts upward after adoption and remains elevated (pooled ATT: 0.050 log points, about 5%; 95% CI: 0.026, 0.078). By contrast, the log 75th percentile is essentially flat (0.010 log points; 95% CI: 0.026, 0.046), a falsification consistent with effects concentrated in voucher-relevant segments. The affordability quantity moves as expected: the share of units with gross rent 30% of HAMFI declines (1.70 p.p.; 95% CI: 2.20, 0.87), mirroring the low-end price rise. The vacancy rate shows no clear

Figure 4: Dynamic Effect Estimates for HCV Household Mobility Outcomes

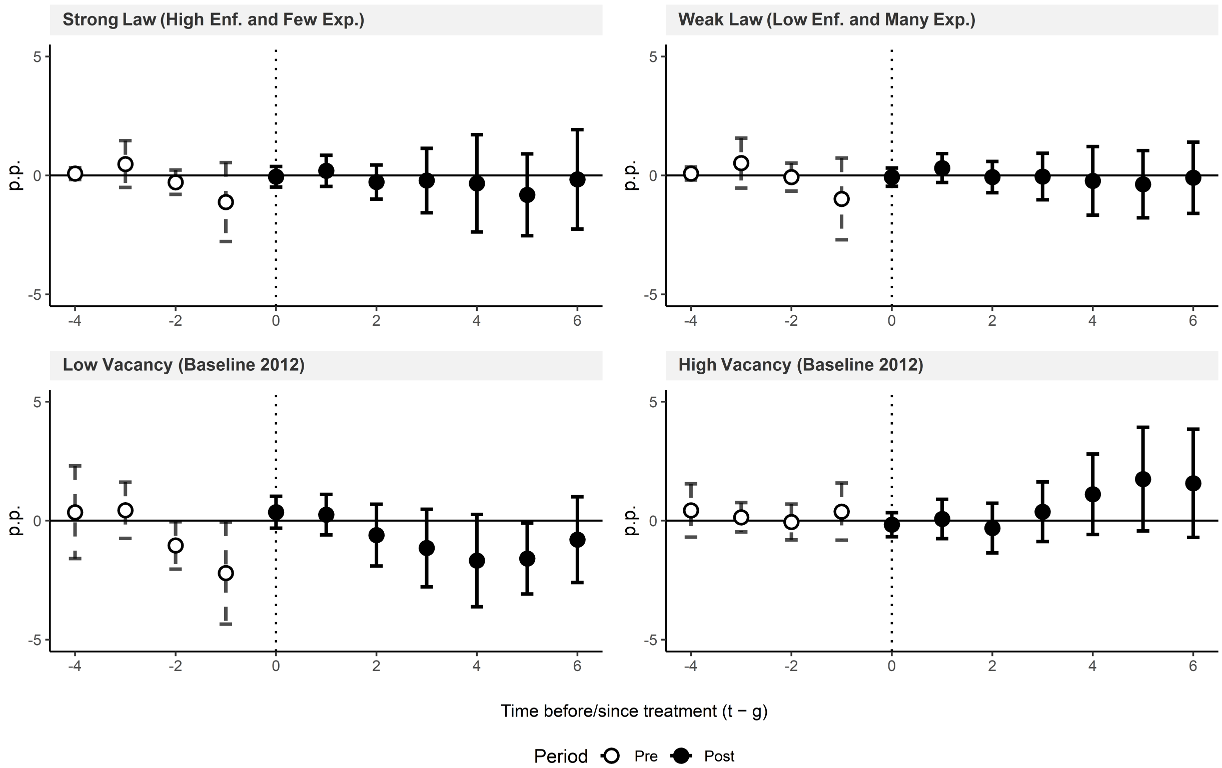


Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on HCV mobility related outcomes from Outcome Table A. Effects are estimated from Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. The results account for conditional parallel trends pre-treatment 2012 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred. "p.p" - Percentage points, "HHI" - Herschman-Herfindahl Index.

contemporaneous shift (0.60 p.p.; 95% CI: 1.43, 0.23). Together, these patterns indicate price pressure concentrated where vouchers transact alongside a measurable contraction in the stock of very low-rent units.

In contrast to the HCV mobility results, the stratified rental outcomes (Figures 8 and 9) show clear heterogeneity consistent with stronger binding where laws are tighter and markets are less slack. The 25th-percentile contract rent rises more in strong-law places than in weak-law places (0.067 log points; 95% CI: 0.036, 0.112; weak-law: 0.010 log points; 95% CI [-0.022, 0.048]), with the gap opening after adoption and persisting. A similar amplification appears by baseline tightness (low-vacancy: 0.075 log points; 95% CI: 0.038, 0.142; high-vacancy: -0.014 log points; 95% CI: -0.021, 0.008), with post-treatment estimates larger and more sustained in tighter markets. The affordability share moves in the opposite direction, falling more in strong-law and low-vacancy places (strong-law: -2.01 percentage points; 95% CI [-2.67, -0.97]; weak-law: 0.27 p.p.; 95% CI [-0.14, 0.31]; low-vacancy: -2.18 p.p.; 95% CI [-3.21, -1.16]; high-vacancy: 0.13 p.p.; 95% CI [-2.25, 2.45]). Together, these heterogeneous profiles align with the interpretation that stronger,

Figure 5: Dynamic Effect Estimates for Weighted Tract Poverty Rate by Stratification



Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on the weighted average poverty rate experienced by HCV Households by census tract across certain stratified groups. Effects are estimated from Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. Strong Law refers to the policy's enforcement and exemption score both being greater than or equal to 3 for a given city. Low vacancy refers to the city having a lower than median rental vacancy rate in 2012 across the entire sample of treated and non-treated cities. The results account for conditional parallel trends pre-treatment 2012 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred.

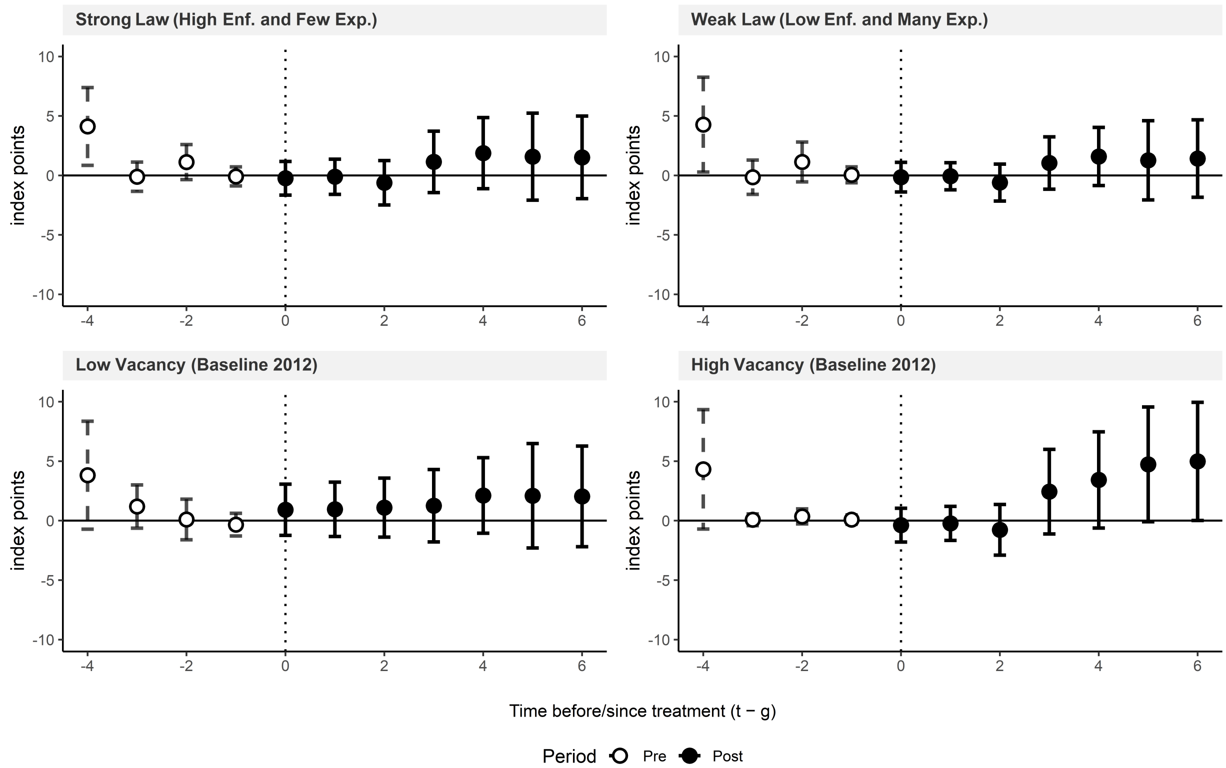
broader-coverage statutes and tighter baseline conditions generate more pronounced price responses at the low end of the market while leaving voucher spatial distributions unchanged.

6.4 Robustness Results

6.4.1 Synthetic DiD Results

The synthetic staggered difference-in-differences results provide strong confirmation of the main findings. As with the Callaway and Sant'Anna (2021) estimator, all four HCV mobility outcomes remain statistically indistinguishable from zero, with point estimates of similar magnitude and direction. The rental market effects are likewise confirmed and, if anything, slightly amplified under the SDID approach. The 25th percentile rent increase is 6.0 percent (versus 5.0 percent in the main specification), while the decline

Figure 6: Dynamic Effect Estimates for Normalized HHI by Stratification



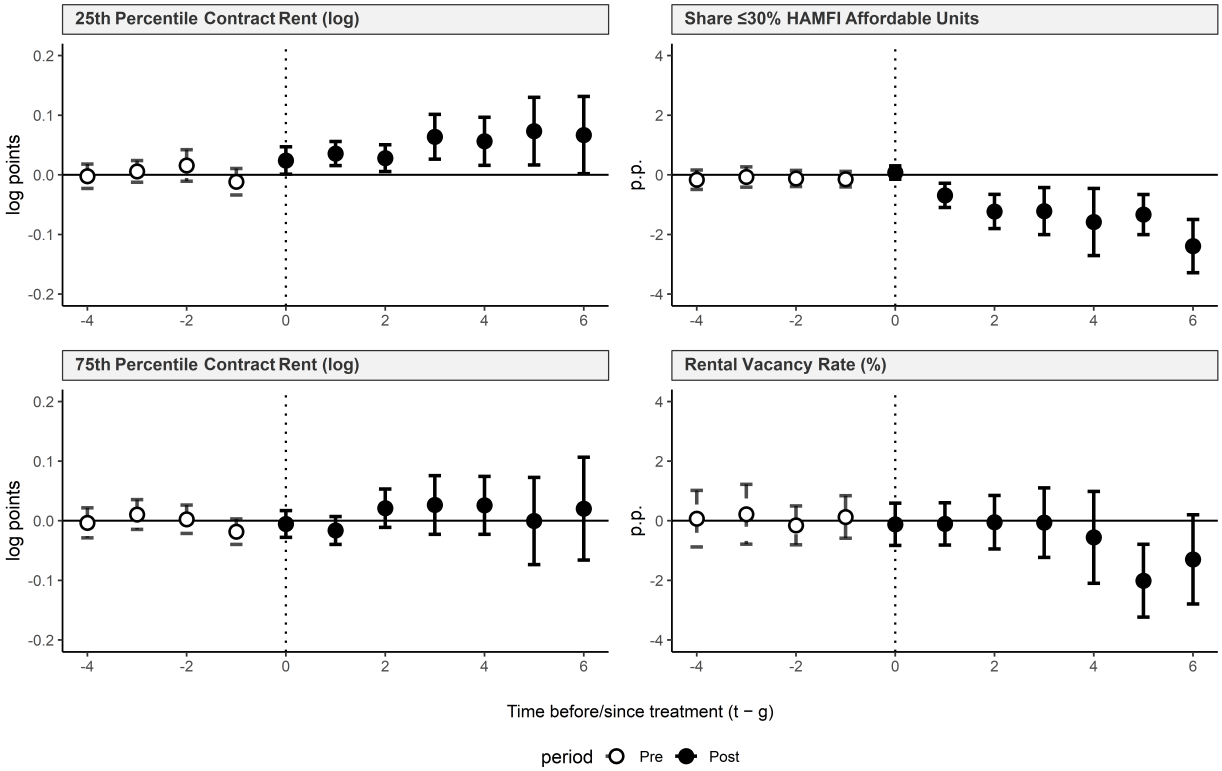
Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on the normalized HHI of HCV Households by occupied census tracts across certain stratified groups. Effects are estimated from Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. Strong Law refers to the policy's enforcement and exemption score both being greater than or equal to 3 for a given city. Low vacancy refers to the city having a lower than median rental vacancy rate in 2012 across the entire sample of treated and non-treated cities. The results account for conditional parallel trends pre-treatment 2012 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred. "HHI" - Herschman-Herfindahl Index.

in affordable units is 2.17 percentage points (versus 1.70 percentage points). The 75th percentile rent remains unchanged, preserving the falsification logic, and the vacancy rate continues to show no significant response. The consistency between SDID and the doubly robust Callaway and Sant'Anna (2021) results strengthens confidence that the findings are not artifacts of the particular identifying assumptions or estimation approach, but rather reflect robust treatment effects that emerge under alternative methods for constructing counterfactuals in staggered adoption settings.

6.4.2 Anticipation Effect Results

Figures 11 and 12 show that the anticipation trim leaves the results materially unchanged. For the four HCV mobility outcomes, the pre-treatment leads at $e \leq -2$ remain centered near zero and the post-adoption

Figure 7: Dynamic Effect Estimates for Rental Affordability Outcomes

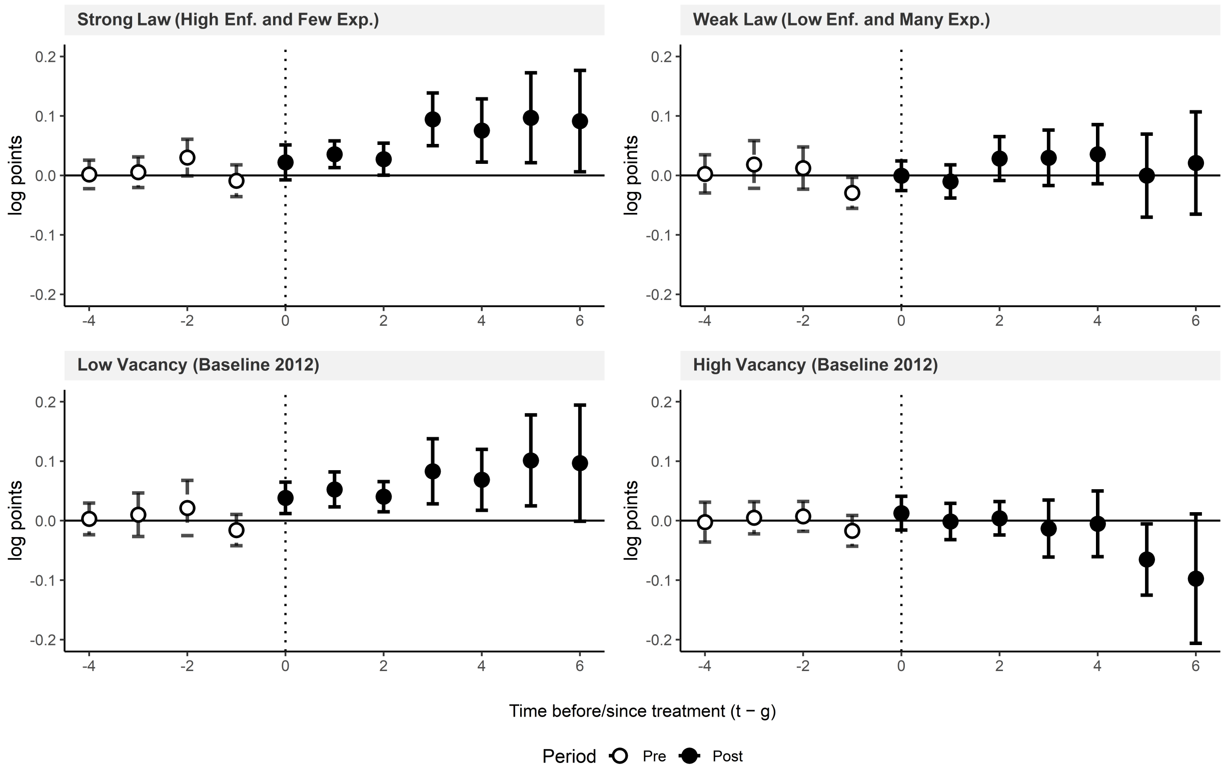


Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on Rental Affordability outcomes from Outcome Table B. Effects are estimated from Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. The results account for conditional parallel trends pre-treatment 2012 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred. "p.p" - Percentage points.

paths stay flat, indicating no detectable re-sorting of voucher households within places even when the final pre-year is excluded. For rental market outcomes, the core pattern persists: the log 25th-percentile rent continues to drift upward over several post years, the share of units affordable at or below 30% of HAMFI declines, the log 75th-percentile rent remains near zero, and vacancy shows no clear contemporaneous break. The magnitudes and precision of the post-period estimates closely track the baseline specification, with any visual shift in onset reflecting the mechanical one-year relabeling of post-treatment time when the anticipation year-period is set to 1.

In sum, allowing for a one-year anticipation window neither reveals hidden pre-trends nor alters the substantive conclusions: SOI protections do not measurably change the within-place geography of voucher residence, while low-tier rents rise and the very-low-rent stock contracts. The robustness of these patterns to excluding the last pre-policy year suggests that pre-effectiveness adjustments, if present, are small in city-level aggregates or occur too close to the effective date to affect the identified dynamics.

Figure 8: Dynamic Effect Estimates for 25th Percentile Rents by Stratification



Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on the 25th Percentile contract rent across certain stratified groups. Effects are estimated from Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. Strong Law refers to the policy's enforcement and exemption score both being greater than or equal to 3 for a given city. Low vacancy refers to the city having a lower than median rental vacancy rate in 2012 across the entire sample of treated and non-treated cities. The results account for conditional parallel trends pre-treatment 2012 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred.

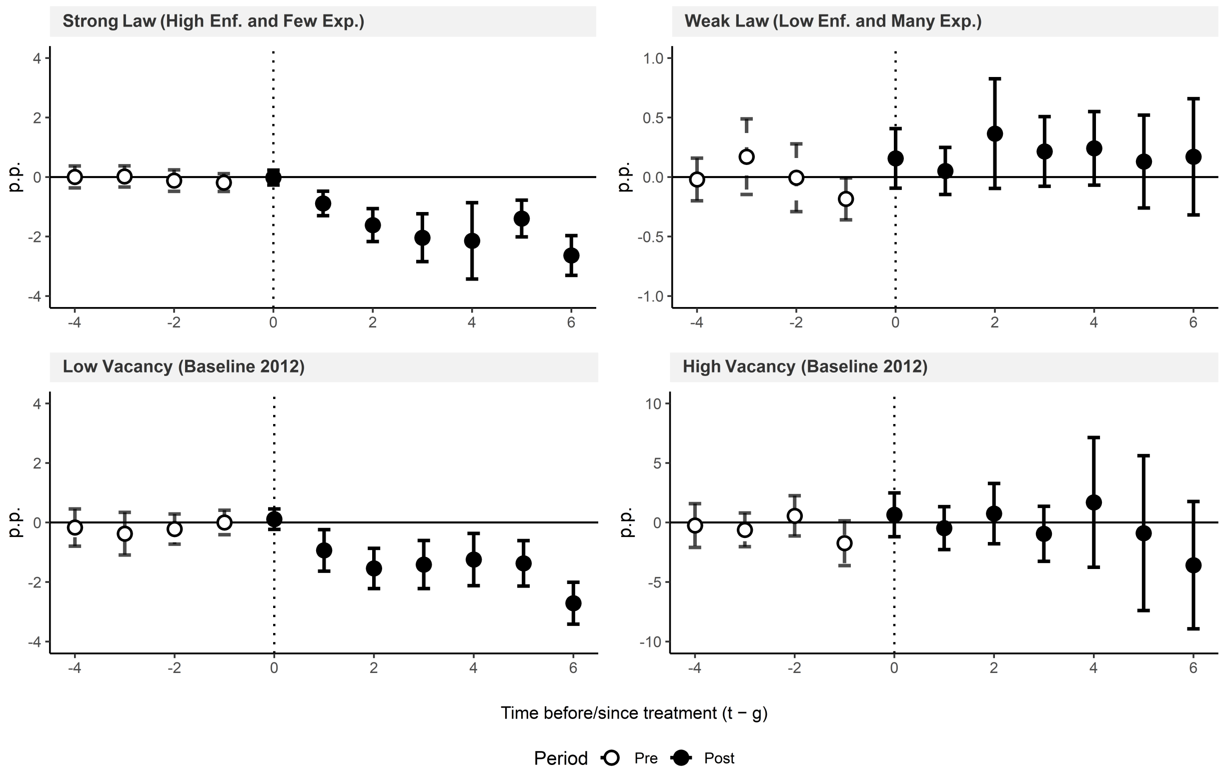
7 Discussion

The results point to a consistent pattern. There is no detectable change in where voucher households live within places after SOI adoption, while prices at the low end of the rent distribution rise and the stock of very low–rent units shrinks. Effects are larger when laws are stricter and markets tighter. This section develops an interpretation of those dynamics, situates the findings in the existing evidence, and notes limitations and next steps.

7.1 Why rents rise after SOI protections even when HCV geography does not shift

SOI protections make categorical refusal costlier but leave ample scope for adjustment on other margins. Landlords that previously excluded vouchers can comply with the letter of the law while altering screening

Figure 9: Dynamic Effect Estimates for the Share of Rental Apartments Affordable at the ≤ 30% HAMFI Threshold by Stratification



Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on the Share of Rental Apartments Affordable at the ≤ 30% HAMFI Threshold across certain stratified groups. Effects are estimated from Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. Strong Law refers to the policy's enforcement and exemption score both being greater than or equal to 3 for a given city. Low vacancy refers to the city having a lower than median rental vacancy rate in 2012 across the entire sample of treated and non-treated cities. The results account for conditional parallel trends pre-treatment 2012 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred. "p.p." - Percentage points.

intensity, search and leasing practices, and asking rents. If voucher tenancy is perceived as higher cost (because of administrative steps, inspection sequencing, rent reasonableness determinations, delayed move-ins, or expectations about turnover and enforcement risk) owners can respond by raising prices where voucher demand is most likely to arrive. That response is most feasible in the part of the market close to payment standards and rent-reasonableness thresholds. In those segments, small price changes can both offset perceived costs and indirectly ration out some voucher applicants without violating the prohibition on categorical refusal.

The event studies line up with that logic. The twenty-fifth percentile of contract rent increases after adoption, while the seventy-fifth percentile is essentially flat. The affordability share moves inversely, consistent with a contraction in the stock of units priced at or below 30 percent of HAMFI. The absence of a

Table 10: Robustness Check: Synthetic Staggered Difference-in-Differences Results

Outcome	SDID ATT	Std. Error	95% Conf. Int.
HCV Mobility Outcomes			
Occupied Tract Share (%)	-1.04	3.56	[-5.94, 4.85]
HCV HH per Occupied Tract	0.93	2.27	[-3.51, 5.37]
Normalized HHI (0-100)	1.83	3.71	[-5.45, 9.10]
HCV-Weighted Tract Poverty (%)	-0.09	0.66	[-1.38, 1.19]
Rental Market Outcomes			
Ln(25th Percentile Rent)	0.060**	0.025	[0.026, 0.081]
Affordable Share $\leq$ 30% HAMFI (%)	-2.17**	0.44	[-2.99, -0.96]
Ln(75th Percentile Rent)	0.015	0.019	[-0.031, 0.045]
Rental Vacancy Rate (%)	-0.53	0.30	[-1.31, 0.38]

Notes: This table reports average treatment effects on the treated (ATT) from synthetic staggered difference-in-differences (SDID) estimation following Arkhangelsky et al. (2021) and Porreca (2022). SDID is applied cohort-by-cohort to avoid problematic comparisons with later-treated units, using never-adopters as donors. Outcomes are residualized on 2012 baseline covariates before SDID estimation. Standard errors are computed via placebo inference with 400 replications. ** indicates statistical significance at the 5% level (confidence interval does not include zero). HCV = Housing Choice Vouchers; HHI = Herfindahl-Hirschman Index; HAMFI = HUD Area Median Family Income.

clear vacancy response in the same window suggests adjustment is occurring primarily on the price margin rather than through short-run quantity changes.

Heterogeneity strengthens the inference in two complementary ways. Where laws are stronger—combining high enforcement and few exemptions—the policy more tightly constrains categorical refusal and raises the expected cost of noncompliance. If owners perceive a "compliance tax" (administrative time, inspection coordination, rent-reasonableness negotiations, legal risk), they can restore target returns by marking up posted rents in the segment where voucher demand arrives and where payment standards and rent-reasonableness determinations provide headroom. Where baseline vacancy is low, short-run supply is inelastic and landlords face thick queues; the same compliance cost can be passed through more readily because the outside option of waiting for another non-voucher applicant is strong. In both dimensions, the event studies behave as the bindingness story predicts: stronger laws and tighter markets show larger increases at the twenty-fifth percentile and bigger declines in the very-low-rent stock, while upper-tier rents remain flat.

This pattern matches equilibrium responses in related protection regimes: models in which added frictions raise landlord costs predict price pass-through in tight segments (Abramson, 2024); reduced-form evidence links stronger tenant rights to higher rents and lower availability at the margin (Coulson et al., 2025); and experimental restrictions on screening have elicited substitution toward other exclusionary tactics rather than neutral compliance (Gorzig and Rho, 2025). The SOI setting fits that playbook: constrain one margin (explicit refusal) and adjustment shows up on margins still available (pricing and screening intensity) in the strata where those adjustments are most feasible.

This mechanism is also compatible with continued frictions early in the application funnel. Complaint-driven enforcement creates low, uncertain penalties ex ante, and discouragement or non-response is difficult to prove, so landlords can comply formally while shaping the pool of applicants who reach lease signing (Cunningham et al., 2018; Unlock NYC et al., 2022; Varady et al., 2017). Several levers remain legal and potent. Owners can quote asking rents just above payment standards or contest rent reasonableness, forcing vouchers to the sidelines without invoking "no vouchers." Minimum-income rules that ignore the subsidy, credit or eviction history screens, and document hurdles raise the cost of applying relative to non-voucher households and steer search back toward the same set of predictable, voucher-accepting tracts. On the supply side, inspection sequencing and Housing Assistance Payment contract timing impose real carry costs; when vacancy is scarce, waiting for a sure non-voucher tenant is a credible alternative. These frictions keep the composition of applicants who survive to the lease stage looking similar across tracts, even if the legal choice set expands on paper. As a result, the within-place geography of voucher residence need not change, while the low-rent segment registers price adjustments where vouchers transact. In short, SOI protections appear to raise the effective cost of outright exclusion; landlords respond on margins they still control, and those adjustments show up where the program actually meets the market.

7.2 Why the HCV mobility results differ from past studies

Several features of the setting and design help explain the divergence from earlier work that finds modest improvements in voucher destinations following SOI adoption (Freeman and Li, 2014; Ellen et al., 2022; Teles and Su, 2022). First, the estimand differs. The analysis is at the place level and follows the stock of voucher households residing in a city each year. Studies that track movers' origin-destination pairs or analyze individual lease-ups are more sensitive to small composition shifts among recent movers. A change at the margin for movers can be washed out when averaged with the large stock of non-movers living in the same place, especially over relatively short horizons.

Second, the outcome set emphasizes robust, scale-free measures of within-place geography: voucher-weighted tract poverty and a normalized HHI of concentration. These measures are less sensitive to mechanical changes in the count of active tracts or to the distribution of small voucher flows across already-active tracts than simple dispersion statistics. The event studies and pooled estimates show those primary measures hovering near zero. Auxiliary dispersion measures tell the same story but are not the focus of inference.

Third, the period and sample differ. The study window covers first-effective dates from 2013 to 2018 in incorporated places with populations above 65,000. Many jurisdictions adopted SOI protections in already tight markets with binding payment standards. Where payment standards lag rents, a legal prohibition on categorical refusal does not expand the set of financially feasible units. Earlier studies detect improvements three to five years after adoption (Ellen et al., 2022; Teles and Su, 2022); effects of that magnitude may require longer horizons, complementary reforms in inspections and payment standards, or targeted landlord engagement to translate formal access into realized moves. The null within-place changes here are therefore consistent with a world in which some movers experience improved opportunities at the margin but the aggregate residential distribution in a city remains broadly stable over the observed horizon.

Finally, enforcement and exemptions matter for any mobility response. Testing work shows substantial noncompliance even in protected jurisdictions and the substitution of new screens when old ones are curtailed (Cunningham et al., 2018; Phillips, 2017; Unlock NYC et al., 2022; Gorzig and Rho, 2025). The heterogeneity design partitions on law strength and baseline tightness. The absence of mobility effects in those splits, together with noisier vacancy-based profiles that lack clean pre-trend support, points to implementation frictions rather than a strong mobility channel in this period.

A useful contrast is payment-standard reform (e.g., Small Area Fair Market Rents (SAFMRs), which operates on the pricing margin rather than the legal-access margin I study. Ellen et al. (2025) finds that SAFMR-based reforms shift successful lease-ups toward higher-rent, lower-poverty neighborhoods without lowering overall lease-up rates and with roughly offsetting program costs across low- and high-rent areas. Because broad SAFMR rollout largely post-dates my window and does not cover the cities driving these estimates, it is not a confound here. Instead it points to a complementary policy bundle: align payment standards with spatial rent gradients while maintaining SOI protections as a backstop against categorical refusal. If the goal is neighborhood improvement for movers, pricing levers appear more directly effective than SOI protections alone in the short run.

Additionally, the null mobility finding may partly reflect revealed preferences among voucher households. Even where SOI protections expand the legal choice set, many households may prefer to remain in familiar neighborhoods due to proximity to family, friends, established social networks, or valued local amenities. If a substantial share of voucher holders would not move even absent discrimination, then removing the legal barrier addresses only one component of observed concentration patterns. Distinguishing preference-based stability from constraint-based concentration remains an empirical challenge, but the results are consistent with both mechanisms operating simultaneously.

8 Conclusion

The findings align with and extend three strands of evidence. First, they complement correspondence and audit studies that document continued discrimination under SOI laws by showing how general-equilibrium adjustments can surface, even when formal refusal is illegal (Cunningham et al., 2018; Phillips, 2017; Unlock NYC et al., 2022). Second, they connect to recent work on tenant protections more broadly, which emphasizes that stronger protections can deliver intended benefits for covered renters while inducing offsetting responses in prices or availability for others (Abramson, 2024; Coulson et al., 2025; Gorzig and Rho, 2025). The concentrated movement of the rent distribution at the lower quartile, together with a decline in the very-low-rent stock, is consistent with those models and with qualitative accounts of landlord strategy (Rosen, 2020; Lucio and Cho, 2025). Third, the results qualify the optimistic interpretation of earlier SOI studies that focus on mover destinations or utilization rates (Freeman, 2012; Freeman and Li, 2014; Ellen et al., 2022). The evidence here suggests SOI protections are not, on their own, a sufficient lever for changing the within-place geography of voucher residence in the short to medium run; complementary tools that reduce leasing frictions and realign payment standards with market rents may be necessary to realize the mobility rationale (Ellen, 2020; Aliprantis et al., 2019; Varady et al., 2017).

Certain limitations deserve emphasis. Policy measurement challenges are inherent in any analysis of heterogeneous local laws. While the Urban Institute's enforcement and exemption scores provide systematic categorization, they inevitably compress complex legal frameworks into broad indices that may miss important nuances. For instance, the enforcement score treats all "private rights of action" equally, but actual deterrent effects likely vary substantially depending on damage caps, fee-shifting provisions, and local legal culture. Similarly, exemptions are coded as binary indicators rather than capturing the share of rental stock actually affected. The policy coding reflects characteristics at adoption or the nearest available year and assumes these features remain constant, but enforcement capacity, complaint processes, and exemption interpretations may evolve substantially post-adoption as agencies gain experience and face budget pressures.

Additionally, outcome measurement constraints limit the precision of effect detection across multiple dimensions. ACS rent quantiles are constructed from published distribution tables rather than unit-level microdata, introducing discretization error that may attenuate estimated price effects. The CHAS affordability measure relies on gross rent thresholds that ignore within-place variation in utility costs, tenant-paid fees, and informal side payments that could shift the effective price distribution. Most fundamentally, HUD administrative data capture residential location but not the search process itself—applications submitted, units toured, discriminatory encounters, or strategic self-sorting by voucher holders who anticipate rejection.

This measurement gap means the analysis cannot distinguish between scenarios where legal protections expand search but do not change ultimate residential patterns versus scenarios where search behavior itself remains unchanged.

Furthermore, temporal and mechanism limitations constrain understanding of both short-run dynamics and long-run equilibrium effects. The maximum post-adoption observation window is six years for the earliest adopters, which may be insufficient to capture full adjustment in housing markets where lease terms, development cycles, and neighborhood change operate on longer timescales. Additionally, the study period predates several important program changes, including expanded Small Area FMR usage and Emergency Housing Voucher deployment during COVID-19, that may interact with SOI protections in ways not captured here.

Several extensions would sharpen inference on mechanisms and welfare. One avenue is to bring earlier stages of the leasing funnel into view by linking PHA leasing logs, inspection timing, and application records to measure delays, denials, and landlord participation dynamically. Another is to combine audit or platform-based inquiry data with enforcement records to quantify how complaint-driven systems miss discouragement and non-response. Unit-level or building-level rent panels would allow decomposition of the P25 movement into within-unit price changes versus compositional shifts in the unit mix transacting at the low end.

Interactions between SOI protections and contemporaneous program changes (payment-standard increases, small-area FMR adoption, landlord signing bonuses) could identify complementary policy bundles that deliver mobility without amplifying price pressure. Border designs that compare treated places to contiguous never-adopters would probe local spillovers and strengthen counterfactual credibility. Finally, distributional consequences for non-voucher low-income renters merit direct study, given the decline in the affordable stock and the concentration of estimated price effects at the bottom of the distribution.

Taken together, the evidence indicates that SOI protections, as implemented in this period, changed landlords' incentives in ways that show up in prices rather than in the residential geography of voucher households. That pattern is not a verdict against SOI protections; rather, it underscores that legal access is a necessary but insufficient condition for mobility gains. Translating protections into moves likely requires reducing administrative frictions, aligning payment standards with market rents, and expanding supply where voucher demand concentrates, so that the cost of compliance does not pass through to the prices faced by the same low-income renters the policy aims to help.

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A Example of "No Section 8" ad

Figure 10: "No Section 8" Craigslist Housing Listings

$3,000 / 4br - 1600ft^2 - Pet Friendly [redacted] Home [redacted]

[redacted]

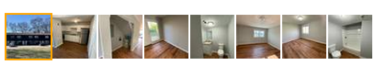
4 bd 2 ba pet friendly home with HUGE backyard just a short walk t[redacted] Located in the very lively and active University neighborhood, this home is perfect for grad students and families alike. Fresh paint & new carpet.

Pets ok! Possible rental time frame can be less than 1 year.

No Section 8.

$1,095 / 2br - *Move-In Special* 2 Bedroom 1.5 Bath Apartment

[redacted]



Charming 2-bedroom, 1.5-bath apartment conveniently located near [redacted] The eat-in kitchen is equipped with a stove and refrigerator, perfect for everyday meals. This unit includes washer and dryer connections, central heat and air, and comes with blinds installed. Water, trash, pest control, and lawn maintenance are all covered by the owner, making this a hassle-free living experience!

No Smoking, No Section 8, No Cats

B Callaway & Sant'Anna (2021) Estimation Details

The estimation process begins by defining the cohort-and-time average treatment effect for places first treated in year g at calendar time t:

$$\mathrm{ATT}_{g,t} \;=\; \mathbb{E}\Big[Y_{it}(1) \;-\; Y_{it}(0) \;\Big|\; G_i = g,\; T_i = t\Big], \tag{1}$$

where $Y_{it}(d)$ denotes the potential outcome for place i in year t under treatment status $d \in \{0,1\}$, G_i is the year of first SOI policy adoption for place i, and T_i is the calendar year of the observation.

Under the usual identifying assumptions of conditional parallel trends and overlap, construct a doubly-robust estimator by first fitting an outcome regression $\hat{m}_d(X_i) = \widehat{\mathbb{E}}[Y_{it} \mid D_i = d, X_i]$ and cohort-specific propensity scores $\hat{e}_g(X_i) = \widehat{\Pr}(G_i = g \mid X_i)$, where X_i is a vector of baseline covariates and $D_i = \mathbf{1}\{T_i \geq G_i\}$. The DR estimator for each (g,t) is then

$$\hat{\tau}^{\mathrm{DR}}_{g,t} = \frac{1}{n_{g,t}} \sum_{i=1}^{n} \Bigg\{ \Big[\hat{m}_1(X_i) - \hat{m}_0(X_i)\Big] + \frac{\mathbf{1}\{G_i = g,\, T_i = t\}}{\hat{e}_g(X_i)} \Big(Y_{it} - \hat{m}_1(X_i)\Big) - \frac{\mathbf{1}\{G_i > t\}}{1 - \sum_{g' \leq t} \hat{e}_{g'}(X_i)} \Big(Y_{it} - \hat{m}_0(X_i)\Big) \Bigg\}, \tag{2}$$

where $n_{g,t} = \sum_i \mathbf{1}\{G_i = g, T_i = t\}$. This estimator is consistent so long as either the outcome model $\hat{m}_d$ or the propensity model $\hat{e}_g$ is correctly specified.

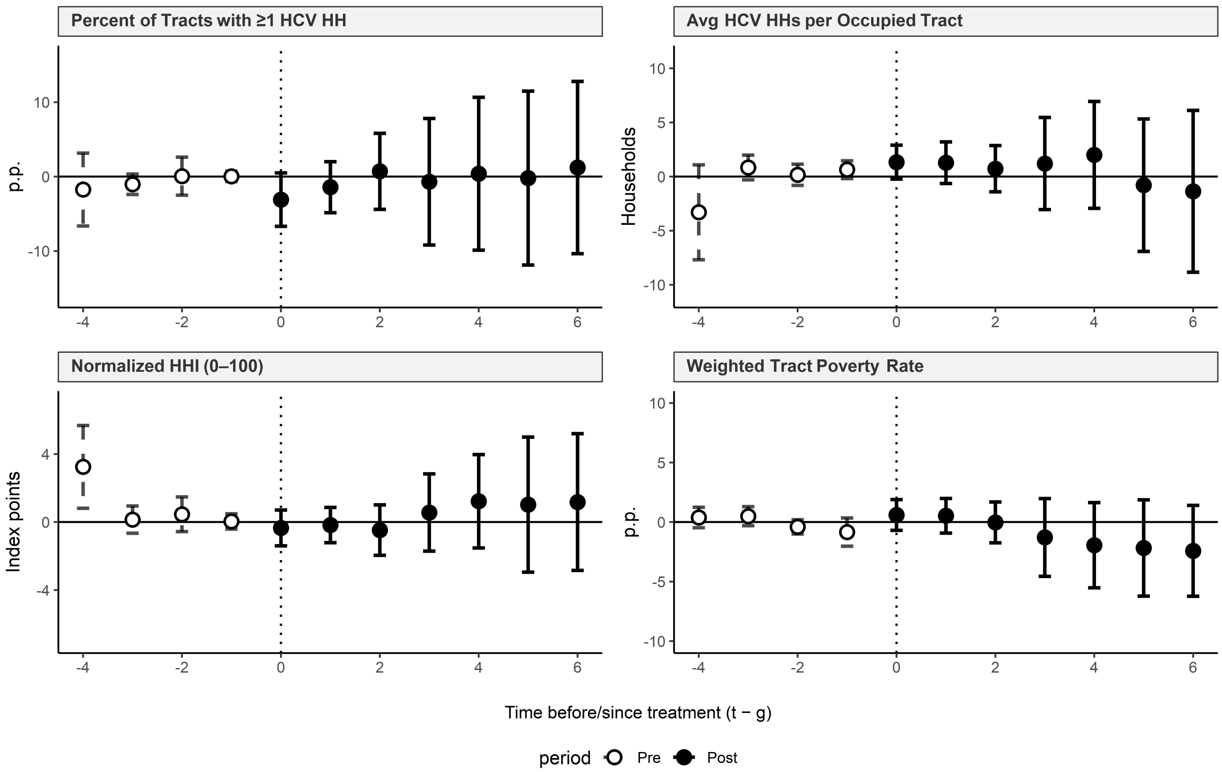
To visualize the dynamic policy response, aggregate these group-time estimates into an event-study curve in relative time $e = t - g$:

$$\mathrm{ATT}(e) \;=\; \sum_{g} w_g \, \hat{\tau}^{\mathrm{DR}}_{g,\,g+e}, \tag{3}$$

where weights w_g reflect the relative size of each cohort.

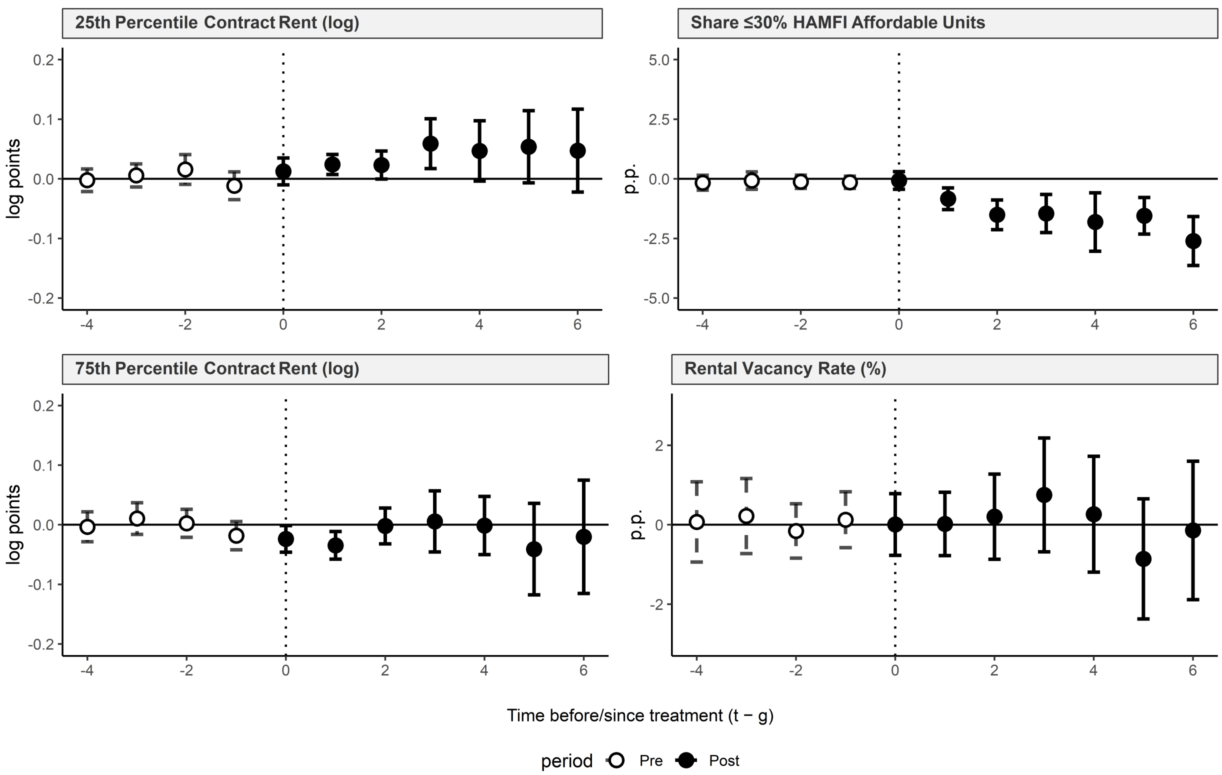
C Anticipation Effects Event Study Graphs

Figure 11: Anticipation Check:
Dynamic Effect Estimates for HCV Household Mobility Outcomes



Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on HCV mobility related outcomes from Outcome Table A with 1 year of anticipation in the Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. The results account for conditional parallel trends pre-treatment 2011 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred. "p.p" - Percentage points, "HHI" - Herschman-Herfindahl Index.

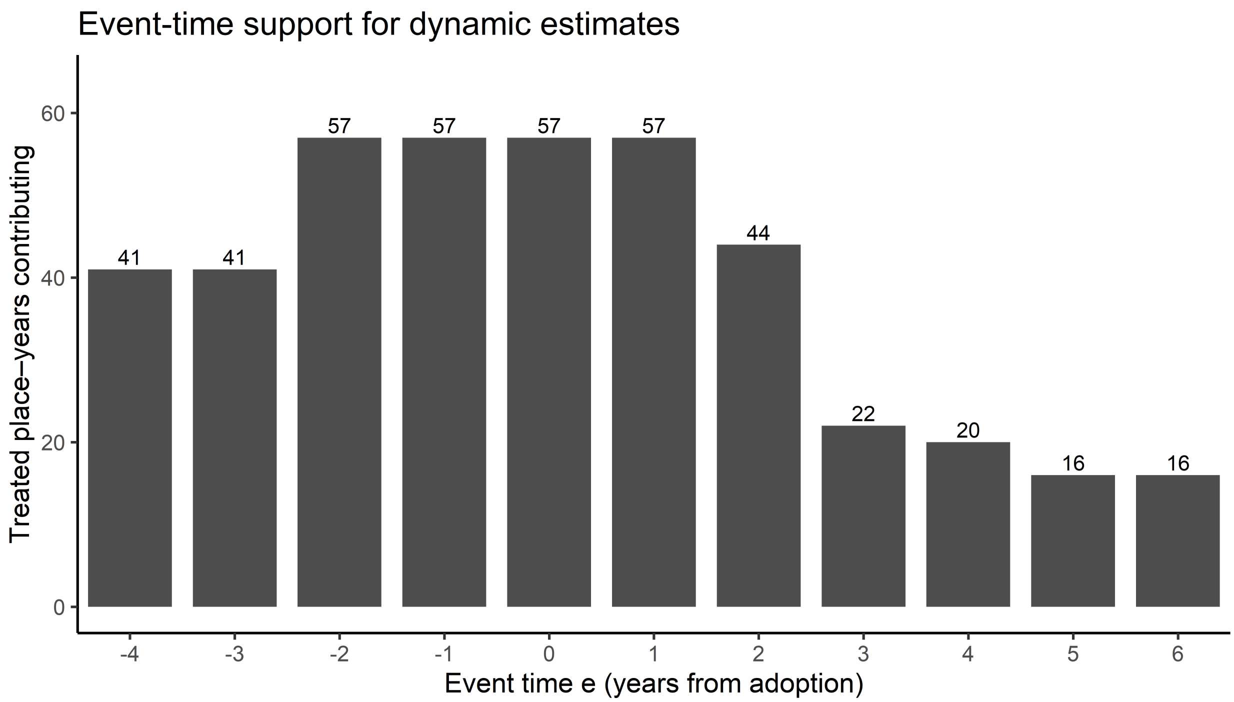
Figure 12: Anticipation Check:
Dynamic Effect Estimates for Rental Affordability Outcomes



Notes: Figure displays 4 event study graphs of the dynamic effect of SOI Protection laws on Rental Affordability outcomes from Outcome Table B with 1 year of anticipation in the Callaway & Sant'Anna (2021) doubly-robust approach. The counterfactual group consists of all census designated places that never passed an SOI protections law in the 2013 to 2018 time frame. The results account for conditional parallel trends pre-treatment 2011 baseline values of median rent, percent of population with a bachelor's degree, median age, median household income, the presence of anti-retaliation laws, the presence of limit fees laws, and the presence of SOI protection laws in other cities within the same state via the doubly-robust inverse probability weighting and outcome regression process. The x-axis represents time in years before or after treatment has occurred. "p.p" - Percentage points.

D Event-time Support

Figure 13: Contributing Treated Place-Year Units by Event Time



Notes: Figure displays event-time support for dynamic estimates. Bars show the number of treated place–years contributing to each event time e = t - g; 0 marks the first effective year.