

Expiring without displacing? Evidence from the first wave of LIHTC affordability exits

Simon Poltak Hamonangan Hutabarat

simon.hutabarat@sampoernauniversity.ac.id

Sampoerna University

Irfan Yulianto

Ministry of Finance

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Abstract

Rent restrictions for Low-Income Housing Tax Credit (LIHTC)- financed projects have an expiration date. Properties placed in service in 1987 reached the end of their 30-year commitments in 2017, opening a wave that will bring 80,000–137,000 low-income units per year to the same threshold through the late 2030s. We study how this exit margin affects properties and neighborhoods. Linking the universe of 55,345 LIHTC properties to a harmonized panel of 22,511 census tracts (2012–2023), we exploit the fact that a property's exit date is mechanically fixed by its placed-in-service year. Three findings stand out. First, exit is common—29% of properties placed in service by 2008 are no longer monitored—and concentrated after Year 30, but it is *less* likely where market rents exceed restricted rents, and recorded commitments beyond 30 years cut exit by 15–22 percentage points. Second, Year-30 eligibility lowers tract median rents slightly (-0.067 log points per unit of exposure; -0.09 per share of rental stock lost in 2SLS) and leaves rent burden, tenure, income, education and racial composition unchanged. Third, once differential trends are controlled, the rent decline is confined to weak markets (-0.115) and absent in tight markets (-0.029 , insignificant). Closing the rent gap would cost roughly one-third as much per affordable unit-year as new credits. Expiration is therefore a targeted rather than universal threat, and preservation resources should follow the rent gap.

JEL classification: H25; I38; R31; R38

Keywords: Low-Income Housing Tax Credit; Affordable housing preservation; Extended use; Qualified contract; Neighborhood change; Displacement; Staggered difference-in-differences

1. Introduction

Most housing programs are remembered for their ribbon-cuttings. The Low-Income Housing Tax Credit (LIHTC) should also be remembered for its expiration dates. Every property financed by the credit signs a contract with time: rents and tenant incomes are restricted for a fixed number of years, after which the owner is, in principle, free to charge whatever the market will bear. For three decades this clause was a footnote. It is now the program's central policy question. The first properties to carry a 30-year commitment reached that horizon around 2017, and, as Fig. 1 shows, the number of low-income units reaching Year 30 climbs from roughly 16,000 in 2017 to between 80,000 and 137,000 *every year* through the late 2030s. Recent projections suggest that about one-third of currently active LIHTC units will reach their scheduled expiration by 2035, and closer to 40% once early exits through the “qualified contract” process are counted (Jaffe & Ingram, 2025).¹

Two very different futures are possible for these units. In the pessimistic scenario, owners convert to market rents, incumbent low-income tenants are priced out, and neighborhoods that LIHTC helped stabilize begin to gentrify. In the benign scenario, the restriction had stopped mattering long ago, because the rent cap—tied to 60% of area median income—already sits at or above what the local market would pay; expiration then changes the paperwork but not the rent. Which scenario dominates is an empirical question with large fiscal stakes, because preservation dollars are scarce and compete directly with funds for new construction.

¹ The qualified contract (QC) is a statutory option, available after Year 14, that allows an owner to ask the state housing finance agency to find a buyer at a formula price; if none is found within a year, the affordability commitment ends (see Section 3). An estimated 155,555 units, about 8.8% of units placed in service by 2008, had left the program through this route by 2024 (PAHRC and NLIHC, 2024; Jaffe and Ingram, 2025).

Yet the evidence base is lopsided. Forty years of research evaluates LIHTC by what it *adds*: whether new units crowd out private construction (Eriksen & Rosenthal, 2010; Soltas, 2024b), how new developments change surrounding prices and crime (Baum-Snow & Marion, 2009; Diamond & McQuade, 2019; Voith et al., 2022), and who lives in them (Ellen et al., 2018). The *exit* margin has received almost no causal attention. The leading national study of post-compliance outcomes covered only Year 15 and predates the first Year-30 expiration (Abt Associates, 2012); recent industry and Federal Reserve studies document the scale of the coming wave and the characteristics of exiting properties, but they are descriptive and do not estimate effects on neighborhoods (Freddie Mac Multifamily, 2022; Jaffe & Ingram, 2025). As a result, policymakers deciding whether to preserve expiring units or fund new ones lack evidence on three basic questions: *which* properties leave, *what happens* to rents and residents around them when they do, and *what it costs* to keep them affordable.²

To fill this gap, we assemble the universe of LIHTC properties in HUD’s national database (55,345 properties placed in service 1987–2024) and link them to a tract-level panel from the American Community Survey (ACS), harmonized to 2010 boundaries for 2012–2023. Our identification exploits a feature of the program that owners cannot choose at the time of exit: the year in which a property *becomes eligible* to leave is fixed by the year it was placed in service three decades earlier. We define each tract’s treatment date as the first year one of its properties reaches Year 30, and its treatment intensity as the number of low-income units in that cohort relative to the tract’s rental stock. Comparing tracts at different points of this mechanically determined schedule—within the same county and year—yields intention-to-treat effects, which we scale by actual exits using two-stage least squares.

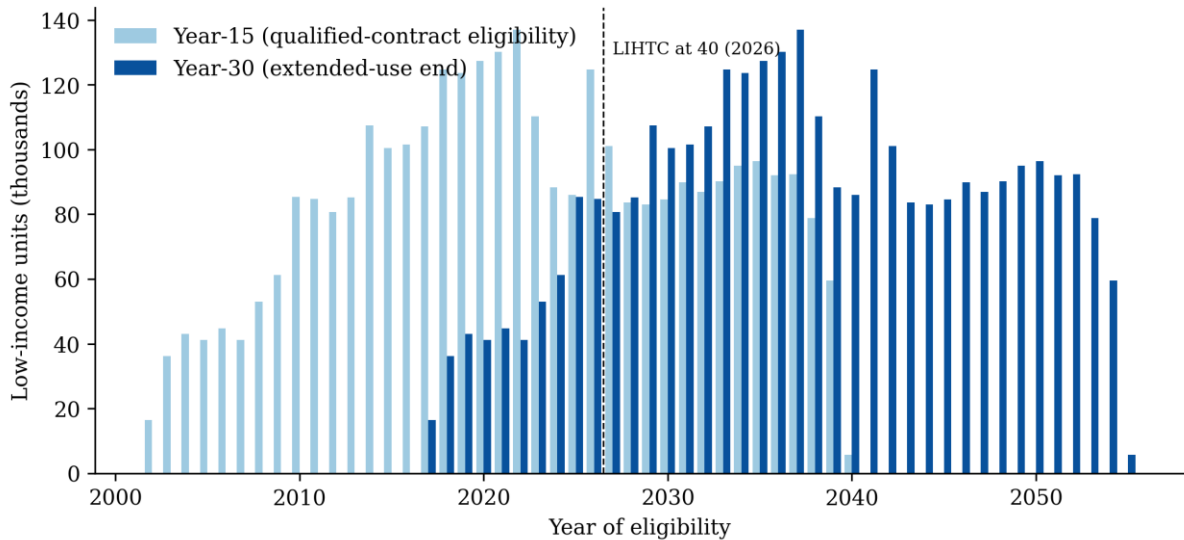
Three findings emerge. First, exit is common but does not follow a simple rent-arbitrage logic: 29% of properties placed in service by 2008 are no longer monitored, and passing Year 30 raises the exit probability by about 45 percentage points, yet exit is *less* likely where market rents exceed restricted rents. Second, Year-30 eligibility does not raise neighborhood rents; it lowers them slightly (about 0.5% at the median exposure) and leaves rent burden, tenure, income, education and racial composition unchanged. Third, the decline is confined to weak markets, while in tight markets—where displacement would be the real risk—we detect no effect. Keeping restricted rents in place costs, on average, about one-third as much per affordable unit-year as building new units with credits.

These results make the paper new in five respects. (i) *Design*: it is, to our knowledge, the first to use the mechanically timed Year-30 eligibility schedule as a staggered natural experiment for LIHTC exit. (ii) *Outcome margin*: it provides the first causal estimates of how expirations affect neighborhood rents and composition. (iii) *Mechanism*: it documents that exit is concentrated where the program’s rent cap does *not* bind, reversing the sign predicted by a pure rent-arbitrage model. (iv) *Method*: it shows that a standard pooled heterogeneity interaction is contaminated by mean reversion in baseline rents, flipping the sign of the market-tightness effect, and that group-specific time effects correct it. (v) *Policy accounting*: it compares new production and preservation per affordable *unit-year delivered*, the metric that matters once commitments are temporary.

The remainder of this article is structured as follows. Section 2 reviews related literature and positions our contribution. Section 3 describes the institutional setting, develops a simple framework and derives testable predictions. Section 4 describes the data. Section 5 presents the econometric strategy; derivations are collected in Appendix B. Section 6 reports the empirical results, Section 7 discusses policy implications and limitations, and Section 8 concludes.

² Exit is not the mirror image of entry. Entry combines construction, a new building, new tenants and a regulatory regime; exit changes only the regulatory status of an existing building. Estimates of entry effects, such as the price spillovers in Diamond and McQuade (2019), therefore cannot simply be reversed to predict exit effects.

Fig. 1. The LIHTC affordability-expiration



Notes: Low-income units by the year in which properties reach Year 15 (first eligibility for the qualified-contract process) and Year 30 (end of the minimum extended-use period). The dashed line marks the program's 40th anniversary.

Source: HUD National LIHTC Database (properties placed in service 1987–2024); authors' calculations.

2. Related literature and contribution

2.1. Supply, crowd-out and the incidence of LIHTC

A first strand asks whether LIHTC adds to the housing stock. Classic studies find substantial crowd-out of private or other subsidized construction (Malpezzi & Vandell, 2002; Sinai & Waldfoegel, 2005; Eriksen and Rosenthal, 2010). The most recent evidence sharpens this conclusion with new data on developer competitions for credits: Soltas (2024b) finds that subsidies add few net units, mainly pull investment forward in time, and that developers capture a large share of the subsidy, while Soltas (2024a) quantifies the fiscal cost of inclusionary incentives and argues that middle-class neighborhoods offer “opportunity bargains.” These papers concern the *entry* margin. If crowd-out is high, the market should also largely offset *exit*—a prediction our small rent effects are consistent with.³

2.2. Neighborhood spillovers of affordable and new housing

A second strand estimates how new developments affect surroundings. Diamond and McQuade (2019) show that LIHTC construction raises house prices in low-income neighborhoods and lowers them in high-income ones. Using the dense Cook County market, Voith et al. (2022) find positive price effects of concentrated LIHTC development, especially in lower-income communities. A closely related recent literature on market-rate supply finds that new buildings in low-income areas lower nearby rents rather than raising them (Asquith et al., 2023) and loosen the low-income market through migration chains (Mast, 2023). Our paper adds a complementary question: what happens when the *regulatory* status of an existing building, rather than the building stock, changes.

2.3. Expiring affordability and preservation

A third strand, mainly policy research, studies what happens when restrictions end. Abt Associates (2012) found that most properties remained affordable after Year 15, often because market rents were close to LIHTC rents. Freddie Mac Multifamily (2022) match exited properties to private rent data in seven metro

³ High crowd-out on entry implies that, absent the subsidy, similar units would have been supplied privately at similar rents. By symmetry, removing the restriction should then move rents little, unless the restriction was binding. Section 3 formalizes this logic through the rent gap.

areas and find that about 61% remained affordable at 60% of area median income, with exits rare in high-rent submarkets. Jaffe and Ingram (2025) project the national expiration wave and warn that early exits via qualified contracts accelerate it, and the PAHRC and NLIHC (2024) preservation report tracks the subsidized stock at risk. These studies are descriptive and do not identify causal effects on neighborhoods; we provide such estimates.

2.4. Econometrics of staggered and continuous treatments

Methodologically, we draw on the recent literature showing that two-way fixed-effects estimators can be biased under staggered adoption with heterogeneous effects (Goodman-Bacon, 2021; Sun & Abraham, 2021; Callaway & Sant’Anna, 2021; de Chaisemartin & D’Haultfœuille, 2023; Roth et al., 2023), on imputation estimators (Borusyak et al., 2024), on continuous (dose) treatments (Callaway et al., 2024), and on sensitivity analysis for parallel trends (Rambachan & Roth, 2023). We contribute an applied illustration of how baseline-characteristic interactions can be confounded by mean reversion, and how group-specific time effects address the problem.

Taken together, and relative to these literatures, we contribute (i) to the LIHTC and affordable-housing literature published in this journal and elsewhere (Voith et al., 2022; Diamond & McQuade, 2019; Soltas, 2024b) by providing the first causal evidence on the exit margin; (ii) to the preservation policy literature (Freddie Mac Multifamily, 2022; Jaffe & Ingram, 2025) by moving from description to identification; and (iii) to applied difference-in-differences practice (Roth et al., 2023; Borusyak et al., 2024) with a cautionary example on heterogeneity analysis.

3. Institutional setting, framework and predictions

3.1. The affordability life cycle of a LIHTC property

LIHTC provides ten years of federal tax credits to developers who agree to restrict rents and incomes on a share of units, typically to households earning at most 60% of area median income (AMI), with gross rents capped at 30% of that income limit. Each property passes through the phases summarized in Table 1.⁴ Since 2012, HUD records whether a property is “no longer monitored” for LIHTC compliance, and since 2018, the reason. HUD cautions that leaving monitoring does not necessarily mean a property stops being affordabl

Table 1. Affordability life cycle and exit routes of a LIHTC property.

Phase	Years	What happens / possible exits
Credit period	1–10	Tax credits are claimed; rents and incomes restricted.
Compliance period	1–15	IRS can recapture credits for noncompliance; exit is rare.
Extended use	16–30 (or longer)	Restrictions continue under state monitoring; owners may exit early through a <i>qualified contract</i> (after Year 14), foreclosure or noncompliance; some states require owners to waive the QC option or impose longer commitments.
Post-expiration	31+	No federal obligation; affordability survives only through other subsidies, mission-driven owners, resyndication with new credits, or “naturally” low market rents.

Notes: Compiled from the Internal Revenue Code §42, Abt Associates (2012) and Jaffe and Ingram (2025).

3.2. A simple framework of the exit decision

Let property j become eligible to exit at date τ_j . The maximum restricted gross rent is

$$R_{jt}^A = 0.30 \times 0.60 \times AMI_{mt} \times \phi_b, \quad (1)$$

⁴ Allocations made before 1990 were not subject to the federal extended-use requirement, which was introduced by the Omnibus Budget Reconciliation Act of 1989. For the earliest cohorts, Year 30 is therefore a weaker legal boundary; we return to this point in Section 7.1.

where ϕ_b is HUD's household-size adjustment for a unit with b bedrooms (Appendix B.1). Let R_{jt}^M be the rent the unit would command on the open market and define the log rent gap $G_{jt} = \ln R_{jt}^M - \ln R_{jt}^A$. Following the real-options view of irreversible conversion decisions (Dixit and Pindyck, 1994) and a random-utility discrete-choice representation (McFadden, 1974), the owner exits if

$$Exit_j = \mathbb{1} \left\{ \sum_{s \geq 0} \delta^s N_j \max(0, R_{j,\tau+s}^M - R_{j,\tau+s}^A) + C_j^{comp} - C_j^{exit} + \varepsilon_j > 0 \right\}, \quad (2)$$

where N_j is the number of restricted units, C_j^{comp} the present value of saved compliance costs (income certification, reporting, inspections), and C_j^{exit} the legal and reputational costs of terminating covenants. The derivation is in Appendix B.2.⁵

The framework yields two regimes. When $G > 0$ (*binding regime*), the rent term dominates: exit rises with G and raises rents for future tenants. When $G \leq 0$ (*non-binding regime*), the rent term is zero and exit is driven by compliance savings; it is cheap, should be common, and has no direct rent effect. If monitoring also disciplines maintenance, exit in this regime may even lower stock quality and, with it, rents.

3.3. Predictions

Prediction 1 (*Who exits*). (a) Exit probabilities jump after Year 30. (b) In the binding regime, exit rises with the rent gap G ; in the non-binding regime it is driven by compliance savings, so the overall sign depends on which regime dominates. (c) Exit is lower where C^{exit} is high: under longer recorded commitments and nonprofit ownership.

Prediction 2 (*Neighborhood rents*). Eligibility raises tract rents where the restriction binds and leaves them unchanged—or lowers them through reduced reinvestment—where it does not.

Prediction 3 (*Displacement and composition*). If rents rise, the share of severely rent-burdened renters rises and neighborhood composition shifts toward higher-income, more educated and whiter households; absent rent increases, composition should be stable.

Prediction 4 (*Cost of preservation*). Because preserving affordability only requires compensating owners for the rent gap, its cost per affordable unit-year is low where G is small and can be far below that of new credits.

Table 2 translates the predictions into a testing strategy, in the spirit of the classification approach of Hahn et al. (2026).

Table 2. Summary of testing strategy.

Prediction	Unit and data	Test	Section / output
1. Who exits	Property; HUD LIHTC	Linear probability model of exit on rent gap and covenants	6.1; Table 6, Fig. 3
2. Rents	Tract-year; ACS	Dose-weighted event study; TWFE; BJS imputation	6.2–6.4; Tables 7–9, Figs. 4–5
3. Displacement	Tract-year; ACS	Same designs on burden, tenure, income, race, education	6.2; Tables 7–8
2–3 by regime	Tract-year; ACS	Group-specific county×year FE by market tightness	6.5; Table 10, Fig. 6
4. Cost	Property; HUD + ACS + CPI	Cost per affordable unit-year	6.6; Table 11, Fig. 7

⁵ Equation (2) is deliberately reduced-form. It ignores, for example, the option value of waiting and the possibility of resyndication with new credits; both would lower exit at a given gap without changing the comparative statics we test.

4. Data

4.1. Data sources and structure

Table 3 lists every data source, its unit of observation, coverage and use. All data are public and were retrieved programmatically.⁶

Table 3. Data sources and structure.

Source	Unit	Coverage	Key variables	Role
HUD National LIHTC Database (2024 release)	Property (n=55,345)	PIS 1987–2024	Tract (2010), PIS year, LI units, sponsor, bond financing, QCT/DDA, monitoring status and reason, affordability period, resyndication, annual credit allocation	Treatment timing, exposure, exit, costs
ACS 5-year estimates (Census API)	Census tract	2012–2023	Median gross rent (B25064), tenure (B25003), income (B19013), race (B03002), rent burden (B25070), education (B15003)	Outcomes
ACS 5-year, county (B19113)	County	2012–2023	Median family income	AMI proxy, restricted rent R^A
Census 2020–2010 tract relationship file	Tract pair	2020/2010	Land-area overlap	Boundary harmonization
FRED CPI-U (CPIAUCSL)	Year	1987–2023	Consumer price index	Deflate costs to 2023 \$

Notes: PIS = placed in service; LI = low-income; QCT = Qualified Census Tract; DDA = Difficult Development Area.

Starting from the HUD file, we drop records with invalid tract identifiers (2,266) or unknown placed-in-service year or unit counts (2,035), leaving 51,044 properties in 22,648 tracts (Appendix Table A.1). A property is coded as *exited* if HUD records it as no longer monitored; blank values denote properties still monitored, because HUD codes only affirmative responses.⁷

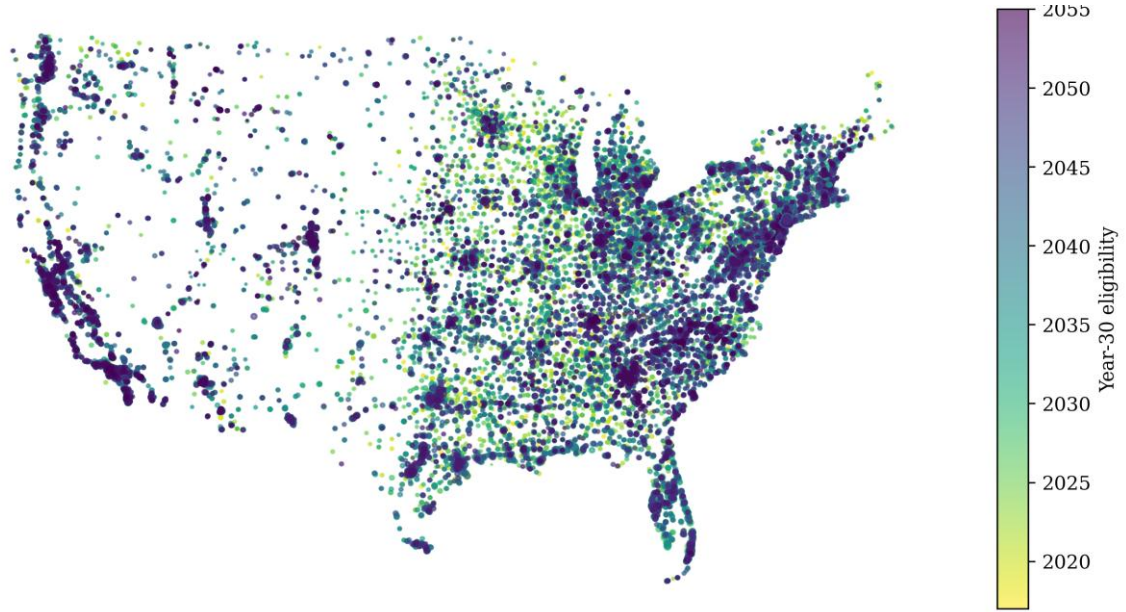
On the outcome side, ACS estimates on 2020 boundaries (2020–2023) are allocated to 2010 tracts with land-area weights (Appendix A.1). The restricted rent follows Eq. (1) for a two-bedroom unit, with county median family income as a proxy for HUD’s AMI.

The resulting analysis panel contains 268,981 tract-years for 22,511 tracts that host at least one LIHTC property (99.4% of eligible tracts match to the ACS). Treated cohorts—first Year-30 eligibility in 2017–2023—comprise 5,492 tracts, each cohort containing at least 547 tracts (Appendix Table A.2); tracts first eligible in 2024 or later serve as not-yet-treated controls. Fig. 2 shows that early and late cohorts are geographically interspersed, supporting within-county comparisons.

⁶ HUD’s server blocks some automated downloads; the LIHTC file can be obtained directly from <https://lihtc.huduser.gov>. ACS tables were retrieved through the Census Bureau API; county median family income comes from ACS table B19113; CPI-U comes from FRED (series CPIAUCSL). Replication code in Python (pandas, pyfixest) is available from the authors.

⁷ Several fields (sponsor type, DDA status, commitments beyond 30 years, resyndication) were introduced in later data collections and are blank for many early properties. We treat blanks as missing in descriptive statistics and report “reported” shares, rather than imputing “no”.

Fig. 2. LIHTC properties by Year-30 eligibility date.



Notes: Each point is a LIHTC property in the contiguous United States; marker size is proportional to the square root of low-income units. Lighter colors indicate earlier eligibility.

Source: HUD National LIHTC Database.

4.2. Descriptive statistics

Table 4 summarizes the data. The median property has 45 low-income units; 24% of properties with reported sponsor type have a nonprofit sponsor; 19% of all properties are no longer monitored. The striking statistic is the rent gap: its median is -0.105 , meaning that in the typical LIHTC tract the restricted two-bedroom rent *exceeds* the tract’s median gross rent by about 10%. Only about 40% of properties face a positive gap.⁸

Table 4. Summary statistics.

Variable	Mean	SD	P25	Median	P75	N
<i>Panel A: Properties</i>						
Low-income units	64.277	69.638	24.000	45.000	80.000	51,044
Nonprofit sponsor (reported)	0.238	0.426	0.000	0.000	0.000	42,519
Tax-exempt bond financed	0.175	0.380	0.000	0.000	0.000	36,834
In QCT	0.214	0.410	0.000	0.000	0.000	50,912
In DDA (reported)	0.170	0.376	0.000	0.000	0.000	28,173
Exited (no longer monitored)	0.192	0.394	0.000	0.000	0.000	51,044
Affordability > 30 yrs	0.051	0.220	0.000	0.000	0.000	51,044
Resyndicated	0.019	0.136	0.000	0.000	0.000	51,044
Rent gap, 2012 (log)	-0.092	0.293	-0.264	-0.105	0.075	48,951
<i>Panel B: LIHTC tracts, 2012</i>						
Median gross rent (\$)	831	296	626	766	966	22,491
Median household income (\$)	45,450	21,198	31,085	41,611	54,808	22,491
Renter share	0.452	0.224	0.274	0.418	0.608	22,491
Rent burden >50% share	0.252	0.115	0.171	0.243	0.324	22,491
Non-Hispanic white share	0.569	0.322	0.279	0.641	0.865	22,491
Bachelor’s degree share	0.146	0.092	0.078	0.125	0.197	22,491
Dose D_c (Y30 cohort units / renters)	0.127	0.153	0.040	0.077	0.154	22,491

Notes: Panel A: properties with valid tract and placed-in-service year. “Reported” variables exclude blank records. “Affordability > 30 yrs” and “Resyndicated” equal one only when reported by the allocating agency. Rent gap: log

⁸ Because the tract median includes older and smaller units, it understates the market rent of a comparable two-bedroom unit, and therefore understates G . The qualitative conclusion that the cap is non-binding for most properties is consistent with Abt Associates (2012) and Freddie Mac Multifamily (2022).

Table 4. Summary statistics.

Variable	Mean	SD	P25	Median	P75	N
tract median gross rent minus log restricted rent (60% AMI, two-bedroom), 2012. Panel B: LIHTC-hosting tracts in 2012. D_c : low-income units in the tract's first Year-30 cohort divided by 2012 renter-occupied units (capped at 1).						

In plain terms. The typical LIHTC property is a mid-sized apartment complex of about 45 affordable units. The most important number in this table is the negative rent gap: in most LIHTC neighborhoods, the legal maximum rent is *higher* than what local renters actually pay. For these properties the “affordability restriction” was not really restricting anything by the time it expired.

5. Econometric modeling strategy

5.1. Who exits?

For properties placed in service 1987–2008, all of which had passed Year 15 by 2023, we estimate linear probability models

$$Exit_j = \theta G_j + \mathbf{X}_j' \boldsymbol{\beta} + \mu_{s(j)} + \eta_{PIS(j)} + u_j, \quad (3)$$

where G_j is the 2012 rent gap of the property's tract, $\mathbf{X}_j$ contains log units, the low-income unit share, a Year-30 indicator, sponsor type, a reported long-commitment indicator, bond financing, and QCT/DDA status, and μ_s , η_{PIS} are state and placed-in-service-year fixed effects. Standard errors are clustered by county.⁹

5.2. Neighborhood effects: dose-weighted event study

Let $E_c = \min_{j \in c} (PIS_j + 30)$ be tract c 's first eligibility year and D_c its exposure: the low-income units in that first cohort divided by the tract's 2012 renter-occupied units. We estimate

$$Y_{ct} = \alpha_c + \lambda_{k(c),t} + \sum_{e \neq -1} \beta_e \mathbb{1}\{t - E_c = e\} D_c + \varepsilon_{ct}, \quad (4)$$

with tract fixed effects α_c and county-by-year fixed effects $\lambda_{k(c),t}$; event time is binned at -5 and $+6$. The summary specification replaces the event indicators with $\text{Post}_{ct} \times D_c$, where $\text{Post}_{ct} = \mathbb{1}\{t \geq E_c\}$. Because the sample contains only LIHTC-hosting tracts, identification comes from the *timing* of eligibility among comparable tracts in the same county and year. The identifying assumption—parallel trends in the absence of eligibility, conditional on county-by-year shocks—and what β_e measures under a continuous dose (Callaway et al., 2024) are discussed in Appendix B.3.¹⁰

5.3. Heterogeneity-robust estimation

Two-way fixed-effects estimators can be biased under staggered adoption with heterogeneous effects (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfoeuille, 2023). Following Borusyak et al. (2024), we fit untreated potential outcomes on not-yet-eligible observations,

$$Y_{ct}(0) = \alpha_c + \gamma_t + \nu_{ct}, \quad t < E_c, \quad (5)$$

impute $\hat{Y}_{ct}(0)$ for eligible observations, and estimate the dose slope of $\hat{\tau}_{ct} = Y_{ct} - \hat{Y}_{ct}(0)$ at each event time, with tract-cluster bootstrap intervals (Appendix B.4).

⁹ A linear probability model is preferred to a logit because it accommodates two sets of high-dimensional fixed effects without incidental-parameter bias and its coefficients read directly as percentage-point changes. The link to the latent-index model of Eq. (2) is shown in Appendix B.2.

¹⁰ Eligibility dates are set by placed-in-service years three decades earlier, so they are unlikely to respond to recent local shocks. The threat is that early and late cohorts sit in neighborhoods on different trajectories; county-by-year fixed effects, pre-trend tests, a placebo date and alternative estimators address it.

5.4. From eligibility to units lost

The first stage relates eligibility to the share of rental units that actually leave monitoring; the second stage uses its prediction:

$$\text{Lost}_{ct} = \alpha_c + \lambda_{k(c),t} + \pi(\text{Post}_{ct} \times D_c) + u_{ct}, \quad Y_{ct} = \alpha_c + \lambda_{k(c),t} + \rho \widehat{\text{Lost}}_{ct} + v_{ct}. \quad (6)$$

Under the exclusion restriction that eligibility affects neighborhoods only through exits, $\rho = \beta^{RF}/\pi$ is the effect of losing restricted units equal to the tract's entire rental stock (Appendix B.5).¹¹

5.5. Heterogeneity by market tightness

Let $\text{Tight}_c = \mathbb{1}\{G_{c,2012} > \text{median}\}$. A pooled interaction model compares tight-market treated tracts with *all* untreated tracts in the county; if baseline rent levels predict later rent growth (mean reversion), the interaction is biased (Appendix B.6). We therefore let the county-by-year effects differ by market type:

$$Y_{ct} = \alpha_c + \lambda_{k(c),t,\text{Tight}_c} + \beta_1(\text{Post}_{ct} \times D_c) + \beta_2(\text{Post}_{ct} \times D_c \times \text{Tight}_c) + \varepsilon_{ct}. \quad (7)$$

5.6. Cost per affordable unit-year

The cost of new credits per affordable unit-year delivered and the rent-subsidy cost of preserving a unit are

$$c_j^{\text{new}} = \frac{10 A_j p \kappa_{t(j)}}{L I_j T_j}, \quad c_j^{\text{pres}} = 12 \max(0, R_{j,2023}^M - R_{j,2023}^A), \quad (8)$$

where A_j is the annual credit allocation, $p = 0.90$ the equity price per credit dollar, κ a CPI-U deflator to 2023 dollars and T_j the realized affordable years (30, or 16 for qualified-contract exits). Appendix B.7 derives both expressions.¹²

6. Empirical results

Before discussing the individual estimates, Table 5 summarizes the main findings against the four predictions. Overall, the results reject a uniform “conversion and displacement” view of expiration and support the non-binding-regime interpretation: exit and its consequences are concentrated where the program's rent cap no longer constrained rents.

Table 5. Summary of main empirical findings.

Prediction	Main finding	Interpretation	Assessment
1a. Exit after Year 30	+45 pp after Year 30	Extended use is the binding legal margin	Supported
1b. Exit and rent gap	Exit <i>falls</i> with the gap (−0.12); weak within state and cohort	Non-binding regime dominates	Binding-regime sign not supported
1c. Covenants and sponsors	Long commitments −15 to −22 pp; nonprofits −6 pp	Higher exit costs deter exit	Supported
2. Rents	−0.067 per unit dose; −0.115 in weak and −0.029 (n.s.) in tight markets	No conversion premium; mild decline where cap did not bind	Non-binding branch supported
3. Displacement	No change in burden, tenure, income, race, education	No tract-level displacement	Supported (no rent rise, no shift)
4. Cost	Preservation \$1,418 vs. new \$4,478 per unit-year (2023 \$)	Preservation cheap except where gap is large	Supported

¹¹ Exit years are not reported by HUD. We date exits at Year 30, or at Year 16 for qualified-contract sales. Measurement error in exit timing affects the first stage but not the reduced form.

¹² The equity price follows the credit-pricing literature (Eriksen, 2009); results are proportional to p . Because c^{pres} excludes rehabilitation capital needs of 30-year-old buildings, it is a lower bound on the true cost of preservation.

Table 5. Summary of main empirical findings.

Prediction	Main finding	Interpretation	Assessment
<i>Notes:</i> “Supported” indicates that the result matches the prediction in the relevant regime across specifications. pp = percentage points. n.s. = not statistically significant at 10%.			

6.1. Who exits? (Prediction 1)

Exit is common and tied to the legal schedule, but it is concentrated where market rents are *below* restricted rents. Longer commitments strongly deter exit.

Table 6 reports linear probability models. The average exit rate in this sample is 0.289, so about 8,900 of the 30,845 properties placed in service by 2008 are no longer monitored. Passing Year 30 raises the exit probability by about 45 percentage points (columns 2–3), confirming Prediction 1a. The rent-gap coefficient is negative in every column: in column 3, a property whose tract market rent is 10% higher relative to the cap is about 1.2 percentage points *less* likely to have exited. Within state and placed-in-service cohort (columns 4–5), the coefficient shrinks to -0.02 to -0.03 , meaning that most of the raw pattern reflects *where* and *when* properties were built and how state agencies monitor them, rather than a property-by-property response to rents.¹³

Properties with a reported affordability commitment beyond 30 years are 15–22 points less likely to exit (Prediction 1c). Nonprofit sponsors exit about 6 points less often in the pooled model; column 5 shows that, relative to for-profits, their exit rises with the rent gap (interaction $+0.045$). Bond-financed (4%) properties exit more within states, plausibly because they are more often recapitalized.

Table 6. Determinants of exit from LIHTC monitoring (Prediction 1).

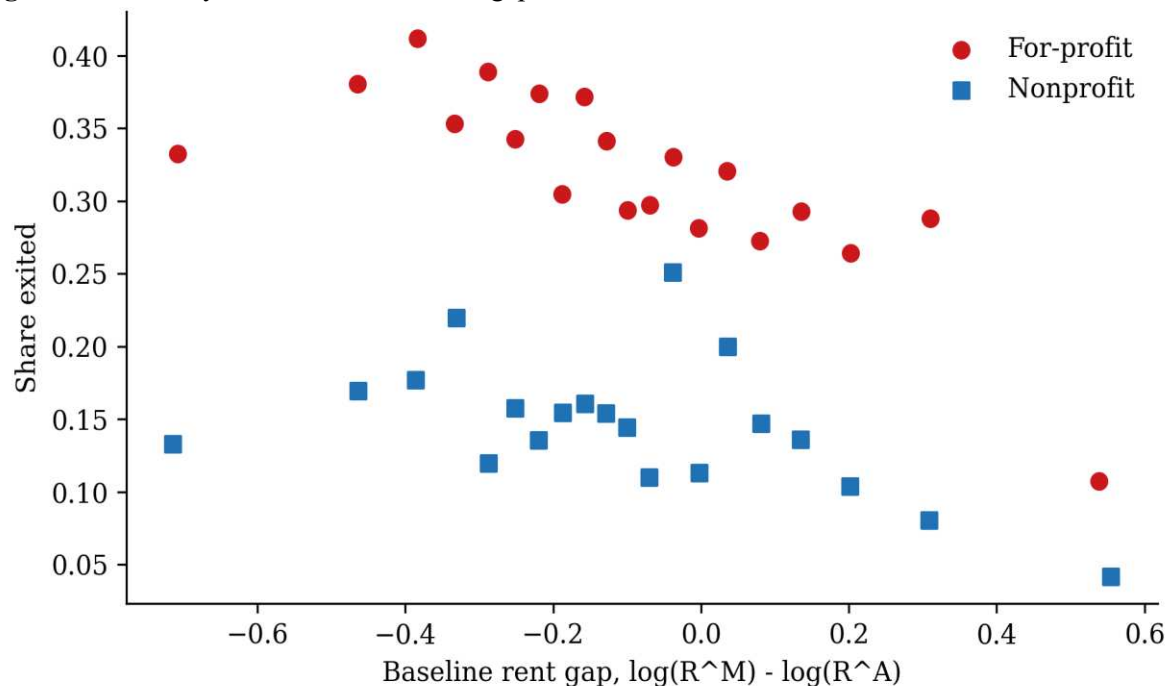
	(1)	(2)	(3)	(4)	(5)
Dep. var.	Exit	Exit	Exit	Exit	Exit
Rent gap ($\log R^M - \log R^A$)	-0.1476^{***} (-2.87)	-0.0966^{***} (-2.82)	-0.1188^{***} (-3.75)	-0.0198 (-1.53)	-0.0285^* (-1.86)
Rent gap $\times$ Nonprofit					0.0450^{**} (2.07)
log LI units		-0.0547^{***} (-3.36)	-0.0599^{***} (-3.43)	-0.0345^{***} (-3.64)	-0.0346^{***} (-3.66)
LI unit share		0.1546^{***} (3.85)	0.1418^{***} (3.11)	0.0603^{***} (3.08)	0.0592^{***} (3.01)
Past Year 30		0.4560^{***} (16.21)	0.4485^{***} (13.84)		
Nonprofit sponsor (reported)			-0.0570^{***} (-4.34)	-0.0105 (-1.26)	-0.0054 (-0.66)
Affordability > 30 yrs (reported)			-0.1545^{***} (-10.67)	-0.2180^{***} (-4.27)	-0.2182^{***} (-4.30)
Bond-financed (4%)			-0.0176 (-1.33)	0.0707^{***} (6.53)	0.0708^{***} (6.56)
QCT			-0.1039^{***} (-11.50)	0.0091 (1.17)	0.0094 (1.20)
DDA (reported)			0.1105^{***} (5.65)	0.2445^{***} (14.90)	0.2439^{***} (14.84)
Constant	0.2738^{***} (15.04)	0.1877^{***} (3.23)	0.2540^{***} (3.93)		
State FE	No	No	No	Yes	Yes
PIS-year FE	No	No	No	Yes	Yes
Mean dep. var.	0.289	0.289	0.289	0.289	0.289
Observations	30,845	30,697	26,625	26,625	26,625
R-squared	0.008	0.309	0.347	0.496	0.496

¹³ The large increase in R^2 from column 3 to column 4 (0.35 to 0.50) reflects the explanatory power of state and cohort fixed effects, consistent with differences in state monitoring practices and extended-use rules.

Table 6. Determinants of exit from LIHTC monitoring (Prediction 1).

	(1)	(2)	(3)	(4)	(5)
Notes: Linear probability models. Sample: properties placed in service 1987–2008 with a valid tract rent gap. Exit = HUD “no longer monitored.” Reported-variable indicators equal zero when not reported. Standard errors clustered by county. <i>t</i> -statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.					

Fig. 3. Exit share by market–restricted rent gap.



Notes: Binned scatter (20 quantiles of the 2012 tract rent gap) of the share of properties no longer monitored, properties placed in service 1987–2008, by sponsor type.

Source: HUD National LIHTC Database and ACS; authors' calculations.

In plain terms. Fig. 3 sorts properties from neighborhoods where the legal cap is far above market rents (left) to neighborhoods where market rents exceed the cap (right). Exit rates *fall* as we move right. Owners are leaving mostly where the rules were not costing them anything—not where they could cash in on higher rents. For-profit owners (red) leave about twice as often as nonprofits (blue) at every level of the gap, and owners who promised more than 30 years of affordability rarely leave at all.

6.2. Effects on neighborhood rents and composition (Predictions 2–3)

Year-30 eligibility does not raise neighborhood rents; it lowers them slightly. Nothing else in the neighborhood changes.

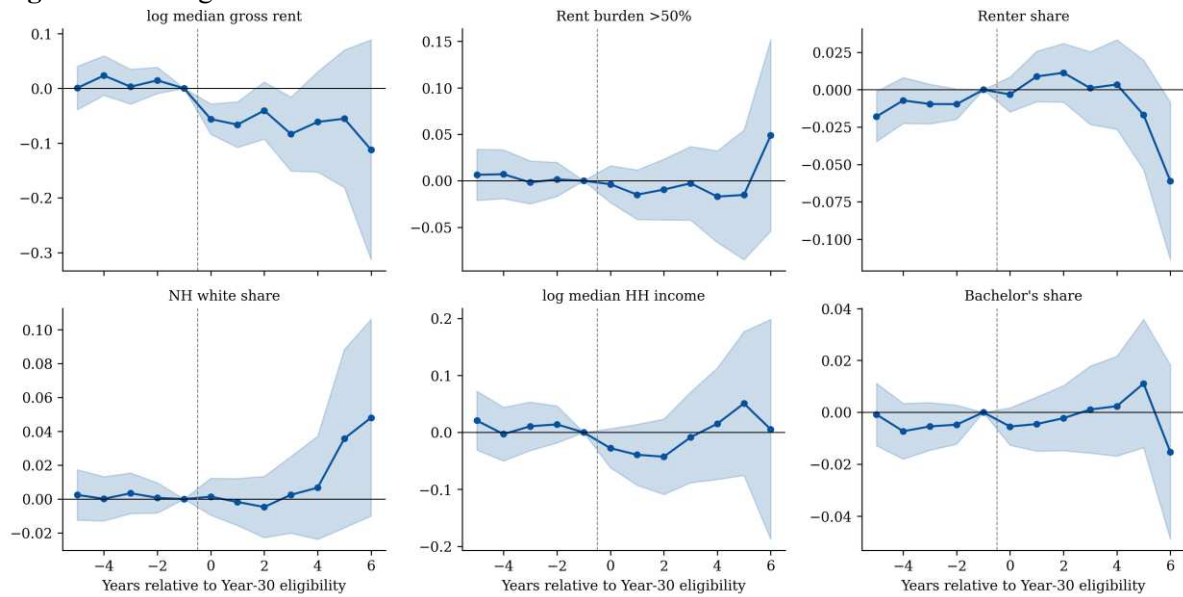
Fig. 4 presents the dose-weighted event studies. For all six outcomes, the coefficients before eligibility are small and statistically indistinguishable from zero—the key evidence that early- and late-eligible tracts were on parallel paths. After eligibility, log median gross rent falls immediately and stays lower; the other outcomes show no systematic movement.¹⁴

Table 7 summarizes the pooled effects. Full exposure ($D_c = 1$, i.e. expiring units equal to the tract's entire rental stock) lowers median rent by 0.067 log points ($t = -3.06$). At the median exposure ($D_c = 0.077$) this is a decline of about 0.5%—roughly \$4 per month on the 2012 median rent of \$766; at the 75th percentile

¹⁴ The late rise in the white share at event times +5 and +6 is estimated only from the 2017 and 2018 cohorts, and its confidence interval includes zero. We do not interpret it.

it is about 1%. The coefficients for severe rent burden, renter share, white share, income, and education are all small and insignificant. The 95% confidence interval for rent burden rules out an increase larger than about 1.5 percentage points even at full exposure.

Fig. 4. Dose-weighted event studies.



Notes: Coefficients β_e from Eq. (4) with 95% confidence intervals; reference period $e = -1$; tract and county-by-year fixed effects; standard errors clustered by tract.

Source: HUD National LIHTC Database and ACS 5-year estimates; authors' calculations.

Table 7. Year-30 eligibility and neighborhood outcomes (Predictions 2–3).

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	log median gross rent	Rent burden >50%	Renter share	NH white share	log median HH income	Bachelor's share
Post $\times$ Dose (D_e)	−0.0669*** (−3.06)	−0.0118 (−0.85)	0.0132 (1.45)	0.0004 (0.05)	−0.0344 (−1.16)	0.0006 (0.11)
Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
County $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	6.808	0.240	0.464	0.552	10.767	0.161
Observations	261,054	261,054	261,054	261,054	260,862	261,054
R-squared	0.936	0.642	0.966	0.985	0.944	0.919

Notes: Sample: LIHTC-hosting tracts, ACS 2012–2023 (2020 tracts harmonized to 2010). Standard errors clustered by tract. *t*-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In plain terms. Each small panel in Fig. 4 follows neighborhoods through time, lined up so that year 0 is the year their LIHTC property could first leave the program. Flat lines to the left of the dashed line show that these neighborhoods were behaving like everyone else beforehand. To the right, only rents move—and they move *down*, by a few dollars a month. Measures of hardship (share of renters paying over half their income in rent) and of gentrification (income, education, racial mix) stay flat. In short: no rent spike, no visible displacement.

6.3. Units lost: first stage and 2SLS

Eligibility strongly predicts actual exit (Table 8, column 1): each unit of exposure translates into 0.75 units of rental stock leaving monitoring, with a first-stage *F*-statistic of 458—far above conventional weak-instrument thresholds. Column 3 implies that losing restricted units equal to 10% of a tract's rental stock lowers median rent by about 0.9%. The 2SLS estimates for rent burden, white share and income remain small and insignificant.

Table 8. First stage, reduced form and 2SLS.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Units lost	log rent	log rent	Rent burden	White share	log income

Table 8. First stage, reduced form and 2SLS.

	(1)	(2)	(3)	(4)	(5)	(6)
Post $\times$ Dose (instrument)	0.7473*** (21.41)	−0.0669*** (−3.06)				
Units lost / renters			−0.0895*** (−2.93)	−0.0157 (−0.84)	−0.0000 (−0.00)	−0.0441 (−1.09)
Specification	First stage	Reduced form	2SLS	2SLS	2SLS	2SLS
First-stage F	458.5	458.5	458.5	458.5	458.5	458.5
Tract & County $\times$ Year	Yes	Yes	Yes	Yes	Yes	Yes
FE						
Observations	261,011	261,054	261,011	261,011	261,011	260,824
R-squared	0.818	0.936				

Notes: Units lost: low-income units no longer monitored divided by 2012 renter-occupied units, with exits dated at Year 30 (Year 16 for qualified-contract sales). Instrument: $\text{Post}_{ct} \times D_c$. Tract and county-by-year fixed effects; standard errors clustered by tract. t -statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In plain terms. Reaching Year 30 is not the same as leaving. Column 1 shows that about three out of four eligible units actually leave monitoring. Columns 3–6 rescale the effects to “per unit actually lost”: if a neighborhood loses restricted units equal to one-tenth of its rental homes, typical rents fall by roughly 1%, and nothing else measurably changes.

6.4. Robustness

Table 9 probes the main rent estimate. Using state-by-year instead of county-by-year fixed effects (column 2) or trimming the top 5% of exposure (column 3) leaves it largely unchanged. Weighting by renter households (column 4) shrinks it to -0.020 : the effect is concentrated in thin rental markets, where one expiring property is a large share of the local stock. A placebo that dates eligibility three years too early, estimated only on pre-eligibility years, yields 0.004 ($t = 0.33$, column 5). Dating treatment at Year 15 rather than Year 30 yields nothing (column 6), consistent with the relative rarity of qualified-contract exits. Dropping event years -4 to -1 , whose five-year ACS windows partly overlap eligibility, leaves -0.050 (column 7).¹⁵

Fig. 5 compares the event-study coefficients with the imputation estimator of Borusyak et al. (2024). The robust estimates are, if anything, more negative (-0.07 to -0.14 over event years $0-4$), and their bootstrap intervals exclude zero in those years.

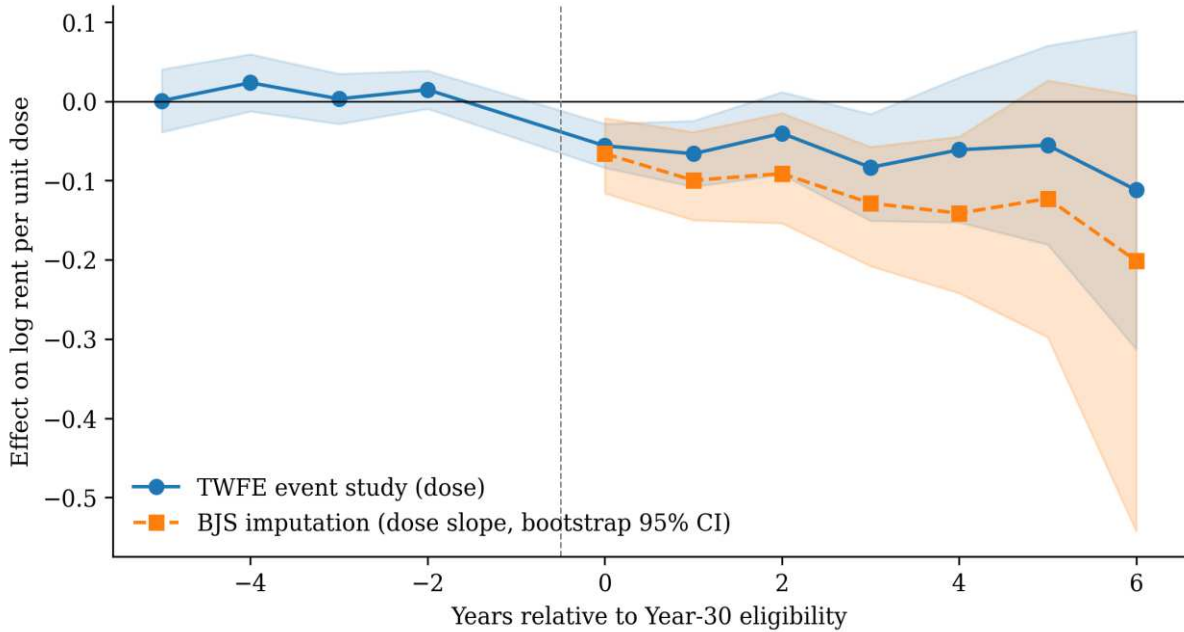
Table 9. Robustness: log median gross rent.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.	log rent	log rent	log rent	log rent	log rent	log rent	log rent
Post $\times$ Dose (Y30)	−0.0669*** (−3.06)	−0.0585*** (−2.83)	−0.0539* (−1.82)	−0.0201 (−0.88)			−0.0501* (−1.90)
Placebo: $\text{Post}(E_c - 3) \times \text{Dose}$					0.0044 (0.33)		
Post $\times$ Dose (Y15 timing)						0.0075 (0.70)	
Specification	Baseline	State $\times$ Year FE	Drop top 5% dose	Renter-weighted	Placebo (pre-period)	Year-15 timing	Drop rel. −4..-1
Observations	261,054	268,981	247,860	261,011	238,795	261,054	230,562
R-squared	0.936	0.927	0.940	0.951	0.941	0.936	0.938

Notes: Column (4) weights by 2012 renter-occupied units and excludes tracts with none. Column (5) uses only pre-eligibility years with a fictitious event three years early. Column (6) dates treatment at Year 15. Column (7) drops event years -4 to -1 . Standard errors clustered by tract. t -statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

¹⁵ An ACS five-year estimate labeled 2023 averages responses from 2019–2023. Effects therefore phase in gradually and can leak into pre-period estimates; column 7 removes these overlapping years.

Fig. 5. Two-way fixed effects versus imputation estimator.



Notes: Blue: event-study coefficients from Eq. (4) with 95% confidence intervals. Orange: dose slopes of imputed treatment effects (Borusyak et al., 2024); shaded areas are 95% tract-cluster bootstrap intervals (300 replications, imputation step held fixed).

In plain terms. Table 9 asks whether the rent decline is an artifact of our choices; it is not. The “fake date” test (column 5) is the key check: if we pretend eligibility happened three years earlier, we find nothing—so the effect appears only when the legal clock actually runs out. Fig. 5 redoes the analysis with a newer method designed for programs that start at different times in different places; it gives the same—slightly larger—answer.

6.5. Where do expirations bite?

The rent decline occurs only in weak markets; in tight markets eligibility has no measurable effect.

Table 10 estimates Eq. (7). Column 1 shows the naive pooled model with common county-by-year effects: the tight-market interaction is -0.101 , suggesting larger declines in tight markets. Allowing county-by-year effects to differ by market type (column 2) reverses the sign: the effect in weak markets is -0.115 ($t = -3.14$), and the interaction is $+0.086$, so the total effect in tight markets is -0.029 . Separate regressions (columns 5–6) confirm this split. The naive result arises because tracts with high baseline rents saw slower rent growth afterward whether or not they were treated; comparing tight treated tracts with all county controls loads this mean reversion onto the interaction (Appendix B.6; the full pooled results are in Appendix Table C.1). Severe rent burden does not respond in either market type (column 4). Fig. 6 shows subgroup estimates; sponsor and racial-composition splits differ little.¹⁶

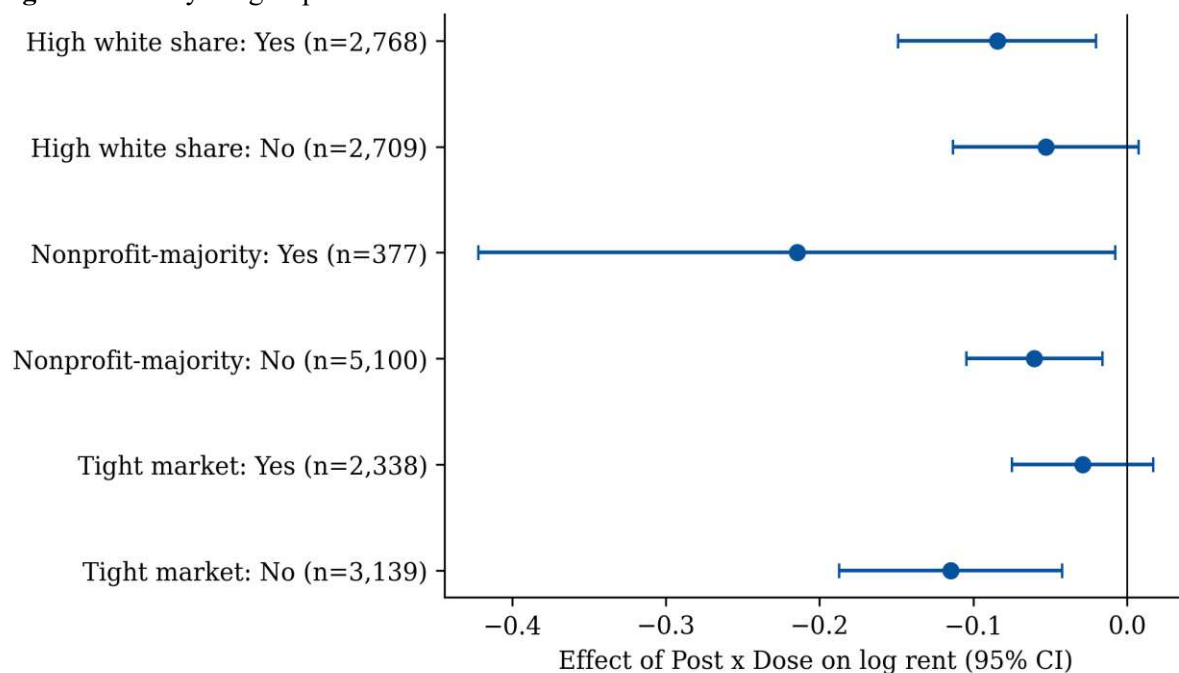
¹⁶ The nonprofit-majority subgroup contains only 377 treated tracts, hence its wide confidence interval. A split by commitments beyond 30 years is not identified because no tract in the 2017–2023 cohorts reports such a commitment.

Table 10. Market tightness with group-specific county-by-year fixed effects.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	log rent	log rent	log rent	Rent burden	log rent	log rent
Post $\times$ Dose	-0.0217 (-0.63)	-0.1147*** (-3.14)	-0.0833*** (-4.10)	-0.0125 (-0.70)	-0.0288 (-1.23)	-0.1147*** (-3.10)
... $\times$ Tight market	-0.1010** (-2.43)	0.0859** (1.97)		-0.0048 (-0.17)		
... $\times$ Rent gap (std.)			-0.0070 (-0.30)			
Fixed effects	County $\times$ Yr	County $\times$ Yr $\times$ Tight	County $\times$ Yr $\times$ Gap tercile	County $\times$ Yr $\times$ Tight	County $\times$ Yr	County $\times$ Yr
Sample	All	All	All	All	Tight only	Weak only
Observations	261,054	252,708	245,353	252,708	127,643	125,065
R-squared	0.936	0.940	0.942	0.657	0.943	0.908

Notes: Tight: 2012 tract rent gap above the median of LIHTC tracts. Columns (2)–(4) compare treated tracts only with untreated tracts of the same market type in the same county-year. Columns (5)–(6) estimate separately by market type. Standard errors clustered by tract. *t*-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Fig. 6. Effects by subgroup.



Notes: Post $\times D_c$ coefficients on log median gross rent estimated separately by subgroup with tract and county-by-year fixed effects and 95% confidence intervals; *n* is the number of treated tracts.

In plain terms. Here we split neighborhoods into “tight” markets (market rents high relative to the LIHTC cap) and “weak” markets. A first, naive comparison suggested rents fell most in tight markets. That turned out to be an illusion: expensive neighborhoods were cooling down anyway during these years. Comparing like with like (column 2 and Fig. 6), rents fall only in *weak* markets—by about \$7 a month at typical exposure—while in tight markets, where owners could in principle raise rents, nothing measurable happens, partly because fewer owners leave there.

6.6. Preservation versus new production (Prediction 4)

Preserving affordability is, for most properties, much cheaper per affordable unit-year than replacing it.

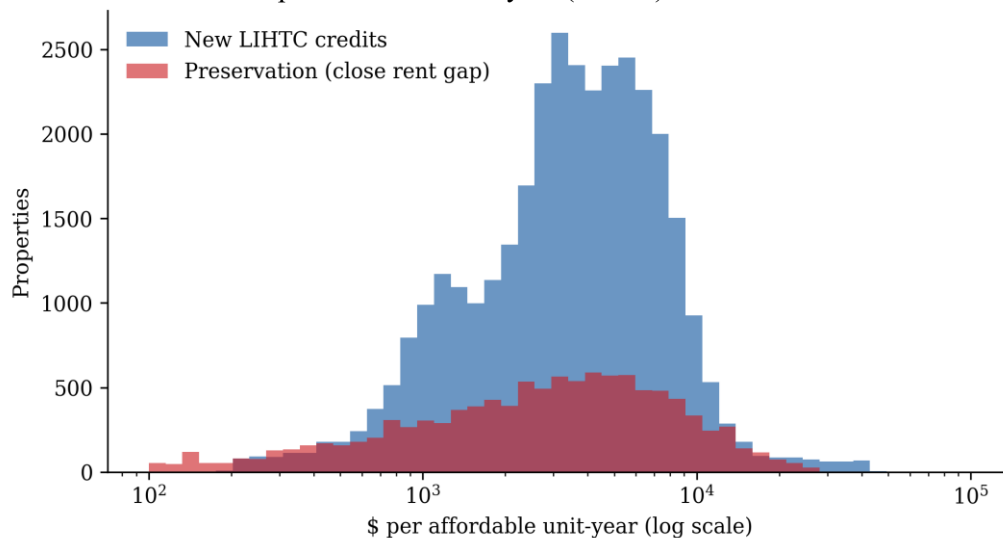
Table 11 and Fig. 7 compare costs in 2023 dollars. New credits cost \$4,478 per affordable unit-year on average (median \$3,558). Closing the rent gap would cost \$1,418 on average and nothing for the median property, whose restricted rent is already at or above market. Preservation is cheaper for 83% of properties. Properties that exited had lower costs on both measures, consistent with exit in non-binding markets and with the smaller allocations of early cohorts.

Table 11. Cost per affordable unit-year: new credits versus preservation (2023 \$).

	New credits		Preservation		Share pres.	
	Mean	Median	Mean	Median	cheaper	N
2-3(lr)4-5 Group						
All properties	4,478	3,558	1,418	0	0.83	33,840
Exited	2,485	2,031	669	0	0.85	4,953
Still monitored	4,820	3,975	1,543	0	0.83	28,887
Nonprofit	5,494	4,651	1,829	0	0.83	8,163
For-profit	4,155	3,258	1,286	0	0.83	25,677

Notes: New credits: $10 \times \text{annual allocation} \times 0.90$ equity price, deflated by CPI-U, divided by low-income units and realized affordable years. Preservation: $12 \times \max(0, R^M - R^A)$ at 2023 tract rents; excludes rehabilitation capital needs (lower bound). Trimmed at the 1st and 99th percentiles of new-credit cost.

Fig. 7. Distribution of costs per affordable unit-year (2023 \$).



Notes: Log scale. Preservation costs shown only for properties with a positive rent gap; they are zero for the remainder.

In plain terms. Building new affordable housing with tax credits costs taxpayers about \$4,500 per apartment per year of affordability—roughly \$370 a month. Keeping an existing LIHTC apartment affordable by paying the owner the difference between market rent and the LIHTC rent costs about \$1,400 per year on average, and nothing at all for the typical property. The catch is that older buildings also need repairs, which this calculation leaves out; the comparison is therefore most useful for deciding *which* properties deserve preservation money.

7. Discussion and policy implications

The results reconcile two features of LIHTC that are usually discussed separately. Rent caps are tied to area median income rather than to neighborhood market rents, so in much of the country—particularly outside high-cost metros—the cap sits at or above what the local market would bear. And commitments are temporary. Where the cap does not bind, expiration removes a constraint that was not delivering below-market rents: owners leave readily and rents do not rise. The mild rent decline in weak markets is consistent

with reduced reinvestment once monitoring ends, or with changes in the composition of the rental stock; tract data cannot separate these mechanisms.

Three implications follow. *Targeting*: preservation funds spread evenly over expiring properties will mostly pay for affordability the market already provides; simple screens comparing local market rents with the 60%-AMI limit can identify the minority of properties where expiration is a real threat. *Commitment length*: recorded commitments beyond 30 years sharply reduce exit, making longer covenants at allocation a low-cost preservation tool for properties sited in appreciating markets.¹⁷ *Measurement*: HUD’s monitoring flag is a weak proxy for affordability; systematically recording post-exit rents would let agencies distinguish genuine conversions from administrative exits.

7.1. Limitations

Leaving monitoring does not by itself imply loss of affordability, and the exit reason is reported for few properties, so exit years are imputed from the extended-use schedule. Allocations before 1990 were not subject to the federal extended-use requirement, so Year 30 is a weaker legal boundary for the earliest cohorts; re-estimating effects for post-1989 allocations is a natural extension. The rent gap relies on tract median gross rent and county median family income as proxies for unit-level market rent and HUD’s AMI, introducing measurement error that likely attenuates estimates. ACS five-year estimates smooth outcomes over time. Finally, tract aggregates cannot follow individual tenants: the absence of tract-level displacement signals does not rule out hardship for specific households, particularly deeply targeted ones (Freddie Mac Multifamily, 2022).

8. Conclusions

Forty years after its creation, LIHTC is entering a decade in which roughly as many units will reach the end of their affordability commitments each year as the program now produces. Using the first national wave of Year-30 expirations as a natural experiment, we find that exits are common but concentrated where the program’s rent restriction no longer binds; that eligibility lowers neighborhood rents slightly in weak markets and has no measurable effect in tight markets; and that there are no tract-level signs of displacement. Preserving the minority of at-risk properties in tight markets is far cheaper per affordable unit-year than replacing them with new credits. As the expiration wave grows, the central policy question is less *whether* to preserve LIHTC units than *which* ones.

Appendix A. Data construction and additional descriptives

Table A.1. Sample construction.

Step	Count
LIHTC properties in HUD database (1987–2024)	55,345
with valid 2010 tract identifier	53,079
and valid placed-in-service year and unit count	51,044
Tracts hosting at least one valid property	22,648
Tracts matched to ACS panel (analysis sample)	22,511
Tract-year observations, 2012–2023	268,981
Properties in exit sample (PIS 1987–2008, valid rent gap)	30,845

Table A.2. Tracts by first Year-30 eligibility cohort.

E_c	2017	2018	2019	2020	2021	2022	2023	2024	2025	2026	2027	2028
Tracts	547	1,028	920	702	796	712	787	757	910	793	762	721

¹⁷ Several states already require owners to waive the qualified-contract option, and since December 2024 HUD restricts eligibility for certain FHA multifamily insurance programs to owners who waive it (Jaffe and Ingram, 2025). Our results suggest such rules matter most in tight markets.

Notes: Cohorts 2017–2023 are treated within the sample window (5,492 tracts); later cohorts are not-yet-treated controls. The small 2017 cohort reflects the program’s partial first year (1987).

A.1. Harmonizing tract boundaries

ACS estimates for 2020–2023 use 2020 tract boundaries. Following the areal-interpolation logic of the Longitudinal Tract Database (Logan et al., 2014), a count Y in 2010 tract c is

$$Y_c^{2010} = \sum_k w_{kc} Y_k^{2020}, \quad w_{kc} = \frac{\text{Land area}(k \cap c)}{\text{Land area}(k)},$$

where k indexes 2020 tracts. Medians are averaged with weights $w_{kc} \times$ occupied units. Area weighting assumes uniform population density within 2020 tracts; it introduces classical measurement error in post-2020 outcomes that is common to treated and control tracts.

Appendix B. Derivations

B.1. Restricted rent (Eq. 1)

Under Internal Revenue Code §42(g)(2), gross rent on a restricted unit may not exceed 30% of the applicable income limit. Under the 40/60 election, the income limit is 60% of AMI adjusted for household size; HUD imputes 1.5 persons per bedroom. For a two-bedroom unit (three persons), the HUD adjustment factor is 0.90 of the four-person limit, giving

$$R^A = 1/12 \times 0.30 \times 0.60 \times 0.90 \times AMI_{4p},$$

in monthly terms, which is Eq. (1) with $\phi_2 = 0.90$.

B.2. Exit decision and the linear probability model (Eqs. 2–3)

Following the irreversible-investment framework of Dixit and Pindyck (1994), conversion is a one-time, costly and largely irreversible decision. Let V_j^M and V_j^A be the values of operating at market and at restricted rents after τ_j . With per-period rents R^M, R^A , compliance cost c per period and discount factor δ ,

$$V_j^M - V_j^A = \sum_{s \geq 0} \delta^s [N_j \max(0, R_{j,\tau+s}^M - R_{j,\tau+s}^A) + c_j] \equiv \Delta_j(G_j) + C_j^{\text{comp}}.$$

The $\max(\cdot)$ term reflects that an owner can always charge less than the cap, so the restriction costs nothing when $R^M \leq R^A$. Adding the termination cost C_j^{exit} and an idiosyncratic term ε_j gives the latent index of Eq. (2), in the random-utility form of McFadden (1974). With ε_j drawn from distribution F ,

$$\Pr(\text{Exit}_j = 1) = F(\Delta_j(G_j) + C_j^{\text{comp}} - C_j^{\text{exit}}),$$

so $\partial \Pr / \partial G = f(\cdot) \partial \Delta / \partial G$, which is positive when $G > 0$ and zero when $G \leq 0$. A first-order expansion of F around the sample mean index yields the linear probability model in Eq. (3), with θ the average marginal effect; covariates $\mathbf{X}_j$ proxy C^{exit} (sponsor type, recorded commitments) and C^{comp} (size, low-income share), and fixed effects absorb state rules and cohort-specific contract terms. A negative empirical θ therefore indicates that properties in non-binding markets—where C^{comp} dominates—account for most exits.

B.3. Dose-weighted event study (Eq. 4)

Let $Y_{ct}(d, g)$ denote potential outcomes under dose d and eligibility date g . Following Callaway et al. (2024), define the average causal response at event time e as $ACR_e = \partial E[Y_{c,g+e}(d, g) - Y_{c,g+e}(0, \infty)] / \partial d$. Under (i) no anticipation and (ii) parallel trends in untreated outcomes conditional on county-by-year shocks,

$$E[Y_{ct}(0, \infty) - Y_{c,t-1}(0, \infty) | c, k(c)] = \lambda_{k(c),t} - \lambda_{k(c),t-1},$$

and (iii) effects linear in dose, the coefficient β_e in Eq. (4) identifies ACR_e . Without linearity, β_e is a weighted average of marginal responses across doses (Callaway et al., 2024). Binning at the endpoints follows Sun and Abraham (2021); flat pre-period coefficients ($e < 0$) provide the standard, though imperfect, test of (ii) (Roth et al., 2023; Rambachan and Roth, 2023).

B.4. Imputation estimator (Eq. 5)

Following Borusyak et al. (2024): (1) estimate α_c and γ_t by OLS on untreated observations $\Omega_0 = \{(c, t): t < E_c\}$; (2) impute $\hat{Y}_{ct}(0) = \hat{\alpha}_c + \hat{\gamma}_t$ for treated observations Ω_1 ; (3) compute $\hat{\tau}_{ct} = Y_{ct} - \hat{Y}_{ct}(0)$. Under parallel trends and no anticipation, $E[\hat{\tau}_{ct}] = \tau_{ct}$ for each treated cell, regardless of how effects vary across cohorts, which avoids the negative-weighting problem of two-way fixed effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2023). Because our treatment is continuous, at each event time e we regress $\hat{\tau}_{ct}$ on D_c and report the slope,

$$\hat{b}_e = \frac{\sum_{(c,t) \in \Omega_1^e} (D_c - \bar{D}_e) \hat{\tau}_{ct}}{\sum_{(c,t) \in \Omega_1^e} (D_c - \bar{D}_e)^2},$$

the imputation analogue of β_e . Confidence intervals come from a tract-cluster bootstrap that resamples tracts and holds the first step fixed; they therefore understate first-step uncertainty slightly.

B.5. 2SLS as a Wald ratio (Eq. 6)

With a single instrument $Z_{ct} = \text{Post}_{ct} \times D_c$ and the same fixed effects in both stages, the Frisch–Waugh–Lovell theorem implies that 2SLS equals the ratio of reduced-form to first-stage coefficients (Angrist and Pischke, 2009):

$$\hat{\rho} = \frac{\widehat{\text{Cov}}(\tilde{Y}, \tilde{Z})}{\widehat{\text{Cov}}(\tilde{L}_{ost}, \tilde{Z})} = \frac{\hat{\beta}^{RF}}{\hat{\pi}},$$

where tildes denote residuals after partialling out the fixed effects. In our data, $\hat{\beta}^{RF} = -0.0669$ and $\hat{\pi} = 0.7473$, giving $\hat{\rho} = -0.0895$, matching Table 8. Under monotonicity, $\hat{\rho}$ is a weighted average of the effect per unit lost among tracts whose exits respond to eligibility.

B.6. Mean reversion in pooled heterogeneity interactions

Suppose untreated outcomes follow $Y_{ct}(0) = \alpha_c + \lambda_{k,t} + \delta_t \text{Tight}_c + u_{ct}$, where δ_t captures differential trends between high- and low-rent tracts (e.g. mean reversion, δ_t decreasing in t). The pooled model with common $\lambda_{k,t}$ omits $\delta_t \text{Tight}_c$. Because treated observations are concentrated in later years, $\text{Post}_{ct} \times D_c \times \text{Tight}_c$ is correlated with $\delta_t \text{Tight}_c$, and the interaction estimate converges to

$$\text{plim } \hat{\beta}_2^{\text{pooled}} = \beta_2 + \frac{\text{Cov}(\tilde{Z}^T, \delta_t \text{Tight}_c)}{\text{Var}(\tilde{Z}^T)},$$

where $\tilde{Z}^T$ is the residualized triple interaction. When δ_t is decreasing, the bias is negative, exactly the pattern in Appendix Table C.1. Allowing $\lambda_{k,t,\text{Tight}_c}$ as in Eq. (7), absorbs $\delta_t \text{Tight}_c$ and removes the bias; this is the conditional parallel-trends logic discussed in Callaway and Sant'Anna (2021) and Roth et al. (2023).

B.7. Cost per affordable unit-year (Eq. 8)

An annual credit allocation A_j is claimed for ten years. Investors purchase the credit stream at price p per credit dollar (Eriksen, 2009), so the equity raised—the public cost in present-value terms at the investor's discount rate—is $10A_j p$. Deflating to 2023 dollars with $\kappa_t = \text{CPI}_{2023}/\text{CPI}_t$ and dividing by restricted units and realized affordable years T_j gives c_j^{new} . For preservation, the minimum annual payment that leaves an owner indifferent between staying and converting (ignoring compliance and exit costs) equals the annual rent gap, $12\max(0, R^M - R^A)$, from the indifference condition $V_j^M = V_j^A$ in Appendix B.2. Both measures

are undiscounted per-year averages; discounting the 30-year credit stream would raise c^{new} relative to c^{pres} and strengthen our conclusion.

Appendix C. Additional tables

Table C.1. Pooled heterogeneity interactions (common county-by-year fixed effects).

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	log rent	log rent	log rent	log rent	Rent burden	Rent burden
Post $\times$ Dose	−0.0217 (−0.63)	−0.0804*** (−4.00)	−0.0609*** (−2.70)	−0.0564* (−1.83)	−0.0054 (−0.31)	−0.0131 (−0.91)
... $\times$ Tight market (2012 rent gap > median)	−0.1010** (−2.43)				−0.0142 (−0.52)	
... $\times$ Rent gap (2012, standardized)		−0.0624** (−2.34)				−0.0064 (−0.64)
... $\times$ Nonprofit-majority cohort			−0.1060 (−1.18)			
... $\times$ High white share (2012)				−0.0245 (−0.56)		
Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
County $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	261,054	260,893	261,054	261,054	261,054	260,893
R-squared	0.936	0.936	0.936	0.936	0.642	0.642

Notes: All models include tract and common county-by-year fixed effects. Column (1) is contaminated by differential trends between high- and low-rent tracts (Appendix B.6); see Table 10 for corrected estimates. Standard errors clustered by tract. *t*-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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